

**Paul Lazarsfeld : The Biography of an Institutional Innovator**

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for the degree of  
Doctor of Philosophy**

**by**

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## A Brief Introduction to the Thesis

## INTRODUCTION

Most available histories of sociology tend to concentrate on the intellectual work of the few key figures whose writings are seen as central to the discipline's academic development. Hence the history of sociology has become more or less synonymous with the history of sociological ideas. As a result, the parallel growth of the institutional forms necessary to develop and transmit this knowledge - the learned journals, professional associations, schools of instruction and research institutes - has gone largely unremarked and unresearched. Mainly this is because most of the leading organisational innovators have also made substantial intellectual contributions which have tended to direct attention away from the significance of their administrative activities. Such is the case with Paul Lazarsfeld.

Thus, although Lazarsfeld is still alive and intellectually productive, and his writing, particularly his contributions to methodology, already occupy a secure place in the recent history of sociological thought, the present study does not aim to provide a comprehensive commentary on his ideas. Rather, it concentrates on his lesser-known, but no less influential role as one of the discipline's major institutional innovators. Lazarsfeld's organisational career cannot be entirely separated from his intellectual development however, since it was primarily the fact that his developing intellectual and academic aspirations could not be accommodated within existing institutional forms which led

him to search for alternative structures, and to establish one of the first institutes for social research. Through a detailed account of the course of Lazarsfeld's career, the study aims to throw light on the more general relationship between sociological knowledge and institutional practice.

The study begins with a discussion of the political situation in Austria between the wars, concentrating particularly on the role of the Austrian Social Democratic Party. It is essential to discuss this period in detail, since Lazarsfeld's experience as a socialist jew in Vienna at this time was to have a crucial bearing on his later life and intellectual work. Indeed, his career cannot be fully understood in isolation from the tragic course of Austrian socialism. Not only is this crucial for a full understanding of the research centre which he established in Vienna; it also lays the groundwork for an understanding of much that he was later to accomplish in America. Thus, in later sections of the work, his Vienna days are frequently referred to in order to elucidate his modes of operation and courses of action, and to demonstrate the strong continuities in his life.

The main body of the work deals with the period from Lazarsfeld's arrival in America in 1933 until the mid 1950's, when the major thrust of his institutional innovation was over. This second part documents the way in which he established a small research institute along similar lines to his institut  in Vienna and then, by skillful manipulation, managed to develop it into one of the

major centres of learning in the country, namely the Bureau of Applied Social Research. In the process of illustrating how Lazarsfeld accomplished this task, a variety of more general questions concerning such research centres are raised, and discussed. Thus, after illustrating Lazarsfeld's final attempt at institutional innovation in 1950, with his proposal to establish an institute for training in social research, and discussing the part that he played in the founding of the Centre for Advanced Studies in the Behavioural Sciences, the work concludes with an examination of the consequences for the production of knowledge which stems from the insecure financial position of research centres. This was a problem which particularly occupied Lazarsfeld. He sought to reduce research centres' dependence on external financing by integrating them more fully into the administrative structure of their parent universities and the writer illustrates, through a particularly graphic case, the likely consequences for knowledge production resulting from the failure to implement this recommendation.

Before presenting the study however, it is necessary to add a brief note on the methodology employed. The research drew upon three main primary sources; repeated interviews with Lazarsfeld himself, interviews with friends and colleagues who were associated with him at various stages in his career both in Austria and America, and the letters, memoranda and other documents deposited in the archives of the various institutions with which he

was associated. The study constantly moved between these various sources, checking and extending points raised by Lazarsfeld against the documentary evidence and the accounts of associates, and then returning to Lazarsfeld to discuss any new aspects or ambiguities revealed by these sources. Consequently, in the great majority of cases, accounts of the same situation were available from several independent sources. Reconstructing the course of an individual's career is not simply a matter of obtaining factually correct information on dates and sequences of events however; it is also, and more importantly, a matter of interpretation based on an understanding of the overall context within which particular events took place. In order to develop this interpretative dimension however, it is necessary to go beyond the narrative evidence and to assign priorities and weightings to particular features of the situation. Lazarsfeld's relationship with Adorno provides a case in point.

In his recent book, 'The Dialectical Imagination', Martin Jay emphasises the intellectual barriers to their successful collaboration. Lazarsfeld himself on the other hand, stressed the difficulties which stemmed from Adorno's personal manner. The writer, however, prefers to situate the whole affair by relating it to the prevailing structure of administrative research, and more particularly to the nature of philanthropic foundations. Thus, although the writer agrees with Lazarsfeld that Adorno was personally "impossible", it is sociologically

more fruitful to attempt to assess what this "impossibility" meant in the context of that particular research setting. The question of overall perspective is crucial since it has fundamentally structured the conception, conduct and presentation of the research.

Lazarsfeld's success in establishing a major research institute owed much to the rising tide of empiricism with American sociology and the disjuncture between this emerging form of knowledge and the institutional bases available for its practice. Consequently, the study could have concentrated primarily upon the history of American social science and the emergence of empiricism, and then situated Lazarsfeld's success within it. Although this would have explained a large part of Lazarsfeld's institutional impact, it would have added little to an understanding of Lazarsfeld as an innovator, or the innovational process itself as a phenomena in scientific advancement. Consequently, the writer has chosen to approach the general development of American social science through a detailed case study of a particularly important instance of innovation. Whilst not wishing to personalise a general movement through concentrating on one individual, neither does the writer wish to depersonalise it by devaluing the contribution of one of the leading actors involved. For in the end it was Lazarsfeld, not as an agent of 'history', but as a person with unique biographical features, who exploited developing trends to establish an institutional format which has proved of tremendous importance for the discipline.

## Chapter One

### Austria Between the Wars : The Shadow of Socialism

CHAPTER ONE"AUSTRIA BETWEEN THE WARS: THE SHADOW OF SOCIALISM"Introduction

The purpose of this chapter is to outline the historical situation within which Paul Lazarsfeld founded the 'Osterreichische Wirtschafts-psychologische Forschungsstelle' in 1925. Translated into English the title reads, 'Austrian Economic and Psychological Research Centre', but throughout the work it will be referred to simply as the 'Forschungsstelle'.<sup>1</sup>

To date there has been no study of the 'Forschungsstelle', or for that matter any real recognition of its importance in the history of empirical social research. To some extent this is due to the relative paucity of its published research output. The Forschungsstelle's only major contribution is Die Arbeitslosen von Marienthal: a study of unemployment in Marienthal, a village some twenty miles outside Vienna. The study was first published in Germany in 1933, but due to the Jewish sounding nature of the authors' names (Jahoda, Lazarsfeld, Zeisel) their signatures did not appear on the title page. Such tactics afforded little protection against the bonfires of National Socialism however. The study was reviewed by Leopold von Wiese in the Kölner Vierteljahrshefte für Soziologie but this proved to be the last issue before

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1. Although the word 'Forschungsstelle' can be quite adequately translated as 'Research Centre', it is proposed to keep the original German word. This will avoid confusion with the variety of Research Bureaux, Institutes and Centres referred to later. In addition, in interviews conducted by the author in Austria, America and England, respondents referred to the 'Forschungsstelle' by a variety of titles; thus to avoid confusion in the reader's mind as to just what institution is being referred to, the writer has uniformly substituted the title 'Forschungsstelle'.

the journal itself was banned in 1934.<sup>1</sup> A report of the study did appear in America in The Nation under the title 'When Man Eats Dog', (McMurray 1933: 15-18) but neither the reputation of the journal itself nor the popular style in which the review was written was sufficient to engage the interest and acclaim of the academic community.<sup>2</sup> Consequently, although the Marienthal study falls broadly within the tradition of Lynd's Middletown (1929) it has largely gone unnoticed in America and England, and was only translated in 1972 (Jahoda et al 1972). This neglect of its works has meant that the Forschungsstelle has been almost completely forgotten, even within Austria itself. Leopold Rosenmayr, Professor Ordinarius of Sociology at Vienna University, for example, admitted in conversation with the writer that it was not until the early 1950's that he became aware of the Forschungsstelle's existence.<sup>3</sup> However, the Forschungsstelle's claim to importance rests not so much on its completed works as on its position as a milestone in the institutionalisation of empirical social research. Not only was it one of the first research institutes of its kind, but more importantly it was the seedling of the Bureau of Applied Social Research which Paul

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1 Von Wiese was editor of the Kölner Vierteljahrshefte für Soziologie which was the leading German sociological journal at that time. The journal ran to twelve volumes before it ceased publication. The journal was started again in May 1948 with Von Wiese once more as editor, but it appeared under the new title of Kölner Zeitschrift für Soziologie.

2 Lazarsfeld did report on the first findings of Marienthal at the International Psychological Congress held in Germany in 1932. Evidently Charlotte Buhler had arranged for him to be on the programme. Present at the Congress was Gordon Allport, Otto Klineberg and Goodwin Watson who approached him for some more details -- see Lazarsfeld's Memoir 1969: 293 and also appendix A for people he visited when in America.

3 The writer wishes to point out that the conversation with Professor Rosenmayr which took place in Vienna (17 Oct: 73) was of an informal nature. It was by no means, given the circumstances of the situation, as 'rigorous' as other interviews which were nearly always taped. Therefore the writer considers that in fairness to Professor Rosenmayr this should be mentioned, since later some of the points he made are criticised. The writer remains grateful to Professor Rosenmayr for making work space available at his Institute and also for making certain written material available.

Lazarsfeld founded at Columbia University in New York.

In 1937 Lazarsfeld established the Princeton Office of Radio Research. The Office moved to Columbia University in 1940, expanded its scope, and in 1944 adopted the title: Bureau of Applied Social Research. In comparison to the Forschungsstelle, the Bureau has of course had a tremendous impact upon empirical social research, not only through its own productivity, but also through its role as a training ground for individuals who later established similar research institutes themselves. The Bureau acted as a working model of the 'new social sciences', and since its pioneer foundation in 1937 a score of similar institutes, including Chicago, Cornell, Harvard, Illinois, Michigan and Washington, have been established at American universities.

It is not claimed that the Forschungsstelle was a direct forerunner of the Bureau; however, it is claimed that for any full understanding of the Bureau's creation in the late thirties it is imperative to understand the Forschungsstelle's establishment in the twenties. The two institutes are linked through the person of Paul Lazarsfeld.

It is essential to understand the peculiarly 'marginal' position that Lazarsfeld occupied in both America and Austria, for his establishment of both institutions was in part a response to this ambiguity in his structural location. In Austria he was a Marxist intellectual Jew, belonging, yet like his whole class, not belonging in a country stripped of its vast Empire and existing in a world where socialism was in retreat before the rising forces of reaction. He was a member of a political movement with the appearance of power

but without the reality. Intellectually he was associated with empiricism, which in turn was marginal to the social sciences in Austria of that time. Later, in America, he was marginal both as an exile and as an intellectual who was classified as a psychologist, but was more of a sociologist than the American sociologists themselves. Having come to America not as a member of a 'school' but as a little known individual, he had to establish himself within American academia and this he accomplished through the establishment of his own research institute. Although an understanding of Lazarsfeld's marginal structural location is indispensable to any full discussion of the two institutes that he established, it is only one aspect of the total picture.

In discussing the Forschungsstelle, its foundation, operation and decline, it is necessary to set it in the context of a turbulent period in Austrian history, and to some extent European history in general - namely the period between the two world wars. In addition to discussing the general social forces which unfolded during this period and impinged on the establishment of the Forschungsstelle, it is proposed to pay particular attention to the social context that those involved with the Forschungsstelle operated in and on. Unfortunately most of the little material which has been written on the Forschungsstelle (Zeisel 1969 and 1968; Rosenmayr 1965; Lazarsfeld 1969) has been concerned mainly with documentation rather than explication. In an attempt to supplement existing accounts the writer has talked both with the abovementioned individuals and with remaining members of the Forschungsstelle such as Marie Jahoda and Gertrude Wagner. In addition, discussions have been held with other individuals

in Vienna who, while not being directly connected with the Forschungsstelle, had contact with the Lazarsfeld circle after the First World War.

What follows is a discussion of inter-relationships between the Forschungsstelle and the wider society. A sociological interpretation of the institution under discussion is made more viable, owing to the relative visibility of social relations existing within Austria between 1918-1938. The Forschungsstelle was born as part of the struggle that gripped Austria, and indeed much of Europe, during the inter-war period, and died a victim of that struggle. Consequently, in addition to its intrinsic interest, the career of the Forschungsstelle illuminates the changing social situation within Austria; the processes and forces that affected every class, group and institution. Nothing escaped the presence of events. In this situation the members of the Forschungsstelle were not simply engagé intellectuals, they were firmly enmeshed in the fabric of the unfolding conditions. They belonged to a social world that insistently forced its presence upon them. Wider social forces enter at every point in the history of the Forschungsstelle, not only because of the forcefulness of the social events of that time, but also because of the political activism of the Forschungsstelle's individual members.

To examine the Forschungsstelle is to enter the social world of Paul Lazarsfeld and his colleagues. In conversation with the writer<sup>1</sup> Lazarsfeld mentioned that the Forschungsstelle would have been created even in his absence. Although history's alternatives must remain forever speculative, there is good reason for presuming that he is

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<sup>1</sup> The conversation took place at Cambridge, England, March 28 1974. Professor Lazarsfeld is given to such self-effacing remarks and this kind of comment will be met again in the main body of the work.

mistaken on that point. Certainly Dr. Wagner and Prof. Jahoda, two close colleagues of his at the Forschungsstelle, consider that his statement should not be taken at face value. Lazarsfeld was definitely the prime mover and driving force behind the Forschungsstelle. Although many of the forces and factors to be discussed would in some form, variety or combination, present themselves in the life experiences of a considerable number of individuals beside Lazarsfeld, it is the unique combination of factors going to make up his particular biography that were in the end responsible for the fact that it was he rather than someone else who created the Forschungsstelle.

For the moment however, it is to the 'shared conditions' that the work will now turn, leaving the more focussed analysis of the Forschungsstelle and the details of Lazarsfeld's biography till the next chapter. For in order to understand the Forschungsstelle it is essential to first understand the situation of Austria between the wars and in particular the situation of the Austrian Social Democratic Party, which played such an important role in the history of the period and in the life of Lazarsfeld and his colleagues.

The Habsburg Empire : Destruction and Hope

"To a degree unique in the history of nations the story of Austria between the world wars, particularly between 1918-1934 was that of the fight for and against the aspirations and achievements of the working class movement. Otherwise stated, this struggle was that between democracy on the one side and reaction and Fascism on the other in political, economic and social fields. The decisive majority of the democratic element was composed of workers. As long as it was possible they and some of the middle class and peasant allies fought by democratic means ... on the floors of city councils, through the printed page and over the radio ... but when Chancellor Dollfuss succumbed to the temptation of authoritarian ideology and began the installation of a native born Clerical Fascism, they continued the battle in the streets, in the courtyards and on the staircases of the municipal apartments of Vienna as well as in other localities."

(Gulick, Vol 1, 1948: 1)

The above quotation encapsulates the principal theme in the history of the First Republic, and it is to the elaboration of this theme, to the role of the Socialist Democratic Party in particular, which we now turn. For, if one was to select a key contributory factor in the establishment of the Forschungsstelle, then it would be the career of the Austrian socialist movement of which Lazarsfeld was a part.

An important factor in delaying the industrialisation of Austria was the restraining influence of the land tenure system which had the effect of limiting the supply of cheap exploitable labour. The 'necessary' alteration in the land tenure system did not occur until after the liberal revolution of 1848. In the wake of the revolution, the Constituent Assembly abolished the system known as Robot under which the rural labourer was obliged to give labour services to the landlord, and therefore released a vast army of peasants for work in the developing industries of the towns.

"Once Robot was ended, the landowners had no interest in keeping a large peasant population tied to the soil: the smaller peasants sold their holdings to wealthier peasants and moved into the town. A labour force was placed at the services of developing capitalism." (Taylor 1961: 73)

The abolition of Robot was, according to Taylor, "the greatest achievement of the revolution of 1848" (Taylor 1961: 72). If that was so, then it was the task and achievement of the Social Democratic Party to eventually organise the new industrial proletariat into one of the most politically conscious and disciplined labour movements that Europe has witnessed.

Prior to the collapse of the Empire in 1918, the working class of Austria had little effective political voice.<sup>1</sup> The end of the old social order when it came was swift and sudden, but it had been preceded by premonitions of its own demise which gave the proceedings a certain ethereal air. This atmosphere was at its strongest in the Imperial showpiece of Vienna which, "on the surface had never seemed more vital than when it was dying" (Crankshaw 1970: 328). As Ernst Fischer writes of the city:

"For a couple of decades however Vienna was in fact one of the most interesting cities in the world: the Vienna school of medicine, of music, of political economy, positivism, Austro Marxism, psychoanalysis, Sigmund Freud, Gustav Mahler, Arnold Schonberg and Karl Kraus. As a rule things tended to come to Austria later than elsewhere; not so the premonitions of impending catastrophe, the heightening sensibility, the loss of reality. Something was coming to an end ... not only the monarchy, not only the century but a whole world, 'fawned upon by decay'." (Fischer 1974: 76)

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1 See Buttinger 1953 : 70 for the lack of political influence of Austrian labour before 1918.

The 'decay' was the rotting of the structural supports upon which the empire was built. It was being undermined through the nationalistic aspirations of its peoples outside German-Austria which eventually advanced to the point of driving Austria into the First World War in an effort to maintain its imperialistic grip. Thus the Empire was not destroyed through the social revolution of the German-Austrian or even the Magyar proletariat, but rather by the national revolutions of the Czech, Polish and Yugoslav bourgeoisie. The proletariat did not offer a particular threat to the Empire's continued existence; it was only after:

"...the Slav nations broke away from the Empire only when the collapse of the Empire had become inevitable and was imminent, did the proletariat in Austria and Hungary revolt. The Empire was not disrupted by the proletarian revolution, but the disruption of the Empire awakened and unchained the proletarian revolution." (Bauer 1925: 74)

The desertion from the Empire of its various national parts thus provided both the opportunity and the impetus for the Austrian working class to seize the remnants and, in particular, to capture Vienna.

The most significant geographical feature of the capital was its strategic positioning across some of the most important European highways which gave it a particularly cosmopolitan air, attracting as it did peoples from all over the Empire. It was the cultural capital, before the First Republic, not only of the German speaking people but of South and East Europe also. However, Crankshaw (1970: 325) labels the culture of the capital before the Imperial collapse as "the decadence of a remarkable civilisation centered on the rich administrative capital of a disintegrating

Empire". This was the decadence of a specialised culture deriving from a limited ruling elite of nobility and higher bureaucracy; an elite that was possibly more tightly knit than anywhere else in Imperial Europe. Restricted as this elite was, it had nevertheless made an art form out of social grace and charm, and had stamped Vienna with its presence. In its support of the arts, the theatre and the opera, it offered a splendid display of talent. Yet this group was in decline, soon to be overwhelmed by the rising middle classes of its own Empire with their thrusting national self determination and also by its own working class, as it was transformed during the development of the very industrial processes by which that circle sustained its wealth, from a class in itself to a class for itself. The unfolding of these forces that pushed up against the ruling elite created ripples in the society that lapped against the confidence and security of other classes. For as Crankshaw notes:

"Its obvious decline affected very closely the mood of the middle classes, who for so long had identified the court and the great families with the Empire itself; this alone would have been enough to induce in the thoughtful a mood of uncertainty and self questioning. Uncertainty all too easily turned into defeatism in face of apparently insoluble problems posed by the nationalities and their drive for self expression." (Crankshaw 1970: 325)

Accompanying this defeatism in politics went a desperation in the arts', a last thrust which injected life into a dying capital. Nevertheless the apparition of cultural vitality could not banish the harsh political realities that were soon to consume Vienna. The changing order of relationships within the empire which were beginning to resonate as waves of alarm for the ruling elite

refracted as hope for the increasingly determined organisations of labour. But the unfolding processes pushing the proletariat forward also contained the forces that would push it back.

Living as a member of Vienna's populace, just before the First World War, was the very individual who was later to embody those reactionary forces. Hitler's own account of his time in Vienna is instructive, particularly as regards the condition of the working classes:

"Even more dismal in those days were the housing conditions. The misery in which the Viennese day labourer lived was frightful to behold. Even today it fills me with horror when I think of those wretched caverns, the lodging houses and tenements, sordid scenes of garbage, repulsive filth and worse." (Hitler 1973: 26, 27)

Such housing conditions existed despite the 'municipal socialism' of Karl Leuger and the Christian Socialist Party, Leuger's policies never extended as far as slum clearance, but remained at the level of public works.<sup>1</sup> It was only later in the 1920's with the enthusiasm of the social democrats for such matters that housing conditions dramatically improved. It was against this backcloth of poverty that the Social Democrats made rapid advances among the working class and eventually became its main channel of political expression. However, although the working class were emerging as a serious political force before the First World War, it was the war itself which gave the necessary dynamic twist to its development by the injection of militancy and the presentation of political opportunity. For, not only did the war compound the depravation of the working class with the increased militarisation of the economy to meet the needs of the unforeseen length of the war, but the appointment of military overseers in industry produced conditions of primitive servitude and summary

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1 See Johnston 1972:65. He considers that Karl Leugers Municipal Socialism should not be compared to that of Joseph Chamberlains in Birmingham for the very fact that Leuger's party made no effort at slum clearance.

punishment became commonplace.<sup>1</sup> This increased hardship, and depravation transformed itself into anger and disgust among the proletariat, especially following the military defeats that punished Austria during the closing stages of the war when the hard pressed German military machine could no longer offer its assistance as it had so often done in the past.<sup>2</sup> With the Italian offensive and the breakthrough in the Bulgarian sector, the front began to collapse, taking the Empire with it. The injection of revolutionary radicalism into the socialist movement came with the returning soldiers, many of whom still possessed their arms. As Otto Bauer states: "The social revolution which arose out of the war proceeded from the barracks rather than from the factories" (Bauer 1925: 56).

With the final secession of the various non-German territories of the Empire and the ensuing collapse of the monarchy the position of the Austrian working class was drastically altered. Instead of a struggling movement they became the inheritors of a revolutionary situation. The disappearance of the monarchist structure meant that practically overnight the old ruling elite of Archdukes, ministers and courtiers became redundant. As Stadler (1971: 81) writes: "The Habsburg Empire went to its doom, leaving behind no constructive idea, but only memories and unsatisfied longings". So far as the monarchists were concerned, their world was fractured beyond

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- 1 See Gulick 1948 Vol 1 : 36-37 for the description of the militarisation of the society, particularly for the harshness of conditions in the key war industries. Being drafted to the 'front' was a not uncommon response to industrial disobedience.
  - 2 See Gulick 1948 Vol 1 : 43 for this point and for the military defeat of the Austrian Army.

redemption, and the bourgeoisie, although not broken, were, amid the economic destruction accompanying defeat in war, a class stripped of power and confidence. The real power had been transferred, and resided in the hands of the workers and soldiers councils. Despite the fact that many of these soldiers had been influenced by revolutionary mood, the actual founding and proclamation of the Republic was bloodless:

"In contrast with the classical examples of revolution, particularly the French and the Russian, it might well be argued that the events of the closing months of 1918 in what became the Republic of Austria do not deserve to be styled a revolution. There were no barricades, no bastille and practically no violent outbreaks of fury or acts of vengeance. The old regime vanished, wounded to death on the battlefield and deserted by the non-German peoples of the Empire." (Gulick 1948 Vol 1: 48).

Whilst the term 'revolution' has lost some of the precision it once possessed, the nature of the change that occurred in Austria with the founding of the Republic cannot, without being unduly inaccurate, be characterised as revolutionary. The Monarchy was the victim of a collapse rather than an actual overthrow, and the transference of formal political power was aided through the collaboration of the Lammasch government<sup>1</sup> with the new States Council. Yet the changes in the political structuring were fundamental, sudden, and drastic enough to warrant the title of 'politically revolutionary'. However, the situation with regard to the underlying social structure is not quite so clear cut. For, despite the fact that many far reaching social reforms were enacted the private capitalist ordering of the economy emerged from the threatening transition period unscathed, with property relationships fundamentally unaltered.

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<sup>1</sup> The last government of the Austro Monarchy. See Gulick 1948 Vol 1 48-51 for detailed account of this transference. Political Power.

"... for all the substantial administrative and constitutional changes it wrought, it certainly did not transform the established order. Notwithstanding its social reforms, it did not interfere with property relationships or control over the means of production." (Leser 1966: 125-126)

Legislation was passed to bring the largest feudal estates under public control, but its impact was small. In addition, several state-owned corporations which had been for the most part orientated to the war effort were brought under a new form of management (*Gemeinwirtschaftliche Anstalten*) whose aim was to rescue those industries facing difficulties over transition to a peace time economy. These new organisations, run jointly by the workers in conjunction with the state and replacing the old imperial bureaucratic administration left industry, as did the nationalisation laws of 1919, basically in private hands. The failure of a thorough-going nationalisation scheme, especially the failure to nationalise the important Österreichischer Alpine -Montanggesellschaft, had far-reaching repercussions and according to Leser (1966: 126) was "destined to seal the fate of the Republic". The Alpine -Montanggesellschaft was Austria's most important heavy industrial combine and a rabid opponent of labour from which the Christian Socialists launched the para military Heimwehr in their final assault on the Republic. However, it should be pointed out that the socialists' failure to nationalise the Alpine-Montanggesellschaft did not result from an oversight or even a lack of socialist zeal, but was due to the failure of the socialisation commission which, under the direction of Professor Emil Lederer, and with the connivance of Professor Schumpeter, let the combine fall into the hands of Italian shareholders. From that point on there was no possibility

of nationalisation without direct allied intervention occurring, and since the Republic was not in a strong enough position to face such an eventuality the socialisation plans had to be dropped.<sup>1</sup> The Social Democrats' failure to nationalise Austria's most important banks which, in the main, controlled all the crucial industries, led Trosky to deride them as "the guardian angels protecting the Vienna Kreditanstalt"<sup>2</sup> (Trosky 1965: Vol III: 913). In short, notwithstanding all the reforms that the socialists did succeed in making, and the movements that did occur in certain sectors by way of change in economic relationships, the programme cannot accurately be described as 'revolutionary'. On the contrary, it can be argued that the actions and reforms made by the Austrian social democratic party had the directly opposite effect. The social democrats' radical rhetoric coupled with their promise of socialism was sufficient to command the support of elements who might otherwise have gravitated towards more overtly revolutionary organisations such as the Austrian communist party.

But an important fact so far as the situation at that time is concerned, and one that must not be overlooked, is that:

"Whatever we may think in retrospect of the character and achievements of the Austrian revolution of 1918, to contemporaries it seemed a genuinely revolutionary situation with all the opportunities and dangers this implied." (Stadler 1968: 93)

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1 See Bauer 1948: 155-157 for a detailed discussion of this 'failure'.

2 One of the most important banks in Austria.

In conversation with Paul Lazarsfeld, for example, he referred to the period just after the war as "the failed revolution" (Lazarsfeld 25.5.73), a factor of no small importance when discussing the creation of the Forschungsstelle. Hence, although the Austrian case does not meet the exacting requirements of a carefully constructed sociological model of 'revolution', there is little doubt that many of those caught up in the turmoil of the times believed they were living through a revolution, or at least a near revolution.

Austria in Ferment : The Treaty of St. Germain

Before describing the Austrian Social Democratic Party and the meaning that it had for its members, it is first necessary to sketch the situation existing within the country after the collapse of the Empire, in order to understand the situation and problems that the socialists faced. Stadler writes:

"The conservative camp was divided between the Habsburg traditionalists and the Deutschen fanatics; the socialist camp dreamt of becoming part of the great German labour movement; neither had a plan or much hope for the small and independent republican state which they were to inherit."  
(1971: 81)

Thus at the psychological level of identification with the new state, problems existed. For, although it was the determination of the nationalists and the political strategy of the allies which had brought the secession states into being, no such determination was present on the part of the German Austrians for their own newly established state. It had been forced upon them by the victor nations of the war and consisted of, to use Clemenceau's words, "that which was left over after the break up".<sup>1</sup> Austrians had neither worked, fought, nor wished for the creation of the new state. It was:

"Provisorium and Transitorium and not the culmination of their hopes; hence there was no patriotism, no identification, no feeling of permanence." (Stadler: 1971: 106)

Therefore, the new republic was confronted with not only the organisational problems of a ruined war-time economy, but was also faced with the overwhelming task of nation building; a difficult enough task for an emerging nation which at least has the comfort of ascendancy as a

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<sup>1</sup> See Stadler 1971: 106 for this quote and for a discussion of the problems attendant upon the Treaty of St. Germain.

propelling dynamic, but Austria had no such reassuring drive. After all, the new Republic was the collapsed rubble of a once great Empire, and as Ernst Fischer writes in commenting upon his harrowing return from the front and the situation facing the country:

"When the end did come and the new social order installed no-one believed in the new state's viability. Everyone regarded it as an interim measure." (1974: 76)

Social and economic disruption produced by the shattering consequences of a lost war produced a state of uncertainty, with the monarchists casting glances back to a glittering past, and filled with contempt for the 'social rubble' that had replaced the old ruling court circle, the pan-Germans wishing for their rightful home among the German Volk, and the social democrats looking for their socialist ambitions to be fulfilled amongst the powerful German labour movement. All these forces worked together to encourage a lack of firm belief in the future of the new state. This lack of commitment produced a stark provincialism. As McCartney writes:

"For stranger than any loyalty to this new and strange republic was the local patriotism which their distinct history, dialect and mixture of blood had engendered among the inhabitants of each province. When the unifying power of the Habsburgs had gone, every man felt himself a citizen of his province: a Styrean, a Carinthian or a Viennese."  
(1926: 94)

The problems of engaging the loyalties of the citizens to form a nation state was compounded, not only by the competing expectations that various groups had, but also by the sheer bankruptcy and poverty of the country. For the Treaty of St. Germain which was imposed by the victors in a somewhat similar spirit to the imposition of the Treaty of Versailles on the Germans, reduced the great Habsburg empire, or rather ratified the reduction of it, from that of fifty million people to a mere six

and a half million, of which two and a half million resided in Vienna.<sup>1</sup> The treaty not only tore away forty three and a half million people from the Empire, but in doing so robbed it of its economic viability, a fact which was to have disastrous consequences for the future possible success of the Social Democrats' programmes. Dr. Karl Renner,<sup>2</sup> a social democrat and the first chancellor of the new republic, protested to the allies that:

"The German Austrian peoples, as a result of the conditions to which they were subjected, would be robbed of the indispensable means for the maintenance of their economic life and their political and civil order. German Austria would be deprived of its richest and most fertile districts and over four million out of ten million German Austrians would be subjected foreign rule. The old balance between the industrial and the agricultural districts of the state would be destroyed." (Bullock 1939: 68-69)

The situation facing the new state was desperate. It could only produce one quarter of the foodstuff needed for its population, and the need to import the balance left the Republic in the position of constantly having to appease the Entente powers. In addition, the treaty expropriated the coalfields and major export industries and cut across the economically indispensable system of canals and irrigation channels leaving the country in an unviable economic position. The resulting distress in Vienna was appalling. Only twenty seven percent of coal needed for the country was available; translated into personal terms it meant that only one third of households in Vienna

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1 See Bullock 1939: 68 for these figures and a discussion of the consequences of such a drastic reduction in the population.

2 See Kann 1951: 243-249 for a brief discussion of Karl Renner.

were receiving a weekly allowance of coal. This, coupled with the harshness of Austrian winters and the reduction in 1919 of the meat and bread ration to three and a half ounces per head per week, meant unparalleled misery for the bulk of the population. For example, the average weight of children of the same age group and in the same schools when compared between the dates 1913-1919 showed a reduction of two thirds. These figures, which are taken from the Austrian Chancellor's plea to the Supreme Council of the Reparations Commission in Paris,<sup>1</sup> are followed by the warning that unless more help is forthcoming to Austria then:

"If this mission fails and if it were impossible to feed the people, the coalition government, which was chosen by a democratic parliamentary election with five sixths of the population behind it, would not remain in office. Only a Bolshevik adventurer would dream of assuming power in succession to it." (Bullock 1939: 79-80)

Even allowing for Renner's special pleading, the seriousness of the situation is evident from other sources. For example, in November of 1919 such was the plight of Austria, that in Germany, itself not in a particularly prosperous condition, the government ordered the reduction of ten grams from each bread card and sent the amount saved to relieve Austria.<sup>2</sup>

What was left after the Treaty of St. Germain was not only the poorest part of the once prosperous empire, but also the most expensive to run and administer. There remained expensive industries, a highly developed railway system and an enormous administrative apparatus. The crux of the situation was that Vienna had developed as the central part of a great empire, but its structure had never been intended or designed to stand alone; its viability depended upon the vast

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1 See Bullock 1939: 75-79 for these facts and figures and also for the Supreme Council sending food to offset the possibility of a 'Bolshevick' uprising.

2 The amount realised was 1,968 tons of bread per week. See Bullock 1939: 76-77.

resources of the Empire which were now denied it. The Austrian civil service had always been an expansive administrative machine, due largely to the amount of manpower required to cope with the complexity of the Empire's problems. Now this abundance of manpower had become a burden that the state could ill afford to carry. Thus a bureaucracy that had been disproportionately large during the hey-day of the Empire now "administered the affairs of six million, of whom they themselves formed no mean proportion" (McCartney 1926: 87). As Fischer bitingly recalls, Vienna was a city "swollen by pension hungry officers and officials from all over the former Empire " (Fischer 1974: 76).

The problem of surplus labour and the economic disruption which bore heavily upon the new republic was compounded by the problems of health administration among a population severely weakened by lack of food and heating, and consequently an easy prey to all manner of sickness and disease. Even at the best of times the imperial city could not be described as the healthiest of places. It had the highest death rate from tuberculosis of any city in Europe, for example. However, between the years 1918-1923 the general mortality rate rose by sixty percent and the child mortality rate by a hundred percent.<sup>1</sup> The poor overall health of the populace was one of the factors behind Otto Bauer's decision to reduce the working day to eight hours.<sup>2</sup> At the same time, however, it must be noted that the promise of an eight hour day had always been part of the social democrats' platform, and in addition due to the lack of fuel the factories could not be run at capacity.

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1 See Bullock (1939: 110-111) for these figures and the measures that the Social Democrats adopted to overcome such problems. Johnston (1972: 73) mentions that in the early twenties influenza epidemics killed thousands due to the poor condition of the inhabitants. Of incidental interest is the fact that Freud's daughter, Sophie, was a victim of the epidemic.

2 Otto Bauer (1925: 134-135) in his history of the Austrian revolution gives this account.

### The Social Democrats as Inheritors

Such was the imperial legacy that the Austrian Social Democrats had to work with, and it may be added, against. The poverty, the vacuum of power, the radicalisation of the working class, and to some extent the peasantry, undoubtedly offered favourable conditions for a left revolutionary party to build up its membership and extend its influence. The old order had not simply fractured to reveal glimpses of a possible future order, but exploded wide open so that new possibilities were clearly visible to the rank and file.<sup>1</sup> However, the leadership of the party, distanced by their intellectual training from the immediacy of events, perceived the dangers of possible failure and drew back from testing the opportunities presented. As Bauer says:

"Large sections of the proletariat did not realise these dangers. It was the duty of Social Democracy to see them. Thus a double task devolved upon social democracy; on the one hand, by taking advantage of the powerful revolutionary agitation among the masses and the severe shocks which the capitalist social order had suffered, to capture for the proletariat the strongest and most permanent positions in the state and in the workshops, in the barracks and in the schools; but on the other hand, to prevent this revolutionary agitation from developing into civil war and open collision with the superior forces of Entente imperialism, which would have opened the gates of famine, invasion, and counter revolution." (Bauer 1925: 91-92)

Nevertheless, the fact remains that by only 'capturing for the proletariat the strongest and most permanent positions in the state' and not actually capturing the state itself, the Austrian social democrats dug their own grave. Even if Bauer's analysis of the situation was correct and defeat

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<sup>1</sup> See Bauer (1925: 98) for the opportunities presented and the recognition of the revolutionary situation by the mass of the party's followers.

would have followed from an attempt to push through the revolution, the decision to draw back merely postponed the defeat - it did not prevent it; only the executioner and the time of the execution was changed. Nevertheless in the interim period social changes did take place that would not otherwise have occurred.

The Social Democratic Party of Austria began officially in 1889 at the small village of Hainfeld in lower Austria. The late arrival of a working class party was due largely to the splintering of the class into moderate and radical factions which had hitherto prevented a unified development. The unification of the various factions was mainly accomplished by Victor Adler with the aid of his weekly paper Gleichheit in which he made constant appeals for reproachment of the left. However, if the early factional struggles which hindered the development of the labour movement were overcome by appeals to unity, then this very unity later produced its own contradictions. The fetish of party unity, the idea that the party must be kept together at all costs, obscured rather than resolved the underlying issues. Not for the Austrian Social Democratic Party the painfulness of Leninist splits, but rather the exercise of a "permanently synthesising influence on polarised elements of Marxist thought" (Leser: 1960: 121). In fact the ghosts of old factional battles were never fully exorcised, and they always hovered over the party. Lazarsfeld in conversation with the writer summed up the party thus:

"It never took a stand on the work system ... the party was more important than what it did. And if you look back at the history of the Austrian party the great idea was the unity of the party .. but this unity was bought, as one sees now in retrospect, by the complete ineffectuality. The party became bigger and bigger, and everyone lived in it, but it had no influence on political events."

(Lazarsfeld 25:5:73)

The work that Victor Adler had done in unifying the left to form the Social Democratic Party was taken up by Otto Bauer, who struggled to keep the party intact when he became its unchallenged leader. He was the only one, according to Buttinger, who "even before the first world war could stand up to Kautsky, Plekhanov, Mehring and Rosa Luxemburg in international socialist discussions" (Buttinger 1953: 170). He became the foremost speaker, parliamentarian and theoretician in a party that boasted men as talented as Karl Renner, an authority on government, constitutional law and administration; Max Adler, 'an original interpreter of Marxian dialectics'; Julius Deutsch who wrote a fundamental study of the Austrian trade union movement, and Rudolph Hilferding who extended Marxist economic thought.<sup>1</sup> However, it was Bauer's comprehensive knowledge which gave him the commanding intellectual authority that allowed him to stand out amongst so many other able intellects. Bauer's influence drove deep into the party, and even in exile, after the debacle of the failed 1934 uprising, his authority was never seriously challenged. Braunthal, who became a leading figure in the party himself, writes of Bauer:

"When I listened to him for the first time -- in awe and admiration of course -- I felt at once that from now on my life was bound to his and that there was no escape for me from the domination of his genius." (Braunthal 1945: 73)

The point about Austro-marxism was that it was a product of a specific historical situation and encompassed an amazing mixture of hybrids. It contained elements of enthusiastic Messianism combined with sober Fabianism, especially after 1903 with the founding of the Fabian type

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1 See Leser 1966: 117 for the intellectual contributions of these individuals.

society Zukunft, which according to Leser, "exerted a lasting and widening influence on the entire structure and performance of Austrian socialism" (Leser 1960: 117-118). To complicate matters further it managed to combine revolutionary Marxism with reformist trade unionism.<sup>1</sup> These cross-currents of thought and competing points of reference, when added to the sturdy pragmatism of Bauer himself, ensured that the party tripped and stumbled with each fresh turn of events; yet always waiting with deterministic insistence for the right 'objective factors' to pronounce the 'real course of history'. Thus it is with some accuracy that the sympathetic biographer of Dollfuss can write:

"They were even in those days among the oddest and most unsatisfactory of Lenin's disciples to be found anywhere on the continent." (Shepherd 1961: 29)

#### The Party in Action: Test and Triumph

The first real test for the social democratic party came not from the right but from within its own orbit of influence; that is, from the revolutionary enthusiasm of the workers and soldiers. The task of the party was to curb this ardour, while retaining the support of its adherents. The danger was that its followers might well transfer their support to the ranks of the communists who were certainly in no mood to form a coalition with the beaten bourgeoisie as the Social Democrats did.

In the period immediately after the collapse of the empire, effective power rested not with the party executive but with the armed soldiers. No army lay at the disposal of the Republic; all

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<sup>1</sup> See Brauntal 1945: 17.

that did exist were twenty thousand men of the Volkswehr (People's Army) which had been hastily recruited by Julius Deutsch, the Under Secretary for the Forces, and later the creator of the Republikanische Schutzbund.<sup>1</sup> The problem was that the Volkswehr itself was extremely unruly and had been created not only to protect the Republic, but also to direct the energies of the volatile returning soldiers along paths more conducive to the party. Even so the Volkswehr was militant, especially the forty first battalion known as the Red Guard, which according to Braunthal<sup>2</sup> had already visited his office in the ministry for the forces on the eleventh of November, the day before the proclamation of the republic by the provisional government, and insisted upon the creation of a Socialist Republic. Braunthal managed to explain that 'conditions were not right' for such a move, yet next day when Karl Seitz, the President, announced from the steps of the parliament that 'Austria is a Republic' the Red Guard ripped the white parts from the new republican flag, hoisted the remaining red sections, and proceeded to fire on the parliament buildings. As Braunthal records:

"This day which was meant to assume a historical significance thus ended with a shrill dissonance. But the 'incident', though engineered by a handful of romantic revolutionaries, undoubtedly manifested the prevailing mood of the working people and soldiers."  
(Braunthal 1945: 223)

Although the mood and atmosphere was one of revolution, the elections that were held in February 1919 under a system of proportional representation showed the 'formal' distribution of power as follows:

Social Democrats	72 seats
Christian Socials	69 "
German Nationalists	26 "

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1 Defence League of the Social Democratic Party.

2 See Braunthal for a very good first-hand account. Braunthal 1945: 222.

Thus the Social Democrats, unable to obtain an absolute majority, combined with their inveterate enemies the Christian Socialists to form a coalition government that lasted until 1920. The Social Democrats, however, controlled Vienna, a condition which gave the rank and file supporters a false sense of their own power since the slogan 'Von Roten Wien zum Roten Österreich' never became a reality. Red Vienna never became Red Austria.

Despite the Social Democrat's agricultural programme of 1925<sup>1</sup> the countryside still presented problems. As Bauer put it in his discussion of the Tyrolean peasant:

"During the war the Tyrolean peasant had learned to hate the militarism, and they were the first to be infected by the Republican idea. On the 11th of November, the Tyrolese National Council demanded the proclamation of the Republic. But the fact that the Republic in Vienna was beginning to assume a proletarian character, was not at all to its taste." (Bauer 1925: 67-68)

The driving force behind the peasants' republicanism was their hatred of the requisitioning system employed during the war, and it was this hatred rather than any commitment to socialist beliefs that drove them into the republican camp. Thus, as Bauer further comments:

"The revolution was bound to disappoint his expectations. At the time of the direst need, it could not dispense with the centralised system of requisitioning and distribution of food. The feeding of the towns and the industrial centres, above all the feeding of Vienna, could not have been effected without state regulations and control. The peasant saw the revolution denied him what he understood by freedom." (Bauer 1925: 88)

The fracturing of the peasants' world through military intervention in the distribution of his labour products, and the uprooting of many

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<sup>1</sup> See Gulick 1948 Vol II: 1380-1382 for the agricultural programme and its deviation from 'Marxist Orthodoxy'.

peasants to fight in the war, which gave them their first contact with alternative realities, especially in soldiers' councils, momentarily ruptured their organic conservatism. However, when they returned to village life, disillusioned with the directional shift of the Republic, and once more under the influence of the Catholic Church, revolutionary spirit melted away. The peasants' hostility towards the proletariat who they had to feed found encouragement not only from the Church, but also from the urban trading class who shared the peasants' dislike of the centralised food distribution system. This alliance between the petit and middle bourgeoisie and the rural peasantry coalesced to form the backbone of the opposition to the Social Democrats, and was eventually transformed into a genuinely reactionary movement which finally defeated the Republic.

Socialist Vienna, however, was swept along on tides of revolutionary fervour yet always cautious to remain within the bounds of reforms acceptable to the entente powers for, as McCartney notes, "as statesmen they could do nothing; as socialists only so much as would not call down a stoppage of supplies" (1925: 103). In fact Bauer writes bitterly of the period and the limitations of circumstances:

"Immediately after the armistice, the Austrian government had addressed a petition to Wilson, to facilitate the import of foreign foodstuff into the starving country. On the 24th of November, Wilson's answer reached us. It promised the import of foodstuffs upon one condition: that 'peace and order' were maintained. Wilson's note of the 18th of October had unchained the national revolution. Wilson's note of the 24th November demanded the closure of the social revolution. The Western powers now confronted the proletarian revolution in Austria as the protectors of bourgeoisie peace and order." (Bauer 1925: 80)

Although the entente laid the responsibility of maintaining 'peace and order' on the Social Democrats' doorstep the question was not that simple, for the issue of power still had to be resolved. Associations

of ex-soldiers, for example, not waiting for official housing, took matters into their own hands and appropriated land and property on which to build their own houses. Land settlement colonies sprang up everywhere, even government officials and artists followed the example set by the soldiers. By 1922 forty five colonies were in existence. The spectre of further land seizures alarmed the government to such an extent that they made land and materials available in an effort to bring the situation under control. However, it must be pointed out that it was one of the great achievements of the Social Democratic Party that they addressed themselves so successfully to the question of housing. Before the war, out of 555,000 dwellings in Vienna, one half consisted of apartments of two rooms only, a situation which worsened during the course of the war since no house building took place at all. The Social Democrats attempted to rectify the situation through massive municipal spending and by 1934 they owned nearly half the city.<sup>1</sup> The most famous part of the building programme, and the pride of the Social Democrats, were the enormous fortress-like tenement blocks such as the Goethe-Hof, Engels-Hof and perhaps the most famous, the Karl Marx-Hof where Paul Lazarsfeld and Marie Jahoda lived after they were married. So great was the building programme that Johnston accuses the Social Democrats of 'gerrymandering'. He comments that:

"Apartment complexes, whose residents had to belong to the Social Democratic Party dotted previously conservative districts guaranteeing a socialist majority for years."  
(Johnston 1972: 75)

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1 See Bullock 1939: 108-110 for a discussion of tenement associations and overall housing achievements of the Social Democrat's building programme.

Such an accusation is a mistake. The socialist majority was ensured not by 'gerrymandering' but by the overwhelming support that the party had in Vienna. In addition, it is also not quite true that one had to be a party member to live in the complexes, although it did undoubtedly 'help'.<sup>1</sup> The municipal building programme enacted by the Social Democrats stemmed from their 'practical' socialism and deep-seated desire to raise the 'humanity' of the working classes through an improved environment. Such was the impact of the building programme, the visible achievement of a socialist administration, that Shepherd caustically remarks that:

"Breitner's<sup>2</sup> Vienna tenement houses and Bauer's universal pipe dreams were socialism in the end; there was nothing in between." (Shepherd 1961: 30)

Whilst the tenement blocks were an important part of the Social Democrats' programme it would be wrong to see them together with Bauer's universal pipe dreams, as representing the sum total of Austrian socialism - it was much wider and more persuasive than that.

During the Republic's early days, however, when President Woodrow Wilson had given the socialists the responsibility for maintaining peace and order, such massive benefits as housing complexes could not be produced overnight. The communist party, although numerically small, was both noisy and vigorous, constantly pushing for the dictatorship of the proletariat, and finding a certain amount of response in the revolutionary atmosphere of the times. Furthermore, they met the Social Democrats on equal footing in the workers' and soldiers' councils and fed the groundswell of

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1 Interview with Dr. Schilder - an old Social Democrat. Vienna 28 June 1974.

2 The municipal treasurer's.

discontent which resulted in several bloody clashes with the government forces. Emboldened by the proclamation of the Bavarian Soviet on April 18 1919 the communists organised an assault upon the parliament buildings which resulted in loss of life. After the collapse of the first attempt to seize power, Bela Kun sent Ernst Bettelheim to Vienna with instructions to succeed where others had failed. A much more serious situation thus faced the Republic, since the only real force at its disposal with which to maintain 'law and order' was itself pervaded by revolutionary ideas. However, the failure of the uprising which resulted in twenty dead and eighty wounded, was sealed by the ineffectuality of the communist propaganda amongst the Volkswehr.<sup>1</sup> Indeed, it was the Volkswehr bullets that put down the insurgents. So long as Bela Kun remained in power in Hungary the communists within the Austrian Republic derived both sustenance and hope. However, with the fall of the Hungarian Republic and the crushing of the Munich soviet the Austrian communists' chance of fermenting revolution collapsed. In addition, the spectre of counter revolutionary violence produced a salutary effect among the left revolutionary romantics pushing them back into the folds of social democratic influence.<sup>2</sup> One of the factors militating against the type of internecine fighting between the left forces that had swept Germany in 1918 was the existence of the workers' councils. Although they harboured some of the more revolutionary elements in the left spectrum, and allowed a wider platform to the communists than they would other-

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1 See Braunthal 1945: 232 for a good account of the attempted seizure of power and the loyalty of the Volkswehr.

2 See Bullock 1938: 78 for extension of this point.

wise have had, the councils had the general effect of shifting the struggle off the streets and into more institutionalised forms. Much of the credit for harnessing the otherwise disruptive forces within the workers' councils through the creation of a national organisation of councils which provided a common forum of the left in general must go to Friedrich Adler.

Friedrich Adler, the son of Victor Adler the party's founder, had gained great prestige and credibility among the working class as a result of his assassination of the Prime Minister, Count Sturgkh, on the 24th October 1916, whom he held accountable for much of the slaughter during the war. Adler's act of assassination had deeper symbolic significance, however, as Bauer points out when he describes it as:

"The turning point in the history of the labour movement. To the masses, who had lived in hopeless and inactive despair, he became a hero who had offered his life to avenge their suffering." (Bauer 1925: 30)

Thus it was not only Adler's achievement of organising the councils on a national scale, but also his standing with their members that safeguarded the Social Democrats' policy of moderation. However, Adler not only had an influence upon the 'revolutionary' situation of the times; he also had a great deal of impact upon the young Paul Lazarsfeld.

In fact, the relationship between Friedrich Adler and Paul Lazarsfeld epitomised the atmosphere within which Lazarsfeld grew up in Vienna, with its peculiar combination of political dedication and academic scholarship.

Adler, during his student days had been a friend of Albert Einstein and in 1908 competed against him for a professorship in physics at the University of Zurich. Although it would seem that Adler would have been appointed, due to the socialist leanings of the Zurich Canton's Education Board, he was not; for in a remarkably generous speech to the Board, Adler informed them that, "If it is possible to obtain a man like Einstein for our university, it would be absurd to appoint me. I must quite frankly say that my ability as a research physicist does not bear even the slightest comparison to Einstein's" (Florence 1971: 44-45). Although Adler never entirely neglected his interest in physics he nevertheless turned increasingly to his main interest in life: politics.

Some time after 1915 Adler became Lazarsfeld's mother's lover, and during the course of the war he became a frequent visitor to the Lazarsfeld household. His opposition to the war not only increasingly alienated him both from the Social Democratic Party, but also from his own family. Hence:

"In time Fritz grew close to Sophie's Lazarsfeld children, fourteen year old Paul and his younger sister, Elizabeth. He would play games with them, discuss their schoolwork, take them to concerts or theatre - always with a gentleness that told of his hunger for family." (Florence 1971: 148)

Whilst Adler was in prison for seven months awaiting trial for his assassination of the Prime Minister, Sophie and her family became "Fritz's link with the outside world .. He began writing to Sophie and her children the first week" (Florence 1971: 197). Professor Lazarsfeld described the importance and meaning this correspondence had for him:

"Well here you know you have a rather glorious hero. Adler corresponded with me. I have a letter ... I reported to Adler my progress in school and I have a letter from Adler in 1916. I was 15 ... 'Dear Paul, I'm glad to hear that you are doing well in mathematics. Whatever you will do later mathematics will always be useful to you'. You see that undoubtedly is of considerable interest if a glorious murderer wrote it to you from jail to stick at doing mathematics." (Lazarsfeld 25:5:73)

As important as the assassination itself was the trial which followed, which Adler, in the course of a six hour speech, turned from a murder trial into a trial of both the government and the Social Democratic Party for their support of the war. The courtroom was crowded and "In the last row of the gallery Paul Lazarsfeld sat with his friends, admirers of Fritz Adler who sympathised with the programme of the old Karl Marx Association.<sup>1</sup> This trial was the biggest event of the war for them" (Florence 1971: 218). After Adler had been sentenced, a demonstration of popular sympathy erupted in the court and carried on out into the streets. Paul Lazarsfeld was among the fourteen demonstrators who were arrested.

Following the sentence, Adler was taken from prison in Vienna and committed to the fortress of Stein some forty miles up the Danube. Paul Lazarsfeld and his mother still kept a close contact and Adler, who was now cut off from the world of politics, resumed his work in physics, and in particular his work on Einstein's 'Theory of Relativity'. The work was completed in September 1918 and Paul Lazarsfeld typed it for him. According to Florence:

"Fritz was relatively happy in prison .. each week he was allowed two visitors, and the regular visits of Victor [father] and Emma [mother] or of Sophie and her children were the perfect interludes in his studies. With Paul and Elizabeth, Fritz would discuss their school work or books they had read. He convinced Paul that mathematics was a good foundation for any future studies. In turn Paul helped Fritz by typing the smuggled manuscripts." (Florence 1971:278)

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<sup>1</sup> The Karl Marx Association in Vienna was organised by Friedrich Adler. It had always taken an internationalist anti-war stance and held lectures and discussion groups in support of such a position.

I will return to Lazarsfeld's interest in mathematics in the later discussion of the Forschungsstelle. However, for the moment it is more important to understand the socialist movement and Lazarsfeld's involvement in it. The revolutionary momentum within the workers' councils gradually declined, especially after the failure of the Hungarian and Munich experiments, and as the new republican government slowly began to establish itself the councils were brought under the aegis of the trade unions, thereby ensuring undisputed control of the party. In fact, as conditions began to stabilise the councils lost most of their functions and withered away. The lost possibilities of that revolutionary situation are superbly captured in Victor Serge's memoirs. He describes the frustration experienced by the many wanting but not daring to make the psychological and physical leap to revolution when he writes:

"If only ... if only a Red Austria had joined with the Hungarian Soviet, would not troubled Bohemia, and then Germany have followed their example? Revolution was maturing in Italy during this period. But perhaps it was already too late. If only, after 1918 ... If only the commission on the nationalisation of the main industries, established by the socialist government, had not been such a farce! If only the social democrats of Austria had had a little of the impassioned energy of the Bolsheviks of Russia...! Its opportunities lost, its hours of daring past, little Austria found herself jammed in the middle of the expanding counter-revolution of Hungary, Italy and Germany; at home Socialist Vienna found itself menaced by the countryside and the Catholic Bourgeoisie. Prince Starhenberg was recruiting his peasant bands against it. I attended meetings of the Social Democratic party activists; they were middle-aged men, few of them fit, who drank their beer as they listened to the speakers. The Schutzbund would march past the town hall with 30,000 bicycles garlanded with flowers! Otto Bauer, who was greeted on all sides by affectionate glances, watched the parade of this working class force, so confident, so deserving of a glorious future. If only it had been a matter of just deserving."

(Serge 1963: 189)

Serge was very much caught up in the Viennese situation, in fact he adopted the pen name Victor Serge for the first time in an article he wrote defending Friedrich Adler. However, he is wrong in describing

Austria as the kingpin of the European revolution; that place was reserved for Germany. The failure of the German revolution seriously undermined the possibility of European revolution in general and revolution in Austria in particular. After the failure of revolution in Germany, Bauer:

"Considered that the future of Austro-Marxism depended upon the maintenance of the spirit of revolutionary fervour in the party on abstract questions of socialist doctrine, combined with a moderation of policy on practical issues." (McDonald 1946: 66)

One outcome of this policy was that in 1920 the party withdrew from the coalition with the Christian Socialists. This withdrawal was prompted in part by doctrinal purity over collaboration with the class enemy and in part by fears that a further extension

"of the coalition might produce disaffection among its rank and file supporters since the majority of the working class were disenchanted with operating the machinery of a bourgeois government." (Leser 1960: 128)

As a result of this move the key posts in the army, the police and the ministries were re-occupied by the trusted supporters of the resurgent bourgeoisie. However the working class still held Vienna, but the surrendering of the machinery of government provided the toe-hold that the forces of reaction needed to muster their forces and strike back at the socialists. The price of purity and party unity was high indeed, as the events of 1927 were to show. These were to break the back of the party and offer a foretaste of the socialists' final fate.

There was nothing particularly unusual about the clash that took place in the tiny village of Schattendorf close to the Hungarian border, since armed clashes were common, especially between the right wing

Heimwehr and the Schutzbund. In this instance, however, the clash was between Frontkämpfervereinigung<sup>1</sup> and the Schutzbund. The Frontkampf was particularly disliked by the Republicans for its disloyalty in wishing to join with Hungary. The Burgenland, within which Schattendorf lay, was a centre of unrest, having only belatedly been joined with the Republic and, according to Gulick, clashes between the Frontkampf and the Schutzbund "rapidly became a substitute for Sunday entertainment" (Gulick Vol I: 1948: 728). On January 30 during the course of a noisy 'demonstration' the Frontkampf fired into the ranks of the Social Democrat supporters and according to Gulick killed a child and a war veteran by the name of Csmarits, although Jedlicka gives the number as several killed.<sup>2</sup> No matter, the main point is not so much the killings themselves, but rather the storm which developed after the court case. Although the judge noted that the moral responsibility for the incident rested with the Schutzbund, he urged the jury to pronounce the accused guilty before the law. The jury, however, acquitted the accused Frontkampf members.<sup>3</sup> This verdict presented the Social Democrat leaders with the problem of having to decide what action to take amidst the mounting anger of their own party members.

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1 The Heimwehr was a para-military organisation of the right, as was the Frontkämpfervereinigung. For a discussion of both see Jedlicka 1966.

2 For the numbers reported killed see Gulick 1948: Vol I: 728 and Jedlicka 1966: 123.

3 See Gulick 1948, Vol I: 731 for a detailed discussion of the jury's verdict.

"This problem of the occasional failure of the jury system was the chief difficulty faced by the Socialist leaders in the hot summer night of July 14; they knew that the reform, indeed the abolition of jury courts was one of the greatest wishes of the reactionaries, a part of a wider programme to curtail democracy. Was it possible under the circumstances to call a major demonstration of protest against the verdict of a jury, a demonstration against democracy?" (Gulick 1948 Vol I: 733)

This lack of direction epitomises the Social Democrat leadership. It also demonstrates the tortuous path which they had threaded between their 'theoretical extremism and practical moderation'. Shepherd rounds on the Democrat leadership thus:

"This record of passivity would be admirable if it reflected the deliberate self control of strong men putting the cause of law and order above their righteous wrath. But it is difficult to resist the conclusion that what it really represented was not so much cool heads, but cold feet .. the constant talk of strikes and 'strong arm methods' which the leaders indulged in at the time amounted to a deception of their followers." (Shepherd 1961: 113)

Ernst Fischer was present in the offices of the Arbeiter Zeitung<sup>1</sup> on the night of July 14 when a decision had to be made concerning the course of action that the party would adopt following the jury's decision to acquit the Schattendorf murderers. Also present, according to Fischer, were Friedrich, Austerlitz (the Editor in Chief), Otto Bauer, Julius Braunthal, Oscar Pollock, Otto Leichter and 'a student, Hans Zeisel'.<sup>2</sup> Hans Zeisel was, in fact, a colleague of Lazarsfeld's at the Forschungsstelle, co-author of Marienthal, and later director of the Forschungsstelle after Lazarsfeld had left for America. The

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1 The Arbeiter Zeitung was the main newspaper of the Social Democrats. However, it was not a newspaper in the modern popular idiom but rather a cross between a newspaper and a magazine. It had many distinguished contributors and carried serious articles. Party statements and theoretical debates tended to overshadow 'news'. For example, the front page would often carry the main article.

2 See Fischer 1974: 149 for people present in the offices of the Arbeiter Zeitung and for a good account of the confused atmosphere.

meeting was unwilling to give the call for action or to issue arms to the Schutzbund, with all that implied in terms of a civil war, yet they feared that a lack of militancy on their part would result in a loss of control over the rank and file party membership who might then act on their own initiative. The Arbeiter Zeitung's editorial which appeared on the 15th July was not a call for action, but rather an outburst of indignation and a stern warning to the government. In short, it was a substitute for action. However, as far as the rank and file membership was concerned, words were not sufficient to express their anger at the 'injustice of the state'. The factories shut and the workers flooded into the inner city for an unorganised demonstration. The moments that followed highlighted the weakness of the Democratic Party. Not prepared to issue the orders for an armed show of strength, yet having in 1920 abandoned the coalition and in doing so handed over the most important ministries to the Christian Socialists, they now placed their members at the mercy of the state's repressive apparatus. Of the demonstrating workers 89 were killed and 1057 wounded. This was the worst trouble that Vienna had witnessed since the revolution of 1848 and also the first time that social democratic workers had been fired on by government forces.<sup>1</sup> The party's folly at having abandoned positions within the state now became starkly apparent. For example, it was Schrober, the Chief of Police, who under Seipels order as leader of the Christian Socialist Party sent the armed police

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1 See Gulick 1948, Vol I: 746 for these figures. According to Gulick the killings were not completely unorganised, but followed a deliberate plan of intimidating the left. The shootings continued into the evening as police cars sped through the working class districts firing spasmodically. Some Schutzbund members who had uncovered arms retaliated to these late attacks, even to the extent of mounting an assault on a police station.

to quell the demonstrators. However Schrober had also asked Seitz, as governor for the state of Vienna, for detachments of the army to be sent in as support. Seitz refused the request,<sup>1</sup> which is a good example of the greater power that the Social Democrats might have been able to exert on political events had they kept control of the important ministries themselves and not abandoned the coalition. Seitz himself was still in a position to exert such power since the Democrats still controlled Vienna. Of the outcome of the events, which was clearly the beginning of the end for the party, Leser writes:

"After the 15 July 1927 there was no turning back from the road leading inexorably to civil war and the tragedy of February 1934. Renner's frantic and repeated efforts to reinstate the coalition failed as lamentably as Julius Deutsch's attempts to prevent the outbreak of civil strife by demanding the disbandment of the rival para-military party formations. The Heimwehr, now riding the crest of the wave, was sufficiently powerful to spurn offers of a compromise peace." (Leser 1960: 129)

The role and the power of the Heimwehr quickly became apparent through its response to the general strike that the Social Democrats called in the wake of the killings. The Heimwehr had come into being during the Winter and Spring of 1918-1919 as voluntary defence units to protect their homes and property from marauding bands of ex-soldiers, and in areas such as Carinthia against attacks from the Yugoslavs. As they developed, however, these units were used not only against foreign enemies but also against Marxists as the enemy 'within'. This in turn led to political, military and ideological links with similar movements in Germany.<sup>2</sup> But it was the events of 1927 that thrust them into the forefront of Austrian politics, although according to Jedlicka the growth of the movement:

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1 See Gulick 1948 Vol I: 744 for the details of these negotiations.

2 See Jedlicka 1966: 129 for the linking up with other right wing movements.

"Was more than a reaction to the events of July or a counter offensive by the anti-Marxist front; it represented also the breakthrough of modes of thought partly borrowed from the fascist ideology and admittedly very much akin to it."  
(Jedlicka 1966: 133)

The problem of the Heimwehr was that rising as it did from the ranks of the peasantry, and led by members of the petty bourgeoisie plus an assortment of faded aristocrats and pensioned-off officers, it lacked firm ideological underpinnings. The material support was provided by Seipel with the aid of the large banks, and by Mussolini, and its ideological structure was provided by the teachings of Professor Othmar Spann<sup>1</sup> who taught sociology at the University of Vienna. It was Spann's work that furnished the Heimwehr's somewhat loose beliefs with a groundwork of theory onto which both purpose and respectability could be grafted. Although the Heimwehr never had a proper understanding of Spann's ideological system, it seized upon his concept of the corporate state.<sup>2</sup> As Jedlicka notes:

"Groups of influential intellectuals, such as the German Club in Vienna, intervened in this discussion about the Heimwehr programme. The club, a meeting place for businessmen and academicians belonging to radical and German national groups, arranged a series of lectures in which both Professor Spann and Dr. Richard Steidle, as well as representatives of the German Stahlhelm<sup>3</sup> took part to discuss a programme. Its first objective was to alter the constitution; the ultimate aim was to set up a new type of state. Even before this programme of a revival of the Stande<sup>4</sup> had been proclaimed, particularly by Pfrimer, the leader of the Styrian Heimwehr. Walter Heinrich, a colleague of Spann's, was the main proponent of the idea of a corporate state. (Jedlicka 1966: 137)

It was this movement then that dealt such a heavy blow to the Social Democrats' general strike. The strike, proposed to last only twenty four hours, was extended by the transport and communications workers,

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1 His actual position was Professor of Economics.

2 See Jedlicka 1966:137 for the questioning of their understanding of Spann's work.

3 The Stahlhelm was a para-military force of the German right and source of frequent trouble to the Weimar Republic .

4 Stande in this context is best seen as state structured according to occupational status (Standestaat)

until an amnesty had been cleared for those arrested by the police in the course of the demonstration. Although the response and discipline shown by the strikers did not fail to impress Seipel and the government, it also illustrated the extent of the shift away from institutionalised political procedures which had taken place within the Republic. In Styria and the Tyrol, for example, the Heimwehr mobilised in a massive and impressive display of strength - 20,000 heavily armed men against the strike. Faced yet again with the threat of civil war, Bauer called off the strike and in return Seipel promised that he would not "avail himself of the outcome of the events for a general attack on the social gains".<sup>1</sup>

The Socialists, defeated firstly by their unpreparedness on the streets and secondly by the threat of force, could draw little comfort for the future. There was little possibility now of turning the anti-democratic tide that resulted in the suppression of Parliament by Dollfuss in 1933. True, the appearance of power remained unchanged, but the Social Democrats occupied the same posts as they had done before the events of 1927. The course of events had revealed the actual base of power relationships, and shown that the left occupied the weaker position amid mounting hostile forces.

"It was Austro-Marxism's tragedy neither to have heeded Renner's plea for unequivocal acceptance of parliamentary democracy, nor to have foregone the pleasures of barnstorming revolutionary rhetoric and of supplementing it with dedicated action." (Leser 1966: 130)

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1 See Gulick 1948 Vol I: 749 for mobilisation and negotiated promises.

The Life-World of a Social Democrat

"It has been my good fortune to see something of several continental socialist movements, in Moscow, Budapest, Madrid, and Salonica as well as in Paris and Berlin. I can recall several talks with Jean Jaueres and it was for years my habit to read everything this humane and generous thinker wrote in his organ L'Humanite. Much of it I still remember clearly. I shared the respect which all who knew them felt for the organising talent and the disciplined steadfastness of the Germans. But it was among the Austrians that I felt most happily at home. More ardently and steadily than any other continental party they lived socialism as a creed that covers the whole of life. It was for them so much more than a political tactic and an economic programme." (Brailsford 1945: 8)

Brailsford's feeling of 'belonging' and his description of Austrian socialism as 'more than a political tactic or economic programme' provides a good point of entry into the meaning that the Austrian Socialist Party had for its members, and this meaning in turn is crucial to an understanding of the world within which Lazarsfeld and his colleagues lived.

The allegiance of the member to the party was not simply a matter of political instrumentalism; it moved far beyond that to provide the meaning of his life. Whilst it is certainly true in most cases, allegiance to a political party or group has an effective as well as an instrumental side; the feeling that the Austrian worker had for his party was one of deep emotional attachment bordering on the quasi-religious. For, as impressive as the party's record was in relation to social legislation (social welfare schemes, housing projects, anti-natal clinics and so on), its distinctive and most impressive characteristic was its penetration into almost every aspect of the worker's life.

"After 1918 the party had not only grown to be a mass organisation of unique size and vigour, but a spiritual power whose effects were lasting and profound. It was equally capable of serving interests and ideals, passions and everyday needs, and as a result its varied ramifications kept multitudes under its spell. Far beyond the realm of politics it shaped the lives and thoughts of its active members." (Buttinger 1953: 21)

Hundreds of thousands of men, women and children lived their lives under the enormous protective umbrella of the party and occupied themselves with one or other of the recreational facilities that it provided. Its broad organisational structure had room for all trades, professions and activities. For many:

"They did what they did not as such, but ideologically ... in the real or imagined behalf of the party and socialism. In this mass of hundreds of thousands anyone capable of rising above the merely personal found that the party gave meaning to his life. This fulfilment was as strong and enduring as a religious tie." (Buttinger 1953: 21)

The party was more than its bureaucratic structure for those who fell under its influence; it lent dignity and purpose to otherwise drab lives. The career of Adelheid Popp is a good example of this process at work.<sup>1</sup> Initially her existence was one of extreme isolation, both in her private life and in her work situation, operating as she did as a home worker in the sweated clothing trade. Eventually force of economic circumstance propelled her into factories and hence into contact with organised labour. Once there, she came under the influence of the Social Democrat party and entered a hitherto unknown world that totally altered her previous way of life. Reading voraciously, studying, attending workers' evening classes, making speeches and contributing

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1 See Popp 1912 'The Autobiography of a Working Woman'.

articles to various organs of the party, she herself eventually became a well known party official. Whilst such patterns of self-improvement are not uncommon throughout the early history of the labour movement, the Socialist Party of Austria held a whole generation within its interlocking web of relationships. Not all, of course, became activists like Popp, yet whether the worker was in the socialist chess club or the debating club, the party touched his life.

The party's strength lay in its combination of hope for the millenium with the benefits of hard pragmatism and concrete social improvements. For those who embraced the party its influence was lasting. At the beginning of a lecture which he gave when he had not been too long in America, Paul Lazarsfeld described himself as a "Marxist on leave", to which a member of the audience responded, "and who gave you permission"?<sup>1</sup> Although amusing, this incident points to the strength of members' allegiance to the party. For a member who had been as deeply enmeshed with the party and as influenced by it as Paul Lazarsfeld, one did not leave the party upon 'exile'. In a small but unpublished article entitled "An Index of Class Consciousness" an insight into what the party meant to him is provided. Lazarsfeld writes:

"... there were certain songs which made your heart beat more quickly, the sight of the Red Flag at the big demonstrations, the wearing of the party button at all possible occasions. This element of being demonstrative about one's party affiliations and emotionally attached to it is almost impossible to recapture in this country, where no major labour movement exists which could elicit such reactions. At this point my set of items is therefore the most unsatisfactory." (Lazarsfeld 2/24/54)

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1 Interview with Professor Lazarsfeld 15 June 1973 in New York. John Marshall of the Rockefeller Foundation had organised a discussion session just before the outbreak of the second world war and the topic was the measuring of socio-economic status. Lazarsfeld said he could not agree with people such as Lloyd Warner with his psychologistic point of view because he considered himself a 'Marxist on leave'. The person who interrupted Lazarsfeld was Cuthbert Daniel who Lazarsfeld then hired.

The Social Democratic Party did not merely represent a card membership, or the internalisation of a particular set of beliefs or party credo; to belong to the party as an active member was 'a whole way of life'. An individual with Lazarsfeld's background did not even come to the party as Popp did but was 'born into it'; thus in a sense Lazarsfeld's comment that he was 'a Marxist on leave' is perhaps better translated as an Austro-Marxist Social Democrat on leave. When the writer approached Bernard Berelson with Lazarsfeld's 'a Marxist on leave' comment, he pointed to the absence of Marxist content in Lazarsfeld's work and added, "well, it was certainly a long leave" (Berelson 12.7.73). However, this misses the meaning that Austrian Socialism had for its followers.

Despite the obvious hold that it had over its members, the Austrian Socialist Movement had none of the theoretical and disciplinary severity associated with membership of a communist party. The record of communist party members who break faith with the movement, for example the self doubt, the absolute denunciation of beliefs once so firmly held, or the bitterness and anguish<sup>1</sup> - none of that appears to be present in past members of the Austrian Socialist Movement. This difference in reaction is due mainly to the differences in the type of party, which is exactly the point that needs underscoring. To be a member of the Social Democratic Party was not simply a way of life; it was to live socialism now, not in some distant future state, for within the party and particularly within Red Vienna, the state within a state, the socialist experiment was an ongoing process. Admittedly the future was looked towards for the realisation of the 'socialist dream', but in the meantime it was semi-

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<sup>1</sup> See the accounts given by past communist party members in 'The God that Failed', Crossman 1950.

operational in the shape of the tenement blocks, and the various clubs and in the youth movements. Unlike a communist, one did not need to break faith with a hard body of party doctrine in order to be rid of the party; since even the doctrine had the flexibility of the various strands of thought that created it, one did not have to break with the party cell since it never had cells, only clubs; and one did not have to abandon future dreams since that dream was in embryonic form already extant. Lazarsfeld was therefore an 'Austro-Marxist' on leave primarily in the sense that he had left the social world he had known - by his physical removal to America. He had not rejected his basic beliefs; these would remain with him as memories - memories that became increasingly redundant in his American situation.

By holding this in mind it becomes easier to understand Lazarsfeld's relative ease of integration into American academic life as against the difficulties experienced by other émigré groups, and by certain members of the exiled Frankfurt School in particular. The reasons for this contrast were firstly the differences in their respective styles of work, and secondly the marked differences in their structural situations within America, a point which will be developed later. It is particularly interesting that the content and style of their work should differ so markedly when both come from schools of European Marxism even though of differing varieties. However, it is not primarily the absence of Marxist content in Lazarsfeld's work which accounts for his acceptance in America; rather it is the presence of certain other sorts of content, contents which were relatively absent in the work of most members of the Frankfurt school. This then takes one back to Lazarsfeld as an Austrian Social Democrat. Being a member of the Social Democratic

Party allowed one to embody one's socialism through an attachment to a whole variety of party activities and structures. Hence it was necessary to have one's 'socialism within one's work' in order to demonstrate commitment. Having said this, however, it would be wrong to consider that Lazarsfeld's socialism did not inform his work at all; it did, especially in Jugend und Beruf, Marienthal and indeed in other works. However, his socialism only directed the choice of subject matter and not the methods of procedure; these were informed from other intellectual sources which, while not directly related to 'Marxist theory', did stem from Viennese socialists. But again the detailing of these influences must wait until the next chapter.

In the case of the Frankfurt school, their Marxism was their work and their work their socialism, thus to 'give up' their socialism was to give up their intellectual work - a much more difficult task. The core of the Frankfurt school never really made the transition into mainstream American sociology, but it should not be supposed that this was because of their antagonism towards empiricism. On the contrary, the work of the Institut für Sozialforschung in Frankfurt represented the "broadest and most advanced efforts in the Weimar Republic of German sociology to establish quantitative empirical social research" (Schad 1972: 76). Indeed, when Horkheimer took over the Institut in 1931 he removed it from the Faculty of Economics and Social Science and established it as part of the Faculty of Philosophy<sup>1</sup> in order to make quantitative empirical social research the Institute's main task. For, as Schad notes:

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See Schad 1972: 78-80 for full reasons behind this move. Horkheimer advocated the role of philosophy as an agent of social change and was alarmed by the social and political conditions in Germany towards the end of the Weimar Republic. Thus it was in order to make 'Philosophy an effective instrument of social change that he moved the 'Institut' to the Philosophy Faculty.

"... it was Horkheimer's goal to lead philosophy back to its ancient goal of trying to make the world a better place to live in, and he realised that in his day and age philosophy could not work speculatively but had to integrate empirical data pertaining to the concrete social world and the human psyche.

Thus, Horkheimer proposed that the Institut, instead of the historical and theoretical studies which had been conducted up to that point, devote itself to pursuit of empirical investigations in order for sociology to become effective."

(Schad 1972: 79)

In fact, when Lazarsfeld was director of the Forschungsstelle in Vienna he was engaged by the Frankfurt Institut to conduct work for them, and he was engaged again in America when he was Head of the Newark 'Centre' which eventually became the Bureau of Applied Social Research. Horkheimer always felt that the empirical methods developed in America would be of great help to the Institut, and was therefore particularly pleased at the prospect of going to America and continuing such work.<sup>1</sup> However, the reality of the American situation was somewhat different from their expectations.<sup>2</sup> In addition to financial difficulties,<sup>3</sup> they faced the practical difficulties of translating their large-scale theories into empirical practice without reducing or compromising the scope of their conceptualisations.

For Lazarsfeld no compromise was necessary or required, since the 'living socialism' of the Austrian Social Democrats had always made it possible for him to be a 'Marxist' and at the same time a strong empiricist. The fact-value dichotomy never arose for him; he was a 'Marxist' and an 'empiricist', separated yet connected in the world of practical socialism so that contradictions never occurred. Thus in America the empiricist compartment could flourish, and yet produce scepticism in a person such as Berelson. Only by understanding the

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1 See Schad 1972: 82-94 for this point.

2 See Adorno's 'Memoir' 1969 for the methodological difficulties which he faced.

3 See Jay 1973: 167-170 for the Institut's financial resources which allowed them to be so independent.

world of Austrian socialism can such a confusion be readily understood.

The Self-Contained World of a Social Democrat

The self-contained world inhabited by the Social Democrat, while it sustained party unity and buttressed it against opposing ideas, had the unfortunate consequence of limiting the possibility of fruitful negotiations with the 'class enemy' and preventing the formation of a popular front against the extreme elements of reaction. Most Social Democrats had little contact with other political groups. The consequence of such containment limited the possibility of reproachment with the Christian Socialist Party. For, since the Christian Socialists never had an absolute majority in Parliament they were forced into alignment with other right-wing groups. However, from 1920 onwards the 'Burgerblock' of Christian Socialists, Pan Germans, Land League and the Heimat-Block began to split. Instead of recognising that this splitting of the Burgerblock was to a large extent responsible for the difficulties of governing the country<sup>1</sup> and therefore seeking arrangements with the Social Democrats, the Christian Socialists pushed by their more right wing members turned to the extremist Heimwehr for support. This was especially so after 1931 when the German Nationalists drifted over into the National Socialist camp.<sup>2</sup> Thus, although the Social Democrats could draw some comfort from the splitting of the 'Burgerblock', the eventual re-alignment of Christian Socialists and Heimwehr forces produced a far greater threat than when the Burgerblock had been intact. The final outcome of such a development was the military crushing of the Social Democrats in 1934.

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1 See Stadler 1971: 116.

2 See McDonald 1946: 72.

As McDonald writes:

"The victory of the extremists was also in part a consequence of the tendency of both sides towards what may be described as party totalitarianism. This was especially the case on the Social Democrats' side, for the tendency of the Burgerblock to split into small groups prevented this principle from working out so fully in their case. The Social Democrats, however, had from the beginning laid particular emphasis on the need for presenting a united front to the outside world. They tried with this end in view, to prevent the members of the party from coming into contact with people of other opinions who might influence them and draw them away from the party orbit."

(McDonald 1946: 72)

Such isolation was possible because the party looked after the workers' every need - his intellectual and physical needs were all satisfied within the party; it provided his flat, the kindergarten for his children and protected him at his place of work. In fact, Bullock goes so far as to say that:

"For a worker to exist, it was necessary for him to be a Social Democrat and to belong to a trade union, and all employers had to deal with the unions. It was not until 1927 that a decision was given in the courts that a workman could not lawfully be dismissed for not being a member of a trade union, and if the union compelled a firm to dismiss a man - union member, the union was liable for damages."

(Bullock 1939: 114-115)

Such was the socialist party's hold over the working class that for a long time it held off the advances and appeals of National Socialism propaganda. However, the main target of 'Nazi' propaganda within Germany was not the working class, but the deposed and disillusioned middle classes, and in particular the new 'Mittlestand',<sup>1</sup> who were a product of industrial bureaucratisation and the expansion of the distributive trades. The new 'Mittlestand' shared a broadly

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<sup>1</sup> Mittlestand cannot be translated into English because of the differences in the class structure of the two countries. There is an absence of an appropriate group with which to make a translation meaningful. Stand or 'status' was given the extremely hierchical ordering of German society - very important since it conferred certain privileges and titles.

similar market situation with the proletariat, since they relied primarily on the sale of their labour which at best was only semi-skilled.<sup>1</sup> However, what separated them from the proletariat, and at the same time made them particularly receptive to National Socialist propaganda, was their Standesdunkel (or feeling of class superiority).

The new Mittlestand were, to quote Pulzer:

"A classic example of a class constantly threatened with depression into the proletariat. Their two pre-occupations were to keep their distance from those below and to secure from their employer better conditions." (Pulzer 1964: 285)

Not for them the protection of their living standards through unionisation, but rather the creation of Verbande (associations) of employees. To have unionised along traditional working class lines would have further undermined their already threatened 'standesdunkel'. Such a move on their part would have required a leap in consciousness which they were incapable of performing. Hence, instead of accepting their new proletarian position they rejected it and instead tried to strengthen and hold their former position, and in that they were reactionary. Yet at the same time they had been radicalised by their new conditions so that a party such as the National Socialists held a ready appeal. Making an impact on the working class itself, however, was an altogether different matter. Because of their level of political consciousness, but inroads were made, and by 1932 at least a quarter of the German National Socialists' votes came from the working class. However, in Austria the task of capturing the working class was even more difficult because of the extent and depth of the Social Democrats' influence and high level of political consciousness which they had engendered among the workers. Pulzer, commenting on this whole situation writes:

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<sup>1</sup> See Pulzer 1964: 285 for this point.

"In 1932 at least a quarter of their vote must have come from the proletariat. Hitler, after all, had learnt his politics in Vienna, where the leaders of anti-semitism had succeeded in conquering the masses,<sup>1</sup> an objective in which all their German contemporaries had failed. It was in Austria, too, that the Nazis alone succeeded in breaking this barrier. Austrian politics were even more rigidly ideological and class bound than in Germany. The Nazis therefore made much slower progress initially. By 1932 they had conquered almost all the Pan-German and right radical camp, but this amounted to only about 20% of the total. It was only after the establishment of the clerical corporate regime that they made spectacular headway." (Pulzer 1964: 326)

The two points made in this quotation deserve elaboration; firstly, the fact that Hitler had received his political education in Vienna, and secondly the fact that Austria was more rigidly ideological than Germany. To take point one first. Hitler was not only impressed by the effectiveness of Christian Socialist leader Karl Leuger's anti-semitism, but also extremely impressed by the Social Democrats' organisation. As Watt writes:

"Their organisation, their use of marches, slogans, songs, salutes and the like, their doctrinal inspiration of the individual party worker, and their integration of all forms of organisation of social activity into the party framework, struck him as immensely effective." (Watt 1969: 24)

Although disliking in the extreme the cosmopolitan nature of the socialist movement (it was the hey-day of the Second International) and also its ideological components, he nevertheless appreciated the organisational efficiency that enabled the Democrats to mount huge demonstrations of working class solidarity. He writes:

"With what changed feeling I now gazed at the endless columns of a mass demonstration of Viennese workers that took place one day as they marched four abreast. For nearly two hours I stood there watching with bated breath the gigantic human dragon slowly winding by." (Hitler 1969: 38)

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<sup>1</sup> By masses, Pulzer is referring to the lower middle classes and some of the working class who voted for the Christian Socialists in the 1897 elections. The workers who supported the Christian Socialists were however mainly in municipal employment. See Pulzer 1964: 205.

The relative ideological resilience of Austrian politics owes much to the Social Democrats' educational programme. Commenting on the effects of such programmes on the political consciousness of the workers Hitler writes:

"[the worker's conversation] was of such a nature to infuriate me in the extreme. These men rejected everything: the nation as an invention of the 'capitalist' (how often was I forced to hear that single word) class; the Fatherland as an instrument of the bourgeoisie for the exploitation of the working class; the authority of law as a means for oppressing the proletariat; the school as an institution for breeding slaves and slave holders; religion as a means for stultifying the people and make them easier to exploit; morality as a symptom of stupid sheep-like patience etc." (Hitler 1969: 37)

Such then was the ideological fare that the worker had gained from the evening lectures, theoretical journals and pamphlets which the Social Democratic Party provided for him. This, coupled with the closed nature of the social world within which he lived, so that at every turn he bumped up against some part of the party structure, made for a formidable barrier against penetration from the right; that is, until the party itself had been crushed.

As powerful as the influence upon the working class was by the Social Democrats, the extent of their influence did not stop there, but continued upwards into the ranks of the professions. The 'Kammern' or chambers of the various trades and professions played an important part in the social democratic life of Vienna; for example, members were elected to represent each profession or occupation and no legislation could be enacted without consultation of the chambers taking place.<sup>1</sup>

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<sup>1</sup> As evidence of the Social Democrats' emphasis upon education it is worthy of note that 'workers and officials chamber' had the largest social science library outside Moscow. Bullock 1939: 116.

However, it is to the socialist youth movement that one must turn for a true understanding of the all pervasiveness of the Social Democrats' influence, but in addition the youth movement is extremely important for an understanding of Lazarsfeld and the Forschungsstelle, since not only was Lazarsfeld influenced by the youth movement, but in turn he himself was influential upon it. For example, Buttinger, when discussing the political career of the worker, Joseph Simon, and his drive for education mentions Simon moving from the Socialist Labour Youth to the:

"Socialist High School Students in the hope of finding there an intellectually superior expression of his socialist beliefs. The work of its former leaders, Ludwig Wagner,<sup>1</sup> Paul Lazarsfeld and Mitzi (Marie) Jahoda, had given this body a reputation among young intellectuals that had attracted him."  
(Buttinger 1953: 82-83)

### Education and the Youth Movement

Among left wing parties there is nothing particularly unusual about the emphasis given to the education of its members. Since left movements are based on a body of theory it is necessary for all members to be acquainted with the main elements of the theory and for party cadres to have thorough working knowledge. Hence educational work tends to be assigned a high priority and status within left wing organisations. However, within the Austrian socialist movement education was given exceptional emphasis. In part this was due to the heavy representation of Jewish intellectuals within the party,

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<sup>1</sup> Ludwig Wagner was a friend of Lazarsfeld and helped him organise a youth camp to develop a 'socialist spirit in young people'. The report of this camp was the first article that Lazarsfeld published. See Lazarsfeld 1969: 273.

especially at the leadership level. It can also be argued that the intellectuality of the Austrian socialist movement was one of the features that attracted Jewish membership; certainly it fostered an atmosphere within which the Jewish intellectuals could feel at home and within which they could excel. The two factors are inter-related, but as with many situations there is a danger of overlooking the simple and the obvious, for the heavy recruitment of Jews into the socialist party of Austria was due less to the traditional Jewish regard for intellect and scholarship, than to the low respect that right wing parties traditionally accorded to Jews. Undoubtedly the number of Jewish intellectuals within the Social Democratic Party raised the value that the party placed upon education. At the same time however, this high valuation was buttressed by the fact that education played a key role within the party's overall policy. As McDonald writes:

"[Bauer] ... firmly believed that the future of socialism depended as much on the intellectual development of the workers as on their conquest of material power and for this reason also he was not averse to a policy of gradualism." (McDonald 1946: 86)

Mention has already been made of Victor Adler's unification of the various socialist groups to form the Social Democratic Party at Hainfeld in 1889, but it is worthy of particular note that the adopted programme emphasised the role of education. It stated:

"In the interest of the future of the working class, compulsory, free and non-denominational instruction in elementary and continuation schools, as well as gratuitous accessibility to all higher educational institutions, is absolutely required and the necessary prerequisite to this is the separation of church and state and the declaration that religion is a private matter." (Partetag 1889: 4)

Later, education assumed a key role in Bauer's policy of gradualism. Important as they were, it is not intended to dwell upon the reforms within formal education, but rather to examine the more informal 'educational' movements, since these are of greater importance in relation to the Forschungsstelle. The two groups that will be looked at are the Kinderfreund (friends of children) which at its height in 1929 had a membership of 100,340 and the Rote Falken (Red Falcons) of which Lazarsfeld was a leader, and which at its zenith in 1932 had a membership of 15,117.<sup>1</sup>

Although youth organisations later became important for the socialist movement, recognition of the need or desirability of such organisations was not present in the early days. In fact youth organisations were neglected by the party for a considerable time, and attention and energy was focussed on the immediate struggle to strengthen the movement's political and trade union base, a struggle in which youth organisations could be of little assistance. However, this pragmatic neglect of youth was also buttressed by deep-seated resistance to alterations in the traditional patterns of youth-adult authority relations. As Gulick notes:

"even after the revolution, which had brought complete political democracy to the working class, the desire for authority in the family was quite common in proletarian circles; fathers remained distrustful of the youngsters who tried to take the future into their own hands. Thus long after organisations for children and youths had been fairly well established and generally recognised as important assets in the fight of labor for a better world, party as well as trade unions kept a watchful eye on them in order to prevent unsupervised steps." (Gulick 1948, Vol 1: 587)

Partly in spite of, and partly as a response to such authoritarianism within the family, youth movements sprang up in Austria and Germany.

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<sup>1</sup> See Gulick 1948 Vol I: 609.

One such movement, the Wandervogel, although completely independent of any political party had a major impact upon the socialist youth movement, mainly by way of example.

The German Wandervogel represented a revolt against authoritarian schools, parental education and to a large extent society, although the movement itself was not particularly articulate about such matters. It was basically a middle class phenomenon beginning in 1896 and ending in 1914 or 1933 depending on how one characterises the essence of the movement. So far as this work is concerned the difference in dating does not particularly matter, but the present writer prefers the stricter interpretation which dates the demise of the Wandervogel from 1914:<sup>1</sup>

"It was always the movement of the minority, accurate figures do not exist, but it seems that the members of the youth movement proper, the autonomous groups, never exceeded 60,000. It was almost exclusively bourgeois .... in social composition."  
(Crossman 1962: 9)

The Wandervogel movement encouraged a spirit of adventure and freedom from the restraints of adult society. Engaging as it did in outings to the woods and mountains, often venturing far afield, it set great store upon comradeship and self organisation. A strange feature of the movement was the replacement of rejected adult values with values abstracted from the teutonic myths of the past. Attachment to a glorified myth of the middle ages with its knights, vassals and guilds gave the Wandervogel an air of unreality. Unsure about the

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1 The writer prefers the date 1914 since with the First World War and the sharpening political conflicts within Germany that followed it became increasingly difficult for the Wandervogel to remain 'pure'. That is, the movement found it increasingly difficult to remain unaffected by such events which thus made their escape into fantasy less successful.

future the movement looked backward to a mystified past age. As Laquer writes:

"Their return to nature was romantic, as were their attempts to get away from a materialistic civilisation, their stress on the simple life, their rediscovery of old folk songs and folklore, their adoption of medieval names and customs."

(Laquer 1962: 6)

Although some of the mysticism and romanticism, plus elements of asceticism can be seen as a precursor of some aspects of Nazi ideology it would be wrong to consider that the Hitler youth movement was a continuation of the Wandervogel. The Wandervogel were too independent to find a place within Hitler's totalitarian regime; in fact the movement was banned by the National Socialists.

Between 1910 and 1913 the Wandervogel movement had extended into both Austria and Switzerland. However, the Austrian branch of the Wandervogel was far more political, in the sense of being nationalistic, than its German counterpart, and also anti-semitic in that it excluded Jews from membership.<sup>1</sup> This latter point is relevant to the development of the Rote Falken, for during a discussion with Dr. Wagner concerning Lazarsfeld's position as a Jew in Vienna, she drew upon the Wandervogel as an example of the Jews' situation as 'outsiders'.

Dr. Wagner: You know, the socialist youth movement was very much influenced by the German Wandervogel. It was impossible for a Jew to be a member. You see there was always certain circles where a Jew couldn't go, where he wouldn't be accepted. You would be asked 'are you a Jew or are you not a Jew?' and therefore the Jews in the socialist youth movement were very strong because they could form there and agitate. They were quite a close circle.

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<sup>1</sup> See Laquer 1962: 42.

D. Morrison: Was the Wandervogel attractive to you as a school child?

Dr. Wagner: Oh yes, very attractive. One knew as a Jew you couldn't, but it would be .....

D. Morrison: The Rote Falken?

Dr. Wagner: That was a development out of the youth movement. The socialists always had some organisation that looked after children and under the influence of the youth movement which was influenced by the Wandervogel and by the ideas of Bernfeld and so on. The Rote Falken was created as a movement which was quite different from what was done before for children, a mixture of boy scouts and Wandervogel I would say. (Dr. Wagner 9.10.73)

Although the socialist youth movement was greatly influenced by the Wandervogel, the differences remained overwhelming. What the Austrian socialist youth movement did adopt from the Wandervogel, however, were elements of style and form; in particular the unsupervised outings, the emphasis on comradeship and group spirit, and especially the absence of adult control and the insistence upon the member's themselves controlling the movement's organisation. A fundamental difference between the two movements was the presence of a strong sense of political purpose among socialist youth, even among those who were not members of an organised group. Writing about the groups of young unemployed, for example, Ernst Fischer comments:

"The young unemployed whom I met were activists, those whose spirits had not been broken. Like all the rest they formed themselves into gangs with their own leaders, customs and conventions, reminiscent of an earlier barbarous age. They drifted around together, met up with different gangs with whom they shared their bread and their girls and planned common enterprises. What distinguished them from other gangs was their political outlook. They called themselves Jungsozialisten (Jusso). It was their political outlook that preserved them from corruption." (Fischer 1974: 186)

The youth groups that Fischer mentions are probably among the most informal of the socialist youth groups, yet it is significant that even they should have been 'preserved from corruption' by their political outlook. The more formal socialist youth organisations certainly had a strong sense of purpose and direction, and great attention was paid to the development of the 'socialist man', through activities such as community help and co-operation among themselves and with others in the class struggle. The Social Democratic Party, ever present as a point of reference, though absent so far as direct day to day control went, prevented the aimlessness that characterised the Wandervogel.

The Rote Falken, perhaps the most famous of the socialist youth groups, developed out of the Kinderfreund, which had been established in 1908 in Graz by a journeyman carpenter, Anton Afritsch, who later became an editor of a provincial democratic newspaper. The Kinderfreund itself was not really a youth movement as such, but rather a parents' association set up with the approval of the City of Graz authorities. The important point about the Kinderfreund was that

"Though created and guided by socialists the Kinderfreund was not originally a party organisation. It showed, however, that the workers themselves were able to provide for a social development of their children in a socialist spirit and to create for them a better and more enjoyable life." (Gulick 1948: 585)

One of the central aims of the Kinderfreund was to supplement what the socialist membership regarded as the unsatisfactory and 'biased' education that their children received in the day schools, despite the reforms that had taken place in that sector. Not surprisingly the educational work of the Kinderfreund, and the Rote Falken, came under heavy attack from the Catholic Church. As Daiment writes:

"The church again and again attacked socialist youth organisations like the Kinderfreund and the Rote Falkan, for subscribing to progressive methods of education which lacked a proper spiritual base." (Daiment 1960: 119)

In fact however, the youth movement and especially the Kinderfreund possessed a strong 'spiritual base' and even went as far as mimicking the catholic church's festivals. An especially solemn celebration took place (Jugendweihe) for those children who were leaving school. For example, Spring festivals were organised that in many ways compared with the Catholic church's own feast of Corpus Christi.

These festivals:

"were designed to bind the children sentimentally to socialist ideas and to the organisation. It was hoped that they would produce in children a feeling similar to that experienced by most children while celebrating the Christmas and Easter holidays in traditional fashion. Socialist Festivals, socialist songs, participation in socialist parades, all that can attach children emotionally to the socialist movement." (Gulick 1948: 604)

In a discussion with the writer Professor Paul Neurath<sup>1</sup> talked about the spiritual and emotional element in the socialist youth movements, and mentioned that he remembered Paul Lazarsfeld opening a socialist youth camp with a moving socialist address which was delivered and experienced as though it was a prayer. It was this emotional attachment within the Kinderfreund which, according to Gulick,<sup>2</sup> was an important factor contributing to the success of the Rote Falkan.

The Rote Falkan differed considerably from the Kinderfreund in that it presented for the first time an effective challenge to the

1 Interview with Professor Paul Neurath, Vienna, 26:6:74.

2 See Gulick 1948: 604.

notion of adult supervision and adult sponsored provision of education and well being - although even in the Kinderfreund able children had been selected to act as instructors. Nevertheless the Rote Falkan represented a distinct break. Indeed it was created by Kanitz as a response to the perceived stagnation of the Kinderfreund on the basis of Tesark's ideas of utilising the gang feelings of the older children:

"He tried to utilise the boys' desire for independence, their longing for activity, yearning for adventure, revolt against authority, desire for close companionship with boys of the same age, and their readiness for voluntary subordination to a beloved leader."

(Gulick 1948: 605)

The Falkan covered the age range 12-16 and were organised into 'Horden' of not more than 10-12 members, although groupings of Horden did take place. An important factor of the Rote Falkan was their mode of organisation and operation. The segmented leadership structure associated with the boy scouts movement was rejected in favour of the direct subordination of every Falkan to the orders of their leader. Lazarsfeld led a Rote Falkan 'Horden' and the atmosphere of closeness and companionship and the patterns of leadership that developed there are particularly important for understanding the Forschungsstelle.

## The Jews in Austria

Although anti-semitism had a long history in the Austrian Empire, the post-war era saw a distinct shift in its tone and style.

As Pulzer notes:

"The main difference between the political anti-semitism of the post-war period lies not in its content, but in its success. There were some changes in emphasis, a general increase in virulence of tone and unscrupulousness, a growing acceptance of physical violence" (Pulzer 1964: 300)

This shift was in turn inextricably bound up with the mounting attack upon socialism.

Having failed to dismantle either the capitalist state or the capitalist economy, the socialists had left the capitalist class with a viable base upon which to regroup, and from which to mount a counter-attack. Hence, as the economic crisis deepened, they were able to attract the support of a sizeable section of both the peasantry and of the middle classes, and in the ensuing struggle to push back the forces of socialism the 'Jewish Question' was increasingly thrust to the forefront of the political arena. Indeed, so closely were the Jews associated with the socialist party that the two become fused in one composite description - Judenrepublik.

Apart from Warsaw and Budapest, Vienna in 1918 had the largest Jewish population of any European city, a population swelled even further by the thousands of Galician Jews who sought refuge there after the Russian invasion of Poland, and by the Hungarian Jews who flocked to Vienna after the fall of Bela Kuhn in 1919. According to the Census of 1934, nine tenths of all the Jews in Austria lived in Vienna, where they formed around a tenth of the city's total population.<sup>1</sup> More significant than

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1. Figures taken from Bentwick 1967: 467

their simple numerical presence, however, was their disproportionate influence on professional cultural affairs.

"Vienna was the city of Freud, Adler, Schnitzler, Kafka, Mahler, Reinhardt and Stefan Zweig ... and 1,200 of the lawyers and 1,500 of the doctors, for whom Vienna was famous, were Jews" (Bentwick 1967: 467)

Jealousy of this manifest cultural sophistication and dominance lay at the root of a good deal of the resentment and bitterness that the Austrian lower middle class harboured towards the Jewish Bourgeoisie.<sup>1</sup> Indeed, "anti-semitism and anti-intellectualism were rolled into one abhorrent ball" (Fischer 1974: 135).

Despite the rising tide of anti-semitism, however, Jews had some reason to feel at home in Vienna. The city was after all controlled by the Social Democratic Party, of which most of them were members. Even so, their structural position remained ambivalent. Assimilation was far from complete and consequently a sense of estrangement remained. They were, in Peter Gay's apt phrase, permanently in the position of 'outsiders as insiders'.<sup>2</sup> This question of Jewish marginality was commented upon by Professor Jahoda in an interview.

"The intellectual professional class in Austria was a class alienated. It wasn't alienated individuals, because you know, we ate together, and talked, and we felt very 'groupy'. We knew hundreds of people.. we talked to artists, musicians, mathematicians and psychologists, and whoever you want to name - and economists. But the whole class was alienated from that impossible situation in which Austria was left after the 1918 revolution, and particularly in Vienna. Vienna could function

1. When Hitler marched into Vienna on March 13, 1938, the Austrian Nazis engaged in 'outrages' against the Jews which far surpassed anything witnessed in Germany. (Bentwick 1967: 467)
2. Although Gay (1969) uses this phrase to describe the alienation of the 'Intellectual' in Weimar Germany, it is similarly appropriate when applied to the situation of the Viennese intellectual between the wars.

as the capital of a great empire, as the capital of seven million people, it was just out of proportion, and without recognised functions in Austria. I think that is important for the understanding of Austria." (M. Jahoda 1973: 105)

It is important to remember that Vienna was surrounded on all sides by provinces hostile to her 'Jewish socialism' and was relatively isolated as a consequence. Thus, for the professional intellectual class to which many Jews belonged, the collapse of the Empire brought not only the curtailment of function noted by Professor Jahoda, but also a marked shrinkage in their social world. It was in this situation of being in a city and a social group increasingly isolated and driven in on itself, that the socialist party became a key point of reference, identity and comfort for Viennese Jews and Jewish intellectuals. In particular, as Stadler notes, "It was in this party that the great majority of Austrian Jews found their political home, and to which they gave some of its most brilliant leaders like Otto Bauer, Max Adler, the young Frederik Adler and many more" (Stadler 1971: 139). The party welcomed and valued Jewish intellectuality, and consequently, in addition to providing a good proportion of the leadership, Jews filled a great number of the jobs calling for intellectual talents and professional skills, such as financial advisers, party lawyers, and authors of party literature. Jews, in fact, wrote the bulk of the trade union press and provided ninety per cent of the editors of the famous Arbiter Zeitung (Buttinger 1953: 80).

In addition to the sense of purpose and security that committed membership bestowed, the socialist party also afforded points of contact with the social world of the working class, contacts moreover which were underpinned by an ideology of brotherhood and internationalism

which acted as a buffer against the expression of anti-semitism.

According to Pultzer:

"The worker did not see the Jews with the same eyes as the upper and middle classes. If they were exploited by a Jewish employer they generally knew that conditions in gentile establishments were no better; and the financier and the broker seemed a less immediate enemy than the capitalist. They did not fail to notice that there were many Jews ... including some who could have led comfortable middle class existences had they chosen to who had taken up their cause." (Pulzer 1964: 280).

This description is somewhat idealised, however, for although overt anti-semitism was absent within the party, undercurrents of tension lay not far beneath the surface, as this extract from a conversation with Elizabeth Schilder, a party activist and friend of Marie Jahoda, makes clear:

D. MORRISON

Would you say that there was much antagonism between the intellectuals in the party and the workers?

E. SCHLDER

No, not really. Bauer you see, it was ... these leaders of the party were intellectuals.

DM But you said earlier that there was a slight antagonism.

ES It was, yes .. there was a sensitivity in the party at the Jewish people, for instance, and at the role of the Jewish people. They were very big in the party. There existed anti-semitism among the students at the university. Of course that's all changed now - nobody dares.

DM Was the anti-semitism such that a Jewish socialist intellectual would experience such pressure?

ES Well, I didn't know enough of Paul Lazarsfeld - he was by his exterior a type of Jew, his movements always were very Jewish. An intellectual Jew, and it is possible that he had the feeling not to be ... that he had not the chance to be a leader of the party by his Jewishness.

Marie Jahoda herself, however, had no doubts that the fact that

Lazarsfeld was a Jew hampered his political career within the party.

JAHODA

Well, Paul was very political. When I eventually came to know him closely, he had great political ambitions. You know, the youth movement was a politically socialist youth movement and I think the great dream of his life would have been to be Foreign Minister for socialist Austria one day, but the political situation came in at the university, but much more in the political party, because of the terrible never-disappearing anti-semitism in Austria. You know, Paul was so obviously Jewish and he just didn't have a chance in the political party. You know, other young people did make a spectacular career in the Social Democratic Party, but for Paul it was difficult because he was so very intelligent that nothing on the second level would have suited him, and the fear of general reaction to a Jewish dominant figure in the Party was very strong." (Jahoda 26.9.73)

After the failure of the 1934 rising, this undercurrent of distrust turned to outright resentment, and recriminations and attacks were directed not so much towards the leadership in general as to the Jewish leadership in particular, and it was they who were held as primarily responsible for the debacle. Up until that time, however, anti-semitism within the socialist party, although undoubtedly present, remained covert and implicit. Outside the party's orbit of influence, however, Jewish members were exposed to the full virulence of anti-semitism, and nowhere more conspicuously than in the university of Vienna.

#### Anti-Semitism in the University

If the Jewish intellectual was somewhat cushioned from anti-semitism within the Social Democratic Party, such protection did not extend as far as the university within whose precincts Jewish students, and particularly socialist Jewish students, were subjected to frequent abuse and attack. Indeed, the University of Vienna was probably the most anti-semitic in the country.

Pulzer states:

"Radical racial anti-semitism among Austrian students was almost universal - beatings up of Jewish students at Vienna were regular. In this they enjoyed the sympathy of many of the teaching staff who joined them in demanding a quota system on the model of Hungary and Poland. Indeed, only the intervention of the law court prevented the implementation of new statutes drawn up along those lines." (Pulzer 1964: 308)

This latter point of anti-semitism among the staff will be taken up later since it has been contested in interviews that the writer conducted. The evidence of anti-semitism at the student level, however, is indisputable.

The question of anti-semitism at the University was broached in a discussion which the writer had with Professor Berta Karlick who had been a friend of the Jahoda family and had studied mathematics with Lazarsfeld during their student days.

D. MORRISON

Was there much anti-semitism at the University?

B. KARLICK

No, I shouldn't say there was. There was strong tension among the students. There were certain groups who felt very nationalistic and anti-semitic. These groups were provocative to other students who gathered together in other groups - for example, the socialist groups, and there were actually unpleasant fights among the groups.

DM Very savage?

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1. According to Siegent (1974: 36) the 'academic senate' of the University did pass such a population quota but the Verfassungsgerichtshof (court which ruled on constitutional matters) ruled on the 20th June 1931 that such a move was counter to the Republican constitution. Although a repeated attempt was made by Minister Czernac, who introduced it to Parliament, it never actually became law despite passing its first reading on 30th April 1932.

BK Yes, people were thrown over the balcony, but this was almost always at the student level. There were some young assistants who were also sympathetic with the anti-semitic groups. I had no contact with such groups, but maybe there was, there was certainly a very liberal spirit among the professors. The professor of this institut, who was my professor, and the director - he was Jewish - and the second professor of the institut under whom Marie Jahoda's brother worked for his thesis was also Jewish, and there were several Jewish assistants in the University. I was non-Jewish, you see. This was perfectly uninteresting to the group of professors. (Karlick 19.10.73)

This image of the professoriat as relatively liberal and untouched by prejudice was endorsed in an interview with Professor Rosenmayer. He argued strongly that Lazarsfeld's Jewish background would not have hampered his academic advancement within the University and pointed to the fact that one of the University's most celebrated professors - Sigmund Freud - was a Jew, as convincing evidence of the lack of anti-semitism among the academic and administrative staff. A closer examination of the facts of Freud's case, however, reveals this argument as somewhat less than convincing.

Ernst Fischer, writing about the famous Jewish editor-in-chief of the Arbeiter Zeitung, Frederick Austerlitz, comments that it was Austerlitz's secret hope that on the occasion of his sixtieth birthday he would be awarded an honorary doctorate by the Faculty of Law. Fischer then adds the biting remark that "At the University of Vienna even Sigmund Freud got no further than a professor extraordinarius."<sup>1</sup> (Fischer 1974: 138) (In the German university structure a professor extraordinarius is not a very powerful prestigious position.) In 1897 Freud had been in the junior position of privat

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1. On this point Fischer is not altogether correct since Freud did become professor ordinarius in 1920. Yet it is also true that in practice Freud never had the full status normally associated with such a position - see Jones 1953: Vol. 1, 375.

dozent for the extraordinarily long time of twelve years. In fact, he had considered leaving the University, having been passed over for promotion so many times in favour of younger people. As Freud's biographer, Ernst Jones, write:

"The anti-semitic attitude in official quarters would have been decisive in itself but Freud's reputation in sexual matters did not further his chances. Against these considerations, the splendid work he had done in neurology, and his European standing as a Neurologist, counted for nothing." (Jones 1953: Vol. 1, 373)

In fact, Freud eventually obtained his post of professor extraordinarius through the system of 'Protektion' which was rampant at that time.

Frau Gomperz, a former patient of Freud's, and the wife of the famous philosopher, Theodore Gomperz, had already tried unsuccessfully on Freud's behalf to persuade the Minister of Education to grant him a professorship, but it was left to another patient, Marie Ferstel, to secure Freud's appointment by giving the Minister a famous painting that he badly wanted." (Jones 1953: Vol. 1, 374) Far from supporting Rosenmeyer's contention then, Freud's case can be seen as very much the exception rather than the rule.

Further, light has recently been thrown upon the anti-semitism among university staff with the publication of the notes that the Minister of Education, Czernac took during a meeting of the 'Spann Circle' called in 1925 to discuss limiting the number of Jews at the university. The transcript not only provides interesting evidence of the anti-semitism among some staff but also of their anti-liberal feeling and of the relation between the two. For, as Michael Siegert writes:

"Den Feind nannte man Die Ungeraden, ein Geheimwort für Juden, und der Jude wieder war ein Deckbild für den Liberalen" (Siegert 1974: 1)

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1. "The enemy has been called the 'ungeraden', (odd or non-conformist) a cover word for Jews and the Jew was a cover picture for the liberal." (my translation)

The guiding light of the 'Spann Circle', Othmas Spann, was the main professor of economics at Vienna, but because of the university's structure at that time also dominated sociology. According to Czermac's notes, Spann attacked his fellow professor of economics, Mayer, for supporting the 'habilitation'<sup>1</sup> of two Jews - Wiesser and Schlesinger - and for liberal tendencies within his own teaching. In the light of such evidence it is difficult to hold that there was little or no anti-semitism among university staff. Indeed, not only was it present but present at a very senior level indeed. Certainly, in Lazarsfeld's case, anti-semitic feeling within the university did form a barrier to his academic career, and was an important factor in his decision to emigrate to America.

Anti-Jewish demonstrations and attacks were greatly facilitated by the autonomy of the university<sup>2</sup> which allowed anti-semitic students to beat up Jewish and socialist students with impunity from police interference. Martin Freud, writing about the situation just prior to the first world war comments:

"At high school I had been used to Jews being a large minority, but this did not apply to the university of course. The students came from all parts of the vast Austrian empire, those from the German Alpenlander being convinced German nationalists. They were

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1. Within the German university structure, especially so during the period under discussion, habilitation was a necessary condition for becoming a dozent (lecturer). In order to habilitate one had to submit advanced post doctorate work. In short, it was an advanced degree, but it was also essential to have the sponsorship of a professor.
  2. Ironically, this privilege had been bestowed as a reward for the students', and particularly the Jewish students', participation in the liberal revolution of 1848.

organised into societies - Burschenschaften - mainly on the pattern of the German universities. It happened often enough that they broke into lecture halls shouting 'Juden Hinaus' and kept it up until the Jews and the very few Jewesses had gathered up their books and filed out in despondency." (Freud 1967: 206, 207)

Koestler gives a rather similar account of his days at the University of Vienna. He comments that about half the students wore colours; that is, they belonged to one of the duelling fraternities (Burschenschaften) which were a relic of medieval times.<sup>1</sup> Every Saturday morning they would parade around the enormous entrance hall of the University challenging other Korp to duels. Although the socialists never organised themselves into Burschenschaften, the Jews did, rapidly acquiring a reputation for both enthusiasm and skill. In response, the Pan German students, possibly fearing this skill, passed the Weidhofen resolution ruling that, because Jews were devoid of honour, no satisfaction could be attained by duelling with them. As a result the traditional form of institutionalised violence collapsed and the University of Vienna became the scene of a series of bloody and indignified riots. Martin Freud gives an eye witness account of one of these disturbances at the University:

"One day when I arrived at the University, the entrance was cordoned off by police and I could not get in. The police were there because a gang fight was going on between the balustrades which edge the two broad sloping approaches to the University. The Adversaries were German-Austrians and Jewish students. They fought with sticks and fists." (Freud 1967: 207)

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1. Like the Wandervogel and Burschenschaften were incompatible with the Fascist State. As Koestler comments: "These strange fossils retained enough pride and independence to make them incompatible with the structure of the Fascist State, and during my time and in the middle twenties they still dominated the scene of the University of Vienna." (Koestler 1955: 114).

Failure of the Republic: Postscript of a Movement

The final act of the Republic was played out amid the rising tide of European Fascism; however, the Fascists' progress in Austria was delayed longer than it was in either Germany or Italy due to the existence right into the 'thirties of an organised working class. For, although inroads had been made into that class, its political organisation and support was still basically intact. Upon Seipal's death, the task of breaking this organisation, both in spirit and power, fell to Dollfuss.

On May 27, 1932, the Austrian Parliament approved Dollfuss' government which had been formed with the assistance of the Heimwehr. However, its majority of one was insufficient for the task which lay ahead, especially since the provisional elections in Vienna, Lower Austria and Salzburg had already shown that a third of the voters had defected to the National Socialist camp (Buttinger 1953: 3), a trend which could be expected to be continued in the forthcoming elections to be held in May. In consequence, on 15th March, Dollfuss abolished representative government entirely on the excuse that since Parliament could not formally be adjourned owing to the resignation of all presiding officers, the calling of a new session was illegal. Following the example set by Mussolini, Dollfuss collected most of the important ministries in his own hands, and THEN pushed forward the attack against the Social Democrats.

Elizabeth Schilder related the feeling of impending doom to the writer as follows:

"I was convinced that the fight was coming. After 1932 I knew the fight had to be fought. I had been in Berlin - I had had contact with the Germans - I was a member of a left group. We knew the fight was coming and we thought we could do something, but we wanted to fight earlier. We always asked Otto Bauer to fight, to fight." (Schilder, 28.6.74)

The party did not fight on the 15th when Dollfuss had staged his coup, always believing that avenues for negotiations were open - "We still believed we could come to a peaceful solution through negotiation".<sup>1</sup> Ill-prepared and ill-armed, the party fought on Monday, 12th February, 1932, having been forced into a confrontation with the State by Richard Beneczech, Party Secretary and Commander of the Lower Austria Schutzbund. He had warned Bauer on Sunday, 11th, that: "if attempts at searches for weapons were made on Monday, in any town of Lower Austria, or if any functionaries of the Party or the Schutzbund were arrested, armed resistance would be offered and the offensive taken as soon as possible." (Gulick, 1948, Vol. 2, 1278). In response to a provoking arms raid, the Schutzbund of Linz in Lower Austria rose, forcing the Party to take action. Of the 20,000 members of the Schutzbund who reported for action in the Party labour settlements, and other such pre-arranged meeting points, only 10,000 saw combat in the three days of fighting that were to follow. The Schutzbund fought in isolated groups without coherent plan and without adequate communications. But the relative ease with which the uprising was defeated was due primarily to the failure of the general strike.

Konig, President of the Railway Workers had on the 12th voted against the call for a general strike, fearing that his workers would not respond to the call. (Fulik, 1948, Vol. 2, 1281). Events proved his fears well-grounded. In fact, the railway workers had some six months previously joined Dollfuss' Fatherland Front (Buttinger, 1953: 75), illustrating

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1. Other Bauer quoted: Gabik, C.A., 1948, Col. 2, 1351.

some of the disintegration that had already set in within the party.

If the immediate failure of the uprising can be put down to the half-hearted nature of the general strike and the tactical blunderings of the Schutzbund<sup>1</sup> under the leadership of Julius Deutsch, consideration must also be given to the political strength of the State at that time. For, as Braunthal in a postscript to the struggle writes:

"It is conceivable that a workers' mass party imbued with an ardent fighting spirit might under certain conditions resort to civil war. But it will triumph only against the government whose power has deteriorated and whose armed forces have become unreliable."  
(Braunthal 1945: 281)

Neither of these conditions applied in Austria. The State was not weak and the Social Democrats were not "imbued with an ardent fighting spirit". The Party's rank and file had become increasingly disheartened with the Party's refusal to take a stand against Dollfuss, and watched in despair as its power was eroded. (Braunthal 1945: 280) The crux of the problem was that the Social Democratic movement was in its tradition, spirit and structure no instrument for insurrection.

Tragically, the party in defeat became much more radical and in many ways revolutionary than it had ever been when legal. For a time it even changed its name to that of Revolutionary Socialists and turned its back upon its former democratic traditions. After the failure, such was the revolutionary spirit within the party that

"the Communist Party of Austria vegetating hopelessly in dark corners until 1934, got its only real boost from the February upheaval. The collapse of social democracy aroused curiosity about the Communist message. The doubt that suddenly seized workers, their need of comfort and their outrage at the disappointments, combined to turn a mentally and politically insignificant handful of Communists overnight into a political movement." (Buttinger 1953: 263)

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1. Prior to the uprising the Schutzbund's armoury had been severely depleted by a series of successful police raids. However, of greater importance in the resulting defeat is the fact that during the opening hours of the battle the police managed to capture known key individuals responsible for the distribution of weapons. Thus, now knowing the exact location of the hidden weapons, many of the Schutzbund remained unarmed throughout the entire battle.

The splintering into groups, the growth of suspicion and hostility, by-products of their new underground role were problems, but none so great as the distancing from masses where the real power had lain. It had been a party based on openness, a party used to running the lives of thousands of members, but now its gigantic structure or organisation was shattered, leaving the mass of its followers, so used to taking directives, without either leaders or a structure through which directives were received. Although valiant underground work was carried out, the party was effectively finished, especially after the National Socialists assumed power. They routed out the socialist underground workers with far greater thoroughness than the Austrian Fascists had ever done, and as part of the underground network, the Forschungesstelle fell victim. Buttinger tersely writes:

"Late in November 1936 the police raided the so-called Office of Economic and Statistical Research; besides the Director, Mitzi Jahoda, and part of her staff, they caught Fritz Jahnel, the Information Chief who was in contact with Mitzi Jahoda."  
(Buttinger 1953: 374)

Marie Jahoda was a natural candidate for suspicion, having been a leader in the socialist youth movement, and still lived in the Karl Marx Hof where she had moved on marrying Lazarsfeld. The Karl Marx Hof was the pride of the social democrats' building programme and scene of some of the bitterest fighting during the February days. The howitzers of the government forces were directed down upon it from the commanding heights of the Hohe Warte (a football field), a fact which tends to give the lie to assertions by individuals such as Dr. Joseph Schneider that such schemes had been built as socialist fortresses within Vienna rather than as municipal housing projects. The most simple-minded military tactician would not have built a fortress under the Hohe Warte; however,

it is true that arms had been dumped in the cellars and roofs of many of the buildings in expectation of armed struggle. If the tenement buildings had been constructed as a belt of fortification to seal Vienna then the Social Democratic Party was not the reformist party described in this chapter. After the February struggle, Jahoda became active in the underground movement. As she related to the writer:

"I still lived where I had lived with Paul in the Karl Marx Hof, in a community building. The cannons of the government were directed against this. Life changed in its total quality fantastically. Also, I became immediately quite active in the underground and I was arrested in the Forschungsstelle in 1936. The whole atmosphere was terrifyingly changed." (Jahoda 26.9.73)

Dr. Wagner, who left for England in the early part of 1936, owing to the impossible position that her husband now found himself in since he had been a journalist on the Arbeiter Zeitung, related in conversation with the writer the situation and atmosphere within the Forschungsstelle at this time:

"Mitzi Jahoda who was always a very ardent socialist and very politically-minded used the Forschungsstelle for the underground movement as a cover and all the letters ... many of the letters which came to it ... well, she used it for that purpose. I think people there knew about it and agreed with it too. I wasn't there any more but our friends were, and the socialist movement was a hundred times more important than the Forschungsstelle." (Wagner 9.8.73)

Following the police raid on the Forschungsstelle, Marie Jahoda was jailed for ten months and then released on the condition that she leave the country immediately. After the capture of Jahoda, the Forschungsstelle can be said to have ceased operations, although it did struggle on under the directorship of Hans Zeisel until it was finally closed by the National Socialists in 1938. This was the end of the Forschungsstelle. Its beginnings, however, form the subject of the next chapter.

## CHAPTER TWO

### "THE VIENNA FORSCHUNGSSTELLE"

CHAPTER TWOTHE VIENNA FORSCHUNGSSTELLESociology in Austria: The Absence of Empirical Research

"The development of sociology in Austria can only be rightly understood if one shows that in the central European sphere, after interesting speculative and theoretical beginnings at the turn of the century, it lasted more than a quarter of a century before empirical research really developed which today forms an insoluble reciprocal bond with theory. Neither in France, nor in Germany, Poland or in the United States was this relationship missing to quite the same extent as in Austria. Nevertheless, empirical sociology which had its beginnings in Vienna around 1930, particularly through the work of Paul Lazarsfeld has enriched and helped methodologically improve American social research, and thus had its influence in the United States and far beyond."  
(Rosenmayer 1966: 12)

Given the obvious importance of Lazarsfeld's empirical work to the overall development of sociology in Austria, it is somewhat surprising to find that a major history of Austrian sociology between 1918 and 1938 contains no reference to him (Knoll 1959).<sup>1</sup> Moreover, where Lazarsfeld's work during this period is noted, as for example by Rene Konig (Konig 1959) it is misrepresented. Discussing the relative ease with which Lazarsfeld was assimilated into American sociology for example, Konig writes:

"Incidentally, we would like to mention that for most of the Austrian colleges the situation in this regard was quite different (to the Germans) in so far as the Austrian tradition, both in social science and in methodology are completely incomparable with the German conceptions in this field. Therefore the Austrian refugees and immigrants did not have to undergo the same

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1. Ferdinand Westphalen (1953) in what is otherwise a good historical overview of Austrian sociology also fails to mention Lazarsfeld's work.

changes as the German refugees. On the contrary, the continuity of their work has remained extraordinarily strong. This is the case for Lazarsfeld, Marie Jahoda, Hans Zeisel and others from the same group. Since their first report on 'The Unemployed of Marienthal', from 1932, they have indeed contributed in the most impressive way to the development of empirical research in sociology. But this achievement was, for them, much easier to attain, since the Austrian logic of science had always been very different from German philosophical logic, in science in general and in social science as well." (Konig 1959: 781-782)

Konig's view of the situation is based on the fundamentally mistaken assumption that Lazarsfeld's work was representative of the mainstream tradition within Austrian sociology. In actual fact, however, it constituted a radical departure from this tradition. Indeed, the Forschungsstelle stands out as a lone outpost of empirical sociology within the Austrian university system. In fact, although Max Adler held a chair of sociology at Vienna, and Max Weber gave visiting lectures, overall sociology was very underdeveloped as a discipline, and what little there was was very heavily weighted towards theory. According to Knoll, the major figure within Austrian sociology was the economist Othmar Spann, who was, as Konig points out, a grand theorist and an implacable opponent of empirical research. In the course of discussing the search for sociological theories capable of encompassing the totality of social being,<sup>1</sup> and the corresponding denigration and dismissal of 'middle range' theories, Konig notes that

"One of the most extreme cases of this attitude is to be found in Othmar Spann and his organic universalism. For him, any attempt to do empirical research conveys the attribute of 'individualistic', the most severe condemnation in his vocabulary." (Konig 1959: 785)

Although Austria had a lengthy tradition of work both in sociology and in statistics, there was no collaboration between the two despite

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1. For a discussion of Othmar Spann and his school see Landheer, 1958.

occasional calls for reprochement. In 1912, for example, a lecturer in statistics at Vienna, Franz Zizek, had published a book called 'Sociologie und Staistik' in which he lamented the lack of co-operation between the two disciplines and proposed that future collaboration should be a main priority for both disciplines. However, as Rosenmayer notes:

"Despite all this, another 15 years were to elapse since the publication of his work until the first conscious application of statistical methods in sociology. Not before the late 1920's did the interest in quantitative analysis of social problems which were studied with specially devised methods, come to the fore."  
(Rosenmayer 1965: 267, 277)

In the meantime, sociology remained fragmented and underdeveloped as an academic specialism. Earlier, however, the prospects of establishing a strong university discipline had looked distinctly promising. Wilhelm Jerusalem's book 'Sociologie des Erkennes' published in 1909 has been greeted with widespread acclaim within the emerging European sociological community, including a favourable review by Emile Durkheim in the *Année Sociologique*.<sup>1</sup> With the devastation of World War One and the ensuing disintegration of the Austro-Hungarian empire, however, the demand was for approaches which would contribute towards illuminating and solving the multitude of social, economic and political problems with which Austria was then faced. Clearly, "These problems could no longer be solved with speculative attempts like those of Gumplowicz and Jerusalem" (Rosenmayer 1965: 276), and Austrian sociologists' failure to respond to the changed situation by adopting empirical methods as part of their procedures, severely restricted the expansion of sociology as a university discipline.

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1. See Rosenmayer 1965: 275

Although the social irrelevance of speculative sociology, when faced with the new states problems, undoubtedly was an important factor in limiting the development of sociology, it must also be noted that financial restrictions also militated against any move away from speculative and theoretical work. The financial difficulties facing the new state were reflected in the universities' extreme impoverishment:

"The financial situation of the university of Vienna was in a critical state, and many professors could hardly exist on the miserable salary they received. An appeal was made to those who had amassed fortunes during and shortly after the war, and had so far done little to help their country, to come forward and assist the University." (Bullock 1939: 105)

Such financial impoverishment hit the natural sciences most heavily and it became impossible for them to keep abreast of new techniques and developments. One effect of this was that the more prosperous German universities acted as a magnet for the more able scholars. Arthur Koestler, in discussing the controversy surrounding Paul Kammerer's work on genetics, mentions the financial difficulties faced by the Institute where Kammerer worked. The Austrian Krone, which originally had been worth the equivalent of one Swiss Franc, had by the end of 1920 fallen to the value of one centime. By the end of 1921 it required 1,400 Kronen to buy one Franc, and in 1922 roughly 14,000 Kronen were needed. Koestler writes:

"Within a few months the Kammerer's family fortune had melted away. Worst hit, of course, were the white collar workers, including academics, reduced to starvation salaries. Kammerer's amounted, in 1923, to the equivalent of £150 a year ...

Hans Przbaum ... had founded and financed the biological institute largely out of his own means until the Austrian Academy of Science took it over in 1914. After the war he wrote pathetic letters to his one time friend Bateson offering rare books from his private library for sale in exchange for British Scientific publications which the Institute could no longer afford to buy." (Koestler 1971: 82)

The financial situation was so desperate within the University of Vienna that during the particularly cold winter of 1921-22 the University had to be closed down because of lack of heating, and the roof of the University had not been repaired since 1914 and could not be attended to.

Consequently, even if a bridge had been constructed at the intellectual level between sociology and statistics, the possibility of turning that into empirical work was very remote given the financial situation within which the universities had to operate. Thus, it is of special significance that the Forschungsstelle was established outside the University of Vienna and operated with the assistance of private commercial contracts.

#### The Organisation of the 'Forschungsstelle'

"One of the permanent achievements of the 'Forschungsstelle' was to make the act of buying academically 'acceptable', and to have raised market research to an academic discipline."

This work was fruitful, and, after a while, was to become fruitful too in Austria where there is today a powerful, promising tradition of empirical sociological research.

In the United States there are today a few sociological university institutes (the first was founded by Paul Lazarsfeld at the University of Columbia), all of whose predecessors was the Viennese Forschungsstelle" (Zeisel 1969: 46)

Three interesting points emerge from the above quotation; 1) the contribution that the Forschungsstelle made to the raising of market research to the level of an academic discipline, 2) the contribution of the Forschungsstelle to empirical research within sociology, and

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1. As evidence for this, Zeisel presents Lazarsfeld's "The Psychological aspect of Market Research" which appeared in the Harvard Business Review of 1934, and a number of chapters which appeared in 1937 in "The Techniques of Marketing Research", American Marketing Society.

3) the contribution that the Forschungsstelle made to a style of research organisation.

It is intended to examine the latter point first, so that, once having outlined the actual organisation of the Forschungsstelle, a basis is provided upon which the other two points, as well as additional ones, can be dealt with.

At the time of founding the Forschungsstelle in 1925<sup>1</sup> Lazarsfeld was an 'assistant' at Vienna University psychology institute. The Institute itself was comparatively new, in the sense that until the appointment of Karl Bühler in 1923 as head there had been, according to Lazarsfeld, "no real psychology" at the University. (Lazarsfeld 1964: 274)

In fact, Karl Bühler had been appointed for the specific purpose of developing psychology as a discipline, his wife, Charlotte Bühler, being appointed as associate professor and placed in charge of the administration of the institute. The Böhlers are particularly important in relation, not only to the Forschungsstelle, but also to Lazarsfeld's career within the University, for, to gain a university position of any standing, it was necessary to be sponsored by someone. In fact, one needed the support of the professor since, in practice, one worked for him rather than the faculty. Thus, it was only with the professor's sponsorship, and the faculty's agreement, that one could be appointed a dozent upon 'habilitation'. This fact has great importance for the path of Lazarsfeld's academic career and ultimate emigration to America. An

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1. Lazarsfeld (1969: 274) dates the establishment of the Forschungsstelle as 1927, but Zeisel (1968: 8) dates it 1925. Zeisel's date is more likely to be correct since he was renowned for his record keeping. Although "not quite sure about the formal date of establishing the 'Forschungsstelle' Marie Jahoda informed the writer that "the difference between 1925 and 1927 may well be the difference between the original idea and some preliminary work on the one hand and the more formal incorporation on the other. I am, however, quite certain that some survey work was already in progress in 1926." (Jahoda letter 19.5.75)

examination of the Rockefeller Foundation's Fellowship records on Paul Lazarsfeld reveals under the heading 'Prospective'; "Will probably also be appointed Privatdozent at University of Vienna".<sup>1</sup> Lazarsfeld never was appointed, but for the present it is sufficient to note the position of the young scholar within the Austrian University system, and his dependence upon the professor for promotion.

During the course of conversations with various people who had been contented with the Forschungsstelle, it became apparent that their knowledge of its actual legal position was very hazy. Even Lazarsfeld did not seem to know. In a discussion that the writer had with Professor Lazarsfeld, he related the position of the Forschungsstelle as follows:

"The situation was that in order to do empirical studies, you needed a machinery and you needed money, and so I obtained permission and there was really no resistance to the idea. I obtained permission from the professor to create this kind of unofficial little Forschungsstelle which was not part of the University. I don't know what its legal position was. But Bühler, who was head of the psychology department, was also head of this little Forschungsstelle" (Lazarsfeld 25.5.73) (my italics)

This confusion concerning the legal position of the Forschungsstelle is indicative of its innovative nature and throws into relief the whole question of the position of such institutes within university structures in general. For example, Lazarsfeld faced a similar situation in America over the legal relationship of the 'Bureau' to Columbia University. The problem was that such organisational forms of research did not fit easily into existing university structures. Frederick Ogg, in his report to the American Council of Learned Societies, highlights a consequence resulting from the lack of administrative forms with which to accommodate such institutes when he writes that:

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1. The Rockefeller Foundation's policy was to destroy all its student records, and since this particular record escaped destruction it was kindly given to the writer for his own files.

"... to an astonishing extent this newly organised research finds its habitat outside the traditional centres of productive scholarship, that is the universities, and the problem is suggested of what this means in the future, both for the university and for the quality and effectiveness of research itself." (Ogg 1928: 156)

One of the difficulties faced by Lazarsfeld, both in Vienna and New York, was that both of his institutes derived part of their funds from private commercial contracts, a situation which generated not only legal problems so far as financial arrangements were concerned, but also psychological resistance from traditional university academics over the question of 'correct' procedures of scholastic activity. This whole question of 'correct' procedures of work will be returned to later when the 'Bureau' is fully discussed, but for the present the following conversational extract with Marie Jahoda illustrates the academic reaction to commercial contract work which Lazarsfeld had to contend with in Vienna:

#### D. MORRISON

The empirical side of the social sciences obviously requires more money for its operation than it did during its pre-empirical days. Was that one of the reasons why the Forschungsstelle was never really integrated into the University ... because it could never afford such a heavy financial burden, or was it more that the University was just so conservative?

#### H. JAHODA

Yes, I think it was. It was both. You see the market research studies were in principle meant to be profit making, not for an individual but the institution. The University in those days felt that really that was quite shocking. You know that we could do work that one gets paid for and use the money for something else. It was, I think, the conservatism of the University. There was a liaison with the University, but it definitely was not part of the university and I think it was the market research that led to the impossibility of making it an integral part of the University.

D.M. That was the reaction of certain academics to it .. I'm thinking particularly of the psychologists.

M.J. Well, there was the two professors, Karl and Charlotte Bühler. Now Charlotte Bühler really got an enormous benefit for all her own work from Paul. You know, he was the only mathematically

sophisticated person. You see, his degree is in mathematics - and she did a lot of interesting work on children and young people and began to see that numeracy in psychology was really very important, but she hadn't been trained that way. So Paul did really an enormous amount for her and I'm quite sure she appreciated it and if she could have established him in a tenure position, which didn't exist for anybody apart from a professor, she would have done it, but she was quite an arrogant person. She exploited Paul as much as he let himself be exploited.<sup>1</sup> She was very good for him and his general career, but I think in the end it suited her to have him available at the University as a Dozent (sic) and do, his quote, "his dirty business" not quite close to her."  
(Jahoda 26.9.73)

However, the uncertainty concerning the legal position that the Forschungsstelle occupied in relation to the University is clarified by Hans Zeisel. According to Zeisel, its legal position was that of a "club under the leadership of Karl Bühler and the auspices of prominent personalities of the city". (Zeisel 1969: 45) The legal responsibility then resided with a board of "prominent personalities of the city", yet it would appear that particular responsibility rested with Karl Bühler since he was only the head of this council but also the link connecting it to the University. Bühler's responsibility is evidenced in his concern over the financial chaos that Lazarsfeld left the Forschungsstelle in upon his 'emigration' to America. Gertrude Wagner related the situation to the writer:

"We tried to keep the Forschungsstelle going when Paul left. I tell you, it was full of debt. We had a terrific amount of debts when he left. So Bühler came along and was very worried indeed as he was connected with it."<sup>2</sup> (Wagner 9.10.73)

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1. It is interesting that Lazarsfeld in his 'memoir' should also raise the question of Charlotte Bühler's exploitative nature: "She had a Prussian ability to organise the work activities of many people at many places. Some felt exploited by her, but I always appreciated her good training and help." (Lazarsfeld 1969: 296)
  2. In fact Lazarsfeld himself mentioned that when he was in America he received numerous letters from the Böhlers, "cursing me for the financial embarrassments I had created for them." (Lazarsfeld 1969: 310)

Evidently Bühler was only too pleased to relinquish financial responsibility for Forschungsstelle to a rich industrialist by the name of Gold who was made president of the institution upon the acceptance of all the debts. However, Bühler was only one of a very distinguished 20 member board of directors which included not only academicians but important public figures from both government and business circles.

As Lazarsfeld informed the writer:

"We had all kinds of people. People from the right and the left were in there. I don't know whether the name of Oscar Morganstern is known to you? Well, he was always the young man on the right and I was a young man on the left. So we took Morganstern in and so on." (Lazarsfeld 25.5.73)

The board consisted of individuals from the chambers of commerce and agriculture as well as labour union officials, employers, and a large group of university professors. For example, the economists Ludwig Mises and Richard Strigl were members of the board, although they left it after a short while. Otto Potzl, the famous psychiatrist, was a member, as were industrialists such as Hugo Günseler, the president of Bally, Kurt Schechner, president of Julius Meinl, and Manfred Mauntner-Markhof who belonged to a very renowned family of Austrian industrialists.<sup>1</sup>

This board, representing as it did a wide spectrum of occupations and political affiliations gave the Forschungsstelle respectability and acceptance. Below the board level, however, at the actual point of production, the spectrum of political affiliation was not so catholic; in fact, it was composed mainly of left wing associates and friends of Lazarsfeld's.

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1. See Zeisel 1968 for list of members.

D. MORRISON

I know that when you studied the village of Marienthal you used a cross section of politically orientated interviewers. When you interviewed the right you used right wing interviewers and vice versa. Is it wrong then on my part to consider that most of the people at the Forschungsstelle were politically left or did you in fact have a wide political spectrum of individuals?

M. JAHODA

No. We had a predominantly left membership. You see the interviewers.. we had a pool of maybe 30 interviewers. Of course they didn't have regular employment. They were paid by the amount of work, and when we went to Marienthal we deliberately recruited people who could go and talk to the Nazi Party members. But you know ... the unemployment was so high among young intellectuals ... it was such a desperate tragedy that we did what we could to help our friends. So most of the people were good straightforward socialists." (Jahoda 29.9.73) 17.

This whole question of the Forschungsstelle's membership and organisational style was raised during an interview with Professor Lazarsfeld. His account of the organisation and its growth out of the socialist youth movement was then checked against other members' experiences of the Forschungsstelle.

It is worth noting at this point that Lazarsfeld's experiences in the socialist youth movement shaped not only the organisation and style of the Forschungsstelle, but also the organisation and running of the 'Bureau'. For as Lazarsfeld informed the writer: "The formula of this Viennese Forschungsstelle remained absolutely the same whatever I have done since." (Lazarsfeld 25.5.73). However, it is also worth noting that the Forschungsstelle style was not so successful in terms of personal relations when transferred to the American setting. Nonetheless, so far as the Forschungsstelle is concerned, the patterns of leadership and loyalty which had been learnt in the various organisations of the socialist youth movement were easily transferred to form the basic operational framework. According to Hans Zeisel, the organisational structure of the Forschungsstelle was one of an "informal hierarchy with Professor Lazarsfeld as director

and myself as associate director"<sup>1</sup>. This question of organisational style was discussed further with Professor Lazarsfeld:

D. MORRISON

When I talked with Dr. Barton (present Head of the 'Bureau') I asked him if there had been any effective opposition from members of the Bureau staff concerning some of the plans you had for the Bureau. Dr. Barton just laughed and said "What with Paul Lazarsfeld and Robert Merton in the chair?"

P. LAZARSELD

No, but look.. In Vienna when I ran the Forschungsstelle I was 27 but it was just the same. It was a very natural style ...

D.M. But at one point you instructed the staff at the 'Bureau' to learn Spanish. Was your power that complete?

P.L. Oh yes. Well, I always had a very hierarchical organisation. I mean, you know what they call a boy scout master who has his little boys who meet him every week when he trains them and they go and make the good deed. Well, it ... but I've noticed that this boy scout atmosphere isn't natural to you ... it's a very American phenomenon, but you see the socialist youth movement was like that. Look, when I go back to Vienna now, they are all ministers and chancellors and such. They still classify themselves as belonging to this group. It has such power over them. Really, it was a style of work rather than an actual hierarchy. (Lazarsfeld 25.5.73)

It may seem that there is something contradictory in Lazarsfeld's statement that his organisational form was 'hierarchical yet not really hierarchical'. The point is that it was a 'natural style' for him and for his colleagues in the Forschungsstelle since they were nearly all products of the Austrian socialist youth movement. Thus, although there was a hierarchy, there was no need for it to become formalised since the members shared a common background of experiences. In addition, the Forschungsstelle was a very small operation. However, at the 'Bureau' things were slightly different. During the early days of its establishment, the style of operation, due to its smallness and to the pioneering spirit of the venture, was very similar

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1. Personal communication with the writer - letter 8.3.73.

to that of the Forschungsstelle and it was only later, as the 'Bureau' grew in size and became established, that a more formalised hierarchical organisational structure developed in order to overcome various administrative problems. Thus, by separating out the American and Austrian situations, Lazarsfeld's comments on organisational hierarchy do not appear so contradictory. Lazarsfeld always did have a very hierarchical organisational form, but the difference was that in Austria it was never formalised, it was 'a natural style', whereas in America the situation demanded that this hierarchy be given a formal ordering. So far as the Austrian situation is concerned, both Dr. Wagner and Professor Jahoda provided the writer with very similar pictures of the organisation and the atmosphere within it.

#### G. WAGNER

The Forschungsstelle was completely loose. Nothing whatever structural. Paul was so obviously the leader, but only in the way that we all accepted, and he was open to suggestions. It was such a nice atmosphere, but of course he was the leader - he made it. I think everybody liked him." (Wagner 9.10.1973)

Marie Jahoda's account is very similar, but in addition she offers an impression of the shared sense of purpose that existed among the Forschungsstelle workers.

"In a great respect the people who worked at the Forschungsstelle really felt that they belonged to each other. Personal ties were so strong and also the newness of the enterprise - the feeling that we were doing something that no one else was doing in quite the same way." (Jahoda 26.9.73)

Despite the fact that Lazarsfeld had obvious organisational talents and managed to secure the loyalty of his co-workers, the Forschungsstelle was never a stable organisation. However, the distinguished board of directors notwithstanding the Forschungsstelle was plagued from the start by financial problems which constantly threatened its existence.

Although Lazarsfeld was a capable organiser, the fact that he was a poor

administrator compounded the problems stemming from lack of finance.

As Marie Jahoda informed the writer:

"You know, the Forschungsstelle was every day in danger of bankruptcy and Paul was not a good administrator. He was absolutely shocking. You know he paid for an old study with the money from a new study and of course the books didn't add up. He was a very messy administrator." (Jahoda 26.9.73)

Although Lazarsfeld may have been a poor administrator, he was certainly the organisation driving force. In addition, he had the commercial contacts which the Forschungsstelle required for its continuation, a factor which created considerable difficulties for the remaining members when he left for America. The original intention behind the establishment of the Forschungsstelle had been to make money from market research operations in order to maintain the staff and to fund other, more academic, projects. This ideal never became a working reality, however. As Lazarsfeld writes:

"The Vienna centre was a sequence of improvisations, and the basic elements of a research organization developed only slowly. In spite of a number of external formalities, it never fused into a stable organisation. It was only when I came to the University of Newark that the different components, all concurrently in my mind, could be integrated into some kind of an institutional plan." (Lazarsfeld 1969: 287)

The basic problem facing the Forschungsstelle, and one that became common to many of the research centres and Bureaux that were later established in America, was that although attached to the parent university in a variety of ways, it received no significant financial support from them. This point concerning the lack of basic support for such institutes will be returned to later when the 'Bureau' is discussed in relation to its engagement in market research, since this lack of basic support from the parent university is a point that critics of such institutes often tend to overlook. The Forschungsstelle itself never received any money from

the University of Vienna.

After mentioning the shortcomings of his own field trips, Leopold Von Weise write:

"The Vienna enterprise had a different structure where more advanced and theoretically better trained observers could devote themselves to their task for a longer period of time (the participation of medical doctors was helpful) so that scientifically valuable results were achieved, which went well beyond the mere purpose of training students." (quoted in Lazarsfeld 1969: 284)

Von Weise's impression that the 'Vienna enterprise' was 'a well structured and professional' affair was far from the truth of the matter as Gertrud Wagner related to the writer:

"Well, the Forschungsstelle was set up in 1925 in some rooms and Paul started to get orders from firms and we started the market research. We started with nothing. Just two rooms in the flat of a friend and people made the questionnaires and we tried them out - actually quite good they were. Paul got a few co-workers to help - actually we were friends of his." (Wagner 10: 73)

Although the Forschungsstelle did move to larger premises later, the continued sense of 'muddling through' is well captured by the following extract from an interview with Marie Jahoda:

"The real idea that developed in 1925-1926 was to do market research in order to finance research of a different kind and there was always market research and other studies going on parallel. But really it was such a terrible mess - nothing ever really worked out right.

I remember - in the early days I still had to go to school. I was not even then at the University and was therefore in the early years just an occasional visitor, but I remember very clearly.. this must have been some time in 1926.. how I got to do my first bit of empirical social psychological writing. I came just as a visitor one day to the Forschungsstelle and found Paul and Zeisel and everybody in a terrible mess because they had done a market research project which had been due two weeks ago. There I was, I came in wanting to gossip, but no. Hans Zeisel took five papers which I remember distinctly. It was about men's clothing, and Zeisel said "Sit down and write a chapter on this." So I sat myself down, not knowing anything about it, you know, apart from being familiar with the general ideas. I took the tables and interpreted them. You know, this research production - never having quite enough money, never being quite able to do everything one wanted and using every

opportunity. That was how it was. That was the kind of atmosphere that prevailed in the Forschungsstelle." (Jahoda 26.9.73)

Thus, the basic financial support for the Forschungsstelle came from commercial contracts, and in the case of the 'Marienthal' study a grant from the Rockefeller foundation in addition to a small subsidy for expenses from the central trades council. Although the Rockefeller grant was only small, it did bring Lazarsfeld to the attention of the Rockefeller authorities, who in 1932 awarded him a travel fellowship to America, which he took up in 1933. The precarious financial basis to the Forschungsstelle's operations is well described by Hans Zeisel:

"Before leaving America I asked Mrs. Ullman, who today works for Marplan in the United States, what memories she had of those years gone by. She answered me: 'I remember coming to see you once to tell you I had not received my pay for four weeks'. To which I apparently replied ... 'Ah yes, four weeks isn't such a long time after all' - I fear that the only thing we paid regularly was the coffee of our collaborators who worked on their labours in the depths of a Viennese coffee cellar. I'm sure that we never gave them any more than this because at the end of the month those among us that had a second job (for example, I was under instruction as a lawyer in my father's firm) had to put in a part of their salary." (Zeisel 1968: 9)

The original idea that the commercial contracts would furnish the Forschungsstelle with sufficient money, not only to pay the salaries of those involved, but also to produce a surplus with which to fund other studies never really worked in practice. For, if it was difficult enough just to sell market research to businessmen who had never met such techniques before, then the difficulty was compounded through the general impoverished state of the business world. In addition, given the novelty of such an institutional form of research, the skills necessary for costing projects were absent. During an interview with Gertrud Wagner, for example, she mentioned that:

"The money was never really there for what we wanted to do. I don't think Paul calculated very well the cost of things. In order to get

contracts he took the money he could get. It fulfilled the purpose in that people were paid for their manual work, and it paid for paper etc., but it never really brought money in. It never paid him a decent salary and also this Marienthal - there was never enough money. The money that was there never served to pay Lazarsfeld but to pay his taxis, because he had hundreds of visits to make, and he always took a taxi. People always used to laugh at that." (Wagner 9.10.73)

Yet, despite the difficulties of accommodating to what was basically a new role within academia; that is, the role of the 'managerial scholar', Lazarsfeld's plans for institutional forms of research stretched beyond Vienna. It was, according to Wagner, Lazarsfeld's intention to establish a similar institution in Germany in 1932 but due to the lack of demand for their work the project became financially untenable. Nevertheless, studies were conducted in Germany before 1932 and in Switzerland in 1935. Although Lazarsfeld left Austria in 1933, the Swiss contract had probably come from a Mr. Gold who lived in Zurich and with whom Lazarsfeld had made contact with before leaving for America. It was this central position which Lazarsfeld occupied in the affairs of the Forschungsstelle which made for difficulties in the running of it once Lazarsfeld had departed. As Wagner informed the writer:

"We found a man, a fairly rich man. I think his name was Gold. Yes, I'm nearly sure a Mr. Gold. He came along and he was fascinated by Lazarsfeld. I don't know where they met, but he met him and he was fascinated by the idea of market research. He had made a lot of money in some business and was rich. He lived in a Hotel in Zurich - one of the famous ones, I forget which. When Lazarsfeld left he said he would become the head and in order to become the head of the Forschungsstelle he had to pay all the debts. So, everybody was very happy. But problems then arose because Lazarsfeld had always written the reports. He had always written them himself and therefore when he left there were really difficulties. I know I was suddenly confronted to write reports and I certainly couldn't do it. Of course, Gold was very disappointed because we were left to him as the

people who could do it and so he was disappointed that he had paid the debts." (Wagner 9.10.73)

Dr. Wagner's difficulties were due not simply to personal failings, but were also indicative of the extent to which traditional forms of intellectual training failed to prepare people for the kind of tasks generated within a research organisation such as the Forschungsstelle. When Lazarsfeld was in charge of the Princeton Radio Research project, at Newark, the inappropriateness of traditional training again became apparent. A new set of skills had to be learnt by the staff, which meant that in the meantime Lazarsfeld shouldered the bulk of the work, particularly the writing, using the pseudonym Elias Smith.

"So far as the pseudonym goes, well, I became director of the Princeton project and had to establish again a staff of young people. They didn't know how to write and I wanted to give the impression what a big group we were, so I wrote most of the things. But I didn't want to have them all to my name so I just invented the pseudonym to hide how much of it was a one man operation." (Lazarsfeld 25.5.73)

This new type of research organisation, with its division of labour, group work and hierarchical structuring of relations which is now a familiar part of academiç, grew out of the application of empirical knowledge to the social sciences. That is, the new forms of knowledge generated new organisational forms to accommodate them, which in turn required the establishment of new roles in scholarship. It is now proposed to examine the factors that contributed to the development of this new form of research establishment.

#### Paul Lazarsfeld: A Brief Intellectual Portrait

To understand the development of the Forschungsstelle, it is first necessary to know something of Lazarsfeld's own development. The intention is now however to trace in detail his specific intellectual

contributions to the world of learning, or to examine his work as a continuation, development or break with that of other thinkers. Such an exercise must remain outside the scope of this work. Rather the aim is to trace the development of his general orientation towards empirical social research for, in the final analysis, it is this general orientation which is the most significant factor in understanding his pioneering role in the development of institutionalised research. So far as the development of research institutes are concerned, theory followed practice since Lazarsfeld's views concerning the role of institutes in the process of research only developed after he had arrived in America and had the time and opportunity to reflect upon his past experiences.

For a person such as Lazarsfeld, coming as he did from a middle class Jewish intellectual background, entry into a university was a matter of course. Lazarsfeld's father was a lawyer by profession and his mother was very well known in Austrian socialist circles.<sup>1</sup> Among her friends was the mathematician and physicist, Friedrich Adler, who has already been mentioned in connection with the encouragement that he gave to Lazarsfeld when he was studying mathematics at school. Nevertheless, despite the many intellectual figures visiting the Lazarsfeld household, his intellectual development owes more to general intellectual socialist milieu to which he belonged than to specific contacts with particular

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1. Lazarsfeld's father, Dr. Robert Lazarsfeld, was author of a book on Jurisprudence, "Das Problem der Jurisprudenz", Wien 1908. In addition, his mother, Sofie Lazarsfeld, had written a book based on knowledge gained whilst working in one of the marital advice bureaux fostered by the Social Democrats (Rhythm of Life: A Guide to Sexual Harmony for Women, London 1934).

individuals or philosophical schools. Although Lazarsfeld was not uninterested in the philosophical basis of empirical methodology, his work ultimately rests on a deep personal fascination with the techniques and procedure by which research is accomplished. An interesting anecdote was told to the writer by Lazarsfeld and then used for the basis of a discussion concerning his whole interest in empiricism. Evidently, every Christmas the graduate students at Columbia gave concerts in which the members of staff would be portrayed. One year Lazarsfeld was depicted as a would-be father, and at one point "a student steps forward and asks me 'Is the child a boy or a girl' and I say, 'I don't know, I'm only interested in the method'". Lazarsfeld followed this story by saying:

"I know that is hard for you to believe. This interest of mine in the process of research, or any kind of research as a question of procedure has been since my student days so dominant. It has always over-shadowed any substantive part of what I did you see."  
(Lazarsfeld 15.6.73)

This interest in technique is of paramount importance when later considering Lazarsfeld and his role in mass communications research. It is paradoxical that, as a 'founding father' of mass communications research, he was only interested in the research and never in mass communications as such. In fact, he was only interested in mass communications research in so far as it provided him with a ready vehicle for his methodological concerns, and once the field was, in his eyes, methodologically exhausted, he moved on to other areas of interest, concentrating initially upon methodology itself and the perfecting of his work on latent structure analysis.

Although Lazarsfeld lived in Vienna at the same time as the 'Vienna Circle' flourished, it should not be imagined that they were in any way generic to his methodological interests. He certainly knew

them, but he was already lecturing as an assistant at the psychology institute before he made their acquaintance. As Lazarsfeld informed the writer:

"Well, I knew them all, but it would be physically wrong to see them as .... you know, I met them later when I had already given those courses in statistics at the University. But the prestige of mathematics among those charismatic Austro-Marxists like Bauer - that certainly played a role. (Lazarsfeld 19.5.73)

During the course of another interview the writer had with Lazarsfeld, the high status of mathematics among certain socialist circles was mentioned again.

"You see, in the socialists in Vienna there was a sub-set, and we were very influenced by the importance of mathematics .. the sub-set of Friedrich Adler. So there was a convergence of social science and some kind of mathematics.. it could easily be Wittgenstein and positivism, or it could be econometrics or relativity theory and Mach. Mathematics had a great prestige." (Lazarsfeld 25.5.73)

Within the intellectual milieu of Vienna mathematics occupied a very prestigious position; one was in addition highly developed as an academic discipline. Yet, when Lazarsfeld first arrived at the University of Vienna he did not immediately specialise in mathematics, but rather enrolled for a variety of subjects, in particular staatswissenschaft. The course of staatswissenschaft is possibly best translated, when seen in terms of its cultural context, as state-craft, although its modern equivalent would be political science. It combined law with economics and political theory, and in many ways provided a basis for entry into the civil service.

"One term I was enrolled in a social science course and then the next term I was doing staatswissenschaft and then sociology when it came to Vienna, and the next term I was enrolled in mathematics and so on. But I didn't do much work in any as I was so politically active." (Lazarsfeld 25.5.73)

In discussing Lazarsfeld's student days, Berta Karlick confirmed that Lazarsfeld's political interests prevented him from doing much academic work.

"I think he was more interested in social problems than in studying physics and mathematics." (Karlick 9.8.93)

Gertrud Wagner provided the writer with additional information on Lazarsfeld's work as a student:

"Paul studied physics and mathematics because he was a very bright young man - what would he choose? - he didn't know exactly what to choose. I think he wanted to become a teacher. You see, he was an ardent socialist with educational interests and he wanted to become a teacher. So what subjects should he choose? Well, if he is so bright and he has good marks in physics and mathematics, why not them? As a matter of fact he really left physics and mathematics as his field of interest in the middle of his study and he only ... it took him one or two years longer than it needed ... he only finished it because of his mother. She was a very strong person and insisted that he finished it off. (Wagner 9.8.73)

Lazarsfeld eventually graduated in mathematics, but as he writes in his 'Memoir' that after several years of studying mathematics and staatswissenschaft "It was almost accidental that I ended up with a doctorate in applied mathematics." (Lazarsfeld 1969: 274) This point concerning the 'accidental' nature of his degree in mathematics was followed up in a conversation that the writer had with him. Evidentially, when a choice had finally to be made concerning which subject to follow for his doctorate, mathematics was selected because he had more credits in that area.

Without necessarily contradicting the above remarks, Lazarsfeld's interest in mathematics was certainly more than casual; indeed, to use Professor Jahoda's words, it was "simply irrepressible".

Immediately after graduating in 1925 with his doctorate in mathematics, Lazarsfeld joined the Psychology Institute as an assistant. His actual position there seems rather vague, which possibly accounts

for Jahoda referring to Lazarsfeld as a Dozent rather than that of assistant. Certainly his duties at the Institute appear to be more those of a dozent rather than an assistant, probably because of his specialist knowledge of statistics. At first he only engaged in seminar work, but after a while he began to give courses not only in statistics but also in social and applied psychology. In order to supplement the poor pay for his work at the Institute, he taught physics and mathematics in a Gymnasium. After establishing the Forschungsstelle he relinquished this post. Rather than resign, however, he took an extended leave of absence which allowed for the possibility of him taking up the job again should his vague and insecure position at the Institute collapse.

The arrival of the Böhlers in 1923, with their systematic psychology and empirical interests, gave added encouragement to Lazarsfeld's existing interests in the social sciences. However, it was Lazarsfeld's mathematical expertise that the Böhlers valued, and consequently they put him in charge of developing empirical social research at the Institute. In his capacity as an assistant, Lazarsfeld gave the first ever course in statistics at the Institute, although, as Lazarsfeld informed the writer, it bore little resemblance to what we would now consider statistics:

"There are two things that I remember, one is that ... I said, look, if you count ... there were no computing machines ... better to do it this way, the fifth stroke goes like this and makes it easier. That was to graduates and then - but, look, Mr. Morrison, you come from an English education, but the situation was different. At another point, of course, this was very early on. I had to explain what the median is. So I needed a really drastic example. My example was the Roman general who was defeated by the Truscans and the soldiers had to go under the cordian yoke. So, would it be better I said

for the general to ask the victorious enemy that the yoke be done on the average of the soldiers or on the median. You see, it was natural for this type of student to know something about battles and the Etruscans, but not about statistics." (Lazarsfeld 19.7.73)

Nonetheless, simple as these early statistical lectures were, as Dr. Wagner pointed out, Lazarsfeld did introduce the work of the English statistical psychologist Charles Spearman into the Institute, as well as Louis Thurstone's work.

"He first went to the psychology institute in order to help Charlotte Bühler and show her what statistical methods were needed in order that she might develop the necessary tests (for her work on children) and give them a sound statistical form. He also lectured on statistics at the institute. He was the first person to do that. He was also the first person to translate Spearman and give lectures on Spearman's findings and of course particularly on the mathematical side, but he also gave lectures on theories about inheritance and so on. So he introduced altogether English thoughts of that kind. Later he also translated, or at least made the Austrian student aware of Thurstone's work. And from there he got interested in questions of opinion research. I don't know, he just got interested in the question "What do people think, how can we deal with it statistically?" How that came to his mind I just can't tell you. Whether he was stimulated by some work - English work. He was a social psychologist - more a social psychologist than a sociologist." (Wagner 19.9.73)

The question of how Lazarsfeld became interested in 'public opinion' work will be returned to shortly, but for the present it is proposed to concentrate on the point raised by Dr. Wagner concerning Lazarsfeld's relation to sociology, since it has importance for Lazarsfeld's work in general.

Writing of his early days in America, Lazarsfeld states: "At the time I did not think of myself at all as a sociologist. I went to no national or regional conventions of sociologists, although my fellowship would have provided the necessary funds." (Lazarsfeld 1964: 294)

According to Raymond Boudon, this refusal to define himself as a professional sociologist permeates much of his writing. According to Boudon:

"The general framework in which he perceives his own research activities is without doubtless sociology - the limits of which he has some difficulty in visualising - than the field which he calls "The empirical analysis of action". (Bourdieu 1972: 419)

Certainly if one examines the market research studies which he conducted both in Austria and America, and more particularly his major works such as, 'The People's Choice' and 'Personal Influence', they are all characterised by a focus on the moment when people act and make choices. This interest in choice and action can be traced in part to the Böhlers' influence, and in part to his membership of a socialist party committed to the democratic road and therefore to elections and votes.

At the age of thirty nine, however, Lazarsfeld 'officially' became a sociologist on his appointment in 1940 as associate professor of sociology at Columbia University, a post he shared with Robert Merton. In fact, the two associated professorships had been created out of a single post of full professor, since the department could not decide on what 'type' of sociologist they wished to appoint. As far as Lazarsfeld was concerned, however, such intellectual demarcations were irrelevant. As he put it during a discussion with the writer on the clash<sup>1</sup> that occurred between the new pollsters and the more classical public opinion researchers:

"Look, by then the quantitative wing in sociology was strong. I was defined as a sociologist which really didn't mean anything. I mean, I always did the same work. In Vienna it was called psychology and here it was called sociology." (Lazarsfeld 25.5.73)

The belief in the essential irrelevance of academic demarcations lies at the heart of Lazarsfeld's intellectual world view since his interests in methodology, in the language of social research and in the logic of empirical enquiry have always transcended his commitments to specific substantive areas. This point emerged particularly vividly during a discussion of Stouffer's American Soldier study, and the question of

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1. In this context see Lazarsfeld "Public Opinion and the Classical Tradition" (Lazarsfeld 1957) for his attempt at reconciling the two traditions.

data banks was raised, and Lazarsfeld commented:

P. LAZARSELD

Look, data bank is a very trivial idea - you just store things; Stouffer did more in that direction, but I never did.

D. MORRISON

Any particular reason?

PL Well, you see, my interest was really always in the logic of survey work. I was never very interested in the results and so a data bank had no attraction for me. I mean, that is completely personal.  
(Lazarsfeld 2.6.73)

Because Lazarsfeld's main interest has always been in the logic of research rather than in the substantive findings of particular projects, his work is informed with a greater coherence than an examination of his disparate empirical studies at first suggests. As mentioned earlier, a crucial underlying theme in his work has been the empirical study of action.

When Lazarsfeld had not been long in America he was invited by Gordon Allport to a seminar on the problems of motivation. Commenting upon this seminar, Lazarsfeld writes:

"After a few introductory remarks, I said, 'Let me give you a first example from a study on mouthwash.' There was a roar of laughter, and I answered: 'I don't quite see why 'lifted weights' are so much more dignified than mouthwash.'" (Lazarsfeld 1969: 298 Footnote)

This is a good example of Lazarsfeld's thinking. The word 'dignified' can easily be exchanged for 'important', since so far as the empirical study of action is concerned, the actual subject of study is not particularly important. The crucial focus for Lazarsfeld is on the methods by which the results are obtained, and that remains a constant whatever the substantive content of the study. Given this background of core methodological concerns it is easy to see that the concept of interdisciplinary research centres would hold a particular attraction for Lazarsfeld. For, as Boudon points out, one of Lazarsfeld's principal

institutions was that shared methodological procedures could bring together "not only socialist voting behaviour and the consumption of soap, but also theory or conceptualisation and research, psychology and sociology", and that consequently his "persistent concern has been to substitute a discipline without frontiers for the hemmed-in fields that make up the landscape of the social sciences." (Boudon 1972: 418, 424)

In fact, in 1950 Lazarsfeld's intention was to change the direction of the 'Bureau' and turn it into a training centre for the social sciences. Although the scheme failed, for a variety of reasons that will be discussed later, he did play an important part in establishing the 'Centre for Behavioural Studies' at Palo Alto. This whole question of interdisciplinary work was raised in the course of a discussion which again provided insight into Lazarsfeld's 'refusal' to circumscribe the field of sociology.

D. MORRISON

Am I correct in thinking that in spite of trying to keep the 'Bureau' interdisciplinary the sociologist eventually came to dominate it.

P. LAZARSELD

Well, yes, but look it was always linked to the sociology department from the start. But the answer to your question goes outside the subject of the Bureau, and the answer is not hard to find. Sociologists have no content. I mean an economist knows what he is doing, a psychologist knows what he is doing, but by definition a sociologist lives in this inter-spatial world you see. You cannot define what the content of sociology is. If you think of the social sciences, the sociologist flows into the cracks of the cake. That is their function. Whatever isn't officially circumscribed is called sociology. So in any interdisciplinary effort I would say the sociologist necessarily becomes dominant, but he will always be the collector - the cement of everything that would probably get them into the more prominent positions."  
(Lazarsfeld 2.7.73)

#### The Fascination with Empirical Methods

Lazarsfeld's interest in the 'clarification of language' and his broad vision of sociology can be traced back to his experiences in Vienna.

It has already been mentioned that Lazarsfeld did not know the Vienna Circle philosophers until after he had given lectures at the University. Nevertheless, it is true that there was a considerable convergence of intellectual interests between them. For as Lazarsfeld himself notes, after discussing his fascination with 'explication':

"At that time I had virtually no contact with the 'Wiener Kreis' although its main leaders had already settled in Vienna. The obvious similarity of what I have just described with their teachings is probably more due to a common background than to direct influence." (Lazarsfeld Memoir 1969: 273)

However, it is one thing to note Lazarsfeld's interests in the clarification of language but it is quite another to attribute to him a philosophical position on logical empiricism, as Professor Rex has recently done. Having discussed the powerful influence upon post-war sociology of logical empiricism, and the role that George Lundberg played in linking logical empiricism to sociology, he then proceeds to link Lundberg to Lazarsfeld.

"Neither Lundberg or Popper, however, was to have a truly lasting influence on sociology. The man who was to have such an influence was Paul Lazarsfeld. Lazarsfeld came to the United States having been brought up in the most profound European theoretical tradition, but became convinced<sup>1</sup> that the social survey, already an established element of American political and commercial life, was the means whereby sociology could be rendered truly scientific."

He continues:

"What Lazarsfeld now envisaged, however, was the opening up of vast new fields of investigation through the specially designed survey which was committed to a quest for kinds of knowledge which no census could aspire to. The improvements of statistical techniques to take account of large numbers of variables, coupled with the technological revolution in data-processing equipment, made the prospects opened up by the new empiricism even more appealing."  
(Rex 1973: 111-112)

Although one would not disagree with Professor Rex that Lazarsfeld's work has been of great importance for the development of empirical sociology,

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1. My emphasis.

one must disagree with his statement that Lazarsfeld was 'convinced that the social survey could render sociology truly scientific'. To impute such a position to Lazarsfeld is a mistake of the first order. Undoubtedly, Lazarsfeld was enthusiastic about empirical methods, but it was never a philosophical commitment and as Boudon correctly notes:

"Lazarsfeld would undoubtedly be the last to insist that all research should make use of questionnaires and proceed by the route of survey research." (Boudon 1972: 424)

In support of his assertion, Boudon cites the passage in Lazarsfeld's Memoir in which he summarises his Austrian research experience and lays down his 'rules of method' as documented by him in 1933.

- a) For any phenomenon one should have objective observations as well as introspective reports.
- b) Case studies should be properly combined with statistical information.
- c) Contemporary information should be supplemented by information on earlier phases of whatever is being studied
- d) One should combine 'natural' and experimental data. By experimental, I meant mainly questionnaires and solicited reports, while by natural, I meant what is now called "unobtrusive measures" - data deriving from daily life without interference from the investigator. (Lazarsfeld 1969:282-3)

Commenting on the above rules Lazarsfeld writes:

"Mere description was not enough. In order to get 'behind' it a variety of data had to be collected on every issue under investigation - just as the true position of a distant object can be found only by looking at it from different sides and directions." (Lazarsfeld 1969: 283)

Thus from the start of his American career Lazarsfeld stressed the importance of diversified approaches. If he had held to the rigid position that Rex attributes to him it would have been extremely difficult, not to say impossible, for him to collaborate with the exiled members of the Frankfurt School such as Leo Löwenthal and Theodore Adorno. Both were members of Lazarsfeld's early research team in America, and Adorno,

while working on his music project, enjoyed particular sympathy from Lazarsfeld, despite the real difficulties presented by Adorno's personality.

D. MORRISON

In the early days in America one gets the impression that you were very sympathetic to critical theory.<sup>1</sup>

P. LAZARSFELD

I still am. That hasn't changed. I would have the same trouble with Adorno now. He was impossible. (Lazarsfeld 25.5.73)

The characterisation of Adorno as 'impossible' refers to his personality not his ideas. For, as Lazarsfeld informed the writer in another interview:

P. LAZARSFELD

I don't remember one moment's disagreement on any intellectual matter with Adorno.

D. MORRISON

That's interesting. I thought it was an intellectual disagreement you must have had with Adorno, and that was why the music study did not continue. I know Adorno was dissatisfied.<sup>2</sup>

PL Well, I suppose that Adorno thought I was a Philistine or something - but from my side we never had any intellectual difference. Look here, when he talks about music then I listen to him with humility because one could learn so much from him. (Lazarsfeld 2.6.73)

This whole question of Lazarsfeld's relationship with the Frankfurt School will be raised later since it throws into sharp relief some of the problems that confronted Lazarsfeld in his attempt to establish the 'Bureau'. However, it is first necessary to clarify Lazarsfeld's intellectual position a little more carefully in light of some of the points that Rex has made. Although Lazarsfeld had never read the Rex article or even heard of Rex, the points that he raised in his article were taken up with Professor Lazarsfeld. <sup>3</sup>

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1. See Lazarsfeld's article on "Administrative and Critical Communication Research" which appeared in the Frankfurt School Journal (Lazarsfeld 1941) and also more recently Lazarsfeld's article on "Critical Theory and Dialectics" (Lazarsfeld 1972).

2. See Adorno (1969) for an account of this dissatisfaction.

3. The writer did later give a copy of the article to Professor Lazarsfeld.

P. LAZARSFELD

Well, look, sometimes people call me a positivist. Now there was a man .. George Lundberg.

D. MORRISON

Yes, Lundberg. It was Lundberg that Professor Rex mentioned in connection with his attempt to establish 'pure science'. He comments that you were successful where Lundberg failed.

PL But look, that's greatly wrong. I have always found .. I was very good friends with Lundberg .. I always found it silly.

DM Lundberg.

PL Yes, Lundberg. This scientism .. to say that quantification is better ... you see that always ... that quantification from Lord Kelvin ... that what you cannot count ... I never had a philosophical position on that. It goes back to an absolute psychological fascination. I don't remember ever having written anything extolling quantification. I simply do not find anything in my whole life ... The reason I want to take your time on this is I want to know how so many people get such fixations.

DM Perhaps a problem that you face is in the teaching of methods. Given your interest in training students for research ... it's much easier to pass on quantitative techniques than it is qualitative.

PL Yes, that is correct.

DM Perhaps it is in the passing on of your work that the quantitative side has become over-emphasised.

PL That is quite likely ... yes. The question of how to teach and even how to learn. You see, if you take such masters as Irving Goffman.. He is unable to say what he is doing and he isn't interested. Ask Goffman, could you give a course even on your own work, explaining what you do and how you do it - you'll find that a waste of time. (Lazarsfeld 19.7.73)

The 'psychological fascination' with 'methods' that Lazarsfeld mentions emerges clearly in the passage in his Memoir where he notes that:

"When I was approximately fifteen, I read the memoirs of Lili Braun. She describes an election evening in Germany in the early 1900's where everyone was waiting for the returns, and then celebrating the socialist victory. I found this extremely exciting, and in the Summer of 1916, when I was living in the custody of Rudolph Hilferding, the socialist leader, I asked him to explain to me what the election business was all about. He found my attitude rather childish, and said I should rather first know what the

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socialist programme was all about. We made a compromise that I would read a book by Kautsky if, at the same time, I also got a book on elections. How seriously I read Kautsky at the time I do not know, but as for the little book explaining elections, more than 50 years later, I still remember the name of the author: Poensgen." (Lazarsfeld 1969: 284-5)

The above passage, which appears as a footnote in the 'Memoir', was followed up in interview in an effort to get at the basis of the 'fascination'.

P. LAZARSFELD

Yes, perhaps you would know Hilferding from history. These people were all, so to say, my uncles in some way. When I was 13 or 14 years old ... the first serious book I read was probably a book by Kautsky.

D. MORRISON

Yes, you mention that in your 'Memoir'.

PL Well, to my life ... the thing I was always fascinated in from the beginning was elections. The whole idea of elections was very interesting for me. I did work at every election in Vienna. (Lazarsfeld 15.6.73)

It is not surprising that Lazarsfeld should be interested in elections given the Austro Marxists' insistence upon the 'parliamentary road' to socialism, and from a very early age Lazarsfeld had known many of its leaders through his mother's involvement with the movement. However, the root of his 'fascination' with 'method' lies deeper than the prominence of elections in Austrian socialism. His interest in elections is only one manifestation of a more general interest in things to do with mathematics.

D. MORRISON

This interest at the non-philosophical level in methodology.. Of course, it is an enormous question and if you were to ask me about certain intellectual interests I have then I'm not sure that I could articulate them .. but your.. I'm not sure one can answer such a question.

P. LAZARSFELD

No, I can't. You have to answer such questions, but I have to provide you with the material. You see, I have memories of the following kind, and these memories would go back well beyond

50 years. I stood in front of a bookstall in Vienna. I would be 19, and by coincidence there is a book which on the outside has one of the scatter diagrams - it was something that one would not use describing a correlation. I didn't quite know what it was, but I found it so exciting. God knows ... like seeing at that age the photograph of a nude girl or something. Then I remember even earlier than that ... I was in a meeting of the socialist young workers' party and someone handed out questionnaires to show how miserable people were. He had got back questionnaires from two or three hundred people and suddenly I had this feeling .. but why doesn't he code them, and I got the questionnaires. It never occurred to this man ... he was just interested in misery you see.

I also remember, that would be around 1928, I was in Hamburg and a German professor ... I can remember his name, Andreas Walter .. he had visited the States and came back with ... you know, the Chicago ecological (yes) ... well, he had ecological tables of Chicago and they were coloured by income levels. That had the same fascination for me and I cannot trace it behind that. There has got to be some child-like experience.

Look, I have often thought about this. I mean, this fascination .. it's like someone saying the first time he heard a violin play and he had to become a professional violinist ... and you don't quite know.

DM Can you pinpoint any other such experiences?

PL Well, the only contributions I can make is the very specific three, four or five incidents ... my seeing the graph or a table or the ecological map .. Well, look, anthropologists claim they saw a picture of some specific Island and from then on all they were interested in was anthropology. I really don't know to what extent non-artists ... of course, with artists it is well known ... to what extent non-artists have this ... how would one call it ... this experience of ... the only thing worthwhile. I suppose there are some.

My vague memories ... there ought to be biographies of people at a very early age ... the composer, the first time he hears a piano and he is lost. How this fascination ... and it can't even be quantified. (Lazarsfeld 19.6.73)

Evidently the psychoanalyst, Siegfried Bernfeld, once asked Lazarsfeld why he wasted his time on statistics. Bernfeld, however, answered his own question, proposing that Lazarsfeld was basically afraid of people and that statistics represented an escape.

Although Lazarsfeld has quite clearly, and rightly, put the onus upon the writer to tease out the factors behind his 'psychological fascination' with anything of a mathematical nature, such an exercise remains outside the writer's competence. For, even though the writer

has had many conversations with Professor Lazarsfeld, in addition to interviewing people who knew him from his Vienna student days, both at the academic and social level, the detailed knowledge upon which to base a satisfactory handling of the question is not in the writer's possession. The important point to underscore, however, is that ultimately Lazarsfeld's interest in methodology and his fascination with anything mathematical was rooted in his personal psychology rather than in the philosophical commitment to empiricism attributed to him by Professor Rex. Once this is appreciated it becomes less of a problem to explain his collaboration with thinkers of such disparate theoretical positions as Adorno, Löwenthal, C. Wright Mills, Merton, Berelson, and Stouffer. In addition, Lazarsfeld's interests appear to have developed independently of any formal alignment with particular schools of thought, a fact which runs counter to what one may reasonably have expected had his empiricism been philosophically based.

On the first page of his 'Memoir' Lazarsfeld writes:

"When my academic career began, the social sciences in Europe were dominated by philosophical and speculative minds. But interest in more concrete work was visible - symbolised, for instance, by the fact that Ferdinand Tönnies, permanent president of the German Sociological Society, instituted in that organisation a section on sociography. Without any formal alignment<sup>1</sup> my research interests developed in this empirical direction." (Lazarsfeld 1969: 270-71)

The words 'formal alignment' have been underscored to indicate Lazarsfeld's lack of attachment to any particular school of thought. Certainly he was familiar with most of the major schools of the day, and not just in sociology, but he belonged to none of them. For, as Marie Jahoda informed the writer:

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1. Emphasis mine.

"You know, Paul says he is a marginal character - he's right because he was really in his research style an American long before he went to America. But he had the advantage of the much more deep and broad intellectual education that one used to get on the continent. Researchers in America are trained in statistics and question-asking and they go ahead and they are technicians. While Paul for many years was familiar with all the major strains of thought in Austria of this time - and so he brought this extra, not just methodological technician, thing with him to America." (Jahoda 26.9.73)

### Factors in the Establishing of the Forschungsstelle: Creativity in Marginality

Writing on the historical importance of innovation in higher education, Lazarsfeld notes:

"Over the last five hundred years, each century has been marked by a major innovation in higher education. In the 16th century it was integration of humanistic studies, the new learning, into the university: the most characteristic institutional event was the creation of the College of France. The 17th century saw the emergence of the natural sciences; the ascendancy of Baconism was carried by the academics and the Royal Society in London was both the leader and the symbol. The 18th century was the age of enlightenment; the French revolution prepared but did not firmly establish a major pattern of university innovation. The 19th century brought about the modern integrated university for which the German institutions beginning with the University of Berlin, set the main pattern. It is hazardous to characterise a century when it has not yet reached far beyond its half-way mark. And yet one can probably state that the rapid expansion of empirical work in the social sciences and the institutional forms this takes will remain one of the outstanding features of the twentieth century." (Lazarsfeld 1961: 1)

In addition to appreciating the fact that the structural innovations have occurred in higher learning, largely in response to new forms of knowledge, Lazarsfeld also recognised that structural changes feed back on to 'knowledge' itself, and in his presidential address to the American Sociological Association he illustrated this reciprocal process whereby new organisational forms, prompted by new forms of knowledge produce in themselves new methods of analysis. He writes:

"The technical and organisational nature of empirical social research leads to formal ideas, to distinctions and inter-connections relevant for many sociological pursuits well beyond the realm of strictly empirical research. My position is akin to the kind of sociology of knowledge which Marxists employ when they stress that new tools of production are reflected in new ways of intellectual analysis." (Lazarsfeld 1962: 760)

Drawing on his own experience as a Bureau director, Lazarsfeld elaborated this point, arguing that the administrative need to make roles explicit in a research institute, to assign each individual specific tasks and to link these tasks to each other, generates in turn a need to make underlying methodological procedures themselves more explicit, and that as a result

"The research operation can provide the model which helps to clarify and unify problems that arise in spheres of enquiry far removed from empirical social research in the narrow sense." (Lazarsfeld 1962: 760)

The basic point, however, is that new forms of knowledge often require a new institutional form to accommodate them. As Raymond Boudon, writing on the 'fit' between forms of knowledge and institutional frameworks, points out:

"The history of French sociology is suggestive on this point: after the decline of the Durkheimian school, in which scholarly work was entirely consistent with academic structure, the development of the social sciences after the Second World War led to institutes outside the universities, such as the 'Centre nationale de la recherche scientifique or L'Ecole pratique des Hautes etudes.' These institutes were successful, because they modelled themselves as "Laboratories", a structure much more appropriate to the development of 'empirical sociology' than faculty positions. It is undoubtedly for this reason that, within a few years, "empirical sociology" became the ideal for the new generation of French sociologists." (Boudon 1972: 423)

In addition to appropriate institutional forms, the growth of new forms of knowledge also requires new forms of training to initiate new disciples and thereby ensure the form's continuation and extension.

Each of Lazarsfeld's above examples of 'innovation' was accompanied by

new methods of teaching. The College of France, for example, established the method of disputation as a way of training; the University of Berlin initiated the seminar and the conducting of research under the guidance of a master, and the contemporary teaching of the social sciences has increasingly utilised the laboratory model. Although one is not suggesting that formal structures are always necessary for the continuation of a school of thought, they do bestow considerable advantages. One of the keys to the 'Bureau's' success in securing a firm place in American sociology was the fact that its structure included provision for student training. A counter example, where the absence of formal structures of training led to a relinquishing of intellectual dominance is provided by Chicago School of Sociology. During its prime in the 1920's, the Chicago School, under such notable figures as Park, Burgess, Small, Thomas and Ogburn, held a dominant position, yet its influence waned relatively rapidly in the face of intellectual competition from other graduate faculties. Commenting on this, Lazarsfeld writes:

"It is my guess that a more formal organisation for social research would have extended the influence of those great Chicago leaders after other graduate schools began to make their bid."  
(Lazarsfeld 1962: 763)

However, this explicit position of Lazarsfeld's on the importance of 'fit' between knowledge and organisation only developed after he had already been a research director for many years. Nevertheless, within Austria, Lazarsfeld was advantageously placed, not only to experience but also to appreciate, the benefits attendant upon organisation.

The enormous structure of the Austrian Social Democrat Party has already been dealt with in some detail, but its importance, in so far as the Forschungsstelle is concerned, rests upon the impact that it had

upon those who lived within its embrace. For, not only did the 'party' construct a semblance of order out of the chaos left by the imperial collapse, but in addition brought purpose and direction to thousands of its supporters through its vast administrative apparatus. Although Lazarsfeld's own personal life seems to have been very disorganised, therefore, his early years were spent within organisations that were to have a deep and lasting effect upon him, and by extension on organised social research. He recognised this himself. Discussing the 'Bureau', he commented that:

"Look, you have to get two things. The dominant theme for me was methodology and I could trace that back to when I was 17 or 18 years of age, but the second theme is the desire to build up the 'Bureau' which has guided my interest. The 'Bureau' was a social event. Sometimes I make the joke - I was always in the socialist children's organisation. In Austria it was the Rote Falken - the Red Falcon - and I was running the children's group - it corresponds to a boy scout leader, and I have always had the feeling that I never stopped being that. So the Bureau is just an extension of my experiences of running boy scouts - socialist boy scouts."  
(Lazarsfeld 15.7.73)

The years that Lazarsfeld spent as an active member of the Austrian socialist party, not only provided him with organisational expertise, but, and more importantly, a heightened sensitivity and appreciation of the benefits of organisation. However, it should not be imagined that such experiences were consciously articulated or that they provided the explicit model for the running of the Forschungsstelle. Rather, its organisation was a logical extension of his experiences in socialist youth organisations. To an Austrian socialist such as Lazarsfeld, the idea of collective or team work and the idea of leadership and hierarchy seemed perfectly 'natural'.

In the previous chapter the Rote Falken was discussed, and its main features mentioned, such as the looseness of organisation,

together with the strong personal ties which bound the members together, and the subordination of members to the leader. These were essentially the features of the Forschungsstelle. Indeed, the Forschungsstelle represented an adaptation and transfer not only of the organisational forms of the Rote Falken, but of a number of its personnel as well. For, as Lazarsfeld informed the writer:

"I was very active in the socialist youth movement. I had a ready-made group of satellites or knights. It was really .. you know what a boy scout is ... well, you transfer - when they are 14 they go camping and when they are 19 years old they go into social ... it was almost transferring my clique of younger people .. by young I mean one or two years younger than I was .. into this whole activity." (Lazarsfeld 25.5.73)

Thus, the social dimensions of the Forschungsstelle are firmly embedded within the socialist movement of Austria after the first world war; the experiences of organisation learnt in socialist Vienna were readily transferred to the academic situation of empirical social research. Certainly, the organisational style of the Forschungsstelle can quite easily be linked directly to Lazarsfeld's youth activities, even perhaps to the extent of explaining his poor administrative ability, for given the non-instrumental nature of the Rote Falken, these skills did not need to be developed. Yet, to understand how and why the Forschungsstelle came to be founded in the first place, it is necessary to go beyond Lazarsfeld's particular experiences in the youth movement, to consider his overall situation, particularly his marginality.

In discussing the 'creation' of the Forschungsstelle, particular attention will be paid to Lazarsfeld's positional and psychological marginality. In reality these two aspects of Lazarsfeld's marginality

cannot be separated. However, for the purposes of analytical clarity, such separation will at times be necessary. The term 'creative' has been used to describe the founding of the Forschungsstelle since it represented a 'new way of seeing things' which radically broke with past ideas concerning scholastic organisation. The Forschungsstelle was certainly very different from the conventional university department. It instituted the new role of managerial scholar, shifted the basis of financial support away from traditional university sources and combined 'pure' research with commercial research. Indeed, the Forschungsstelle had many of the features that are now associated with the modern research Bureau. Furthermore, because the Forschungsstelle was probably the first of its kind to be established anywhere<sup>1</sup> it seems particularly appropriate to describe its foundation as an act of creation.

Before moving on to discuss Lazarsfeld's marginality, however, it is first necessary to briefly sketch the general context within which the Forschungsstelle was established. For example, it has already been mentioned that the University of Vienna was virtually bankrupt, which meant that the possibility of the University financing costly empirical social research was virtually non-existent. Yet, paradoxically, the very economic conditions that resulted in the lack of available research funds also produced the necessary 'labour force' for the Forschungsstelle. The poverty

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1. Lazarsfeld, commenting on the Forschungsstelle, states "It antedates, as far as I know, all such university institutions in this country except the one at the University of North Carolina created by Howard Odum" (Lazarsfeld 1962: 758). Odum was a dedicated southerner who established his institute to help the south, but it does not pre-date Lazarsfeld's Forschungsstelle. According to Siegfried Kracauer (date unknown) Odum's institute was established in 1925, the same year as Lazarsfeld's Forschungsstelle. It may be that Lazarsfeld's above mistake is based on his thinking that he established the Forschungsstelle in 1927 and not, as previously shown, 1925.

of the students meant that any work was appreciated and, as Gertrud Wagner cryptically informed the writer, "We earned a bit and learned a bit."

(Wagner 9.9.73) Secondly, and perhaps more importantly, the unemployment among young intellectuals struck a sympathetic chord in Lazarsfeld and he recognised that an institution such as the Forschungsstelle could help to alleviate the situation. For, as Marie Jahoda informed the writer:

"I should, however, say that there was another reason that made the idea of Forschungsstelle such a great success and this reason was the terrible unemployment and poverty among students and graduates from the university. You know, we were all very active in the socialist youth movement and we knew hundreds, literally, of gifted young people who had a contribution to make but nobody paid them a penny for it. So I'm sure that in Paul's thinking the possibility of providing some income for this large group of gifted young people also played a major role in not wanting to give up." (Jahoda 26.9.73)

The fact that the Forschungsstelle provided money for unemployed students may well have been one of the factors behind Charlotte Bühler's favourable response to the whole idea of such an institute.

Lazarsfeld's marginality within the academic world derived firstly from his interests in empirical social research, and secondly from the fact that he was a socialist Jew. His interest in empirical social research placed him outside the mainstream of Austrian social science of that time, and it was only within the more empirically oriented Psychology Institute that this interest could be accommodated. However, even here, the fact that he was a Jew made for marginality at the structural level. His position within the Institute was always vague and insecure, but because he was a Jew any 'normal' academic career was blocked. Thus, whilst not suggesting that the Forschungsstelle was established to provide Lazarsfeld with a career, it is suggested that had Lazarsfeld occupied a more secure position at the Psychology Institute, with the possibility of career advancement along institutional paths,

then his energies may very well have been directed towards that end and not found outlet in the Forschungsstelle.

When the writer interviewed Professor Lazarsfeld in New York, he made only brief reference to his position as a Jew within Vienna University, saying that it was an important fact in his leaving for America. Because no embellishment was given to that statement, this point was followed up when the writer was in Vienna. In light of the previous discussion concerning anti-semitism at the University, it is not surprising that the facts given the writer could not be checked at the documentary level. Nevertheless, from the previous examples of anti-semitism at the University, there would be good reason to suspect that Lazarsfeld would not escape the effects of such prejudice, and the following conversational abstract with Dr. Wagner would appear to support that supposition.

D. MORRISON

In conversation with Professor Lazarsfeld, he seems to be very aware of his Jewishness and the part that it played in his life. How far would it be correct to consider that he would have had great difficulty in gaining a senior post at the University because of the existence of anti-semitism?

G. WAGNER

Well, its true, he couldn't have.

DM Carnap was a professor and Carnap was a Jew.

GW Yes, and Charlotte Bühler was a Jew herself or at least partly Jewish. Well, he couldn't have at that time .. Carnap was much older you see. Paul was not only Jewish, he was a social democrat. His family was a very well-known socialist family - very friendly with Friedrich Adler. For him it would in fact have been impossible to even get 'Habilitation' and there might be all sorts of difficulties, I don't know. Karl Bühler who was the main professor there - he would have had to have him put forward because Charlotte Bühler was only assistant professor, and Karl Bühler couldn't do it.

DM Why was that?

GW Oh, he wouldn't do it, he was afraid of that. One could say .. now I interpret it .. I don't know because he never said to me.

His wife was a Jewess, or at least .. and he managed to get her a post. One couldn't refuse him to let her teach and he probably thought - Well, that is all my colleagues at the senate will allow me and I can't put forward any other Jew because in his Institute there existed -- the main assistants were two women Jews. I don't know whether Paul spoke about it to you - a woman named Wolf. She became a professor in America but is dead now, and a woman named Else Frenkel, and she got married to a professor but I can't remember his name. (It was in fact Egon Brunswick.) Anyhow, Wolf was the main assistant of Karl Bühler and he didn't make her .. I feel either he really couldn't any more at this stage - they wouldn't accept it or he was frightened to." (Wagner 9.10.73)

Fortunately, the writer had the opportunity to meet with Professor Lazarsfeld again after this interview with Dr. Wagner and discuss the above situation. Professor Lazarsfeld's account is very close to that of Dr. Wagner's, but does add some further interesting facts. Evidently, when the question of promotion within the Psychology Institute arose, Karl Bühler appointed the gentile Brunswick.<sup>1</sup> According to Lazarsfeld, Bühler believed that there was a blacklist at the University against Jews, and because he, Bühler, promoted Brunswick rather than Lazarsfeld, Lazarsfeld felt that by way of consolation Bühler recommended him to the Rockefeller Foundation for the travel fellowship that was eventually to take him to America.<sup>2</sup>

The fact that Lazarsfeld was a Jew therefore made for difficulties within Viennese academia and produced a sensitivity and awareness of marginality. This position and feeling of 'outsider' was compounded upon his arrival in America where his official status was that of alien. The

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1. Although Dr. Wagner refers to the 'professor' who married Else Frenkel, he was, in fact, only appointed to the post of privatdozent. Else Frenkel Brunswick later became famous as Adorno's co-author on the authoritarian personality studies in America.

2. Conversation with Lazarsfeld at Cambridge 28.3.74.

following conversational extract, for example, highlights Lazarsfeld's awareness to his position when in America.

P. LAZARSFELD

You know, when I first came here there was still a certain amount of genteel anti-semitism. And my accent. As a matter of fact I was less affected because my being a foreigner over-shadowed my being Jewish. I think I would have had more difficulty as an American Jew at that time than as an Austrian Jew. I think I could not have been appointed at Columbia at that time - not really - not if I had been an American Jew. No one thought of me as a Jew because of my foreignness - the accent saved my life.

D. MORRISON

Yes, if one looks at Adorno's experience, I think that is one of the difficulties he faced, or was it just rigidity of position?

PL Yes, Well, you know, that is one thing and then he wasn't aware of it like I was. I was aware that I sounded funny. As a matter of fact, I remember one day going to Robert Lynd's house and his boy said to his father "Why does the man talk so funny?" Well, Adorno, he would have blamed Lynd and said "Aren't you ashamed that ..." - he would blame the other fellow. You know for years I had a collection of jokes that I used immediately in public about my accent, because I knew people were shocked, especially when I talked as some official representative of Columbia. The first minute I would make a joke about I didn't come with the Mayflower or something like that. Adorno would never have done that - it's your problem if you don't understand." (Lazarsfeld 2.6.73)

In discussing Professor Lazarsfeld's friendship with Robert Merton,

Marie Jahoda also commented upon Lazarsfeld's sensitivity to his Jewishness:

"Paul's relationship with Merton was a very strong one and it still is... But, I think, as far as I could see - you know, all sorts of personal factors play a role. Paul, for reasons of his early period, always really wanted politics and he was always sensitive about his Jewishness. So there was the established Merton, and Stouffer was another friendship - non-Jewish leading Harvard people who would take him seriously - he had the most idiotic, but persistent, inferiority feeling." (Jahoda 26.9.73)

It is interesting that Marie Jahoda should mention that Lazarsfeld wished to be taken seriously by 'Harvard people' since Merton himself was a Polish-American Jew who adopted the name Robert Merton to facilitate his career. But, to be sure, the psychological characteristics associated with

positional marginality do appear to have been present in Lazarsfeld. That is, he was acutely sensitive about the factors which contributed to his marginal position, notably his Jewishness in Austria, and his status as an emigré, or alien, in America. This question was raised during the course of a conversation the writer had with John Marshall, who had been assistant director of the Humanities division of the Rockefeller Foundation which had supported Lazarsfeld's early radio studies. In relation to the question of marginality, John Marshall commented:

J. MARSHALL

He published something on that. I thought this was exaggerated and I probably wrote Paul to that effect at that time. I think its truth would be in Paul believing it was so. I think it was not so, but if Paul believed it was so he would have acted as if it was true.

D. MORRISON

Did he give any indications of the ....?

JM He was always very manipulative. I think Paul would agree with me. He was one of those people who was never completely sure of his mind. I think Paul was very. He had enormous energy, but I suspect that there was something in his psychological make-up that forces him to belittle himself .. forces him to shrug off his own success.

I think Paul won't hold it against me if I said that at times he was a highly manipulative person and I was and still am very fond of Paul ... He was after all - he was typical of a refugee - they felt they had to manipulate the people around them in any way they could to secure their position." (Marshall 6.7.73)

An appreciation of Lazarsfeld's insecure position is indispensable for a proper understanding, not only of his early years in America, but more particularly of his Vienna days, where, in addition to his personally insecure position at the University, he was caught up in the general political insecurity of the socialist party.

Lazarsfeld, in common with many of his young socialist colleagues, experienced the almost unreal atmosphere of Austrian socialism which stemmed from the Party being suspended between success and failure, and

as he put it himself, "Those of us on the socialist side were very much affected by this ambivalence." (Lazarsfeld 25.5.73). Having failed to secure a comprehensive socialist victory, the party occupied itself with pushing forward social reforms, particularly in Vienna where it held effective power. Within this atmosphere of concern over social questions, it is not surprising that Lazarsfeld and his socialist colleagues should gravitate towards those areas of academic work which were most directly relevant to the tasks in hand, namely the social sciences. In Lazarsfeld's case, however, an interest in the social sciences was combined with a commitment to mathematics to produce an involvement in empirical research in social psychology. As Lazarsfeld informed the writer:

"I remember we had this joke. I quoted somewhere that a rising revolution needs economists, the victorious revolution needs engineers, the defeated revolution needs social psychologists. And in one way or another that was quite an obvious thing to do." (Lazarsfeld 25.5.73)

Certainly, Lazarsfeld's move into social psychology was 'an obvious thing to do' given his intellectual background and the fact that other channels into which he could direct his political energies were not particularly available to him. In short, he was a typical representative of the wide group of politically conscious intellectuals who, when faced by the ambivalent situation of defeated socialism immersed themselves in academic work. In this situation the Forschungsstelle was particularly attractive since it enabled "this whole defeated socialist group" to transfer to a "new activity which was close enough to social reality and had some academic glamour." (Lazarsfeld 25.5.73).

In Lazarsfeld's case, this drive to engage in some work that was 'close enough to social reality' was particularly strong, since not only

had his Jewishness blocked his academic career, but also lessened the possibility of advancement as a politician within the social democratic party. Yet, even had such 'anti-semitism' not existed, his ambition of holding high office in a 'socialist Austria' was an impossibility given the failure of the revolution itself. In discussing with Marie Jahoda the methodological and political aspects of the Forschungsstelle's work, Lazarsfeld's political conflict came out very clearly:

D. MORRISON

I have assumed that Professor Lazarsfeld's work was always more informed by his methodological interests than his politics.

M. JAHODA

I think so, I think so .. but still the issues that we studied were ... 'political' ... he must have told you ... we did a lovely study on the style of life of the Viennese beggars.

DM No, he didn't mention that one.

MJ Really! Well, we just walked up to the beggars and said 'Can I buy you a cup of coffee?', then learnt the profession from them in unstructured interviews. Then we did another study during the Abyssinian war. We went to the general population and let them draw a map of Africa, and Abyssinia comes out as three quarters of Africa ... you know, highlighting perception in response to public events. We did a lot of studies of this nature. Small interesting studies which inherently are of social interest, but this is no more and no less political than all social science has to be and I think Paul has a little conflict in his life over, or had at least in those years over his political ambition, and the impossibility of fulfilling it in the Austrian situation. So, for a personal solution was to be concerned with social affairs whilst not influencing them in a leading fashion. I think to that extent the work was political, but if I try to see the contribution objectively, I see him all the time as an outstanding methodologist and never in content." (Jahoda 26.9.73)

### The Political Orientation of Paul Lazarsfeld's Vienna Work

In support of the argument that Lazarsfeld's marginality was an important factor making for creativity, emphasis will now be given to the influence of politics upon his work. For, if his marginality stemmed in part from his politics, then it is reasonable to expect that

his politics would in some manner inform his work. Notwithstanding the previous discussion concerning ideological separation of work and politics, the fact remains that politics influenced the general orientation of his work if not its actual content. Having said that, however, it is interesting that the original monograph that Lazarsfeld submitted to Charlotte Bühler for 'Jugend and Beruf' had a marked ideological tinge to its style of reportage. Lazarsfeld writes:

"She was pleased and also accepted my position on the need to distinguish between middle-class and working class adolescents. But she objected strenuously to the tone in which my section on proletarian youth was written. I was, indeed, full of compassion, talking about exploitation by the bourgeois society, and the hortative style of this section was quite different from the rest of the manuscript. I could not deny this fact, and finally rewrote it. None of the argument was omitted but the tone became descriptive and naturalistic instead of critical."  
(Lazarsfeld 1969: 285)

Lazarsfeld follows this by saying that this episode affected his subsequent writing and was "a contributory factor to the debate on the role of sociology that was led by C. Wright Mills." Certainly, Mills considered Lazarsfeld's research, although rigorous, was at the same time sterile and lacking in imagination.<sup>1</sup> However, Mills' criticisms extend further than this to an emotional distaste for the type of commercial contract work that 'Bureaux' are generally associated with. During the course of an interview with Bernard Berelson, the question of Lazarsfeld's politics was raised. Berelson related:

"I can still hear Wright Mills saying 'Why, Paul Lazarsfeld Works for True Story magazine' as though no self respecting academic of any persuasion would do that. Paul always felt 'I don't understand what Wright is complaining about. These people are generous with the research funds, they allow me to raise the questions ... we have an opportunity to do the Dacateur study.'<sup>2</sup> After all, it was

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1. See Mills 1959 for his attack on abstracted empiricism".

2. Dacateur formed part of "Personal Influences", Katz and Lazarsfeld 1955.

Wright .. Wright went out there to do the magazine survey on women in a small middle western town ... interesting the way all the involutions worked their way out. I never ... Paul was known of course of being very close to the research director then the President of Columbia Broadcasting and his unkind critics would feel he was a paid lackey of the capitalist media with True Story and CBS and some newspaper accounts. I didn't feel it ever troubled Paul. His socialist background was there, but his foreground was with American business and Paul was always delighted that he could phone up the president of Columbia Broadcasting System and get him on the phone." (Berelson 12.7.73)

Lazarsfeld's relationship with commercial interests might appear on the surface to conflict with his socialist beliefs, especially during his Vienna days, but what needs stressing is the practical necessity, both in America and Vienna, for commercial sources of finance. The inability of traditional sources to provide the necessary funds for empirical social research demanded that Lazarsfeld turn to less conventional sources for support. Berelson was definitely correct when he said that it did not trouble Lazarsfeld, for there was no reason why it should, quite the reverse in fact given the high level of student unemployment. In a conversation with Professor Paul Neurath the writer was given a very good example of Lazarsfeld's pragmatic approach to questions of research. Evidently, when Neurath was working under Lazarsfeld in America, a critic attacked Lazarsfeld for accepting commercial contracts and, according to Neurath, Lazarsfeld became quite angry and pointed to Neurath saying, "if he ever does anything worthwhile it will be because of the money from Bisodol".<sup>1</sup> Two points can be extracted from this example which allow

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1. Not verbatim. (Neurath, June 1974)

an insight into Lazarsfeld's thinking. Firstly, the fact that Bisodol was a stomach powder mattered nothing to him as long as methodological training could be given to the researcher while at the same time providing income. Secondly, he believed that the triviality of a study's immediate focus does not necessarily mean that broader generalities cannot be made from them. The Austrian projects, for example, included studies of beer, butter, electrical appliances, vinegar, perfume, chocolate and many others which, while seemingly trivial, facilitated the development of general concepts such as the notion of 'the proletarian consumer' as:

"less psychologically mobile, less active, more inhibited in his behaviour. The radius of stores he considers for possible purchases is smaller. He buys more often at the same store. His food habits are more rigid and less subject to seasonal variations. As part of this reduction in effective scope the interest in other than the most essential details is lost; requirements in regard to quality, appearance and other features of merchandise are less specific and frequent the more we deal with consumers from low social strata." (Lazarsfeld, 1969. 280-281)

Hence, although many of the studies conducted at the Forschungsstelle were of a trivial nature, it did not necessarily follow that they were completely lacking in academic value. At the same time, such market research studies were essential not only as a source of desperately needed funds, but also as a pretext for valuable methodological training which, given the newness of such research, was understandably poor at times.<sup>1</sup> By the time Lazarsfeld established

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1. Lazarsfeld related to the writer that such was the awareness of research procedure that questionnaires for a radio study were left at tobacco kiosks for customers to fill in if they so wished. (Lazarsfeld, 19.7.73)

the 'Bureau' however, it cannot be justifiably claimed that the market research studies which were conducted provided methodological benefits to anyone but the graduate students, nor can it be convincingly argued that they were a useful way of tackling concept formation. The purpose of such studies was undoubtedly to provide the necessary money to keep the Bureau financially solvent.

The question of the 'Bureau' and its financial support will be returned to later, however, for the present it is sufficient to note that the seeking of support from the world of commerce by no means detracts from Lazarsfeld's 'socialism' as an important factor informing his Viennese work. For example, the Marienthal study of unemployment grew directly out of 'socialist' concerns and the rather sentimental nature of the work exhibits genuine sympathy for the plight of the unemployed. Yet Lazarsfeld's original intention had been to study not unemployment, but leisure time activity for "reasons I cannot remember" (Lazarsfeld, 1969: 751). Although a study of unemployment has on the surface a closer proximity to Lazarsfeld's political interests, even the leisure time study was prompted by interest in the proletariat's life style. Hans Zeisel provides the 'missing reasons' behind the idea of a leisure time study, and also illustrates the closeness of the Forschungsstelle members to the socialist party, and the fulfilment of their desire to be close to 'social reality' when he writes:

"The style of life and the culture of the Austrian proletariat, and more particularly the Viennese proletariat, occupied an extraordinary part of our activities and we had conceived a plan to furnish it with scientific documentation. We wanted to give concrete form to the idea of social accounting

by researching into exposing the real aspects of the new forms of the organisation of leisure ... When we told Otto Bauer of our plan to study the organisation of leisure he was annoyed and told us 'you want to study leisure in a country which has suffered for a number of years from heavy unemployment. It is inconvenient if you want to do research; why don't you look at the consequences of unemployment in the long run.' (Zeisel, 1968: 8)

In fact it was Otto Bauer who suggested to the Forschungsstelle members that Marienthal would be an ideal location for their study, since the villagers depended for their employment on the textile mill which had been run down in face of the general industrial slump. The mill was eventually to close completely causing nearly the whole village to be unemployed, thereby providing ideal conditions for a study of the effects of widespread unemployment upon community life. It is the study's emphasis upon the unemployed community, and not the unemployed individual, which gives the work its sociological stamp. The Marienthal study is by far the most important study that the Forschungsstelle conducted and the one that was taken most seriously in socialist circles. Most of the studies undertaken by the Forschungsstelle were ignored by the socialists since, as already noted, although informed by the groups political leanings they were not political in content. But Marienthal was different, and contributed directly to the socialists' stock of knowledge. As Marie Jahoda stated:

"When the work on Marienthal started going on we reported to the class (social-intellectual club under the leadership of Otto Bauer which used to meet every second Sunday) and discussed it with various people. You see before Marienthal came out there were two theories in the Austrian socialist party about what unemployment would do to people. One group said it would lead to revolution because people wouldn't take it, and the other group said it would lead to apathy and destruction. The debate was really solved by Marienthal which showed very clearly

that it leads to apathy. So it was very highly regarded in political circles and anybody interested in social sciences." (Jahoda 26.9.73)

Lazarsfeld's political interests form an ever-present backcloth to his Viennese work, yet in emphasising that aspect of his life there is no intention to deny that given his intellectual milieu many of his interests would probably have been present in the absence of an involvement in socialism. His intellectual ideas do not necessarily stem directly from his socialism therefore. For example, his interest in the concept of 'Action' would make him no different from many of his academic peers since it was a linking theme throughout the social sciences.<sup>1</sup> If one was engaged in an intellectual history, then certainly the influence of people such as Karl Bühler would need examining in detail since Bühler had been a member of the Wurtzburg School of psychology which, since the turn of the century, had been actively engaged in empirical studies of action. However, as influential as Bühler may have been in giving an added thrust to Lazarsfeld's interest in 'action' it was his political involvement which gave his work its particular direction.

D. Morrison: Am I mistaken here, but I formed the idea that one of Professor Lazarsfeld's interests in 'action' stemmed from his political background and the Austro-Marxists' emphasis upon voting - is this ...?

M. Jahoda: I think so. Not in a completely articulated fashion, but you know all the time Paul was in Vienna and was Director of the Forschungsstelle it was still a time that he was very conscious that if he only could he would be in politics rather than social research, so

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1. See Lazarsfeld 'Historical Notes on the Empirical Study of Action'. (Lazarsfeld 1972)

you know I don't recall conversations in which he said 'look we are fundamentally interested in politics and this is why we have to study action', but obviously his following of political events and all this..!" (Jahoda 26.9.73)

Thus this backdrop of political interest ran parallel to the more academic work on 'action' that Lazarsfeld was engaged on under the Böhlers at the university. Marie Jahoda again provided the writer with a very good example of this in relation to the need to clarify the distinction between action and behaviour.<sup>1</sup> In the course of a baby development study of Charlotte Bühler's:

"We observed for a twenty four hour study. My, how we suffered, but took turns.<sup>2</sup> But over the development of the descriptive protocols the question arose of what do you record - do you record an action or do you record a behaviour unit like finger bending when it is obviously the action of gripping something and we decided in discussions of these questions - after very interesting seminars that the minute units required by behaviouristic research were unsuitable and that action had to be defined as a thing that once it has reached a goal to an end and something else begins. Paul did a lot of work on the standardisation and sampling etc. for the development of the baby tests which ran parallel with the Forschungsstelle and I think it was discussion about suitable protocols for describing behaviour of infants that got the action concept into the Forschungsstelle." (Jahoda, 26.9.73)

Not only did Lazarsfeld's 'intellectual' interests inform his more 'social' work therefore, but his 'social' work provided a vehicle for the clarification of his intellectual concerns. For example, Lazarsfeld states that his interest in how people voted resulted in work upon how young people develop their occupational plans. Evidently

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1. Lazarsfeld was certainly familiar with Behaviourist Psychology. In fact, according to Wagner he gave the first lectures on it at the University of Vienna. However, it is likely that his first real contact with behaviourism came from association with Karl Bühler who had familiarised himself with it whilst in America.
  2. 'We' refers to herself and Lazarsfeld. In fact the baby in question was their daughter.

Lazarsfeld was obliged to substitute the occupational study for the voting study since the conservative nature of the Vienna University establishment made it unwise for a member of staff to engage in an unbiased study of people's voting decisions. However, he was then presented with the difficulty of analysis which was only resolved with the aid of Lotte Danziger who had knowledge of American market research techniques. Since occupational choice was a much more complicated phenomena, because of its longitudinal nature, than consumer choice, Lazarsfeld at first concentrated on the latter in order to later tackle the former. Lazarsfeld writes:

"Such is the origin of my Vienna market research studies: the result of the methodological equivalence of socialist voting and the buying of soap." (Lazarsfeld, 1969: 279)

Thus at one level the goal remains the same, the methodological clarification of the empirical study of action; yet it is in this manner that Lazarsfeld's 'social' interests often led back into and clarified his 'intellectual' interests.

#### The End of an Episode: Lazarsfeld's Leaving for America

It has already been mentioned that Karl Bühler was partly responsible for Lazarsfeld leaving for America by putting him forward for a Rockefeller Fellowship. Professor Lazarsfeld added additional information regarding this scholarship when he informed the writer that:

"The Rockefeller Foundation had an office in Paris under a man called Ketrich. This whole fellowship world we have now didn't exist and this office of the Rockefeller Foundation picked one young man from each country and gave them a two year fellowship (sic)\* and Ketrich came to Austria and I was brought to his attention probably through Bühler. One day I am invited - that was in the summer of '32 - and I am invited by an American who talked very good German and was interviewed. He gave me an application form back. I took the application form and I say to myself 'why should I get myself into all this disappointment?' and so I just never filled it in. It just seemed impossible to me where people were poor and unemployed and all that ... I just put it out of my mind, and about the beginning of December or the end of November of '32 I got a cable from Paris - 'your application mislaid, please send another copy'. You know what had turned out ... it was inconceivable to them that I wouldn't apply and it seemed inconceivable to me that I would get the fellowship. They had made up their mind to give me the Austrian fellowship on the report of Ketrich and as they came to process it, as it would not be called, there was no application as I hadn't applied for it." (Lazarsfeld, 15.6.73)

It was not simply the attraction of the fellowship which led Lazarsfeld to leave Austria however, even though it was certainly in those days a very prestigious award. Rather, it was a combination of factors: the obviously deteriorating political situation, and the increasing difficulty of engaging in social research, and the fact that as a Jew Lazarsfeld had no real career prospects within Austria. Marie Jahoda provided the writer with a very good description of the circumstances surrounding Lazarsfeld's departure.

"This is quite a complicated story. In part it was a great distinction and at the time he thought he was only going for a year. Now going to America where there was much more going on in the social sciences was reasonable, and another point, our marriage didn't function properly so the break seemed reasonable from that point of view. He had the Forschungsstelle as you rightly say, but you can't imagine

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\* The fellowship was in actual fact for one year, but was later extended.

What a struggle it was to keep it going, and here was this chance - it was like a sabbatical which was undreamed of then, and also the chance not to worry - can I pay the interviewers, and where do I get a new study. It was a chance of breaking out. So I think the great prestige of the Rockefeller fellowship, the personal problems and the fantastic effort of running the Forschungsstelle ....

It was that sort of situation where a break which was originally planned for one year seemed from every conceivable point of view the right thing to do and also, by 1933, the political situation in Austria had very much deteriorated. Paul didn't want to avoid this as a person, but it seemed already to spell the end of the institute and unfettered social research." (Jahoda, 26.9.73)

Although Lazarsfeld had always intended to 'return' to Austria at the end of his fellowship, he never did since the movement of political events rapidly made that impracticable. In many ways Lazarsfeld was a premature exile, soon to be joined by many other Austrian scholars following the triumph of National Socialism. Upon Dolfus's seizure of power and the subsequent outlawing of the socialist party, Lazarsfeld's position at the Gymnasium was cancelled, although his vague position at the university was left unaffected. In consideration of this situation the Rockefeller Foundation extended his fellowship until the Autumn of 1935; however, the foundation was evidently pleased with his progress since Stacey May, an officer of the foundation, wrote on the 31.4.34 "has done excellent work despite worry entailed in his family situation".<sup>1</sup> The worry over his family situation must refer to the fact that being well-known socialists they were jailed by the Dollfus government. In a conversation the writer had with Dr. Schilder, she mentioned that when

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1. Rockefeller Fellowship Docket (writer's files)

Lazarsfeld returned to Austria in 1935 to obtain an immigration visa for America, he was extremely worried over Jahoda's political activities. A fear well-founded considering her subsequent arrest at the Forschungsstelle in 1936.

Lazarsfeld never returned to Austria to take up an academic post, yet he did help establish the Institute for Advanced Studies in Vienna in 1963. The fact is that by the time his fellowship ended it was impractical to return to Austria, and by the time it was possible for him to return he was already an established figure in the American academic world where his interests lay in building up the new institute of the Bureau for Applied Social Research.

#### Final Years of the Forschungsstelle: A Postscript

In addition to the political suspicion which hung over the Forschungsstelle after the outlawing of the socialists there was the continuing problem of finding financial support for its work. The shortage of money in the business world meant a shortage of money to finance market research; in fact, as the whole Austrian situation deteriorated, market research tended to become increasingly redundant. Yet, despite the jailing of Jahoda in 1936, the Forschungsstelle continued right up until 1938 when the National Socialists closed it down. After the jailing of Jahoda, although many members were also in danger, Hans Zeisel kept the Forschungsstelle going with the aid of a Mr. Fallowday, whom the financial backer, Gold, had drafted in as 'adviser'. However, as Zeisel informed the writer, the Forschungsstelle began to die as early as 1934<sup>1</sup> and although

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1. Letter from Zeisel 8.3.73.

Lazarsfeld's leaving in 1933 undoubtedly made for difficulties, it was factors outside which led to its demise. After all, the idea of a Forschungsstelle had been born out of Austrian Socialism; it was inevitable that it should finally go under to those forces which its members had always stood out against.

### CHAPTER THREE

#### PAUL LAZARFELD THE EMIGRÉ: ADJUSTMENT AND SUCCESS

Paul Lazarsfeld The Emigré: Adjustment and Success.

"Thus a clean break - psychological, social and economic - had to be made, and a new life started.

But England was not the country in which to do it. Much as I (and all the others) loved England, her society was too homogeneous and too solid, her opportunities (particularly under conditions of unemployment) too narrow, her politics not too agreeable. One could I felt never quite become an Englishman. Thus the United States appeared as the sole country where, perhaps, an attempt would be successful to carry out the threefold transition: as a human being, an intellectual and a political scholar.

That this transition has been successful, not only in my case, but hundreds of others, is primarily due to the United States, her people and her universities. This demonstrated by the astounding fact that only a few exiles chose to return to Germany, in spite of the fact that the material and non-material rewards of German universities are, on the whole, greater than they are here."

(Neumann 1953: 17-18)

For Lazarsfeld, the choice of the United States as his new home was not the conscious decision that it evidently was for Neumann, but rather stemmed from a variety of factors, not least the fact that he was already resident in the United States when the change in the Austrian political situation made return inadvisable. Although Lazarsfeld had been in America since 1933 he faced essentially the same difficulties of 'transition' after his Rockefeller Fellowship expired in 1935 as the wave of intellectual emigrés arriving between 1936 and 1938. He was in the early stages of his academic career, he was relatively unknown, and he did not arrive as a member of any particular 'school' of thought. Even where these conditions did not apply and scholars were relatively well known and well advanced in their intellectual careers, problems of assimilation and adjustment still arose however, as the case of Theodor Adorno illustrates.

Adorno's problem was that he lacked both the flexibility to accommodate to unfamiliar situations and the social awareness which would have enabled him to exploit the opportunities which were presented to him. As he himself put it:

"I consider myself European through and through, considered myself as such from the first to the last days abroad, and never denied it. Not only was it natural for me to preserve the intellectual continuity of my personal life, but I quickly became fully aware of it in America. I still remember the shock that a housemaid, an emigrant like ourselves, gave me during our first days in New York when she, the daughter of a so-called good home, explained: 'People in my town used to go to the symphony, now they go to Radio City'. In no way did I want to be like her. Even if I had wanted to, I wouldn't have been capable of it. By nature and personal history, I was unsuited for 'adjustment' in intellectual matters. Fully as I recognise that intellectual individuality can only develop through processes of adjustment and socialisation, I still consider it the obligation and at the same time the proof of nature individuality to transcend mere adjustment. Through the mechanisms of identification with images of authority, one must emancipate oneself from this very identification. This relationship between autonomy and adjustment was recognised by Freud long ago and has since become familiar to American scholarship. But for a refugee thirty years ago, this was not yet true. 'Adjustment' was still a magic word, particularly for those who came from Europe as persecuted people, of whom it was expected that they would prove themselves in the new land not to be so haughty as to insist stubbornly on remaining what they had been before." (Adorno 1969: 338-339)

Adorno's refusal to make concessions to his new milieu caused problems not only for himself but also for those around him. His inflexibility in the face of the unfamiliar turned easily to scorn, and the memoir which he wrote on his years in America rings with denunciations of the people he encountered, even though they were often contacts from which he may well have benefited had he retained a more open mind.

His intransigent attitude was to drive Lazarsfeld to the edge of desperation during the time they collaborated on the Princeton

Radio Project. Adorno's reports for the project, for example, made no concessions to the potential readership, and evoked a strong rebuke from Lazarsfeld.

"... I implored you repeatedly to use more responsible language and you evidently were psychologically unable to follow my advice. I shudder to imagine the invectives which a reader would use against you if he read your text in the same mood in which you write about other people. Don't you think it is a perfect fetishism the way you use Latin words all through your text? There is no doubt that the words 'necessary condition' express everything which the corresponding Latin words can express, but you evidently feel magically more secure if you use words which symbolise your education."  
(Lazarsfeld letter to Adorno undated June 1938/Winter 1939)

Given the centrality of the concept of 'fetishism' in Adorno's theory of music, its evocation in this context is clearly intended to be heavily ironic. Despite his impatience with Adorno's stubbornness however, Lazarsfeld greatly respected his ideas and always regretted the fact that they never managed to convert them into researchable propositions. Nor did he lay the blame for this failure entirely at Adorno's door:

"The defeat of this hope in the Princeton project has left a troublesome question in my mind. After the war Adorno was an active member of the Berkeley group that produced 'The Authoritarian Personality'. Their basic concept of the fascistic character was developed by Adorno and was certainly no less speculative than what he wrote for us; nevertheless, his colleagues in California were able to convert his idea into the famous F-scale. I have an uneasy feeling that my duties in the various divisions of the Princeton project may have prevented me from devoting the necessary time and attention to achieve the purpose for which I engaged Adorno originally." (Lazarsfeld 1969: 325)

Commenting on the contrast between the failure of the Princeton project and the success of the Berkeley study, Martin Jay points to the lessening in Adorno's hostility to American research styles as a key factor:

"Whatever the real reasons, the music project was unsuccessful, while the Authoritarian Personality became a classic of social science immediately after its completion. The explanation for this change cannot be sought solely in Adorno's own development - he was only one of a large number of co-workers on the second project - but with time, he did gain valuable methodological experience that caused a modification of his initial hostility to American techniques. Thus, for example, his stress on grasping the 'objective spirit' rather than measuring subjective reactions to it had diminished by the end of the decade." (Jay 1973: 224)

Jay is correct. Adorno did modify his position, prompted in part by the fact that the subject matter of the new study appeared more amenable to empirical techniques. For as Jay adds: "culture might not be measurable, but it seemed as if bias easily could be". In addition, his co-researchers shared his basic theoretical orientation towards Freud, a fact which as Adorno himself points out, greatly facilitated amicable collaboration (Adorno 1969: 358). These were not the only factors in the situation however. Adorno's success as part of the Berkeley team was due not simply to his more accommodatory intellectual position and spirit of teamwork that prevailed on the project, but also to the change in the overall research context. Even if Lazarsfeld had been able to devote more time to Adorno, he could not have altered the wider milieu within which the Princeton project was carried out, and it was primarily this milieu which forced Adorno into contact with people and matters which he found deeply distasteful, thereby driving him further into his role as 'Herr Doktor' and reinforcing his refusal to make accommodations.

Lazarsfeld on the other hand not only adjusted to this milieu, but turned it to advantage, producing a series of major

works which came to occupy a secure place within mainstream American sociology, his contribution being marked eventually by the American sociological community's highest accolade - the presidency of the American Sociological Association. He achieved this success however through markedly unorthodox means. He never 'worked his way up' through university departments - the conventional path to academic success - but instead developed an institutional framework for empirical social research which attracted not only young scholars but also the interest and esteem of the established academic community.

#### Arrival in America and Early Work at Newark

"On the whole, the German exile, bred in the veneration of theory and history and contempt for empiricism and pragmatism, entered a diametrically opposed intellectual climate: optimistic, empirically orientated, a-historical, but also self-righteous." (Neuman 1953: 17)

Lazarsfeld was certainly not 'diametrically opposed' to the American intellectual climate when he arrived in 1933. He was sufficiently close to it to feel 'at home', yet sufficiently removed by his insistence upon qualitative work as a necessary accompaniment to quantitative research to feed back and form a link with the older, more humanistic European traditions of scholarship. It was this added dimension which set Lazarsfeld apart and gave his work its distinctiveness and special attraction. After he had worked with Samuel Stouffer on the 'Depression and the Family' study<sup>1</sup> for

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<sup>1</sup> For a shortened version of this see chapter 6 of Stouffer 1962.

example Stouffer wrote to him praising him for his qualitative work. He writes:

"Your personality is present everywhere and ... you know as well as I what a temptation it would have been to have kept the study down to a rather dull level 'too close to facts'."

(Letter Stouffer to Lazarsfeld, 19.3.37)

Lazarsfeld's relationship with Stouffer will be returned to at many points in the work. Yet, for the present it is sufficient to note Lazarsfeld's comfortable acceptability as a quantifier with a humanistic strain. The attacks upon his empirical style of work were only to follow much later after he was already an established figure in the academic world.<sup>1</sup>

As far as intellectual reception is concerned then, Lazarsfeld was particularly advantageously placed. He had all the legitimacy of the quantifier, yet extracted himself from the sterility of pure quantification by his insistence upon the necessity to transcend any one approach to arrive at broad conceptual integration.

This catholic intellectual approach of Lazarsfeld's made for ease of contact with a variety of scholastic centres and individuals within America - his actual geographical mobility being aided by the Rockefeller travel scholarship. The number and variety of his formal intellectual contacts can be seen by consulting appendix A.

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<sup>STEIN</sup>  
1 In particular see Mills (1959) and Vidich (1964). Both these attacks were against what Lazarsfeld represented in American sociology, but <sup>STEIN</sup>Vidich's in particular is concerned with the style of the 'Bureau's' work, considering it far too one-sided.

His most important career contact, however, was undoubtedly the sociologist Robert Lynd who, because of his 'Middletown' study was one of the first people Lazarsfeld visited upon arriving in America. Lynd not only helped Lazarsfeld to gain his first academic appointment after his fellowship expired in 1935, but was also instrumental in his obtaining the position of Associate Professor of Sociology at Columbia University.

Above and beyond the intellectual comfort that Lazarsfeld derived from fitting easily into the quantitative enthusiasm of American social research however, was the social and political comfort to be obtained from arriving in America when the New Deal programme was at its height. The strident reformist spirit and atmosphere generated by the Roosevelt administration entirely suited Lazarsfeld for, as he informed the writer:

"You know as an old Viennese socialist I really felt completely at home with the New Deal - with the Roosevelt administration, and really as long as Roosevelt lived there was, so to say, nothing to criticise from my point of view. I was just completely in agreement with the whole of the New Deal line." (Lazarsfeld 2.6.73)

Such factors, although nebulous, should not be neglected when considering the process of adjustment. Indeed, within a short time of arriving in America Lazarsfeld left New York for Washington to work in the New Deal Administration - in the statistical unit of the Federal Relief Administration - although by consulting appendix A it can be seen that at first he interspersed his work at the Bureau with visits elsewhere. According to Lazarsfeld, "the people who worked there were very naive

analysts and I had a great deal of experience" (Lazarsfeld 2.6.73). Certainly his talents appear to have been much appreciated, since on the 23rd May 1934, H.B. Mayers of the Federal Emergency Relief Administration wrote to Stacey May of the Rockefeller Foundation, requesting Lazarsfeld's services full-time. He wrote:

"We are about to undertake a comprehensive study of personal and occupational characteristics of the unemployed and with your authorisation we would like to have Lazarsfeld undertake as part of our study a more intensive inquiry into the more obscure psychological and sociological effects of unemployment along lines of his study of Marienthal. This would probably occupy him for two or three months, possibly in the Chicago area. Study would give Lazarsfeld opportunity for first hand contact with American unemployed, with access to all sources and materials available to this Administration and would provide for us valuable supplementary material for interpretation of our nation-wide study."

(Rockefeller docket)

So far as this work is concerned however, the main point to note about Lazarsfeld's American fellowship years is the range and variety of contacts which he made, not only with sociologists and psychologists but also with individuals from political and commercial circles, a situation which takes on particular importance when viewing Lazarsfeld in the future role as Bureau Director.

After his fellowship expired in 1935 Lazarsfeld was employed for a short while at the University of Pittsburgh's Research Bureau of Retail Training.<sup>1</sup> Before appointing him the University Chancellor, J.B. Bowman, had written to Stacey May requesting a character reference. Stacey May replied:

"Have a great respect for Lazarsfeld's ability and personal characteristics. Lazarsfeld contacts have been many and he has attracted interest of business groups, government officials etc. Has great ability for initiating projects and carrying them through."

(Rockefeller Docket 7.8.35)

Previous to the offer of a temporary post at the Pittsburgh Bureau Lazarsfeld had conducted a number of studies there under the Director, David Craig, on such subjects as "How Pittsburgh Women Decide Where to Buy Their Dresses" and "How Pittsburgh Drivers Choose Their Gasoline". In his Memoir Lazarsfeld writes of them, "The first study once made me a house guest of a local Pittsburgh tycoon, Edgar Kaufman, and the second brought me into repeated contact with Paul Mellon".<sup>2</sup>  
(Lazarsfeld 1969: 289)

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1 The Research Bureau of Retail Training had been established by W.W. Charters. However, at the time in question the Director was David Craig but in 1935 he resigned and moved to Washington which meant that Lazarsfeld was placed in a difficult situation. Craig had arranged an appointment for Lazarsfeld but had left before it was ratified by the Bureau's trustees. Lazarsfeld had returned to Vienna to obtain the required immigration visa. Lazarsfeld had obtained the necessary visa on the strength of an appointment letter which Craig had written on official paper. The day after getting the visa Craig cabled Lazarsfeld only to inform him that his appointment would have to be delayed since he had resigned and a successor had not yet been appointed. Thus, not really having a job to go to in America, Lazarsfeld's visa was of doubtful validity. The course of action which Lazarsfeld took shows his scant regard for formal procedures and 'legal' niceties. Rather than inform anyone Lazarsfeld merely went ahead as if nothing had changed and purchased a railway ticket with his last \$150 to arrive "in New York as the classic immigrant, penniless". (Lazarsfeld 1969: 304)

2 The Mellon family along with the Rockefellers and du Ponts constitute one of the three great dynasties of wealth in America. Apart from petroleum (Gulf Oil) their holdings extend to banking, aluminium, chemicals, insurance and real estate.

It is interesting that Lazarsfeld should mention these particular contacts in his Memoir since they are not featured again. However, they do serve to illustrate the importance which Lazarsfeld attached to such contacts and to indicate his ability to take advantage of every opportunity which presented itself, a trait which undoubtedly stood him in good stead in his role as Bureau Director.

It has already been observed that new forms of knowledge often require new institutional settings, and certainly by the 1930's the development of empirical social research in America had thrust this question to the forefront. However, above and beyond the recognition of research bureaux as an appropriate form within which to set empirical work, the question of the particular talents needed to organise and sustain such institutions requires some elaboration.

Structurally, most research bureaux, although linked to parent universities academically, are independent of them financially and are therefore obliged to seek external sources of funding. This means that research directors require many of the skills of a commercial manager, most notably the business acumen to keep the enterprise on a sound financial footing. Hence, of necessity, one of their main roles has been to secure work contracts and research funds from commercial, military, governmental and philanthropic sources. To be sure, traditional university departments look to a variety of sources for financial assistance, but their legitimacy derives to a large extent from their teaching role. The legitimacy

of a research bureau in contrast rests not so much upon the competent communication of knowledge but upon the prior production of original knowledge. Given the expensive nature of empirical social research then, quite apart from organising the institute to efficiently produce knowledge, the director must secure the funds to allow production to take place. It is in this respect that the skills required to successfully operate a research bureau are likely to be those which would furnish the individual with a successful managerial career in the business world. In fact, in Lazarsfeld's case both John Marshall and Robert Lynd wondered whether he would finally opt for an academic or a more business-oriented career.

In 1941 when Lazarsfeld was already an associate professor at Columbia University and the 'Bureau', whilst not being fully integrated into the University was nonetheless reasonably well-established, Lynd called at John Marshall's office to discuss Lazarsfeld's career. The position was that Lazarsfeld had part of his professorial salary paid by the university and the balance was made up from the 'Bureau's' finances. In addition, he was paid considerable amounts for the private contract work that the 'Bureau' engaged in, although Lazarsfeld himself never benefitted personally from such arrangements since all such contract payment was turned over to finance the 'Bureau'. Yet, such was the extent of his commercial work that both Lynd and Marshall wondered where his true interests lay - within the world of commerce or the university world.

"In short, the acceptance of any request (consultancy) such as is outlined will make it necessary for Lazarsfeld to decide which of the two alternatives confronting him he is to take. If he chooses the academic course he must do so with a clear recognition of the fact that he is giving up the other alternative where with the present interest in polling and marketing research, he could undoubtedly earn a considerably larger salary. Lazarsfeld has always said he preferred academic research but he must now decide."  
 (Marshall's summary of conversation with Lynd 21.4.41)

Whilst it would appear that Lazarsfeld possessed skills that would have enabled him to be a success in the business world, it is not being suggested that all bureau directors need possess such skills to the same extent that Lazarsfeld did, since the necessity for them varies between different bureaux and at different points in their development. A research centre such as the one at Michigan, for example, is far more financially secure than the Columbia Bureau since it receives a greater proportion of its funding from the parent university. Similarly, it is much more likely that the director's own business acumen is a significant factor in the success or failure of the enterprise during its formative years, since in the absence of stable institutional supports the institute is thrown back upon the performance of key individuals.

Although when the 'Bureau' was first established the key stable source of its finances was the Rockefeller Foundation's radio grant, it nevertheless accepted commercial contracts in order to supplement its income. Not only was such an institution new and therefore not easily accommodated within the existing<sup>s</sup> university structures,

but even if Lazarsfeld had discovered an academic parent willing to adopt such a wayward child, it is doubtful if it could have provided the necessary funds. For it must be remembered that the American universities in the late 1930's were still recovering from the effects of the economic depression of the late twenties and early thirties. Thus it is in this context that Lazarsfeld's personality and experiences made him particularly well equipped for the pioneering role of bureau director.

Firstly, Lazarsfeld's personality seems to have been well-suited to the role of the 'friendly director' building contacts that would prove beneficial for his bureau. His attractive manner lent for easy relationships with a wide variety of people. Frank Stanton of CBS, for example, wrote to Robert Lynd describing Lazarsfeld thus:

"He has a keen understanding of human behaviour, he is tolerant in every way and in addition, has an enviable personality." (Stanton to Lynd 21.2.41)

Secondly, Lazarsfeld had already 'learned' to talk with the business community from his years as director of the Forschungsstelle in Vienna. This ability made the securing of commercial contracts that much easier, but also had beneficial effects on the 'Bureau's' general public relations. In the course of the Radio Research Project, for example, it was necessary for Lazarsfeld and John Marshall to deal with a radio industry that was initially hostile to such research taking place, and with opposition from within the Rockefeller Foundation. John Marshall described the situation to the writer in the following discussion.

D. Morrison: Did you ever have to fight within the Foundation for acceptance of the type of work that the Princeton project involved, or was the Foundation enthusiastic?

J. Marshall: No, we had to fight for it once or twice.

D.M. What was the opposition?

J.M. The basic opposition from the time we first proposed aid to the Princeton Office of Radio Research was in the polling. We had on our foundation's board at that time Sulzberger, involved with the New York Times, and Sulzberger was particularly opposed to polling on the grounds that still come up. I think his particular opposition was that polling would skew the outcome. There would be a band wagon effect which has been observed so often. Furthermore, there was a good deal of skepticism of how crude polling could gain anything, and in some discussion with the board of trustees - I don't remember what it was now - but we came to a virtual impasse on this point. The president of the foundation, the late Raymond Fosdick, wisely called a halt to the discussion at that time and said that the officers would take the offer under advice. President Fosdick called me in and asked me if I would go down to Richmond to see Douglas Freeman, the editor of the Richmond ... I forget its name. It's the foremost Richmond newspaper. Freeman was also a member of our board. Fosdick had talked with both Sulzberger and Freeman and they agreed that if Freeman came to the conclusion, he being the more scholarly of the two men, that polling had a possibility ... Sulzberger and others on the board would withdraw their objections. In other words Freeman was to serve as a referee.

So I went down to Richmond, and Freeman was generous with his time. He gave me virtually the whole day. We sat in his office all morning, then went home to lunch and we talked and talked. In the end he said 'I agree with you'. I will tell Fosdick that I think it venturesome but I think the foundation ought to be venturesome and so on. From that time on I was not aware of any serious opposition in the board of trustees.

(Marshall 6.7.73)

Within philanthropic foundations there exists two main levels of decision making: the first level of the foundation officers and the higher level of the board of trustees. Although nominally the board of trustees have the ultimate power to decide which projects the foundation will fund, in fact the officers are the key individuals in the decision making chain. Over time the officer becomes familiar

with the type of project that the trustees are willing to support and comes to act as a "gatekeeper".<sup>1</sup> This was highlighted to the writer during the course of an interview with the Director of the Twentieth Century Fund.

"We sometimes surprise the board, but no, over time I've got to know them pretty well and I think I have a good board in the sense of one that is willing to take risks and is willing to back questionable projects because they believe the Foundation really does have some risk capital and that we ought to risk it and not just play safe. So it's fair to say relatively few projects are turned down but they also want to show from time to time, to demonstrate that they are trustees and they will turn down and sometimes I think they make the wrong choice. I don't mind them rejecting some but it depends on which they reject."  
(Rossant: 19.6.73)

This whole question of foundations and the production of knowledge will be returned to in detail in a later chapter, but for the present it is sufficient to note that external pressure brought to bear upon a board of trustees can, and as will be demonstrated later, has resulted in the refusal to support a project that is defined as controversial. The fact remains however, that foundation officials are structurally much closer to the intellectual world of ideas than are most members of the boards of trustees who are mainly recruited from what has been termed the 'establishment'.<sup>2</sup> Thus, whilst John Marshall never met any protest from his fellow officials he did face opposition at

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1 Coser (1970) actually refers to foundations as "gatekeepers of contemporary intellectual life". However, in this instance the writer is referring to the internal mechanics of the gate-keeping process - see White (1950) and Breed (1955).

2 See Domhoff (1967) 64-71 for a brief account of trustees social position.

board level, and although he eventually overcame this his task would have been far more difficult had the radio industry mounted a concerted opposition to the project. As previously stated however, the industry was initially hostile to the radio project as the following interview with John Marshall reveals.

D. Morrison.

I know you are on record as saying that the radio industry viewed the early work of the 'Bureau' with suspicion despite the close association you had with Frank Stanton at C.B.S.

J. Marshall.

Well I can give you a concrete reply to that. When we were considering the first assistance from the Foundation for the Princeton office the question was raised - I forget by whom - as to why if this was to be of benefit to the broadcasters, as we were arguing, why shouldn't it be financed partly by the industry. By that time I'd made it my business to get to know the principal people in the broadcasting companies, so I simply got appointments and walked over to their offices and said 'why won't the industry support this?' I remember the answer came in a very succinct form from a contact at Columbia. I can't remember who his name was, but he had a position which was something like special assistant to the president and he said, 'it's perfectly simple we don't want to rock the boat. We are doing fine as we are now and we don't want to rock the boat. If your people want to rock the boat we will have to go along, but we don't want to put any of our money in it at this point'.

D.M. Did they make co-operation difficult?

J.M. No, not that I remember. (John Marshall: 6.7.73)

Although the industry men co-operated with the study, albeit reluctantly, they remained suspicious, and had they been antagonised in the course of the research they would certainly have placed obstacles in the way of the project, both at the Foundation level and at the level of day to day research operation. It is unclear

whether or not Lazarsfeld knew of Marshall's problems at the Foundation, but he was certainly very keenly aware of the need to maintain good relations with the industry, and it says much for his managerial skills that there were no problems once the project was under way. He was not exactly helped however by Adorno's behaviour, which Lazarsfeld described as follows:

"First Adorno was very intolerant .. and I think after a while I felt embarrassed by Adorno. He never made any concessions in his behaviour, he remained a foreigner and aggressive. I sent him to talk with a lot of very important people in the radio music industry. There were a lot of music education hours on the radio and they were atrocious. I mean for any real musician ... in America at that time there was no musical culture. The lack of musical culture in this country in 1937 is hard to imagine - there was no classical music. Now, especially for a professional musician like Adorno or a Viennese like me who grew up with chamber music in every house and so on, we found it all very interesting. So I had Adorno introduced to some of the people who felt they were musicians, who felt that they distributed culture to the American High School children - they were furious talking to Adorno. Instead of him trying to understand them .. when they opened their mouths Adorno would tell them what idiots they were. Now Adorno was absolutely right, but that's not the way you conduct an investigation, a study .. you have to listen. You see, Frank Stanton who was then at C.B.S., not important then, but a very cultivated man, he would arrange for meetings for Adorno with various people and they would phone back to Stanton 'why do you waste my time talking to this mad man?' "

(Lazarsfeld; 2.6.73)

Evidence of Lazarsfeld's own adroitness in negotiating situations is provided by Robert Lynd in a letter to John Marshall where he states:

"He (Lazarsfeld) and I differ temperamentally as to the wisdom of being candid rather than mildly evasive, but I have no question at all as to his basic integrity ... I find him open minded and co-operative."

(Lynd letter to Marshall 16.3.39)

Lazarsfeld not only possessed the ability to communication with a variety of individuals, but was also aware that publicity was a valuable tactic and indispensable to the nature of the enterprise upon which he was engaged. For example, he never overlooked the value of press coverage in gaining general acceptance for his work and he always sent newspaper reports of the project to the Rockefeller Foundation. Similarly when he was director of the Newark Centre he wrote a report for the university's trustees in the Spring of 1937 in which he attempted to justify the idea of such a Centre by similar tactics. The report itself will be discussed later, but it is worthy of note in this present context, that he mentioned to the trustees that about a thousand newspaper articles contained references to his work and the 'Centre'.

Lazarsfeld cleverly mediated between the academic world and the lay world, using each to justify to the other the importance of his work and by extension the importance of the organisational setting within which the work was conducted. Charles Siepmann mentions Lazarsfeld's stress on popularising his work when he contrasts him with Adorno in a letter which he wrote to John Marshall:

"Many thanks for your letter of December 11th. I know what you mean about Adorno. You know him, of course, much better than I, but I should judge on slight acquaintance that he isn't likely to become acclimatised in the way you hope. I do however believe that his criticisms and approach in its present form has real significance and that there are those whose reactions to it would be sympathetic. He is, I fear, essentially an intellectual and not like Lazarsfeld either capable of or concerned to communicate at a popular level of interpretation." (Siepmann to Marshall: 12.12.40)

Reference has already been made to the fact that in 1941 Lazarsfeld instructed the 'Bureau' staff to learn Spanish as

illustrative of his personal control and authority. When set in a different context however, this incident also provides a very good example of Lazarsfeld's shrewdness as a business manager. The writer, when sifting through the Rockefeller archives on the radio project, discovered a very cryptic reference to the fact that the staff of the 'Bureau' was learning Spanish. Unable to meaningfully interpret this reference the writer raised the point in a subsequent discussion with Professor Lazarsfeld.

D. Morrison:

When I was going through the Rockefeller files I came across this reference to the 'Bureau' staff learning Spanish.

Professor Lazarsfeld:

Oh, I'd forgotten that, that's right.

D.M. Why did the staff learn Spanish? Was it that things looked difficult for future research?

P.L. Yes, yes, I remember that now. I can remember it very vividly - we paid an instructor .. Look that was just a complete political misjudgement. I thought that .. this was during the war .. I was sure somehow that the building up of South America would be the main task .. post war task of the United States. I didn't know that the main task would be to build up Germany and Japan. I didn't foresee the cold war. You see if it hadn't been for the cold war I still think that would have been very important. I was absolutely sure there lay the cultural international future of the United States. I still think if it had not been for the cold war - it was not a stupid exercise. I cannot remember why I was so convinced of it, but I remember the lessons. I sat in on them." (Lazarsfeld: 15.6.73)

There can be few better examples of the forward-looking bureau director than the above. For, although the Rockefeller Radio Research grant provided the financial basis for the study it was

never intended that the Foundation should support the 'Bureau' after the research was finished. Thus, it was essential for Lazarsfeld to think ahead to the time when Rockefeller money would no longer be forthcoming. However, as Lazarsfeld mentions, his prognosis that South America would become the United States' primary sphere of interest, thereby providing the 'Bureau' with easily obtainable studies, proved mistaken. Yet for any bureau director the ever-present need to secure research money demands an ability to understand and negotiate the situations which will place him in a favoured position viz-a-viz potential sources of finance, and the more skilfully this is accomplished, the greater the likelihood of a bureau securing the type of funds and studies that it prefers to handle. In addition, once a grant is secured, the director must organise resources and individuals to gain the maximum advantage in terms of productive output. Hence, the research director, in order to successfully fulfil his role, must manipulate not only the money situation but also his staff, and this manipulative aspect of his role runs counter to values held in traditional university departments. Whilst it is not being suggested that these elements of manipulation are entirely absent in university departments, it is argued that given the context within which research bureaux operate, they are not only more prevalent but are actually a functional prerequisite of the director's role. The extent to which such role attributes conflict with university held norms and values is, of course, an empirical question which cannot be handled here. However, the point to be underscored is that the strain within a research bureau operating under the pressure

of contract deadlines to be met, future contracts to be secured, reports to be written and payment of non-professional staff such as interviewers, coders and so on, results in a much more commercial ambience than exists within the more academically leisured atmosphere of a university department. Consequently the skills required by a director are closer to those demanded within the world of commerce than to those that characterise a traditional university department.

Not surprisingly, this situation is reflected in Lazarsfeld's own dealings as a successful bureau director in manipulation of situations, and to a lesser extent of individuals. John Marshall for example, in a letter to Howard Odum, the Director of the Institute for Research in the Social Sciences at the University of North Carolina, gently informs him that whilst having the highest respect for Lazarsfeld he is nevertheless manipulative. This manipulativeness is well illustrated by Lazarsfeld's handling of Adorno after the latter had failed to produce some work which he had been paid for. Evidently a Miss Kohn was given the task of editing Adorno's work, but instead of doing a 'real editing job', she corrected his English which then destroyed the sense of what Adorno was attempting to say. The impossibility of using the finished work prompted Lazarsfeld to inform his two associate directors, Hadly Cantril and Frank Stanton that:

"I impressed upon Wiesenrund<sup>1</sup>, during the summer, my criticism rather strongly and he finally saw my point. Of his own volition he made the following suggestions.

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1 'Wiesenrund' was the common form of address for Adorno. 'Wiesenrund' was dropped from his name after Friedrich Pollock asked him to do so. Evidently Pollock was concerned over the number of Jewish sounding names on the Frankfurt institute's roster. (Jay 1973: 34)

He would be willing to withdraw the memorandum in its present form and in order to compensate us for the material loss involved (about \$150) he offered to work full time during Wiebes presence in New York in order to make the most of it. He has indeed kept his promise religiously, and the two really worked day and night on the planning and the first steps of the experiment.

It seems to me, as a result, that we did not lose, and probably even gained by this arrangement with Wiesengrund. If you do not agree with me, I am sure that I could get him to make some additional contribution on this account.

(Lazarsfeld letter to Cantril and Stanton, 26.9.38)

Elsewhere Lazarsfeld comes close to openly acknowledging his manipulative behaviour. For example, after writing to Hadley Cantril endeavouring to have the Rockefeller grant housed at Newark rather than Princeton University he reflects on the letter thus:

"Re-reading the four page single spaced letter over today I am amused by the Machiavellianism that I seem to have attributed to myself, at least by implication." (Lazarsfeld, 1969: 307)

#### Institutional Involvement: The Newark Centre

The fact that research institutes provided Lazarsfeld with an ideal context within which to pursue his abiding interest in methodology has already been remarked upon. Yet, in accounting for his early involvement in American research institutes, such intellectual factors must take second place to his structural position, and more particularly to the initial difficulties which he faced in securing an academic appointment within the American university system. For, although he may well have been able to obtain a post in a minor university, he certainly could

not secure a position commensurate with his intellectual abilities and aspirations in a major university. This whole question was raised with Professor Lazarsfeld and as the following conversational abstract clearly illustrates, he certainly perceived difficulty in obtaining a university post during his early days in America.

"While in my first two years of my Rockefeller fellowship I didn't need anything and during that period there was the Dollfuss Putsch in Austria and I decided that I would like to try and stay here. There was then not a problem of the acceptance of my work, but the problem of getting a position. That was a completely different problem and remember that this is at the depth of the depression still and the depth of the misery of the American university. The question of staying here had nothing to do with acceptance or non-acceptance of my work but of finding an appointment. I think people were still anti-foreigner, there was hardly any refugees here and ... so the question of getting a permanent faculty position at a major university was a very difficult problem." (Lazarsfeld 25.5.73)

Although it would be wrong to see Lazarsfeld's academic career in America as being blocked in quite the same rigid fashion as it had been in Vienna, he was nevertheless made acutely aware that his chances of following a traditional career path were decidedly slim. Hence, whilst it may be a slight overstatement to say that Lazarsfeld consciously sought other channels through which he could establish himself academically, it is nevertheless true that when alternative opportunities presented themselves such deliberations were certainly in his mind.

This awareness of the alternative career path, presented by association with a research institute, is well illustrated by a letter he wrote to Hadley Cantril during the summer of 1937.

Whilst holidaying in Austria Lazarsfeld received word from Cantril offering him the directorship of the Princeton Radio Research

Project. Although the job itself was extremely attractive to Lazarsfeld he had certain reservations concerning the abandonment of the Newark research centre which he had built up and which he saw as a path to academic advancement. The letter attempts to persuade Cantril to try to get the research grant transferred to Newark rather than have it located at Princeton. The letter will be quoted at some length since it affords insights into Lazarsfeld's position on a number of matters germane to this study. He writes:

"I invented the Research Centre (Newark) for two reasons. I wanted to direct a rather great variety of studies, so that I was sure that from year to year my methodological experience would increase and that is, as you know, my main interest in research. And I tried to build up groups of younger students to be educated just in this kind of research procedures I tried to develop. Now as to the first point I think that your project would do splendidly. Radio is a topic around which actually any kind of research method can be tried out and can be applied satisfactorily. But I am somewhat worried in regard to the second point. I don't know if you are aware that by now there are each term graduate students from other universities coming to Newark to work with me on different studies and that is one of the main features of the Research Centre from my point of view. The question is therefore would it be possible to set up at least part of your project in such a way that it could be used as a sort of training institution? There are many forms possible, but as a matter of principle we should see eye to eye in regard to this point because it is so important for me.

This leads to another question. I am not worried about the limited duration of your project - it seems as if it were always possible to find a job in America. But it is probably not always possible to set up a Research Centre. The University of Newark is certainly not an important place at this moment but I feel rather certain that its Research Centre can become important in ten years from now. I would hate to see it collapse and I am sure it will if I drop it now. You have to understand me in the right way: the Research Centre does not need my work for the next year - what it needs is that I identify whatever I do in some way with its name ...

You see all comes back to an European attitude which might be not so easy to understand from your point of view. I feel strongly that I don't want to go ahead

alone, that I want to stand for an institution and I try to build up an institution which is able and willing to stand for me. Of course, I will have to do very different things, less glorious but about the same way as you are professor in Harvard, then in Columbia, then in Princeton. But as my poise and my post and my name cannot compare with yours, I try to identify whatever I do with an institution which might after some time acquire the dignity which I myself to reasons of destiny and maybe personality can hardly aspire to. Of course in spite of this European attitude I am now American enough to see the great advantage whch your offer implies and I would love to accept it. So this whole letter is an effort to get your help in my effort to coordinate the actual situation brought about by your project and the long range plans which I have tried to explain in this letter."

(Lazarsfeld to Cantril 8.8.37)

With regard to his 'European attitude' mentioned at two points in the letter, the following conversation with John Marshall goes some way to explain what he meant.

D. Morrison: I always got the impression that Lazarsfeld was very aware that if he was going to make it he had to make it attached to an institute rather than particularly as an individual. This seems to be a very great motivating force with him - that is, it was his institute and he rose or fell depending on the institute.

J. Marshall: Yes, well this is of course a typical central European attitude. Any scholar worth his salt in Germany or Austria would have his own 'Institut'. He brought that attitude over to this country with him.

This explanation of Lazarsfeld's 'European attitude' further helps to explain why his career followed a path of institutional innovation. He came from an academic culture where professors were much more closely identified with their departments than their English or American counterparts, and where the pinnacle of success was to have a successful 'Institut' associated with one's name. The possibilities of Lazarsfeld fulfilling this criterion by becoming the

head of a major American university department were however extremely restricted. Hence he was obliged to fulfil his academic ambitions by establishing his own institute, and in this he was fortunate. He arrived in America at the moment when the rising styles of empirical social science were increasingly demanding new organisational settings. Hence his previous experience of running a research centre, coupled with his relatively disadvantaged occupational position, provided him with both the expertise and the motivation to be at the forefront of new institutional developments.

The Newark Centre: Inauspicious Beginnings of the Bureau of Applied Social Research.

"... it never occurred to me to aspire to a major university job. I took it for granted that I would have to make some move similar to the creation of the Vienna Research Centre if I wanted to find a place for myself in the United States."  
(Lazarsfeld 1969: 301)

The opportunity to create such a Centre came with an offer to analyse some data collected under the auspices of the National Youth Administration which was housed at the University of Newark. Robert Lynd had recommended Lazarsfeld for the job to Frank Kingdon, the recently appointed President of Newark University, and it was Kingdon's idea that in addition to analysing the N.Y.A. data Lazarsfeld should teach a research course at the university. The possibility of establishing an institute modelled along the lines of the 'Forschungsstelle' was not missed by Lazarsfeld who suggested to

Kingdon that the N.Y.A. project should be extended to enable the formation of a research centre. Kingdon agreed to this suggestion and work was started towards the end of 1935, although the 'Centre' was not officially recognised until May 21 1936. The aims of the 'Centre' were, according to the report of its activities which Lazarsfeld presented to the university President in 1936:

1. To give research training to students.
2. To develop new methods of research.
3. To publish finished studies.
4. To help the City of Newark to a better understanding of its social and economic problems.
5. To act as a consulting service to social and business agencies in the city.
6. To give students the opportunity for gainful employment.
7. To accumulate funds for the perpetuation and enlargement of the Centre's activities.

(Lazarsfeld Report to Newark University 1936.)

Although the University of Newark could in no way be considered a prestigious institution it was Kingdon's intention to make it so, and it suited Lazarsfeld to be attached to a university whose future looked promising. Certainly he was hopeful for the future, for as he informed the writer, "When I was offered the Princeton project I was still so optimistic about the Newark situation that I didn't want to leave" (Lazarsfeld 15.5.73). However, as optimistic as the future may have seemed to Lazarsfeld it was not to be secured without overcoming the familiar difficulties resulting from the lack of firm and substantial financial support. Although Kingdon had agreed to

the idea of a research centre, the university was in no position to provide the required finances. For the first month of the 'Centre's' existence it was supported by the Welfare Federation of Newark, and in the subsequent four months of financial instability by the International Institute for Social Research - Horkheimer's group - since at that stage they were still prosperous enough to be able to afford such financial aid. Lazarsfeld in his report to the University pays special tribute to the Horkheimer group:

"The International Institute for Social Research has not only undertaken and financed the study on authority in relief families(1) and subsidized the study on job hunting, but it also carried the Research Centre through four critical months when the University had not yet taken over the affairs of the Centre by putting at its disposal the sum of two thousand dollars. It was agreed at the time that half of this sum, one thousand dollars, would be considered as debt and repaid later.

(Lazarsfeld Report to Newark University, 1936: 6)

Lazarsfeld was very grateful for the help which the Horkheimer group gave to the Newark Centre during this early crucial period of its existence, and he subsequently repaid the debt by employing Adorno on the Princeton Radio Project. Although it would be inaccurate to consider that gratitude was the only motive involved; his employing Adorno was nevertheless a genuine expression of friendship towards the Frankfurt group whom he knew wanted to have Adorno join them in New York.

After the 'Centre' had been officially recognised by the University in May of 1936 they contributed, in addition to the necessary rooms, part of the secretary's salary and telephone bills. However, since

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1. See Komarovsky, M. "The Unemployed Man and his Family" (1940). The study itself was Mirra Komarovsky's thesis, but the whole project was supervised by Lazarsfeld. He also wrote the introduction to the book.

Lazarsfeld was not teaching full time at the University he only received part of his director's salary from that source, and even then he turned that over to the 'Centre' to assist with operating expenses. Given this lack of financial backing, much of Lazarsfeld's time was spent trying to make contacts and raise money to further the purposes of the 'Centre'. The importance with which he attached to the research director's role in establishing contacts can be gauged by the amount of committee work which he engaged in. During 1936 for example he was a member of:

The Committee on Social Research of the Sociological Society.

The Committee on Social Effects of the Depression of the Social Science Research Council.

The Committee on Youth and Radio of the Federal Communication Commission.

The Committee on Family Research of the American Association of University Women.

Commenting upon such associations Lazarsfeld writes: "As a result of these connections and activities, the university is quickly becoming known as leading in social research" (Lazarsfeld Newark Report 1936).

Certainly, in 1936 both Everett Hughes and Robert Park were interested enough in the activities of the Newark Centre to visit it, and George Lunberg of Benington College and John Jenkins of Cornell sent graduate students there for special training. Lazarsfeld of course saw research centres as being ultimately tied in with student training, but in addition it must be remembered that the 'Centre' had originally developed out of the National Youth Administration programme which, in line with the principles of the New Deal philosophy, emphasized work relief. The idea was that

unemployed students and students with unemployed parents would be provided with work at various centres throughout the country in return for a small remuneration. However, as Lazarsfeld informed the writer:

"No one knew what to do with the students. There were certain centres which had a supervisor whose task it was to invent work. I was a supervisor and that meant I had to invent work for the students. The natural thing was to let them do very large scale statistical work. There were no computers or anything so I let them do that." (1) (Lazarsfeld 15.6.74)

Lazarsfeld's general approach to the problems of training students, securing finances and developing the 'Centre' as a place of learning is further illustrated by his work for the Milk Research Council of New York on the dislike of milk among young people, his work for the Eastman Kodak Company on the use of home movies, and his work for Dupont on the perception of fabrics. Although he himself notes<sup>(2)</sup> that applied psychology had a low academic status, projects like this were well fitted to the demands of the situation. They not only provided the 'Centre' with valuable income, but could be utilised for training purposes. It never troubled Lazarsfeld that such work was not academically respectable; the question he asked of any situation was how could it be exploited to achieve the maximum advantages and to overcome the greatest number of problems that he was presented with. Hence, in addition to using

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(1) Despite the fact that Lazarsfeld was obliged to 'invent' work for his students it should not be supposed that this work was of no intellectual consequence. For example, one of the first works to come out of such activity was a large scale readership profile of magazines (Lazarsfeld 1937) which appeared in Hadley Cantril's newly established journal, Public Opinion Quarterly.

(2) See Lazarsfeld (1967: 298)

market research studies as a way of alleviating the pressing financial difficulties he faced in trying to establish a research institute, Lazarsfeld extracted as much academic meat as he could from them. An article based on his Dupont fabric study appeared in 1936 in the prestigious Journal of Applied Psychology, for example. In fact, in many ways the situation facing him at Newark was very similar to the situation he had left in Vienna, and the lessons he had learned there were readily transferred. Hence, where market research studies could be obtained they were, and used to provide work and methodological training for his students, support for the institution, and where possible, academically valuable material.

In addition, market research work helped the 'Centre' to legitimate itself within the University and by extension, the local community. For it must be understood that American universities and colleges have a tradition of being much more closely integrated into the local community than their European counterparts, a tradition reinforced in the 'thirties by the ethos of the New Deal. Certainly, if one examines the seven 'Centre' aims which Lazarsfeld listed in his report to the University then two of them: to help the City of Newark to a better understanding of its social and economic problems, to act as a consulting service to social and business agencies in the city, clearly express the hope of involving the 'Centre' with local interests. Whilst recognising that the report was a deliberate attempt on Lazarsfeld's part to justify the continued existence of the 'Centre', it must also be recognised that given the New Deal ethos such service work represented the major means through which it could become accepted. Indeed, had the

'Centre' not co-operated with business, commerce and the local community Lazarsfeld's difficulties in justifying its continued existence to the University would have been very greatly increased.

As Lazarsfeld began to establish the 'Centre' he also began to establish himself in accordance with his original hope that such an institute "would stand for me". The elements of a staff were built up during 1935-36 when the Works Project Administration of the Federal Government organised a national research project on re-employment changes and technological changes. Lazarsfeld was employed on the project as the general psychological consultant in charge of the studies conducted in the state of New Jersey,<sup>1</sup> and concentrated on the town of Millville which had become almost totally unemployed following mechanisation of the town's main industry, glass-blowing, and the transfer of the main company to Ohio. Although the work was never completed due to the discontinuation of study funds, it did allow Lazarsfeld to place sixteen students from Newark University in good posts of employment, a point which Lazarsfeld does not fail to emphasise in his report to the University.

With the money that the university employment offices delegated to the 'Centre' for thirty one National Youth Administration students a framework of operations was slowly established. For as Lazarsfeld writes to Cantril concerning his position at Newark in 1936:

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1 In overall charge of the study was Professor Frederick Gaudet of the Psychology Department of Newark University. Hilde Gaudet, Professor Gaudet's wife, became one of Lazarsfeld's collaborators on the Millville project. She later worked with Lazarsfeld at the 'Bureau' and was co-author along with Berelson and Lazarsfeld on the 'Peoples Choice'.

"I was full time employed with the national research project of the W.P.A. and did not draw any salary from the university. But I accepted the government position under the condition that my headquarters would be in Newark and so my Millville and a few similar studies helped to build up the Newark name."

(Lazarsfeld to Cantril, 8.8.37)

One of these 'similar studies' was "The Effects of the Depression upon the Family" financed by the Social Science Research Council.

The study brought Lazarsfeld into contact with Samuel Stouffer and provided the beginnings of a long-lasting academic and friendship bond between the two men. Professor Lazarsfeld related Stouffer's importance to him in the following manner:

"I was then at Newark - I would say 1936 and the Social Science Research Council commissioned this series of studies on the depression. Stouffer was made the Research Director and laid out the programme - the effect on religion, on reading, on education and so on. They were not original studies but only used available material. And Stouffer consulted with Lynd who he should invite, and Lynd mentioned my name and Stouffer invited me to go to Chicago. Anyhow Stouffer and I met in 1936 in Chicago to get the ... to see if he could use me and at that time I had done work on unemployment in Austria and was supervising Komarovsky's dissertation. That was a very strange meeting because Stouffer and I hadn't known each other. I had never met him or read his work or he mine, but we immediately got on extremely well. I remember very vividly something which was meant to be a kind of lunch to feel each other out - we stayed for hours and hours and then Stouffer immediately asked me to work with him on one of those monographs. Stouffer and I met repeatedly on those monographs and I required a tremendous respect for him and I think he rather liked me. And then when I wrote the 'Radio and the Printed Page' I asked him to write in it. So we became very close friends and there was hardly anyone I respected as much. There was continuous contact with Stouffer. He bought a little farm near Dartmouth College - New Hampshire - and I spent a summer in Dartmouth. We were together at the outbreak of the Second World War. I remember we were at his farm and listened to it on the car radio. So that was probably the most important personal contact I had."

(Lazarsfeld 2.6.73) 31.

The relationship which Lazarsfeld and Stouffer established during the Newark period was to prove extremely fruitful for, quite apart from the personal meaning the friendship had for Lazarsfeld, both men benefitted academically from future collaboration. It has already been noted that Stouffer respected Lazarsfeld for his insight when working on the depression monograph, but the respect was mutual. Lazarsfeld's academic respect for Stouffer is evident from the review of The American Soldier which he wrote for Public Opinion Quarterly in 1949,<sup>1</sup> from his introduction to 'Social Research to Test Ideas',<sup>2</sup> and from the following interview:

D. Morrison: Within American sociology it would be correct to say that The American Soldier has been a central piece.

P. Lazarsfeld: Oh, undoubtedly.

D. Morrison: And your defence of it, because it did come under attack, particularly from what one would call the Humanistic students.<sup>3</sup>

P. Lazarsfeld: Yes, certainly.

1. Lazarsfeld's review of The American Soldier was not only the longest review that P.O.Q. had printed, but was also exceptionally eulogistic. For example, he finishes the review by asking who will follow in Stouffer's footsteps: "As he approaches the last pages of these two volumes, the reader develops a feeling of frustration. Here was a gripping and seemingly inexhaustable reading material which suddenly comes to an end. Why was a war necessary to give us the first systematic analysis of life as it really is experienced by a large sector of the population? As Stouffer is about to turn off the lights he remarks, 'What happened afterwards is a story which must be told some day from data other than that assembled by the Research Branch'. 'Where, O Lord, will they be coming from?' (Lazarsfeld 1949: 404)

2. "Social Research to Test Ideas" was the collected writings of Stouffer and published posthumously. It is therefore of special significance that Lazarsfeld should write the introduction. (Lazarsfeld, 1962)

3. See in particular Lerner (1950) and Schlesinger (1949). Schlesinger bitingly calls the American soldier the 'Statistical Soldier'.

- D. Morrison: Your defence of it, how much was it personal respect, I've always thought that it went beyond that and into a defence of a style of work that you wanted to see go ahead?
- P. Lazarsfeld: Well someone made the joke at the time that I was so generous to what Stouffer did that I just wanted to say make it my own and become the main propagandist - so not only out of respect but out of jealousy. It was a spectacular event and Stouffer's ability to work, he was just perfectly unbelievable. When you talk to Merton (1) ... because something is very strange. Merton, who is very different, is also a really spectacular person. Merton and Stouffer never clicked.
- D. Morrison: Personal reasons or ...?
- P. Lazarsfeld: I have never really worked that out. Something may help you here. Stouffer always felt that Merton overdoes ... you know this thing Merton and I put out, the continuities thing on the American Soldier. Well I mainly traced the statistical procedure and Merton traced more the qualitative material and Stouffer always felt that Merton didn't appreciate enough what he ... indeed, what is best known about Stouffer's work in today's generation would be what Merton wrote. Stouffer felt you see that by giving it ... subsuming it in a more general terminology like reference group it was unfair. And that was Stouffer's feeling. Merton felt that Stouffer was too dry. I don't know what Merton missed in Stouffer, but they never had the intensity of respect for each other which I had for both of them. I would think of Merton as equally impressive intellectually as Stouffer.
- D. Morrison: Did Merton not see Stouffer's work, to use the term, as 'fact grubbing' because of Merton's theoretical sophistication?
- P. Lazarsfeld: Yes, I suppose so, but the point is Merton would be wrong.
- D. Morrison: I'm not arguing it myself.
- P. Lazarsfeld: Yes, Stouffer was extremely thoughtful. But look, meeting Merton would be a great event for you. First he is Mr. Sociology in this country, and unbelievably brilliant. (Lazarsfeld, 2.6.73)

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1 Unfortunately, due to illness on Professor Merton's part, the writer never actually got to see them.

Lazarsfeld's relationship with Merton will be returned to later when discussing the Columbia 'Bureau' in detail; however, for the present it is important to note his establishment of contacts which were subsequently to aid the establishment of 'his institute'.

When Stouffer became director of the professional staff at the Research Branch, Information and Education Division, of the War Department, he channelled some of its work through the 'Bureau', which not only provided it with a valuable source of finance but in addition conferred official recognition and status. Both Robert Merton, who was at the 'Bureau' by then, and Lazarsfeld himself, who was a consultant<sup>1</sup> to the Office of War Information on survey work, contributed a great deal of time and energy to the American Soldier studies. A sizeable part of the third volume of the study, which Karl Hovland was largely responsible for, was taken up with the Bureau's contribution, and after the war Lazarsfeld spent a full term at Harvard helping Stouffer with the measurement procedures for the first volume.

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1 In fact, this was how the money was paid to the 'Bureau'; that is, money could not be paid direct, but only in the form of consultancy fees which Merton and Lazarsfeld then turned over to the 'Bureau'. However, a difficulty did arise at this time which provides insight into Lazarsfeld's disorganised life style. Although Lazarsfeld had arrived in 1933 he had never bothered to acquire American citizenship; thus, when the war broke out "I was so to speak an enemy alien, and during the war no enemy alien could become a citizen". Thus, not being an American citizen, Lazarsfeld could not engage in such military research. However, as Lazarsfeld informed the writer, "By a lucky coincidence we had to fill out - as resident aliens - to fill out papers. Austria was occupied in 1938 and most Austrians began to call themselves Germans, but I just out of spite kept on calling myself Austrian. That was the legal loophole by which I could then get citizenship during the war - because the fact that I kept calling myself an Austrian while legally America had accepted Austria as part of Germany you see. Somehow Stouffer managed all that, because he wanted me as a consultant. I was one of the very few people who got citizenship during the war - because Stouffer couldn't have used me or the 'Bureau'". (Lazarsfeld, 2.6.73) Indeed, it makes amusing reading to see some of the 'loyalty checks' which were made to the Rockefeller Foundation before Lazarsfeld was granted citizenship.

Undoubtedly, Stouffer was the main intellectual contact that Lazarsfeld made during his initial years at Newark. However, as important as this contact undoubtedly was, it should not be seen in isolation, but rather as one element within a wider cluster of factors which aided the development firstly of the Newark Centre and later of the Columbia Bureau. Nevertheless, it was largely through the contacts that he made and the work which they generated, that Lazarsfeld, together with 'his institute', was gradually pulled into the mainstream of American academic life. Indeed, when he was well established, Lazarsfeld acknowledged his debt to Stouffer by dedicating one of the Columbia Bureau's major works, The People's Choice, to him. At the same time that his institute came 'to stand for him' however, it also came to stand for an important strand within American sociology; large-scale, institutionalised empirical research.

#### Institutional Involvement: The Newark Centre

The suitability of research institutes within which Lazarsfeld's intellectual interests could be accommodated has already been mentioned; however, in so far as accounting for his early American involvement with research institutes is concerned other factors were of over-riding importance. Although Lazarsfeld's main interest has always been in methodology, and undoubtedly a research institute provides an excellent vehicle for satisfying such interests, but also for transmission and propagation of such knowledge through the practical training which such institutes equip students with. Yet, Lazarsfeld's own structural position is of greater over-riding

importance when considering his institutional association. Such factors as the above, whilst adding a further dimension to such involvement, are nevertheless of secondary consideration when framed in the context of the difficulties he faced of securing an academic appointment within the American university structure. In all likelihood he may well have been. For, if Lazarsfeld can write of Stouffer that "His academic life coincides with the development of empirical social research in the United States" (Lazarsfeld 1962: 15), then one can equally write of Lazarsfeld's academic life, that it coincides with the development of social research bureaux. Hence, having traced the establishment of the Newark Centre as one of the first social research institutes, the work now turns to a study of its development into one of the leading research bureaux in America.

## CHAPTER FOUR

### THE PRINCETON RADIO RESEARCH PROJECT

CHAPTER FOURTHE PRINCETON RADIO RESEARCH PROJECTRadio Research: A Director Without Interest?

Whilst it would be a perfectly valid exercise to concentrate upon Lazarsfeld's contribution to mass communication research since he was, to use Schram's phrase, 'a founding father' (Schram 1959: 6) it is not the writer's intention to do so. Undoubtedly Lazarsfeld has been of singular importance in the history of that field, especially when one considers that he was the first to apply survey methods to mass communication research. In addition Berelson notes:

"In my view, Lazarsfeld was the only one of the four (founding fathers) who centered on mass communication problems per se: Lasswell was interested in political power, Lewin in group functioning, and霍夫兰 in cognitive processes, and they utilised this field as a convenient entry to those broader concerns."  
(Berelson 1959: 5)

However, even accepting that Lazarsfeld was the only one of the four 'founding fathers' to address himself to communications problems per se, it is nevertheless true that he also utilised the field as a convenient point of entry into his broader concerns. Mass communications research was never his main area of interest, and it is doubtful whether he was ever really interested in the substantive content of the studies at all. It has already been noted that upon being offered the Directorship of the Radio Project, he wrote to Cantril expressing his lack of interest in the area, but stating that the project fitted well with his other interests. Furthermore, Lazarsfeld

himself has been quoted directly as saying he had no interest in the field. However, in consideration of the fact that he has played such a seminal part in the development of mass communications research it is intended to clarify this point further.

Roger Brown, whilst admitting that the "full history of mass communications research still waits to be written" (Brown 1970: 41) nevertheless goes some way towards providing an interim account. Unfortunately, in the course of his analysis he nominates a number of potential influences on early mass communications research which, whilst appealing, are difficult to substantiate. For example Brown, in common with a number of other commentators, sees the notion of 'mass society' as having been particularly influential in generating interest in mass communication research. For example, commenting upon Shil's<sup>1</sup> summary of the growing concern over the increasing scale of industrial societies as being one of the central themes of nineteenth century sociology, Brown writes:

"A whole chapter in the history of social thought is summarised here, but it was of course the concept of the isolated, unrestrained, atomised, possibly anomic individual which had the greatest impact on ideas about the mass media. If societies were like this, then persuasion via the mass media was all too easy, since a person was restrained from accepting new ideas by very few social ties or traditional orientations."  
(Brown 1970: 46)

This argument certainly does not apply to Lazarsfeld's work and one suspects that, apart perhaps from Laswell, it would not

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1 The article referred to by Brown is Shil's "The Study of the Primary Group". (Shil 1951)

fit other cases either. In the history of ideas there is the over-present temptation to attribute a thinker's interests or state of consciousness to factors which, when examined closely, were simply not present at the time. To be sure much mass communication research fits readily into the general concerns of "mass society" theorists but it is a mistake to argue that the research itself was prompted by these concerns. Rather it is more accurate to see such research as contributing indirectly to the debate through the provision of empirical data rather than as a direct product of the debate itself. Although concern over the increasing scale and fragmentation of industrial societies has featured centrally in many seminal sociological works, the 'mass society' debate itself was restricted to America in the 1950's. It never gained much currency outside those shores, and has now largely faded even within America. In the light of this, it is particularly noteworthy that Lazarsfeld, in his introduction to "Personal Influence" written in 1955, relates mass communication research to concerns with the 'mass' in a not too dissimilar manner to that of Brown.<sup>1</sup> Undoubtedly "Personal Influence" centred upon areas relevant to the 'mass society' debate, but then so did its historical precursor, "The Peoples Choice", in which no attempt was made to 'pull' the work into such areas of interest. Whilst it is admitted that "Personal Influence" is probably more central to the debate than "The Peoples Choice" the explanation for the difference in 'presentation' lies in the

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<sup>1</sup> See in particular page 17 of "Personal Influence" <sup>KAR</sup> (Lazarsfeld 1955)

passing of ten years between the production of the two books. Certainly if one is attempting to trace the historical roots of mass communications research, then the absence of such comment in the earlier work is not good evidence for supposing that it derives from 'mass society' concerns. In addition, it is worthy of note that "The Peoples Choice", written in 1944, was at one point intended as a study of a farming programme which the department of agriculture periodically transmitted to gain support for its policies, and not an election study. It was only with the November elections in 1940 that the plan of action was actually changed. Whilst admitting that a study of agriculture may well have produced conclusions relevant to the mass society debate it does detract somewhat from the central area of concern and hence support for such a thesis.

While readily admitting that the 'mass society' debate and the issues which it raises concerning the structure of society constitute an important theme within sociology, the writer objects to interpreting modern mass communications research in light of such concerns without first documenting very closely where such concerns impinged on the formulation and execution of particular studies. It is not sufficient to simply examine finished works and then assert that they 'fit', especially since the concerns of the 'mass society' theorists are so broad and such an integral part of overall sociological concerns that they are almost bound to fit. Whether or not early mass communications research were stimulated by such concerns is an empirical question, and ought to be handled as such. With this in mind the case of Lazarsfeld will be further examined.

As previously noted, Lazarsfeld was interested in mass communications research primarily as a means of academic advancement and

as a means of refining methodological techniques. As he readily admitted in interview, he had little or no substantive interest in the field;

D. Morrison: I appreciate the situation that you faced job wise in America, and the fact that you had already done some work on mass communication in Vienna.

P. Lazarsfeld: That's true.

D. Morrison: But what attracted you to mass communications? Were you influenced by what was happening on the continent? That whole culture debate which the Frankfurt school engage in - did that orientate you towards ...?

P. Lazarsfeld: Well first, look, you have to understand that I had no interest whatsoever in mass communications. I mean no - I mean everything in a way is interesting to a methodologist, but I certainly didn't find that in the beginning an important topic at all. It was exclusively that it was a rather spectacular job, and then my conviction that anything you do research on is interesting. I mean, there was always objections to my work all my life that I was really only interested in any - only methodological work. But then the way I got interested, really interested in the mass culture debate was my ambivalence. On the one hand I got money from the industry - everything depended on that; and on the other hand I was tied to a team - my union card as a socialist. So I continually tried to mediate between the culture critics." (Lazarsfeld 15.6.73)

The "mass culture" debate was a later addition to Lazarsfeld's interests and came well after he had helped to establish mass communications research. The ambivalence deriving from his socialist 'union card', and his financial entanglement with the culture industry, is well illustrated in his introduction to the collection of essays in 'Culture for the Millions' (Lazarsfeld 1959) where he adopts an intermediate position between the leftist critics and the straightforward managerial apologists. Lazarsfeld's ambivalence towards his past as a committed socialist on the one hand, and his involvement

with business concerns on the other, was raised in the course of discussion the writer had with Bernard Berelson, one of Lazarsfeld's co-authors on "The Peoples Choice". According to Berelson, Lazarsfeld:

"was substantially interested in the voting studies. He could pretend that it was a political topic of importance - how do people make decisions in a capitalist democracy? - which gave him some bona fide link with his socialist history, but he never was - socialist - and Hans Zeisel neither. Hans Zeisel worked for one of the major advertising companies for most of his career here,<sup>1</sup> and you don't do that against a genuine solid background in European socialism."  
(Berelson 12.7.73)

However, the fact remains that Lazarsfeld was basically uninterested in mass communications, a point which did not escape John Marshall of the Rockefeller Foundation, as the following conversational abstract illustrates:

D. Morrison: I'm not sure as to whether I have a disagreement with Professor Lazarsfeld when he informed me that he was never interested in mass communication research. When I talked to Dr. Barton he found it difficult to accept as well. What do you say to that? Were you aware of it?

J. Marshall: I suspect Paul's right. In other words he wasn't interested in the media if that is what he means. He perhaps says it inexactly. He was interested primarily in methodological opportunities which research on the media offered. He might have been just as interested in some other field of research where methodological opportunities were the same. I think Paul is quite honest in that - his basic interest was always methodological. He didn't care much what field he worked in. Methodological considerations were always uppermost in his mind. In fact we went along with that. I remember distinctly that when the 'Bureau' was, as it were, becoming two-sided - it earned a good bit of its way from research that it was paid to do. That was a little bit troublesome from the point of view of the Foundation in that we could not be directly involved in any profitable enterprise, and had to resolve the difficulties by saying that the 'Bureau' would not be open to all comers for paid research; that it would accept research just as it would contribute to its methodological lot."  
(Marshall 6.7.73)

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<sup>1</sup> Zeisel did in fact work for McCann-Erickson, one of the three largest advertising agencies in the world.

Such an insistence by the Foundation was very easy for Lazarsfeld to subvert, not in any deliberate manner, but from the position that any study contributed to methodology, although the intrinsic triviality of some of the paid-for studies did create strains within the 'Bureau' itself. However, the point to underline in the present context is that such studies, whilst admittedly providing the 'Bureau' with money, are an index of Lazarsfeld's wide-ranging methodological interests, of which mass communications research was merely one part. It mattered little to him what the studies were, a point Feralson stressed to the writer:

"Well Paul liked to say, partly true and partly to tell a good story on himself, that what he wanted to study was how people made decisions and the Rockefeller Foundation wouldn't support him - to decide why people would choose this coffee rather than that coffee, but they would support him to find out why they voted for Roosevelt rather than Wilkie. So he studied that. He would have studied anything that they would pay for so to speak. Well, that was part of his so-called Viennese charm, but there was a little something to it - Paul was much more interested in methodological innovation and methodological subtleties and niceties than I ever was, but I'm not putting myself in comparison with him. I was always much more interested in the substance. I'm not denegrating him in saying that, but Paul would be - he was more delighted in having a clever methodological table come out of the Kolarith machine - in those days - and whether it had major substantive importance was secondary to that."

D. Morrison:

"You never felt that he was interested in mass communications?"

B. Feralson:

"Well, we may get into semantics here. I would have said that Paul was first of all interested in - much more absorbed with technical virtuosity. I don't mean that in a necessarily bad way - the methodological aspect of investigation, and secondly, on the substantive side, if one had to generalise, I would say he was interested in how people came to take positions. Mass communications fitted into both in that it provided opportunity for the

former, and it was at the same time a convenient way to do the latter because it was an attractive subject and funds were in the air. If he were what, twenty five years younger, he might be doing it on population because population became an attractive subject in the sixties for funding. Mass communication just happened to have a vogue then, but it could have been another subject."

(Berelson 12.7.73)

The point which Berelson raises concerning semantics is apposite, since whilst recognising that Lazarsfeld had no 'real' interest at the substantive level in mass communication research, it is possible to discuss levels of interest. It has already been noted that Berelson considered that Lazarsfeld was interested in the voting studies, and it would indeed appear anomalous for someone to remain in a field of enquiry for approximately fifteen years without some genuine interest in the subject matter itself. In consideration of this, the following remarks of Marie Jahoda's are illuminating:

D. Morrison: When I met Professor Lazarsfeld in New York he repeatedly impressed upon me that he had never been interested in mass communications. Were you surprised when he moved into mass communication research?

M. Jahoda: No, it started, the whole of the Forschungsstelle started with the idea of a radio barometer. Mass communications had been very central. I wasn't at all surprised when he went into it.

D. Morrison: So you think such statements might be a rationalisation of some kind? I thought it might be a rationalisation to his biography. You see, at first it appeared to me that he was aware of his own biography, and his role as a methodologist - because that's what he is - so he says he was never interested in mass communications as such.

M. Jahoda: You know, when you put it that way, I'm not so sure (about being interested in mass communications). I agree mass communication research is just so good for methodological problems ... but then, you know at that time he was married to Herta Herzog and she was very intensively involved. She did the soap opera studies<sup>1</sup>. She was very good, excellent studies.

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<sup>1</sup> See in particular "On Borrowed Experience" which first appeared in Hertzbeimer's Journal "Studies in Philosophy and Social Science" (Herzog 1941).

It was natural - the voting studies were just as natural - areas suitable for the development of sophisticated methodology. After all, he edited those volumes on the subject, and the voting studies - mass communication voting studies - there was a clear transition. Because you know, mass communication in 'The Peoples Choice' plays a major role. No, it was a competent area which he was familiar with from Vienna, and which during the war years, because of the propaganda studies in America, was very salient, and which lent itself to methodology. So he can't get away with saying he was never interested in mass communications."  
(Jahoda 26.9.73)

Professor Jahoda remains a somewhat lone voice, however. The rest of Lazarsfeld's friends, colleagues and co-workers corroborate his own testimony that he was only interested in mass communications research as a convenient means of pursuing his main interests. Berelson is essentially correct when he characterises Lazarsfeld's basic concerns as methodology, and "how people come to take up positions". This latter interest, which grew out of his early concern with the explanation of 'action', led directly to his substantive interest in the voting studies. Thus the interests which Lazarsfeld developed in Vienna can be seen as constituting a consistent theme throughout his life work, a theme which was later joined by an interest in educational institutions arising out of his own institutional career path.

Because Lazarsfeld has played such a central role in the history of mass communication research it would not be amiss to focus upon that aspect of his life. Such an exercise would, however, detract from the work's central aim of tracing Lazarsfeld's role as an institutional innovator. The two tasks cannot be entirely separated however, since it was primarily because of his work in mass communications that Lazarsfeld was able to pioneer

the development of research bureaux. As John Marshall noted in 1941, in his reflections on the Princeton Radio Project:

"To complete the record, attention should be drawn to the fact that the history of this Project is likewise the history of Dr. Lazarsfeld's establishment in this country. The officers were informed on his selection as director by Princeton but not consulted on it. But through the work of the Project an Austrian fellow of the Foundation, who was unable to return to his native country because of political development, within ten years time won a reputation for himself as an expert in his field and found a permanent place for himself on the faculty of one of the leading American universities." (Marshall, September 1941)

#### Radio Research: A Foundation with Interest

"It was, on motion, resolved that the sum of sixty-seven thousand dollars or as much thereof as may be necessary be, and it hereby is, appropriated to Princeton University towards a study to be made in the School of Public and International Affairs of the value of radio to listeners, during the two year period September 1, 1937 - August 31, 1939, the amount available in each year not to exceed 33,500." (Resolution 21.5.37)

The above represents the first important move on the part of the Rockefeller Foundation into the field of mass communication research; a field upon which it was to have a tremendous impact. Indeed, according to Berelson (Berelson 1959), the Rockefeller Foundation was not only responsible for co-ordinating mass communication research, but also for stimulating the modern academic version of it. The commercial interest in such research was stimulated, in Berelson's view, by the radio industry's need to prove its audience. However, the industry's own research, and the research of its servicing agencies, was particularly crude and

it was not until the Princeton Radio Project began to develop more sophisticated techniques for research and analysis that such a state of affairs was gradually corrected. For example, Lazarsfeld writes to John Marshall in 1940 that:

"I thought you might be interested in a cute example of the relationship of foundation research and commercial research.

When I started surveying the radio field in 1937, I realised that the programme ratings which were used only for gauging the size of the audience could also be used for the study of social stratification. We obtained the permission of the Cooperative Analysis of Broadcasting to analyse their material and our publication "The Social Stratification of the Radio Audience" came about. Then the POB published a summary of our findings.

Now the CAB has put the summary in pamphlet form and is sending out many thousands of copies for their own promotion. It took them all this roundabout travelling to find out what they had at hand. Incidentally, you will find the typical inventor's tragedy; we have only been given no more credit than a footnote at the end of the foreword."

(Lazarsfeld 6.9.40)

In addition to illustrating the limitations of the radio industry's own research, this instance provides an insight into Lazarsfeld's skill as a project director. For, not only did he apparently secure valuable data from C.A.B., but, in addition, had the presence of mind to inform John Marshall of the compliment bestowed upon the project through the reproduction of its results, thereby helping to cement relations with the Foundation.

The position of the industry meant that most of its research hardly rose above the level of head-counting. The industry was more or less exclusively concerned with the listener as a prospective purchaser. The research divisions of the radio companies, almost without exception, were organised to promote the sale of time, and actually engaged in very little direct research themselves. Most of their energy was devoted to the interpretation of data obtained

from outside agencies such as the C.A.B. or Crosley Survey, and the Clark-Hooper Survey. Such commercial agencies served not only the radio industry itself, but advertising agencies as well; consequently their studies were mainly concerned with identifying which members of the radio audience who were likely to buy the product advertised on the radio. In addition, since most of the agency enquiries were conducted by telephone it meant that even in the area of establishing audience listening, such research was deficient, not least because many radio listeners did not possess a telephone. Undoubtedly this data, crude though it was, provided a certain basis for broadcast planning which served the interests of advertisers, but it was the 'needs' of the audience which the Foundation were most concerned about. The close interlocking interests of the radio industry with advertising troubled those in the Foundation who were interested in the qualitative improvement of radio broadcasting, and the development of a radio network servicing the nation. The Foundation's concern about the industry's research collaboration with advertising can be gauged from the following abstract of a memorandum which is probably the summary of a conversation that John Marshall had with Frank Stanton of the Research Division of C.B.S.

"More important still in this respect is the relation which has grown up between the industry and the advertising agencies on the one hand, and between the agencies and buyers of time on the other hand. It is a fact that one of the national chains felt obliged deliberately to curtail its research to maintain good relations with the advertising agencies who acted as intermediaries of sponsors in the purchase of time. Data which the chain was collecting proved contrary to some of the contentions on which the agencies were selling time. To maintain its profitable relations with the agencies, the chain's only choice was to give up the research." (Memorandum, May 1937: 6)

The suspicion which the industry harboured towards more penetrating research and its unwillingness to experiment led John Marshall in the course of an interview with the writer to describe the industry's position as one of operating from "unenlightened self-interest" (Marshall 6.7.73). The situation which confronted Frank Stanton at C.B.S. provides an excellent illustration of this attitude. Stanton in 1936 had been given the responsibility of presenting C.B.S.'s evidence before the Federal Communication Committee's hearings on the allocation of radio facilities. During the course of his preparatory work he discovered that the industry's assumption that listeners interests were directly related to signal strength was incorrect. In fact, a majority of listeners indicated as their favourite programmes those which were third or even fourth in the order of reception quality. Consequently he urged C.B.S. to conduct a study to discover the basis of listeners' liking for various programmes. The proposed study was turned down on the grounds that it would have "no immediate commercial value".<sup>1</sup>

It is a characteristic feature of foundations to support an area of research only so long as foundation money is essential to its success. Once other finances begin to flow into that area they tend to disengage themselves from support, and search for other areas of work where their monies can have greater proportional effectiveness. In the case of radio research, however, not only

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<sup>1</sup> Recorded interview between Frank Stanton and John Marshall 6.11.35

was the field virtually unmapped; there was also little possibility of other agencies channelling funds in that direction. For as George Gallup wrote to John Marshall regarding Hadley Cantril's proposed radio study:

"Most of the research in the field of radio has been concerned with the measurement of the size of audience listening to various programmes. We know virtually nothing about the influence of radio on listeners, the reasons why persons listen to certain programmes and not others. The organisations which are now engaged in radio research are not in a position to do research of this character largely because research procedures are required. Since the results have no immediate commercial value it is extremely unlikely that such a project would be undertaken by industrial concerns. Again, let me say that I am most enthusiastic about this plan and hope that your committee will pass favourably upon it." (Gallup 19.5.37)

Concern already existed within the Rockefeller Foundation over the state of American broadcasting, and they felt that if the public was to be provided with a better service socially relevant research had to be conducted, and given the absence of interest on the part of other agencies the Foundation, as early as 1934, began to consider the matter themselves. The responsibility of such preparatory groundwork rested with John Marshall as the assistant director of the humanities division, but the actual initiation of the Princeton Project stemmed from the research proposal submitted by Hadley Cantril, Professor of Psychology at Princeton University, and author of "The Psychology of Radio". The circumstances surrounding the launching of the project were described by John Marshall in an interview with the author:

D. Morrison:

I know that you supported linguistic work by Professor Cable Greet in 1936. Was that how your interest in mass communication was generated? ... and you also supported Robert Sonkin in that area.<sup>1</sup>

John Marshall:

No, I think it had no connection whatsoever. Well the story is relatively simple. We had a committee of trustees of the Foundation that was established in 1933, and I think reported in 1934. They were supposed to review everything the Foundation was doing, and in the end make recommendations to the full Board of Trustees as to what ought to go on. Mid-term in their enquiries they put a series of questions to the officers. In this case the officers of the Foundation in the division of the Humanities of which I was an assistant director. One of the questions they asked was "to what extent the Foundation could usefully concern itself with other media of communication other than print? - well radio and so on. The director of the Humanities at that time was a man named Stevens - David H. Stevens and I was his only assistant.<sup>2</sup> That's how he knew that I had a long interest in the radio as a medium. As a matter of fact, as a boy I had my own radio transmitter, and had always been interested in what went on in radio, and therefore he asked me to take responsibility for enquiries as to what the Foundation might do with respect to radio and also incidentally film.<sup>3</sup> I began studying possibilities probably in 1934.

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- 1 Gable Greet was a professor at Barnard College, Columbia University and was particularly interested in radio and speech. Robert Sonkin of Columbia University was similarly interested, particularly in style of voice and its pitch. Whilst going through the Rockefeller archives the writer discovered applications for support in the above research areas and thus presumed that it formed the basis of the Foundation's interest in radio research. Interestingly, Bernard Berelson, in conversation with the writer (12.7.73) made the same mistake. However, such work formed a completely different interest of the Foundation and rested much more in the mainstream humanities than did the Lazarsfeld Radio Research which, although supported by the Humanities Division, was distinctly 'sociological' from the outset.
  2. A surprising fact of Foundations is the small number of staff employed in the dispensing of such large funds.
  3. A striking feature of mass communication research is the absence of research in the area of motion pictures; the only major work is Handel's audience study (Handel 1950). Handel had been a student of Lazarsfeld's and applied the whole Columbia mass communication tradition to the area. So far as Lazarsfeld's own non-engagement in studies is concerned he informed the writer that: "the movie industry was terribly disorganised. I mean, there was nothing like networks - it is in the nature of the movie industry. The movie industry was so hectic - this continuing change of ownership - so there was never any movie research similar to newspaper research" (Lazarsfeld 15.6.73). The Rockefeller Foundation was certainly very serious about attempting research for "the general improvement of motion pictures in this country as Lazarsfeld's work now seems likely to be in the improvement of broadcasting". However Marshall and others despaired "of ever getting the temperamental executives of the industry at the point where they both want it and can pay for it" (Marshall Suter, Office Correspondence 17.9.40).

Well, from 1934 I was very much concerned in studying the situation in radio broadcasting. I had good access to the industry - naturally the name Rockefeller Foundation took me almost anywhere I wanted to go. We began to flounder around as I see it now, under a general heading "Experimentation with Radio Programmes and Cultural Barriers or Cultural and Educational Barriers". Well naturally during this period, while we were giving on few experiments and programmes of that character, I was reading as widely as I could, and one day I was finishing as I recall "The Psychology of Radio" by Hadily Cantril and - I forget his name - the Harvard psychologist

D.M. Gordon Allport.

J.M. Yes, that's right, Gordon Allport, and towards the end of that book there is a statement to the effect "of course we really don't know whether or not what we are saying is true but we shan't know until we have done some research".<sup>1</sup> As I say the statement caught my attention. I talked it over with Stevens, and I tried to find Cantril at Princeton to discover he was up at Harvard for a year. I got in touch with him there. I went up to see him, and in the course of the talk said "well, why don't you get some research going?" He said "I would like very much to". That was the beginning - as simple as that. I don't think I had any interest in the possibilities of research in that field until I read that sentence."  
(Marshall 6.7.73)

Such were the beginnings of the Princeton Project. If one wished to pin-point a particular catalyst in the whole process of development then Marshall's introduction to Cantril's work would appear to form an appropriate starting point; yet, to search for a specific mechanism of release would be to miss the context within which the radio research began.

The driving force behind the Project was undoubtedly John Marshall, and in many ways the radio research programme reflected the clash between the content of commercial broadcasting and Marshall's own

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<sup>1</sup> Marshall is probably referring to page 271 of Cantril and Allport, 1935 where such an appeal is made.

cultural background. The writer had the fortunate experience of spending a whole day with John Marshall at his home in Connecticut, just outside New York. A variety of matters were discussed which, whilst not directly bearing on the Radio Project, nevertheless had considerable importance when attempting an appreciation of its overall setting. Although previous to seeing Marshall, Lazarsfeld had described him to the writer as "an extremely cultured person", the dimensions of such a description were only fully appreciated upon an actual meeting with him. Born into the Boston middle class, Marshall studied medieval history at Harvard University before going on to read for a Doctorate in that field. However, he discontinued his academic work, preferring a career in the Rockefeller Foundation. Marshall's respect for Lazarsfeld stemmed in part from the fact that Lazarsfeld came from a cultural tradition he greatly admired, and was, as he put it, "a fine example of the highly cultured Viennese Jew". Similarly, Marshall's corresponding distaste for the type of cultural product produced by the broadcasting companies sensitized him to the need for alternative arrangements.

"Basically, I felt that our interest in the Foundation in radio research was for the possibilities we believed it had of demonstrating that we didn't have to have the kind of radio programmes we had. That is, that there was an audience for radio programmes of a higher educational cultural value, and that by demonstrating this through research to the broadcasting companies leverage could be asserted to adopt a more cultural and educationally valuable type of programme... Our slogan was widening the area of public appreciation. I think we meant widening in two senses. One, widening an audience that was appreciative of the better things in life and two, widening the area that programmes of that character covered. How do you take people into literature? How do you take people into art and music? Music was one of the things we were particularly interested in, and we financed in that foundering effort to give support to experimentation in radio programmes of culture. I'm using cliches because these convey the central task."

(Marshall 6.7.73)

Hence the interests of the Foundation as represented by Marshall lay in demonstrating to the broadcasting companies that there was an audience for a different kind of cultural fare from that which was currently being offered. At the same time, his position carried a distinctive proselytising element as evidenced by his wish to create an audience that would appreciate a different cultural content. However, whilst accepting that such a position does have connections with the mass culture debate, which later came to form part of the mass society debate in the 1950's, it would be mistaken to see such a concern as determining the direction of Marshall's research interests. The question of cultural standards and levels of appreciation has very ancient historical roots, going at least as far back as Plato, and if one wished to place Marshall's interest within a particular tradition, then this strain of social thought would be a much more appropriate context than the concern with the increasing scale of industrial societies and the standardisation of cultural products.

Marshall's concern over the dearth of cultural programming on radio was shared by a number of others, including Hadley Cantril, who in the Spring of 1936 sent Marshall a proposal for a "Preliminary Study to Devise a Method for Ascertaining the Effectiveness and the Effect of Radio Programmes of a Broad Cultural Nature". The proposal reads:

"Techniques for following up the success of commercial radio programmes have already reached an advanced stage of development due to the sponsors interest in the effectiveness of his programme. Techniques for following up the success of what might be termed programmes of a 'cultural' nature have, on the other hand, scarcely begun. This is due not only to

the relative lack of interest in such studies and to the lack of funds to carry out such research, but also to the difficulties of evaluating the listener-value and the listener-effect of such programmes. Whereas the commercial sponsor is concerned chiefly with sales and is satisfied with a rough quantitative statement of the value of his radio programme in terms of sales promotion, the individual interested in finding out the cultural value and listeners reactions to a given programme is confronted with a much less definite task. He wants to know not only how many people listen, but why they listen and how the programme is affecting them in their personal lives. As a result of this state of affairs we have relatively little data that might be used to guide a broadcaster in the formulation of a cultural programme. And as a necessary corollary we have just as little data that might be used by the educator or the social scientist anxious to learn something of radio as a social force. The investigation proposes to attack this problem in a very modest way." (Cantril 16.5.36) 15

Marshall undoubtedly agreed with this line of argument and encouraged Cantril to submit a fuller proposal. Hence, on December 31st of that same year, Cantril sent Marshall a lengthy letter in which he laid out the detailed proposals which formed the basis for the Princeton Radio Research Project. The opening part of the letter reads:

"It seems to me that the time is ripe for a thoroughgoing investigation of certain basic problems confronting all of us interested in the best use of radio. Radio has quite obviously established for itself a permanent and important role in the cultural life of the nation. Its growth in the past decade has been phenomenal. Radio listening has become a habit with the vast majority of the population. The techniques of broadcasting have been enormously perfected. In brief, radio has reached its maturity. But the growth of the medium has been so rapid and its direction and organisation have been so largely fortuitous that there is comparatively little knowledge concerning the basic human factors upon which the success of radio depends. Radio has developed because it has satisfied genuine human needs. But what, precisely, these wants are is still an open question." (Cantril, 31.12.36)

The proposal's central argument was that radio had developed as a mass medium without any systematic study of it having taken place.

Furthermore, unless the Foundation shouldered the financial responsibility for such research, the questions which Cantril raised would, in the foreseeable future, remain unanswered since, as already noted, the industry would neither undertake the necessary research itself nor support others to do it for them. Commenting on the Princeton Project Marshall wrote:

"Representatives of the industry readily agree to the importance of the proposed study for the development of radio's public service. But they see little likelihood that the industry could undertake such research for itself at present. This view is explained by various considerations thought to be important commercially. For example, advertisers have come to believe that the tastes of one socio-economic group are much the same as the next; this leads the industry to feel that it cannot afford to support research which might threaten existing concepts of mass appeal. Or again, studies of when listening takes place involve a similar threat to the existing rate structure. Thus the industry, though willing to have such research undertaken by outside agencies, is not ready to run the risk of taking the initiative." (Marshall, 21.5.37)

It was a source of considerable disappointment and even anger to Marshall that the industry was not more forthcoming in offering financial assistance to potential researchers. Rather the industry followed the line of least resistance which, given the interlocking interests which had developed between the radio industry and the advertising agencies and between the agencies and programme sponsors, meant that out of self-interest the radio companies did not wish to produce results which ran counter to the sponsors' and advertisers' expectations. Cantril summarises the situation thus:

"A further instance of the lack of the industries' interest in detailed listener research is revealed in their attitude towards securing data on what the listener does while he listens. While this is important in an understanding of listener behaviour, the broadcasters, in most cases at least, are not interested in the findings because they cannot afford to indicate that people do anything but listen while the radio is in use!

Finally, business is so 'good' in broadcasting that the industry is not inclined to spend money for research until they are forced to do so, not only because of the embarrassment such data might cause, but because business does not demand it. Ultimately the industry will have to go deeper, in a research sense, in order to maintain its position. This will be in the distant future, perhaps. In the meantime, we might obtain data to guide the educator and direct the appeal of broadcasts.

I have taken the liberty to go into these problems at much length, since you may wonder why the industry did not assume some responsibility for Project I."

(Cantril undated, probably late 1936 or early 1937)

'Project I' refers in fact to the Princeton Radio Research Project which went under the heading of "The Essential Value of Radio to all Types of Listeners", and was almost word for word the same as the proposal which Cantril detailed in his letter to Marshall following their discussion together. Although submitted by Cantril, the proposal had in fact been drawn up in collaboration with Frank Stanton, and carefully reviewed for the Federal Radio Education Committee by an informal committee comprised of three educators, and three broadcasters, representing the National Broadcasting Company, the Columbia Broadcasting System, and the National Association of Broadcasters, since it was felt that for the results obtained to have the maximum chance of influencing the industry the FREC's endorsement was essential. In the event the project was 'supported' by the FREC, and once it became operative the committee of six became the FREC's executive committee responsible for reviewing the project's progress. As well as securing the industry's support by having three members of the industry "whose authority cannot be questioned" on the executive committee, it was also considered that the presence of three prominent educationalists would assure "due protection for educational and cultural interests" (Consideration of resolution to finance Princeton Project, 21.5.37).

One of the interesting aspects of the proposal was its lack of systematic formulation. The section headed "Problems and Methods" lacks both rigor in formulating the problems to be tackled, and precision over the methodological procedure to be followed. Although some of this 'looseness' can undoubtedly be attributed to the novelty of the venture, a point recognised on page four by the statement "in view of the lack of methodological technique for studying such problems, it will be necessary to devote two years to the development and test application of such techniques" (Project I proposal The Essential Value of Radio to all Types of Listeners), nevertheless the whole proposal has a hurried and adhoc appearance to it, almost as if matters have not been clearly thought through. In fact Lazarsfeld, after discussing the Project with Cantril, wrote to Robert Lynd that:

"As to the subject-matter side of the project, it is the most amazing story that I have ever heard. The name of the project reads: "The Essential Value of Radio to all Types of Listeners". That is all that is known about the project."  
(Lazarsfeld, 9.9.37)

Lazarsfeld further mentions that there was nothing in existence apart from Cantril's "absolutely general seven page statement. Therefore the work has to be completely thrashed out by Cantril, Stanton and myself". However, such a situation suited Lazarsfeld's purposes very well indeed: "From this point of view it seems to me a very good opportunity to do a series of interesting studies, and I am very glad indeed to go ahead under such conditions". The conditions upon which Lazarsfeld accepted the Project directorship will be returned to shortly, but such a poorly formulated plan of action did mean that he could give free rein to his interests, especially on the methodological side, since that was one of the most pressing

areas in need of development. In the "considerations presented" to the trustees of the Foundation for their approval, Marshall submitted the following summary of Cantril's plan:

"The general premise on which this proposal rests is stated as follows: 'Radio has developed because it has satisfied genuine human needs. But how precisely it meets those needs is still an open question, and a very important one if the medium is to develop the greatest good for the greatest number'. The proposal accordingly outlines a study designed to answer the basic question, what role does radio play in the lives of listeners? Answering this question, it points out, first involves gathering evidence not at present available on a number of subsidiary questions: who listens? where and when does listening take place? what is listened to? why and how people listen? and finally, what are the effects of listening? Of the four years the proposed study will require, the first will be devoted to developing and testing techniques by which evidence needed to answer these questions can be secured. In the first two years these techniques will be put to use in gathering such evidence from a representative sample of the total population." (Marshall, 21.5.37)

Although it was clearly stated, both in Cantril's proposal and Marshall's summary of it that the Project would gather evidence "from a representative sample of the total population" this design was quietly dropped once Lazarsfeld took over as Project Director, since it was never his, or indeed the Bureau's style<sup>1</sup> to use the kind of statistical significance tests which the above design would have demanded. Indeed, Lazarsfeld set little store by significant tests and was highly critical of those who automatically employed

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1 For example in Appendix C of the "Student-Physician" (Merton et al 1957) entitled 'Note on Significance Tests' one reads "The reader will find that no traditional significance tests have been reported in connection with the statistical results in this volume. This is intentional policy rather than accidental oversight. It is a policy, furthermore, which the Bureau of Applied Social Research has always adhered to in reporting the results of exploratory studies such as are presented in this volume". For the whole debate about significance tests see "The Significance Test Controversy", Morrison and Henkel 1970. See in particular Kendall's article "Note on Significance Tests" for a defence of the Bureau's position, as well as Zipot et al and the articles by Dehrin.

then<sup>1</sup> and throughout the Project their use was ruled out by the nature of the samples selected. Fortunately, however, the Project's 'charter' was vague enough to allow Lazarsfeld a very great deal of freedom in the choice both of the methods to be employed and the substantive areas to be covered. This was in line with the Foundations' general policy of allowing researchers as much latitude as possible while still keeping to the defined purpose of the study, although in certain cases limiting conditions are placed on the award of a grant. In the project under discussion however, no such restrictions existed. This point was raised with Professor Lazarsfeld, stemming from a comment made by Adorno. Complaining about the restricted possibility of engaging in 'critical sociology' due to the Rockefeller Foundation's charter, Adorno wrote:

"Naturally there appeared to be little room for such critical social research in the framework of the Princeton Project. Its charter, which came from the Rockefeller Foundation, expressly (emphasis mine) stipulated that investigations must be performed within the limits of the commercial radio system prevailing in the United States. It was thereby implied that the system itself, its cultural and sociological consequences and its social and economic presuppositions were not to be analysed. I cannot say that I strictly obeyed the Charter." (Adorno 1969: 343)

It is difficult to understand how Adorno arrived at this definition of the situation. Perhaps it is only further evidence of his culturally blinkered vision and his refusal to examine his host

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1 Patricia Kendall in discussing the 'Bureau's' lack of use of significance tests informed the writer that "As much as any - Paul was a model in this respect. He never used them, never believed in them and as a matter of fact was quite critical of people who automatically use factor analysis and things of that sort." (Kendall 9.6.73)

situation in anything but a distorted fashion. However, having read Adorno's statement prior to examining the Rockefeller files, the writer assumed that his view was a correct interpretation of the situation.

D. Morrison: I haven't seen the original 'charter' but if I remember correctly it was restricted to commercial broadcasting.

P. Lazarsfeld: No, not at all. We did a great deal of educational broadcasting, but again there is no relation between the charter of the grant and what you do.

D. Morrison: Perhaps I have been thrown by Adorno's comment. I read it as somewhat of an attack when he says something like "What I misunderstood about the project that they did not want to study the relationship between ... music and society. They merely wanted to study the object."

P. Lazarsfeld: Look, if Adorno means that I didn't have sense enough to give it a personal attack, that I didn't see it as a problem, that is quite a problem. But if he says I didn't see it ... no, no. (Lazarsfeld 2.6.73)

Paul Lazarsfeld, Research Director: A New Field but a Familiar Role

On the fourth of August 1937 Lazarsfeld, whilst staying in Austria, received the following cable from Hadley Cantril:

WOULD YOU TAKE FULL TIME POSITION BEGINNING SEPTEMBER  
DIRECTING ROCKEFELLER RADIO RESEARCH SALARY SEVEN  
THOUSAND ANOTHER THOUSAND HERTA ASSISTANTSHIP TWO  
YEARS SURE POSSIBLY FOUR HEADQUARTERS PRINCETON LYND  
ADVISES ACCEPTANCE CABLE ANSWER (Cantril 4.8.37)

As instructed, Lazarsfeld replied by cable, stating that although interested in the post he wished to raise certain questions concerning the proposed project. Thus, a few days later he accordingly wrote Cantril the previously mentioned letter in which he tried to persuade him to

transfer the Project funds to Newark, rather than have the Project housed at Princeton. But, the important point in this context is Lazarsfeld's surprise at being offered the Directorship: "Neat! Had, it is a queer experience to sit in a mountain village and to discuss American research problems. Your wire was a pleasant but unexpected surprise" (Lazarsfeld 8.8.37). This letter crossed with one which Cantril sent giving full details of the Project and the situation surrounding it. After briefly mentioning the origins of the grant he informed Lazarsfeld that:

"I had thought Stanton would take the Directorship but Columbia (CBS) made things so attractive that he decided to stay there at the last minute, hence my late call to you. Also, I had not thought of you seriously at first since I assumed you might want to stay at Newark. However, when I wrote to Lynd for suggestions - illustrating my point by saying I wanted someone like Lazarsfeld, Lynd came back and said that I should ask you and that he thought that from your point of view this would be an excellent bet and he would recommend you to take it and relieve yourself from worry about finances and tenure for two and possibly four years." (Cantril 9.8.37)

The question of the circumstances surrounding Lazarsfeld's appointment were raised in an interview:

D. Morrison: Were you approached for the Directorship or was it the other way around?

P. Lazarsfeld: Oh absolutely .. the other way around. They tried desperately to find an American director, but no one would take it. You see the Project was new, and the depression was still here so no experienced American professor wanted to give up a regular position, and run such a new thing like a project. I think the Princeton Project was offered to every American before they finally out of despair chose me.

D. Morrison: Was it Lynd who recommended you?

P. Lazarsfeld: Yes I'm sure, but I mean, that was already quite late in the game. For about a year the Rockefeller Foundation tried to find a director and finally appointed me. You see it was a completely new field. I remember one day, a friend of mine, in 1937 or so, introduced me to a group of colleagues and said, 'this is a European colleague who is an upmost authority on communication research', and he saw that no-one was especially impressed, so he wanted to press the point and said, 'as a matter of fact, he is the only one who works in this field'." (Lazarsfeld 25.5.73)

A few points can be drawn out here. The selection of a research director had nothing to do with the Rockefeller Foundation, but was the responsibility of Hadley Cantril since the money had been given to him, or more accurately, the School of Public and International Affairs of Princeton University. It is nevertheless true that Cantril found it extremely difficult to find someone willing to take on the job. Whether this desperation led him to approach a variety of individuals with the offer is doubtful however, since Stanton had only pulled out "at the last minute". Certainly John Marshall was unaware of approaches being made to anyone besides Stanton.

D. Morrison: When I talked with Professor Lazarsfeld, and asked him about how he became Director of the Project he said "Oh, it was after it had been offered to every-one else". Would that be accurate? Did you approach any-one else before Lazarsfeld?

J. Marshall: I can tell you that exactly. We approached no-one. We made the grant to Hadley Cantril, and it was his responsibility. Well, I think it had been generally understood that the job would go to Frank Stanton. Frank was then a rather modest figure in the Research Department of CBS under a non-entity. I forget the name of the man - it's just as well - the research department did nothing of importance whatsoever, but just about that time, if I'm not mistaken, it was evident that the non-entity was to be eased out, and consequently, after the grant was made to Princeton, Frank decided that he could not afford to leave CBS and I think everybody would agree his decision was wise. I was up in Maine at the time, and Hadley called me there, and said Frank had to bow out, and what did I think of Paul Lazarsfeld. Well, Hadley was surprised when I said "Who's he, I don't know him". I had never seen Lazarsfeld, but I immediately said to Hadley, "if you think Lazarsfeld's the man it's certainly alright with us. Yours is the choice and we will go along with it". Hadley said I think Lazarsfeld is the man and that was it. I don't know that the job was offered to anybody else except Frank Stanton. Then of course Frank and Hadley turned around and became associate directors of the enterprise. (Marshall 6.7.73)

Marshall's comments notwithstanding, it is always possible that other individuals were 'sounded out' before Lazarsfeld was offered the directorship, but it would seem unlikely since the only time the post could have been 'offered around' in the manner suggested by Lazarsfeld is during the brief period before Cantril consulted Lynd. However, if as several of the participants stated, Stanton only withdrew from the directorship at the last minute, Cantril would have had little opportunity to approach many other people. In addition, his decision to consult Lynd, although it could be taken as evidence of his having unsuccessfully attempted to interest others in such a proposition, is much more likely to have resulted from Cantril not knowing who to offer the post to, and therefore turned to Lynd who was admirably placed to be familiar with most individuals in the small world of American social research at that time. However, the generally depressed state of American academia and its effect upon intra-job mobility, no doubt did play an important part in contributing to Cantril's anxiety over finding someone of sufficient stature willing to take the risk of accepting the directorship of such a new type of research project. In this respect such considerations did not apply to Lazarsfeld, for it can hardly be said that his own personal history had been characterised by security, or that his existing position at Newark was one of entrenchment. It is true that he was concerned about allowing the Newark Centre to decline, but the point is that both structurally and psychologically Lazarsfeld was poised to take advantage of the opportunity presented by the offer of Research Director at Princeton. Thus, the main question for Lazarsfeld became one of how to turn an attractive offer into an even more attractive proposition by combining the advantages of one with the benefits of the other. Cantril had written to Lazarsfeld that:

"Your suggestion about a tie-up with the Newark Research Center is, I fear, out. I'm sorry you didn't get the money there but it is one of these somewhat absurd American grants to an "institution" and it would be impossible to have the work carried on elsewhere." (Cantril 9.8.37)

As will be noted at many points in this work however, Lazarsfeld was never one to accept the immutability of conditions, and in this case he did eventually succeed in his aim of transferring the work to Newark. This is very much reflective of his refusal to be bound by details at the cost of endangering longer term aims. To be sure, such qualities are probably characteristic of all innovators, yet his refusal to accept conventional procedures did not derive from the kind of intransigence displayed by Adorno. Rather, Lazarsfeld accepted the basic ground-rules of procedure, but rejected specific 'details', thereby allowing himself sufficient freedom to expand the possibilities presented within that framework without evoking censure measures. Although it is always difficult, if not hazardous, to attempt to trace the tenuous relationship between broad currents of social experience and their particular manifestations in individual behaviour, it is nevertheless true that Lazarsfeld's way of coping with restrictions is not dissimilar to the overall strategy of the Austrian Social Democratic Party. Indeed, the parallels are quite striking. Both accepted an overall framework of 'rules', yet both refused to accept the restriction on change and movement which full acceptance of those rules would have meant, and consequently both sought to expand the possibilities of transforming the existing situation in accordance with their ambitions.

However, the intention is not to draw a precise analogy between Lazarsfeld's strategy of innovation and that of the Social Democratic Party, but rather to draw attention to the ways in which his experiences

as a party member shaped his subsequent behavioural style. His Viennese political experience was one of pragmatic reformism. Rather than launching a total opposition to the existing order, the party concentrated on working within the existing structural contradictions in order to achieve piece-meal change. Whether temperamentally, like many of his fellow Austrian Social Democrats, Lasarsfeld was capable of total opposition is open to question, but undoubtedly he was sufficiently radical not to accept the existence of things as they are. He understood very well, both intellectually and from first-hand experience gained during his Vienna days, that within certain broad limiting conditions situations were highly malleable. He had matured in an atmosphere in which men had made their own history but where the circumstances had nevertheless remained not entirely of their own making.

When Lasarsfeld began his academic career in America contradictions existed between the new developments in empirical knowledge and the existing social settings for knowledge production which he placed in a disadvantaged career position exploited to the full. His experience of being raised in a political movement, which attempted to exploit every turn and twist of unfolding social processes, produced in him small regard for the conventions of procedure. His understanding of the mutability of circumstances meant that obstacles were seen as no more than obstacles, and not as fundamental barriers. To him these obstacles could be overcome within the existing framework, and innovations pursued in a manner that did not fundamentally interfere with it to the extent of evoking restricting opposition. Instances such as his disregard for the conventions that foundation grants remaining at the institution awarded to; his refusal to accept the

convention that editors of American scientific journals did not accept guest editors;<sup>1</sup> and his failure to be impressed by the academic community's low estimation of applied psychology and market research, are not in themselves of great significance but, when taken together, they do indicate an individual of determined and innovatory disposition.

Although Lazarsfeld's social background was undoubtedly influential in determining the nature of his innovations, the concrete situation in which he operated in America cannot be ignored. For example, although it was certainly a break with convention for him to shift the Radio Project from Princeton to Newark, Cantril's difficulty in finding a research director of Lazarsfeld's ability played no small part in the matter. Cantril's position of not wishing to run the Project himself, but of needing to delegate responsibility to someone he knew would not mishandle it, probably gave Lazarsfeld an additional negotiating power. Certainly Cantril was eager to hire Lazarsfeld, and as the following letter illustrates made a considerable effort to 'sell' the Project to him:

"With Stanton not taking the directorship I am stuck and will refuse to carry on myself, as you know, my heart is not in this type of research as much as yours. Furthermore I am not too good at it. Lord but I'd be relieved and happy if you would accept.

Stanton and I will act as Associate Directors of the project no matter whom we get as Director. That was Marshall's understanding. The point of that is that we may be able to contribute an idea occasionally but we would neither of us in any sense try to 'boss' the job, especially if we could get someone of your calibre to run things....

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<sup>1</sup> Lazarsfeld became the first guest editor of the Journal of Applied Psychology (February 1939). Although common in Europe at that time, it was unheard of in America for an editor to turn his journal over to someone else. See Lazarsfeld 1969: 317.

... I asked Gallup and Stanton about the line-up for you as objectively as I could and they both thought it would be a good move. Gallup thinks very highly of you, as does Frank, and both thought that you could readily land something you liked when this was up ...

... My thought was that with two years at good pay and with plenty of money and help, you could really put out a couple of first class studies that would put your name at the top of the list not only for radio research but for that type of research in general ...

Your title throughout the job would be Research Associate in the School of Public and International Affairs at Princeton University. Doesn't that appeal to your bourgeois soul! ...

... My thought was that Herta could be one of the assistants. Now in order to 'get' you both I should be quite willing to have you set your salary at 7,000 a year and Herta's at 2,000 a year. When I write this I'm almost tempted to take the job myself ...

... What else can I think of as an inducement. Better stop. It's hard to be objective. But really I don't see any drawbacks to it except that it will end. And I'm sure you could get back to Newark if it still exists or find something even better after you had put out a good volume or two on social research."  
(Cantril, 9.8.37)

As far as can be gauged from Lazarsfeld's letter to Lynd (9.9.37)

Cantril had submitted the proposal to the Rockefeller Foundation while he was still uncertain as to his own position at Princeton, but since receiving the grant his position had improved and consequently the Radio grant was less attractive due to other pressing business. Hence, he did not wish to devote himself solely to the Project, especially since he had little genuine enthusiasm for such work. Consequently the grant had become something of an 'embarrassment' to him and he wished to be rid of it in the most 'decent manner' possible while at the same time retaining some credit for Princeton by having it based there.<sup>1</sup> According to Lazarsfeld, Cantril was "absolutely fed up with

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<sup>1</sup> All the letter heads of the Project had to carry Princeton University's name and not Newark's. However, the only concrete manifestation of the Project residing at Princeton was a single office and one secretary.

the entire situation", and knew that if Lazarsfeld accepted the post of Director he would have as much interest as Cantril "in not making it a flop, or jeopardising it by doing too many other things". Cantril was certainly well pleased at having Lazarsfeld as Director, as indeed was John Marshall for, after what was probably the first meeting between the two men, Marshall recorded, "Lazarsfeld is evidently admirably equipped for the work".<sup>1</sup> This was certainly true; indeed it is hard to imagine anyone at that time being more suitably qualified for such a post. From the original letter which Lazarsfeld wrote in reply to Cantril's telegram it would seem that he was, despite certain reservations, much taken with the idea of the Project and would probably have accepted without Cantril's acquiescence to his request to have the headquarters located at Newark, especially since Lynd, whose opinion Lazarsfeld greatly respected, had recommended him to accept the post. However, it would also appear from a reading of Lazarsfeld's letter to Lynd that Lazarsfeld had once more raised the question of the project's location with Cantril. Cantril's subsequent agreement to the request more than likely resulted from his wishing to secure Lazarsfeld's total cooperation, and from the fact that he no longer saw the point of opposition once he was assured of Princeton's and his own position being protected, especially since the whole situation had become extremely tedious for him. The fact that his request was acceded to appears to have somewhat surprised Lazarsfeld, but from that point on the possibility of his establishing the type of institute he desired became a distinct possibility. Obtaining the Rockefeller funds to map out research in an uncharted yet socially relevant area gave to him an unprecedented opportunity for rapid academic advancement and widespread recognition.

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<sup>1</sup> Record of an interview between Marshall, Stanton and Lazarsfeld (Marshall 19.8.37). Evidently Marshall was very impressed by the work Lazarsfeld had conducted in Vienna.

The need to find an appropriate institutional setting for the 'Centre':  
Problems of Growth.

The arrangement which Lasarsfeld made with Cantril meant that in principle the Radio Project simply became one more study which the Newark Centre was to conduct. Yet in practice the sheer size of the Rockefeller grant, which amounted to three times the total operating budget of the 'Centre', meant that the two became more or less synonymous. However, the growth of the Newark Centre created strains in its relationship with the parent university.

It will be remembered that Lasarsfeld had taken Directorship of the 'Centre' in the expectation that the University itself, under the guidance of its newly appointed President, Frank Kingdon, would prosper and develop into a prestigious seat of higher learning. Yet this was not to happen. Kingdon, although undoubtedly able, developed interests in a more overtly political direction and stood for the American Senate with the result that he neglected the University to such an extent that it not only failed to develop, but collapsed completely, and today exists only as a branch of Rutgers University. This instance provides a good illustration of the writer's earlier point concerning the importance of key individuals during an institution's formative years. Whether or not the University would have achieved its original aims had Kingdon's interests not been divided is an open question, but the fact remains that the university's decline placed the Research Centre in the position of having to find a new location. Although it was on Kingdon's own suggestion that Lasarsfeld remove the 'Centre' from the precincts of Newark, such a move represented no great loss for Lasarsfeld, since the decline of the University was only too obvious to him. For, as he informed the writer:

"I made it one of my conditions that the headquarters of the Princeton Project would be at Newark. I mean, I never spent a night in Princeton or anything. I went down there every few weeks for one day or so, and that lasted for about one year when the Princeton Project was in Newark. Then the University began to decline, and I mean it became clear it had no future. Then we moved the Princeton Project from Newark to New York.<sup>1</sup> (Lasarsfeld 15.6.73)

The university's decline meant that the 'Centre' would be left standing alone. This was neither consonant with Lasarsfeld's conception of what a research centre ought to be, nor did it hold much promise for the future. In fact, by 1938 the 'Centre' had already become functionally autonomous from the University, a point which probably prompted Kingdon to ask Lasarsfeld to find other premises. But, as already suggested, of greater importance from Lasarsfeld's point of view was the fact that unless some action was taken to pull the 'Centre' more into mainstream academia its structural isolation would severely circumscribe its overall impact and success. In order to avoid this it was necessary to ally the 'Centre' with an already successfully established university which would aid its academic development and provide it with the kind of support that neither Newark, nor even Princeton could offer. In light of this it was understandable that Lasarsfeld should transfer the Project to New York, and push to have the Rockefeller grant transferred from Princeton to Columbia University.

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<sup>1</sup> In Newark the Princeton Project was housed in an old brewery which Adorno considered to be in "a somewhat pioneering spirit". Indeed, Adorno contrasts the informality of this American research setting with that existing on the continent. "I was very much taken by the lack of embarrassment about the choice of a site that would scarcely have been conceivable by the lights of the European academic community" (Adorno 1969: 342). In 1938 the 'Centre' moved to Union Square in downtown New York. Then in 1940 it moved to Amsterdam Avenue, near 59th Street. Later it moved to its present location on 117th Street near, but not actually on, the University campus.

Lazarsfeld in his 'Memoir' is slightly histrionic concerning the Project's release by Princeton:

"Meanwhile, internal difficulties had developed between Cantril and me. During the negotiations with the Rockefeller Foundation for a renewal of the grant it became clear that an administrative decision had to be made. Either the project would stay at Princeton with Cantril as the main figure but with a new director, or, if I were to remain, the project would have to look for another institutional base. The Foundation naturally turned to Stanton as a third insider to arbitrate the situation; from the outcome I gather that Stanton put his weight on my side. Lynd prevailed on President Dodds of Princeton to release the project. In the Fall of 1939, the Office of Radio Research was turned over to Columbia University, and at the same time I was appointed lecturer there."  
(Lazarsfeld 1969: 329)

In actual fact there appeared to be very little opposition either to Lazarsfeld remaining as Director or to the Project going to Columbia; indeed it would seem that Princeton University wished to rid itself of both the project and Lazarsfeld. Certainly there was mounting friction between himself and Cantril. On January 13 1939 an article appeared in the Princeton Alumni Weekly entitled "Psychologists to Study Martian Hysteria". The article referred to a study of the panic reaction to the broadcasting of Orson Wells' "War of the Worlds".<sup>1</sup> The probable offending passage in the article reads:

"Dr. Cantril's study will seek two objectives: first, determination of the general extent and nature of the public reaction to the broadcast; second, the social-psychological reasons for this reaction in various types of individuals.  
This novel research undertaking will be greatly aided by work already performed at Princeton by Dr. Cantril /my emphasis/ in the Princeton Radio Project."

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<sup>1</sup> The study was published as "Invasion from Mars" under the authorship of Cantril, Gaudet and Herzog (1940).

Lazarsfeld must have written a complaining letter to Cantril very soon after the article appeared, for on the 26 January Cantril wrote to Lazarsfeld expressing his disappointment, anger and bewilderment over his reaction. From the content and tone of Cantril's letter it would seem that Lazarsfeld must have objected to the emphasis given to Cantril's role in the Project by the newspaper reporter. Cantril's letter reads:

"I am glad you expressed yourself on the release, but I must say that the reaction seems a bit infantile. Perhaps we should have directors' uniforms with differential insignia. It is hard to imagine that people like Frank, Gallup, Allport, Katz, Stouffer would maintain petty jealousies, and I should like to think that you, too, would have sufficient perspective in things not to let such trivia bother you.

As soon as I got your letter I went over to the office of the Alumni Weekly to see what the fuss was all about. No one had mentioned the piece to me, and I seldom see the sheet. I must say that the report seemed quite harmless ...

In the official university release that was sent out for approval I clearly indicated that the whole project was under your direction. I also indicated that this was a separate grant given to the University.

After our conversation the other day it should be quite clear to you that I am not one bit interested in deriving any prestige from the radio project. It just happens that whatever ego-enhancement I crave will be sought through more systematic research in social psychology and through teaching and graduate guidance. I do not in the least mean to be contemptuous of the radio research. On the contrary, I am fully convinced that you are making of it an organized body of studies that will equal, if not excel, anything done in the social sciences. And that you deserve 90 percent of the credit for this, Frank 9 percent and myself 1 percent. I am the first to admit anywhere to anyone. I have told that to Dodds, Poole and Marshall.

If the project could go on completely without me I should honestly be much happier. But apparently I am a strategic link in the chain. I am willing to play the role only for two reasons: 1) Dodds feels that we should not tell the Foundation outright that we do not want a renewal; 2) I am anxious to help you make a reputation and attain some sort of eventual security in these highly insecure days. Please believe that these are my only motives.

Since they are my only motives, I frequently feel that I should drop the whole thing and concentrate my time more on two books: Mass Psychology and Public Opinion and Propaganda. If I have to become involved in many emotional reactions, I may reconsider my whole position.

You will know that I do not like to write this way. Yesterday I dictated a gentle answer but last night I began to wonder why in hell I was letting myself in for more years of minor interruptions to the work that interests me most. Few people would go on with this when they were so anxious to focus spare energies on other things. Hence from a purely logical point of view, your reaction was untimely."

(Cantril 26.1.39)

The above letter has been quoted at some length since it provides insight not only into the situation surrounding the Project's transfer, but also into Lazarsfeld's attitude concerning his position as Director. During the series of interviews which the writer conducted with Professor Lazarsfeld he was always particularly forthcoming and open in his answers, even to those questions which could be considered to be of an embarrassing nature. But, as Lazarsfeld quoting Oscar Wilde informed the writer, "questions are never embarrassing - only answers". However, his disagreement with Cantril was the only instance where he preferred not to answer! Reading the Alumni article today it does seem rather innocuous, and clearly Cantril, even at that time, found Lazarsfeld's attitude somewhat mystifying. Nevertheless, Lazarsfeld's reaction is intelligible in terms of his self-image as Director and his awareness that identifying himself with an institute presented him with his best chance of making his way in American academic life. Although it is difficult to believe that Lazarsfeld really considered that Cantril had designs on the Project it must be remembered that the Project really was the basis of Lazarsfeld's 'Institute' and it is

therefore quite understandable that he should have reacted strongly over his name not being sufficiently linked with it. After all, at that time Lazarsfeld was still in a state of considerable uncertainty with regard to his future position and the recognition that had been forthcoming had been largely due to his building up a research centre and obtaining the Rockefeller funds. Thus given any threat, real or imagined, to the progress which he had made towards his desired goals, then understandably his reaction was likely to be rather petulant. Had Cantril appreciated Lazarsfeld's position of psychological uncertainty which at times manifested itself in over-reaction and manipulation of situations, then perhaps he would have sent the more 'gently' worded first letter and not the more pointed and unfriendly second one. Fortunately for the present work however, Cantril's display of anger in the second letter helps to illuminate the situation surrounding the Project and, taken together with the material in the Rockefeller archives, offers a slightly different picture to the one Lazarsfeld gives in his Memoir.

In his letter Cantril reiterates the original point which he made to Lazarsfeld upon offering him the post of Directorship; that his main interest lay elsewhere. Although Cantril had originally been pleased to have Lazarsfeld rid him of the responsibility for running the Project, it would seem that by this time he wished to be rid of it altogether. In addition, it would also appear that the university authorities, in the personage of President Dodds, did not want the grant to be renewed, but felt it inappropriate to inform the Foundation. Consequently, it could not have taken much persuasion

on Lynd's part to have the Project transferred to Columbia University. John Marshall was certainly well aware of this developing situation, and went to Princeton to discuss the whole matter with Dodds and Cantril. Reviewing the situation in January of 1939 Marshall recorded that:

"Dodds repeatedly stressed the fact that he was glad to see Princeton made the location of radio research and in general hoped that research of this kind might go on there. But he felt that a decision on that score for him must rest on whether or not his faculty could profit by it. If work on public opinion proves to be one of the best interests to cultivate in his faculty, radio research certainly ought to be encouraged. If not Dodds fears that work of this kind may not only get little from the faculty, but in making demands on them may detract them from other interests worth developing. In all this he has at present an entirely open mind."  
(Marshall 18.1.39)

However, judging from the letter Cantril wrote to Lazarsfeld only eight days after this review it would seem that Dodds' mind was no longer so open but was beginning to close in favour of having the Project undertaken elsewhere.

Whilst at Princeton, Marshall took the opportunity of seeing Cantril who informed him that he was uncertain of how much time he could justifiably give to the Project in subsequent years. He impressed upon Marshall that he wished to concentrate on his book on mass psychology and give more time to the development of his journal, the 'Public Opinion Quarterly'. In summing up Cantril's position Marshall wrote:

"It is therefore patently a question in Cantril's mind if further work on the project will contribute enough to warrant his taking time from his Princeton obligations. He would probably like to continue as an associate director of the project but with the understanding that his functions as an associate director is merely to check Lazarsfeld's judgment on questions of policy. Stanton probably wishes to assume a similar function as his

present status at C.B.S. subjects him to an increasing pressure of obligation there. Cantril is unequivocal in saying that Lazarsfeld is the man for the position of director, but Lazarsfeld does need someone to check decisions with. In short, Cantril and Stanton, having seen the project through the developmental stage, both feel that they can retreat to advisory positions." (Marshall 18.1.39)

These talks left Marshall wondering whether a university was a fitting place for such a study at all. Although he considered that the project had to "follow its own course", he was worried lest the fact that it did not fit easily with existing university social science work might mean that it would be "definitely harmful to ... have a university influence its direction". In light of these difficulties, Marshall was seriously thinking of alternative locations for the study, and appears to have considered two possibilities. One was the new School for Social Research, since it had recently been suggested that training in broadcasting should be undertaken there. The other was to hand the Project over to the radio industry itself, or rather the Joint Committee on Research which it had recently established.

However, Lazarsfeld was by now indispensable to the Project. He had stamped his presence on it so strongly that any consideration of its future had to take account of him as its leading figure. Thus, although it may well have been possible to make alternative arrangements for the future institutional setting of the Project it is very difficult to envisage the Project having continued without Lazarsfeld, since the key position which he occupied made it extremely difficult to dispense with his services. Not only had he cultivated the necessary contacts and drawn together the research team, but his methodological expertise was indispensable. Lazarsfeld's crucial

importance to the likely success of the Project was recognised by both his two associate directors, and Marshall himself. Yet, while he did not doubt Lazarsfeld's research ability, Marshall did have certain reservations about his 'personality' and, more particularly, about his lack of organisation. The possible repercussion of this disorganisation for his academic work caused Marshall repeated concern. For example, Marshall informed Howard Odum that:

"... I have some reason to believe Lazarsfeld a fellow rather more apt to get research going than to carry it through to formulation... The result is that we are holding him, so far as we can by financial means, strictly to the job of formulating the findings his project has now amassed, and that is just the word for what he has done." (Marshall 22.6.39)

Lazarsfeld's own intellectual interests and personality is well captured in the above letter. Given his overwhelming enthusiasm for anything methodological it is not too surprising that he should 'gass' material without possessing the commensurate enthusiasm to analyse it. In addition, his enthusiasm for moving from one area to the next tended to produce a number of loose ends. Even Samuel Stouffer, writing in support of Lazarsfeld's professorship application noted:

"The mortality of Paul's ideas is rather high, but he himself is ruthless in committing infanticide among his brain children; and the net fertility is still very great." (Stouffer 17.2.41)

Lazarsfeld's consuming interest in methodology, coupled with the fact of his enthusiasms, although pursued with single-mindedness while they lasted tended to be dropped once his interest was exhausted, alarmed those around him who wished to see a more carefully planned and coherent pattern of activity. Yet, when viewed in terms of a Bureau Director's role Lazarsfeld's style of work, rather than being a flaw in his intellectual make-up, offered distinct and positive advantages for overall institutional productivity.

A Bureau Director is usually an academic of senior standing with a good deal of published work to his credit, and whilst it is true that Lazarsfeld's own productivity was extremely high, it is nevertheless correct that it would have been much greater had he not allowed his wide-ranging and intermittent enthusiasms to obstruct the following through of ideas. However, as Stouffer pointed out, he may have committed infanticide amongst his own brain children, but such was the proliferation of his ideas that the net fertility was still of an impressive order. However, the important point to note is that unlike an individual scholar, a Bureau Director does not have to carry through ideas himself. Rather his main role is to throw up ideas and then to set others to work on them. Hence, when placed in this context, Marshall's comment that "Lazarsfeld was a fellow rather more apt to get research going than to carry it through to formulation" appears as more of an accolade than a condemnation. To be sure, Marshall did not intend it as an accolade, but rather an expression of concern, since in 1939 Lazarsfeld's role was closer to that of individual scholar with personal responsibility for seeing the Project completed. Marshall's concern about Lazarsfeld's personal style is well illustrated in the records of a meeting between Marshall, Cantril and Stanton:

"Stanton and Cantril called evidently to make sure that there was no misunderstanding about their future part in the Princeton Radio Research Project if further support for the project's work can be found. Clearly Lazarsfeld's personality is a big factor. No-one doubts his ability in research. But the fact is that he is so single-minded in the pursuit of research as to need a good deal of checking and balancing. Both Cantril and Stanton are definite in saying that they will continue to check and balance Lazarsfeld if the project goes on, but neither of them can undertake to do any active work regularly."  
(Marshall 2.3.39)

Thus the difficulty of replacing him and the fact that both Cantril and Stanton could no longer afford the time they had previously expended on the Project meant that Lazarsfeld was once more well positioned to push the situation in a direction favourable to himself, and by extension to the development of 'his Institute'. Indeed, the day after Cantril and Stanton had called at Marshall's office to discuss the future of the Project, Lazarsfeld paid Marshall a visit and presented his views on the situation. Marshall recorded the meeting as follows:

"Lazarsfeld is evidently not too happy at the prospect of having the project continue under Princeton auspices, preferring to see it located in some university like Columbia, where it would gain advantages of being a real university enterprise." (Marshall 3.3.39)

It is not suggested that Lazarsfeld was operating from some kind of machiavellian strategy, but simply that he appreciated the problematic situation within which the Project found itself. Quite clearly Lazarsfeld's views had to be taken account of in any decision on what was to be done, and in that sense the situation rested very much on his shoulders. Since the Project was now operating from New York it made good sense to sever the connections with Princeton and transfer it to Columbia, especially since Lynd was enthusiastic about having it housed there. In his appraisal of the Project's history Marshall summed up the situation thus:

"Another outcome is worth noting, particularly in view of the transfer of the project from Princeton to Columbia. At Princeton, as President Dodd's comments imply, it never really found a home; there was not at Princeton other work in progress closely enough related to the work of the project to provide an adequate background with which it could articulate and from which it could draw personnel and guidance. At

Columbia, the Project found sympathetic and hearty sponsorship through Prof. P.S. Lynd. As a result, in June 1940 (sic) Dr. Lazarsfeld was given a permanent appointment to the faculty of sociology with the rank as an Associate Professor, the University taking over the major portion of his salary. At Columbia the Project has close relations with the School of Journalism, with the graduate departments in the Social Sciences and with Teachers College." (Marshall 31.8.41)

It is certainly true that the Project "never really found a home" at Princeton, nor was it likely that it would have done so had it remained. From the point of view of the future prospects of both the Project itself and Lazarsfeld's own career, Columbia was a much more suitable location. Yet it would seem that even had Lazarsfeld wished the Project to remain at Princeton matters would have been far from easy. Quite apart from the fact that at Princeton the Project could not attach itself to existing intellectual interests which would have fed into and given support to it, there was also the fact that Lazarsfeld appears to have been personally unacceptable to the faculty members.

"According to Cantril there will be no difficulty in the project continuing under the nominal sponsorship of Princeton, but Cantril believes its office must be in New York City. He doubts if it would be advantageous to Lazarsfeld or the Project to have Lazarsfeld working in Princeton, particularly on the grounds that Lazarsfeld would not be personally very acceptable to the members of the Princeton faculty he would come into contact with." (Marshall 2.3.39)

Why this 'hostility' existed on the part of faculty members towards Lazarsfeld is difficult to say with any measure of real certainty; however, a few suggestions can be made which help to highlight the problems of his 'acceptability'.

Lazarsfeld was undeniably 'foreign', and to this day one still hears remarks about Princeton and its 'superior attitude', and this

'Ivy League' self imagery certainly did not make for easy cultural acceptance. Furthermore, despite his 'Viennese charm' Lazarsfeld could be rather forceful, a fact which no doubt underlined his appearance of 'outsider'. Samuel Stouffer, writing to Frederick Mills concerning Lazarsfeld's proposed appointment as professor at Columbia obviously felt it necessary to inform him of Lazarsfeld's personal characteristics.

"Paul has his defects. He is not an orderly worker, and if he gets a bright idea he is likely to pursue it to the detriment of orderly routines. I rather admire that kind of guy myself. In spite of the fact that he has lived in this country for seven years or more, he has a distinct foreign appearance and speaks with a strong accent. This prejudices some people against him, and I think some are further prejudiced because they feel that there is an occasional arrogance in his manner. Actually, Paul is one of the most modest of men, but he does have a rather heavy Germanic way of presenting a topic which tends to make some people feel that there is not as much in the topic as the difficulty in following would suggest. I think such critics would be occasionally right, but I can testify from experience that there is plenty of pure gold in them thar hills." (Stouffer 17.2.41)

This use of popular idiom was typical of Stouffer. Lazarsfeld informed the writer that despite being an extremely cultured person Stouffer often characterised himself as a 'hillbilly' who really knew nothing. Nevertheless, it is interesting that Stouffer should feel the necessity to protect Lazarsfeld by mentioning his 'foreign appearance' and his occasional surface arrogance. Factors such as these could well have led to difficulty of assimilation into the world of Princeton. In addition, all those that knew Lazarsfeld appear to have been particularly impressed by the power of his intellect. Lynd, for example, wrote to Marshall in 1939 describing

Lazarsfeld as "the ablest combination of great technical training and ability and of imagination and energy to carry through work that I know of in this social-psychological field... He has far more imagination than Stouffer of Chicago" (Lynd 16.3.39). Given his position as an 'outsider', his easy mastery of intellectual matters, exhibiting itself as confidence or at times arrogance, may well have provoked hostility in certain quarters. Hence, at a place like Princeton with its own established feeling of social superiority it was hardly surprising that Lazarsfeld, as the 'clever foreign outsider' culturally distanced from the other faculty members, should prove in Cantril's words to be "not very acceptable".

Although Columbia is a different kind of university to Princeton, even there as Stouffer's letter indicates social considerations were not entirely absent from his appointment as Associate Professor. The whole question of Lazarsfeld's appointment as Associate Professor at Columbia is complicated, yet interesting in that it reflects the developing rift in the sociological world. Columbia at that time had one of the most active and prestigious sociology departments in the country, but was completely divided between the empiricists, represented by Robert Lynd, and the more theoretically speculative sociologists represented by Robert MacIver.<sup>1</sup> Although on moving to New York Lazarsfeld had lectured at the university he did not have

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1 For a full account of this rift see MacIver's own biography 'As a Tale that is Told' (1968) p.137-148. In brief the difference within the department was over the "focus of sociology and on the function of university instruction". However, personal bitterness entered when MacIver gave a particularly critical review of Lynd's "Knowledge for What" (1939) in which he emphasised the utilitarian conception of elucidation to which MacIver was so trenchantly opposed. The department then formally polarised over the future role of the department, a polarisation and disagreement which lasted for several years.

the security of tenure which went with a senior professorial position. In December of 1939 Lynd had called at Marshall's office to discuss Lazarsfeld's possible future at the university. Marshall recorded the discussion as follows:

"Lynd can at the present time hold out no assurance that Lazarsfeld can find a permanent place at Columbia. In that respect any arrangement must be speculative, but he is inclined to feel that Lazarsfeld in a period of two or three years may make a place for himself there. Certainly his addition to the social science faculty is something which Lynd personally would aim for but Lynd's influence at the present time is uncertain in view of the current discussion between his group and the more conservative group who stand with MacIver, at present the Chairman of the Department of Sociology." (Marshall 13.12.39)

Lynd's estimation of the time factor proved amazingly accurate, for within two years Lazarsfeld was appointed Associate Professor, and Robert Merton was made Assistant Professor.

Whilst still an instructor at Harvard, Merton had already established himself as one of the leading young theorists in his field with the publication in 1936 of his seminal paper, Social Structure and Anomie. Within two years of the paper's appearance he had become a professor at Tulane University, and Chairman of the Sociology Department. However, in 1941 he accepted the invitation to move to Columbia. But as Hunt points out:

"Not until some time after his arrival on Morningside Heights<sup>1</sup> did Merton learn that he had been hired almost as much for his symbolic value as for his ability. The sharpest fight in modern sociology - a field in which there is even more sectarian bickering than there is in psychiatry - between the mathematically orientated

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<sup>1</sup> District of New York where Columbia University is located.

opinion-sampling empiricists and the pontificating concept-making theorists. The Sociology Department at Columbia was so badly split between these factions that for several years it had been impossible to hire a new man, simply because the two sides could not agree on one. At last, a compromise was arranged and each side would make one appointment. The empiricists chose Lazarsfeld, who had already created his 'Office of Radio Research', and the theorists chose Merton, at that time an excellent specimen of a concept-maker."

(Hunt 1961: 59, 60)

The situation was that a Professor Chadlock, who held the post of full professor, had committed suicide and therefore someone was required to fill his post. However, due to the intellectual split in the department agreement could not be reached over who to appoint and consequently the post remained vacant. The impossibility of this situation led Nicholas William Butler, the patriarchal President of the University,<sup>1</sup> to establish an outside committee to resolve the deadlock. Despite this unusual administrative step, the committee itself found it impossible to nominate anyone for the candidacy of full professor, and instead reached the compromise decision of splitting the post in two. Lazarsfeld related the situation to the writer as follows:

"I remember even now. I remember the figures exactly. Chadlock had 8,000 dollars, and that was split into 4,500 dollars associate professorship and 3,500 for the assistant professorship. MacIver and Lynd would each name their candidate and whoever was older got the associate professorship and MacIver picked Merton, and Lynd picked me.<sup>1</sup> And we were supposed to perpetuate the MacIver split - never turned out that way.<sup>2</sup> But it was as simple as that. It was a judgment of an ad hoc committee which forced this

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- 1 See MacIver 1968: 141 for brief account of Merton and Lazarsfeld's selection.
  - 2 Rather than perpetuate the 'split' Lazarsfeld and Merton fused traditions. For example, Merton's classic paper on theories of the middle range (Merton 1948) was a direct response to Parson's grand theorising - the original paper was a reply to one by Parson given at the 1947 meeting of the American Sociological Association. A revised version of this paper, and the most commonly known, appeared under the title "On Sociological Theories of the Middle Range" published in 1949 in "Social Theory & Social Structure". The paper undoubtedly drew much of its intellectual arguing from contact with Lazarsfeld and the studies undertaken at the Columbia Bureau.

solution on the department, and then you see MacIver and Lynd could not interfere with the appointment.

MacIver was chairman of the department and so his agreement was necessary by the constitution. Lynd wasn't chairman. Lynd became chairman later, and MacIver still retained a certain power. He could have vetoed Lynd I suppose. He knew me vaguely. He knew Lynd would make an empiricist, a professor, and MacIver wouldn't like that, but MacIver couldn't change that, but he could have objected against this particular man. But MacIver was extremely - he was a Scot - and extremely restrained, a very great gentleman. In a way an extremely mean person, but extremely civilised, and so one day I got invited - several times in fact - to play bridge with MacIver, and a friend of his. And that was MacIver's way to wize me up. How do I behave at bridge, because he could not object against an empiricist, but he could say the man wouldn't fit or wouldn't be appropriate for Columbia. If he had said that, then probably Lynd would have had to choose another man. But he never talked to me at that time about any substantive matter - he just wanted to know an I acceptable, so to say. And I knew exactly why I was invited to bridge." (Lazarsfeld 15.6.73)

Again one witnesses the social component involved in academic advancement. As Lazarsfeld suggests, MacIver may well have been looking for an opportunity to dish Lynd by refusing his choice on extra-academic grounds, and had he been particularly ruthless he could no doubt have found some aspect of Lazarsfeld's social behaviour to object to as not fitting with Columbia's expectations of professorial demeanour. As it transpired, however, Lazarsfeld was made Associate Professor and so one can reasonably assume that forewarned by his own social insight into the situation he must have demonstrated sufficient of the social graces expected of a prospective American professor. Lazarsfeld, in his 'Memoir', comments on Stouffer's letter of recommendation to Mills, and presents it as evidence of having to "fight the ghost" of 'foreignness', which is true. Yet the letter must also be placed in the context of the

intellectual strife within the sociology department. The battle which centred around the respective positions of MacIver and Lynd was well known within American academia, and consequently Stouffer must have known of the difficulties that Lazarsfeld's nomination for professorship would face. Hence, out of his friendship for Lazarsfeld and his undoubted intellectual respect for his academic abilities Stouffer felt bound to try and forestall the type of social objections that could be raised by people who did not know him well.

In discussing the transfer of the Project from Princeton to Columbia a variety of factors have been noted, yet perhaps the single most important reason for this shift was the unsuitability of Princeton as a continued point of location, especially since the Project was physically based in New York. However, the reason why such concentration has been accorded to Lazarsfeld's social position, and the difficulties which he faced, is that as Marshall noted "the history of the Project is likewise the history of Dr. Lazarsfeld's establishment in this country". The fact that he may have faced opposition had he attempted to retain the Project at Princeton was not the determining factor in his wishing to have it removed to Columbia. Certainly Lazarsfeld had a disagreement with Cantril, but whether he was aware of a more widespread and socially-based animosity is difficult to say. Rather the situation was that once again the Project had outstripped its institutional base, and required a new home, as indeed Lazarsfeld himself did. There can be no question that from the point of view of Lazarsfeld's own future career prospects, and the future of the Project in general, Columbia University offered benefits which could not be gained by remaining at Princeton. The whole question of the Project's transfer to Columbia was raised in the course of a conversation with John Marshall.

D. Morrison:

Perhaps you could expand on the reasons surrounding the transfer of the Project from Princeton to Columbia. Perhaps you don't wish to go into it in any detail since when I asked Professor Lazarsfeld he didn't wish to talk about it since for him it was so linked up with conflict with Cantril.

J. Marshall:

I wouldn't put it that way at all. I think Paul may have been a little cunning with you. There was some personal animosity between the two men. I know that perfectly well, but it seemed to me that the moving fact was that he had no real position at Princeton other than Head of a kind of Office of Research, and was offered (sic) a professorship of sociology at Columbia. He may have nourished that offer, I don't know. I do know that Robert Lynd who I think was instrumental in taking Lazarsfeld to Columbia was very much pleased that Lazarsfeld went there and I have every reason to believe that Lynd promoted his appointment there.

So far as we were concerned when Lazarsfeld went to Columbia the question was somehow raised ... by Lazarsfeld, or more probably Lynd who was his sponsor.. as to whether the Foundation was prepared to transfer its support to Columbia. We were perfectly willing to. By that time Cantril wasn't the leading figure in the picture, and Lazarsfeld quite clearly was. Furthermore, there was an institutional interest in the whole enterprise at Columbia and it seemed to me a far better institutional base for the enterprise than Princeton, and accordingly I think when the grant to Princeton ran out we simply transferred our grant to Columbia. From that time on it was the Columbia Office of Applied Social Research.

D. Morrison:

I did get the impression that there wasn't much possibility of growth at Princeton. That they didn't have the same supportive academic culture that Columbia had.

J. Marshall:

Yes, I don't think the highly personal conflict between the two men which I know of course, affected it. It is true that Princeton has, as one of its Deans once said to me, that "Princeton is a small university and has no pretensions to anything else but a small university. For example, at that time and perhaps it is still so, it had no department of anthropology. No, it's certainly true that Columbia was far the way better for the enterprise. It certainly went very happily to Columbia from my point of view and acquired really strong allies there, particularly in the persons of Robert Lynd and Robert Merton ... Lynd was a very very strong advocate in Columbia. He was a powerful person in the faculty and once he took Lazarsfeld under his protection it was pretty well situated at Columbia.

(Marshall 6.7.73)

It is understandable that with the passing of time John Marshall should confuse the actual date of Lazarsfeld's offer of a professorship, but no matter; the point he makes remains correct. Although Lazarsfeld was not offered a professorship at the time of the Project's transference, the likelihood of such an offer in the near future was no doubt present in his mind when considering such a move, for as previously stated Lynd considered that such an offer would be made within a few years.

John Marshall's statement that, so far as he was concerned, the Project went "very happily to Columbia" would appear to be correct. Indeed, from the Rockefeller archives it is evident that Marshall and Lynd had repeated 'contact' and had actually agreed between themselves to have the Project housed at Columbia before informing Princeton of their decision. On December 5th, 1939 Lynd wrote a letter to President Dodds of Princeton and sent a copy to Marshall, across the top of which he scribbled "Dear John, the wheels are moving here. This is just for your information", a clear indication that the two men had already arrived at an understanding. Dodds, however, whilst presumably informed that negotiations with the Foundation were taking place, did not realise that Lynd and Marshall had actually come to an agreement on the future location of the Project. Lynd's letter reads:

My Dear President Dodds,

As I suggested after our telephone call yesterday, I am confirming its substance.

It is my understanding that Princeton University is entirely willing to relinquish any claim to such continuence, if any, of the Princeton Radio Research Project as the Rockefeller Foundation may decide upon, and that we shall not be cutting across your plans in this respect if we make an effort to secure a continuation of the study here at Columbia University. As I told you over the phone, I believe it will be of material advantage to the study if it can be carried on under an arrangement whereby its director can have access

to interested graduate students. Owing to our location we here at Columbia can naturally provide such access to advanced personnel in training more readily than can Princeton.

It is understood that any continuation of the Project will be renamed<sup>1</sup> so as not to identify it further with Princeton University."

(Lynd 5.12.39)

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<sup>1</sup> It was in fact renamed "The Columbia Office of Radio Research"

## **Chapter Five**

### **The Radio Project : A Case Study in Research Production**

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### The Radio Project : A Case Study in Research Production.

#### The Context of Research

Through a detailed examination of the Radio Research Project this chapter aims to illustrate the factors which influence research production. Whilst readily accepting that many of the circumstances which Lazarsfeld was required to negotiate were specific to the Radio Project it nevertheless remains true, that the types of situations which he faced were by no means unique since they stemmed in large part from the general institutional setting within which the research was conducted.<sup>1</sup>

It has been argued at various points in the work that the new forms of empirical knowledge required new institutional bases from which they could flourish, and that imperative prompted the growth of research institutes, or social science research laboratories. However, these new research organisations, whilst 'necessary' for the furthest extension of such knowledge, have produced in their wake conditions which in certain instances have had repercussions for that knowledge itself. In particular, their economic instability and their consequent dependence upon a 'client' for financial support opens them to 'non-intellectual' considerations and pressures to a

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1 The term 'bureau' is used in this chapter to cover both the Project's Princeton and Columbia institutional setting.

far greater extent than their colleagues in traditional teaching departments. This distinction was drawn very forcefully by Max Horkheimer in a letter to Leo Lowenthal in 1942:

"Scientific institutions here exercise a constant pressure on their junior members which cannot be compared in the least with the freedom which has reigned in our Institute ... People don't want to understand that there can be a group of scholars working under a director not responsible to big business or to mass culture publicity." (Horkheimer 8:11:42) <sup>1</sup>

Horkheimer's argument is far too crude an interpretation of the situation, though certainly, 'pressures' exist within a Research Bureau which are absent in a traditional teaching department, or even an Institute such as Horkheimer's. This is not to say that pressures are absent in these other institutions, simply that they stem from different sources and take different forms. In fact for him to talk of American scientific institutions such as Lazarsfeld's 'Bureau', as exercising "constant pressure on their junior members" is somewhat ironic, since the pressures he exerted may have been of a different order in his Institute, but nonetheless real enough for those who had to work under him. For as Lazarsfeld informed the writer: "You know also, Horkheimer was an extremely dictatorial

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1 Martin Jay (1973:116) attributes this letter to the fact that Horkheimer's attempt to have his Institute integrated into the Columbia Sociology department, or Lazarsfeld's 'Bureau', had been "politely declined". Insofar as integrating it into the 'Bureau' is concerned then Jay is mistaken. The point was raised with Lazarsfeld who said that no such approach was ever made to him, although the Institute members may well have discussed such an approach amongst themselves.

person and dominated his whole group -- they were afraid" (Lazarsfeld 2:6:73). Indeed the 'Intellectual School' tradition, allied to the dependence of junior staff on the Professor's patronage, can be considered to be just as much a 'pressure' and perhaps even more so, than anything experienced by junior staff in the looser atmosphere of an "American Scientific Institution".

As far as Horkheimer's comment concerning autonomy goes, it is true his Institute was independent in the sense of having a substantial private income - this meant that they did not have to engage in client/contract work. However, in 1938, due to some unfortunate investment of the Institute's capital, it was placed in a somewhat similar situation to that of traditional American research institutes. So serious did the Institute's financial position become that Horkheimer wrote to Löwenthal saying that if no sponsor could be found:

"... not only the work but our lives as scholars with specific tasks and responsibilities .. and not only our intellectual lives but the material basis of our lives .. will be destroyed." (Horkheimer 31:10:42)

It was against this background that contacts were made during the summer of 1942 with the American Jewish Committee, as a result of which Horkheimer was given:

"A grant of considerable size, which helped to keep the Institut together as well as to finance the most exhaustive study of prejudice ever attempted. In May, 1944, a two day conference on prejudice was held in New York, at which an ambitious research programme was outlined for

the future. At the same time, the AJC established a Department of Scientific Research, with Horkheimer at its head. It was here that the Studies in Prejudice<sup>1</sup> which were to employ a variety of methodological approaches to the study of social bias, were officially launched. Thus began the Institut's most extensive and sustained concentration on empirical research" (Jay 1973:221)

Although he later complained to Lowenthal that during the Institute's years in New York it had been forced into becoming a Betrieb,<sup>2</sup> there is little doubt that being forced into a more contractual relationship with other institutions was extremely beneficial and productive to the empirical side of their work.

The need to search for funds can be, and indeed often is, academically productive. Thus, as well as seeing the dependence of research institutes on external financing agencies as a constraint on knowledge, it is also appropriate to consider the same relationship as a stimulus to knowledge. Indeed the fact that research institutes are closely 'locked in' to agencies external to the university precincts often means that a much wider scanning of social problems' takes place. In addition, the ever-present need to obtain funds, to write reports, and to meet deadlines gives such institutes a vitality and productivity which often tends to be absent in teaching departments. However, this positive element also possesses a negative component, in that, productivity may be

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1 For a variety of reasons 'Studies in Prejudice' were never to be completed - (see Jay 1973 chap. 7). But, much of the data and questions were used for the Authoritarian Personality.

2 Jay translates 'Betrieb' as 'Research Enterprise', but it is worth noting that 'Betrieb' in German has distinct business or managerial connotations.

obtained at the expense of contemplative, and critical thought.

Although research can be stimulated by outside agencies, the question of how far their 'concerns', or interests, should be accepted by the academic community as worthy of attention remains highly problematic, particularly when such 'concerns' do not fit with the ethics of the institute's members, or fall outside their intellectual interests. However, in times of economic stringency value positions are often relaxed so that work which would have been refused in more prosperous times is accepted as a consequence of financial necessity. Certainly if the universities wish their associated institutes to engage in 'pure' work then alternative sources of funds must be provided which would allow them more financial security. For the fact is, research institutes are expensive institutions to maintain, and there is little point in holding up university departments as a yardstick since it was these departments very inability to handle large-scale empirical work that led to the establishment of such institutes in the first place. Their presence is now an accepted fact of the social science research scene, and it is to an examination of the factors determining their operations that we now turn, using the Radio Research Project as a case study. Later, in the closing stage of this work a graphic example of the impact of 'external' funding of research will be provided, but for the present, the main aim is to offer a broad analysis

of research as an academic and social activity.

The Foundation Officer : an Important Variable

Of the various agencies impinging on research, the Philanthropic Foundations are undoubtedly among the most important, and the roles played by their officials are often decisive. The officials' relationship to the academic world is somewhat akin to the aristocratic patron of the arts, but with one major difference: it is not his own money which he is dispensing - rather he is a salaried 'expert', employed by the donor's family, or rather its legal representatives. As a result, his judgments of what the foundation should or should not support, depend not simply on his view of a projects' intrinsic merits, but also on his desire to back someone who will enhance his own career prospects within the foundation. Hence, in addition to any intellectual interests he may have on a project he also possesses a strong interest in making sure that the work is completed, and that it adheres to the original terms of the grant. One of the best safe-guards in this direction is of course to back the 'right' man and the 'right' project in the first instance. With respect to funding however, it must be mentioned that foundation monies can be considered to be much more free of 'restraining conditions' than, say, commercial or governmental funds. The relationship between the academic and the foundation official

is a complicated phenomenon, and has within it many of the stresses and strains generally associated with patronage: the academic being grateful for the money which allows him to work, yet resentful at having to submit his work to someone whom, Lazarsfeld described to the writer as, "A little fellow -- who you can't be quite sure whether he could get a professorship, and suddenly he has an enormous amount of power to give money away". (Lazarsfeld 2:6:73) Adorno was even more resentful and, according to John Marshall, saw the Radio Projects as "subject to the whims of a young ignoramus", meaning Marshall in his capacity as a foundation officer. In discussing 'interference', much depends of course on the nature of the intellectual enterprise itself, the personality of the producer, and the character of the external agency's officer. Certainly, Lazarsfeld's own manipulative skills allowed him to survive, by satisfying the Foundation's demands while at the same time, not letting them interfere with what he considered the central tasks of his work. In addition, the more applied the work then perhaps the less destructive such 'interference' is. Adorno for example, was working at such a high level of abstraction, that any 'interference' necessarily went to the core of his ideas. The difference between the two situations was that, taken as a whole, the Radio Project was very open to suggestions, of a practical and procedural nature. Indeed the Projects success depended in part on

co-operation and suggestions from a variety of interested parties, Given the administrative nature of large parts of the work then, it can be argued, that 'interference' in that sector had positive benefits. However, in Adorno's case, he was still working more in the European tradition of the lone scholar where only 'pure' intellectual criticism could be of benefit and thus, by extension, could only be provided by scholars of equal intellectual standing to himself. To a person of Adorno's persuasion, that was a very limited circle indeed and it certainly did not include foundation officers or other 'lay' individuals who were asked to comment on his work.

Foundation Interference as Productive Intervention: The Case of Paul Lazarsfeld

During the Spring of 1939 the question of renewing the grant for the Project came to the fore. Consequently, a review committee of nine distinguished academics and media personnel was convened with Lloyd Free as its secretary. The purpose of the committee was to review the Project's progress to date, and then make recommendations to the Foundation regarding future financial support. Although the Committee was favourably disposed to the Project's continuation, proposition 27 of the report reads:

"In the next phase of the project time and money should be provided for the detailed analysis and interpretation of some of the material collected to date." (Review Committee 10:3:39)

Six days later Lazarsfeld, whilst in Chicago, received the following Western Union Cable from John Marshall in New York:

DISCUSSIONS IN OFFICE INDICATE RELUCTANCE TO INVEST IN NEW RESEARCH PENDING FORMULATION OF PRESENT FINDINGS FEELING HERE THAT NEED IS FOR BREATHING SPELL TO SAVE PROJECT FROM BEING VICTIM OF ITS OWN SUCCESS STOP RESULT DECISION TO MAKE NO RECOMMENDATION TO TRUSTEES NOW STOP READY TO REVIEW SITUATION IN JUNE IF FORMULATION IS SUFFICIENTLY ADVANCED BY THEN TO PROVIDE BASIS.  
(Marshall 16:3:39)

In a sense both the reviewing committee, and John Marshall were correct to note that the Project was in danger of becoming a "victim of its own success". A vast amount of data had been gathered, but there was a distinct lack of any overall themes running through the work, and, after two years of operation this absence gave rise to alarm. Although it had always been understood that the first two years of the Project's life would be mainly devoted to methodological development and data gathering, it would seem, that the foundation was worried that Lazarsfeld would continue on such a course without answering the basic questions for which the Project was first established. Admittedly, the Project's original brief had been rather vague, partly because of the uncharted nature of the subject, and partly to avoid premature restrictions on the scope of the enquiry, but such a state of affairs had played into Lazarsfeld's rather wandering intellectual hands. Whilst there were no reservations about his intellectual capacity, since that had been demonstrated

clearly enough, there were doubts as to whether, or when, he would produce anything substantive. Lazarsfeld himself was well aware of which way the wind was blowing and in a letter, which he wrote to Cantril and Stanton in the winter of 1938, he specifically mentioned that he was:

"much worried about the fact that the prolongation of the project will come up with Marshall and the Foundation at a time when no major unit of the project will be finished" (Lazarsfeld 18:11:38)

Yet, despite his recognition of the need to produce a sizeable report before the Project came up for renewal, nothing substantial was produced. The absence of completed work, presented a problem for Marshall, since he had to justify an extension of the grant to the Foundation trustees. He wrote to D. Poole that although there was:

"... no question that it (the Project) has developed in the past two years in accordance with our expectations ... when .. its work is subjected to scrutiny - particularly on the basis of the inventory submitted by the directors of the Reviewing Committee - one fact stands out- namely, that, suggestive as the Project's findings are, they still in large measure await formulation and final appraisal as to their full significance.

A necessarily hardboiled view of this situation raises one salient question for us: acknowledging all that the Reviewing Committee sees in the project as undoubtedly valid, are we justified in recommending any further investment to our trustees until the formulation of present findings has advanced much further and until their full significance has been explored and reported.

With this the case, our present feeling is, as I told you today, that we should prefer to make no recommendation to our trustees at this time. An alternative course might be for us to ask the directors of the Project to concentrate their efforts during the next months on the task of formulating

present findings. Possibly for the First of June, the directors of the Project can formulate at least a part of their present findings. If so, we should have at least some basis, which we now lack, for a recommendation, if one is called for, to our executive committee at its June meeting" (Marshall 16:3:39)

This letter to Poole followed a telephone conversation between Lynd and Marshall as a result of which Lynd sent Marshall an appraisal of Lazarsfeld. Judging from the letter it would seem that Marshall had expressed certain reservations regarding Lazarsfeld's work performance, for Lynd, first reassures him as to Lazarsfeld's great intellectual capacity and then proceeded to spell out where the basis of the problem lay, and how it could possibly be corrected:

"I think what he needs is a stronger hand (you, a committee or?) holding him to a defined program. I don't believe he had a clear-cut set of definitions and of criteria as to priority in undertaking this job. This was in large part due to the situation: the Foundation did not know what it wanted but wanted a field of alternative possibilities opened up. This played into Paul's over-wide field of interest and aided and abetted him in following his curiosity rather than narrowing a program.

I believe Paul will produce an amazing amount of valuable material on the work done to date. I have no question about that. The money has been well spent. But if you go on - and my judgment would be to do so, tho' in June - I believe you must set up a situation that will help Paul to explore selected problems. He is so darned able that there is no point in throwing out baby and bath. Every researcher has an Achilles heel. His is his intellectual curiosity about everything interesting. He can be channelled. ... The need, therefore, is to use his great strength, but to see that his sailing orders are more explicit." (Lynd 16:3:39)

Although Lazarsfeld had clearly predicted the problem which was to arise over the lack of demonstrable evidence of the Project's progress, he was in all likelihood unaware of the true dimensions of the dissatisfaction. For, in response to Marshall's cable informing him that no further continuation grant would be recommended at the present time, he wrote to Cantril and Stanton that:

"I have not seen Marshall, so I don't know what is behind his decision which he wired me in Chicago. However, there seems no doubt that we shall have to prepare a number of reports on our findings so far. I am not very much disturbed by the request because as you remember, I always felt that this is what we should do for the spring. The situation is only technically rather difficult because we have relatively short time." (Lazarsfeld 20:3:39)

However, the day after the above letter was written Lazarsfeld went to see Marshall, and discussed the whole situation with him.

D. Poole had already sent Marshall's letter, in which he had stressed the need for some formulation of the data, to Hadley Cantril. Poole, in addition, sent a covering letter in which he recommended that, "at least a book manuscript should be in presentable shape by June 1st". If that was forthcoming, Poole felt that, "the Foundation as well as the University<sup>1</sup> will be highly appreciative and proposals for further research would then receive all the sympathetic consideration they deserve". This much Lazarsfeld probably knew, if not by direct communication from Cantril, then by his own insight; however, the warning and the crux of the problem is captured in

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1 Poole was Director of the School of Public and International Affairs at Princeton University where the Project at this time was based.

Poole's message that "meantime anything like the opening up of new research must be sedulously avoided" (Poole 18:3:39) It was this last point, which Marshall discussed at some length with Lazarsfeld during the course of their interview together. It would seem that, although Marshall himself was relatively happy about the work to date, he was, at the same time, apprehensive as to whether the material would be exploited to the full if the present mode of operation continued. In addition, he was under some pressure from other individuals concerning not only the usefulness of such research, but also Lazarsfeld's own capabilities. Thus Marshall recorded:

"There has been a good deal of scepticism as to what the Project could accomplish. J.M. naturally believed with Lazarsfeld that the Project can and does offer much information of significance for bettering broadcasting; but others do not share his belief. The burden of proof is now Lazarsfeld's. More generally there is scepticism about what the methods of social psychology can accomplish. Here is an application of those methods in a new and important field. If this Project in any sense fails, particularly since it seems to have been a more ambitious venture than any other in social psychology Lazarsfeld knows of, the general reputation of social psychology suffers. Finally J.M. said quite candidly, that in some quarters Lazarsfeld himself suffered from the reputation of being a starter and not a finisher.

Lazarsfeld clearly understands that there will be no further consideration of the Project's needs until June, when some tangible outcome of his work in formulating present findings can be put into consideration ... J.M. stated that he would be reluctant to see any consideration of needs involving fresh research till virtually all present data had been thoroughly exploited ... He agreed that in some instances formulation might be postponed if further data

were essential to satisfactory formulation, but he suggested that Lazarsfeld should aim at least at 95 per cent formulation on present findings." (Marshall 21:3:39)

Matters were now desperate for Lazarsfeld, for, although there was no problem about producing a report eventually, in the short term he was faced with very considerable time pressures. Marshall had to have a report to put before the trustees meeting in June so that a case could be made for continued support. To provide the work with coherence, and a point around which the collected material could be organised, the theme of a comparison between the medium of print and radio was selected. By working, "day and night, literally, in relays, to accomplish it," the work report was completed, and according to Lazarsfeld: "submitted .. on the morning of the deadline, which, if I remember correctly, was July 1, 1939" . Lazarsfeld 1969:328:329) The report was sufficient to satisfy Marshall, and provide a basis for recommending a further continuation of the grant, which was duly forthcoming. However, the question of finding a publisher now arose.

Lewis Coser's description of foundations as "gatekeepers of contemporary intellectual life" (Coser 1970), applies equally aptly to publishing houses.<sup>1</sup> The role of publishing houses in the dissemination of knowledge, and thereby the establishing of academic

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1 This was a point brought out by Coser in a discussion with the writer. Graz 1974. In fact Coser is at present engaged in writing on the subject.

reputations, is a question which afflicts the social sciences to an extent unknown in the natural sciences. Unlike the natural sciences where professional communication is dominated more by articles, in the social sciences the book still has pre-eminence, as the major vehicle of research communication and serve as the key, to establishing a solid professional reputation. The importance of book publishing for the social scientist in the transmission of his work is well illustrated in Mullins' discussion of Talcott Parsons.

He states:

"Also important to structural functionalism's success in the speciality stage was Parson's relationship with the Free Press. During the 1940's Parsons had experienced difficulty arranging for the continued publication of his work. The Structure of Social Action (1937) had been a small success outside Harvard, but McGraw-Hill was not interested in publishing a second edition. At this juncture, Parsons learned that Shils, his friend and (later) co-author, was consulting editor for a small house, the Free Press. Its editor, founder, and man of all work, Jeremiah Kaplan, became interested in collecting Parsons' essays up to 1949 and in reprinting The Structure of Social Action. After those publications, the Free Press became Parsons publishing house, enabling those interested in his work to find it in a single place. Many of his students also began submitting most of their book-length material to the Free Press, with the result that this house became the major publisher of sociology for a time." (Mullins 1973: 61,62)

Thus, consequently, following the report's submittal to the Foundation, it was essential for Lazarsfeld to find a publisher for it, in order not only to establish the work of the 'Bureau' within the academic community but also to ease future relations with the Foundation.

By and large, Foundations are problem orientated. To be sure, in the case of the Radio Project, the Rockefeller Foundation was quite prepared to recognise the necessity of two years methodological development but in the final analysis, the research was expected to yield results which might further the improvement of radio, as a medium. Consequently, quite apart from Lazarsfeld's own interest as a professional social scientist in having his work published, he was obliged to legitimate the work in the eyes of the Foundation by communicating the results to as broad a spectrum of individuals as possible. Certainly he was well aware of the political value which could be extracted from publication since he saw fit to inform Marshall that:

"This is a list of academic journals which gave published reviews of Radio and the Printed Page as I have run across them incidentally. Journal of Applied Psychology, Public Opinion Quarterly, Sociometry, Annals of the American Academy of Political Science, Journalism Quarterly, Studies in Philosophy and Social Science, Social Frontiers Modern Music.

These Journals went into the text at considerable length and gave it some prominence. There have been a number of shorter reviews<sup>1</sup> of which I have not kept track. I know of a number of further journals which have given the text to reviewers but I have not yet seen the result." (Lazarsfeld 30:4:41)

Despite the fact that Radio and the Printed Page received widespread attention in academic journals when it came out, securing

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1 For example it was reviewed under the title of Radio and Culture in 'Newsweek' Sept. 2 1940. N.Y.

a publisher had been no easy matter. As early as the Winter of 1938, Lazarsfeld had broached the question of finding a publishing house willing to take the 'Bureau's' work. Cantril and Stanton had already expressed their doubts on this matter considering that the work was too specialised to be of interest to a commercial publisher, and that it would have to be channelled through the Princeton University Press. However, Hugh Kelly from McGraw-Hill heard about the Project's work, and wrote to Lazarsfeld to enquire about his plans for publication. Lazarsfeld was optimistic, but a lunch date with Kelly proved correct, his co-directors' estimation of the works' appeal to a commercial publisher for, on returning to his office Kelly wrote to Lazarsfeld telling him that:

"... I discussed with my associates the proposed publication programme of the Institute, and we have come to the conclusion that we should not submit a proposal for publication. We feel that the books for the most part are fairly limited in their appeal, and while we appreciate your intimation that there might be some funds to assist in publication, we have concluded that it would probably not be wise from your standpoint or ours for us to attempt to publish the series." (Kelly 23:12:38)

Lazarsfeld was clearly upset at McGraw-Hill's decision. As he put it in a letter to Cantril and Stanton: "I am frankly somewhat disappointed because I thought you were too pessimistic but evidently we shall have to work through the Princeton Press". (Lazarsfeld 23:12:38)

Consequently Lazarsfeld was obliged to use the university press rather than a commercial enterprise for their first book, The Invasion from Mars which appeared on April 15 1940. However, it was not a situation with which he was satisfied, and he attempted to find a commercial publisher for the Bureau's second book Radio and the Printed Page. This refusal of Lazarsfeld's to accept his co-directors' advice concerning the unsuitability of their material for other than a university press, is illustrative of his optimistic stubbornness in the face of conventional procedures. Indeed, the manner in which Radio and the Printed Page finally managed to secure a commercial publisher is in itself demonstrative of such unconventionality. Lazarsfeld explained the situation surrounding its publication to the writer as follows:

\*You know, when my manuscript on 'Radio and the Printed Page', which was the main book (for getting his name known) .. it's now reprinted incidentally .. well when the manuscript was finished it wasn't at all clear that I would find a publisher for such a somewhat dry book. Then a friend of Lynd wrote an article about it in Readers Digest on my manuscript. Then it was easy to find a publisher." (Lazarsfeld 25:573)

The article appeared under the provocative title Radio Versus Reading (Muller 1940) and, in accordance with the popularising style of the magazine was written in a particularly 'racy' fashion. This publicity served to arouse considerable interest in the Project and led directly to Duell, Sloan and Pearce's decision to publish the book during the

winter of 1940. In addition, they agreed to publish without asking for a contribution towards its cost, a fact which it would seem Lazarsfeld had not entirely expected, for he wrote Marshall that:

"I suppose you will be pleased to hear that the publishing firm of Duell Sloan and Pearce has agreed to publish "Radio and the Printed Page" without a printing subsidy. I am enclosing a list of their 1940 publications because they are a new firm and you might be interested in knowing in what company we shall be moving." (Lazarsfeld 14:11:40)

Within a few months of the book's appearance it had sold over 1,200 copies,<sup>1</sup> and began what was to be a short but fruitful period of collaboration between Lazarsfeld, and Duell, Sloan and Pearce during which they published not only the two subsequent Radio Research books, but also the Peoples Choice.

Although Marshall, by the 'threat' of refusing to recommend a further release of funds had temporarily 'disciplined' Lazarsfeld into selecting a theme which then formed the basis for a report, and subsequently 'Radio and the Printed Page', his main desire was to get Lazarsfeld to confront his own work, and to clarify his aims. It was not simply a question of seeing that "his sailing orders were more explicit" as Lynd had suggested, but of creating a situation whereby Lazarsfeld would do the task for himself.

Marshall's concern over the general state of mass communication research is well demonstrated in a letter he wrote during the Summer

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1 Figures discovered in Rockefeller Archives, they had obviously been used in a general appraisal of the Radio Project's progress.

of 1939 to Professor Richards of Magdalene College, Cambridge:

"In the last couple of years it has been increasingly clear that most of my work has been in a field which for a lack of a better name I have to call mass communication. It has also been increasingly clear to me that work in that field is hampered for lack of a systematic and disciplined approach. Talks during the last year have convinced me that it is possible to work out some such approach and further that can best be done by a small group of people who in themselves represent different possible approaches to the subject." (Marshall 16:8:39)

Consequently, Marshall instituted a series of seminars which began in September of 1939 and continued through until June 1940. Although these seminars, were originally established as a means of focussing "Lazarsfeld"s shotgun" approach<sup>1</sup> they had much wider ramifications in that they aided the coordination and stimulation of mass communication research in general. Bernard Berelson, for example, reviewing the history of mass communication research in 1959 wrote that:

"The modern version of mass communication research began about twenty five years ago with the development of both academic and commercial interests -- the former largely coordinated, if not stimulated by the Rockefeller Foundation Seminar of the late 1930's." (Berelson 1959:1)<sup>2</sup>

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1 For example James Rowland Angell of the NBC wrote to Marshall "I am still quite unconvinced that Dr. Lazarsfeld will in fact focus his shotgun upon a few central problems. His mind is abnormally prolific of new ideas and these do not seem to me to fall naturally into significant coordinated patterns". (Angell 28:2:39)

2 In all, these seminars produced ten memoranda. Apart from number eight which is missing they still remain in the Rockefeller Archives. The importance of these seminars from the point of concept formation is worthy of special note. It was during the course of proceedings that Lasswell first formulated his now famous model of the communication process: who, says what, in which channel, to whom, and with what effect. Although this formation did not appear in print until 1948 it quickly became extremely influential in mass communication research during the 1950's.

Following the plan which Marshall had outlined to Richards, of keeping the discussion group small, as the best method to proceed in a systematising of the field, only the following individuals were regular seminar participants: Charles Siepmann, Lyman Brydon, Lloyd Free, Geoffrey Gorer, Harold Lasswell, Paul Lazarsfeld, Robert Lynd, John Marshall. Donald Slesinger and Douglas Warples. However, as small as the actual group was, the memoranda were sent out to an extremely large, and distinguished number of academics drawn from a variety of disciplines.

The seminar occurred at a particularly important historical juncture. Although America did not actually enter the war until December 1941, the gathering momentum of the struggle in Europe signalled America's future involvement. It is of interest, that a report on the seminars, written in July 1940, and signed by the above participants reads:

"This report is a statement of belief, belief in the significance of three facts. We believe first, that in the exacting times which lie ahead, public opinion will be a decisive factor. If America is to meet the necessity of adapting to a changing world, and at the same time preserve the ways of life that Americans hold dear, that adaptation must be achieved with public consent. In securing consent, public opinion and the influences affecting it will be crucial. We believe, second, that for leadership to secure that consent will require unprecedented knowledge of the public mind and of the means by which Leadership can secure consent. To secure it, public policy, as never before, will have to take account of public needs and predispositions. We believe, third, that we have available today methods of research which can reliably inform us about the public mind and about how it can be influenced in relation to public affairs." (Seminar Memorandum July 1940)

Throughout the seminar memoranda, which include detailed discussion on methodology, concept formation and the organisation of research, there appear frequent references to the possibility of America entering the war, and the changes that such involvement would mean in terms of altering prevailing attitudes, and organisational practices; the success of which was seen to depend very much on the free flow of communication. Considering such references, the writer presumed that the seminars had originally been convened to consider such matters, but it was only in conversation with John Marshall that the actual history and significance of the seminars came to light.

D. Morrison     Perhaps I could ask you about the main impetus behind the setting up of the mass communication seminars of 1939 to 1940. There seems to be two main thrusts to me. One was the idea of coordinating. That is, the idea that mass communication research really must be coordinated into some kind of body if any useful advancement was to be made. And the second, seems to be the difficulties that America would face if she entered the war, and it seems to me, that the first part, that is the coordinating factor, gets swamped by the actual difficulties which America would face and you thought you should address yourself to that.

J. Marshall     I wouldn't read the record that way. No.

D. Morrison     You wouldn't?

J. Marshall     No. Well my interest in those seminars ... I guess my interest was the basic one ... I got the money and I organised it ... to arrive at a concept of what research in mass communication should be undertaken. Now let me go back a little bit. There

was a prior episode. At a date when maybe you can ascertain, when the question of further support for the 'Bureau' came up. There was a feeling among a number of us who were concerned ... not necessarily in the Foundation ... on the part of Bob Lynd and probably on the part of Charles Siepmann who was by that time in the picture, and others. We felt that Lazarsfeld's research had been admirable for the first period, but that it was scattered and unfocussed. With Lazarsfeld's agreement we therefore subjected him to a days examination. My recollection is -- I can't be sure of this, that the meeting was held at the Century Club. I'm not quite sure.

D. Morrison I think maybe it was the Graduate Club.

J. Marshall No ... It may have been, it may have been ... Yes, the Graduate Club, yes it would have been down there. Yes that's where it was. We had a group of people, and we sort of cross-examined Lazarsfeld all that day, trying to get him to define some focus for his work -- in the next period of his work.

While we did get Lazarsfeld to agree to certain foresight to what he would go on to do, the work was still in a conceptual muddle. There was no sharpness to it what-so-ever. So we came to agree in the Spring of 1939 that we would hold this series of meetings at monthly intervals through the coming academic year. That is September 1939 until June .. (1940) .. well as the first meeting took place the war had broken out. Someone said, 'of course our discussions will have a much firmer orientation if we regard the War as a 'theater'. Public attention will be predominantly focussed upon the war, and it gives us a kind of laboratory situation in which can perhaps really sharpen our thinking'. Now I would therefore strongly correct the idea that the initiating force here was the needs of the country. It was rather that we saw in this rather disastrous situation of the war an unhappy opportunity to conceptualise this whole field of research. The war as it were put all the factors into sharp focus.

D. Morrison Good ...

J. Marshall To round that up .. it was only as we completed .. only as we contemplated mass communication research in war-time that it became clear to us that perhaps this did have some practical outcome. Do you know what happened then?

D. Morrison No.

J. Marshall Well Harold Lasswell of course was a member of that group. Lasswell had just achieved some attention in Washington by utilising his developing methods of content analysis in a trial in which the government was prosecuting someone for alleged subversive activities. This meant that Lasswell had established a good many contacts in Washington. We all said that if this can be made use of in the war time situation then we wanted it to be made use of. I suppose I got some specific release from the Foundation for this document. It was a private document, and supposedly not for publication. Lasswell had the bright idea of introducing this to government thinking at the level of secretaries to the secretaries of various departments.

So Lasswell organised a group of secretaries to secretaries who turned out to be rather extraordinary men. We went down to Washington, Lasswell and I and one or two others for a days discussion of how the implications of this report could be realised in government research. Lasswell was perfectly right, and in this way these secretaries to the secretaries made their principals immediately aware of how research such as this could be used for the advantage of government and I think from then on a very considerable part of the research went over into government offices.

D. Morrison Did you ever have a crisis of confidence in Lazarsfeld? Or was it that you saw that he had ability, but needed closer direction?

J. Marshall The latter.

D. Morrison There is a letter in the archives I think dated March 30 1940<sup>1</sup> from Professor Lazarsfeld to yourself, and he mentions that Adorno had been given some time on the Damrosch Hour. He says something like "I suppose

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1 The date of the letter was the thirteenth of March 1940 and not the thirtieth. The letter reads: "You will be interested, I am sure, in hearing that on the basis of Dr. Adorno's analysis of the Damrosch Hour, WNYC has offered him the opportunity to give a music appreciation hour of his own every Sunday. He will start in April. I suppose that this is the kind of direct application of our work you are looking for. (Lazarsfeld 13:3:40) (my emphasis)

this is the direct application of our work you were looking for". Did you ever feel that the work of the 'Bureau' was not practical enough. I wondered whether if this was so that it developed conflicts or strains. It's interesting that he should say "I suppose this is what you are looking for".

J. Marshall      No, I take his statement on it. I can't elaborate on that point. Temperamentally I would have been interested in the theoretical elaboration of his work as well as the empirical work. In fact the whole exercise of the mass communication seminars I think was to get a solid theoretical basis for what was going on. (Marshall 6:7:73)

John Marshall continued at some length discussing the importance of the seminars, and the significance they had for him in that after 1940 he felt much more comfortable with the direction the work was taking. In particular, he considered that the celebrated formulation that Lasswell had come up with during the seminars - who says what, to whom, and with what effect - helped greatly to conceptualise the field and bring a precision of thought that had hitherto being lacking. Commenting on the role that Lasswell's formulation came to occupy in mass communication research he stated that, "It seems almost platitudinous today, but that concept of mass communication had not arrived at that point.. Imagine if you can, research in mass communication that wasn't guided by those thought considerations and you have a picture of much of what Lazarsfeld was attempting before 1939".

Thus, there is no doubt that Marshall, quite apart from

financial contributions did, acting as an agent of the Rockefeller Foundation, help guide, and shape early mass communication research at the intellectual level. However, it is not the writer's intention to argue the merits of such 'intervention', but rather to illustrate the process of research, and the context within which it proceeded. Whereas in Lazarsfeld's case, Foundation interference can be seen as having generally beneficial results, the case of Adorno presents a somewhat different aspect.

Wiesengrund Adorno and the Non-continuation of the Music Project:

An Alternative Case

Earlier it was mentioned that Adorno had the unfortunate habit of upsetting many of those he came into contact with, which, given the nature of the Radio Research Project could certainly be considered a handicap. In fact one wonders whether some of the criticisms of Adorno's work, in the letters written to Marshall, are not in part prompted by personal animosity towards him. Such a question is difficult to answer, yet John Marshall, whom it must be remembered had financial control over the future of the Project, related the following observation on Adorno's personality.

D. Morrison     I know that Professor Lazarsfeld considered Adorno a complete embarrassment to him -- he related that to me -- he said that he would send him along to see various people and Adorno would insult them.

J. Marshall      Well Adorno would have been an embarrassment to anybody, he was an embarrassment subsequently to his colleagues in Frankfurt. I was visiting Frankfurt in 1959 and one of my very congenial contacts there said I probably ought to see Adorno. I said I just don't want to and he said, "I don't blame you, I don't want to either". He said "as a matter of fact although he is a colleague of mine I see as little of him as possible". He was a highly abrasive and cock-sure little man. (Marshall 6:7:73)

Despite Marshall's obvious dislike of Adorno's personality he nevertheless spoke very highly of his intellectual capacity; relating how impressed he was with him after hearing a paper which he gave to a staff seminar at Columbia University. Indeed, even to this day Marshall remembered much of Adorno's work on music. Thus the question was raised as to whether the Music Project was refused funds because Adorno worked within the tradition of 'critical theory' and consequently had difficulty in fitting the Foundation's expectations for the Project.

D. Morrison      I wondered whether the Music Project was refused further funding because of difficulties at the personal level, that he was impossible to get along with? Or whether it was because of criticism against his academic work that it was dropped? But you say that you were quite favourable to his work.

J. Marshall      Well that's my recollection. I'm a little surprised to hear that I cut it off. I'm a little curious myself to know what the reasons were.

D. Morrison      I thought that being in the tradition of 'critical theory' that it was far too critical and offered little indication of how the situation could be improved.

J. Marshall      Well, that may well be. If so, it's possible that I had taken the opinion of people in music who I was then close to. I doubt I would have arrived at that opinion myself, but then I'm not sure.

(Marshall 6:7:73)

To begin to understand the reasons for the non-continuation of the Music Project, which now seems to have acquired a certain mystery, it is essential to examine the background to its original establishment, and the very real difficulties that it faced. Briefly the failure can be attributed to the task which Lazarsfeld set himself of allying 'critical theory' with administrative research, and his misjudgment with regard to the structural setting of his own work. Lazarsfeld in his article on Critical Research and Administrative Research, which first appeared in Horkheimer's Journal in 1941, wrote:

"If it were possible in terms of critical research to formulate an actual research operation which could be integrated with empirical work, the people involved, the problems treated and, in the end, the actual utility of the work would greatly profit." (Lazarsfeld 1972:165)

Lazarsfeld may certainly have been intellectually equipped for such a difficult task, and undoubtedly addressed himself very seriously to the question; yet it was not simply a question of integrating critical theory with empirical research, but of accommodating that structure of knowledge within an administrative structure, and on that score he failed. In the same way that the 'new' empirical knowledge required for its furthest extension research institutes, so 'critical theory' flourished best in the kind of institute established by Horkheimer, in which private patronage underwrote untrammelled reflection. Not surprisingly, such work, when transferred to the situation of administrative research, could not easily accommodate itself to the expectations of the Foundation and their advisers.

Whereas the critical analysis of society can be considered to a certain extent as an exercise valid in itself for Adorno, the Foundation expected criticism plus corrective solutions. Yet Lazarsfeld did consider, that given time and fuller cooperation, Adorno's criticism could have led to changes within music production. However, before such developments could occur the Music Project was discontinued.

Quite apart from the intellectual desire on Lazarsfeld's part to marry 'critical theory' with empirical research, the Music Project's beginnings can be traced back to the deep significance which music had for him. Having spent his childhood, adolescence, and early adult years amidst the bourgeois culture of Vienna with its particular emphasis on musical appreciation it is understandable that music should hold some meaning in his life. The depth of that meaning he described as "a very deep unhappy love to music. I played a great deal but was really never good at it. So music plays a very complicated role in my life" (Lazarsfeld 15:6:73). John Marshall expressed the collaborative comment that, "Paul always had a strong personal interest in music. As I remember, when he proposed that the enterprise should take music into account he said he was really doing this to absolve his conscience" (Marshall 6:7:73). The Music Project, and the difficulties presented by such an ambitious attempt to combine Adorno's work with empiricism were

discussed with Professor Lazarsfeld in some detail; the following is a transcript of part of that discussion:

P. Lazarsfeld Well its best to start ... look you have to see how that came about. I have always been interested in music, and already in Vienna I wanted to study popular songs. I invented this machine -- this programme analyser. In Vienna I even had student dissertations. I wanted to find out for some reason why ... what it is in the musical structure which makes songs popular. I had this idea that people would just push buttons, and say that I like it or dislike it, and then a musicologist would analyse the musical structure and would relate musical structure to the action somehow. Then I became Director of the Princeton Project in 1937. I immediately wanted to have a music division, and to create that right away. In the first volume of Radio Research there is McDougal's paper on the popular music industry I had a man studying the whole plugging machinery and so on . So it all began with my great interest in music and wanting to have a music division. I had heard about Adorno but hadn't known him.

D. Morrison Presumably you had read some of his material?

P. Lazarsfeld Yes, but only at the ... look, look I had better be precise. I had read, he had written under a pseudonym one or two papers on jazz<sup>1</sup> but my motivation in inviting Adorno was first in wanting a music division, and secondly I wanted someone -- probably with this European background, at least with a theoretical background, and I think a very important part was that the Horkheimer Institute had been very helpful to me when I had this little place at Newark -- this Research Centre. Horkheimer subsidised us -- paid our secretary or something, so I felt very indebted to Horkheimer, and I knew he wanted to get Adorno here, and it was really almost repayment of the help he had given me. I knew by rumour how good a musician Adorno was, but I think wanting to, so to say,

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1 This presumably refers to Adorno's article "Uber Jazz" which appeared in the Zeitschrift fur Sozialforschung 1936 under the pseudonym Hektor Rottweiler.

repay a debt to the Horkheimer group played a great role. So I went to Horkheimer, and said, "Look I could help get Adorno here, I will make him Director of my music division". And so, I think Horkheimer paid half his salary and I paid half of whatever it was. So I developed all sorts of ideas to work with, I also hired at the same time a young psychologist from Ohio State (University) who Frank Stanton had mentioned to me - Wiebe. He is now a very important man - Dean of the School of Communication at Boston University. Wiebe had just gotten his degree ... he was a professional jazz player -- worked his way through school, and I had this idea that the combination between such an esteemed theorist abstract man like Adorno, and such a typical American Middle Westerner like Wiebe. So as usual I suppose I didn't have any very definite plans -- I usually put something together, and hope it will work. I wanted music -- I wanted a European, and then an American Empiricist together, and it worked miserably. First Adorno was very intolerant - he held the view that Wiebe was just the lowest kind of human being,

... When we talked about the project then I got desperate because he wouldn't produce anything I could use or send to the Foundation. I mean he would write very long memoranda which were so unintelligible that I couldn't use them or what was much worse he embarrassed me when he talked to people, and then when it came to the extension of the project in 39 it just ...

D. Morrison Yes, did the Rockefeller Foundation put pressure on you to drop the musical side or did you realise it was moribund. Did the Foundation put pressure on you or ... ?

P. Lazarsfeld No, you know, I told you this man John Marshall was the main person. I remember a ten or fifteen pages address to a group once on his ideas, and John Marshall was there, and after all John Marshall was a civilised man, a historian, and again everyone was so confused by what Adorno said.

D. Morrison    So it was out of practicality that you dropped the programme. Did you not consider pushing Adorno out and getting someone else in?

P. Lazarsfeld    No, that I couldn't have done, no I mean that would have been against my .. so to say, respect for him.

D. Morrison    I gathered also that Adorno was also dissatisfied ... I know he didn't like the programme analyser.

P. Lazarsfeld    Oh I know completely. It just petered out.

D. Morrison    Some of the things I am going to ask you is really going over old ground, but I would like to clear this up in more detail. I'd like to ask you about the article that appeared in the Frankfurt Journal which I read in England --- the attempt to relate communication interests with the Frankfurt tradition.<sup>1</sup> Did this represent a serious focus of interest or was it written to make good relations with the Frankfurt School.<sup>2</sup>

P. Lazarsfeld    No, no that was very serious. No, my relations with the Institute are usually not given to such moral scruples -- but to somehow come to terms with something which seemed to me to have a core of intellectual integrity, and at the same time seemed to be foolish and irresponsible. It was

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1        This refers to Lazarsfeld's article "Remarks on Administrative and Critical Communications Research" (Lazarsfeld 1941)

2        This was a point suggested by Dr. Barton. He himself was unsure about the nature of Lazarsfeld's relationship with the Frankfurt School.

always a mixture of curiosity, interest, respect and irritation.<sup>1</sup> So anything, any contact with the Institute was always quite a sincere quality.

D. Morrison    What, you thought you could rescue them from their own follies<sup>2</sup> or would that be too grand?

P. Lazarsfeld    There is a famous joke which I have remembered now for forty years because it completely reflected -- you can take this back as real American folk lore --- During the first Roosevelt election, Roosevelt had three very famous advisers. One was Smith -- was Governor of New York, a devout Catholic, Moley who was a Protestant, and a professor of economics at Columbia, and the famous Baruch, a Jewish businessman. And the story went that the three men on this campaign have to sleep in one room, and on Sunday morning Moley and Baruch are snoring, and Smith gets up to go to morning mass at seven o'clock, and as he staggers out sleepy he says, "wouldn't it be Hell if they were right and I was wrong". This was really -- that describes it -- I always remember that.

D. Morrison    So it really was that serious?

P. Lazarsfeld    Oh yes, it was far from being a public relations gesture. I felt it would be just wonderful to relate Adorno to empirical research.

D. Morrison    Looking through the Rockefeller files there is correspondence in there saying that you took music and allowed critical faculties to operate there .. you took music because it was rather a safe option. That it was distant from the central media issues.

P. Lazarsfeld    That's John Marshall's interpretation?

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1        At the intellectual level these emotions are captured in his article "Critical Theory and Dialectics" (Lazarsfeld 1972)

2        At the personal level Lazarsfeld was concerned about the Frankfurt School's institutional position at Columbia. Consequently he repeatedly advised them to write in English rather than German. For example, after failing to have the 'Institut' integrated with the Sociology Department Lazarsfeld wrote to Theodore Abel that: "The wholl mess is due to the idiocy of the Institute group. I told them for years that publishing in German will finally destroy them. But they had the fixed idea that their contribution to America will be greater if they preserve in this country the last island of German culture". (Lazarsfeld 5:2:1946)

D. Morrison     It was conversations with you, and then they are written up as memoranda, then obviously circulated around the office to let everyone know what was happening.

P. Lazarsfeld     Oh really, my word.     You have that .. you can see that?

D. Morrison     Yes, so I presume you must have suggested to him ...

P. Lazarsfeld     I might .. you know if you feel this "wouldn't it be hell if they were right and you were wrong".     You try a different interpretation.     (Lazarsfeld 2:6:73 and 15:7:73)

The above has been quoted at some length since it captures both the aspirations and the difficulties which Lazarsfeld faced with Adorno.     However, too much emphasis should not be given to Adorno's personality at the expense of overlooking his style of work, which, in the final analysis was the essential factor leading to the Project's demise.     For, although Adorno was perhaps an unfortunate choice for work in the realms of administrative research when viewed from the social position, nevertheless, he was a good example of what can be referred to as the Frankfurt tradition     of social research, and in that, fulfilled Lazarsfeld's requirements perfectly.     Furthermore, he was a 'professional' musician, having trained under Alten Berg in Vienna, and taken piano instruction with Eduard Steuermann as well as being editor of 'Anbruch', a Viennese periodical of modern music.     Yet even as a young man Berg, "found Adorno's uncompromising intellectuality a bit disconcerting".     Whilst in Vienna Adorno moved

in the radical cultural circles which:

"If anything his frequent attendance at readings by Karl Kraus, that most unrelenting upholder of cultural standards, and his participation in the arcane musical discussions of the Viennese avant-garde only reinforced his pre-disposition in that direction. Never during the remainder of his life would Adorno abandon his cultural elitism." (Jay 1973:23)

Thus it can certainly be said that Adorno's personality promised for difficulties at the level of 'adjustment', yet it remains doubtful whether such a factor in the end made for failure with respect to the Project. Had Adorno been more capable of compromise, both intellectually and socially, and more able to recognise alternative possibilities, then perhaps Lazarsfeld could have helped him more. However, one is confronted by the question of personality as a component of intellectual creativity, and in that respect, to separate out the social and intellectual elements of Adorno's make-up is to render a disservice to his obvious brilliance. To speculate on what may have been had his personality been different is to miss seeing Adorno the person, and treat his work in abstraction from the individual who created it. His cultural elitism was an essential ingredient of his cultural concern and creative thrust, but at the same time this intolerance severely restricted the possibility of full cooperation.<sup>1</sup> At times, Adorno's arrogant superiority hindered his

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1 Donald Macrea reviewing Adorno's Negative Dialectics says of him: "I thought Adorno on our one meeting, the most arrogant, self-indulgent (intellectually and culturally) man I had ever met. Some twenty years later, I can think of additional claimants for that position, but I doubt if they are serious rivals. Negative Dialectics, one of his last works, bears out my opinion." (Macrea 1974:786)

own development since he was dismissive of ideas contrary to his own. Further, his rejection was not always based on understanding, but sometimes on ignorance. Lazarsfeld for example upbraided him for his ill considered dismissal of other people's verification techniques and pointed out that his "text leads to the suspicion that you don't even know how an empirical check upon a hypothetical assumption is to be made". In addition, Adorno's general disregard for empirical evidence infuriated Lazarsfeld in the extreme, and again illustrates his arrogance. Thus, after discovering that contrary to his expectations, people remembered news better when transmitted via radio rather than print, he labelled such a situation as paradoxical, simply as Lazarsfeld noted, "Because the fact does not correspond to your assumptions". Further, Lazarsfeld complained that; "you make statements about what people do 'in America", also you have had personally hardly any experience, and even if you had, you could neither prove nor disprove such kind of generalities in which nowadays not even travelling journalists write any more". Yet despite such dissatisfaction on Lazarsfeld's part, he never wished to undermine Adorno's theoretical position adding that "You know that I agree with you that empirical research should be guided by theoretical considerations". Neither did he particularly wish to curtail Adorno's critical stance, or even to censor his insulting

remarks. He did however object to the arbitrary manner in which the criticisms and insults were arrived at.

"You seem to confound the independence of the critical mind with the readiness to be insulting. So if you find a juicy insult you feel very satisfied without considering other insulting possibilities. On page 111 you state that the radio networks are interested in catering to the preferences of their listeners because of their fear of losing their licences. Could it not be that they are at least as interested to sell time to advertisers by providing the big audiences and therefore cater, etc.? On pages 107 and 109 you express a theory that the broadcasting officials who decide on programs pick out so low grade programs because they are as bad taste as the broad markets have. Could it not be that these officials are not morons but scoundrels who corrupt the masses against their better knowledge? (How many radio officials listen to their own programs in private life?) You see at this point of my argument I don't try to keep you from being insulting, I just try to show you how illogical and without foundation you are when you select one insult rather than another. And if insults are necessary in a critical study -- I don't want to argue about that now -- don't you think that they should be based on an orderly procedure." (Lazarsfeld undated)

Yet, Adorno did face very real difficulties in adjusting to the demands of administrative research, and in particular to the question of applying empirical methods. As he later put it, he:

"was particularly disturbed by the danger of a methodological circle: that in order to grasp the phenomenon of cultural reification according to the prevalent norms of empirical sociology one would have to use reified methods as they stood so threateningly before my eyes in the form of that<sup>1</sup> machine, the programme analyser." (Adorno 1969:347)

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1      Emphasis mine.

Although the above was written some thirty years after the actual experience, it still conveys a vivid impression of Adorno's disquiet at being confronted by a new style of work. For example, he even refers to the Music Project as, "the so-called Music Study", almost as if he cannot bring himself to accord it the respect of using the correct title. Certainly, the whole research 'set-up' was an entirely new experience. His lack of familiarity is abundantly clear from his own admissions that he did not really know what a research project was,<sup>1</sup> and that he understood "the word method more in its European sense of epistemology than in its American sense, in which methodology virtually signifies practical techniques for research". (Adorno 1969:343) Such misunderstanding on Adorno's part cannot be attributed to any lack in his command of English since Lazarsfeld, after first meeting him wrote to Stanton and Cantril that, "he worked in Oxford for the last few years and speaks a better English than I do". (Lazarsfeld 7:3:1938)<sup>2</sup> Rather, such misunderstandings are better attributed to his distance from the situation which greeted him in America. Adorno had accepted Lazarsfeld's offer on the strength of the feeling that Horkheimer would not have recommended him to take the post unless, "I, a

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1 See Adorno 1969:340. He states that "the American use of the word "Project", which is now translated in German by forschungsvorhaben, was unknown to me".

2 It would seem that Adorno himself was reasonably confident about his command of English for he writes: "In three years in Oxford, I had learned English autodidactically but fairly well" (Adorno 1969:340).

philosopher by calling could handle the job" (Adorno 1969:340). Nevertheless, despite such trust in his friend's judgement, the shock of seeing administrative research in progress seems to have been as penetrating as it was abrupt:

"The Princeton Radio Research Project had its headquarters at that time neither in Princeton nor in New York, but in Newark, New Jersey, and indeed, in a somewhat pioneering spirit, in an unoccupied brewery. When I travelled there through the tunnel under the Hudson I felt a little as if I were in Kafka's *Nature Theater of Oklahoma*. I was very much taken by the lack of embarrassment about the choice of a site that would scarcely have been conceivable by the lights of the European academic community. My first impression of the researchers already in progress there was not exactly marked by any great understanding. At Lazarsfeld's suggestion, I went from room to room and spoke with colleagues, heard words like "Likes and Dislikes Study", "success or failure of a program", of which at first I could make very little. But this much I did understand: that it was concerned with the collection of data, which were supposed to benefit the planning departments in the field of the mass media, whether in industry itself or in cultural advisory boards and similar bodies. For the first time, I saw 'administrative research' before me. I don't now recall whether Lazarsfeld coined this phrase, or I myself in my astonishment at a practically orientated kind of science, so entirely unknown to me" (Adorno 1969:342,343).

As revealing as a "practically orientated kind of science" was for Adorno, it would be mistaken to consider that he bore the full brunt of such research practices. For although Adorno considered that at the time he misunderstood what was required of him, and only later realised that, "Insights into the relationship

between music and society were not expected of me, but rather information" such was not actually the case. To be sure, Adorno came to perceive it to be so, and consequently, "felt a strong inner resistance to meeting this demand by turning myself inside out". Yet it is not a true interpretation of the situation when seen as a whole. As far as Lazarsfeld was concerned, such a demand would have defeated the object of the exercise, which was the combination of approaches, not the domination of one by the other. But, very real difficulties remained. Administrative research, grounded as it was in the application of quantitative methods, raised basic questions for Adorno which he was obliged to tackle. The fundamental questions of objectivity and subjectivity proved particularly stubborn. He was unconvinced that empirical methods could illuminate the basic structures of society, and concerned about their claims as a prime source of sociological knowledge; worrying in particular about the structure of society which resisted direct empirical investigation. He refused to accept that one could proceed from the opinions of individuals to the social structure and the "social essence". This proved to be an acute point of concern on the Music Project, and can possibly account for his bitter reference to the program analyzer as "that machine", symbolising as it did the naivety of American empiricism. As

far as Adorno was concerned, the statistical average of respondents' opinions about the stimulus remained, despite the seeming objectivity of the data, at the level of subjectivity. To Adorno there was little possibility of coming to terms intellectually with such an approach:

"... it appeared to me, and I am still persuaded today, that in the cultural sphere what is regarded by the psychology of perception as a mere "stimulus" is in fact, qualitatively determined, a matter of "objective spirit" and knowable in its objectivity. I oppose stating and measuring effects without relating them to these "stimuli" i.e. the objective content to which the consumers in the cultural industry, the radio listeners, react. What was axiomatic according to the prevalent rules of social research, namely, to proceed from the subject's reactions as if they were a primary and final source of sociological knowledge, seemed to me thoroughly superficial and misguided. Or, to put the matter more prudently: research had still to determine how far the subjective reactions of the persons studied are actually as spontaneous and direct as the subjects suppose; and how far not only the methods of dissemination and the power of suggestion of the apparatus, but also the objective implications of the material with which the listeners were confronted, are involved. And finally, it had still to be determined how far comprehensive social structure, and even society as a whole, came into play. But the mere fact that I proceeded from art as from something objective in itself, instead of from statistically measurable listener-reactions brought me into a certain conflict with prevailing habits of thought" (Adorno 1969:343,344).

Furthermore, the task of translating theoretical considerations into empirically researchable propositions persistently defeated him. As he later put it: "verbalising what music subjectively

arouses in the listener, the utter obscurity of what we call "musical experience". I hardly knew how to approach it". It was for that very purpose that Lazarsfeld had assigned Gerhart Wiebe to the music side of the Project, as it transpired however, the two men never fused intellectually, or would it seem emotionally. Even accepting the fact that Adorno was never an easy person to collaborate with, their relationship would appear to have been an absolute disaster. How far this can be attributed to differences in intellectual background is hard to say, but Adorno's comment, some thirty years after that, "I once had an assistant of Mennonite lineage,<sup>1</sup> (emphasis mine) whose ancestors had come from Germany long before", is particularly revealing to the depth of his dislike, and equally of his petulance. Although not naming Wiebe in person Adorno comments that:

"He hardly grasped what I was after. A certain resentment in him was unmistakable: the type of culture that I brought with me and about which I was genuinely unconcerned, critical of society as I already was, appeared to him to be unjustifiable arrogance. He cherished a mistrust of Europeans such as the bourgeoisie of the eighteenth century must have entertained towards the emigre French aristocrats. However little I, destitute of all influence, had to do with social privilege, I appeared to him to be a kind of usurper."

(Adorno 1969:348,349)

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1 The Mennonites were an ancient Christian Protestant sect from Friesland. It is a strange way for Adorno to categorise Wiebe. It may be that Adorno wished to stress Wiebe's protestantism, but more likely, to associate him with the 'strange' beliefs which the Mennonites held.

What seems to have particularly annoyed Adorno is that instead of helping him to transfer his, "formulations of the problem into strategies for research ... he wrote a sort of protest memorandum in which he contrasted, not without emotion, his scientific conception of the world with my idle speculations (as he regarded them)". (Adorno 1964:348) The complaints which Adorno makes against Wiebe may well be true. It must be remembered however that Wiebe was then a doctoral student, and one would have thought no match for someone of Adorno's ability when it came to an intellectual 'show-down'. It therefore seems likely that in the personage of Wiebe the whole difficulty of the situation which Adorno faced was personified. That is, it was Wiebe's unfortunate experience to have the animosity of the situation projected onto him at the individual level. Thus, by understanding the general situation and criticism which faced Adorno it becomes easier to understand that the diatribe against Wiebe, was directed not so much against him as against what he represented.

Despite the difficulties which were to follow, Lazarsfeld upon initially meeting Adorno, had high expectations of a fruitful collaboration. After saying that Adorno "looks exactly as you would imagine a very absent minded professor, and he behaves so foreign that I feel like a member of the Mayflower Society", Lazarsfeld proceeded to point out to his co-directors the advantages

of having a person such as Adorno work on the Project:

"He treats me in about the same manner as I treated you in the beginning. Every day I get from him twenty to forty pages of memoranda and suggestions and he is very offended if I haven't read them all two hours later. I spend considerable time with him lining up his plans and sometimes goes off on musical tangents from which I have to get him back to the realities of our project. It is, however, a great relief for me to have someone around whose problem is that he has too many ideas and not too few.

I have to take his musical abilities on other peoples testimony but can judge his abilities of social analysis and they are quite remarkable even if I don't always agree with him. He might be of very great suggestive value on other parts of the project.

My summary impression is very favourable and I think it was good luck for us when we took him on. I am, at this moment, inclined to consider him our most valuable staff member<sup>1</sup> in spite of the technical difficulties which might come up from time to time in view of his un-American personality" (Lazarsfeld 7:3:38).

Although Lazarsfeld was greatly pleased at the fecundity of Adorno's ideas, and looked forward to their translation into research-able propositions, certain questions were soon raised regarding both his attitude and his competence. For example Stanton wrote Lazarsfeld that:

"At your suggestion, I asked Mr. Chamberlain to read Dr. Weisengrund's (sic) resume of his interview. Mr Chamberlain reports that part of the material is correctly summarised. However, he was in doubt regarding Dr. Wiesengrund's comprehension of many points. One of Chamberlains criticisms was that answers he gave to W-A's questions were connected with other questions

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1 My emphasis.

in the summary. In other words, sections of the resume are badly garbled.

In light of this situation Mr. Chamberlain made the suggestion that Adorno should send a list of all the questions on paper so that a written response could be given and in that way:

... Mr. Chamberlain feels he will be protected and W-A will get accurate answers. Further more, Chamberlain will have the opportunity to designate which of the answers are what might be called expert opinions and which are based on experimentation.

One of the difficulties, apparently, is that Dr. W-A approached the interview with certain pre-conceived notions which were obvious to Mr. Chamberlain. Chamberlain has the feeling that W-A might have given a distorted picture because of his pre-conceived position" (Stanton 6:12:38).

This question of whether Adorno's preconceptions actually interfered with the 'reliability' of the memoranda, presented itself as a very real and difficult problem for Lazarsfeld, since it struck deep into the heart of questions concerning appropriate procedures of research. For example Adorno, reflecting on his American experience noted:

'... I was later confronted with the argument that if too many ideas are developed as hypotheses before empirical investigations, one may succumb to a bias that might endanger the objectivity of the findings. My very friendly colleague preferred to regard me as a medicine man rather than make room for something that lay under the tabulation of speculation. Taboos of this nature have a tendency to spread beyond their original sphere. Skepticism towards the unproven can easily turn into a veto upon thought' (Adorno 1969:349).

The above raises some long-standing debates within the social sciences, not least of which is the role that one accords to observable data, and the whole frame of reference which one selects for research; however, the question for Lazarsfeld was not that of 'dodging' such

issues in any debate with Adorno, if such a debate was warranted, but actually determining the precise issues raised by Chamberlain's complaints. Was it that Adorno's interpretations were simply in opposition to those of Chamberlain, or had Adorno actually 'distorted' the material which Chamberlain had provided him with? Lazarsfeld was extremely sensitive to this issue, and wrote Stanton that:

I don't quite know how to proceed from your memorandum on Chamberlain's reaction to the WA interview. The one item which Chamberlain raises is that WA connects answers to those points he has gotten. I think that it is the right of anyone who interprets information he gets. WA's reply could easily be that he does not intend to write up what his partners in interviews tell him but to give it a theoretical meaning. His responsibility is not to distort facts but his interpretations are his business and have to be discussed in open battle after they are published.

Even a preconceived notion obvious to Mr. Chamberlain would be more a sign of WA's bad taste than of his inabilities as long as he does not distort the replies he gets.

The reason why I made WA write this memorandum was that I wanted a written document on such an interview so that Chamberlain could actually point his finger towards a distortion (original). It would be of greatest importance for me to get such an actual example and if Chamberlain's reaction was justified in any way, it would be extremely easy for him to point out, in WA's memorandum, one or two concrete cases of such distortion.

After all that is exactly the question I have to decide: Whether WA has just a queer way of behaving of which he might be cured or whether he has a basically wrong attitude which might disqualify him in spite of his other abilities. I am quite undecided as to which of the two alternatives I should accept and feel most uneasy that all my efforts to get some evidence on the matters fail. If you feel that Chamberlain should not be bothered any more, we might as well drop the matter but as your memorandum stands now, it does not go as far as any of the periodic vituperations I give WA.

If you dislike molesting Chamberlain once more, we might as well wait for a new test case. For the rest, I shall convey to WA the advice you give in the second paragraph of your memorandum" (Lazarsfeld 8:12:38).

It would seem that it was shortly after this correspondence that Lazarsfeld appointed Wiebe to work with Adorno and not, as Lazarsfeld appeared to suggest in the interview, that he appointed Wiebe at the same time as he appointed Adorno. The confusion probably arose because prior to his work with Adorno, Wiebe had undertaken work on the Project in general. Furthermore, it would also appear that Lazarsfeld had considered getting rid of Adorno, despite his statement to the contrary, "that would have been against my respect for him". For he wrote to Cantril and Stanton that:

"As you know, I am a bit undecided as to what should be done about the status of Dr. Wiesengrund and I hope to have enough evidence in about a month so that I can make concrete suggestions. We might either keep him in his present status for some more time, discard him completely, or give him some consulting capacity. In all three ways, however, an executive assistant for the music section will be necessary who will carry through in empirical studies the great wealth of suggestions which Wiesen grund has made during this year.

My suggestion is to use Wiebe in this capacity if we can come to an acceptable arrangement ... " (Lazarsfeld 23:12:38).

### Internal difficulties as External Problems

The problem to be discussed now is of a different order, yet not unrelated to the internal difficulties which had arisen within the Music Project itself. Without in the least wishing to minimise

the serious internal difficulties which had arisen, through attempting a fusion of disparate styles of work, it is nevertheless possible that had those directly involved been allowed to continue in their endeavours, greater success of that score might have been achieved. But the internal difficulties cannot be divorced from the administrative setting within which they were embedded. The difficulties within the Project became the property of the Foundation, and were thereby, transformed from an intellectual problem into an administrative question and, when placed in this context, Adorno's work, which was both difficult to understand and often obscure,<sup>1</sup> could not depend upon its intellectual substance as a line of defence. Whereas Lazarsfeld had always appreciated Adorno as a thinker of great ability, and consequently, was prepared to overlook or tolerate both his unusual social behaviour, and his lapses of academic

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1 Frederic Jameson in his article T.W. Adorno, or Historical Tropes makes the following interesting observation on the difficulty of Adorno's language. After commenting on Adorno's "Hegelian spirit" he states that he was faced with the formal problem of "... how to write chapters of a phenomenology when there is no longer any possibility of a whole? How to analyze the part as a part when the whole is not only no longer visible, but even inconceivable? How to continue to use the terms subject and object as opposites requiring a synthesis to be meaningful, when there is no synthesis even imaginable, let alone present anywhere in concrete experience? What language to use to describe an alienated language, what system of reference to appeal to when all systems of reference have been assimilated into the dominant system itself? How to see phenomena in the light of history, when that direction that history seemed to have and which gave it its meaning seems to have disappeared". This dilemma, according to Jameson, was, "...the source of the difficulty of Adorno's work and of his language" (Jameson 1967:40).

'tidiness', the Foundation could not see its way clear to extend such generosity. The Music Project was a clear loser in terms of its perceived utility. Yet, having said that, one does not wish to portray foundations as having a purely utilitarian approach to research. Certainly foundations in general are problem orientated, but at the same time are not blind to the dynamics of research, and its own internal demands. Neither are they entirely unaware that knowledge creation is often a long term process which if encouraged, can, in the end, lead to solutions of questions which they wish to reanswer. Thus, whilst not having been noted for their altruism they are prepared at times to support, work which for the want of a more appropriate term can be described as 'pure research'. However, such considerations did not operate in Adorno's case. The fact was, that the Music Project had produced very little that they considered to be of much practical value, nor from their point of view did it look as if this would be corrected in the future. This will shortly be documented, but first, it is important not to forget the worries which the Foundation had in relation to the Radio Research Project in general. As noted previously, upon taking outside advice, Marshall had been advised to try and restrict Lazarsfeld's wide-ranging intellectual interests. It is therefore argued, that the Music Project was the unfortunate inheritor of these much more general concerns, and that once set

in this wider context of operations it becomes easier to understand how the Music Project, which in many ways was the most unformulated aspect of the larger project, should fall victim to the Foundations appraisal. For example, in the December of 1939 following an interview which Marshall had with Lazarsfeld he wrote:

"... present feeling is that Lazarsfeld now should substantially reduce the scale of work. Second, J.M. felt that Lazarsfeld should reduce his commitments, attempting only some relatively modest programs whose accomplishment would still leave him free to follow up leads it develops. In other words, Lazarsfeld should avoid the heavy commitments which has made the completion of his present research difficult and the full exploitation of its findings virtually impossible" (Marshall 11:12:39).

Although these comments also refers to commitments external to the Project itself, the general attitude within the Foundation was that Lazarsfeld had over-extended himself, and should not undertake any further demanding work until the present research was given more form. Despite the fact that the Music Project could not be considered 'new' work, it was in such an embryonic stage that it was obvious that a great deal of effort would have to be expended if it was to fulfil Lazarsfeld's expectations of it, and, one might also add, the Foundation's expectations. Yet it would be wrong to merely consider the Music Project as falling victim to the wider general concerns with the Radio Project's overall progress. Since the critical nature of Adorno's work and his own behaviour, which

was not particularly engaging, made it a particularly obvious contender for the refusal of a continuation grant. Although its demise can only be fully explained by understanding the total patterning of events, it is to the more immediate concern over the nature of the work itself, which we now turn.

In an inordinately long letter of almost essay length Lazarsfeld wrote to John Marshall in December of 1939 attempting to justify and explain the music side of the Project in the following terms:

"I am quite aware that the entire approach of the music study is unorthodox and, as it were, a gamble. I have, however, become more and more optimistic about it as work progressed. It is a process of increasing returns ...

This is one of the reasons, I feel, that the study should not be dropped now. With a relatively small additional investment, a great wealth of information and ideas will be made available.

... But there are two strong reasons that the entire effort had to be undertaken. One is that music covers more than half of the available radio time. The other is a sort of moral responsibility. I feel that one just should not study so important a tool as radio without looking into its setting in the whole framework of our culture. Since I am aware that such an analysis might lead to somewhat controversial results<sup>1</sup> it seemed best to make the experiment in the field of music, which is least exposed to public distrust. Besides the great intellectual expectations I attach to Dr. Adorno's work, I should feel that the project had failed its major task if nowhere in its work were a social critique attempted" (Lazarsfeld 27:12:39).

The importance which Lazarsfeld obviously attached to the necessity of a 'social critique', stemmed from his socialist background.

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1      Emphasis mine.

Furthermore, his Viennese socialist training had left him well equipped to appreciate the strengths of Adorno's social insight, and his intellectual depth. Yet, Lazarsfeld, whilst not skirting the issue was pragmatic enough to recognise the possible controversies which such an analysis might generate, and to note the suitability of music as an appropriate entry point for a social critique. Indeed as far as the Project Directors were concerned, the main advantage of music as a research focus<sup>W/K</sup> <sub>A</sub> its 'safety'.

For example Cantril wrote:

"The general work of the Office would not be well balanced if the effect of radio upon social groups alone is studied. It is reasonable to assume that the social system determines what radio does and can do. This leads to rather precarious questions, but at least they should be opened to discussion. The Directors, aware of their responsibilities (for reasons of tact as well as scientific integrity) have selected the field of music for the investigation of the influence of the social system upon today's broadcasting" (Cantril 7:3:39).

The above is an extract from a proposal to continue the Princeton Radio Research Project, yet, a few months later, Lazarsfeld wrote a somewhat similar continuation proposal, and once more expressed the need for a more total approach to radio, but again, showed recognition of likely controversy which he considered could be mitigated by focussing upon music:

"The second and final project exhausting the material of the past two years of the Radio Research Project is the carrying to completion as a published book of the studies already made under the Music Project. This study has

been a bold but difficult attempt to formulate the impact of American socio-economic institutions upon the upward of fifty percent of radio time devoted to music. It is obviously undesirable to omit entirely from a study of a privately owned medium of communication in a democracy the pervasive factor of what such private control does to radio broadcasting and listening. Since this is a delicate matter, the first section of the Radio Research Project selected music as the mildest and least explosive area of broadcasting. The general theoretical approach has been formulated and five specific aspects have been studied and are in tentatively final form. The joining of the segments and the final preparation of the book as a coherent whole remains to be done" (Lazarsfeld 27:12:39).

There is evidence to suggest that this book was to be written by Adorno in collaboration with George Simpson, but, owing to the non-continuation of the Music Project, was never forthcoming. Between 1938 and 1940 Adorno, with the aid of Simpson produced four music papers but not all were published.<sup>1</sup> Simpson was thoroughly familiar with European scholarship, and according to Adorno, "encouraged me to write as radically and uncompromisingly as possible" (Adorno 1969:351). However, as helpful as Simpson was productively, the resulting 'radical' content met with little favour from certain quarters. For example, two letters exist in the Rockefeller Archives which were sent to John Marshall as commentaries upon a paper which had been prepared by Adorno, and Simpson entitled, On A Social Critique of Radio Music. This

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1 These four papers were 'A Social Critique of Radio Music' which appeared in the Kenyon Review 1945, 'On Popular Music', printed in Studies in Philosophy and Social Science 1941, 'The Radio Symphony' published in Radio Research 1941 and finally an unpublished paper of A Study of NBC's Music Appreciation Hour.

was read by Adorno at a Princeton Radio Research Project seminar on October 26 1939, and not as Adorno notes, in 1940 -- a mistake which now appears to be gathering historical momentum.<sup>1</sup> This paper, or a derivative of it, subsequently appeared as a published article in 1945,<sup>2</sup> however, the original paper prompted W.G. Preston Jr., Assistant to the Vice President in Charge of Programs at N.B.C., to send a most dismissive letter to the Foundation:

"I thought you might be interested in the following comment made by one of our music experts, after reading the mimeographed paper.

Since our telephone conversation of last week regarding the paper ... I have given further study to the report and have reached the conclusion that detailed criticism of it would be superfluous.

The paper is so full of factual errors and colored opinions, and its pretense at scientific procedure is so absurd in view of its numerous arbitrary assertions, that it is hardly worthy of serious consideration, except possibly as propoganda. In short, it seems to have an axdom to grind.

I pass this on to you for what it may be worth, if anything" (Preston 18:12:39).

Although Adorno's paper On a Social Critique of Radio Music had angered the "music expert" at N.B.C., the real problem, whilst admitting that such adverse comments played some part in shaping the Project's future, was the difficulty of knowing how to handle

1 Adorno 1969:351 for original mistake. Jay 1973:191 reproduces it as well as Pauling 1974:214.

2 The paper mentioned in the above footnote: A Social Critique of Radio Music.

such work --- what was its value? To be sure, to a person of Adorno's persuasion such thinking would no doubt be evidence of a reified mind, yet the Project could not escape the fact that such questions were being asked of it, and since it had not moved rapidly enough in the direction which Lazarsfeld had hoped for, no satisfactory answer could be provided. Yet John Marshall did make strenuous efforts to come to terms with what Adorno was saying. Indeed he regarded it as exciting work which fell broadly within a humanistic tradition to which he was favourably disposed. As he later put it;"(Paul Lazarsfeld) felt that the enquiry so far had been basically too sociological and it should also be humane and artistic, And I said I couldn't agree more. Then he brought Adorno into the picture and I was impressed" (Marshall 6:7:73).

There is sufficient material in the Rockefeller Archives to collaborate Marshall's statement that he was impressed by the originality of Adorno's approach to music, but paradoxically it was this very originality which presented the major problem for the Foundation. For in many ways there was an absence in Adorno's work of what the Foundation officials defined as "evidence", and this was seen to severely undermine its practical usefulness. For example, Geoffrey Gorer, a British anthropologist who had worked closely with the Foundation in respect to mass communication research, being one of the members of the communication

seminars, wrote a commentary for Marshall on one of Adorno's papers.<sup>1</sup> Marshall respected Gorer's academic opinion, and indeed the commentary is of a total different order to that received from Preston of N.B.C., being both thoughtful and constructive.

He wrote:

"The content of the paper really falls into two categories: 1) the reaction of an extremely sensitive person to the sound of transmitted music, and 2) the hypothesis about how an appreciative public reacts to this radio music. The first part I am not competent to judge, but I am willing to believe that Dr. Adorno's statements about the deformation produced by the technique of radio are justified. All the way through he assumes that, and other undesirable forms of listening are a strictly modern phenomenon dependent on radio transmission, with the underlying assumption that up until recent years all listeners of music listened with the intensity and comprehension which he considers desirable. Beyond that, that up until the middle of the 19th century composers were subsidised by aristocrats as a means of obtaining prestige. I see no evidence to presume that sympathetic listening was statistically more intense then than now, although fewer people were exposed to the experience.... Consequently, all the arguments about the degeneration of listening seems to me to be founded on the unproven hypothesis that earlier listening was of a more intensive character. I do not know of any evidence of this....

The statement on page 6 that music today is not an art but a commodity seems to me quite meaningless. Musicians have always had to eat!

... To sum up, I think Dr. Adorno has presented some very interesting hypotheses which might be verified by future research. I do not think he had succeeded in proving anything or in justifying his interpretive method of social critique. I have frequently maintained that in any social situation it is necessary to know both the stimulus and responses. As a corrective to the bias of the Princeton Radio Research, it is useful to have some analysis on the stimuli, but this only becomes meaningful

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1 The paper Gorer commented on would appear to be the original On A Social Critique of Radio Music.

when and if it is collated with specific responses"  
(Gorer 2:1:40).

However, despite the thoughtful nature of Gorer's memorandum, it shows clear failure to understand Adorno's approach. Nevertheless, Adorno later conceded some of Gorer's basic points, and admitted that:

"It is an open question, to be answered only empirically, whether and to what extent the social implications observed in the content analysis of music are understood by the listeners themselves, and how they react to them. It would be naïve to take for granted an identity between the social implications to be discerned in the stimuli and those embodied in the "responses". It would certainly be no less naïve to consider the two things as totally uncorrelated with each other in the absence of conclusive research on the reactions. If in fact, as was deduced in the study "on Popular Music", the standards and rules of the popular music industry are the congealed results of public preferences in a society not yet fully standardised and technologically organised, one can still conclude that the implications of the objective contents do not completely diverge from the conscious and unconscious awareness of those to whom they appeal -- otherwise the popular would hardly be popular" (Adorno 1969:353).

Thus, whilst Adorno himself would have appreciated reliable empirical evidence on listener response, the fact was, that they did not exist for a past age, and in the present age he was not convinced that it was at all possible to obtain them. Yet the recognition that both 'sides of the fence' ought to be examined only presented him with the paradox that by attempting to grasp the phenomena of cultural reification one had to use the existing reified instruments. Given this impossible intellectual position, then

the only way out for Adorno was to concentrate on the cultural product itself, not perhaps the 'best' method of procedure, but the most appropriate in the prevailing circumstances. As far as Adorno was concerned however, it was not necessary to study listeners' responses directly in order to be able to say something about them, since such responses were partly contained within the music itself, and under the conditions in which it had been reproduced. For example, in his work "On Popular Music" Adorno writes:

"Listening to popular music is manipulated not only by its promoters, but as it were, by the inhere[n]ts of this music itself, into a system of response mechanisms wholly antagonistic to the ideal of individuality in a free, liberal society. This has nothing to do with simplicity and complexity. In serious music, each musical element, even the simplest one, is "itself", and the more highly organised the work is, the less possibility there is of substitution among the details. In hit music, however, the structure underlying the piece is abstract, existing independent of the specific source of the music. This is basic to the illusion that certain complex harmonies are more easily understandable in popular music than the same harmonies in serious music. For the complicated in popular music never functions as "itself" but only as a disguise or embellishment behind which the scheme can always be perceived....

... Popular music, however, is composed in such a way that the process of translation of the unique into the norm is already planned and to a certain extent achieved within the composition itself.

The composition hears for the listener. This is how popular music divests the listener of his spontaneity and promotes conditioned reflexes. Not only does it not require his effort to follow its concrete stream; it actually gives him models under which anything concrete still remaining may be subsumed. The schematic build-up

dictates the way in which he must listen while at the same time, it makes any effort in listening unnecessary. Popular music is "pre-digested" in a way strongly resembling the fad of "digests" of printed material". (Adorno 1941:21,22).

However as far as "serious music" is concerned, or rather "good serious music", then according to Adorno, the listener cannot supply the framework automatically since, "every detail derives its musical sense from the concrete totality of the piece which, in turn, consists of the life relationship of the details and never of a mere enforcement of a musical scheme" (Adorno 1941:19). Consequently, the music is not "pre-digested"; "no such mechanical substitution by stereotyped patterns is possible in serious music. Here even the simplest event necessitates an effort to grasp it immediately instead of summarizing it vaguely according to institutionalised prescriptions capable of producing only institutionalized effects. Otherwise the music is not "understood". (Adorno 1941:22)

Given such analysis it was possible for Adorno to take the objective musical structure, and say something about its receptivity in the past, and in the present without necessarily 'knowing' anything about listeners' responses. However, notwithstanding the preceding comments on the structure of "good serious music", Adorno considered that given the new medium of its transmission, an alteration or distortion had occurred so that it could no longer be considered the same music. Furthermore, the way in which the music was

'packaged' by the radio stations, quite apart from technical reproductive faults, had turned it into a commodity. On this particular point it becomes increasingly evident that Gorer did not follow <sup>his</sup> argument failing completely to understand the notion of fetishism. One can only presume that such a concept was alien to him, since it would appear to be not simply a question of an honest intellectual disagreement as to whether Adorno was correct or not, but a failure to understand; as illustrated by his comment that: "the statement on page 6 that music today is not an art but a commodity seems to me quite meaningless. Musicians have always had to eat!" Adorno makes it perfectly clear in his paper<sup>1</sup> what he meant by the commodity nature of music, and in this particular case he cannot even be accused of obscurity:

"Bach in his day was considered, and considered himself, an artisan, although his music functioned as art. Today music is considered ethereal and sublime, although it actually functions as a commodity. Today the terms ethereal and sublime have become trademarks. Music has become a means instead of an end, a fetish. That is to say, music has ceased to be a human force and is consumed like other consumers' goods. This produces "commodity listening", a listening whose ideal it is to dispense as far as possible with any effort on the part of the recipient --- even if such an effort on the part of the recipient is the necessary condition of grasping the sense of the music. It is the ideal of the Aunt Jemima's ready-mix for pancakes extended to the field of music. The listener suspends all intellectual activity when dealing with music and is content with consuming and evaluating its gustatory qualities -- just as if the music which tasted best were also the best music possible. (Adorno 1945:211)

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1 The published paper quoted below, is a shortened version of the original paper which Gorer commented on. Since there is no substantive difference it can, for the purposes at hand, be regarded as the same paper.

As if such an explanation was not sufficient to make his point, Adorno proceeded to give "a drastic illustration" of musical fetishism through the example of musical instruments, and the attendant value placed upon them; stating, "Whereas only the expert is able to distinguish a 'Strad' from a good modern fiddle, and whereas he is often the least preoccupied with the tone quality of the fiddles, the layman is induced to treat these instruments as commodities, gives them a disproportionate attention and even a sort of adoration". (Adorno 1945:211) Evidently one radio company went to the extreme in fostering this process by arranging a cycle of broadcasts which concentrated not on the music played or even the performance, "but what might be called an acoustic exhibition of famous instruments such as Paganini's violin and Chopin's piano". The fact that such a commodity attitude was passed off as culture and erudition was a point of central concern in his paper.

Whilst the above discussion of the comments made upon Adorno's work has been used to highlight misinterpretations at the intellectual level, such a discussion also serves the function of allowing an insight into his work and of showing its inappropriateness in the context of administrative research. Indeed, the inter-office correspondence of the Rockefeller Foundation shows their own fine appreciation of such a point. Even allowing for the

misunderstandings that may have arisen on the part of the Foundation officials through consultation with individuals who do not appear to have understood the contents of Adorno's work, the fact remains, that the style of work was not to their administrative liking. It is true that favourable 'letters' were written to the Foundation, such as one by Charles Siepmann, in which he stated that Adorno, "has an intellectual grasp that seems to me to be rare and a very salutary good to those who deal in a kind of pseudo-intellectualising which is liable to make popularization something dangerous". (Siepmann 12:12:1940) Nevertheless the decision taken by the Foundation was that the Music Project would not continue.

Shortly after receiving a letter from Provost Fackenthal on December 22 of 1939, outlining the research which Lazarsfeld wanted to undertake at Columbia, Marshall contacted both Lynd and Lazarsfeld to discuss the whole matter with them. During the course of the discussion Marshall, "reported his tentative view"; that no further provision could be considered for Dr. Adorno's study of music broadcasting on the grounds that provision made in the Foundation's last grant to Princeton University presumed the completion of those studies by March 1st." However, the Foundation had always presumed the completion of studies, and as with much research, completion dates are not always met; it may even be said, without wishing to extend the point too far, that the

non-fulfilment of research deadlines is an almost accepted rule of the 'granting game', or at least one that is usually catered for. Consequently, Lazarsfeld with seeming diplomatic skill expressed his regret at Marshall's view, with the result that Marshall agreed to review the situation, "on the grounds that Lazarsfeld's belief in the value of Adorno's work might justify retreating from what Lazarsfeld agreed to be a perfectly justifiable administrative position" (Emphasis mine). Indeed, Marshall did review the situation, and as part of that review read all of Adorno's material which had been produced whilst working on the Music Project, but:

"This review left JM still much engaged by the originality of Adorno's approach. Certainly this approach has many novel features, and in many respects promises a view of the social significance of broadcast music beyond anything JM has seen. But JM's final conclusion may be summarized as follows:

The real issue is the utility of the study, and that utility must be measured by the effect which can be anticipated for it in remedying the present deficiencies of broadcast music." (Emphasis mine)

Furthermore, Marshall had certain reservations about the style of Adorno's writing, to the extent of questioning the mainspring of his motivations:

"Adorno's present critique, just as it undoubtedly is in many of its adverse comments, is written in a tone which stresses the present deficiencies of music broadcasting to a degree that would be bound to put all those responsible for it definitely on the defensive, with the probable result that they would be left more inclined to rationalize those deficiencies than attempt any remedy for them. . . .

Adorno's present work has been done with the collaboration of a presumably competent editor, but this collaboration patently has not resulted in the kind of statement which would have remedial utility.

This leads JM to believe that Adorno at present could prepare a useful statement only if he had the collaboration of someone representative of the present system, but tolerant enough of Adorno's position to see what was useful in it and interpret that for people certain to be less tolerant. Finally, the tone of Adorno's paper leaves some room for doubt that Adorno would be able at present to collaborate in any such way. He seems psychologically engaged at the moment by his ability to recognise deficiencies in the broadcasting of music to an extent that makes questionable his own drive to find ways of remedying them". (Marshall 5:1:40)

To be sure, Lazarsfeld had repeatedly told Adorno to moderate his language, but in this instance it is difficult to say, since Marshall does not point to any particular instances, whether he was actually objecting to the 'style of writing', or to its over-all critical thrust. In fact the latter seems more likely since Marshall went on to suggest that: "If Adorno's work is to have the utility which JM would ask of it, it would have to undergo pretty complete reformulation". But it remains difficult to see how such a shift was possible without blunting the critical cutting edge of Adorno's thinking. For, his critique of music was not simply a musical critique, but as he rightly calls one of his papers, "A Social Critique"; that is, a critique of the society within which such music was produced and made possible:

"In our present society the masses themselves are kneaded by the same mode of production as the artcraft material foisted

upon them. The customers of musical entertainment are themselves objects or indeed, products of the same mechanisms which determine the production of popular music. Their spare time serves only to reproduce their working capacity. It is a means instead of an end. The power of the process of production extends over the time intervals which on the surface appear to be "free". They want standardised goods and pseudo individualisation, because their leisure is an escape from work and at the same time is moulded after those psychological attitudes to which their workaday world exclusively habitates them. Popular music is for the masses a perpetual "busman's holiday". Thus there is justification for speaking of a pre-established harmony today between production and consumption of pop music. The people clamour for what they are going to get anyhow.

To escape boredom and avoid effort are incompatible - hence the reproduction of the very attitude from which escape is sought. To be sure, the way in which they must work on the assembly line, in the factory, or at office machines denies people any novelty. They seek novelty, but the strain and boredom associated with actual work leads to avoidance of effort in that leisure time which offers the only chance for really new experience. As a substitute they crave a stimulant. Popular music comes to offer it." (Adorno 1941:39)

Such a line of argument presents a total critique, and hence it is impossible to extract parts from it without violating the analysis as a whole. However, this is not to say that one had to wait idly by for a total societal restructuring in order to secure improvements or aid the progressive forces in the cultural superstructure. Adorno considered that the 'vulgar Marxists' were wrong to see cultural phenomena as entirely determined by their socio-economic base, but then neither were they taken to be fully autonomous. Rather, they had to be viewed, "as mediated through the social totality, not merely as the reflection of class interests",<sup>1</sup>

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1 Jay 1973:54. For this quote, but see Chapter two in general for a broader discussion of the Frankfurt School's position in relation to this.

and by extension the economic social organisation of the society.

For as Martin Jay states, in his article on the Frankfurt School's

"Critique of Marxist Humanism":

"By 'Negative Dialektik' his last major work, Adorno was expressly critical of the way in which Marx's early writings had been used ideologically by humanists like Fromm. Although they did not deny the special role of the economy, and by extension the labor process, in capitalist society, they never de-historicized labor into man's "ontological" activity. Accordingly they were reluctant to accept the merely reflective character of the cultural superstructure posited by more orthodox Marxists. While never minimising the important influence of socio-economic factors on the cultural phenomena, Horkheimer and Adorno always avoided reducing the latter to a mere epiphenomena of the former." (Jay 1972:295)

Late on in his life Adorno expressed the cryptic comment that,

"Marx wanted to turn the whole world into a giant workhouse "

(Jay 1973:57), and he considered it necessary for man to approach self-realisation through cultural activity as well as through work.

However, the existing conditions of American society, presented a depressing prospect for such possibilities. Although clearly he

knew, "what monopolistic capitalism and the great trusts were",

(Adorno 1969:340) he had not, before his arrival in America, realised how far rationalisation and standardisation had permeated certain

cultural forms - namely popular music. It appeared to him that the

harmonising of the individual with technological society had reached such proportions that most people thought in rigid and pre-conceived

categories so that they were no longer capable of spontaneous

experience. In the case of popular music, he saw the general tendency of society, its thrust towards uniformity, as having robbed many individuals of their ability to invest such music with their own feelings. Whilst he recognised that such a situation was not universal and that young people were still capable of investing popular music with meaning;<sup>1</sup> the radio industry itself, with its monotonous and repetitive 'plugging' fitted well the rhythms of the industrial process and undermined the possibility of such 'meaning'. That is, the boredom produced by the work situation, which then resulted in a craving for excitement during their non-working hours - the time which was the only area remaining for freedom - erected a machinery for distraction based upon intermittent attention:

"... the industry faces an insoluble problem. It must arouse attention by means of ever-new products, but this attention spells their doom. If no attention is given to the song it cannot be sold; if attention is paid to it, there is always the possibility that people will no longer accept it, because they know it too well. This partly accounts for the constantly renewed efforts to sweep the market with new products, to hound them to their graves; then to repeat the infanticidal manoeuvre again and again.

On the other hand, distraction is not only a pre-supposition but also a product of popular music. The tunes themselves lull the listener to inattention. They tell him not to worry for he will not miss anything "  
(Adorno 1941:39)

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1 For example Adorno (1941:39) provides the footnote that the process was not a completely universal one, stating that; "particularly youngsters who invest popular music with their own feelings are not yet completely blunted to all its effects."

As much as Adorno objected to the existing state of Radio Music, complaining that, "... music under present radio auspices serves to keep listeners from criticising social realities; in short it has a soporific effect upon social consciousness" (Adorno 1945:212). The question remained as to how exactly to alter such an arrangement.

At this point, one returns to John Marshall's doubts concerning the 'utility' of Adorno's work and their fundamental disagreement as to what would constitute 'utility'. For, although both were agreed about the necessity for changes in the content of the available output with regard to the means by which such changes could be brought about, there was little meeting ground. Adorno questioned the whole supposition upon which the question, "How can good music be brought to as many people as possible?" was based. His approach was to, "Abandon the form of question indicated by a sentence like: How can we, under given conditions, best further certain aims? On the contrary, this approach in some cases questions the aims and in all cases (my emphasis) the successful accomplishment of these aims under the given conditions".

(Adorno 1945:208, 209). Thus Adorno further states:

"These questions have arisen out of the consideration of so simple a phrase as 'bringing good music to as large an audience as possible'. None of these or similar questions can be wholly solved in terms of even the most benevolent research of the administrative

type.<sup>1</sup> One should not study the attitude of listeners, without considering how far these attitudes reflect broader social behaviour patterns, and even more, how far they are conditioned by the behaviour patterns and, even more, how far they are conditioned by the structure of society as a whole. This leads directly to the problem of a social critique of radio music, that of discovering its social position and function". (Adorno 1945:210)

Such an analysis inevitably points to revolutionary praxis as the way out: either through changes in the substructure itself, or alterations in the cultural superstructure along more progressive directions. The prospect for changes at the superstructural level, given the absence of a mobilised mass, looked bleak, yet that did not deny the possibility of 'subverting' the media industry itself. In Adorno's own terms, was it not possible to overcome some of the 'faults' which he perceived as acting as a brake on self-realisation? Thus, it was not simply a question of transmitting 'good' music; that is, music considered good by social convention, which had ceased to be the living force it once was, but of making available music that possessed the capacity to break down rigid, and pre-conceived ways of experiencing the world. However, this seems to be the fundamental difficulty of critical theory: synthesising theory with praxis, and handling praxis in any other than a theoretical

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1 This is clearly a criticism based on his experiences whilst working on the Radio Project. Indeed, he names Lazarsfeld in person as the coinor of the phrase "benevolent administrative research". However, he does differentiate between exploitative administrative research and benevolent - see Adorno 1945:208.

manner. To a large extent this must be considered one of its major shortcomings if judged within its own terms. Yet ironically, at the very same time that the decision was being taken to discontinue Adorno's work there appeared a recommendation made by John Marshall which, if acted upon, may well have forced Adorno to confront this question at a very practical level. Whether he would have been successful remains an open question. The recommendation suggested that he should write:

"... a critique of the present situation dealing with radio's deficiencies in terms of difficulties which Adorno recognizes would be encountered in giving any such program as he proposes effect; in other words, Adorno could in this way make his adverse criticism, and in a sense challenge those responsible for music broadcasting to meet difficulties sure to stand in their way". (Marshall 5:1:40)

As matters transpired, Adorno did not have any opportunity to act upon such a suggestion, for in the very same memorandum it is noted that Marshall reported to Lazarsfeld that he could not make any, "further recommendation provision for Adorno's going on with his work at present". Lazarsfeld was, according to Marshall, "plainly embarrassed by the decision", but as Marshall saw it such a position could not be helped. Marshall's only suggestion was that "Adorno's research papers be filed with the Project materials and so kept available for possible future

development" (Marshall 5:1:40). Yet on the question of Marshall's suggestion, it must be admitted that Adorno had always had the opportunity to produce such a statement, and never did. Had Adorno drafted a practical programme then in every likelihood both the Foundation and the Radio industry would have refused it; especially since the critique alone had already met with scant regard from certain quarters. But even at the last moment of the Music Project's history, a certain lingering hope was held out that it might be rescued by a kind of salvaging operation. Lazarsfeld, was not content to let Marshall's decision stand, and once more attempted to obtain a small continuation grant for Adorno; he wrote to John Marshall on the 9th June 1941, "If you could see your way clear to give Dr. Adorno a grant of, say, 3,000 dollars, then I think that, from our Consulting Account, I could give him enough research assistance so that the musical part of our office could be tied together and a summary publication, worth all the effort could be brought out." (Lazarsfeld 9:6:41) Quite apart from illustrating Lazarsfeld's desire to retain Adorno, the suggestion of transferring funds from the 'Consulting Account' illustrates his own adroitness as a Bureau Director. How irregular such a practice was is difficult to say, but it was his common practice to ignore 'correct' procedures when he considered they hindered

more important matters. The very persistence with which Lazarsfeld pressed Adorno's case provides a fine example of one of the factors making for his success as an institutional innovator. Only eight days after the above letter, he wrote Marshall once again, stating: "By good luck I can add another argument to my proposal regarding Adorno". The "good luck" was that Virgil Thomson of the Herald Tribune had devoted the entire music column of the paper to a review of Adorno's article 'On Popular Music'. Thomson, who Marshall respected as, "both a hard boiled and competent critic", (Marshall 19:6:41) had reviewed the article particularly favourably stating, "I recommend (it) to the serious attention of musicians. I append a few choice bits below, not as any digest of the study's substance, but as a come on for those who might be induced thereby to procure and read the whole" (Thomson 15:6:41) Lazarsfeld, in this final effort to persuade Marshall to reconsider his decision on Adorno xeroxed Thomson's review, and attached it to his letter, the last paragraph of which interestingly reads:

"I have had repeated discussions with Dr. Adorno about how the psychology of the average person might have changed as a result of living under modern industrial conditions. This problem is customarily seen only as an economic one. Not enough attention has been paid to the fact that as a result of the change in role of knowledge and free choice, all efforts at mass education need to be rescrutinised from the aspect of recent social trends. If Adorno were able to spend some time on

thinking these things through in close contact with myself and others of my staff who now know so much about what is going on in radio, something quite remarkable might turn up in the end".

(Lazarsfeld 17:6:41)

Two days after this letter Adorno, possibly with Lazarsfeld, went to the Foundation's office to discuss the matter with Marshall. Marshall, "tried to explain as tactfully as possible to Adorno the administrative reasons which prevented a consideration of Lazarsfeld's request for a further grant in aid to enable Adorno to continue his work. The reasons given were that there was an urgent need for funds from the limited amounts available for grants in aid, especially in view of their support for refugees. In addition to which, "the last grant made to Lazarsfeld was understood to be the total he would have available for radio research". How Adorno accepted this "tactful" refusal cannot be said with certainty, but no doubt, found part of its expression in his own less than tactful comment, "that the Project was at the whims of a young ignoramus". However, Marshall was not unsympathetic and appears at the last moment to have nurtured regrets about his decision, noting in a memo that, "the grounds given Adorno undoubtedly justify declining to consider further aid, but this left JM rather unhappy about the decision. He is convinced now (my emphasis) that Adorno's work has substantial value and this view is fully confirmed by Thomson". What particularly disturbed

Marshall was that although Adorno could earn his living by undertaking work with the Horkheimer Institute to which he was attached, "these studies which he began with Lazarsfeld and which he is at present most keenly interested in do not come within the Institute's program. Hence he cannot complete them unless he finds some further support". In consideration of such a situation and the fact that Adorno, "clearly has something to say about the social position of music in this country", Marshall attempted to use his contacts to obtain money for him from the Carnegie Corporation. Furthermore, if no money was forthcoming from that, or other sources, then Marshall was even prepared to approach David Stevens, his superior, to consider the possibility of a smaller grant in aid than Lazarsfeld had originally asked for -- perhaps 1,000 or 1,500 Dollars.<sup>1</sup> Despite these last minuted hopes however, Adorno's collaboration with Lazarsfeld was over. No money was forthcoming, and when Horkheimer, following the deterioration of his health, moved in the early part of 1941 to the more temperate climate of California, Adorno accompanied him giving a physical completeness to the intellectual break.

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1 All quotes on this page taken from the interview between Marshall and Adorno. As suggested, there is reason to believe that Lazarsfeld was also present at the interview. As usual Marshall recorded the interview for circulation as an office memo. (Marshall 19:6:41)

### Lazarsfeld and Adorno: The Research Context in Perspective

This chapter has analysed the context within which the Radio Project operated, concentrating in particular on the impact which the Foundation's interference had upon it, and by extension, on mass communication research in general. Yet, whereas such 'interference' could be considered beneficial in terms of the Radio Project's overall development, it patently was not so in the case of the Music Study. Leaving aside much of the detail, and moving on to consider the central issue of 'Critical Research' vis a vis 'Administrative Research', the writer will not attempt to make some general observations. Although there is no intention of departing from the central tenet of the chapter, that in the end 'critical research' was inappropriate to the structural setting of administrative research, the skeleton of this argument must be fleshed out a little more.

The difficulty of integrating empirical techniques with a truly critical approach which stressed the primacy of theory should not be underestimated. Martin Jay in reflecting upon Adorno's achievement whilst working with the 'Berkeley Group' states:

"... this was more than merely a methodological dilemma; it reflected real divisions and contradictions within the society as a whole. The success of the "Studies in Prejudice", it might be argued, had resulted in part from an avoidance of the issue. The analysis of anti-semitism in 'The Authoritarian Personality' and in the

'Elements of Anti-Semitism' -- the one dealing with the subjective dimension, the other more with its objective side -- were never really reconciled. In fact, one reason why the Berkeley project succeeded while Adorno's collaboration with Lazarsfeld was a failure was that the former did not concern itself with the 'objective spirit' of modern society in the way in which the latter did." (Jay 1973:251,252)

However, it has already been mentioned that Lazarsfeld's problem was not only to <sup>MARRY</sup>critical theory with empirical research, but to then translate the results into policy recommendations. That was what was demanded within the context of the research. The fact that Adorno failed at the first task by not producing any empirical evidence severely prejudiced his case from the beginning, since, within its administrative setting only empirical 'findings' were acceptable as evidence and everything else was taken as no more than "expert opinion" -- useful but restricted in its power of argument. However, his failure on that score, whilst undermining the perceived 'practicality' of his work, did not completely destroy the possibility of some "applicability" within his own terms, and perhaps, the Foundation's also. 'Critical theory' never sought the 'truth' for its own sake, as something valuable in itself, but rather as a means for affecting social change. Their implicit goal being a "synthesis of contradictions in a socialized society which had motivated Marxists of all kinds for generations" (Jay 1972:344). Yet the difference between them, and the

'traditional Marxists' belief, that the proletariat was the "universal class" where victory would end the contradictions of capitalist society, was their pessimism that the historical moment had passed, and the opportunities lost. Hence, "with the increasing absorption of social and political forms of negation, the Frankfurt School began to focus its hopes on cultural phenomena" (Jay 1972:345). Yet, even in that realm of hope their prognosis was marked by pessimism. In fact the search for a revolutionary subject, and the failure to integrate theory and praxis at no more than a theoretical level leaves one with the distinct impression that 'critical intellectuals' were the revolutionary subject, and that genuine critical theory became a kind of praxis in itself.<sup>1</sup> However having stated this last point, the ground for arguing that the failure of the Music Project stemmed from its inappropriateness for administrative research, begins to fall away, but not completely. For the facts of the case are, that John Marshall in his role as administrative agent of the Foundation did, despite a personal attraction to Adorno's work, refuse to recommend it for a further

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1 Phil Slater commentating on the Frankfurt School and its failure to specify the precise relationship between correct consciousness, and the most 'progressive social forces', states, "It is unclear whether the 'subjects of the critical position' are the political avant-garde or the critical intellectuals. (Slater 1974:177) For a further discussion of this difficult question see Slater 176-179.

continuation of funds. The reason being, that instead of a social critique which he couldn't use, he wanted policy suggestions which he could recommend as a corrective to the existing broadcasting of music.

However, at this juncture a caveat must be added. It needs stressing that despite the lack of any empirical basis to Adorno's work all was not lost, and it is here that the responsibility for the Music Project's failure to gain further funds shifts, to a certain extent away from the administrative setting, and on to Adorno himself. For, if the historical agent of change could no longer be easily identified, that is, if the structural contradictions of capitalism had ceased to be of sufficient centrality to bring about a 'jumping out of progress'<sup>1</sup> then perhaps the possibility of change remained in the cultural sphere. But faced with the opportunity to demonstrate the unity of theory and praxis within that sphere he failed. Consequently Marshall received nothing from him upon which to judge his 'policy', and come to a decision one way or the other. What therefore remained was a social critique of music at a theoretical level, and that certainly did not fit with administrative demands. Consequently, the conclusion to the episode was predictable: there was little Lazarsfeld or anybody else engaged in the Radio Project itself could do, to alter the decision to refuse it further funds.

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1 See Jay 1972:348 for the use of this term.

## Chapter Six

### The Bureau of Applied Social Research

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The Bureau of Applied Social Research.

The Lessons of Institutionalisation

Reference has already been made to the institutional reforms which have historically accompanied innovation in higher education, as well as noting the attendant benefits for the preservation of work through firm institutional structures. However, this now needs to be expanded further, and will be introduced by considering the case of the Institut für Sozialforschung, the thesis being that the Frankfurt School's work would not have survived, and indeed expanded, had it not been for their institutional organisation.

Edward Shils in commenting upon institutionalisation and the part that organisation plays in the transmission and acceptance of ideas notes:

"Institutionalization is not a guarantee of truthfulness: it only renders more probable the consolidation, elaboration, and diffusion of a set of ideas. It is not the sole determinant of the acceptance or diffusion of ideas. Intellectual persuasiveness, appropriateness to "interesting" problems, correspondence with certain prior dispositions and patterns of thought of the potential recipient are also very significant. Institutionalisation serves however to make ideas more available to potential recipients, it renders possible concentration of effort on them, it fosters interaction about them, and it aids their communication. Insofar as it offers the possibility of a professional career in the cultivation of the particular intellectual activity, it

both makes possible the continuity of exertion on a full-time basis and it adds a further motivation for its performance. The existence of a practical or executive professions, which require the study of an intellectual discipline as a qualification and as a constituent of professional practice, provides a student body and teaching opportunities - and therewith research opportunities which develop in the interstices of teaching. In these ways, institutionalization makes a difference to the fate of ideas." (Shils 1970:777)

Certain of the above points will be specifically referred to later, since they relate more to the operational position of the 'Bureau' than they do to Horkheimer's Institute. However, the importance which Shils attaches to the role of institutionalisation in the acceptance of ideas is clearly evident from the above. Interestingly, he compares the fate of Karl Mannheim's ideas with those of Max Horkheimer considering that whilst Mannheim has had little lasting influence whilst "Horkheimer is in a certain sense one of the most influential of modern theorists". (Shils 1970:773) In large measure Shils accounts for this disparity in acceptance by reference to institutional establishment. Whereas for Shils,<sup>1</sup> Mannheim, "was the more original and many sided of the two", the fact was that he failed to establish any institutional provision for the transmission of his work. Furthermore the London School of Economics, to which he moved in 1933 upon leaving Germany,

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1 Shils was always an outspoken critic of the Frankfurt School and some of his following comments are obviously coloured by his dislike. However, this does not detract from the points he makes.

lacked the necessary institutional arrangements for such purposes, since at that time it had very few post-graduate students of sociology, in addition to which, there was no "institutional provision for the organisation, support, and supervision of research in sociology". (Shils 1970:774) The case of Emile Durkheim is also pertinent at this point for although it could be argued, as indeed Shils does, that Durkheim, the only one of the "founders" of sociology who institutionalised his work during his own lifetime, he did so, not so much through the provisions of the French university system, but rather through his own organisational efforts in establishing the Année Sociologique. However, even then, whilst the Année Sociologique aided the establishment of Durkheim's work it was not sufficient in itself to keep his work alive; for as Shils comments further:

"It is often said that the death in battle of some of the most distinguished of the younger collaborators of the Année Sociologique was the cause of the cessation of the Durkheim outlook in French sociology, and there is some truth in this. I think, however, that the more important was the fact that the institutional structure built by Durkheim rested only on him and was not integrated into the institutional structure of the French university system."  
(Shils 1970:767)

Whilst not wishing to overlook the multitudinous factors combining to aid the establishing of one set of ideas, or style of work, over others, (which requires in any given instance a detailed empirical examination) a journal remains of undoubted importance in such a

process. Yet, as the above example shows, the firm establishment of ideas is neither guaranteed nor vouchsafed by a journal alone. Indeed, it can be considered that a journal without an institutional base detracts from its establishing power.

Admittedly, a journal with a distinct and recognisable framework of ideas may well have its content taken up, and taken over by some body of scholars who do have the appropriate institutional anchorage, but it must remain in doubt how far ideas 'on their own' can sustain themselves without such a base.

Thus, to return to the position of Horkheimer vis a vis Mannheim. Horkheimer had both an identifiable journal and an institutional base whereas Mannheim had neither. Yet even then, whatever success the Frankfurt School have managed in the reception of their ideas, owes more in the long run to their institutional base than the journal itself.<sup>1</sup> No doubt, the individual works of some of the members would have stood reading independently of institutional association, but the fact is that much of the 'Schools' output is in the form of articles,

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1 For example, for most of its history the journal appeared in German only. Vol. 9 (1941) of "Studies in Philosophy and Social Science" appeared completely in English. Volume 8 (1934) of the journal under its old title of *Zeitschrift für Sozialforschung* included a few articles written in English. However it is interesting that Mills should say, "... there is now no (original) periodical that bears comparison with this one testifies to the ascendancy of the higher statisticians and the grand theorists over the sociologists. It is difficult to understand why some publisher does not get out a volume or two of selections from this great periodical". (Mills 1970:572) Mills, despite collaborating with Hans Gerth on the translation of Max Weber's work, could not read German, thus points to the importance of institutionalisation as a factor making for intellectual visibility.

reviews and critiques of other scholars works, and as such, are not in a particularly appropriate form to safely secure a place in academic history. It is therefore arguable that much of their work would easily have been lost to sight without the focussing force of institutional attachment. Even works which would have stood independent reading are aided in their 'staying power' by being seen not in isolation, but as a product of an identifiable tradition. Furthermore, as a result of their institutional base their works have been kept, or rather continued, as a living force in sociology through the original members and acolytes.

Since the 'School' is still in existence it is too early to gauge its overall impact upon sociology, but the main point to stress is that by institutionalization not only has its past work been made available, when it may otherwise have failed to gain an audience, but their new work continues to be produced.

Commenting on the history of the Institute Shils writes:

"The history of the Institut für Sozialforschung in Weimar Germany, the United States, and the Federal German Republic is not just the story of the cat that landed on its feet. It is a testimonial to the skill of a shrewd academic administrator, who by good luck and foresight inherited a favourable institutional situation and developed its connections within the various universities in which it was located, maintained its internal structure, and extended its connections outside the university. As a result it became the mechanism by which some of the most influential ideas in present day social science developed."

(Shils 1970:776)

By way of contrast, Shils cites the case of Mannheim who, "having created no following, has found none since his death, despite the repeated calls for a sociology of knowledge" (Shils 1970:776). However, Martin Jay disagrees with Shils' interpretation, stating, "Although Shils may be right about Mannheims's isolation, his picture of the Frankfurt School's manipulation of its institutional solidarity as a springboard to success is highly oversimplified, if not basically incorrect". In support of his assertion Jay quotes a letter which Fromm wrote to him concerning the productivity of the Institute:

"Here was an institute which had as its members some excellent minds of left wing radical, though mostly noncommunist, thinking. It got out of Germany before Hitler and was one of the few, if not the only institute which saved its funds ... Yet what were its achievements? The only real scientific achievement is the volume on *Autorität und Familie* and a number of valuable papers in the *Zeitschrift*. But that is all, and I do not think that was enough, given the great possibilities the Institute had. In fact, I think it was relatively little. This does not mean to imply that the individual work of its members ... has not been of value, but these authors would have pursued their work without the Institute also."  
(Fromm 14:5:71)

In disagreement with Jay, and in fairness to Shils, it must be mentioned that Fromm is not really making the point which Jay attributes to him, and his quotation from the letter is a clear sign of that misunderstanding. Nowhere does Shils stress the productivity of the Frankfurt School, in fact, the relevant section of his article is a comparison of Horkheimer, albeit as the embodiment

of the 'School', with Mannheim and the respective success of one and the failure of the other to gain attention through institutionalisation. Indeed, Shils goes so far as to say that Mannheim "wrote more and on more particular topics than Horkheimer" (Shils 1970:773) and in doing so underscores the crucial role of institutional association as a vehicle for the communication of ideas over time. Thus, the point to emphasise is not the productivity of the Frankfurt School, but rather its institutionalisation. Certainly, it was never firmly embedded in the American University system, remaining very much on the periphery; but sheer quantity of work is, of itself, not particularly important in the reception of ideas, or for their survival over time.

Whereas quantitative empirical work, which is commonly associated with research centres, can, by the division of labour along functional lines increase the overall productive capacity of the enterprise, and by extension, aid individual output, no such mechanisms can be considered as operative in the case of Horkheimer's Institute. Their operative practices and organisational format were not of that type. For example, even in the case which Fromm considered as an important part of their main "scientific achievement", "Studien über Autorität und Familie" was not a truly 'collective' piece in the same sense of many research projects,

since, even though it involved contributions from several members, of the Institute they each worked alone on selected problems. Indeed, although a work such as 'Autorität und Familie' could not, in all probability, have been produced without the financial support and framework which membership of the Institute afforded, this form of the intellectual division of labour was characteristic. Hence for the most part, the Frankfurt School acted individually, bound together only by a 'common' intellectual orientation under the umbrella of the Institute, and not as a 'collective' of scholars integrated at the point of production. Quite apart from the fact that their intellectual differences produced fissures in their cohesiveness, the actual nature of the work they engaged in militated against any closer collaboration. Thus, Fromm is probably correct in considering that the individuals who constituted the 'School', "would have pursued their work without the Institute". However, whilst it may be speculative to consider history's alternatives it is not speculative to state that most of the work produced was not dependent upon the organisational setting. Thus, even though the institutional structure was not necessarily productive for the work itself, it has meant the survival of that work over time and historically that has been the Institute's main strength. This is not to deny that individual works would not have survived, in much the same way that the individual works of Mannheim have, but, without the institutional apparatus it is unlikely that they would have survived as teachings.<sup>1</sup>

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1 This is notwithstanding the adoption of works by Left Wing political groups since the selection process of political movements are not likely to be those which would have preserved the overall body of their work.

The Institutionalisation of Empirical Work: the Impact of the 'Bureau'

If, as has been argued, the sustaining of the Frankfurt school's work has benefited from institutionalisation, then the processes operative in that instance pall into insignificance when compared with the role that institutional forms have played in the establishment and continuation of empirical social research within America. It is readily admitted that an exhaustive study of the establishment of empirical sociology within America would need to cast the investigative net much wider than the part which organisational structures have occupied in that process. To be sure, such a task would require a detailed analysis of the development of sociology in general, a process which by extension would involve an analysis of changing social structures as well as ideas and the interrelation of the two. However, the enormity of the task must prohibit any such ambitious programme here. Yet, this proviso notwithstanding, the role which institutional organisation has played in the fostering of empirical work should not be underestimated since, even though it is only one piece in the overall pattern, institutional organisation is of undoubted importance when considering the way in which empirical knowledge, and more particularly quantitative methodologies, have managed to largely overshadow other branches and come to occupy such a dominant position in defining social knowledge within sociology. The ascending of such a cognitive framework owed much to organisational enterprise; an enterprise of which Paul Lazarsfeld can be considered to be a major, if not the major figure.

However, although Lazarsfeld has been particularly 'responsible' by way of institutional innovation for this trend, it must be stressed

that empiricism in its most extreme forms, or indeed when taken as the solitary definition of social knowledge, has been neither his wish nor has it been consummate with his own intellectual preferences.

The above point needs extending further since it is closely bound up with the nature of research centres. Lazarsfeld noted in his memoir that:

"Some critics oppose survey research as restrictive and one sided, pointing to the Columbia tradition as an evil influence.<sup>1</sup> It is useful to point out that, from its beginning, this tradition stressed the importance of a diversified approach. Legitimation, like women's work, seems never to be done." (Lazarsfeld 1969: 283)

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1 See in particular Maurice Stein, "The Eclipse of Community" (1964). Stein had been a 'student' of Lazarsfeld's and attacks what he saw as the one-sided training he received whilst a graduate at Columbia. In one passage he states: "The issue, however, is not evaluating the survey method because that is impossible. For certain purposes, obviously, the survey method is the only way to obtain data, but it should never become the only kind of data that sociologists gather, for if it does thought is stopped before it starts. In fact, however, survey logic lends itself to such a thought-stopping role, as indeed does the logic of any method if it is consciously or unconsciously presumed to be the necessary or exclusive method. One of my favourite fantasies is a dialogue between Mills & Lazarsfeld in which the former reads to the latter the first sentence of The Sociological Imagination, 'Nowadays men often feel that their lives are a series of traps'. Lazarsfeld immediately replies, 'How many men, which men, how long have they felt this way, which aspects of their private lives bother them, do their public lives bother them, when do they feel free rather than trapped, what kinds of traps do they experience, etc. etc. etc.' If Mills succumbed, the two of them would have to apply to the National Institute of Mental Health for a million dollar grant to check out and elaborate that first sentence. They would need a staff of hundreds, and when finished they would have written Americans View Their Mental Health rather than The Sociological Imagination, provided that they finished at all, and provided that either of them cared enough at the end to both writing anything. (215-216)

Indeed, Lazarsfeld in a proposal which he wrote with Merton for the establishing of an institute for training in the social sciences particularly emphasised the use of a variety of approaches in order to infuse a breadth of scholarship into such a training programme.<sup>1</sup> It cannot in all accuracy be said that Lazarsfeld possessed a narrow view of sociology, or that he himself engaged in the abstracted empiricism of which Mills accused him. Nor, from a reading of his philosophical papers, can it be concluded that he ever made appeals for such a style of work.<sup>2</sup> From his early Vienna days there is a clear insistence upon the utilisation of a variety of approaches to social scientific enquiry coupled with the wish to erode the restricted disciplinary frontiers of social science and establish a more totally integrated whole. Nor was his insistence on rigorous methodology intended to deny the importance of structural analysis within sociology. Indeed, his perfecting of polling methods was intended to rid them, by way of contextual analysis, of their atomistic and individual character.<sup>3</sup> For as Boudon writes:

"In contextual analysis, the respondent is defined not only by a number of individual characteristics (age, sex, opinion on various questions, educational level, occupation etc.) but also by variables that describe the milieu from which he comes. In this way, the investigation loses its atomistic character and can, by means of adequate analysis, regain the macrosociological tradition typical of Durkheim.

Thus, it is wrong to say that Lazarsfeld's methodology leads to a restrictive notion of research unsuited to the preoccupations of macrosociology. On the level of intentions he always tried hard to refine this methodology to the point where empirical sociology could meet the classical requirements that sociology take into account "social structure" and the situation "as a whole".

(Boudon 1972: 425)

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1 This is discussed in depth later.

2 See for example his article "The Obligation of the 1950 Pollster to the 1984 Historian (Lazarsfeld 1964).

3 In this context see Herbert Blumer's attack on polling in his article "Public Opinion and Public Opinion Polling" (1948).

It is pertinent that Boudon should place the word "intentions" in italics for it is the question of Lazarsfeld's intentions which the writer wishes to take up and develop since such a line of enquiry will serve to illuminate the structure of research centres as well as offering insights into Lazarsfeld's innovatory drive. For, although Lazarsfeld's own career has been marked by intellectual achievements with which most scholars would be satisfied, his institutional ambitions are marked by a certain disappointment and frustration. But, before developing this point in detail the writer wishes to briefly consider two important centres of learning which emerged in the late 'forties and had a tremendous impact upon American sociology - namely, Columbia and Harvard. The purpose behind such a comparison, no matter however brief, is to throw into sharp relief the impact which Columbia secured by way of its organisational structure, and then to consider why, despite his intellectual success, Lazarsfeld was basically disappointed with the institutional position which the 'Bureau' occupied.

Shils, commenting upon the early dominance of the University of Chicago in American sociology attributes its pre-eminence not only to the "intellectual power" of some of its staff, but also to its superior institutionalisation when compared to other universities.<sup>1</sup> However, "although Chicago sociology was embedded in a more ramified co-operative network, its institutionalisation was in one important respect like that

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<sup>1</sup> Chicago University was the first university anywhere in the world to establish a department of sociology. This it did in 1892. Its instruction programme was based on an adapted form of the German seminar, but with closer supervision of doctoral students.

of Durkheim's school. It depended very much on one important intellectual personality at a time" (Shils 1970: 793). Consequently, after Park and Thomas left, its dominance declined. But, as Shils also notes, "not only internal developments reduced the centrality of Chicago. New centres were emerging in the East, places which had never been sub-centres of Chicago" (Shils 1970: 794). Among these new centres, the most important were undoubtedly Harvard and Columbia. At Harvard, under the triumvirate of Talcott Parsons,<sup>1</sup> Henry Murray and Clyde Kluckhohn, "a deliberate attempt was made to integrate the theories of social structure, culture and personality". However, although the teaching programme was adapted to such a conception of the subject, the research training programme did not move forward at the same intellectual pace. Furthermore, Harvard's Laboratory of Social Relations, "never became the intellectual drill ground which the Bureau of Applied Social Research became shortly after the end of the Second World War". Even though the 'Laboratory' became the home of numbers of studies, it never developed the identity which the 'Bureau' achieved, and as a result lacked the solidarity among its intellectual mentors which characterised the 'Bureau'.

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1 Parsons, amongst others, had his convictions of the need for organised research facilities strengthened by organised war-time research. Dean Buck commissioned Parsons "to visit and report on some of the principal organisations of this character in universities and government". One of the places he visited and reported on was Lazarsfeld's 'Bureau'. See "Department and Laboratory of Social Relations, Harvard University. The First Decade 1946-1956". (Parsons 1956: 14)

"... it was in fact a centre consisting internally of several non-communicating segments - and this reduced its capacity to impose itself effectively on the subject as a whole. At the same time, it should be stressed that each of the major segments was a powerful intellectual personality - Parsons, Murray, Kluckhohn, Bruner, Stouffer, and Homans - each of them in one way or another a forceful generator of ideas and works. A high degree of consensus among them might have swept the field.<sup>1</sup> (It did pretty well as it was!) It also lacked what Chicago in the 1920's and early 1930's possessed, namely organs of publication<sup>2</sup> and stable extra-academic institutional links with the local community for research and training purposes." (Shils 1970: 795)

However, the impact of Harvard sociology should not be underestimated, for although lacking the more formal organisation of Columbia its impact, both by way of "free-floating" ideas and through the placement of students in key university positions, was tremendous. Nevertheless, seen historically, it can be considered that Columbia has had a greater, and probably more lasting impact than Harvard. The source of this success lay in the marrying of ideas with an institutional setting capable of transmitting them in practice. Merton, for example, came out of the Harvard stable having studied under Parsons, and although it would be correct to say, without for a moment denying Merton's originality, that he drew his support from large scale thinking in the mode of Parsons, his own more limited theories were much more articulate and manageable. In fact his

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1 See Parsons 1956: 23 for Institute's list of members. So distinguished is the list that single domination is unimaginable. In addition, it was difficult to combine the varying interests of the members. Thus, instead of a centralised structure the organisational response was "... a good deal of decentralisation" (Parsons 1956: 19).

2 The Sociology Department of Chicago published the "American Journal of Sociology". Albion Small, who was the first Chairman of the Sociology Department, founded the journal which was not only the first professional journal in the field, but can also be considered as one of the most important.

celebrated paper on theories of the middle range, although since given further expression,<sup>1</sup> was originally produced as a discussion paper four years after he had joined the 'Bureau' and intended as a reply to Parsons' address to the American Sociological Society on 'The Position of Sociological Theory'.<sup>2</sup> Although Merton was not at total variance with Parsons he did object strongly to Parsons' suggestion that our chief task is to deal with "theory" rather than with "theories". Slightly further on in the paper, Merton states his position on sociology very clearly indeed:

"Sociology will advance in the degree that the major concern is with developing theories adequate to limited ranges of phenomena and it will be hampered if attention is centred on theory in the large. I am confident that this is not in basic disagreement with Mr. Parsons; that it is a difference in emphasis rather than substance; indeed, later passages in his paper suggest as much. But I think it important to supply just that emphasis. I believe that our major task today is to develop special theories applicable to limited ranges of data ... I am suggesting that the road to an effective conceptual scheme will be more effectively built through work on special theories, and that it will remain a largely unfulfilled plan, if one seeks to build it directly at this time."

(Merton 1948: 165, 166)

Merton was suspicious of attempts to evolve total systems of theory "adequate to the entire range of problems encompassed by the discipline", preferring the model of the 'older' sciences with their emphasis on theories of specific types of phenomena. Such theoretical goals fitted very well with the operational framework of the 'Bureau'.

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1 In particular see Merton 1968 "On Sociological Theories of the Middle Range".

2 Paper presented by Parsons to the Annual Meeting of the American Sociological Society, December 28-30 1947, and appeared in the "American Sociological Review", 1948.

Indeed, Lazarsfeld's methodological brilliance, coupled with his acute appreciation of the relationship between theory and empirical work, provided a firm intellectual basis for the friendship between the two men.<sup>1</sup>

The relationship between Merton and Lazarsfeld was of great significance, not only for the 'bureau' but also for their respective academic reputations, which in turn fed back and helped legitimate and strengthen the 'Bureau'. Lazarsfeld suffered somewhat from the reputation of being an arch-empiricist, with all the unfavourable connotations that such a term implies, and whereas his friendship with Stouffer had tended to reinforce this image, his collaboration with Merton served to ameliorate such impressions. By the same token, considerable benefits accrued to Merton as a result of his relationship with Lazarsfeld. The nature of these benefits was explained to the writer by Bernard Berelson, who had been on the staff of the 'Bureau' in the 1940's and later became its Director. After stating that "Paul could get theoretical respectability etc. by having some of Merton rub off on him - if Merton would take him seriously then the fraternity had to take him seriously", Berelson went on to note that:

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1 In discussing his days as a graduate student at Columbia Stein comments: (The) "... assumption of methodological neutrality was bolstered by the kind of structural-functional theorising which worked systematically with abstract, value-neutral categories that could as easily be applied to analysing coffee as to interpreting reaction in a revolutionary situation. The dominant figures of course were Professors Lazarsfeld and Merton, working, as the latter phrased it, in 'double harness'. Many of us spent a great deal of time and energy trying to discover what held these two horses together as well as trying to learn what made each of them run." (Stein 1964: 214, 215)

"Bob (Merton) on the other hand could present an image of his relationship with the new empirical world of Stouffer and Lazarsfeld. Even though the empirical work he did, the big housing study, never got home - never came off, and the Kate Smith study,<sup>1</sup> which is really a puny little thing. That is about as much empirical work as Bob did in those days, although he was always around and so on. In addition, in later years I always felt - I don't mean to say there was any motive on Bob's part, but the consequences ... Merton got a positive rub off from ... that he was ... when you looked at Columbia for a distinguished sociologist, Lazarsfeld was impossible, and that made Merton stand a little higher relative to him. Bob got the first honorary degree. They couldn't give it to Talcott Parsons because no one could understand what the hell he was talking about and so forth. Lazarsfeld was still quote "not one of us" unquote, and so there was Bob Merton, and he got a lot from both of them. He was the heir apparent and he, quote, "united the traditions" unquote. Parsonian Grand Theory and Lazarsfeldian empiricism, and there stood Bob at that historical juncture being the man who was going to unite them - middle level theories and so forth. It was just a kind of ... everybody got something out of it." (Baralson 12:7:73)

If both Merton and Lazarsfeld gained from their association, their close collaboration was even more decisive for the overall intellectual impact which the 'Bureau' made upon American sociology. Shils has already been quoted as noting that Harvard suffered from the lack of consensus among all its leading figures and that this dissipated some of its strength. In the case of Merton and Lazarsfeld however, an unusual degree of empathy appears to have existed which made not only for a reduction of intellectual conflict, but also for cohesion and direction at the administrative level. To take the intellectual point first. Whilst not wishing to cast them both in the same intellectual mould, it is nevertheless true that the differences between them were neither so great as to inhibit meaningful collaboration, nor so great as their present reputations would suggest. For example, the paper

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1 The Kate Smith study is a reference to Mass Persuasion: The Social Psychology of a War Bond Drive, (Merton 1946)

which Merton read before the American Sociological Society in 1946 entitled: "The Bearing of Empirical Research Upon The Development of Social Theory" could easily, for the most part, have been written by Lazarsfeld. To quote just one pertinent section with which Lazarsfeld would have had no disagreement:

"in general, the clarification of concepts, commonly considered a province peculiar to the theorist, is a frequent result of empirical research. Research sensitive to its own needs cannot avoid this pressure for conceptual clarification. For a basic requirement of research is that the concepts, the variables, be defined with sufficient clarity to enable the research to proceed, a requirement easily and unwittingly not met in the kind of discursive exposition which is often called "sociological theory"." (Emphasis mine) (Merton 1948A: 514)

Furthermore, Marie Jahoda, who went to the 'Bureau' in the late 1940's at Merton's insistence, to help on a housing study, and who, knowing both Lazarsfeld and Merton very well indeed, was particularly well placed to appreciate their relationship, commented upon Lazarsfeld's influence on Merton thus:

"It (housing study) was a brilliant study, a wonderful unpublished study.<sup>1</sup> He really is an old criminal for never having published it. When I see Merton these days I still tell him he is a criminal ... You know, under Paul's influence Merton did much more empirical work than he would otherwise ever have done. You see, I wrote these two volumes of this first rate empirical study which was under Merton's overall direction. And he didn't publish it because it didn't make a contribution to a theory of the intermediate range. So you see Merton has his complexities and difficulties." (Jahoda 12:7:73)

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1 The study was funded by the Lavanburg Foundation. The intended book was to be entitled, Patterns of Social Life: Explorations in the Sociology and Social Psychology of Housing. Although never published substantial reference to it can be found in Merton's part of a joint article which he wrote with Lazarsfeld entitled, "Friendship as a Social Process: A Substantive and Methodological Analysis" (Lazarsfeld and Merton 1954). See also "The Social Psychology of Housing" (Merton 1948).

His refusal to publish on the grounds cited by Jahoda is evidence of the undoubted primacy of theory among his sociological interests. However, although his style of intellectual life centred around theory, he also recognised the importance of empirical knowledge to the extent that he not only engaged in empirical work and wrote methodological articles, but also opened himself to the influence of Lazarsfeld's more empirical intellectual style. Enough has already been said of Lazarsfeld's own intellectual flexibility as well as his refusal to separate theory and empirical work to understand the compatibility between the two men. However, their friendship goes beyond the intellectual realm and into the realm of interpersonal relationships. The following abstract from an interview with Bernard Berelson superbly illustrates the closeness of the two men:

D. Morrison: Was it difficult to be innovative in the 'Bureau'? I'm talking particularly from your point of view, but ... younger members. Was it often very difficult to be innovative? Did it usually come down from the top?

B. Berelson: Well, there are different periods. In the early days - the days when I was a staff member ... the days back around the forties. I don't think there was any institutional obstruction to it, but Paul was so imaginative, innovative, sparking off all sorts of ideas that there wasn't much room for anyone else. Then, added to that, Merton was such a distinguished figure. He and Paul were so closely married, and each of such stature, that there just wasn't any room around. I remember one point ... a few of us raised a question

about some issue, and so it went to a sort of staff meeting. I don't think they discussed it beforehand, but Paul and Bob so thought alike, or they got such signals from each other, or ... if you said it's grey today then I quickly put the tone of grey on it ... they just took it over in such a way that a few of us as a matter of fact just left the meeting ... what's the use, why try this, they were running the thing."

(Berelson 12:7:73)

Thus, the impact of the 'Bureau' resulted from a variety of interconnected factors. Merton's theorising of the middle range was clearer, and easier to understand than Parsonian grand theorising, a factor leading for ease of transmission at the teaching level. At the same time, Merton's work was not in competition with, but part of the rising structural-functionalism which came to be the most regarded theoretical framework of American sociology during the 1950's and 60's. In addition, Merton's work had the added attribute of bridging the gap between theory and empiricism in that it was much easier to translate into researchable projects. These factors, when added to Lazarsfeld's empirical knowledge, helped to turn Columbia into one of the major and most influential centres of learning in America. Thus, the cohesion of its leading figures, plus the rigorous training provided by 'on the job' learning:

"increased the proliferation of the Columbia centre, at a relatively high level of technical competence, and encouraged the diffusion of its procedures and modes of thought to many non-academic institutions which sociology had not penetrated before. It also made Columbia a national and international centre for sociology. (Partial sub-centres were formed at Chicago and at Berkeley - wherever, in fact, a "research facility" was created. The survey Research Centre at the University of Michigan was an exception to this; it did not derive from Columbia.)

(Shils 1970: 794)

Hence the organisational structure of the 'Bureau' and the internal cohesion of its members meant that not only was the production of work prolific, but that in addition it bore a common stamp, almost a house style which, due to the techniques involved, was capable of easy transportation and reproduction at other centres throughout the country, a fact which "led to a wide diffusion of Columbia sociology and generated - more on the level of procedure than of substance - a consensual element of sociology which will probably endure" (Shils 1970: 795).

#### Research Centres: An Unfulfilled Promise

In the previous section the writer noted that Lazarsfeld's institutional ambitions have been marked by a certain disappointment and frustration. Considering the stress which was placed upon the 'bureau', impact upon American sociology in general and research centres in particular, such a statement may appear somewhat contradictory, but it is not. For, although Lazarsfeld's own career has progressed with unquestioned success, it is with some regret that he views the position of research centres as a whole. It must be remembered that the first such social science research institute anywhere in the world was born half a century ago in Vienna, and as the originator of that institute Lazarsfeld worked not only to establish a leading centre of research in America, but witnessed the spread of his original idea, so that now similar centres form an integral part of the social science scene. Yet the basic problem of a lack of firm financial support which confronted his small Viennese Institute remains to this day a problem which has restricted the potential of the new institutes. Such centres,

albeit more stable than the 'Forschungstelle', nevertheless derive many of their problems from the basic fact that their existence is dependent upon the securing of contracts. Indeed, as Lazarsfeld himself notes:

"Most institutes obtain some support from their parent university, but the main part of their budget comes from foundations, government agencies and private clients which provide funds for specific studies.<sup>1</sup> This haphazard kind of financing is both a symptom of the ambiguous role which the institutes play and a factor which greatly affects their activities. One cannot understand their organizational and personnel problems without discussing the financial structure.

The crucial fact to keep in mind is that practically none of the institutes discussed in this memorandum receive regular support for their basic operations from their universities or from any other source." (Lazarsfeld 1961: 42)

Thus, it is scarcely surprising that having spent virtually all his academic life associated with the form of research institute he helped to pioneer, Lazarsfeld should feel concern and a certain amount of regret over their continuing weak structural position. Indeed, Lazarsfeld's later academic writings have turned particularly to this question. It is an index of his interest and concern over the position of research institutes that the topic chosen for his presidential address was "The Sociology of Empirical Social Research"<sup>2</sup> in which he argued for increased appreciation of the difficulties confronting research institutes.

That Lazarsfeld should feel concern and frustration at the position of research centres is thus understandable and, indeed, some of his

1 Using the total of outside contributions as a base Lazarsfeld estimates that government grants play the largest role, accounting for slightly more than 40%. Foundations contribute about 40% and private contract, that is, studies for business and voluntary organisations, provide the rest. (1961: 45)

2 The shrewdness of Lazarsfeld as a Director in always 'pushing his case' is born witness to by the fact that he appended the most pertinent part of his presidential address to his 'Memoir' - see Lazarsfeld 1969: 763-765.

writings exhibit a certain degree of annoyance that more basic support has not been forthcoming. Ironically, some of the criticisms levelled against research centres Lazarsfeld would agree with but, unlike him, what many critics fail to appreciate is that many of the features they object to often derive from the position in which research centres find themselves. That is, unless one is disagreeing in principle, then it must be recognised that many of the 'faults' of research centres are largely structural faults and, as such, really require structural solutions rather than observations on their shortcomings. For example, the criticism that too much applied work is undertaken is, seen from the position of disciplinary advancement, probably correct, even accepting that such work is not as 'valueless' as many suppose. However, the criticism is meaningless in the context of their operating position. It is not primarily from their own volition that research centres choose to engage so heavily in such work, but because force of circumstances has led over time to dependence on such work. If research centres were more financially stable, then the basis for a meaningful debate concerning the 'correctness' of various activities and practices could take place but, given the present circumstances it is somewhat misplaced to blame them for having adopted the only course of action which could have ensured their survival. The development of such a situation, and the failure to correct it during the formative years of research institutes has led to a confusion which will not now be easily overcome without far reaching reforms within higher education. Near the end of his 'Observations on Organized Social Research', Lazarsfeld focusses on the central problem and sounds the warning that:

"Altogether there seems to be little doubt that the problem of finding at least some basic support for institutes, rather than relying only on individual grants will have to be solved if the whole development under study in this report is not to be abandoned to dangerous anarchy."

(Lazarsfeld 1961: 48)

### The Position of Research Centres

Peter Rossi, a one time member of the 'Bureau' staff and subsequently Director of the National Opinion Research Centre at Chicago University, in discussing the location of research centres on the periphery of the University campus and the associated distribution of prestige, goes so far as to note:

"The physical marginality of the new academic organisations reflects their academic marginality. Traditional university tables of organisation lose their branching symmetry in attempts to place them in their proper places in chains of command, and university officials sometimes ignore them in the planning of university expansion, perhaps in the hope that if ignored they will vanish. Academic departments or schools to which the research centres may be attached are somewhat at a loss to deal with them, the personnel of the centres and institutes are hard to assimilate into the rank and privilege system of academia. The personnel of the centres are not quite sure of their identity, for on the one hand they are members of the university community, while on the other hand their major commitments are not to teaching and training functions which are at the centre of the university's activities."

(Rossi 1964: 1142, 1143)

To be sure, one of the major factors operating against the total integration of research centres into the universities has been the problem of fitting them into the existing administrative structures. But, as difficult as that may have been, it nevertheless remained a technical difficulty, and as such was not beyond the bounds of alteration. In addition, however, there have been other, less tangible barriers to acceptance, most notably the idealised notion of the solitary independent scholar, working alone to push back the frontiers of knowledge. As a characterisation of academic endeavour this image has entered the academic's

mythology about his world. Professor Wilson, the physicist, writing about his own struggle against team work and the idealised notion of the 'lone scholar' states:

"These men are doing creative, poetic and enduring work - true intellectuals they, not bureaucrats enslaved by a computer. Team research, the cliché tells us, is bad; individual research is good.

I have come to think differently. As a young man, I accepted the cliché, and I worked hard to attain that exalted image of scientific purity - the lone scientist in pursuit of truth. But my search for truth led me deep into the nucleus of the atom, and it is almost as hard to reach the nucleus by oneself as it is to get to the moon by oneself. To reach the moon one must join a large team, and to reach the nucleus one must also use the help of others. I have resisted joining a team, but in the end I have succumbed." (Wilson 1970: 1076)

In general team work is an alien method of operation for those working in university departments, for essentially an academic department is a collection of scholars who, despite cross-fertilisation of ideas, remain poorly integrated at the point of intellectual production. Although courses are organised, or supposed to be organised, in a rational and integrated manner, in general that is the extent of the collaboration. Once the scholar has discharged his teaching responsibilities he is 'free' to follow whatever intellectual pursuits he wishes, bounded only by the amount of funds available, and the established conventions of intellectual standards.

So far as differences in the organisational structure existing between research centres and university departments are concerned, then perhaps the single most symbolic expression is to be found in the respective titles accorded to their chief administrative officers, namely director and chairman. The very term, director, is indicative

of the greater authority which he exercises. Certainly the greater complexity of roles which stem from the division of labour within a research centre, which in turn stems from the nature of the work engaged in, requires much clearer lines of authority than those demanded in a teaching department. Within a research centre decisions have to be made continually, though not necessarily by the director. There is a need not only to allocate responsibilities in accordance with individuals' talents, but also to co-ordinate the work in a rational manner. Consequently, a continual surveillance of operations must be maintained so that corrective measures can be taken should various components of the work threaten friction by lagging behind, or even outstripping other components of the total operation. Any lack of co-ordination may result not only in the dislocation of ongoing projects by causing hold-ups which then may offend the client, but may have significant consequences for the future employment of the research staff. Usually only the most senior personnel of a research centre have security of tenure, and the rest of the members depend for their material existence on the continuation of contracts.<sup>1</sup> Therefore, it is absolutely essential to ensure the smoothest possible continuation of production, a factor which results in an increased strain towards rational bureaucracy since, in the final analysis, only that type of organisational structure can meet the demands placed upon it.

Whilst the nature of the work traditionally engaged in by research centres has its own imperatives tending towards division of labour and its congruent organisational forms, such a structuring is further reinforced by their dependence for a large part of their income upon

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<sup>1</sup> It is not uncommon for even the Director of a research centre to have to find his own salary.

contract work. Hence, not only does the work itself have its own internal demands, but the client also has demands which, when meshed together, push the organisational structure into a bureaucratic mode. Rossi, in discussing the hierarchical nature of research centres links the organisational structure to the type of work engaged in, stating:

"Large scale survey research in the universities is conducted by institutes and centres whose organisational principles involve a hierarchy of command and a distinct division of labor. Indeed, the larger the scale of research, the steeper the hierarchy and the more elaborate the division of labor. Thus the two university affiliated centres which conduct national surveys (Michigan's Survey Research Centre and Chicago's National Opinion Research Centre) have more complex structures than that of the Bureau of Applied Social Research at Columbia or the Institute for Social Research at North Carolina, the scope of whose work is more restricted in scale."  
(Rossi 1964: 1150)

Large scale empirical work, particularly where surveys are involved, is a very expensive enterprise. Consequently, few funding bodies capable of providing the required money do so lightly. Not only do they tend to require 'value' for money, usually interpreted as applicable results to a given set of problems, but more importantly for the structure of research centres they set deadlines which, within reason, must be adhered to at the same time as maintaining scholarly standards. This situation stands in marked contrast to the circumstances with the individual scholar faces in a teaching department. Whereas in the final analysis he is the arbiter of his own work, the scholar within a research centre is not. Corrective and disciplinary procedures, often of a subtle nature, are built into the context in which he works in order to ensure a certain standard of performance. In fact this mechanism of control can well be considered as one of the major strengths of team working. Individual brilliance, by its very definition, is not commonly exhibited, yet, due to the close interdependence involved in

team research, where each individual is accountable to others for his performance, then, since standards are set by the more senior colleagues, and presumably the most able, one has inbuilt structural factors pushing for higher performance. Performance 'checks' occur at all levels in the organisational chain, but particularly in the lower echelons where the new and inexperienced member must learn very rapidly the skills and standards expected of him. The director has the function of overseer, and it is his duty to ensure that the standard of work is of a high order which accords not only with his own internalised norms of what constitutes scholarship, but which also enhances the general reputation of the centre. More particularly, it is his responsibility to ensure that the centre is in the position of being offered the most attractive contracts which can be obtained. Inability to establish a reputation, or failure to maintain it once gained, only compounds the difficulty of attracting worthwhile contracts. The above argument cannot be countered by the consideration that funding bodies, not being professional social scientists themselves, are therefore disqualified from passing judgement on a centre's work. Quite apart from the fact that such bodies often employ expert consultants from the world of learning, it is exactly in such a position that reputations gained in the academic world take on greater importance since, in the absence of knowledge, reputation is the lodestone of confidence.

One of the main problems confronting research centres is the difficulty of building a good research team. The bringing together and matching of individuals, not only intellectually but also psychologically is a painstaking process which rests on skills not necessarily related to intellectual abilities. Furthermore, when compared to a university department, research centres tend, on the whole, to lack an adequate career structure which means that maintaining

a once established team is extremely problematic. For example, the statistician, Patricia Kendall, and wife of Paul Lazarsfeld, remained at the 'Bureau' for over twenty years without achieving faculty status and never obtained security of tenure. What this has traditionally meant is that quite apart from researchers being attracted into commercial enterprises by the offer of high wages, there has developed a constant pressure to seek career advancement by moving to another research centre. Whilst it can be considered that such a situation was an advantage during the early development of research centres, since it resulted in a wide dispersal of talent, once research centres became an acknowledged part of the social science scene its disadvantages rapidly became apparent. Whereas during the initial period it was beneficial for the development of empirical social research to have as many centres established as possible, at the consolidation stage it became more important to strengthen the existing centres by the creation of career structures which could hold good research teams together.

University departments in Europe, whilst having much stronger career structures do, at the higher reaches of that structure, face a somewhat similar situation to research centres. The fact that they have traditionally carried only one professorial position has meant that the most able senior staff have been obliged to move to another university in pursuit of a chair. Owing to the highly personalised nature of the work within a department however, this does not present itself as an academic problem but only as personal inconvenience. The American universities, in adopting the German model, introduced a fundamental modification. Hence, instead of a

single professor, one had the appointment of many professors within each field, an innovation which probably stemmed from the larger number of American universities and the competition among them for personnel.<sup>1</sup> Although the introduction of multiple professorships may have overcome staffing tensions within university departments, this solution was not extended to research centres, and even today they remain largely outside the university career structure. Not only could it be argued, when viewed from the point of production, that research centres are more in need of such an administrative arrangement than university departments, but in addition the lack of integration between the two career structures produces its own tensions. Although it may be a slight overstatement to consider that the scholar embarking upon an academic career must choose one or the other career structure, the fact remains that the separation of the two makes for considerable difficulties in transfer. Certainly, during the early period in the development of research centres such career considerations probably acted as a brake on recruitment, and even now operates as a factor limiting lateral mobility between the two structures. Whereas such career uncertainty may well have aided the development of research centres by attracting adventurous students who were heavily committed to their associated style of work, the present bifurcation operates to the disadvantage of both. That is, although research centres' staff often lecture within their respective universities, by and large most research positions relieve the researcher of the time-consuming teaching duties and associated administrative tasks. Consequently, what this has meant in practice is the increasing separation of knowledge production from knowledge dissemination, and the consequent undermining

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1 See Rossi (1964) for this point. Footnote on page 1149.

of the traditional concept of a university. Admittedly, at the level of graduate training research centres have within them the possibility of operating the traditional ideal of master and apprentice in which the student learns through working for a member of staff on some specific research task. However, because research centres exist on the periphery of the university system, 'credits' are often not forthcoming for the work which the student engages in, and more often than not the relationship tends towards that of employer and employee. As Samuel Sieber notes:

"An alternative to the inducement of money, of course, is the offer of academic credit for research internship. Providing credit of research work automatically converts the staff researcher and his assistant into a teacher and a student respectively. Thus, the staff researcher would be paid out of the university's instructional budget, thereby releasing him from full-time commitment to his project, and the student would seek to gain specific internship experiences in an allotted period of time as part of his normal academic career. However, the practice of giving credit for research assistance appears to be rare in the social sciences, even when the mentor has full faculty status. (Sieber 1972: 38)

Thus, in the absence of formal academic recognition for the work which the student undertakes whilst engaged on a project, economic reward forms an important incentive for such activity. For example, even though the 'Bureau' receives some financial contribution towards student training from the University, the relationship between the 'Bureau' and the student is essentially a financially contractual one which works to the detriment of both parties. Pratt, in his report on the in-service training programme of the 'Bureau' notes the tendency to evaluate a potential student according to the benefits he could offer in relation to ongoing contracted projects. Describing the selection process Pratt records:

"From a training standpoint, however, the significant factor is the pattern of questions during the interview.

The sequence of questions was orientated around the ability of the applicant to perform a specific job. There was a presumption of a training or experience background that would make the prospective trainee capable of fulfilling a then active need.

In most cases, the specific selective factor was the ability to do the given job when available. The applicant was in competition with others only on this point. He was not considered as a whole. His student record and qualifications were not considered comparatively with other possible candidates. It was a hiring process. It was not a trainee selection process."  
(Pratt 1954: 51)

Even though a dominant motive for joining the 'Bureau' was to gain research experience, in practice the students were not "generally selected for learning potentialities, but because of an already established skill that fits a given existing need" (Pratt 1954: 53). This is not to say that training did not take place; indeed it did, but the point to be stressed is that the process was haphazard. The students considered that the training they received was sound, but

"The situation is one where these techniques they were taught were well taught. It is not one where they felt that they were receiving all the training that they needed or that which the Bureau was capable of giving them. They tended to see the potential learning experience as greater than what they actually received. What training they received was both needed by them and well taught. But all they needed and all that the Bureau could give them was not taught them."

(Pratt 1954: 100)

There is no reason to presume that the training situation in the 'Bureau' is basically different from that existing in other bureaux since all such institutions occupy similar structural positions which generate similar organisational imperatives. Again one returns to the basic question of the lack of stable sources of financial support. In

fact, the problem of student training at the 'Bureau' throws the problems posed by this situation into particularly sharp relief.

Although student training is one of the 'Bureau's' goals, "projects are not selected first and foremost for their training potential" (Pratt 1954: 25), and consequently there is continual conflict between the demands of research and the demands of training. Projects are usually staffed by those individuals considered most able to perform the task since "the sponsor assumes both qualified personnel and unitary policy objectives and agrees to a budget on this basis". However, the available funds are usually only sufficient to complete the fieldwork with the result that "just as scientific needs are about to be met the funds are exhausted" (Pratt 1954: 26). There is, therefore, a pressure to complete the routine aspects of the project as swiftly as possible, and thereby create time to concentrate on the more stimulating aspects of the work. However, as so far as the student is concerned, almost all his time is taken up with mastering routine research procedures. For example, among the sample of 'Bureau' trainees studied by Pratt, the majority spent between six and eight months at the institution, during which time thirty percent had held only one position, thirty percent two, and thirty percent three. No trainee had occupied more than five such jobs.<sup>1</sup> The most frequently learned skills, or improved skills, were machine tabulation, table construction, search techniques, coding, preliminary and revision report drafting. The least frequently learnt or improved skills were those involved in analysis, design, and the final drafting of reports. Only one in four students were trained

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<sup>1</sup> For these figures see Pratt 1954: 96. Also the fact that a student moved positions did not necessarily mean that the function of the trainee had changed.

in any one of these last skills. Indeed, the principle of hiring as opposed to training is evinced very clearly in the fact that "at all levels improvement of skill was as frequent as first learning of skill" (Pratt 1954: 98). In other words, the student was basically employed to fit research goals, and these goals were not modified to accommodate the needs of trainees. This is not to deny that valuable training took place since, even where skills had already been acquired, the value of applying them in concrete practice by working on a 'real' as opposed to imaginary project should not be underestimated.

The training position within the 'Bureau' highlights the general operating conditions of such establishments. For, although training was a goal, it conflicted with other more important goals, and consequently suffered in priority due to situational circumstances.

- "The major criteria in the selection of projects include:
- 1) the availability of qualified supervisors known to the present staff;
  - 2) the degree of interest of this known supervisory staff in the project under discussion;
  - 3) the adequacy of the funds for the required project;
  - 4) the co-ordination of the project with Bureau research policy;
  - 5) the value of the project for theoretical or methodological development;
  - 6) the likelihood of continuing support to permit steady growth;
  - 7) whether or not the project should be taken simply on the basis that additional funds are needed to keep the Bureau solvent; and finally
  - 8) is it possible that student training potentials of the project may be considered." (Pratt 1954: 27)

Although Pratt gives no weighting to the above criteria, apart from placing student training last, a fact which none of the above considerations can escape is the question of finance. For as Lazarsfeld himself states:

"... we should frankly face the fact that in our system of higher education the matching of budgetary funds with substantive intellectual interests is a characteristic and enduring problem. The institute director knows the skills and interests of the faculty members, and he brings men and money together. This is not badly described as the role of "idea broker". Often he will have to work hard to obtain funds for a more unusual research idea suggested to him; at other times a possible grant looks so attractive that he will try to discover, among some of his faculty colleagues, what he would diplomatically call a "latent interest"."

(Lazarsfeld 1962: 765)

Admittedly, the better a grant fits all the above criteria, then the greater the likelihood of it being accepted, and indeed sought after. However, since decision making usually involves selecting among competing sets of goals, then student training is the most readily ignored. To a certain extent this sums up the position of research centres since, no matter what value is placed on training as an idealised goal, it is not of paramount importance to the immediate, or even middle term functioning of the organisation - though in the long term the lack of suitably trained staff would make its presence felt. Although a good bureau director must always operate with due regard to likely long term positions, it cannot be denied that the immediate contingencies of their financial position usually play a determining role. From the point of view of student training, limited term projects are more appropriate than long term projects, since they provide opportunities for students to participate in the whole range of research phases from conceptualisation to completion. But such projects are not the most readily sought after by bureau directors since they do not offer the same overall benefits as long-term projects. Once secured, a long term project lifts the pressure of immediate financial concern by providing employment for a number

of people for a sufficient length of time to allow the director to organise along more rational lines than would be the case in securing a large number of small projects. Indeed, the effort required in securing and administering small grants is out of all proportion to their value. Furthermore, generally speaking the larger the project the greater the possibility for applying theoretical and methodological sophistication, in addition to which, almost by virtue of its size, the large project is usually of a more general nature than its smaller counterpart with its likely linking to a specific concern. Consequently, the larger the project the greater the possibility of fitting it in with existing interests. Further, from the point of view of administration, large project grants are favoured since they tend to carry the greatest prestige; a factor which enhances the reputation of the receiving institute and in turn aids the obtaining of future contracts. In short, the large project accommodates more readily to the previously quoted criteria and more easily fits both research goals and organisational goals. Hence, although it would be wrong to consider that even the most successful research centre only engages in large scale work there is a marked tendency to seek this kind of contract. If one analyses the work of the 'Bureau'<sup>1</sup> over the entire course of its history one finds that a greater number of small projects were undertaken during its early rather than later years. This is paradoxical when seen in terms of student training, since it has already been established

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1 This includes unpublished reports, which the writer had the opportunity of examining whilst at the 'Bureau'.

that small projects are more suited for training purposes. Yet it is these very projects which suffer most when the possibility of obtaining larger ones presents itself as a centre grows and gains in recognition. Thus at the very time when such projects are most required for their training value they are least likely to be accepted. The tendency during the founding period of a research centre characterised by smallness of staff and the associated blurring of roles, means that the student can benefit from the intimacy of the situation through being able to watch, listen and talk with staff members on an informal non-regimented basis as part of a common enterprise. Thus there is no need for a highly formalised training programme; his training is assured by simply being 'around', and through partaking in a number of tasks. Indeed, as emergencies arise he is more likely to be given greater research responsibility than would ever occur once the centre became established; the bureaucratisation which characterises a well established research centre is specifically intended to ensure that such emergencies do not arise.

However, although the internal development of research centres has been characterised by increased rationalisation, their general development has been without any systematic planning in terms of higher education as a whole.<sup>1</sup> This has meant that variations between research centres is as characteristic of them as their central problem - lack of stable financial support - is uniform. Consequently, a study of research centres in themselves would require a detailed analysis of as

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<sup>1</sup> See Lon Hefferlin (1969) "Dynamics of Academic Reform", especially chapter four, for the impetus of change and development. See also Lazarsfeld 1961 for the stage of development.

many of them as possible in order to understand individual adaptations to localised as well as historical problems. Yet, in so far as general purpose units<sup>1</sup> are concerned, then the above discussion of the 'Bureau', and in particular its training programme exhibiting as it does certain central features, allows a generalised understanding of the overall position of research centres.<sup>2</sup> It also provides the groundwork for understanding Lazarsfeld's frustration at the way in which higher education has developed, or rather the inability of the system to come to grips with strains that have emerged within it. For example,

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- 1 That is, non-special units such as educational research units which have been established and operate for specific purposes. However, even these units face many of the problems being discussed. But their special problems cannot be addressed here.
- 2 Siegfried Kracauer in an essay featuring a research proposal entitled "The Social Research Center on the Campus: Its Significance for the Social Sciences and its Relations to the University and Society at Large" (undated, on file at the 'Bureau') states: "to explore these areas thoroughly one might think of a comparative study covering the operations and products of the most important research organisations since their inception. Yet such a study would be impractical because of its excessive scope. Nor is it absolutely needed". Kracauer then goes on to state why the 'Bureau' provides an adequate focus for a study of research centres, commenting that "the B.A.S.R., Columbia University, suggests itself as a fairly paradigmatic case. It is one of the oldest university institutes in the field, and has done a great deal of pioneering work, influencing similar organisation in this country and abroad. In fact, some of them are patterned on it. A historico-systematic study of the 'Bureau' - its activities in the areas indicated and its related organisational patterns - would therefore seem to constitute an adequate approach."

'competing' might be too strong a word to use, but certainly a double hierarchy has developed in many universities between the teaching departments and the research centres which can be considered as detrimental to both. Whilst not for a moment suggesting that the respective institutions each represent a monopoly of either theoretical or empirical work, the fact is that such a separation is built into the structures of each institution. Furthermore, this separatist tendency is not based upon ignorance, or deprecation of one form of work in favour of the other, even though preferences probably play some part in an individual's institutional affiliation. Quite clearly, for reasons already mentioned, most work within research centres is of an applied nature, and consequently grounded in empiricism, yet it would be mistaken, and particularly in the case of the 'Bureau', to consider that such emphasis represented the conscious triumph of one approach to social science over another; the 'triumph' is rooted in the context of the situation and not in social thought. Admittedly, the more empirically minded scholars are likely to be attracted to such institutions in the first place, but the situation within such centres then takes over, and over-determines initial leanings. Given the pressure to produce, the ever present necessity to furnish 'results' and acquire new contracts, there is little time or provision in the budget for the pursuit of theoretical goals. Interpreting data in terms of wider theoretical considerations, or even planning an empirical study in terms of theoretical ambitions, is difficult. 'Basic' research is often beyond the granting agent's interest, and, even though a thoughtfully executed study can usually furnish insights into social process beyond the boundaries of the study, it is not the

most appropriate way to proceed towards such knowledge. One institutional response to such difficulties is the technique commonly known as 'robinhooding'. As Rossi states:

"What I have just described might be called passive 'robinhooding' - as some researcher facetiously named the broadening of objectives of a policy-orientated sponsor to include concerns which are of intrinsic interest but for which no funded (or vested) interest is likely to be found to supply support. There is also the active type of robinhooding, in which one starts out with an objective of some intrinsic interest and then fits it to the applied interest of some agency or foundation." (Rossi 1964: 1157)

However, as effective as such techniques may be they only serve to highlight the difficulties that research centres face, since it cannot be considered the optimum approach to research, but rather the necessary exploitation of available opportunities. Neither are such techniques without stress for "robinhooding in both its passive and active forms leads to considerable tension between the policy maker and the researcher. On the one hand it looks as if the researcher is hoodwinking the policy maker; on the other hand it can be viewed as a process of bargaining in which the research centre agreed to do something in return for support to do something else in addition" (Rossi 1964: 1157).

Although techniques such as secondary analysis, or re-working the data, have emerged as a direct outcome of the pressurised conditions facing much empirical work, it can hardly be said, beneficial as such techniques have been, that they compensate for the fundamental structural difficulties which prohibited the exhausting of the material in the first instance. To be sure, secondary analysis can often prove

useful no matter what the situation since it is in itself a productive method of operation as well as being extremely useful for training purposes. However, the point to be made is that along with 'robin-hooding', it is an adaptation to the basic difficulties in the structural setting of research.

To return to the basic 'separation' of styles of work. It can be argued that because the sheer quantity and importance of the work produced by research centres has made them an integral part of the knowledge construction process within higher education there is a pressing 'need' for more integration with teaching departments. To have research bureaux fully integrated into the university system was the central core of Lazarsfeld's own institutional struggle. Not only was this desired for the organisational benefits which could be rested from having a guaranteed source of income, but also in an attempt to correct the fragmentation of higher education as functions split to cope with new demands and pressures. Indeed, research bureaux were in part a response to the demands of various agencies for knowledge suitable for coping with increasingly complex social organisations and social problems. The fact that the organisational form necessary for the provision of such knowledge was not present within the traditional university structure, and their subsequent inability to meet such a demand, resulted in the establishing of sub-units outside or on the periphery of universities which could handle such work. What has occurred is a kind of organisational pluralism within the universities which, to re-quote Lazarsfeld, "threatens anarchy". The ever-present danger is that the various sub-units will part company completely, thereby producing distortions in both intellectual training and in

the approach to intellectual questions. He saw the separation of scholarly and administrative roles as a particular weakness.

"We are confronted, nowadays, in our universities, with a serious problem which can be classified as an 'academic power vacuum'. When graduate education in this country began, no one doubted that the university president was an important figure. Gilman at John Hopkins and White at Cornell were intellectual as well as administrative leaders. Stanley Hull at Clark was impressive both as a president and as a psychologist. Inversely, individual professors were deeply involved in organisational innovations. John W. Burgess forced the creation of the graduate faculty upon the Columbia trustees ... Today, however, we witness a dangerous divergence: academic freedom is more and more interpreted in such a way as to keep the administration out of any truly academic affairs, and the faculty, in turn, has come to consider administration beneath its dignity. But educational innovations are, by definition, intellectual as well as administrative tasks. And, so, they have fallen into no-man's-land: the President and his staff wait for the faculty to take the initiative; the professors on their side consider that such matters would take time away from their true scholarly pursuits. As a result, many of our universities have a dangerously low level of institutional development." (Lazarsfeld 1962: 764)

The above has been quoted at some length since it is not 'reading off the meanings' too much to state that Lazarsfeld obviously had himself and similar individuals in mind when writing it. Indeed, a few lines further on he notes, "one institutional consequence of research institutes is that they inevitably train men who are able to combine intellectual and administrative leadership". To be sure, Lazarsfeld was personally, both by intellect and training, admirably equipped to fill the "academic power vacuum", and no doubt other detailed studies would reveal similarly suitable individuals. Just what reforms would be necessary to re-integrate the component parts of higher education is beyond the scope of this work, and only a detailed and far reaching study of the whole of higher education would be capable

of producing the material necessary for informed policy decisions. However, in the 1950's Lazarsfeld did make an attempt to reform which, whilst not entailing the possibility of transforming higher education may have, in the process of implementation and operation, produced far reaching consequences. The fact that Lazarsfeld failed in his efforts will be dealt with shortly, but first it is necessary to detail the administrative process involved in establishing the 'Bureau' at Columbia, which, once accomplished, resulted in dissatisfaction with the existing order to the extent of wishing to counterpose a new institutional form.

#### The Bureau: Acceptance and Change

The original plan which accompanied the Rockefeller Foundation grant to study radio in American society was that upon completion of the study the research organisation would be disbanded. However, as previously noted, during the course of its operations contacts were made, other work accepted, and consultation fees obtained which laid the basic groundwork for the continuation of the organisation on a permanent basis. Despite the financial possibility of continuation however, the problem remained of legitimating itself with the University so that a firm foundation could be laid which would aid its development and growth. A factor working for success in that direction was that:

"... the Office of Radio Research made manifest a widely felt need for a research organisation in the social sciences on the university campus. On the one hand the requirements of sociological field work had by that time grown beyond the capacity of any one individual. The Office of Radio Research with its facilities, staff

and equipment made it possible for members of the faculty to undertake studies requiring the gathering of social science data on a mass scale - studies which working alone, they would have been unable to undertake. On the other hand a campus research organisation in the social sciences meant an opportunity to add a new dimension to the training of students. Unlike physical scientists, social scientists had hitherto been trained almost exclusively through attending lectures and reading books. The Office of Radio Research was seen as a possible prototype for a new kind of empirical training parallel to that given to physical science students in the chemistry and physics laboratory." (Monograph on file at the Bureau. Undated, unnamed, but probably written by Lazarsfeld in early 1950's.)

This memorandum points to strains within the existing operations of the social sciences which the 'Bureau' could help alleviate. Furthermore, Columbia, and one might add other universities, were under increasing pressure to fulfil their service functions by proving 'useful' to the general community. Without over-stating the point, it can be argued that a disjunction had arisen between what was defined as 'useful' knowledge by various societal agencies, and the knowledge which the universities were providing. For example, owing to the demand for knowledge of a more applied nature, or perhaps information may be a better description, a whole host of research agencies had developed outside the universities. Consequently, whilst it could not be said that the 'Bureau' either saw its purpose as merely providing required information, or that such activities were paramount when viewed in terms of its overall work, the applied nature of its work went some way towards accommodating to social demand and thereby demonstrated its benefits to the University. However, it is quite another question as to whether these benefits, even though recognised, were to be accepted as part of the correct ordering of things.

From the administrative position acceptance is easier when the new organisation approximates in form to the existing administrative structures. However, the ad hoc development of the 'Bureau' meant that formalised sets of roles had not developed to any significant extent and those roles which did exist did not match the existing status positions within the university. The main roles within the 'Bureau' were those of project directors who carried ultimate responsibility for projects. Below them were the informally designated roles of study directors, and they in turn were assisted by technical and clerical staff. Hence there were no equivalents to the conventional departmental roles of full professor, associate professor, assistant professor and instructors. Furthermore, some of the 'Bureau' staff were not academics, but individuals hired for their technical skills rather than their 'intellectual ability'.

It was not Lazarsfeld's own administrative style to have a formalised hierarchy with distinct roles, but rather to have a looser structure under his own control and bridged through his personal authority. It was a style of leadership 'natural' to him, and one born of long experience; indeed, it can be traced back to his Viennese experiences, both in the socialist movement and as an institute director. The smallness of the operation made such a style not only feasible but, considering the lack of institutional establishment, advantageous. Indeed, one can consider that it was the most appropriate leadership style for the early stage of the 'Bureau's' development. For, in the absence of a defined career structure, the holding together of staff members required that personal commitment and allegiance should be accorded the director. Hence Lazarsfeld's

ability to generate loyalty and to infuse members with a belief in the value of this new style of work was the only real stabilising factor in an otherwise volatile situation. Pratt, commenting on the early years of the 'Bureau' notes:

"The contracts were small, financially, and the scientific problems great. The contract future was uncertain, particularly in terms of continuity of problems. Under these conditions a rare combination of ability, skills, congeniality, and tremendous motivation was needed to overcome the developmental problems of this new departure in social research. Identification by the senior people, especially by the senior directors, with the Bureau was intense. The same intensity of commitment was demanded from the trainees."  
(Pratt 1954: 63)

It was due to Lazarsfeld's personal charisma that the necessary cohesive identity was created to sustain the 'Bureau' in the absence of firmly established structural supports. To be sure, it would seem that Lazarsfeld never possessed the tidy mind of administration, the love of detail, but what he did possess was the flair for organisation and in particular the ability to communicate, and infect others with his own enterprising enthusiasm. By comparing the 'atmosphere' and 'style' of the 'Forschungstelle' with that of the 'Bureau' during its early history one can, to a certain degree, isolate the influence of Lazarsfeld's own personality. This can be gauged from the fact that historically and culturally the settings of the two institutes was entirely different, yet the 'style' and 'atmosphere' within them both was similar. Since Marie Jahoda worked at both institutes she provides a particularly good witness to the impress of Lazarsfeld's personality in creating a close-knit, friendly working relationship as well as generating a high degree of commitment to each other and the form of research undertaken. Bearing in mind the past comments on the workings of the 'Forschungstelle', Jahoda informed the writer that:

"I felt immediately at home (at the 'Bureau'). The scale was bigger, there were more people involved, the relationship to the university was closer. The difficulty of administration and arrangement was almost as bad as in the 'Forschungstelle'. Paul again undertook too much, but he had better secretaries than he did in Vienna. Yes, in the beginning it was the same style, and very much supported by this close personal and intellectual friendship between Paul ... you know they really created an atmosphere, you didn't have a job, but it was an intellectual community ...

... when I first came personal relations, men-women relations were of course central affairs in the Forschungstelle, they certainly were in the Bureau. The goings on were really quite remarkable, but I mean, the Bureau's Christmas parties in those days were great events. Everybody making poetry and sketches and drinking too much - great fun, very nice. Bernie Berelson was there, one of the central events was when I challenged Bernie Berelson to a game of table tennis with the whole Bureau watching the champions." (Jahoda 26:9:73)

Although the last part of Jahoda's commentary cannot be associated particularly with Lazarsfeld's operative style it has nevertheless been included almost as a check on the argued establishment of a close-knit group. For, although such pleasantries might be common to many organisations other than the 'Bureau' the absence of such informalities would indeed be surprising given the writer's description of the innovative enthusiasm and personalised, as opposed to formalised, roles. Furthermore, even though Jahoda compares the 'style' of the 'Bureau' to that of the 'Forschungstelle' and says that she felt "immediately at home", it must be pointed out that she arrived at what can be described as the end of the old 'Bureau' and the beginning of the new. By the time Jahoda arrived in 1948 the 'Bureau' was undergoing a process of stabilisation after having achieved its major struggle for recognition by the university following the report of the Cheaten Committee in 1945. To be sure, without wishing to be too strict, the writer will not venture much beyond 1950 in a discussion of the 'Bureau' since after

having steered it through its most turbulent period Lazarsfeld resigned as Director in May 1949,<sup>1</sup> becoming Chairman of the graduate department of sociology, but retaining his connection with the 'Bureau' by remaining as an associate director. Kingsley Davis, former professor of sociology at Princeton University, became the new director. Yet, quite apart from this change in personnel at the crucial director level, a series of events external to the 'Bureau' ensured its change from a personalised informal structure to a more identifiable bureaucratic one.

Although previously recognised by business and industrial concerns as of value in their operations, social research rapidly moved to the fore in government thinking in the post-war readjustment period. The service that such research had rendered to the war effort carried over as a recognition that such work could continue to aid military as well as civil purposes.<sup>2</sup> The flow of government contracts

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1 Various references give Lazarsfeld resigning in 1948. However, May 1949 is probably the correct date, in the sense of the official date of resignation, since that is the one given in a report of the 'Bureau' to the Board of Governors in 1951.

2 Harold Orlans in his work "Contracting for Knowledge" states, "the significant point is that the modern style of government is to 'contract out' much research and development, not to mention management and other services, formally performed by government staff" (Orlans 1973: 141). This change from intra to extra mural work has been one of the major changes in federal policy and one which has benefited research centres and laboratories in both the physical and social sciences. A key closing link between the federal agencies and the universities was undoubtedly the second world war. For, as Penwick in his "Politics of American Science" states, "... the critical question for science in the United States stood out starkly clear: could research affect military events quickly enough to determine the outcome of the war? The modest research programs of the armed services were entirely inadequate in the new situation. There was no time to build new laboratories, to train new career scientists to enter government services. The only realistic hope for deploying science lay with the university scientists, the laboratories and the weakness of the existing link between the government, university science, made formidable the task of bringing the two together" (Penwick 1972: 10). Based on figures for 1953-54 then more than 70% of all research conducted by American universities was financed by the Federal Government. This proportion varied from more than 90% in physics and mathematics to 25% in the social sciences. Universities themselves financed less than 10% of all research they conducted (Kidd 1959: 51).

into the Bureau is indicative of this change. Until 1948 the 'Bureau', apart from having received consultation fees during the war, had received no government contracts. However, the fiscal year 1948-49 saw the first appearance of government contracts in the 'Bureau's' budget, yet they only constituted 5% of its total income of 182,000 dollars. The year after, such monies rose dramatically to form 36% of its total income of 134,000 dollars, rising in 1950-51 to form 75% of its total income of 380,000 dollars, and rising again the next year to an all time high of 83% of its income of 523,000 dollars.<sup>1</sup> The influx of these monies meant a rapid expansion of the 'Bureau' and its activities. Furthermore, the flooding back into the university of 'veterans' meant that the senior staff of the 'Bureau' were overwhelmed with teaching duties.<sup>2</sup> Thus, the problem of rapid growth and the accompanying task of training new research teams placed a severe strain on the 'Bureau's' organisational structure.

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1 See Appendix B for figures. These are taken from the submissions of the 'Bureau's' sources of funds to a Unesco enquiry into research institutes 1946-1960. It is difficult to break the figures down, but the writer did discuss them with Allen Barton the present Director of the 'Bureau'.

A. Barton: "In the 1950's -51-52 period, the grant contract with the Federal Government was mainly airforce contracts. One was for Lindsey Davis....his city study....his urbanisation study on a World wide basis. There was a contract for interviewing methodology which had to do with interviewing returning German prisoners of war from Russia. The 'Bureau' provided the technical services - the military actually carried out the project. The project, I have heard, dealt with very detailed information about the location of specific factories in the Soviet Union where the prisoners had worked as prisoners of war... I presume for bombing targets."

D.Morrison: That would be classified work?

A. Barton: "That was classified, very classified"

D.Morrison: You stopped classified work?

CONT...

Although after 1945, in the post Cheatham Report era, roles began to be more formalised. The 'Bureau' was still really run by the Director and a core group of researchers in a personalised manner. But that was beginning to change as Leiba Brown writes:

"In the informal atmosphere of the early Bureau, members of the staff were easily accessible to one another, communication was intense, and generally taken for granted by the entire group, and consensus was achieved very often through direct face to face interaction.....  
In the more formal and complex situation which prevailed after 1945, the project units became insulated from one another. Each part of the staff had a legally defined job with a set of obligations to be carried out, and tended to become autonomous from the other parts of the staff. Communication, consensus, and the imparting of research skills became problematic" (Brown: 14 undated but probably 1957)

In 1947 for example the 'Bureau' instituted a seminar programme organised around both substantive and methodological topics. Previously such an articulated forum was unnecessary due to the close personal interaction of the whole 'Bureau' where it was possible for everyone to know what was going on through sharing a common work situation. Increasingly, especially in the late forties and early 'fifties, the 'Bureau' had expanded to the point where the focal unit of work and interaction was no longer that of the entire 'Bureau', but rather the project unit. A factor making for insulation of personnel and communication. This increasing growth and diversification of the 'Bureau' is illustrated by the fact that in 1951 the Board of Governors,

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A. Barton: "Yes, in 1952 the Bureau Board of Governors passed a resolution saying they wouldn't take any more classified work (Barton 12:7:73)

2 See Stein 1954:217

which had previously been solely composed of members of the faculty of the graduate department of sociology, was extended to include representatives from all the university social science departments. In addition that same year, the previous single directorship was abolished and replaced by a directorial committee of 'Bureau' staff members. This committee assumed active day-to-day direction of the 'Bureau's' affairs, and was responsible for coordinating work and developing plans and policies which after approval by the board of governors, became part of the organisation's programme. The old director position which Lazarsfeld and Davis had occupied was not scrapped entirely however, rather it was transformed into the post of executive director which Charles Glock acceded to in July 1950. He was permanent chairman of the directorial committee and responsible for implementing decisions taken in that committee. This more formalised hierarchy and committee framework was needed in the interests of coordination. No longer could one individual's personal authority span the various divisions which had been added to the 'Bureau's' activities. Whereas the 'Bureau' had originally developed on the basis of mass communication research, so far as Lazarsfeld was concerned, that field had been exhausted by 1943,<sup>1</sup> and his research interests began to move elsewhere. The addition of Merton as Associate Director in 1943, served to widen the 'Bureau's' range of interests even further, and it was Merton who, according to Patricia Kendall, in 1944 suggested the change in title from Office of Radio Research to 'Bureau of Applied Social Research' as a more accurate symbol both of the existing nature of the institute and of its planned development.

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1 On 29:3:1943 John Marshall sent a letter to Lynd, stating, "I suspect that yours, his and my views of the situation are not identical". What this refers to is Lazarsfeld saying that communication research has "Come to the end of an era" and "One doesn't know what to do next". By 1943 Lazarsfeld thought the field was exhausted in the terms within which they had been operating. A fact which is particularly noteworthy given Berelson's statement on "The withering away of mass communication

With the diversification of research operations new divisions were added and in the process, a new middle range of roles between the directorial level and that of trainee was created. The gradual emergence of a career structure involving more definite steps in the hierarchy,

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# 1 Cont...

research some sixteen years later. (Berelson 1959)  
 This is not to say that the 'Bureau' and Lazarsfeld himself did not continue for sometime in the field; they did, but the fact remains that Lazarsfeld could not see it advancing much along the lines the 'Bureau' had instigated and a one which dominated the Field. In conversation with the writer Lazarsfeld admitted that institutional studies would have been a new and worthy area of study, but that he himself was both uninterested and without knowledge in that area. In parentheses, it might also be added that such studies would have held little 'methodological' interest for him. However he was critical of those who continued in the 'old tradition' for example, his afterword of Gary Steiner's book "The People Look at Television" (1963) should be read in this light. The afterword was originally intended to be the foreword to the book, but one gathers, almost as a disclaimer that such a book should have been written. Instead, Berelson wrote the foreword. Steiner was Berelson's protege and Berelson in his eulogistic foreword at one point states, "A few years ago (referring to his 1959 article) I had occasion to make some critical remarks about the present state of communication research. Had this study been available, then I would have had to qualify a part of what I said" (Berelson 1962 :X ). By the time the book was published Berelson had resigned as Director and left the 'Bureau'; However the foreword and afterword can be read as taking the Lazarsfeld-Berelson split (mentioned later) into the public arena. Allen Barton and Rolf Meyersohn agreeing with the Lazarsfeld position had worked on the 'draft' of the foreword and then given it to Lazarsfeld to work on. But Steiner, Knopff the Publisher and C.B.S. who had funded the study wanted nothing to do with the foreword, but according to Barton, were forced into placing it as an afterword. This was a particularly bitter period between Lazarsfeld, Berelson, and Lazarsfeld, true to his position then, still considers the study should have been totally different. Indeed, in conversation with the writer Lazarsfeld was critical of another ex-student's work, Robert Bower's Television and its Public. Lazarsfeld simply considered that the studies had not progressed much from those which the 'Bureau' had conducted in the late thirties and early forties.

consequently lessened the need for personal allegiance as a factor making for organisational stability and continuation. Furthermore, the increased clarity of a career structure meant that the 'Bureau' could draw on personnel outside its own locale of personal contacts. Since it fitted into a pattern of developments taking place in other research centres the result of this process meant that a much more stable framework of operations emerged since not only could staff move within the organisation but also between organisations. This emergence of a middle range of roles within the 'Bureau' was of singular importance in filling the gap between the higher and lower echelons and providing organisational linkage and progressive training. Indeed, it was out of this middle range that the new breed of research personnel was to develop who went on to populate other such centres at a senior level. The actual structure of the 'new' Bureau was as follows. At the head of every research division was a division director who was also on the directorial board. He provided general supervision for all the projects engaged in by his division; the division itself representing some particular area of research in which he was intellectually interested. Below him were the project directors; the new middle range of relatively mature personnel who, whilst consulting with the division director, were left in control of the operation of the individual projects. Under the project director there were assistant directors who were normally graduate students, but they in turn, could delegate the technical and more or less routine operations, to the central processing unit made up of graduate students engaged on work such as tabulating and statistical computation.<sup>1</sup>

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<sup>1</sup> See Appendix c for structure of the 'Bureau' in 1951 and Appendix d for the structure in 1960. By comparing the two, one can see that although more divisions have been added, it has remained basically the same. That is, a block hierarchy.

Although the development of the 'Bureau' on the university campus presented the administration with problems of incorporation, the intellectual suspicions of the teaching staff also had to be overcome before acceptance would follow. The intellectual and administrative barriers to acceptance cannot be easily separated however, since the 'Bureau's' structure stemmed directly from the nature of the work in which it was engaged. Consequently it is not easy to distinguish between intellectual objections as such, and those arising from the general form and conduct of the 'Bureau'. Lynd, as Chairman of the sociology department, adamantly defended the 'Bureau' to the Cheatem committee stressing the contribution that such an organisation could make to sociology as a discipline. His letter reads:

"The sheer difficulty of gathering data of a new sort has meant that in the past sociology has stressed over-much theory and generalisation by data. This has given the discipline a bad name at a time when economics has been forging ahead as an empirical science supported by far more ample government and business collected by statistics. History likewise tends to look down its nose at sociology from the peak of its complacent confinement of its own efforts to presented library materials. It is precisely in order to get away from this kind of different situation in a new and sprawled field that we have stressed in our department the Bureau of Applied Social Research. If sociologists are to use empirical data they simply have to go out and dig them up. And that is precisely what the Bureau is doing"

(Lynd 31:1:45)

However, whilst Lynd supported the 'Bureau' in face of the inquiry, his general attitude was a good deal more ambivalent than the above question suggests. The following extract from a conversation with Patricia Kendall, captures very well the schismatic attitude towards the 'Bureau' that existed within the sociology department among both staff and students.

D. Morrison: Perhaps you could talk about the schisms that did develop within the sociology department?

P. Kendall: Well it was at two levels. Some of the faculty in the department didn't see anything particularly useful about the 'Bureau'. Robert Lynd for example; he was a great friend and sponsor of Paul's, he felt the 'Bureau' was a trivial enterprise and discouraged people from working in the 'Bureau'. I had a university fellowship at one point, and Lynd got quite excited.

I was working at the 'Bureau' in addition to having the university fellowship, because the fellowship stipulated that I could not do any paid work for any organisation. So he was very much on the look-out, watching what the 'Bureau' was doing, and some of the students felt the same way. They did not want to work at the 'Bureau'.

D. Morrison: Yes, among the sociology students...what was the reaction of your peer group about going into such an innovative field?

P. Kendall: Well, as I say there was a schism between the students who were theoreticians and didn't want to have anything to do with the 'Bureau' and those of us who were more empirical, and wanted to work on these kind of studies; even though we were doing the most menial kind of tasks. Seymour Martin Lipset for example never worked at the 'Bureau'. Much later in 1957 he did his union democracy study through the 'Bureau' but when he was a student - he was a contemporary of mine - he did not work at the 'Bureau' and didn't want to.

D. Morrison: Because of the quantitative work?

P. Kendall: Yes. Some of us were very pleased to work there. I'm sure I started publishing much earlier than my class mates. I started to publish with Paul and Bob Merton material at the centre.  
(Kendall 9:6:73)

Although criticisms were made against the 'Bureau' for being too quantitative, the question of attitudes is much more complex than that. Certainly, the more theoretical elements within the sociology department could find ready criticism of the 'Bureau's' emphasis on empirical work. Yet, it would be wrong to see the criticism polarising simply around the theoretical/empirical divide. For example, although it would seem that Lynd had reservations concerning the 'Bureau', he had nevertheless sponsored Lazarsfeld's place at Columbia in opposition to the more speculative sociologists proposed by McIver. One would wish to argue that, quite apart from the overt intellectual dislike of empirical work in some quarters, an important objection to the 'Bureau' was the manner in which it proceeded; that even where members could not object to empirical sociology there existed a disquiet about the nature of the empirical work undertaken, and its

framework of operations. (This will be gone into shortly in discussing the Cheatham committee). Lazarsfeld recognised the criticisms of triviality and was well aware that some of the studies might appear to be insufficiently academic. He knew that if the 'Bureau' was going to legitimate itself in the eyes of the University, its work had to be seen as intellectually respectable. Thus, as a former 'Bureau' staff member informed Leiba Brown,:

"There was continued pressure to keep academic. We all looked down our noses at our bread and butter. Lazarsfeld would say 'make something out of this'. We tried to pull our market research into an academic context. The university questioned the commercialization of the 'Bureau', yet forced us into it....There was an effort all the time to academize it,,," (Brown undated:7)

Lazarsfeld is on record as stating that seemingly trivial studies could produce important generalisations, and as noted earlier, there are instances which bear him out. Nevertheless such a position also acted as a rationalisation which put a convenient academic gloss on the institute's financial dependance on market research work.

Certainly, by no means all the studies lent themselves to generalisation as Patricia Kendall pointed out:

P. Kendall: Some of us disapproved, no not disapproved, felt reluctant about some of the studies that were taken on. The first study that I ever did on my own was Sloan's Linament. I felt it was a pity...it was a good experience for me. I did everything from beginning to end. I felt it was a pity, it was a good study. A pity that it had to be on something so trivial as that. But I knew if I was to say at the 'Bureau' my salary had to be paid.

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1 There were very few student fellowships on scholarships until the 1950's; Thus the 'Bureau' was a place where the student could earn extra money, an economic fact, which made for a ready pool of workers.

D. Morrison: You couldn't see it generating generalisations?

P. Kendall: Yes, it was a very trivial study, a very small study. Besides from the fact that it gave me experience and brought in a small amount of money to the 'Bureau' it was very hard to make any generalisations" (Kendall 9:6:73)

To be fair, Lazarsfeld would not claim that higher level generalisations can be developed from a single 'trivial' study. Rather he saw them emerging through the development of integrating constructs from a number of such studies. However, there is no evidence that such a practice was pursued in any systematic fashion at the Bureau. The actual benefit of such contracts and the reason for their acceptance was that they facilitated student training and provided needed income. The university's stance was therefore somewhat contradictory in that it questioned the commercialism of the 'Bureau' yet at the same time forced it into such a position by its refusal to provide substantial financial support. The outcome was that the 'Bureau's' basic financial insecurity resulted in practices which ran counter to prevailing perceptions of academic propriety. However, the intellectual antagonism towards the 'Bureau' weakened considerably after Merton joined in 1943. Although the style of operations never really altered Lazarsfeld had, by such a recruitment, managed to secure a particularly powerful ally from the camp of the theorists and in doing so blunted the more direct criticisms of rank empiricism. During the course of a discussion with Bernard Berelson, the question of the somewhat hostile reception to the 'Bureau' was raised and whilst Berelson recognised that there were genuine intellectual objections, he also argued heavily that these were heavily fudged, if not overlain by considerations of a more personal nature, rooted in the faculty's distrust and resentment of Lazarsfeld himself.

"Well the academics didn't really trust him. He was -- well you know how it is with academics -- he was too pushy, he was foreign, he was too bright, he was too self confident, arrogant sometimes to them, and too tied in with the business and commercial world. And he was supporting this personal institute of his which attracted all sorts of bright young people around Columbia, and this was a source of resentment. With commercial contracts he was always wheeling as he was doing it -- as indeed he was -- with a kind of sleight of hand. You know the joke around the Bureau was that, you paid the deficit of the last study with the grant for the next study, that's how people lived around there. But it wasn't a political criticism, and in many ways it wasn't even -- I think it's fair to say -- a scientific or academic criticism, it was a little that .. that it wasn't theoretical enough, it was all dirty empiricism, fact grubbing and so on. It was a little unfair, but a large part of it was sort of personal on a grand scale." (Berelson 12:7:73)

Berelson suggested that Lynd, whilst always being sympathetic and helpful, had reservations of a political nature concerning the 'Bureau'. Whilst Lynd never really understood what was going on in the 'Bureau' he nevertheless considered that important methodological advances were being made for the betterment of American sociology, but, at the same time was slightly concerned over the question of academic freedom. Presumably this concern stemmed from the 'Bureau's' close relationship with the world of commerce, and it is certainly true that such financial links did tend to prejudice the 'Bureau's' position vis a vis the University. Yet, as repeatedly stressed, it was the location of the 'Bureau' on the periphery of the University which drove it into such commercial contractual relationships. Indeed, this was exactly the point which Lazarsfeld particularly emphasised to the Cheatham Committee when it was set up to examine the whole position of the 'Bureau'.

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1 Indeed Lazarsfeld accepts this in his memoir in footnote 49, page 310, when he states, "At the time when I turned over the Directorship of the Columbia Bureau to my successor, Charles Glock, we had accumulated a moderate deficit, which I, as usual counted on covering with funds from future studies. The change in directorship was taken as the occasion for a financial review, and the University preferred to cover the deficit from general funds, so that thereafter a stricter accounting system could be set up." (1969)

The Mills and Cheatham Committees: A Crystallisation of the Situation

It cannot be said that the Mills Committee which reported in 1944, or the report of the Cheatham Committee in 1945, offered the 'Bureau' any definite solutions, or even suggestions, by which it could overcome the difficulties of its position. What they did do was to generally ratify its existing practices by laying down a codified framework of procedures. The interesting features of both Committees, but particularly the Cheatham, is not so much the final recommendations but the display of concerns and the 'Bureau's' argued defence of its practices.

Frederick Mills, the economist and chairman of the executive committee of the Columbia Council for Research in the Social Sciences, was commissioned by that body on May 18th 1944 "to make a general study of the present status of research in the social sciences in the university, its future support and the best organisation for such research in the university, with authority to make commitments in connection with such study after consultation with the Dean and Provost".<sup>1</sup>

Having surveyed the field he recommended on August 1st 1944 that a .... "special committee of the Council be created to supervise the Office of Applied Social Research and other agencies of the same sort that may operate under the auspices of the Council ..... This supervising committee should give particular attention during the present academic year to an evaluation of the work of the Office of Applied Social Research, and to means by which the work of this office might be more effectively related to other Columbia research activities". (Council Minutes 1:8:44)

Following certain minor revisions, most of which were of a semantic nature and not of principle, Mills submitted the final recommendations to the Council on October 25th 1944 where it was agreed that: "the Council appoint

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<sup>1</sup> Taken from the Minutes of the Council for Research in the Social Sciences, Columbia University 18:5:44.

a Committee on the Administration of Social Research Agencies". The Committee was to have general responsibility for the supervision of research agencies which operated by the authority of the Council; namely the 'Bureau'. The specific duties of this body were to be:

- a) To approve the formulation of appropriate procedures for the administration of research agencies operating by authority of the Council, and from time to time to review the execution of these policies.
- b) To formulate general policies concerning the public relations of research agencies operating by authority of the Council and to advise such agencies concerning the application of these policies, with special reference to the public use of the name of the university.
- c) To check on the degree to which research and research training are maintained as the central objectives of each agency. This will involve an occasional examination of the projects undertaken by such research agency, whether on its own initiative or on solicitation by outside organizations or individuals. The Committee will be available for consultation by the Directors of research agencies when individual projects are being considered.
- d) To relate and coordinate the work of research agencies with other research activities of the University.
- e) To make periodic reports to the Council on the work of all research agencies operating under the authority of the Council. (Council Minutes 25:10:44)

The above conditions laid down by the Council were in principle intended to apply to any agency operating under the auspices of the 'Council', but in fact, the only such agency in existence was the 'Bureau'. Although the above were the "specific duties" of the Committee, the Council also laid down "general principles in the acceptance of research contracts with organizations and individuals outside the university". Mills had written to Lazarsfeld on the 12:6:44 that "there can be no question as the desirability of continuing the work you are doing.

The problem of financing is one the Council will wish to give further thought". However, the financial principle which the Council accepted was not one which Lazarsfeld wished for, since it did not free the 'Bureau' from its dependence on contract work. The Council agreed that: "Emphasis will be on training for research. Conducting work under contracts with commercial or other organisations will not be considered inconsistent with this condition provided the research emphasis be maintained". (Abstract of Council Minutes 25:10:44). Such a principle ratified the process by which the 'Bureau' was managing to sustain itself financially. Nevertheless it relieved none of the pressures, the fact that such practices were recognised as legitimate, ensured the continuation of the 'Bureau' as a research entity. Indeed, despite his disappointment at the University's failure "to accept the positive duty of integrating into its general institutional programme training in empirical social research", Lazarsfeld recognised the importance of such a decision, and considering it to be "a real turning point in the history of American Universities" (Lazarsfeld 1969:332). However, the question of the 'Bureau's' relationship to the University did not rest there. The Committee, which had been established by the Council's resolution of 25:10:44 began a very thorough investigation of the 'Bureau' and its coordination with other research activities in the University, with the intention of formulating principles for their guidance. The Committee was headed by Elliot Cheatham, a professor of Law and assisted by Ralph Blanchard and Arthur Macmahon.<sup>1</sup>

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<sup>1</sup> The Committee was set up according to Lazarsfeld because "we wanted Columbia to accept the positive duty of integrating into its general instructional programme training in Empirical Social Research. We protested the Committee's failure to respond to this possibility, and, as a result, the Council appointed a special Committee (Cheatham) to decide on the role of the 'Bureau' within the structure of the University. (Lazarsfeld 1964:332,333)

In all, the Cheatham Committee produced three reports which it submitted to Dean George Pegram, Chairman of the 'Council', on 15th May 1945.

However, in the present context the writer wants to focus particularly on the correspondence, and memoranda surrounding the Committee, since they provide clear insights into the kinds of concern generated by the 'Bureau's' existence, and the strategic stances adopted by Lazarsfeld in attempting to convince the University of the 'Bureau's' compatibility with its own goals of research and teaching.

Lazarsfeld with his accustomed political skill argued from an offensive rather than a defensive position. Whilst he himself was only too aware of the 'Bureau's' faults when viewed from the University's position, this did not bar him from laying the blame for some of the shortcomings on the University's doorstep. It may well be that Lazarsfeld, after having struggled so hard to establish 'his institute', felt personally indignant that others could not, or would not, accept such an innovation as an integrally necessary part of modern social research, which was entitled to a fully integrated position within a modern university. Certainly, some of his correspondence to the Committee has an air of aggressive 'righteousness' about it. But, that notwithstanding, given Lazarsfeld's known administrative and political acumen, the word tactic is perhaps a better description for the course which he followed in his effort to justify the presence of the 'Bureau', and secure its full recognition by the University. Basically, he sought to demonstrate that the high quality of the 'Bureau's' research was entirely compatible with the University's own standards, and that

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1 Frederick Mills and Robert Merton aided the Committee by conducting a survey. This had been passed at the meeting of the Executive Committee of the Council on Research in the Social Services 1:8:44. Mr. Wallace proposed the motion and was seconded by Mr. Tannenbaum that "The Committee authorize Mr. Mills, with the assistance of Mr. Merton to assume responsibility for a survey to be relieved of some portion of their course assigned (minutes of Executive committee of the Council 1:8:44. The survey was not restricted to the 'Bureau' but applied to the Social Sciences in general within the University.

considerable benefits would accrue to the University by having such an institution on the campus. As stated, the general tone of Lazarsfeld 'evidence' was one of forceful confidence, rather than defence or apology. He was greatly supported in this tactic by the fact that the University had already 'accepted' the presence of the 'Bureau', in principle, and what was being negotiated were the terms of that presence. to her paper on the 'Bureau' comments that:

"Part of the Bureau's success in establishing itself at Columbia resulted from the fact that it encountered no overriding opposition by the University administration. Therefore the Bureau was able to utilize the power vacuum that often exists between the administration and the department to push its own cause. Had there been a "strong man" in the administration opposed to the Bureau, much more political manoeuvring and many more defensive moves would have been necessary to achieve the same goals". (Brown date unknown:16)

To be sure, Lazarsfeld has previously been quoted as lamenting the fact that a power vacuum has developed within the modern university, and it can be tentatively posited that his several references to such a phenomenon stem in part from his experiences gained during this period, or at least, that they made him extremely sensitive to such a situation. If the power vacuum existing within the University enabled the 'Bureau' to establish itself to the extent that it did, it was that same power vacuum which worked against the total integration of the 'Bureau'. Whereas a power vacuum may well be suitable for someone as determined as Lazarsfeld to gain adjustments or accommodation within the existing structure, it required the purposeful presence of authoritative leadership to re-map institutional arrangements in the manner which Lazarsfeld would have preferred. In the absence of such leadership, Lazarsfeld was unable to achieve the long term solution of total integration into the University which would have rid the 'Bureau' of its ad-hoc operational practices and many of the problems which flowed from them. This situation helps to

illuminate Lazarsfeld's strongly expressed regret over the wedge driven between administration and scholarship and the resulting absence of the 'intellectual' leadership necessary to carry through far reaching educational reforms.

Tactical Manoeuvring and Reasoned Arguments

Lazarsfeld informed the writer that preparing and writing the material for the Cheatham Committee, "was the core of my life for six months" (Lazarsfeld 2:6:73). Indeed, the amount of material still in existence shows quite clearly how seriously Lazarsfeld regarded the Committee and the extent to which he applied himself to the task of justifying the 'Bureau' in an effort to extend its position within the University. Much of the material surrounding the Committee's enquiry, not only illustrates Lazarsfeld's line of argument, but also has the additional benefit of providing substantial information on the operation of the 'Bureau' itself. However, given the nature of the situation, not all the material can be accepted as truly representative of operating practices. That is, some of Lazarsfeld's reports to the Committee ought to be seen as 'public' or 'diplomatic' statements, intended to advance the image of the 'Bureau'. For example, after reading some material which Lazarsfeld had sent to Cheatham, where it was implied that the 'Bureau' saw itself as gathering sociological data on an almost data bank basis, this 'anomolous' situation was raised with Professor Lazarsfeld. He replied "that was the correct thing to say. Look, the best thing is we never did it." (Lazarsfeld 2:6:73)

One of the most powerful lines of approach adopted by Lazarsfeld was to impress upon the Committee the benefits to the University from the presence of a social research institute such as the 'Bureau'. Appealing to their pride in the University's prestigious reputation Lazarsfeld wrote: "It should be noted that similar laboratories will doubtless be established in other universities.

The Bureau seeks to give Columbia University a pre-eminent place in a development which is also certain to occur throughout the academic world in the next few years."<sup>1</sup> Further, on February 27th 1945, Lazarsfeld, in response to a general survey of social research activities in the University,<sup>2</sup> wrote a particularly long report in which he constructed "A typology of research studies in our field" and then proceeded to list the types, "in order of their requirements for an organised research laboratory within the department". It is not necessary to discuss Lazarsfeld's typology in detail, but what is of interest is the manner in which he uses the opportunity to demonstrate the various types of research undertaken within a research bureau. Cleverly using the works of two most senior professors in the sociology department, Lynd's 'Knowledge for What' and McIver's 'Social Causation' as examples of work "essentially done by an individual scholar", he goes on to note:

"Such work, necessarily individual, is also found in the purely quantitative field. The development of a new statistical formula or the abstraction of a new concept from census material often has to be carried out in toto by the scholar himself. This individual work is always necessary where no direction which would lead to comparable results by other and probably less trained workers can be devised"

Whether it was Lazarsfeld's intention to collapse myths about all work in a research bureau being group work is difficult to say; however, the function of doing so is obvious. Proceeding from individual work,

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1 Columbia University Bureau of Applied Social Research. Its objectives and purposes". Undated, but probably written in the early part of January 1945.

2 The Mills - Merton Survey which was handed over to Cheatham.

Lazarsfeld's next type, and "the first step to such organised research" is when "a professor can profitably use assistants for the elaboration of specific points in his research". Commenting on this type of work Lazarsfeld notes that it is "possible in those situations where a professor has developed ideas to a point where he could turn to further developments on the fringes of his own abilities if he had assistants to test or document the previous phases of his work." Gradually then, Lazarsfeld presents a general picture of social research which at no point is inconsistent with work associated with a research bureau. He goes on to offer examples of work where collective organisation is not only beneficial but absolutely necessary. Thus, in discussing his third type he states:

"We come now to a type of study where the professor needs the assistance of a considerable number of people able to perform rather routine work. Suppose for instance, that he has developed an attitude scale and now needs a thousand interviews made with specified types of respondents. Or, suppose that there are a large number of correlation coefficients to be computed, or a lot of records in some archives are to be copied off, or material is to be translated. This is a well-known type of organised research where the help needed is usually called "charity assistance". Very often one will not even use students for such work, although it is probably a useful part of their experience if they have done such routine work at least for a few weeks".

Lazarsfeld's research typology is therefore one of progressive complexity of organisation leading inexorably in the direction of the hierarchical structure exhibited by the 'Bureau'. Thus, using "charity assistance" as the example, and his work with the National Youth Administration as the case, Lazarsfeld presents his fourth and final type of research stating:

"Only if the work performed by clerical help is of a very standardized nature can it be performed satisfactorily. The moment it needs much supervision the entire system breaks down because the professor cannot possibly supervise 20 to 30 workers; the N.Y.A. however, never provided for adequate supervisors and that destroyed much of the value of its work. Thus we come to the last and most interesting kind of organised research. The empirical studies which will be the standard of the future are characterised by a

hierarchical structure of skills".

Hence Lazarsfeld's classification of research moves from the completely individual type to that involving a complicated administrative structure. Furthermore, he could, and indeed did, present the 'Bureau' as engaging in all four types of work. Yet at the same time he was careful to emphasise that the last type of work, -- "the empirical studies which will be the standard of the future" -- were best suited to a research bureau, and "best exemplified by the Bureau of Applied Social Research".

In addition, by presenting types of work as the most significant factor to be taken into account, Lazarsfeld could easily demonstrate the collaboration possible between the Department, the 'Bureau' and other agencies. However, negotiating the enquiry did present some very real problems for the 'Bureau', particularly in the area of its commercial enmeshment, and its use of students. There was no escaping 'the Bureau's' relationship to commerce, and even though the Mills report allowed for commercial contracts, the point was still a sensitive one. For, in the final analysis many of the commercial contracts which the 'Bureau' accepted during its early years had little academic merit to recommend them, but were undertaken as a valuable source of needed income. In a letter written in January 1945 Lazarsfeld explains this to Cheatham, but even so, a defence based on academic grounds was also included: "The present Bureau was built up around an original Rockefeller grant, without budgetary assistance from the University. Service jobs for commercial and government agencies were the main source of income. Practically all of these studies had scientifically valuable aspects, but only surplus time and money could be devoted to completely scientific purposes" (Lazarsfeld 30:1:45). However, on the very next page of the letter Lazarsfeld obviously sees fit to mention the changing nature of the Bureau's work, stating that "While our budget of several years ago was pieced together from a dozen little studies, today we have a few large grants which represent a marked shift from commercial to foundation funds."

This question of Lazarsfeld's involvement with the world of commerce requires further expansion since not only did it bring him repeated criticism from erstwhile colleagues, but in addition, produced reservations on the part of the University. It is not that Lazarsfeld somehow actively enjoyed the sponsorship of commerce for he, above all, knew from experience the academic limitations of many of the contracts thus obtained. But, as a person committed to the advance of empirical social research, then, if the solution was to accept commercial contracts as a means of establishing the desired organisational structure within which such work could be fostered, he was quite prepared to compromise the ideal situation for actual working alternative. He states as much in a memorandum written to Lynd and Merton in 1943:

"I consider myself mandated to build up a self-supporting research outfit connected with the Sociology Department. It would obviously be more desirable if our department had twenty thousand dollars research money to run a "laboratory" on its own terms. But it would be very bad if our students and we, ourselves, couldn't do empirical research at all. The present course is therefore a compromise and as far as I can see it is also viewed in this way by the Rockefeller Foundation."

(Lazarsfeld November 1943)

The fact that the 'Bureau' was self-financing created untold worries, problems and limitations for its personnel, but as Director, Lazarsfeld suffered most from such financial practices. His feelings towards commercial work is well captured by his statement to Elliot Cheatham that: "It is obvious that I, as well as all my associates in the 'Bureau', would much prefer to be free of all commercial entanglements. Nothing would make me happier than to be able to spend all of my time on actual research, without the necessity of worrying about public relations, negotiations for funds, and the like". (Lazarsfeld 30:1:45)

Obviously then, the "compromise" of the dealing with commercial contracts,

was not to his liking, but it was a compromise that provided a material base with which to support individual researchers and upon which empirical work could move forward. Furthermore, ever optimistic, he hoped "that in the end it (the Bureau) will be converted into a regular part of the Department's activities and budget".

Yet, Lazarsfeld had none of the distaste for commercial involvement that often marks the liberal scholar; since his personal experience now vividly brought home to him the need for a steady income. Not only had he been witness in his youth to poverty, far worse than that occurring in the rest of Europe or America, but in addition, he had experienced the material uncertainty of most emigres lacking a secure position, or reputation. Thus, through a rather harsh historical schooling Lazarsfeld had cultivated a fine understanding of the necessity for a firm material basis for intellectual work.<sup>1</sup> Consequently, as Berelson noted, Mills' criticism of commercial funding was beyond Lazarsfeld's comprehension, and further, as the incident related to the writer by Paul Neurath shows he was angered by his critics' refusal to appreciate either the objective situation or the very real benefits which could be extracted from it. Neither can his enmeshment with commercialism be taken, as Berelson suggests, as evidence of his lack of European Socialist conviction. No doubt they modified or even changed within the totally different historical situation of America, but the writer would argue, that even his European Socialism, or more specifically his Austrian Socialism, gave him none of the nice purism often associated with the liberal intellectual, or the strident righteousness which Berelson probably

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1 For a good personal discussion of the position of the exile, especially his financial insecurity, see Henry Pachter 'A Memoir'. He notes that, "The myth that exile produces Dantes, Marx's, Bartok's is not justified in the mass. More often exile destroys talent, or it means the loss of the environment that nourished the talent morally, socially and physically" (1970:17)

associated with dedicated European Marxists. The lessons to be learnt from Austrian Socialism were the need for organisational survival in the face of unfavourable circumstances, the ability to drive a bargain from a position of weakness and above all the need to accommodate to circumstances. There was not much that was sacred when put to the test of objective conditions, yet, pragmatic means continually threaten to compromise the ends themselves. To take the case of Lazarsfeld: To many it may well have seemed that the goals of scholarship and student training were jeopardised through the acceptance of commercial contracts. However, the contradiction is that those very contracts also allowed the possibility of those goals being achieved. Indeed, Lazarsfeld's insistence that rigorous standards of scholarship be applied to even the most trivial of studies stems not only from the desire to make the 'Bureau's' work respectable, but it could also be argued, from the recognition that the acceptance of such contracts threatened the goals of scholarship. It would have been very easy, and no doubt tempting, to relax his own high standards when dealing with such work, and merely use them as a source of valuable income. The fact that he did not, supports the above line of argument. Not only did he resist to the greatest extent possible, the threat such contracts represented for scholarship, he utilised many of them for the furtherance and underwriting of the goal of student training. Whilst Lazarsfeld could certainly present much of the 'Bureau's' work as easily compatible with the University's scholarly standards, in the realm of student training the matter was much more complex and difficult. Elliot Cheatham expressed severe reservations concerning the 'Bureau's' use of students, and wrote to Lazarsfeld informing him that:

"..... in so far as the students of the University have a part in the work of the Bureau, the time devoted to that work must be as valuable proportionately

in the education as the rest of their university time. If that is not so, then any university bureau is misusing the students. Ten or fifteen years ago we in the law school had a somewhat similar matter before us in connection with legal aid work at Greenwich House. All of us on the law school committee considering the matter were agreed that the law students should not be permitted to do such work unless the educational objectives were uppermost, even though we realised the advantages to the community and to the Clients which could come from a wider legal aid service."

(Cheatham 1:2:45)

Cheatham's position was entrenched enough for him to state, "I do not believe my notions will change in the course of our discussion."<sup>1</sup>

It is certainly true that the use of students was problematic, and that Cheatham's veiled suggestion of 'exploitation' had a certain force to it. Yet even if 'exploitation' is the correct word, such a situation flowed from the research setting and not from any deliberate practice.

For some students the research training was probably thorough and valuable in educational terms, whilst for others, it no doubt provided little more than a useful source of income. The clash with the University was based around the fact that although in principle student training was an important goal of the 'Bureau', in practice it occupied a position of low priority; whereas, for the University, the goal of a student training was dominant not only in theory but also in practice. As long as the 'Bureau' sustained itself by contract work there was a constant tendency to use the students for the contribution they could make towards the research process.

Projects were not selected with students needs in mind, rather, the students

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1 The 'discussion' referred to related to a lunch date which Cheatham and Lazarsfeld were to have on the 9th February. It is interesting to note that Lazarsfeld sent a copy of Cheatham's letter to Lynd, as he had with his own letter to Cheatham of January 30th. Lynd returned Cheatham's letter and scribbled across the top, "admirable letter from a civilized and humane man who carries his own supergo! We can work with him."

were selected according to the 'needs' of the projects. Thus, although it would be an over-statement to say that students received their training as a by-product of the research process there was nevertheless a strain in that direction. Furthermore, the student who fitted into the research process as a valuable functioning member was more likely to further his own educational development than a student who did not. Personality factors aside, it was much more likely that the 'gifted' student either through sheer intellectual ability, or due to an already existing knowledge was the most likely to fit in and thereby benefit intellectually. In other words, a reverse educational process was in operation. Although it is always likely that the most gifted students benefit in educational relationships because of the orientation towards research rather than training, this process reinforced at the 'Bureau'.

From Lazarsfeld's point of view, the real difficulty with student training was that it is very expensive for the 'Bureau'. It takes at least three months before a student can turn out any kind of useful work. The mere guidance of training requires half-time of a staff member; his salary has to come out of commercial earnings of the 'Bureau' (Lazarsfeld 27:2:45) Lazarsfeld continues by stressing that, "pure training work, in itself, does not create any problems beyond the budgetary aspect". However, he did acknowledge the 'Bureau's' limitations as a training institution which the writer has mentioned in previous sections; most notably the retention of students on routine work rather than their progression to tasks of increasing difficulty. It is in this respect that Cheatham probably feared that the 'Bureau' was "mis-using the students". However, it is worth remembering that the criteria of misuse was whether the work undertaken was, "as valuable proportionately in the education as the rest of their university time." Of course, it is easy to be flippant, and demand to know educationally how valuable the rest of the students' university time was, for the fact remains, that even though one institutional setting was distinctly geared to education, that in itself guaranteed nothing.

Nevertheless, the principle stands, and there was a recognition on the University's part that training within the context of the 'Bureau's' operations did not have sufficient safe-guards to protect educational goals. Lazarsfeld's main rejoinder, and perhaps the most valid, was to stress that the student benefitted by being involved and having contact, "with research workers who are more mature and advanced" than he. Even so, the cash nexus of the arrangement that students had with the 'Bureau' was a source of alarm to many. To be sure Lazarsfeld recognised the existence of different levels of motivation relationship and stated, "the situation is more difficult if a student works for pay because then his training can only be a secondary goal. But, even that is much better than if a student works his way through school by washing dishes in a cafeteria where no one raises the problem of educational values". "at a few points in the Mills-Merton memorandum Lazarsfeld stresses the financial benefit to the student of an operation such as the 'Bureau'." For example, whilst admitting that at the time of writing, "there are jobs galore", he continues by reminding his readers of the services which the 'Bureau' has provided in the past:

"It should not be forgotten, however, that up to the beginning of the war the 'Bureau' was one of the few places where students could get work that was somehow related to his field of study. A large number of cases could be traced where students could finish their studies only because they worked here. It might be worth mentioning that in former times the 'Bureau' was practically a life-saver for many refugee professionals; this of course was partly due to the biography of the Director"

(Lazarsfeld 27:2:45)

It is interesting that Lazarsfeld should have considered it "worth mentioning", that in former times the 'Bureau' had been "practically a life-saver for many refugee professionals". Why should it be worth mention since it had little direct bearing on the matters under discussion?

Two suggestions can be put forward for consideration, both of which shed light on Lazarsfeld's thinking. Firstly, one must return to Lazarsfeld's sensitivity towards economic instability. Clearly, the refugee situation was historically specific and extremely unlikely to present itself again in the form that it did, yet, having been psychologically, and to a certain extent physically,<sup>1</sup> part of that diaspora he was well aware of the disruption and threat to scholarship deriving from economic impoverishment.

As Berelson noted, it was certainly true that in the early days of the 'Bureau', "there were always a few German and Austrian refugees doing coding around the 'Office'. Everyone always thought how humanitarian of Paul to take in these socialist refugees and just give them something to do, just to keep them alive" (Berelson 12:7:73). His personal appreciation of the necessity of financial support for scholarship, was further reinforced by his Newark experience with the National Youth Administration programme, and the opportunities that certain studies had provided for the useful employment of students. Consequently, drawing on his Newark experience and discussing the menial nature of "charity assistance" work, he commented to Merton and Mills that, "During depressions, however, such work might be an important source of self support for students. The N.Y.A. projects probably consisted, in the main, of this kind of work". It is difficult to say whether this simply provides an entry into Lazarsfeld's thinking in the sense of articulating 'worries' stemming from his own biographical experience or if he was skillfully off-setting criticism over the employment of students on any kind of menial work where the educational benefits were somewhat limited. Perhaps both are operative. But, even accepting that he was arguing tactically, upon what was that tactic based? The

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1 By "to a certain extent" is included since it will be remembered that Lazarsfeld left Austria before it became absolutely physically necessary.

question of material sustenance which the 'Bureau' provided was much more likely to be of significance to a person of Lazarsfeld's experimental makeup than to his American contemporaries - even though the universities were just emerging from financial difficulties themselves. The 'tactic' or line of arguing, was based upon Lazarsfeld's own feeling of righteousness. Interpretatively, it was an appeal for recognition of the problem which the 'Bureau' had faced, and of the work and general contribution that had been achieved in the face of these difficulties. Hence, the inclusion of assistance to professional refugees as being "worth mentioning". The struggle to establish this new form of academic institution cannot be overestimated, neither can the commitment he had to empirical work. Thus it is understandable that Lazarsfeld should feel some impatience at the authorities' response to 'his institute'. For here was a new and intellectually valuable contribution to the social sciences as such, servicing not only the community, but the sociology department as well, which in addition, had generously provided the money necessary for individuals to continue their academic life. In some ways the role it had played for impoverished students and refugees was the role it played for itself; that is, impoverished by the absence of University funds it had engaged in commercial work in order to sustain the organisational fabric which allowed it to engage in worthwhile sociological work.

#### A DIFFICULT PROBLEM OF APPROACH

The situation Lazarsfeld faced in his dealings with the Cheatham Committee embodied a central contradiction. On the one hand legitimation demanded that the 'Bureau' be presented in the most favourable light possible, yet at the same time, the flaws in its operational practices had to be shown up in an attempt to persuade the University to accept, either then or at some later date, increased budgetary and personnel responsibilities for it.

To steer a course between such imponderables was not an easy matter.

However, the approach Lazarsfeld adopted was to stress, as has been shown, the benefits of such a bureau to the University, then to point out the flaws, or the drift from the ideal, and proceed to lay part of the blame for such shortcomings on the University. As bold as this may seem, Lazarsfeld once again showed keen political sense, though the course he adopted did at times involve elements of brinkmanship, but whereas in the past, particularly during the Newark-Princeton period, Lazarsfeld was a relatively unknown figure in the American academic world, he was by this time a person of some note, especially within Columbia itself. Furthermore, he had the support of two other notable figures within the Sociology Department, Merton and Lynd. Thus, to a certain extent, Lazarsfeld was in a position of strength. Indeed, only a person who was confident of his position, or needlessly reckless, would have used the following negotiatory arm-lock. He wrote to Cheatham that "Perhaps the unorthodox operation of the Bureau creates problems which far outweigh its advantages. It seems to me perfectly legitimate to consider whether it might be better to close it and to wait until the time is ripe for it to be resumed on the basis, perhaps, of full support by the University". (Lazarsfeld 30:1:45) Lazarsfeld never had any intention of giving up the 'Bureau', such an action was not his style, and certainly would have been out of character with the single-minded determined innovator described in this work. He admits as much himself in his own 'memoir' when, before using the above quote, he states, "I then played my final gambit. Can a modern university really do without something like our Bureau? If not, could I take a change on something very close to blackmail? I tried along the following line". (Lazarsfeld 1969:334) It is likely that with the passing of time a slight histrionic element has crept into Lazarsfeld's account, since it will be remembered, that the previous committee headed by Frederick Mills had assured Lazarsfeld that "There can be no question as to the desirability of continuing the work you are doing".

Such commendation must have given Lazarsfeld confidence. In addition, it really is difficult to see the University wishing to disband the 'Bureau' - for after all, Lazarsfeld was right, the empirical element within modern sociology did require something akin to a bureau. A final part of Lazarsfeld's letter to Cheatham requires quoting to support the writer's interpretation, and that is, his statement that:

"Feeling as strongly as I do the need for having something like our Bureau as an integral part of the Department, I could actually have taken two courses. I could have spent all my energies in promoting the idea that the University administration should provide the necessary minimum budget of \$25,000 to establish such a social research laboratory. Had I succeeded, this laboratory would have been set up in an integral part of the University from the outset, and its work would have proceeded in familiar, academic form"  
(Lazarsfeld 30:1:45)

However, Lazarsfeld also notes, in commenting upon a slightly abridged version of the above quote, "I did not believe then, nor do I believe today, that there was the slightest chance that this would come about" (Lazarsfeld 1969:333:334). One could add further, that such an alternative probably never entered Lazarsfeld's consciousness initially. Certainly the 'Bureau' was not a product of planning in any formal sense, but rather the ad hoc outgrowth of factors previously mentioned. Indded, in answer to question four of a U.N.E.S.C.O. enquiry into social science research institutes "What were the reasons at the foundation of the institute for choosing its structural relations to other organizations and for possible later changes in this respect?" the Bureau's reply was that:

"The Bureau's structural relations to other organizations was not chosen in the sense that there were a range of alternatives that offered themselves for selection. The Bureau was evolved at a time when the usefulness of applying the social sciences to the illumination of contemporary problems was still relatively unknown. This coincided with the developed conviction that teaching the social sciences without a link to their

application was to exalt form over substance. Thus the basis of the Bureau's being, or its reason for being, had no relation to a then established sense of its value.

It was part of the training methods of a small nucleus of teachers; one might say more accurately, of Dr. Lazarsfeld himself. Thus he was not in a position to make a choice as to its structural relations; nothing nearly so formal was involved." (U.N.E.S.C.O. Report 1959)

Lazarsfeld more than anyone recognised the unplanned nature of the 'Bureau's' development. The manner in which he had guided its direction was one of skillful manoeuvring within a very restricted range of possible alternatives. Certainly, the freedom of action which he described to Cheatham had not existed. Yet, it appears to the writer that Cheatham slightly 'misunderstood' the nature of Lazarsfeld's letter of 30th. Lazarsfeld presented himself as a pioneering, over-worked and harassed bureau director whose efforts and success at establishing a research centre as a viable working reality were not fully appreciated. The 'blackmail' to which Lazarsfeld refers, is indeed a kind of blackmail, but the blackmail of the (supposedly) tired administrator refusing to go on, or at least questioning the point of it all unless he is relieved by the University administration of some of his problems: "I might mention incidently that even if nothing other than the problem of space were solved and the office were moved to Morningside Heights, many difficulties would disappear". It was a clever line of approach since although involving elements of brinkmanship it had the strength of appealing to the University for help in overcoming perceived operational shortcomings and at the same time indicating the basis of those shortcomings in structural terms. Thus, in a sense, Lazarsfeld had managed to shift responsibility on to the University. The ploys involved in the letter met with a degree of success in that Cheatham's reply was full of sympathy for the position of pioneer. Nevertheless, as stated above, a certain degree of 'misunderstanding' appears to have been present on Cheatham's part when he wrote:

"You have put most engagingly the position of the pioneer and creator, when much of his work has been done and he finds himself about to be subject to the ordinary rules of a placid society. Rightly he feels that his achievements must not be sacrificed to the mere niceties of the established order, which has forgotten that it too had its pioneering times...

Cheatham finishes his letter by saying:

"If you do not object, I will show your letter to Karl Llewellyn when our work is farther along, because he will enjoy it as a picture of an institution originating and then struggling on against efforts which seem to smother it even though intended only to make it conform" (Cheatham 1:2:45)

It is difficult from a reading of Lazarsfeld's letter itself, to see how Cheatham acquired the notion that somehow Lazarfeld was fearful of sacrificing his achievements, "to the mere niceties of the established order". One can only suggest that this stems from Cheatham's own legal background and his definition of his function as illustrated by the phrase: "struggling on against efforts which seem to smother it even though intended only to make it conform". That is, whereas Lynd praised Cheatham and said "we can work with him", a problem did exist in that it would seem that Cheatham, whilst being very sympathetic, interpreted his role as regulating the activities of the 'Bureau' and producing rules for its conduct. On the other hand Lazarsfeld wished the Committee to adopt a more dynamic and progressive position of not simply regulating but integrating the 'Bureau' firmly into the University's financial and academic structure. Thus, it can be suggested that Lazarsfeld's letter and Cheatham's reply both missed the central target of the other's concern. Lazarsfeld's tactic of presenting himself as a struggling pioneer ready to concede that, "perhaps the unorthodox operation of the Bureau creates problems which outweigh its advantages", was interpreted by Cheatham, not as an attempt to force the hand of the University, but rather as an innovatory bureau director demonstrating his spirit at not wishing to be restrained by an administration coming late on the scene after the major struggle for survival was over.

However, in the course of the Committee's enquiry there could have been no escaping Lazarsfeld's desires and expectations. For, in the early part of 1945 he sent Cheatham a document describing the 'Bureau's' "Objectives and Purpose" and noted that:

"No graduate school would think of having a department of physics which lacked a well equipped laboratory. Yet it is still considered feasible to have a department of sociology without any budget or personnel provided for empirical social research. In this sense actual developments in sociology are far in advance of the present administrative set-up in the typical department of sociology" (Lazarsfeld undated 1945)

Indeed, in response to the Merton-Mills questionnaire Lazarsfeld is even more pointed about the necessity for the University to provide for the 'Bureau', stating that; "It is a perfectly absurd situation when the Department has to earn the money necessary to run its research laboratory. There is an absolute need for a social research laboratory supported by the University on a regular budget" (Lazarsfeld 4:2:45) Lazarsfeld used this "absurd situation" to his own advantage in pressing the case of the 'Bureau'. Since, whilst holding fast to the core of his argument concerning the need for some institution such as the Bureau, he could point to the University's failure. That is, if certain arrangements were unsatisfactory, fault lay with the University and not with the 'Bureau'.

#### The Cheatham Report

The Cheatham Committee submitted three reports to the "Council on Research in the Social Sciences" on the 15th May 1945, but noted that: "The Committee's study and reports constitute only the beginning of an enquiry which may continue for years." The work of the Committee itself fell into three parts:

- 1) Consultation with each research agency operating by authority of the Council and review of its adherence to appropriate policies and procedures.
- 2) Coordination of the work of research agencies with other research activities in the University.

- 3) Formulation of principles for the guidance of research agencies operating by authority of the Council.

Whereas the titles of the second and third reports basically follow the above descriptions, report number one is simply entitled "The Bureau of Applied Social Research", the reason being that it was "the only research agency now operating by authority of the Council, so it is the only agency dealt with specifically. Thus, in practice, the 'Bureau' served as a model for understanding research agencies in general. This model was not only to draw up a statement of principle to govern its own practices, but to provide "a formula appropriate to any agency of a similar character".<sup>1</sup> The status which the Committee recommended and which the 'Bureau' still has to this day, was that of; "a research unit of the Graduate Faculty of Political Science of Columbia University". However, the major question of the financial basis of its support was left unresolved in that the 'Bureau' remained responsible for finding the bulk of its funds with the University only agreeing to provide 10% of its operating budget. The most interesting aspect of the Committee's enquiry however is not so much its final recommendations, even though these were of great importance to the 'Bureau', but rather the fact that it illustrates very well the difficulties this new form of organised social research presented for the University administration.

Although the amount of money available to the University was no doubt an important factor governing any decisions to be made with regard to the 'Bureau', it remains true that Columbia, as one of the prestige private universities, possibly also suffered the inertia of established sets of relationship and practices which both structurally and psychologically

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<sup>1</sup> All the above quotes are taken from Page 1 of the introduction of the report submitted to the Council 15:5:45.

militated against radical alteration in the basic ordering of things.

Consequently, the 'Bureau' was 'grafted on' rather than transplanted as a totally integrated organ of the University. Yet having said that, the 'Bureau' probably suffered the fate of most innovations in that there could be no appeal to precedence with which to inform and guide the decisions to be made. Therefore, rather than initiate any far reaching institutional reforms the preference was for accepting the 'Bureau' as it was but regulating its operations. That is, if the 'Bureau' was to be a part of the University then it had to conform to the standards laid down by the University, and the appropriate organisational apparatus had to be set up to ensue regularity. In practice what this meant was that the 'Bureau's' operations, were underwritten by the University and at the same time pulled into the orbit of its control.

The reports which the Cheatham Committee submitted to the 'Council' are, when taken as a whole, extremely long, and as one might expect from the nature of the Committee's work particularly detailed in their dealings with specific points of concern. Thus, extreme selectivity has been employed in choosing sections for quotation. The selections are primarily intended to illustrate just how informal and 'irregular' the 'Bureau's' operations actually were. It will be seen, and can be argued, that not only did this state of affairs not lend itself to ease of accommodation by the University, but perhaps more importantly it provides a good example and insight into the organisational chaos of innovation. In other words, the institutional innovation which Lazarsfeld was responsible for and temperamentally suited to, was to a large extent made possible by the very absence of regulating rules or conventions of practice. A more formalised situation may well have limited experimentation and most certainly would not have allowed Lazarsfeld the freedom of maneruverability that institutional innovation demanded.

However, as the 'Bureau' stabilised and developed internally into a more clearly demarcated organisational structure, such flexibility would increasingly have been limited by the sheer force of its own organisational imperatives. Hence, it can be argued that during this later developmental stage the 'Bureau' required a more formal and legalized position which the University could offer to offset the decreasing dynamic of innovatory ingenuity and drive.

The ambiguity of the 'Bureau's' formal relationship to the University and the resulting administrative problems, are noted by Cheatham in the very first pages of report number one. Where he informed the Council that:

"The unusual history of the Bureau, has given rise to legal and administrative problems. On some of these matters the Committee expresses its views. On others the Committee is unable to do more than state the questions, because the position of the Bureau in the University system, the measure of its autonomy, and the location of authority within it or over it are not explicitly dealt with in the University statutes or other governing regulations"  
(Cheatham Report No. 1.3)

Dealing with the subject under six headings, of which not all need concern us here, Cheatham begins by noting that, "the source of authority for the Bureau in the University is not clear. Indeed, it would seem that the Bureau have even been acting illegally in various matters since, as Cheatham noted, "neither the Bureau nor the Department of Sociology is a legal entity". Presumably the 'Bureau' had acted illegally in accepting contracts in the University's name without having any authority to do so.<sup>1</sup>

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1 On page 5 of report 1, Cheatham even raises the question of who has legal title to the equipment of the 'Bureau' and notes, "since it is improbable that the title of the University will be challenged the Committee believes it best not to seek to clarify the situation by asking for deeds from Princeton University, the Rockefeller Foundation or others. As to future acquisitions, it will be well to have dealings and title in the name of Columbia University".

However, the main point to stress, and the one which Cheatham emphasises was that "The Bureau has heretofore acted with great informality in its undertakings with sponsors and others". From the suggestions that the Committee made, it would appear that the 'Bureau' had entered into arrangements without always putting them in writing. As a lawyer, such procedures must have appeared to Cheatham as outright bad administration. This lack of formality certainly bothered him, since if the Bureau was to be legally responsible to the University, and the University in turn legally responsible for it, then it was essential to have operations clarified: "To inform the outsiders of the applicable University policies and to prevent misunderstandings". In order to clarify the authority of the 'Bureau' (that is the Council for Research in the Social Sciences) which had by resolution approved the Bureau, should accept responsibility for it. However, the actual administration of the 'Bureau', was to be vested in a Governing Committee. This had been in 'existence' since a meeting of the Sociology Department on November 6th 1944 when the Department, in recognition of the need for a more formal relationship with the Bureau as an activity of the Department, had placed it under the supervision of a Governing committee chosen by the Department itself.<sup>1</sup> However the Cheatham Committee formalised the situation by recommending that;

"...authority for the approval of projects for research, including those to be undertaken for sponsors, be vested in the Governing Committee of the Bureau by resolution of the Council for Research in the Social Sciences; and that this approval carry with it acceptance for the University of payments and grants by sponsors"  
(Cheatham Report 1:4)

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1 The original committee consisted of Professor Lynd (Chairman), Professor Edmund des Brunner, Professor Robert MacIver, Dr. Frank Stanton of C.B.S.

The governing Committee did become the Bureau's source of authority and its key position in the Bureau's organisational chart can be seen by consulting appendix c . However, although the Cheatham Committee formally legitimated the 'Bureau', one can by examining other sections of the first Report gain a clear insight into the administrative difficulties which the 'Bureau' represented for the University administration not the least of which was the question of the 'Bureau's' personnel. For as Cheatham noted: -

"Employment of the personnel of the Bureau raises questions of appointment, compensation, tenure, retirement allowances or social security, and liability of the University to them or for their acts. These questions are complicated by the fact that the personnel is quite varied, including directing personnel; research associates, statisticians, and other technicians; clerical and secretarial staff; and investigators"

(Cheatham Report 1:5)

Without wishing to go into particular detail, a few illustrative points should nevertheless be made. Certainly, the variety of roles and tasks performed within the 'Bureau' did not make for ease of administrative accommodation, but it is on this whole question of employment that the Cheatham report is perhaps the most unsatisfactory in that it failed to recognise the need to create a career structure comparable to that existing within the Department.<sup>1</sup> As previously noted, security of tenure was never established as a generalised operating principle at the 'Bureau', and became, in the post-Cheatham years a point of serious debate and contention among the middle and junior levels of staff.

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1 It must be noted that such a question was not in the Committee's mandate. However, it must also be noted that Cheatham did attach statements and observations that were considered important but which fell outside its mandate.

Indeed, a double hierarchy of career structures was allowed to develop, a characteristic which has become a feature of research centres in general. Commenting on "certain built-in difficulties for which solutions still have to be found" Lazarsfeld provides a good example of the strains resulting from this dual structure:

"In a teaching department the associate professor is not the superior of the assistant professor; but on an institute project, something like a chain of command is unavoidable. If the two professors meet within both institutional contexts, the transition of the role which is required is often an uneasy one. The matter becomes even more difficult if a well qualified person is working in the institute only because the departmental table of organisation does not permit a teaching appointment, even if he enjoys the assimilated rank of research associate, he is likely to feel discriminated against."

(Lazarsfeld & Sieber 1964:12)

Clearly, the difficulties which Lazarsfeld mentions above are not open to easy solution, especially the first. Hence the second could have been mediated to a certain extent through the closer integration of career structures in teaching and research rather than have the 'Bureau' develop along its own path. Yet here one comes to a central point concerning the Cheatham Committee. Its purposes, its function, and for examination of the reports themselves, the way the Committee defined its task, was fundamentally to control and regulate the unorthodoxy of the 'Bureau' and indeed one might add, its unconventional Director. Even though in Report 3, wider issues are addressed, it was never intended, that the committee should propose any radical administrative innovations. A good illustration of its basically regulatory nature, and its preoccupation with the propriety of operations, is provided by the question of the 'Bureau's' personnel and Lazarsfeld's own conduct. The report reads:

"Under present practice nominations of administrative aides on the top level, as the study directors, are submitted for approval to the Governing Committee, but the remainder of the staff are hired by the Director alone, and their compensation is fixed by him. He also engages the investigators, in so far as they are not the employees of some other organisation

which undertakes field work for the Bureau. Whether this system is in accordance with the practice of the University in related matters or with sound policy the Committee does not assume to state. But it does recommend that the authority in these matters be vested in the Council in defined persons or bodies, and that all employment be underwritten by contract instead of informal arrangement" (Cheatham Report 1:6)

"Informal arrangement" is rather an understatement to describe some of Lazarsfeld's past practices in handling financial affairs. Indeed, as he himself informed the writer, "I would say the first year or two at Columbia all the contracts were made by me on my personal account, and then the salaries were paid this way as well. It was a most unlikely situation and I think if the Bureau of Internal Revenue should ever have,...I might have great trouble" (Lazarsfeld 2:6:73) Judging by the financial balance sheet which Lazarsfeld presented to the Committee, matters were probably not quite so anarchic by the time of the enquiry, but a director who had behaved in such an unorthodox way was obviously one who needed careful checking, for his own benefit as much as for the staff.<sup>1</sup> The solution provided was for the University to take over the accountancy of the 'Bureau', "so that all income and disbursements be handled by the Bursar of the University".

The objectives of the 'Bureau' which had been laid out and approved by the Governing Committee of the 'Bureau' on January 17th 1945 are summarised as follows:

- 1) Training of Students in research techniques
- 2) The theoretical integration of empirical social research

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1. There is no suggestion here that Lazarsfeld through impropriety, ever made any financial gain personally. On the contrary, much of the money which Lazarsfeld put out could rightfully be considered his own.

- 3) The development of an institutional pattern for a social science laboratory
- 4) The collection of sociological data
- 5) Research in specific subject matters

The Cheatham Committee considered the above statement of objectives to be in accord with the general principles which the Council on Research in the Social Sciences had laid down on 25th October 1944 in its resolution for the guidance of research agencies.<sup>1</sup> However, although the Cheatham Committee effectively ratified the scope and objectives of the 'Bureau' a word of caution was sounded. It has already been mentioned that in the body of the enquiry, Cheatham had expressed concern in relation to the question of student training, stating that, "there are a few matters on which I feel strongly and I do not believe my notions will change in the course of our discussion". Indeed, they would not appear to have done so, for in the report itself, he restates his concern and suggests close supervision of the 'Bureau's' activity in respect to that issue. Whilst accepting Lazarsfeld's point concerning the benefits to the student by his being part of actual on-going research, Cheatham warned, "But it has its dangers. Students may well be imposed upon consciously or unconsciously by faculty members who need help in the prosecution of their research".

"The Committee: would emphasize this danger and suggests that it can be controlled only by continuous supervision, aided by periodical reports of time given by each student to each type of work. Students may themselves become over zealous in the pursuit of what seems to

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1 The same meeting in which the Committee on Social Research Agencies was set up.

be real rather than theoretical, or may attach excessive importance to their earnings and overlook lost educational opportunities. 2 Both of these possibilities may bring about over emphasis on training to the detriment of education work in theory. It should be remembered that the University's function is to turn out students ready to learn and develop on the job, rather than trained to do a specific job. So far as Columbia is to be regarded as a source of research workers, it should build its reputation for capable men and women ready for development as practical workers, rather than for workers trained in immediately applicable skills.

The Committee recognizes the value of the Bureau as a workshop contributing to the development of a sense of reality, of cooperative spirit, and of enthusiasm for research. It only warns against over emphasis to the detriment of other work".

(Cheatham Report 1:9,10)

Thus, even in the sensitive area of the students' situation at the 'Bureau' despite certain reservations, no really serious objections were raised. However, on Lazarsfeld's major point that none of the problems of student training "would be really serious if more money were available and earmarked for student training".

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- 2 In answer to the Merton-Mills questionnaire Lazarsfeld shows great concern over students' misconceptions of the training value of various work. For example, he states, "There is also rampant among students a certain inability to grasp the methodological value of material when it comes in commercial disguise. They would consider a body (sic) study on crime rates an object of reverence; they would brush aside an exquisitely sophisticated study of peoples radio listening habits as "commercial stuff". Furthermore, Lazarsfeld also pointed out the dangers of paid work in student training stating, "once students receive pay for work they do not do anything without being paid....it is quite shocking to see how little initiative this kind of student develops in the use of the resources of the Bureau." However, he goes on to criticise the type of student by noting, "one of the concerns over student training would probably not arise in English Universities, but appears to stem from the fact that American Graduate students are much more protected and guided".

The Council's response was to provide 10,000 dollars per annum for such purposes.<sup>1</sup> It is particularly interesting that money was forthcoming in this area while other meaningful contributions to the operating costs of the 'Bureau' were not. For example, Lynd and Lazarsfeld had written to President Butler on February 23rd 1944 requesting, "a fund of \$15,000 to serve as a revolving fund", which after two years, the 'Bureau' was to pay back in three yearly instalments of \$5,000. It would seem that the Executive Committee of the Council in their report of April 3rd 1944 felt somewhat uncertain about providing funds to supplement non-university income. However, on October 25th of that year, the Council did appropriate the sum of \$5,000 for the general support of the 'Bureau' for the year 1944-45.<sup>2</sup> Phillip Hayden, Secretary of the Council, relayed this information to Lazarsfeld, and informed him that; "In the circumstances, it appears to me that you should make application for a further allotment for 1945-46" (Hayden 30:4:45)70. Acting on Hayden's instruction, Lazarsfeld submitted a further application in May of 1945 and enclosed a reasoned argument in furtherance of the support claim. The basis of his case was that firstly the 'Bureau' now assumed, "a much larger share than previously for student training in the Department of Sociology", and that secondly, the movement towards larger and more extensive studies meant that the 'Bureau' had to compensate for the income which was formerly provided by smaller and less significant commercial studies.

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1 Patricia Kendall mockingly described this as, "the magnificent sum of \$10,000 a year". (9.8.73)

2 The above information and quotes are taken from a letter written by Phillip Hayden, Secretary to the Council, to Frederick Mills on 28:4:45.

Here Lazarsfeld informed Hayden that: "It would greatly help the planning of our research programme if we could have a continuation of the grant for the next two years. But if this is not possible, one year, of course, would be of considerable help" (Lazarsfeld 16:5:45). Back came the reply from Hayden that; "The Council for Research in the Social Sciences, at its meeting on May 25th, voted an allotment of 5,000 dollars for the Bureau of Applied Social Research, in accordance with your request of May 16th (Hayden 28:5:45)

However, this was not quite in accordance with Lazarsfeld's request since what he really desired was a two year grant and not the one year's allocation which was given. As disappointing as this probably was for Lazarsfeld, the main point to be stressed is the University's attitude towards the 'Bureau': After all, the sums involved were not particularly large. The reason for the splitting of the grant was probably to afford some fiscal control over what was seen as a new departure in operation and one which had to be carefully watched.

The difference between the relatively generous contribution towards student training and the paucity of contributions to basic support costs,<sup>1</sup> can be tied to the question of responsibility.

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1 In response to the U.N.E.S.C.O. questionnaire, question 50 "What percentage of the budget can be locked on as basic budget, on which the institute can count for a longer period". The 'Bureau' replied, "At the present time, there are no funds on which the 'Bureau' can count that are free of the commitment to perform specific research for specific sponsors. The University contributes to the Bureau's work in student training. It advances funds for overhead costs and for publication, but these are reimbursed from the Bureau's operations". (U.N.E.S.C.O. Report 1959)

One is lead to the conclusion that the University was simply more concerned with student training than with social research. That is, student training was something which the University was accountable for, it defined it as part of its business, and consequently took steps to ensure its support and practice. As far as research was concerned, the University was quite prepared to have it conducted at the 'Bureau' but unwilling to commit itself to the extent of providing support for its institutional base: Admittedly the matter is not quite as simple as that since research was also a goal of the University. However, student training as a public function of the University, and as a shared activity of the staff went to the heart of the University affairs and hence support for it was much more easily agreed on. The opposite was true for research, not only was it seen more as a private affair but, by giving basic research support to an agency such as the 'Bureau', the administration would have altered its traditional relationship to departments. Indeed, it has already been mentioned that Cheatham informed Lazarsfeld that there were a few points upon which he felt so strongly that he doubted whether his opinions would be altered in the course of discussion; and apart from the use of students, the other was, "that responsibility in planning and execution must be in the department concerned, rather than in the Council or any committee of it" (Cheatham 1:2:45)

As a result, the administration refused to 'interfere' to the extent necessary to integrate the 'Bureau' totally into the University and it was left to continue on an ad-hoc basis.

Although the Cheatham Report did suggest various structural alternatives especially in Report 2, by and large, the recommended course was to leave responsibility at the departmental level thereby effectively blocking any possibility of major reform. While responsibility for innovation continued to depend upon the department, minor solutions to problems were likely to be preferred.

Thus, what one witnessed was an early example of what Lazarsfeld has termed a "power-vacuum". That is, the administration did not combine sufficient intellectual and administrative imagination to provide the necessary leadership to grasp the changes which occurring within the social research. Instead, it accepted the presence of the 'Bureau', appreciated its value to the University, but preferred to ensure that its operations were in accord with accepted University procedures, and standards, rather than vouch-safe its existence through basic support. Indeed, Lazarsfeld commenting in his 'Memoir' on the Cheatham Report and the administrative changes which followed states:

"Twenty three years later I find myself chairman of a board of sixteen representatives of professional schools and graduate departments at Columbia - a board which the Cheatham Committee, when it legitimized the Bureau, set up to keep a close watch on its risky director. <sup>1</sup> It would be nice if this symbolised the final victory of an idea born in Vienna almost half a century ago. But it does not. Today scores of such Bureaux exist throughout the country, many of them directed by alumni of the Columbia prototype, but they are not really integrated into the university structure. The effort continues."

(Lazarsfeld 1969:334)

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1 Lazarsfeld would appear to be slightly mistaken on this point. From the documented material remaining in existence the Board, as previously mentioned, was already in existence; what Cheatham did do was to recommend that it be expanded to include non-sociology department members.

An Attempt at Further Innovation : An Incurable Reformer

Although Lazarsfeld may have failed to have the 'Bureau' totally integrated into the University he did achieve the very important step of having it officially accepted and with the passing of time it has, by sequential stages, moved closer in the direction he hoped for.<sup>1</sup>

However, whilst Lazarsfeld's career and institutional achievements presented a remarkable record, an element of regret over what might have lingered on. Even though the 'Bureau' was a prototype of many similar such institutions Lazarsfeld remained discontented with the limited sphere of its operations. The remaining part of this chapter will therefore be given over to an account of his attempt in the 1950's to introduce a further reform or innovation into the practice of social science research: namely, the introduction of an institute for training in research methods.

Although as various people informed the writer, Lazarsfeld was a teacher of some distinction he was, in addition, an educationalist in the strict sense of that word. That is, whilst having the teachers concern with the imparting of knowledge, he was also concerned over the state of education as such. Having been brought up in the intellectual atmosphere of post-war Vienna, amidst a party that not only boasted scholars of great ability, but stressed the importance of education as an important

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1 An important physical factor was the moving of the 'Bureau' from the old medical school site on Amsterdam Avenue to its present location on West 115th Street which is of close proximity to the University. This is something Lazarsfeld had asked for, and the Cheatham Report recommended.

component part of a social movement, Lazarsfeld was understandably imbued with a firm appreciation of the need for adequate educational systems. In addition other factors fed into, and gave added thrust to these interests. Most notably, his over-riding methodological interests. For any methodologist not particularly interested in the substance of a research topic a strain towards 'teaching' will be present, since, this emphasis necessarily demands that the techniques if not always the underlying principles, be communicated to practitioners or future practitioners.

Bearing this in mind, and given the situation of the 'Bureau' in the fifties the direction of Lazarsfeld's next innovatory thrust is easily explained. By 1950 the 'Bureau' had overcome many of its initial uncertainties, and as similar institutions were increasingly being developed elsewhere, it lost some of its innovatory excitement.

This change in atmosphere within the 'Bureau' is well illustrated in the following conversational abstract with Patricia Kendall:

P. Kendall        In the early days the 'Bureau' was more than a job, and was a way of life. We used to arrive early in the morning and study late at night, and we had coffee hours every afternoon where we all gathered and gossiped about what we were doing, and we went out to dinner together and came back and worked. We worked Saturday and Sunday. We were extremely dedicated and involved with our work. That changed over the years and it became a nine to five institute.

D. Morrison       Is that due to size - although in sociology one puts so much down to size?

P. Kendall        Perhaps that, but perhaps the very different projects people were working on, so they couldn't compare notes so easily.

D. Morrison       Reading the Hanover meeting reports, there seemed the concern that the 'Bureau' didn't have a line. Did that lead to identity trouble?

P. Kendall        Well certainly in the Hanover seminars this was the main concern. Of that I'm sure ... that was a concern. But it was also, there wasn't

any longer this core of younger people who were the novices and who were getting training by coding and so on. It was this phenomena which started in the fifties of giving pre-doctoral fellowships....had a disastrous effect on the 'Bureau' I think.

- D. Morrison      In that students had different expectations?
- P. Kendall      They were their own bosses, they weren't reporting to anybody except their own advisers.
- D. Morrison      Who were in the sociology department rather than the 'Bureau'?
- P. Kendall      He could be in either.
- D. Morrison      You felt they weren't getting the same thorough training.?
- P. Kendall      They weren't getting the same thorough training, they weren't being apprentices the way we had been 10 years earlier. They were working on small problems of their own without particular loyalty to the 'Bureau'.

(Kendall 9:6:73)

The reports of the staff conference referred to above, which was held at Hanover, New Hampshire on June 28/29th 1956 and the memoranda surrounding it, provide a good insight into the 'Bureau's' changed position and into the resulting 'crisis of identity'. The conference crystallised the questioning which was taking place within the 'Bureau' as to exactly what its future role should be, and the records of the proceedings provide a useful documentation of the collective uncertainty which, at the individual level, prompted Lazarsfeld to embark on a new and ambitious venture. The agenda committee for the conference consisted of Charles Glock, the then Director of the 'Bureau', William McPhee, Natalia Rogoff and Hans Zetterberg. This committee, six days prior to the conference, distributed copies of the proposed agenda to all the participants, in which the purpose of the conference was expressed as:

.....to lay the groundwork for more fruitful deliberations about the 'Bureau's' future content and form during the forthcoming years. It is not expected that any of the topics can be wholly explored nor that closure will be reached as to what might constitute an 'ideal' Bureau. It is hoped, however, that the conference

will provide a more satisfactory set of ideas for viewing the Bureau's activities than we have heretofore"

(Hanover Memorandum 22:6:56)

Topic two of the proposed agenda, "The Bureau's Intellectual Activities and its Goals", had as its topic chairman McPhee, and Lazarsfeld as topic analyst. This section raised very clearly the question concerning the amorphousness of the 'Bureau' and the lack of an identifiable *raison d'etre*, for example, the opening passage of the topic's introduction read:

"Most research institutions, like organizations in general, are identified with some central idea, for example, Hovland's institute with experimental studies in communications; the Cowles Commission with econometrics; the Princeton Institute for Advanced study in mathematical physics and so on. This sort of central emphasis functions in part merely as a kind of "line" for external consumption. But at its best it may be the heart of the excitement or morale of an organization giving the sense that "it is really getting somewhere in X direction" or "it is the best in the world for Y" and so on. After starting with a clearly defined external line and internal core of excitement centring around the then-new field of communications research, the Bureau probably does not present any such clearcut image to outsiders or provide a sense of chief or central purpose to insiders today"

(Hanover Memorandum 22:6:56:4)

Whilst the conference was extremely self-critical of the 'Bureau' it should not be supposed that the 'Bureau' was beset by difficulties that in any way cast doubt upon its future continuation. On the contrary, the criticism is indicative of an institution which is structurally strong enough to withstand such self examination. For example, prior to the conference, Lucci York wrote to Charles Glock with suggestions for inclusion in the conference agenda, stating:

".....it's inevitable that in order to endure continuity of staff the Bureau must mediate its professional goals and take on commissioned research often of a kind which should be left to the commercial organizations (although we usually

gloss this over with phrases about how they fall into the Bureau's programme). This can't help but evoke charges of BASR prostituting itself, seeking only self-perpetuation, functioning only to provide jobs from some people who for some reason are unwilling to take or are not qualified for academic positions. And if outsiders don't raise such questions, certainly we should. If we are honest, we should have to admit that although for the most part such work is done with integrity, it would be difficult to justify it professionally. The only answer is that the overall and long term product of the Bureau justifies its doing whatever is necessary to keep its staff more or less intact. While we undoubtedly believe this to be true, can we convince an impartial judge that the real contributions of the Bureau stem only from the peculiar nature of organized research and more than outweigh the non-important work done. I raise this only to suggest the need for some self-evaluation based on something more than pointing to the list of publications and somewhat more precise and objective than our personal feelings about the work we are doing"

(York 12:6:56)

The above has been quoted at some length for not only is it a good example of fundamental questioning characteristic of the conference as a whole, but it is also illustrative of the situation which allowed such criticism. The type of formal questioning which York exhibits would have been unimaginable during the 'Bureau's' establishing period, since, most 'faults' were more likely to be interpreted as unfortunate situational products, or else, given the manifest pioneering ethos perceived not as faults, but as exploitable arrangements. However, whilst recognising that the pioneering nature of the Bureau's early days mitigated against the above kind of questioning since the very activity of establishing such a research institute was sufficient justification for a whole variety of practices, it must also be recognised, that not only had the 'Bureau' itself changed, but that in addition, the context in which it existed had changed as well. To take point one first. Quite apart from the fact that its early attachment to mass communication research had originally provided the Bureau with an identifiable "line" which lent for coherence, its internal solidarity

was undermined by the changing nature of the staff. Whereas the early 'staff' consisted of a small core of 'cadres' committed to the development of empirical social research, increasingly the 'Bureau' had attracted individuals who did not share the same core values and were not enthused with the feeling that something new was going on, for in many ways it was not. This leads on to the second point. Having helped pioneer a certain institutional format for the conduct of empirical social research the 'Bureau' was witnessing the fruits of its early endeavours in that it was no longer alone in offering such facilities. Other centres had been established along similar lines with the result that novelty alone no longer sufficed as a *raison d'être* for the 'Bureau'. Being able to appeal neither to uniqueness, or to a corpus of work as a keystone of recognition produced a 'crisis of identity' in which the 'Bureau' was thrown back on itself in critical self-analysis. As with most such self reflections the conference at times exhibited the air of a confessional, but above all, the dominant question was, 'what should the 'Bureau' be?

After the Conference, the proceedings were written up in note form as a *précis* provided of the varying points of view expressed. Although the 'notes' run to some thirty pages, by interspersing quotes from them, with memorandum subsequently submitted to the 'Bureau's' policy committee by conference participants, it is possible to briefly illustrate the points of concern.

Hence the following ignores much of the detail of the conference, and concentrates instead on broad themes.<sup>1</sup> Thus, under the heading

"Features of the Bureau which are Sources of Satisfaction and Sources of Malaise"

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1 It is noted in the introductory pages of the report that "In the interest of succinctness, the points of view here stated in the form of unqualified assertions; the reader is asked to remember that there was unanimity on few of the judgements, and that in more than one instance assertions of fact are in dispute" (Hanover Report 1956:3)

one reads:

- 1) The Bureau pioneered the idea of a laboratory for field research in the social sciences as a permanent fixture of a university.

but

- 1) This pattern has now become institutionalized and is no longer a sufficient reason for existence

- 2) The Bureau has provided a haven for social scientists who wished to devote themselves to a career primarily of research rather than primarily of teaching

but

- 2) This task also is now accomplished here and at other institutions, and is no longer a unique enough feature to command our pride.

(Hanover Report 56:3:4)

In all, eight such "points and counterpoints" including various sub-points were laid out, before collecting numerous observations together in listed form:

- 1) It was recommended that we define the personal goals that we wish the Bureau to serve: We want the Bureau to be the kind of organization which will yield us -- what? Is it that we want to be proud of the Bureau? and if so, vis-à-vis of what reference group? Is it that we want the work to be exciting? Do we want financial remuneration? Do we want the Bureau to give us easier access to academic positions? Do we want it to be more effective in influencing policy? and if so, whose policy? Is it that we want the world to know better what the Bureau is? and which world? Is it that we want its products to add up to something -- e.g. to make sense as accomplishments in the social sciences?
- 2) One of the principal sources of our malaise is the seeming absence of an organizational goal. It is getting so it is difficult to tell an outsider -- or oneself -- what it is that the Bureau is all about. Different people outside have entirely different images of the Bureau (if they have any), according to the particular aspect of the Bureau's programme that they have come into contact with. Thus, so to speak, some people think the Bureau is Lazarsfeld; some officials in Washington identify it with Jaffe's work;

a few now identify it as an organization devoted to the study of the sociology of religion, Riesman identifies it with the Traditionals Group; and so on.

The absence of clear identifiability is not only personally dissatisfying but also has practical disadvantages. An identifiability is not only personally dissatisfying but also has practical disadvantages. An identifiable programme, something that the organization stands for, a "label", so to speak, especially one that can be easily communicated to outsiders, is an important asset in public relations -- e.g. the securing of continuation grants, in making potential clients aware of the Bureau as the logical place to take certain problems or in other connections"

(Hanover Report 1956:10)

Reading the above, one is provided with a clear indication of uncertainty within the 'Bureau': uncertainty as to its own identity and uncertainty as to what it ought to be doing. This lack of identity presented considerable problems at the level of personnel. For example, Clara Shapiro, informed the Policy Committee that, "From the point of view of Bureau Status in the Profession: It would be easier to recruit competent personnel if the Bureau enjoyed a clearer position and an enhanced status in the profession". She further goes on to note, that from the point of view of the self esteem, a staff member "could be made happier in his job if he felt more certain of the status of the Bureau" (Shapiro 9:7:56)

The picture that emerges is of an institute which to a certain degree exhausted its initial innovatory capital of excitement and purpose. At the organisational level as the 'Bureau' developed and expanded, the response had been to simply add new research divisions. Consequently the original sense of belonging resulting from intense interpersonal contact had been severely weakened. Social intercourse where it existed took place within the division, rather than across the 'Bureau' as a whole. The resulting fragmenting of interaction served to militate against a feeling of belonging to the 'Bureau' other than as a place of work. Perhaps this is boldly put but nevertheless it is born out by the type of concerned questioning which took place at the conference.

Yet having said that, it should not be interpreted as in some sense a pathology, but rather as a logical consequence of the Bureau's growth and maturation. As Natalia Rogoff states in discussing the establishment of an intellectual theme:

"The theme cannot be legislated, but has to correspond to the interests of the staff....This represents something of a coming of age of the Bureau, in at least two respects. First, the staff now has certain intellectual commitments, individually and collectively, such as were probably not present among the staff of ten years ago. Second, the organization has become well enough established so that its research can be guided by the intellectual commitments of the staff, rather than wholly by the projects it is commissioned to do" (Rogoff 9:7:56)

Clearly, Rogoff perceived the situation not as some incipient pathology but as a new stage in the Bureau's life which demanded organizational devices "for stimulating and encouraging the emergence of common themes among projects".

Nevertheless, this search for a theme is indicative of the position in which the 'Bureau' found itself. Admittedly, only similar detailed studies of other general purpose research bureaux could substantiate the fact, but it can be postulated that those bureaux established late in the genealogy of research institutes would not suffer to the same extent, from the concern which the 'Bureau' exhibited in relation to the absence of a theme. The Bureau was almost a prisoner of its own pioneering role and prestige. Having so manifestly occupied a major position within the social research world, it now saw its pre-eminent position being steadily eroded by the growth of a network of similar institutions. Thus the fundamental question of the conference was whether the 'Bureau' was going to be just another research institute among others, or something recognisably special.

#### Lazarsfeld's Own Decision

As far as Lazarsfeld was concerned the 'Bureau' was not going to be just another research institute, but rather the centre of a new idea in research training.

The original proposal <sup>1</sup> for such a centre was written in 1950 in collaboration with Robert Merton and although clearly pre-dating the Hanover Conference, was nevertheless equally clearly related to the 'crisis of identity' which the conference signalled. Yet the situation is more complex than that; since there is also Lazarsfeld's own biography to be taken into account, and it was the combination of these two factors which informed the action which Lazarsfeld adopted. In many ways the proposal to establish a training centre was the logical conclusion of Lazarsfeld's career to date. His methodological interest which had given coherence to his intellectual work had gained him a wide audience both through publication and through institutional association. Indeed, if one wished to select a lasting feature of his impact upon American social science, then one would not be mistaken to single out for special attention his seminal position as an institutional innovator. However, although the 'Bureau' had acted as a model for other similar institutes, the economists' concept of 'diminishing marginal returns' is not inappropriate when applied to Lazarsfeld's situation. Not only had his work now been taken over by other individuals, trained by himself, but more importantly, when seen from the position of continuing progressive influence, the basic idea of a research centre had won acceptance. Yet, to firmly cement his life's work and to correct faults that had manifested themselves in the new empirical science, required additional influence on his part. It was impossible to wield such influence through the 'Bureau', since it no longer enjoyed the pre-eminence it once had, and although it was to play a part in the overall scheme of things a new

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1 A very much abbreviated form of this proposal can be found in Lazarsfeld 1972 entitled "A Professional School for Training in Social Research".

instrument was now needed.

The term 'empire-building' is readily applied in the academic world as a derogatory description. The sensitivity to such approbation and recognition of its ready usage is exhibited towards the end of the Hanover report when:

"It was asked whether the Bureau might not become less exclusively an arm of the sociology department, but invite members of other departments to utilise it as a research apparatus. It was estimated that some faculty members would react positively, others negatively to such a proposal, one much be aware not to arouse the fear of 'empire-building'"

(Hanover Report 1956:29)

Perhaps the basis for such sensitivity rests on the shibboleth of academic freedom so that any move on some other academic's part can be translated into a fear about one's own position. The emphasis upon individual creativity, tends to foster insecurities which are easily transformed into spiteful jealousy at others' advancement. Whilst it is doubtful that Lazarsfeld himself, given his rather harsh administrative schooling, would have been particularly bothered at the personal level by such admonishment; the fact that his new designs raised that type of objection formed a powerful barrier to their acceptance. In a sense the charge of 'empire building' was correct in that he would certainly have greatly extended his influence, but the fact that it was so readily used says more about the nature of academia than it does about Lazarsfeld.

In the opening pages of Lazarsfeld's proposal to establish a training centre the faults of the existing situation within social research are already laid out:

"Today, departments of sociology all over the country have become spearheads in new ways of collecting and processing the type of data now available to all social science. But this development, infact, has not yet been paralleled by the creation of places where systematic training in such skills can be acquired. Everyone now working in the field will probably agree with the following five deficiencies:

- a) At no university can the student find a comprehensive exposition of all these new techniques.
- b) Even where some of them are taught, they have not been well integrated with the older and better established procedures of, say, the historian or linguist.
- c) Little scrutiny has been made by other social sciences as to where this "sociological" research might or might not be useful to them.
- d) There is nowhere established a continuing study of the relation between empirical procedures and the theoretical analysis of the workings of society as a whole, which certainly is the basis of all the social sciences.
- e) No provisions are made at any university for the continuous developing of devices of social book-keeping, testing their usefulness and eliminating waste"

(Lazarsfeld and Merton 1950:6)

To correct the above situation Lazarsfeld's suggestion was for a five year experiment to be proceeded by one year of planning of which the main task was "to begin the collection and processing of suitable training material." Basically, the institutional model to be followed was that of a medical school with particular emphasis placed not only on the method of learning used within medicine, but also upon its learning roles. That is, the case study was to be a central plank within the teaching programme, and the position of the intern the major training role. On the question of training method Lazarsfeld states that:

"After some hesitation, it was decided to use for the proposed programme the term "case study approach". We are aware that this is a dangerous term. Once it played a constructive role in many areas of professional training; more recently it has become somewhat controversial. Yet, after considerable discussion, we feel that the situation is about as follows: a new area of advanced training usually develops in a field of "know-how" that is not yet properly systemized and is therefore difficult to transmit to students. The task of a training institute then becomes difficult to transmit to students. The task of a training institute then becomes to translate pioneer work into more generally available knowledge. The case study approach does exactly this; advanced practice is carefully described; it is at the same time translated into principles, and its general implications are examined. Quite naturally, after sometime in every field, as a certain degree of systemization is reached, the case study approach becomes less important. But, every new field which is opened up for advanced training has to go through its case study period" (Lazarsfeld & Merton 1950:10)

Lazarsfeld recognised that the idea of case material differed from field to field,<sup>1</sup> that selecting relevant cases for study was therefore a difficult task. Nevertheless, he did provide the relevant outlines. In addition, his appendix, "Case study Materials Needed for Training in Social Research", provides a very good insight into his general conception of social sciences. Because of the length of the appendix the writer can do no more than pull out some central features. But once again, what is particularly noticeable is his wide ranging vision and his refusal to be confined by narrowly defined intellectual parameters.

According to Lazarsfeld case materials needed to be centred on the area of "strategic operations in the analysis of research data". The idea was that, through the skilful collecting together of widely scattered material, a kind of compendium of case material could be produced, and "rigged" to require the student, "to come to terms with a distinctive kind of problem by introducing the operations adequate to clarify and resolve it". Hence each set of material would introduce the student to the kind of situation which the researcher commonly had to confront in his professional practice.

Lazarsfeld particularly stressed the importance of "interweaving quantitative and qualitative analysis so that each supplements and enlarges the significance of the other". However as the previous conversation with Lazarsfeld revealed he saw the question of transmitting

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1 Lazarsfeld gives the examples of "The Case" as used in law and advanced business education, "the instance" used in engineering and the "clinical case" used in medicine. He states, "These systems of training have one thing in common, they require the student to engage in the disciplined, supervised and detailed practise of diagnosing or analyzing problems of the type he will encounter in professional practice" (Lazarsfeld Merton 1950:20:30)

qualitative skills as particularly problematic, due to the absence of codified procedures. As he noted in his appendix;

"with regard to the collection of qualitative field data. Training is negligible and training material virtually absent. Not that qualitative data fail to be recognised as of the highest importance in social research. The cogency and intellectual impact of the Lynd's Middletown studies or, in another vein, of Thomas and Znaniecki's monumental volumes on the Polish peasant is generally acknowledged to depend on their perceptive and skilful utilization, not of quantified, standardized field data which belong to the category of important but, on the whole, routine field work operations to the category of important but, on the whole, routine field work operations but of significant qualitative data, adroitly analyzed, which set forth the nature of qualities of the constituents of human behavior. These qualitative materials, typically unanticipated by the field worker before he went into the field, often represent the growing points of the discipline since they call attention to basic aspects of social behaviour not caught up in the data assembled through the use of more nearly standardized procedures of data collection."

(Lazarsfeld and Merton 1950:69,70)

It was therefore to the problem of correcting the neglect of systematic training in the qualitative methods that Lazarsfeld particularly addressed himself, for as he rightly noted, "the graduate student of social research has received little training in the collection of field materials such as these. In effect, he is told only that perceptiveness or ingenuity is a previous asset for the field worker". Clearly, the conversations on this topic quoted previously make clear Lazarsfeld saw no easy solution. However whilst accepting that students vary in their capacity for qualitative work, and that such differences could not in all likelihood be erased through formal training, he nevertheless maintained that "perceptiveness cannot be induced by training, it can be enhanced". As always, on Lazarsfeld's part there is a refusal to mystify procedures either by submerging them in obtuse and vague writing or by not being explicit. Indeed, his insistence that clear and direct articulation is the basic

precondition for progress in social research and theory was the source of some of his annoyance with the Frankfurt School. His refusal to mystify is evident when he states that;

"All too widely it is assumed that qualitative procedures must remain an elusive, private, and incommunicable art. Yet it is plain that there are recurrent situations and problems in field work which can be met successfully by communicable and transmissible procedures. This basic aspect of training must not be allowed to go by default"  
(Lazarsfeld and Merton 1950:71)

This situation was to be rectified by collecting case materials drawn from, "the literature of previously unstandardised kinds of qualitative field observations", and identifying the common observational procedures.<sup>1</sup>

The premise was, that the repeated working through of such case materials would provide not only more nearly standardized modes of observation, but would also "sensitize the student to a wider range of strategic data than is ordinarily recognized by students schooled only in the more routine technique of collecting field data".

However, it is in the area of, "Case materials centred on the Translation of Social Science Classics into terms of Contemporary Social Research" that Lazarsfeld most forcefully exhibits his appreciation of the pitfalls

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1 What Lazarsfeld had in mind was the gathering of field observations of a seemingly different and unconnected nature which when examined closely are seen to involve the same characteristic procedures of observation. For example, he states, "the observation of a Lynd on the major place of the automobile in the life of Middletown, for example, are then seen by the student to involve essentially the same order of qualitative observation as the observations of a Malinowski on the functions of technological collaboration among the Trobrianders or the observations of a Thomas on the functions of fraternal organisations for polish peasants"  
(Lazarsfeld and Merton 1950:72)

of over-specialization in either research or theory. Thus he complained that:

"....the graduate student of social sciences tends towards one or the other of two opposed, and equally indefensible positions, in regard to social research. He is, at one extreme, forever subject to the dangers of intellectual parochialism. Taught to place great store by precision of research, he may come to limit his studies to those narrowly constrained problems which have little importance for advancement of the discipline.

As the other extreme, some students, not infrequently the more capable among them, may rebel against what they take to be the excessively confining nature of research aimed primarily at precision (even at the expense of the intellectual significant problem). These students are tempted to abandon systematic empirical research altogether. Over-reacting against the threat of parochialism, they turn exclusively to the 'largest' problems of the time, after the fashion of the social philosopher. They confuse the abstract importance of a problem with the prospect of advancing knowledge about the problem"

(Lazarsfeld and Merton 1950:73,74)

There can be few statements by Lazarsfeld which show so keenly his insistence on rounded although cautious completeness, as the above. Thus, for him the object of professional training in the social sciences was, "to provide for that combination of skills and scholarship which exhibit the distinctive merits of both the distinctive limitations of neither ... it must produce neither mere social technicians nor mere social philosophers". Presumably, what Lazarsfeld is describing here, is his concept of the ideal social scientist - in other words, himself. For to train students in anything other than one's own ideals is different to imagine in a free situation.

Another major concern of Lazarsfeld's was that the student often emerged from his training equipped to conduct social research, but not, "schooled in a range of problems to which he might most favourably apply his research skills" and he proposed to overcome this through the explication text, a process Lazarsfeld held in high esteem. That is, the 'classics' of sociological writings, such as those of Simmel,

Durkheim, Weber or Spencer, to use Lazarsfeld's examples, were to be, "converted into intensively examined rather than superficially scanned texts". By this method,

"A single page may be the occasion for an extended reformulation. The student is brought to see that behind each significant statement in such a text, there lies a problem, more often implied than stated. For in the period antedating systematic research, these authors characteristically set forth 'answers' to a problem, without stating the problem itself in strict terms. Seemingly simple conceptions -- the concepts of "common will" implied by Rousseau, or the concepts of social class implied by Marx, for example -- are found, upon due scrutiny, to involve multiple meanings in different contexts"

(Lazarsfeld and Merton 1950:76)

By following such a method the student was supposed not only to broaden his own conceptual vista, but to convert aspects of the work into operational research terms. However, as a protection against abstracting concepts and thus destroying the meaning they had for the original author, the student was also required to return "them to their contexts in the original sources, with the intention of uncovering the central intellectual concerns of the 'classical' author."

It was hoped that by such procedures the student would see his own work in a more historical setting. In addition, the careful and detailed examination of the 'classics' was intended to inject a degree of humanism into the student's training. Indeed, it would seem that in viewing the development of social research within America, Lazarsfeld was somewhat concerned by the over-emphasis on technical virtuosity at the expense of deep understanding, and commenting on the need for humanism Lazarsfeld stated his hope that, it "would curb the inadvertent tendency for a new barbarism in the social sciences. Above all, it would save the would-be technician from himself". In line with the idea of professional training went the notion of "Case Materials, Designed to Train Students in the Application of Social Research to Problems of Public and Private Policy".

For as Lazarsfeld recognised only too well, social scientists were increasingly in demand by a variety of agencies external to the university. What particularly concerned him, about this situation was that in some quarters, the social scientist was still regarded, "either as an academic scholar or as a freelance advisor merely, not as a professional practitioner of social research".

However, as he noted:

".....social reality has out-distanced these partly obsolete images of the social scientist. Social scientists during the last two decades or so have been providing professional services to a most varied clientele. They have not been characteristically engaged in giving advice based simply on personal wisdom, or on academic knowledge. Rather, they have been concerned, on a growing scale, with social research and with the application of this research to specific problems of society.

This research has been directed towards and utilized by the community at large or, more typically, by specific organizations within the community. In this latter category, the most readily visible clients have been found in a wide range of governmental departments and agencies, who turned to the social scientists, not for advice on policy, but for knowledge to be taken into account in the formation of policy", (Lazarsfeld and Merton 1950:16,17)

Hence Lazarsfeld saw better professional training partly as a means of improving the quality of professional practitioners in areas of policy formation. However it was by no means to be considered a one way trade, but was also to aid in the clarification of the relationship between basic and applied research. As far as Lazarsfeld was concerned this dichotomy was both false and harmful. Hence bringing together trainees and practitioners, who had participated in research which had actually been applied to policy, offered not only a new teaching experience, but also the possibility of reappraisal and intellectual development. Hence the conventional situation of professor and student was to be replaced by "a more active and more varied learning situation, in which professor, practitioner and students are all diversely learning from collective examination of the case in hand".

More particularly he argued that sessions:

"....based upon diverse cases introduced by practitioners," are more likely to lead "to an effective assimilation by students of basic principles of analysis, in place of more improvisations. It will undoubtedly be found, for example, that principles and precepts which worked through in connection with a problem introduced by one practitioner will enter into the discussion of a problem subsequently presented by another practitioner. This unpremediated experience of drawing upon general principles may be expected to give the student a healthy but not exaggerated respect for principles of analysis, and this wean him from any tendency toward rank empiricism. But it is important to notice: these principles thus earn his respect; they are not authoritatively and doctrinally imposed upon him" (Lazarsfeld and Merton 1950:83)

In addition weaning the student away "from any tendency towards rank empiricism" Lazarsfeld also intended to offset the parochialism of much empirical social research through the use of "Case Materials Designed to Train Students in the Procedures of Comparative Social Research". As often in Lazarsfeld's reflective writings he gives the impression of wishing to protect empirical social research from the enthusiastic endeavours of its own practitioners, and quite clearly, his aim in training was to guard against the retreat into triviality and social insignificance. Thus one reads:

"Much empirical research is being done on very specific problems, with narrowly circumscribed samples. Thus the impression is created that 'little' subject matters go together with clearly defined research operations while 'big' issues -- for better or worse -- have to be approached in a vaguer more 'philosophical' way. This is a misunderstanding which can best be dispelled by studying topics for which larger social systems form a natural research unit. Such topics can be provided by systematic comparison of different nations and different societies. Therefore, case material is needed from the field of comparative sociology, which will permit the detailed analysis of various social structures". (Lazarsfeld and Merton 1950:85)

Quite apart from his aim of 'broadening' research, Lazarsfeld's stress on comparative work was underpinned by his general insistence on the clarification of concepts. Since it was by way of conceptual precision that meaningful comparisons could be made at all, The example provided by Lazarsfeld is of the comparison of living standards in vastly differing

societies. Faced with such a task, he argues the student would be compelled to clarify the concept 'standard of living', "if only because such seemingly different elements appear to be involved in different societies". Thus, once more through the careful selection of case materials Lazarsfeld could introduce and hold the student to his own intellectual standards, and to the logic of his research procedures.

Even though Lazarsfeld talks of "various training centres", had the scheme been successful then undoubtedly Columbia would have occupied a pre-eminent position within the world of social research -- at least until other such centres established themselves, and even then, its influence would still have been considerable. Whereas the 'Bureau' had largely recruited, especially at the student level, from its own localised environs on the Columbia sociology department, the catchment area for the training centre was to be all graduate departments both within and beyond Columbia. However, training offered by the proposed institute was not intended as a substitute for graduate education, but rather as a supplement. Since, whilst he recognised the shortcomings of existing training facilities for those wishing to engage professionally in social research, Lazarsfeld also acknowledged that the basic academic groundwork had to, and indeed was, being laid in the graduate departments. However, if the training centre was to be successful in its own terms, it had to attract a wide variety of students.<sup>1</sup>

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1 The four major types of student which the centre envisaged catering for were:

- 1) Students of sociology who wanted to specialise in empirical social research
- 2) Students from other departments of social science seeking subsidiary training of the kind mentioned
- 3) Ph.D's in sociology from other universities seeking supplementary professional training at Columbia
- 4) Practitioners in Government, Industry, Labour Unions etc. who wished for 'refresher courses'.

Indeed, Lazarsfeld's own concept of the social sciences was not bounded by disciplinary parameters, but focused around the logic of enquiry which bridged substantive areas, and allowed the bringing to bear of a wide variety of approaches on any given problem. The idea of such a centre was, in a sense, the institutional culmination of a wide-ranging intellectual catholicism leading back to Vienna; although sociology as a major, or even the major, contributor to methods held a central place in his scheme of things.

#### Difficulties and Disappointment

The precise details of how and why the proposed innovation failed are not available, but at the most obvious level one can state that Lazarsfeld failed to get the necessary funds for its establishment. Yet from the point of view of this particular work this obscures one of the most interesting aspects of the whole episode; which is, the way that it demonstrates once again Lazarsfeld's reforming and innovatory nature. The opposition which he was likely to encounter, and indeed did encounter, was not perceived as an immovable obstacle, but as a negotiable difficulty which he considered himself entirely capable of surmounting. Had he persevered in his struggle( and the reasons for his non-continuation will be mentioned shortly), then he may well have succeeded, but probably only in a muted form: an approximation of the ideal. For in this instance, the cooperation of significant sections of academia was required, not just within Columbia, but beyond the frontiers of its influence. Since, whereas the 'Bureau' had developed on the rising tide of empirical social research there was no comparable desire for an individual personification of that movement, and although this is a somewhat bold statement, in the final analysis, this is indeed what Lazarsfeld was asking other academics to accord him. This point, and others relating to the proposed training centre were brought out in a discussion the writer had with Bernard Berelson:

"As I remember it -- they probably couldn't have gotten the money for it.. that's not the whole.. why couldn't they? It was, in the first place -- as I remember it -- the plan was never really spelled out. Bob (Merton) tried to help Paul and got his hand into it, but I think I can even recall his acknowledging to me once when I made the point that it really didn't make sense -- what it was, and this was at the core of it. In effect Paul was saying nobody knows how to train sociologists or social scientists -- nobody knows how to train them except me, and I've got a very small field here, just a sociology department at Columbia University and some boys who come across the street in the Bureau of Applied Social Research. Give me a big post-graduate field to operate in, and all these people who have been mistrained at Chicago and Harvard and Pennsylvania and Cornell and so on will come and I'll train them the right way. Well nobody -- you're not going to institutionalize that, or very seldom does that happen-- lets put it that way. One has to have very great distinction. Paul had some of that, that's true, but he didn't have enough to carry that off without a sort of spelling out of what that would mean in detail. Because there are a lot of people who used to say, "Well what that would mean in detail. Because there are a lot of people who used to say "Well Paul's very clever with four-fold tables, but he thinks that the world lives in them and there's a lot more to the world". So, they would no more have sent one of their students over to him than they would have dropped them out of the window"

(Berelson 12:7:73)

Infact, Lazarsfeld's proposal is a detailed and very well thought out document. Nevertheless, Berelson is correct in stating that it is pervaded by a sense of intellectual 'imperialism'. This surfaces in particular in the section headed; "The Skills of Directing Social Research":

"Much, though by no means all, of professional social research is conducted by research teams, rather than by individual scholars pursuing their separate, if not unrelated, inquiries. Cooperative or team research at once calls for the exercise of special skills in the organization and direction of research. And, this too sets distinctive tasks for professionals, as distinct from academic, training in social research. It is an important commonplace that only the man experienced in

in the diverse operations of a social research project can effectively direct the project. But the future director of these researches must also acquire the more difficult skills of planning and organizing the research undertaking as a whole. And this manifestly calls for special training procedures to be incorporated into the educational structure of the professional and school" (Lazarsfeld and Merton 1950:25)

Clearly this is a valid intellectual point, and one which Lazarsfeld was particularly keen to stress on numerous occasions, but equally Lazarsfeld was the one who would be doing the training, and thereby reproducing other Paul Lazarsfelds. He himself would not deny this, as the following extract from a conversation illustrates:

D. Morrison: You have been, I would say, a campaigner for a certain style of research

P. Lazarsfeld: Yes, yes.

D. Morrison: How much was it that you were dissatisfied with the type of sociology being done, and you saw an opportunity to, I shall even say produce a Paul Lazarsfeld type of sociology. That you would get students from all over and really revamp the field?

P. Lazarsfeld: No, I would even put it a little more sarcastically, and cut off one part of your sentence. I think I wanted to produce Paul Lazarsfeld's and not a Paul Lazarsfeld type of research because -- it's difficult to say -- if I was to count my blessings it would be essentially half a dozen very prominent students.

(Lazarsfeld 15:7:73)

It is certainly true that the establishment of such an institute would have afforded Lazarsfeld a superb opportunity to advance his own influence not only through the intellectual training of students, but also by nurturing the development of future managerial scholars. However, this fact should not obscure Lazarsfeld's genuine intellectual commitment to the type of institute which he proposed. The fact that self-aggrandisement would have occurred in the process does not necessarily imply that it was the primary motivating factor. Whereas the 'Bureau' was the outcome of the dysjunction between developing forms of knowledge and existing institutional bases,

surveying the field in 1950 Lazarsfeld was convinced that a new form of training was required to wrest the greatest advantage from what had become a rather assorted, if not disorganised, institutional development.

It will be remembered that at the Hanover Conference the 'Bureau' laid the charge against itself that it did not train its students in all the skills and knowledge required for research, but only gave thorough training in those areas specifically required by the projects it handled and the body of knowledge which it had accumulated. The proliferation of this situation was something he deeply regretted, for if anything, Lazarsfeld had always valued the rounded scholar. Indeed, he himself refused to demarcate intellectual boundaries, or to accept the label of any particular discipline.

Thus, whilst readily acknowledging the part that his desire to extend his own influence played in the proposed scheme, the intellectual and situational context must not be ignored.

In the very process of failure, Lazarsfeld once more demonstrated those facets of his personality which had made for institutional success in the first instance. In response to the writer's suggestion that perhaps the idea of a training centre was impracticable, he replied:

P. Lazarsfeld: I don't believe it. I still think I was right. Look there are two reasons that I didn't fight it out. There was quite a number of meetings with Deans and outside I found no resonance. Not even at Columbia. I mean, I had meeting after meeting and people politely listened but...and it would probably have been my taste to battle it out, but I was under a very strong influence of Merton and my wife. We were fairly recently married. This all sounds absurd in the sense that the arguments proved so wrong. They said I was getting old, I am fifty and I should lead a more contemplative life, and I still have a lot to write, it would be strenuous, I'm getting old and I'm entitled to a rest and why should I ... why should I start organising anything new, but just let the 'Bureau' take it's course...I'm sure I was wrong, or they

were wrong, but I didn't press it. I think I made the wrong decision. Would I spend another five years convincing the University and interesting others. Now my wife's influence played of course a role, but Merton's influence played more of a role because I was aware that he didn't want to be bothered anymore.

D. Morrison: But Merton was very interested as well in setting up , or am I wrong?

P. Lazarsfeld: He was very interested. He helped me tremendously in writing it, but he didn't want to fight. He had had enough. He fought for the 'Bureau' and it was always a little against his style anyhow. He was always much more individualistic than I. He was most loyal and important a comrade in arms, but basically I knew he wouldn't do it.. he wouldn't follow into this second phase. And if you take together the apathy and resistance -- not apathy the personal opinion of my wife and the personal pressure from Merton not to do it for my sake, but knowing he hoped I wouldn't do it, because he would then be under the conflict for another ten years fighting somehow.

(Lazarsfeld 15:7:73)

The amazing thing is that Lazarsfeld could even contemplate "ten years fighting" for after all, to establish the 'Bureau' had been an exhausting, and by no means easy task. The enquiries into the 'Bureau's' negotiations about its future and more particularly the perpetual worry of operating and institution without the security of sound financial support had been both time-consuming and immensely wearing and had hampered his own intellectual production. Even though the administrative scholar presents a combination of roles which Lazarsfeld had an obvious capacity for, the innovatory task he had set himself was one of inordinate difficulty, to the point of causing concern in those fond of him. This concern was not only over the fact that Lazarsfeld might be letting himself in for a lot of effort and trouble but also that the plan was not entirely feasible. This last point requires clarification. In practical operational terms the scheme seemed feasible enough and had been carefully worked out. Yet doubts existed, or certainly the question was raised, as to whether the plan would be able to raise financial and intellectual support.

Was it that Lazarsfeld would exhaust himself needlessly when he could devote his talents towards more fruitful concerns? Clearly he was no longer young, and to spend five or maybe ten years on something which might fail was not the type of mistake his friends wished to see him make at the expense of the other intellectual contributions he could offer.<sup>1</sup>

The problem of acceptance operated at two levels, although defeat at the first level largely forestalled the second. At the immediate level, the problem was one of gaining financial support for the scheme within Columbia, and at the secondary level, one of engaging the support and collaboration of the wider academic community. In Lazarsfeld's own words "Columbia was at its lowest as to leadership." And "the university did not sparkle to the idea". This barrier of dull inertia was formidable and had he proceeded with the scheme the struggle would have been both difficult and drawn-out, necessitating the tiresome drudge of attempting to generate interest in the scheme. It is of little surprise then that Lazarsfeld at various points in his writings returns to the theme of the need for administrative leadership, since in this case as in others he had personally suffered the consequences of its absence.

Had he continued and persuaded Columbia to support his scheme the question still remains as to whether the wider academic community would have accorded it the necessary recognition, since in the final analysis this was essential if the centre was to truly fulfil Lazarsfeld's ambitions for it. Certainly, Berelson thought there was a strong possibility that other

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1 Of course, the intellectual longevity of Lazarsfeld could not have been predicted then, thus in hindsight, he could have afforded the possibility of "wasted" years of a younger man. Indeed, it is to this question of support that Merton may have confided to Berelson that the scheme was unfeasible, and not to the question of practicality of operation. That is, Merton really was, as Lazarsfeld suggested, daunted by the prospect of a long drawn out struggle. However, Merton's genuine interest in the scheme should not be underestimated. He was working on professions at that time. In addition, Barton informed the writer that Merton was very disappointed when the scheme fell through.

academics would withhold their support and cooperation and whilst not totally disagreeing with Berelson, it was nevertheless possible that due to the increasing population of other research centres by ex-members of the 'Bureau', plus the reputation of Merton, a sufficient network of initial support might have been forthcoming. The emphasis which Berelson gives to the likely opposition to Lazarsfeld's scheme stems in part from his own experience with Lazarsfeld. The clash which occurred between the two men, and one that is still alive in both their memories, does not particularly concern this work, except insofar as it casts light on Lazarsfeld's administrative style. After having discussed various matters with Berelson the writer mentioned in conversation with Lazarsfeld certain points that Berelson had raised, and Lazarsfeld made the following most interesting remark; "I'm asking you whether you think Berelson is aware how much I think of him as a very gifted person, but a traitor and careerist". (Lazarsfeld 19:7:73)

The idea of "betrayal" captures very well Lazarsfeld's personalised style of operating. One of his basic characteristics as a leader and one which was particularly valuable during the early informal years of institute building, was his ability to command the loyalty of his staff and to weld them into an effective research team. But in return, he demanded absolute personal loyalty from them. In such a characteristic, is a very easily traceable throwback to his formative years in a socialistic youth movement marked by an informal hierarchy based on communal loyalty where loyalty to the leader cemented and held the group together. It was a style which only really worked when the leader although open to questions had in the end undisputed personal authority over his subordinates. It was a style which Lazarsfeld had transferred with a great deal of success to academic organization, and given the situation with which he was confronted it was very appropriate.

However, such a style of leadership cannot operate smoothly where, for whatever reason, the principle of subordination fails to operate. It was easy during the early days of the 'Bureau' when Lazarsfeld had a young research team under him and relationships were close to create the atmosphere within which that principle worked. Indeed, Lazarsfeld informed the writer that he had always preferred the company of younger people, and even today favours it in preference to that of "big shots". It is arguable that the two features are not unrelated.

From his earliest days Lazarsfeld had been accustomed to being in a position of leadership within the authority structure of the socialist youth organisations because the looseness of structure allowed the domination of his commanding intellectual power. Further, the age relationship of the organisation, where the leader was usually slightly senior to that of the other members, served to allay what Marie Jahoda termed, "his idiotic but persistent inferiority feelings". Hence the gathering around him of a number of younger, eager and questioning individuals allowed him a style of authority pleasantly compatible with his personality traits. Within such a situation his superior ability could easily be demonstrated without provoking psychological difficulties. However, although his ability was such that his superiority could easily be asserted against potential challenges it does not follow that he felt personally comfortable in this position. Clearly Lazarsfeld often had to assert himself and as this work has repeatedly shown his personality was such that battling things out came very readily to him. Yet this is not to deny that in general he felt the most comfortable within situations resembling his past experiences in the socialist movement where he was surrounded by junior people who freely accorded him undisputed authority which he could then exercise in a comfortable personable manner. This had been the case at the Forschungsstelle, at Newark and Columbia.

However, it would be wrong to consider that Lazarsfeld actively sought such arrangements in a conscious deliberate manner; rather they were the product of circumstances. Given the new territory within which he was working, and especially the applied nature of much of the work, it would have been virtually impossible during the early years to collect a team of senior personnel with existing established reputations, in addition to which, the limited budget did not allow for such a contingency. Furthermore, both in Vienna and Newark social conditions were such that Lazarsfeld was virtually 'presented' with a pool of young unemployed labour. Undoubtedly the situation was slightly different in America in that a more 'contractual' relationship existed, since in Vienna the labour force had been largely drawn from his own circle of acquaintances and friends. Nevertheless, a central typifying feature of both situations was the intensity of personal relationships, the emphasis on loyalty and the domination of the group by Lazarsfeld.<sup>1</sup>

That Lazarsfeld should feel let down by Berelson leaving the 'Bureau' to seek his 'own' career is therefore understandable particularly since he had been particularly favoured by Lazarsfeld and was consequently expected to identify very closely with it.<sup>2</sup> In that sense it was much more than the 'drill ground' which Shil describes. In short, during the early days,

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1 Berelson even went so far as to describe the 'Bureau' to the writer as a "family".

2 Out of respect for both men the writer does not wish to enter into any detail on this point since not only did their interpretations differ, but also their factual account. The important point is Lazarsfeld's perception of the situation.

socialisation into the 'Bureau' involved more than simply learning the procedures of research; it involved a definite commitment to the 'idea' of the 'Bureau'. This socialisation together with the rigorous training provided, produced the core of a new generation of research workers who directly extended Lazarsfeld's influence far beyond the confines of Columbia. Thus, at the same time that having younger people working under him was a situational necessity, and one which he found psychologically comfortable through long association with such group organisation, it was also extremely functional in propagating the new style of research which he pioneered. Consequently, the writer would argue, that Lazarsfeld's scheme of developing a professional training centre in the social science had elements of likely success attached to it, since, leaving aside the pragmatics of funding, it would have been possible for him to draw on the loyalties of those who had trained under him in order to provide an initial basis for such a centre. Yet whether he could have widened this basis of support, and turned the centre into the over-arching scheme which he envisaged must remain an open question.

The fact was, that whereas Lazarsfeld had personally dominated the 'Bureau', the plan he proposed would have guaranteed him a key structural position from which to exercise a tremendous dominating influence over social research in general. However, the proposal failed to gain the necessary support. Though his efforts do not end there, since a few years later the whole question of a training centre resurfaced when the Ford Foundation took an interest and established 'The Centre for Advanced Studies in the Behavioural Sciences' at Palo Alto.

Berelson who was then the head of Division Five,<sup>1</sup> the division of the Foundation which established Palo Alto, informed the writer:

"Now you probably know I am the father of that. I was in the foundation and Paul thinks that that began as his institute, and Paul will tell you that "my institute failed because the Ford Foundation with all its prestige and money took over my idea and then corrupted it". Early on Paul thought that he would go to Palo Alto and he would have his institute out there and everybody would send their bright students out there that they had contaminated with their Doctoral training, and Paul would set them right. There are two or three people who will tell you they were the intellectual fathers. I was the institutional father. Paul will tell you that... Nathan Leites could tell you that, and with rather more justice. I first got the idea from him"

(Berelson 12:7:73)

It is actually very difficult to determine the sequence of events and ideas, but what is definitely correct, is that Lazarsfeld, with Merton in very much support role, had a tremendous impact on the discussion within the Foundation concerning the establishment of the Palo Alto Centre.

Reading the Ford Foundation docket on the establishment of the 'Centre' one is struck by the number of points which Lazarsfeld made which are accompanied by the note: "Merton agreed, or Merton supported". Indeed, the uniform front of these two men probably did generate anxieties about the direction that the 'Centre' would take. Yet the main point to understand is, that once the arena of discussion about a training centre shifted from Columbia to the Foundation any possibility of establishing a

1 Division five of the foundation was the 'behavioural science' division. As far as can be ascertained it would seem that it was the Ford Foundation which coined the phrase 'Behavioural Science'. The term itself tells one much about the Foundation's positivistic view of the social sciences.

rigorous training centre along the lines proposed by Lazarsfeld was doomed to failure for although the main preparatory work was conducted by a planning committee of twelve<sup>1</sup> the Foundation drew upon the advice of a galaxy of leading academics, and this plurality of interests undermined any possibility of Palo Alto ever becoming an effective training centre for the social sciences. Whilst it might be an overstatement to consider Palo Alto as the 'Centre for the Leisure of the Theory Class'; as Allen Barton called it, (12:7:73) it certainly cannot be said to have met the aspirations expressed for it. For example in the above docket under the heading "Why the Proposal is Recommended" one reads:

"There are at present too few outstanding behavioural scientists to meet the demands for leadership in the universities, in government, in business and in research organisations. The scope of the behavioural sciences has expanded so rapidly and qualified personnel have been spread so thin that, at present, no one university is able to bring together enough high level scholars to organise and conduct the advanced education and research needed for continued advancement of the field. A centre that would bring together leaders in all of the various fields of the behavioural sciences would help to fill the need for post-doctoral education, in addition to contributing directly to the further development and integration of the behavioural sciences. The advantage of attacking the problem at the post-doctoral level is that it tends to increase immediately the number of competent behavioural scientists who can return to the university and train others in the field". (Docket July 15-16 1952 Ford Files)

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1 Lazarsfeld informed the writer that it was a planning committee of eight, but it appears to have been twelve. However, even after so many years Lazarsfeld's memory could be correct since the Foundation docket is very disorganised. This is probably due to the fact that although the Ford Foundation was established in 1936 the 'Behavioural Sciences' were not supported until 1950; thus the newness of operations probably resulted in poor documentation.

As grand as the above aspirations were <sup>2</sup> the basic question to be resolved was how the 'Centre' ought to be organised and, by extension, who should train whom. Once that question was left open to competing academic discussion the likelihood of a decision capable of meeting the Foundations' aims was slight. It is by no means easy to determine what forms of organisation are most appropriate for various forms of knowledge but, reading through the material surrounding the 'Ford' discussion, one is left very much with the impression that in the end, their decision was based on a naive underlying belief, that merely bringing together various renowned scholars was a solution in itself. The whole proceedings must have been a very frustrating and annoying exercise for Lazarsfeld who, drawing on his own institutional experiences, argued for a much more hierarchial and disciplined organisation than the loose 'non-organisation' which triumphed.

#### Lazarsfeld as Educationalist

An educationalist Lazarsfeld is particularly interesting, but unfortunately, and one might add to the loss of educational theory he has

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2 The centre at Palo Alto was only one element of the overall ambitious plans and aspirations which the Foundation had for the 'Behavioural Sciences' in their first report of September 1950 the Trustees of the Foundation summed up, in their judgement, where the work of the foundation ought to be concentrated. They stated, "The critical problems towards democratic goals are today social rather than physical in character. The problems and opportunities of our time arise out of mans relation to man....rather than his relations to the physical world. (Ford Foundation Report 1950) Accordingly four large areas for action were blocked out: World peace, freedom and democracy, economic wealth and education. The nature and scope of the fifth programme of which Palo Alto was an element of, reads: "Permanent Progress towards the solution of the problems heretofore discussed...from war to individual adjustment....require a better understanding of man himself. Everyone of these problems untimately involves man and his conduct and relations with other men. Efforts to increase such understanding must be intensified. Programme area five has this as its purpose. To learn more about man, what he needs and wants, and what incentives are necessary to his productive and socially useful life, what factors influence his development and behaviour, how he

to date left no writings on the subject -- perhaps a characteristic of the practitioner. However, by drawing upon his various comments expressed throughout this work, a view of education can be presented and one which has bearing upon his plan for a training centre. In a memorandum concerning the selection of participants from the Palo Alto Planning Committee, Berelson informed Rowen Gaither, that; "on the basis of my talking around, the following are, in my judgement the most appropriate names suggested." Included in the list is Lazarsfeld's name with the following attached reason for selection.: "Not only as a research technician, but for his thought in developing the Columbia training idea"

(Berelson memo 21:1:52) <sup>1</sup>

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2 Cont....

learns and communicates with other persons, and finally what prevents him from living at peace with himself and his fellow men".

Accordingly the Trustees concluded, "The 'Ford' Foundation will support scientific activities designed to increase knowledge of factors which influence or determine human conduct and to extend such knowledge for the maximum benefit of individuals and of society". (Ford Foundation Report June 1953)

- 1 Paul Hoffman, President and Director of the Foundation had asked Rowen Ghither to assume the responsibility for developing the programme mentioned in the previous footnote. Gaither then appointed Hans Speier of the Rand Corporation and Donald Marquis, Chairman of the Department of Psychology at Michigan University, to act as consultants. They were appointed in the Spring of 1951. This memo was sent to them by Berelson. Berelson himself had been appointed in the summer of 1951 from Dean of the Graduate Library School and Professor of Social Science of Chicago University to head the work. By autumn of 1951, after many discussions with a variety of academics and university administrators, the behavioural science programme was complete. It was then accepted by the Trustees of the Foundation in February 1952. Berelson was made Director of the Behavioural Science division to carry out the proposed programme.

Merton's name was also included with the attached note, "For the integration of empirical and theoretical activities". Yet equally from Lazarsfeld's own proposal for a training centre, one can see that his real value lay in his breadth of vision and not in any technical virtuosity. Indeed during the fourth session of the 'Informal Planning Group' which met on December 20th 1952, at which Berelson was present, Lazarsfeld in discussion with Thomas Cochran remarked that: "any sociologist without a historical bent becomes a dreary technician". (Lazarsfeld 20:12:52) To train "dreary technicians" would have been an anathema to Lazarsfeld, as indeed would any concentration in a training programme that produced a lack of appreciation of other methods of approach. In a fine exchange with Berelson and Hovland, Lazarsfeld exhibited a keen understanding of factors involved in intellectual procedures. For example, Hovland mentioned that Lazarsfeld, "would always tell me to study something else -- decision making not attitude change", and then went on to note, "In 1948 the main difference was that Lazarsfeld was using psychological analysis on a concrete problem (Peoples Choice); I was interested in how one or two principles could be applied to interpret a limited set of data, rather than in bringing in a dozen principles adhoc". Berelson then attempted to clarify the situation by saying, "The Lazarsfeld-Hovland relations were not mainly concerned with methodological devices but with the substance of the problem -- opinion change and attitude formation. If you put it in the form of propositions, the relations would all be clear to both". Demonstrating an acute awareness of the processes behind research Lazarsfeld replied, "It is much more intangible than this. It is a way of going at things. You have to read a lot of other things before you can even begin to think in Hullian terms -- it takes a long time to learn to do that systematically" (Planning Committee Fourth Meeting 20:12:52)

This phrase, the "way of going at things" encapsulates much about Lazarsfeld, not least his concern with the overall procedures of research; "approaches" were not simply a question of altering terminology or of presenting things in terms of propositions understandable "to both" -- "you have to read a lot of things before you can begin to think in Hullian terms". Both whatever terms one wished to think in then, as far as Lazarsfeld was concerned research required direction and that direction was not to be discovered in 'free' association but by the deliberate structuring of relationships within a planned course of work. There was little point for Lazarsfeld in letting the student lead himself. For example, in his own training proposal he states that:

"...the student would have considerably less choice of courses, since to a considerable degree he would be required to select patterns of interlocking courses, rather than a distantly related aggregate of scattered courses, as is so often the case in present graduate departments of a social science. This more rigorous programme of study is intended to lead to a basic grounding in substantive knowledge which can be later utilized as a basis for new enquiry and research"

(Lazarsfeld and Merton 1950:22,23)

Yet it should not be imagined that Lazarsfeld was particularly dictatorial as an educator, indeed one of his criticisms of existing graduate instruction concerned the distant relationship and the lack of involvement between the instructor and pupil in the lecture theatre situation. So far as research training was concerned he felt that "each course must be such as to allow for the student taking an increasingly active part in the work of the course, rather than being a passive and silent witness to formal lectures".

(Lazarsfeld and Merton 1954:22)

Lazarsfeld did not share the view that the student, left to his own devices and choice, would, or even could, master the areas of knowledge required for research: It was not the best method of intellectual development. At the very first meeting of the Informal Planning Group "visions" were present on

types of training. Lazarsfeld's "vision" was that of a "coach" and was followed by the summary statement,

"With reference to Lazarsfeld's vision: there is no doctrine that everything in the development of creative scholars can be made explicit (as for example, is attempted in a bad teacher college). Sometimes we would have to be satisfied with the condition under which appropriate training is communicated without being able precisely to analyse how. The objective of the stress upon codification is to enable the new generation to routinize the creativity of the old, to cut down the area of intuitively creative in the field and thus to extend the line of communication within and between generations"

(Informal Planning Group 8:11:52)

Certainly, Lazarsfeld would admit that everything could not be made explicit, but the object of the exercise was to make matters as explicit as possible in order to purposefully communicate to others. Furthermore, teaching was important to Lazarsfeld in that it made for clarification in the same way as empirical formulation forced clarification of concepts.

Lazarsfeld's view that research was not a mysterious activity, but one which could be articulated and indeed had to be articulated, made him a forceful supporter of the desirability of reducing the teaching situation to sets of simple demonstrable forms. Having mastered the basic sets, the student then moved through a series of increasingly difficult, but tailored situations, until he emerged as a creative researcher in his own right. Undoubtedly social insight and creativity as components of research activity, are not easily reducible to convenient teaching form. However, in Lazarsfeld's view even these elements should not be made a mystery of as something that could not be catered for within the training process. Whilst recognising that students differed in their "natural capacity" for creativity and insight. Lazarsfeld nevertheless knew from experience, that these abilities were rooted in the systematic study of broad areas of knowledge. Thus, by skilful incorporation of a variety of

approaches to problems and the inculcation of a broad yet systematically based knowledge, even these most elusive of qualities could be provided for. On the basis of Lazarsfeld's training then, the student presented with an intellectual problem could, it was hoped, recognise the socially relevant as opposed to the spurious correlation and proceed to construct patterns of socially significant relationships. Further, by bringing to the task of observation and analysis a broadly based knowledge, the student was less likely to miss the idiosyncratic or seemingly trivial factor which, when recognised for what it represented, enriched the studies' interpretive quality. In short, if creativity could actually be induced in the student it could at least be provided for.

However, this training process required a structured organisation, and as Berelson clearly intonated, it also involved the question of control. This question of control as a sensitive point in the administration of the Palo Alto Centre is clearly highlighted. In a progress report written in June 1954 by Ralph Tyler, formerly Dean of the Social Sciences at Chicago and first Director of the Centre:

"A chief problem in programme planning grows out of the two polar views of the role of the Centre. On the one extreme are those who view the Centre primarily as a place for individuals to work along on their research, freed from the distractions of other university activities. On the other extreme is the view of the Centre as a post-doctoral teaching institution, where juniors would be taught more advanced techniques by able seniors. After a great deal of discussion on the part of the Advisory Committee, a proposed statement of programme policy was formulated, which was adopted by the Board on February 13th 1954. This policy emphasizes a balance of individual study and group seminars."

(Tyler 23:6:54 Programme Report)

This approach represented a defeat for Lazarsfeld's arguments for a more centrally organised, and rigorous programme of training for younger scholars. Basically, the 'Centre' was turned over to the established academic community for its own "leisure" and not to the new young up-and-coming generation of researchers. From the evidence contained within the Ford archives, it is

difficult to make substantiated judgements on the motivations behind the preference for a loose organisation rather than a more centrally planned one. Given the stated aims of the 'Centre' the pattern of informality which developed did not make very good administrative sense. Thus, one is left with the conclusion that personal doubts and fears of domination played an important part. A supportive to such thinking can be found in the brief statement "The Board felt that the Centre should not be located on the campus of a major university <sup>1</sup> because of possible distractions, request for services, institutional jealousies and the like" (Report 1953 General Files 'Ford') 109-112-itals mine.

Rather than have the 'Centre' develop into an important force in research education along the lines suggested by Lazarsfeld, a more individualistic emphasis was adopted in order to guard against the excessive influence of anyone or person or group. But in so doing it also undermined its effectiveness.

Of the many matters which the writer discussed with Lazarsfeld, it was on the question of failing to establish a training centre that he gave the greatest impression of personal disappointment. His own account of the proceedings are as follows:

D. Morrison: I gather you were very disappointed with Palo Alto?

P. Lazarsfeld: Yes well you see..

D. Morrison: Did you have high hopes for Palo Alto?

P. Lazarsfeld: It's important that you understand that -- the same same document you see (Original proposal) -- this was submitted as a proposal for the Ford Foundation, and out of that ...this is very important that you understand..because of the time sheet really.

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<sup>1</sup> However, it was felt that the 'Centre' ought to be located near one or more major University, so that advantage could be taken of established library and other academic facilities. The closest University was Stanford.

P. Lazarsfeld: This proposal was submitted to Division Five after I gave up getting it through Columbia. Berelson was then at Ford Foundation and they appointed a committee of eight people. The agenda of this committee which met a whole year was to advise the Ford Foundation -- should they create this training centre. In the course of these debates increasing opposition developed mainly guided by Herb Simon and Karl Hovland.

D. Morrison: On what basis?

P. Lazarsfeld: That it is too hierarchial, that it doesn't leave enough freedom, it's too programmed. Well remember it was to be, so to say, a graduate school in the social sciences. It was very much meant like a medical school. The memorandum (original proposal) - if you read it carefully - is very obvious and that is what they were against. The Ford Foundation said: "If you spend money on a centre don't make it a school make it" - now there are two terminologies. My terminology is to make it a gentleman's club for the benefit of the publishing industry to get manuscripts, but their information was to make it a "free assembly of inter-disciplinary groups". Look, even the age distribution would have been different. I would have said have sixty or seventy percent of people around thirty, and twenty five percent around fifty five to sixty. I remember I used to use the term "passing on the torch" etc. The actual age of the 'Centre' is around forty-five with a few young and a few old. So it was the anti-school idea...and the idea of a highly subsidised -- well I don't know too much about that, but it probably was an Oxford rather than a medical school. But an Oxford without students, only fellows..... Three hundred years ago one used to call a college a society of fellows, or something, and it's the society of fellows which won out. And then I went. I am the only one of those first eight people who ever went to the 'Centre'. You see I went the first year trying to rescue, but you know, it was then settled by the appointment of the first director, Ralph Tyler, who was appointed as the main exponent of the laissez-faire. You have to understand -- on the one hand this document inaugurated the discussion of the Centre, but what happened to the 'Centre' is far away from that.

D. Morrison: Do you wish you'd fought far more for the...?

P. Lazarsfeld: Well, in the Committee, in the Ford Committee no fight would have helped me. The only place I could have fought it through would have been at Columbia. You see, once it moved from Columbia to the Ford Foundation the fighting...that's what I meant before. Now, that is a defeat which I still mind. And I still feel it's wrong.

(Lazarsfeld 15:7:73)

Looking back over Lazarsfeld's life it is understandable that he should say that "it is a defeat I still mind". It was a defeat for the culmination of the work begun half a century ago in Vienna. Of all the many and varied contributions which Lazarsfeld had made to social enquiry the writer prefers to single out his contribution as an institutional innovator as the most neglected aspect of his work, yet at the same time, one of fundamental importance. Academics tend to be remembered for the work they have produced in the course of their life, and certainly, along with others of his generation, Lazarsfeld will also be accorded a place in the history of the social sciences. However, with the development, and movement in thought, 'history' tends to be forgetful of those who have contributed towards it; their work is represented by name rather than as a body of knowledge which is still read for stimulation and guidance. Yet Lazarsfeld's lasting contribution, and one which will outlive his contemporaries, is his overwhelming contribution to a whole system of procedure-organised empirical social research.

Despite John Rex's insinuations, the fact that so much empiricism is barren is not Lazarsfeld's responsibility, but the fault of the practitioners themselves. Certainly, his own intellectual interests lay in the development of empirical methods of research, but they were matched by educational aim of imparting the knowledge which would allow those methods to be utilised in an intelligent and imaginative manner - "to save the would-be technician from himself". The manner in which this could be done has already been discussed in relation to his idea for a training centre in the social sciences,

but it is his genuine intensity of interest in education which must be emphasised. This is something which Berelson overlooked in Lazarsfeld, with the result that he could not understand why he spent so much of his later years involved with educational institutions.<sup>1</sup> Lazarsfeld's move to Palo Alto for the first year of its operation was not simply to, "have his institute out there", but to have an institute which he considered appropriate to the demands of the educational situation. Perhaps this is a symantic slight, and both Lazarsfeld and Berelson's interpretation of his residence are fairly proximate. However, not to see his move in wider educational terms is to miss much about Lazarsfeld which is important. His interest in an educational institute was not that of "empire-building", but reflected a deep-felt desire to improve student training, and by extension, to improve social research. Unquestionably, Lazarsfeld would have vastly extended his sphere of influence, but to see it purely in those terms is to confuse process with motivation, and to over-simplify the complexities of the latter. Without doubt, he wished to establish an institutional framework which could have communicated the best in existing knowledge, and, as he himself put it, "to produce Paul Lazarsfeld's". Yet it was not the holding up of his own work as the ideal model of research, but rather the communicating of his own accumulated knowledge on research which was of major importance for him. Lazarsfeld would never claim that his work represented all that was best in the discipline's history, but what he would claim, was that he had a vast amount of knowledge which the student could fruitfully draw upon, and indeed needed to do so. He would further claim that he could lead the student through the history of the field and point to what was best within it, and then set him to work

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1 Interview with Berelson 12:7:73.

on concrete problems in a supervised situation so that the student had to demonstrate his knowledge in real rather than abstract terms. There was little of the modern 'educational relativist' about Lazarsfeld. There was no confusion as to who possessed the greater wisdom, and little confusion as to the benefits of rigorous planning in educational matters. However, this rigorous planning was to be within a co-operative rather than authoritarian learning situation.

The fact that he failed to establish such an institute was bound to be a source of disappointment to him, indeed his regrets may well have increased on reflection. Since his earliest days in Vienna Lazarsfeld had been keenly aware of, and involved in, educational experiments and the discussions which the Austrian Social Democrats had instigated. Further, to establish a viable research institute Lazarsfeld had not only to build a workable organisational format, but as an integral part of that process, to train the researchers necessary for its functioning. For his own part, the whole idea of training and the establishment of a research institute cannot be meaningfully separated. What Lazarsfeld learned in the process was that much knowledge required for research could be formalised and taught. Thus, once Lazarsfeld had established the 'Bureau' it was understandable that his long-standing interest in education should turn in a new and innovatory direction. In a sense it would have meant the cementing of his past institutional and educational work. Having failed to interest Columbia in his plan, as well as witnessing the Foundation's deviation from it at the Committee level, it was characteristic of his commitment and tenacity to go to Palo Alto to attempt to rescue operations and save it from becoming an "Academic Monastery". (Fortune Nov. 1955:222) An academic monastery was far from his concept of what Palo Alto ought to be, and also, far from his own beliefs as an educator.

## Chapter Seven

### The Continuing Problem of Funding and its Consequences for the Production of Knowledge

## Chapter Seven.

### The Continuing Problem of Funding and its Consequences for the Production of Knowledge.<sup>1</sup>

Whereas the main body of the work has concentrated on the internal difficulties arising from particular organisational practices, this final chapter broadens the scope of the analysis and considers the problems presented by external pressures. More particularly, it takes up the issue raised by Lazarsfeld and examines the ways in which the lack of guaranteed independent funding structures the range and content of the sociological knowledge produced by research centres.

Charles Glock has usefully distinguished two main sorts of research situation; those where money is given for work on a project defined by the donor, and those where finance is provided for studies proposed by the research organisation. Further he adds that while there are a range of sources for the first sort of funds including "government agencies, social action and welfare groups, labour unions, political parties, business and industrial concerns, until now philanthropic foundations almost alone typify the second". Whilst pointing out that in general, foundation support allows research organisations a greater degree of autonomy

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1 This chapter is based largely on a paper which the writer delivered at a Conference organised by the Vienna Institut für Höhere Studien und Wissenschaftliche Forschung entitled "Determinants and Controls of Scientific Development", (Graz, June 1974). The original paper is to be published with a commentary by Lewis Coser in "Determinants and Controls of Scientific Development", Knorr et al (ed) Reidel Dordrecht 1975.

and operational flexibility he adds that this freedom and fluidity is in fact "more limited than it first appears" since "like other groups, the foundations are responsive to the needs and concerns of their environment" and that consequently "their foci of interests are not wholly a product of their academic environment" (Glock 1951: 131). Having raised this crucial point, however, he neglects to develop it. This chapter therefore begins where Glock's analysis leaves off, and explores the ways in which the grant giving policies of philanthropic foundations are structured by pressures within their social and political environment, and the ways in which these policies in turn structure the production of knowledge within research institutes. In order to do this however, it is first necessary to briefly outline the nature of the philanthropic foundations and their position within American society.

#### Philanthropic Foundations: The Problem of Legitimation

Much of the work written on philanthropic foundations has been produced by persons who have at some time been connected with such organisations, and usually at a very senior level. Whilst furnishing valuable information with regard to the running of foundations, such writings have tended towards the anodyne, offering little critical perspective. Works by non-foundation men, on the other hand, have usually attempted an

overview of foundations' activity, and have suffered from a surfeit of description at the expense of analysis. One result of such work has been the neglect of an important aspect of foundation activity, namely their role in the production of knowledge. Certainly, all works on foundations note at some point the support that these institutions have given to knowledge production and often they appraise trends in funding, but rarely have the mechanics of this relationship been analysed in any terms other than a crude cash nexus. A notable exception is the perspective developed by Coser (1970). Taking this as a starting point, this chapter explores the role of the foundations as 'gatekeepers of contemporary intellectual life' and examines the ways in which changing social and political climates have influenced the operation of foundations, and through them the production of knowledge.

The vulnerability of philanthropic foundations to changing social and political climates is in part due to their failure to legitimate themselves. The concept of legitimation is used here in Lipset's (1960: 46) sense of "the degree to which institutions are valued for themselves and are considered right and proper". It is argued that the foundations as institutions have failed to become 'accepted', in the sense of being taken as part of the natural ordering of things, having an unquestioned right to exist and operate. Further,

such is the lack of acceptance that when controversies do break, in contrast to the universities which have legitimated themselves, the whole existence of the foundations can be called into question. This lack of basic acceptance is illustrated by the fact that the perceived indiscretions of one particular foundation have often been seen as symptomatic of foundations as a whole.

To understand this lack of legitimation two important factors must be taken into account. Firstly, the circumstances deriving from the historical and cultural situation surrounding the establishment of many of the large foundations, and secondly, the way in which their operations have developed over time. For despite the gestation difficulties confronting the formation of many foundations, it is likely that they could, in time, have legitimated themselves had it not been for certain operating principles which led them into the controversial area of the social sciences. Once foundation funds began to flow into the social sciences it proved a quagmire from which there was little or no possibility of extraction into acceptance. The foundations were sucked into a vortex of controversies which they were ill-suited to withstand. Thus, one has two stages in the barrier to legitimation: the early suspicion surrounding the foundations' establishment which they gradually began to erode, followed by their entry into the social sciences when suspicion once more engulfed them.

Despite antecedents in other periods and in other cultures, the modern philanthropic foundation is a peculiarly American, and essentially twentieth century institution.<sup>1</sup> For as William Whyte Jr. notes:

"The concept of a private foundation for public progress is a very American development and a fairly recent one. Up until the late nineteenth century, businessmen used to give largely for the alleviation of misfortune, local usually, rather than prevention of it. But capitalism kept piling up more and more personal surpluses. Even with Newport chateaux, yachts and heirs, there was more left to give than they could give intelligently." (Whyte 1955A: 111)

Leaving aside the psychological motivations behind this philanthropy,<sup>2</sup> it is true as Lindeman notes, that:

"The distinction between ordering private charity and large scale philanthropy is the difference between a small and a large surplus. The former may remain on a personal level, but the latter involves organisation." (Lindeman 1936: 9)

Philanthropy was put on the same entrepreneurial footing as productive enterprise. However, if the foundations provided their originators and patrons with an organisational solution to the problem of rationalising the distribution of large scale

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1 For a brief but comprehensive history of the evolution of philanthropic foundations see Hollis (1939).

2 One of the factors producing suspicion against foundations is the difficulty in assessing the 'real' motives of the donors in setting up foundations. On the surface, such generosity simply did not fit with their other public behaviour. For a pertinent description of the donors see Nielsen (1972: 10-11).

personal wealth, they were not always greeted with universal applause. This lack of acclaim stemmed in part from the recognition of the dubious sources and methods which had facilitated such endowments. Fosdick, in his history of the Rockefeller Foundation, notes that such wealth was made "under conditions unique in the history of the country and not infrequently by methods which, if permissible at that time, no longer accord with social conscience or the requirements of the law". (Fosdick 1952: 19)

The 'tainted money' spectre is a familiar and long standing theme in the history of foundations. The establishment of Guys Hospital in London out of profits made by Sir Thomas Guy from the South Sea Bubble adventure<sup>1</sup> provides an early example. A more recent instance is the refusal of Mayor Hylan of New York to accept foundation assistance to build a hospital on the grounds that the money was tainted.<sup>2</sup> Another suspicion hanging over the early foundations was that their founders would use them to support their own economic views and practices.<sup>3</sup> There

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1 The South Sea Bubble adventure of 1720 was a commercial swindle and caused widespread alarm coming, as it did, among many other such swindles.

2 For a discussion of this see MacDonald (1956: 23).

3 For an argument against conspiratorial theories of foundation influence, in favour of a more subtle analysis of foundation power in protecting the value system in existence, see Lindeman (1936: 12). This is very similar to the position adopted by Whyte Jr. (1955b: 22) when he considers that foundations 'mirror the times'.

were therefore two strands to the early suspicion of foundations, one associated with the sources of endowment, and the other with the uses to which such monies might be put.

Although the cry of tainted money was a factor to be overcome if the foundations were to legitimate themselves, it has in recent years been raised less and less. Not so, however, the concern over the use to which foundation funds would be put. That fear has been persistent but, at the same time, it must be stressed that the focus of this suspicion has varied over time. For example, whilst the original criticism tended to cluster around fears that funds would be used to restrict social change, during the 1950's there was a volte-face, and criticism focused on fears that the foundations were encouraging radical social change. The suspicion that donors would direct funds to enhance their own industrial interests has, however, remained a constant source of criticism.

In the 1915 hearings before the United States Industrial Relations Commission, for example, one can clearly see concern over both the purpose to which such funds were being put, and the power of foundations to exert influence over recipients. Thus the Report of the US Senate Commission on Industrial Relations claims that:

"... the so-called investigation of industrial relations has not, as it is claimed, either a scientific or a social basis, but originated to promote the industrial interests of Mr. Rockefeller."

In addition, the Report outlines as an example of foundation power:

"The abandonment by several colleges and universities of sectarian affiliations and charter clauses relating to religion in order to secure from the Carnegie Corporation pensions for the professors and for the advancement of teaching. It would seem conclusive that if an institution will willingly abandon its religious affiliations through the influence of foundations, it will even more easily conform to their will in any other part of its organization of teaching."

(Manley Report 1916: 83)

This investigation into foundation activity made headlines for weeks and generated great concern and apprehension as to what might follow from the foundations' power.<sup>1</sup>

These early suspicions have not been easily shaken. The Texas Democrat, Wright Patman, still launches attacks upon the foundations in this vein. He considers that foundations are often used firstly as tax dodges, and secondly to maintain control of industrial concerns by having stocks held by one of the foundations associated with the industrial enterprise. Although it is certainly true that in some cases foundations, such as the Howard Hughes Foundation, have at times not operated entirely within the spirit of the law,<sup>2</sup> it is a mistake

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1 It is interesting to note the emergence of the press as a factor in the generation of concern over foundation activities - see Nielsen (1972) for an example of this case.

2 Cook (1963) provides a very good outline of Patman's grievances against foundations, singling out the Howard Hughes Foundation as an example of a 'tax dodge', the Lilly Endowment for supporting right wing political groups, and the Rockefeller Foundation for commercial advantages.

to argue that foundations were created and continue to operate primarily as a tax benefit. At the same time, it is true that benefits can accrue to the donor's family in the course of operations, and that foundations have been loathe to sponsor work that may run counter to the donor's industrial interests.<sup>1</sup> However, if the suspicions surrounding the motivations behind donations have still not entirely evaporated, they have abated. Hence, in the natural course of events, one could reasonably have expected a growing acceptance and legitimation of the foundations. This was not to be however, for just as they were beginning to become 'established' in American culture they created a new difficulty for themselves. Propelled by their own principle of 'improving man's estate', they moved into the social field which, when allied to the professional ideology of 'risk capital philanthropy', created problems from which there was little respite.

#### Entry into the Social Sciences - The Paradox of Involvement

"... whenever foundations get into trouble, as they do from time to time, it is practically always because of something they have done in the social field, or in the application of questioned social theories to business or industry." (Hollis 1938: 245)

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1 For example, the foundations connected with the chemical industry have been reluctant to take action on air and water pollution, and those connected with the automobile industry have been reluctant to finance research into car safety. Nielsen (1972: 319)

As long as foundations operate within the area covered by the social sciences then the possibility of them extricating themselves from controversy is extremely limited. It is therefore valuable to briefly trace the manner in which foundations came to find themselves in this uncomfortable situation.

Whereas the very early American foundations were usually established through endowments for limited purposes, later foundations were established for general purposes.<sup>1</sup> The generalist nature of modern foundations is reflected in the vagueness of their establishing charters. 'Education' is the term that occurs most frequently in the legal instruments, in fact nearly all the donors appear to have had education in mind when making their bequests.<sup>2</sup> This orientation is explicable firstly by reference to the symbolic role that education occupied as the "sovereign remedy for social ill" (Hollis, 1938: 116), and secondly to the unfavourable public estimates of the elder John D. Rockefeller and of Andrew Carnegie which militated against direct reformist grants and in favour of general non-controversial endowments.<sup>3</sup> This is not to say

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1 For a discussion of the variety of foundations and the establishment of those for general purposes see Andrews (1956: 11-13).

2 Lindeman (1936: 26) provides an interesting discussion of donors' interest in education, and their belief in it for making the democratic ideal workable.

3 Hollis (1938: 128) comments on this, and notes that, except where there were scruples against accepting tainted money, there could be no objection to accepting grants for the raising of professorial salaries or for general building endowments.

that many of these early general grants did not have far-reaching reforms skilfully embedded in them - they did.<sup>1</sup> For example, the system of 'Carnegie Units' by which educational institutions could qualify for professional pensions only after certain conditions had been met, had far-reaching consequences within the American education system.<sup>2</sup> Such large-scale across-the-board grants eventually came to a halt however, partly through the sheer economics of the situation, and partly because such gifts became increasingly redundant as government assumed greater responsibility for education and welfare.

The economics of the situation are perhaps best summarised by the President of the Rockefeller Foundation. Reflecting on the situation in the 1920's, he noted that:

"... there was a sense therefore in which 1928 [the amalgamation of many of the foundations in the Rockefeller group] marked the turning point in the thinking of the Trustees. This was in part due to the fact that they had come to the end of an era in philanthropy, an era that was reflected in many other foundations as well. Huge sums had been spent in the endowment of medicine, public health institutes and programs in higher education. Apart from the foundation's contribution, the General Education Board had given 50 million dollars on a matching basis to raise the endowment of colleges and universities, and over 90 million dollars for American medical schools. This type of giving could not continue without involving the rapid liquidation of the Rockefeller Boards", hence "a new orientation of target ... became a vital necessity as the twenties drew to a close."

(Fosdick, 1952: 159)

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- 1 There is some confusion regarding the Carnegie Units, centred on the question of the deliberacy of the ensuing reforms and responsibility for them. President Pritchett (1935: 31) lays claim to the responsibility for implementing such reforms and absolves Andrew Carnegie of any responsibility - Hollis (1938: 128-129).
  - 2 Horowitz (1972) makes a scathing attack on foundations' role in shaping American higher education - he particularly criticises the Carnegie Units for institutionalising the PhD qualification. In addition, see Jencks and Riesman (1968: 240-242) for the importance attached to the PhD qualification, even within areas of teaching not considered to benefit directly from such a qualification.

In addition to the basic economics of the situation there was the fact that the early part of the century had provided plenty of opportunities for donations on a massive scale: there were colleges and libraries to be built, obvious social reforms to be aided, and, since government contributions to such areas was small, foundation money represented a large proportion of the whole.<sup>1</sup> But, as government funding began to catch up, foundations were forced to search for new areas in which to maintain the effectiveness of philanthropy, and hence, they increasingly diverted funds to what Horowitz (1972: 245) calls "the growing edge of knowledge".

Support for the social and humanistic sciences was virtually non-existent before the First World War, but during the 1921-30 decade, as well as for the succeeding half decade they became the dominant foundation interest (Hollis 1938: 116). The First World War was undoubtedly the principal factor behind this shift. The war had demonstrated the benefits attendant upon social science research and consequently the tide of opinion began to run in favour of intervention in areas that had previously been closed. As Lyons notes:

"... the wartime uses of intelligence testing set a precedent elsewhere in government and especially in private industry. Indeed, the research work that psychologists had performed for the military was called a 'war gift to industry'; private companies began to use psychologists, consulting firms were organised and new techniques invented." (Lyons 1969: 31)

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1 Whyte Jr. (1955a: 112) suggests that, because foundation giving is not a large proportion of the whole, foundation influence has declined. However, Berelson (1960: 6) argues that the importance for influence is not the overall amount but the trend of support.

It was within this improved atmosphere that Beardsley Rumel, as head to the Laura Spelman (Rockefeller) Memorial, spent fourteen million dollars of the foundation's money between 1922-29 "in order to put the social sciences on the academic map". (Macdonald 1956: 24). As a result of this massive injection of financial support:

"In the years prior to World War Two it was practically impossible to write a text in any of the social sciences without relying on findings from research financed by the private foundations." (Young 1969: XV)

But seen in a broader historical perspective, the importance of the foundations' contribution to the social sciences rests not so much on the individual studies that they supported, but rather on the general style of academic work that they encouraged. Although America was not the original home of quantification, it certainly adopted those techniques on a large scale,<sup>1</sup> and a significant factor in aiding this adoption was the willingness of the foundations to provide the necessary funds for such expensive research. In addition, and perhaps more importantly from the point of view of propagation, they supported and encouraged the institutional forms within which such work could flourish and expand; research institutes, centres,

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1 Lazarsfeld mentions that "Nothing is stranger than the idea often expressed by European colleagues that quantification is a United States export endangering their tradition. It is true that when this country [America] took over the European research technique it did so on a large scale." (Lazarsfeld 1961: 332)

bureaux and other similar offices. In fact the driving idea behind Ruml's giving of the Spelman Memorial money was "to develop major centres of research",<sup>1</sup> and as Ogg notes:

"The single fact that, doubtless for good and sufficient reason, foundations and other patrons and benefactors will rarely place money directly at the disposal of an individual, but will turn it to the support of a council or bureau or other continuing, responsible, co-operative organisation, would alone account for the emergence of many of our present research groups or bodies." (Ogg 1928: 156)

The 'good and sufficient reasons' mentioned by Ogg derive from the high value which the foundations placed upon quantitative work, and in particular "the fact finding non-controversial areas of the social and humanistic sciences"<sup>2</sup> which afforded them protection against criticism. To understand the attraction of the 'scientific' aura, associated with quantitative work, it must be remembered that the foundations' interest in serious research developed first in the natural and physical sciences, and that their attitude towards the social sciences was to some extent coloured by that experience. Hence the foundations tended to concentrate on those areas of social science which in their view most closely resembled the natural sciences, and which at the same time gelled with their own organisational needs and with their problem solving approach to society. For

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1 Fosdick (1952: 217), also for a list of institutes that the Laura Spelman Memorial under Ruml helped support.

2 Hollis (1938: 289) and also for public attitude towards science.

example, Hollis, writing in 1938 accuses the foundations of having foisted natural science methods upon the social sciences, and writes:

"The point to be established is that the social sciences are undergoing a profound and rapid development, and that more than any other supporting agency philanthropic foundations are pointing and guiding this phase of American higher education"

and further:

"... They have I fear been the chief offenders in forcing the techniques of research which developed in the natural sciences onto the social sciences and humanities." (Hollis 1938: 255)

Yet ten years later, Donald Young, Director General of the Russel Sage Foundation comments:

"The appreciable measure of success which has attended the increasing concentration on making social science more scientific gives promise that further effort in direction will be rewarding." (Young 1948: 330)

Of course the spread of empirical methods and associated forms within American social science was a complex phenomenon produced by the interplay of a number of forces. Nevertheless, although foundations were only one of the influences operating to encourage the empirical strain within American social science, they were undoubtedly a very important one. According to Bernalson:

"The foundations contributed to the predominant tone of the social sciences in these (early) decades, namely the empirical rather than say the reflective. This was a distinguishing feature of the 'new social sciences' and one

hardly to be discarded or even modified much. But the foundation did go along with this trend and even encouraged it, with all that meant by way of natural observation, segmentalisation of problems, quantification and the rest.\* Certainly the foundations did not try to halt or delay the movement towards empirical analysis of human problems ... quite the contrary." (Berelson 1960: 8)

Operating in the Social Sciences - The Continuing Problems of Legitimation.

As social problems have become 'visible', and as a body of knowledge has developed laying claim to the possibility of solution or amelioration, the foundations have been impelled by their principles of improving man's welfare<sup>1</sup> to engage the services of this knowledge to discharge their philanthropic duty.<sup>2</sup> This is the developing paradox. For, in order to fulfil their philanthropic obligations, the larger foundations have been obliged to engage in activity that requires the help of social scientists. But to enter such a world is to enter the world of

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\* One might include research institutes among "the rest".

1 See Bremner for factual evidence of the principle of 'improving man's welfare', particularly with reference to the commitment of Rockefeller and Harkness to this principle. Bremner (1960: 193-194) cites, for example, the Rockefeller charter, which includes a commitment to "promote the well-being of mankind throughout the world".

2 One of the problems with foundation activity is that, because the role is one of high affectivity, it is difficult for officials to gauge when their role has been discharged. One ironic index suggested to the writer during an interview with Rossant, Director of the Twentieth Century Fund, was that of 'criticism'. Rossant was possibly bolder than most foundation officials in suggesting that by using the principle of 'risk capital philanthropy', criticism was an indication of role fulfilment. (Interview with Rossant in New York, 19 June 1973.)

values and controversy at a level so direct that it has thrown into sharp relief the weakness of their position.

This paradox of affirming their principles at the possible cost of endangering their existence has within it a further paradox. Recognising the possible consequences of entering controversial areas, the foundations have entered cautiously and at times defensively, and have by and large withdrawn in the face of criticism. As a result they have failed to develop internal support systems, which in turn means that they constantly risk further attacks which threaten their existence when controversial areas are entered.

Despite this lack of structured supports the foundations have not been completely unprepared in the face of criticism however. Quite apart from the fact that, as some commentators have noted, they tend to support safe and non-controversial projects, they have developed certain protective procedures. For example, nearly all foundations take the precaution of inserting statements of disengagement in published reports, either in an introductory note or else in a separate statement. But this device is not particularly effective for, as Emerson Andrews (1956: 164) points out, the findings "will inevitably be attributed to the foundation itself". A much more effective defence, and one that has had a major impact on the type of work that foundations are willing to support, is the insistence on 'objectivity' in the conduct of research. It is difficult, of course, to separate the foundation officer's insistence upon

"objective" methodology as a product of his situation, from insistence based upon intellectual training. Nevertheless, there is good reason to suppose that within the foundations, the emphasis upon the 'objective and scientific' is promoted far more than it is in the university by considerations of defence from criticism. It may be true, as Gouldner (1973: Ch.3) argues, that the insistence upon objectivity was one process by which sociology legitimated itself as an intellectual discipline, but within the foundations there is in addition a conscious awareness of its protective value which can be set aside from its perceived intellectual merits. This was revealed very clearly during conversations with several foundation officials, and is well expressed in the following extract:

"Where foundations have got into trouble is where they have done something political and not scientific. It is rare that a really good solid research project with good scientific methodology which is supposed to produce objective results is going to get criticised." 1

Clearly, a defence resting upon an adherence to 'scientific methodology' can only have persuasive power within a culture which regards this as the correct method, and what is considered correct procedure at one time can change and turn into

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1 Dr. Klein (Russell Sage Foundation) and four colleagues in New York, 24 May 1973.

its opposite at some other time. Such a situation occurred during the 1950's when the Reece Investigation stridently attacked the foundations for their scientific procedure.<sup>1</sup>

It is crucial to understand the social pressures surrounding the foundations since it is these which generate or fail to generate confidence, and thereby set the parameters of operation. The intention now, therefore, is to examine the impact of the changing social climate on foundation confidence and to indicate the repercussions for funding policies. Following this general outline, the analysis will focus in depth on one particular case where an unfavourable social climate undermined a foundation's confidence and led to the closure of a particular line of academic enquiry. The initial overview of fluctuations in foundation confidence is necessary in order to provide a more total understanding of the structural situation of foundations and to situate the case study within a more general context.

#### Changing Times - Changing Confidence

Foundation support for the "social sciences" began in earnest with the Rockefeller Foundation's entry into the field of industrial relations research. From the beginning this was a highly controversial move, but the delicate balance of the

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<sup>1</sup> Norman Dodd, Research Director of the Reece Committee, in particular strongly attacked the foundations in his report to the committee. The dislike of empiricism was based on the distaste for fact gathering at the expense of promulgating 'traditional American values'.

situation was finally tipped in 1914 by the eruption of a strike in one of the Rockefeller controlled companies - the Colorado Fuel and Iron Company - which culminated in the savage 'Ludlow Massacre' of strikers by the State Militia.

"The Rockefeller Trustees were given an unforgettable lesson about the hazards of becoming involved in social and economic issues, especially if the foundation itself undertook to carry out the work. As a matter of policy they decided thereafter to restrict the foundation's direct operation to scientific areas such as public health, medicine and agriculture. If the foundation supported work in controversial social fields, it would do so through grants to other independent institutions." (Nielsen, 1972: 53-54)

This incident was followed by the establishment of the 1915 United States Senate Commission on Industrial Relations, which as mentioned above, was heavily critical of the Rockefeller Foundation. As a result, the Foundation offered no further significant support for the social sciences until the 1920's.

Between the "unforgettable lesson" of the 'Ludlow Massacre' and Rockefeller's second attempt at entry into the social arena, the climate of opinion had changed considerably. Not only had the social sciences become more acceptable through their war services, but agencies such as the Department of Agriculture were engaged in more systematic research and in a more sustained utilisation of social science research.<sup>1</sup> This

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1 Some progress had already been made in this direction prior to the war - for example, the Progressive Movement in the rural mid-West, which agricultural economists and rural sociologists were to continue during the New Deal (Lyons 1969: 31).

increased 'visibility' of the social sciences, and more particularly their growing acceptability, facilitated by the 'war gift to industry' provided by psychological testing, allied with the growing public toleration of trusts, both commercial and philanthropic,<sup>1</sup> lessened the possibility of a repeat disaster. In the words of Fosdick, President of the Rockefeller Foundation, writing of the grand entry into the social sciences by the Laura Spelman Memorial and commenting on the favourable social atmosphere of the times, "it must be admitted that for this activity the environment of 1922-23 was propitious".

(Fosdick 1952: 215)

If the First World War eased the foundations' path of entry into the social sciences, a further impetus was provided by the employment of social scientists in the New Deal Administration,<sup>2</sup> and even greater encouragement was given by the Second World War. Nevertheless, criticism is a constant reminder of the foundations' tenuous position, and even though the social sciences may have been receiving increased support from the foundations,<sup>3</sup> particular areas within these disciplines

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1 It is important to recognise the antagonism which was directed against philanthropic trusts as part of a more comprehensive antagonism directed against holders of great wealth.

2 The interesting point is not so much the actual benefits that social scientists gave during the New Deal, but the recognition by the pragmatic administration of Roosevelt of the social scientist as adviser, a role traditionally reserved for businessmen and lawyers.

3 This refers mainly to the large foundations.

remained dangerous places to venture. The awareness of this situation is well illustrated by Carnegie's trembling involvement with the area of race relations.

Despite the increased confidence built upon the growing acceptance of the social sciences, the foreword to Gunnar Myrdal's 'An American Dilemma', written in 1942, is pregnant with worry and the bated expectation of punitive criticism. The whole of the four page foreword is a justification for attempting such work, even to the extent of extolling the past works of the Corporation and urging the reader "that he make every effort to read these statements intellectually and not emotionally". Despite engaging in a search for a scholar from a country "with no background or tradition of imperialism", Keppel, President of the Carnegie Corporation, still felt it necessary to exonerate the foundation:

"Provided the foundation limits itself to its proper function, namely to make the facts available and let them speak for themselves, and does not undertake to instruct the public as to what to do about them, studies of this kind provide a wholly proper, as experience has shown, sometimes a highly important use of their funds." (Keppel 1944: V)

The favourable atmosphere which facilitated this venture into such a sensitive area as race began to ebb with the build-up of the Cold War, especially after the outbreak of the Korean War in 1950. The ensuing tensions resulted in a renewed suspicion of the foundations which was later translated into two Congressional Enquiries - the Cox Commission of 1952 and

the Reece Commission of 1953. The cloud which formed over the foundations, blown by the winds of a more general suspicion, resulted in accusations of communist infiltration and misfeance in allocating grants to communists or to communist sympathisers. In a recent conversation with an official at the Carnegie Corporation who had worked through that period, the atmosphere inside the foundation was characterised as "oppressive",<sup>1</sup> and John Marshall, former Head of the Rockefeller Humanities Division, described the situation as follows:

"... from that time on we were virtually required to consider the kind of suspicion that the committee had manifested. I say suspicion because that is all it was. I don't think we were ever afraid of criticism but one had to be aware that suspicion of that kind could be handicapping to us." (Marshall, 6:7:73)

Although John Marshall considered the atmosphere within the foundation to be more one of 'circumspection' than of fear, this atmosphere nevertheless had a direct and influential impact on granting policies. Hutchins, Head of the Fund for the Republic, wrote:

"Congressman Reece was scoffed at. It was agreed that his investigation was a farce. I think he had good reason to be satisfied with himself. Without firing a single serious shot, without saying a single intelligent word, he accomplished his purpose, which was to harass the foundations and to subdue such

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1 During this interview, mention was also made of the tremendous amount of work that both the Cox and Reece investigations involved. In addition, reference was also made to the necessity for considering the suspicions raised by those investigations resulting in the need for closer scrutiny of recipients' backgrounds.  
(Interview at Carnegie Corporation, New York, 7 June 1973.)

stirrings of courage, or even of imagination, as could be found in them. As I have said, there were not many there when he came on the scene. Congressman Cox had been there before him. If there was a foundation that was willing to be controversial, that was willing to take risks, it learned its lesson by the time Cox and Reece got through. Who will venture now?" (Hutchins 1956: 207)

Reading the Reece Report affords a glimpse into the working of a true conspiratorial mind, ranging, as it does, from attacks upon the Rockefeller, Carnegie and Russell Sage Foundations, for initially sponsoring the Encyclopaedia of the Social Sciences<sup>1</sup> to attacks upon the "dubious staff of the Ford Foundation" (Reece Report, 1954: 36) in the personage of Bernard Berelson. Nevertheless, despite the farfetchedness of some of the charges, Hutchins' comment "who will venture now?" is apposite given the reaction of the foundation world.

Although foundations in general in the early and mid-1950's were held under suspicion, the Ford Foundation was singled out for particular attention. According to MacDonald (1956: 25), the pillorying of the Ford Foundation first gathered momentum in 1951, when the Chicago Tribune ran a news story under the headline 'Leftist Slant Begins to Show in Ford'. This was an allusion to the participation in various Ford programmes of individuals such as Paul Hoffman who, as Head of the Marshall Plan, "had given away 10 billion dollars to foreign countries". This lead was taken up by the Hearst columnists such as Westbrook Pegler,

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1 Reece (1955: 31) objected to the fact that the articles dealing with subjects of a left wing nature were given to 'leftist' contributors and articles of a right wing nature were given to similar contributors.

George Sokolsky and Fulton Lewis Jr. Hoffman was described by Pegler as "a hoax without rival in the history of mankind" and the Marshall Plan was described as "the fabulous Roosevelt-Truman overseas squanderbund" (Reeves 1969: 15).

This mounting criticism of the foundations' activities, besides being extremely vocal, reached a wide and devoted audience. Time and again Fulton Lewis Jr. had called upon his audience to exert pressure on behalf of some personality or cause, the result of which was that his broadcasts had been responsible for starting over a dozen Congressional Enquiries (MacDonald 1956: 25). Lewis reached over 16 million people each weekday evening on radio, many watched him on one of 50 television stations, while millions read his syndicated columns in the Hearst press (Reeves 1969: 124). Attacks were also delivered from groups such as the right-wing Constitutional Educational League which produced pamphlets against the Ford Motor Company linking it with communism, the reasoning behind such tirades being that, through ownership of a Ford motor car, one unwittingly gave sustenance to communism, since the Ford Motor Company's profits were spent by the leftist Ford Foundation (MacDonald 1956: 27).

The singling out of the Ford Foundation was the result of two main factors. The first was the newly expanded programme of support for the social sciences, both in its specific application to social problems and in its more general scientific

development. In fact, Division Five of the Foundation, which as documented previously was responsible for the establishment of the Center for Advanced Studies in the Behavioural Sciences at Palo Alto, received particularly adverse comment in the Reece Report (Reece Report, 1955: 36). The second reason, and perhaps the key to Ford's unpopularity, was the granting of 15 million dollars for the establishment of the Fund for the Republic. This radical foundation, whose purpose was the defence of civil liberties, and which sponsored such works as Stouffer's communism studies (Stouffer, 1955) and Lazarsfeld's 'Academic Mind' (Lazarsfeld and Thielens, 1958) time and again stung the foundation's critics to fury over its practices. Although the Fund was independent, the Ford Foundation was obliged to assume parental responsibility for its wayward child. Not only were the Fund's practices controversial but, in addition, its birth in 1953 came at a particularly defensive time for the foundations. This unfortunate timing - although the Fund had in fact been conceived some eighteen months before - resulted from the decision to postpone the founding until after the Presidential Election of November 1952. The attacks upon the Ford Foundation left their mark, both at the personal level and at the level of granting policies.

MacDonald quotes Henry Ford as saying:

"The dealers send us in letters from customers accusing the foundation of being communist and warning that they'll never buy another Ford. But I don't bother much with that sort of mail. Why should I?" (MacDonald 1956: 27)

The above statement by Ford is strictly for the 'official history' of the foundation however, since the reality is somewhat different. He was worried, in fact he showed some of his mail to his speech writer and "expressed deep concern" (Reeves 1969: 15). Neither does Hoffman's view of Ford lend support to the above nonchalant statement.

"I told him [Ford] that I wanted to experiment ... to change things and that change always means trouble. But every time we got a dozen letters objecting to something we had done ... a radio show of an overseas program or what-not ... I'd have to spend hours reassuring the Board."  
(Reeves 1969: 15)

Henry Ford's behaviour under such pressure is also evidenced by his denunciation of the Fund for the Republic in 1955. This denunciation, circulated as it was in every major newspaper in the country, was an unexpected blow to the Fund (Reeves 1969: 177). Why had Ford done it? It is not attributable to immediate economic considerations since, although boycotts of Ford products had been threatened, the company records reveal that sales and profits were breaking records (Reeves 1969: 177). The reason would appear to be the amount and intensity of general criticism, plus criticism from dealers and customers. In an interview between Elmo Roper and Reeves, Reeves comments that:

"[Roper] knew that there had been heavy pressure from officials at the Ford Motor Company; Roper was given hints that his own business relations might be cut if he failed to resign from the Board, or to support the removal of the Fund's outspoken President. At one point, Ford himself had asked Roper to leave the Fund's Board." (Reeves 1969: 177)

If the pressure was too much for the Ford Foundation, it was also too much for the Fund for the Republic. Following a particularly blistering attack by Fulton Lewis Jr. after his discovery of the Fund's intention to study the American Legion, the Board Meeting of September 1955 was extremely worried. The outcome was a decision to drop certain controversial programmes (Reeves 1969: 138).

It must be remembered that the social class background of both foundation officers and trustees<sup>1</sup> had not prepared them for the unsophisticated and libellous nature of the attacks which were delivered. Nor were the Henry Fords of the world accustomed to give account of themselves before Congressional Enquiries, as Ford had to do in the autumn of 1952. Lyons (1969: 278) however, considers that these onslaughts "had no appreciable effect on foundation policies" and that whatever the effects were they were largely "attributable to the general anti-communist hysteria and anti-intellectualism of which they were a part". In support of this contention, he cites the fact that, in the early 1950's, the Ford Foundation increased support for the social sciences. Admittedly, the Ford Foundation did expand its programme in the

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1 Big foundations are a microcosm of what has been called 'the establishment', 'the power elite' or 'the American ruling class'. (Domhoff, 1967) Both Lindeman (1936) and Andrews (1956) show the trustees to be men of considerable affluence, having attended the 'best' universities and having been members of the 'best' clubs. Foundation officers closely resemble these trustees to a large extent, although not ranking so highly on affluence or age. The data provided by Lindeman and Andrews on their social background shows remarkable consistency over time, and Nielsen (1972) gives little evidence to suggest any major alterations in this picture.

social sciences, but that begs the question since it was this activity which was in part responsible for the attacks. The more interesting point is, what happened to granting policies once the attacks began in earnest?

At the Quarterly Meeting of March 1955, the Ford trustees approved three big programmes on which they expected to spend 85 million dollars, all, or most of it, within the next three years. This expenditure would have accounted for nearly half of the regular annual budget through until 1957 (MacDonald 1956: 166). In addition to the 20 million dollar National Merit Scholarship, there was a 50 million dollar programme to raise the salaries of college teachers, and a 15 million dollar fund for research into mental illness during the following five to ten years. Their attraction for the foundation was precisely that they offered the maximum possibility for spending money and the minimum danger of getting into controversial territory. MacDonald (1956: 170) further adds the biting comment that "even this program was not large enough for the well-Gaitherised<sup>1</sup> foundation and at the end of 1955 another was announced that was even safer and a great deal bigger". This refers to the grant of  $\frac{1}{2}$  billion dollars for privately supported institutions within America.

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1 President of the Ford Foundation.

"The trouble probably was more lack of daring than of sophistication; those philanthropists at Ford are reasonable intelligent men, and it is unlikely that it did not occur to them that a more productive use could have been made of half a billion dollars than just giving it out pro rata to everybody. They were scared or more accurately, Henry Ford, Donald Young and other trustees were scared, and their fear communicated itself, through channels to the philanthropists who ran the foundation for them." (MacDonald 1956: 170)

The above outline provides some indications of the unfavourable climate within which foundations had to operate during the 1950's. Even though by the 1960's this climate of distrust had largely evaporated in the buoyancy generated by the 'New Frontier' in Washington, and the emergence of many foundation officials in high places within the White House and State Departments,<sup>1</sup> the possibility that the foundation's own actions might stir up controversy remained as a reminder of their delicate position. The action of the Danforth Foundation in the sensitive area of race illustrates the price of 'risk capital philanthropy'. Following through its 1968 programme of aid to young persons from poor neighbourhoods, a young black militant, Percy Green, was awarded a scholarship in 1970. This resulted in pressure upon the foundation, even though the award had not been made by the foundation itself, but by a committee of prominent private citizens. In response to this pressure,

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1 This is a point made by Nielsen (1972: 386) but it should be mentioned that the relationship between high ranking foundation officials and government has not been altogether absent in the past - note the cases of John Foster Dulles and Dean Rusk of the Rockefeller Foundation.

several members of the foundation's Board wanted to rescind the scholarship. In the event, it was allowed to stand, but a price was exacted. In June 1970, in a "wide departure from its general pattern of grants", the foundation awarded a 15 million dollar support gift to Washington University, and another one of 1½ million dollars to St. Louis University for similar purposes. According to the "privately expressed view of the trustees", this grant was a way of placating the "foundation's critics in the St. Louis area for having given educational assistance to a black radical" (Nielsen 1972: 106).

Before turning to the case study, it is interesting to note that the recent 1969 Tax Reform Act, prompted to some extent by the support given by some foundations to the voter registration movement, has again undermined foundation confidence. Barbara Shenfield, in her summary of the debates of a recent Anglo-American conference on philanthropy, suggests that "the new legislation has encouraged a climate of suspicion towards foundations generally which in turn has caused many of them to be more cautious in their grant making than the restrictions in the legislation actually require". (Shenfield 1972: 11)

#### Social Pressure and Foundation Collapse: The Case of Television Research

One of the problems confronting the history of mass communication research is to explain the absence of any major sociological work on television in America during the 1950's. Indeed, the first

major work on television originated not in America but England with Himmelweit's (Himmelweit et al., 1958) study of 'Television and the Child'. Although it is true that scholars such as the Maccobys and the Rileys were engaged in research in that area, it was not until 1961 with the publication of Schramm's (Schramm et al., 1961) book 'Television in the Lives of our Children' that a major American work was produced.<sup>1</sup> Commenting on this situation Charles Wright in his UNESCO report on American mass communications research for the decade 1945-55, writes:

"In reviewing this recent history, one is struck by the absence of material on what is perhaps the most salient development on the mass media in the United States during this period; that is, Television. There has been no major sociological study of the new medium to date."  
(Wright 1956: 83)

Whilst not wishing to become enmeshed in the Berelson controversy over the 'withering away of mass communication research' (Berelson, 1959), the absence of foundation support for television research presents a problem requiring explanation, particularly as it runs counter to what may have been expected, given the foundations' past record in the area of mass communication research. For example, in the 1920's, when motion pictures were a cause of concern, the Payne Fund supported Charters'

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1 It is worth noting that Schramm in the preface to his book gently complains about the lack of money for his research and contrasts his situation with that of Himmelweit when he notes that "who knew in advance the amount of their support, and could therefore plan the entire study before they gathered any data".

studies (1933); then, in the late 1930's, with the spread of radio, the Rockefeller Foundation provided support for a study of its impact upon American society. Yet, the situation arose in the 1950's, when a new medium of enormous potential influence was developing, and no foundation came forward to offer support for an examination of its impact.

The Rockefeller Foundation, following the familiar path of support and withdrawal, had already left the field of mass communication research, and could therefore not really have been expected to re-engage itself in such work. Further, although the Rockefeller Foundation had given generous support to the social sciences, especially through the activities of the Laura Spelman Memorial, its influence was steadily being eclipsed by the entry of the Ford Foundation. The question then arises: why did the Ford Foundation never see fit to lend its support to television research? Of course, one possible explanation would be that such research never held any intellectual interest for the foundation, but such an explanation would be mistaken since the evidence indicates that they were very interested indeed.

During the winter of 1951-52 the Ford Foundation began to discuss seriously the feasibility of studying the impact of television on American society. Several preliminary seminars and conferences were held to explore the possibilities of such work, and leading figures in the academic world and in

the media were consulted. The outcome of this preliminary work was the establishment of an Advisory Committee on Television. The Committee met for the first time in New York on August 21 and 22, 1952, with Paul Lazarsfeld as its Chairman. The Committee's brief was to develop proposals for something akin to a commission to study television and to make policy suggestions. The Report was finally submitted in the summer of 1953 with expectations that the proposals would be acted upon, as the Committee's members had been led to believe they would. In the event, however, nothing ever became of the Committee's recommendations.

A point of entry into the reasons surrounding the still-birth of the commission is provided by a reading of the Proceedings of the Kefauver Committee on Television and Juvenile Delinquency, Spring 1955. In discussing the lack of funds for a study of television, Paul Lazarsfeld who had been called as an expert witness stated:

"Unfortunately, the chances for such a turn of affairs [foundation support] are limited at this moment, because of the kind of criticism which has been levelled against foundations in recent years. A Congressional Committee [Reece] has criticised the foundation boards for certain action in other areas. The boards are frequently cautious in making funds available for new areas of study. When radio appeared on the scene, the Rockefeller Foundation was still quite willing to finance large studies on the effects the new medium might have on American life. Now that television is here, with presumably more intensive effect, no foundation has yet seen fit to sponsor the necessary research." (Kefauver, 1955: 54)

In the Report of the evidence given to the Committee, part of Lazarsfeld's original testimony was omitted. The missing part<sup>1</sup> reads:

"Just as our Committee [refers to the Ford Advisory Committee on T.V.] submitted a detailed plan endorsed by industry as well as by critical reform groups, the attacks on foundations began and the sponsoring organisation [Ford] decided to drop the whole matter."

The reason for this omission is that a high-ranking official at the Ford Foundation wrote to Kefauver objecting to Lazarsfeld's interpretation of the reasons for 'dropping' the proposed project. Understandably, the suggestions put forward by Lazarsfeld did not make pleasant reading for the official and resulted in correspondence between them to 'clear the matter up'. Far from clearing the matter up however, the correspondence provides further evidence of the uncertainty surrounding the whole situation. For example, the letters indicate that the foundation considered that it should have been obvious at the time why the project was never funded. If it was obvious to the foundation, then it was certainly not obvious to Lazarsfeld, for he wrote to Vice-President McPeak:

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1 The missing part of Lazarsfeld's testimony was discovered in the Ford Foundation files (PA 53-16).

"I am, of course, very eager to avoid any misunderstanding as to what happened with the Citizens' Committee on T.V. Your letter came too late for changes to be made in the Congressional record. I am sure you realise where the difficulty lies. I have never had any communication from the foundation as to what disposition was made of our proposal. Whenever I was in New York some member of the Citizens' Group was likely to ask me about it. The most reasonable interpretation was that the matter had got forgotten in the turnout created by the Reece Committee. This formulation saved me from embarrassment with my colleagues who had worked so hard on this assignment and I thought it also saved the foundation from the reproach of being discourteous to this distinguished group of men. Let me assure you that I will be equally cooperative in adjusting to your way of looking at the matter once you have explained it to me." (Lazarsfeld 15:6:55)

Considering the amount of work that Lazarsfeld had expended on behalf of the foundation, the above is a remarkably restrained and polite letter, but it is shortly followed by another and somewhat sterner letter asking for clarification. He wrote:

"... at the time I rendered my report every member of the group got a thank-you letter and was told that in due time we would learn of the disposition the foundation made of our recommendations. Since then, neither I nor any other member of the group as far as I know has heard from you. We of course took it for granted that the foundation had decided to drop the matter but because we were left without any information most of us developed the theory that the Reece episode accounted for all of it ... including the silence. Therefore, there are two things I will want to clear up with you. One is laying the ghost of the old proposal which I think can be done in a few lines from you to me." (Lazarsfeld 27:9:55)

This request by Lazarsfeld for an explanation as to why the proposal was refused funds received a reply, but no explanation.

It was not even a case of receiving 'no adequate explanation'; there was simply no explanation at all, except to inform Lazarsfeld of what he already knew - that the project had been refused funds. The following conversational extracts give some insight into Lazarsfeld's understandable confusion over the whole situation:

D. Morrison: In your evidence to the Kefauver Committee on Juvenile Delinquency you mention that the foundations were reluctant to support T.V. research because they were feeling the pressure of the times.

P. Lazarsfeld: Oh yes, the foundations - there was a congressional investigation of all the foundations.

DM : Well, there was Cox and Reece.

PL : Yes that's right, they were badly affected - not financially - there was nothing you could do - the atmosphere was very unpleasant. This Reece was really just like McCarthy.

DM : I wanted to ask you about some information I discovered on the T.V. Commission while I was going through the Ford Foundation files.

- PL : Oh yes, that I wanted to make you aware of - that is something we never touched on before because it never had the slightest consequence.
- IM : Yes, that's exactly what I wanted to ask you about. I've got my own interpretations.
- PL : So have I. Well, I have a definite theory about it, but I have no evidence really. There was one man on my committee, or whatever you want to call it, who was very famous for many reasons but also completely erratic - that was Rum1. I think Rum1 took a very definite dislike to me. I always had a theory that Rum1 torpedoed the whole project and torpedoed it in the following way. There was a Vice-President who has since died - McPeak - who in my opinion was a very typical foundation bureaucrat, not in the slightest a John Marshall. And you know the reason. This committee just disappeared and my theory was always that Rum1, who had a great influence on McPeak - and McPeak had no reason to like me either [sic]. I had hardly any contact with him. But between these two men it was killed. It's a very funny episode. One day, Hutchins asks me to take it over. I think he had asked Stanton's advice, and he suggested me. You know there were eight or

ten monographs written around it. Seipman and others and I worked. We worked endlessly and this was a very distinguished group. After all, these were top people on this board. I was very proud. One day, out of the blue, it was all ended. (Lazarsfeld 15:6:73)

From the position that Lazarsfeld occupied in the affair, the situation may well have appeared that way - and his supposition may well be correct with regard to the actual mechanics of the ending of the project. But the concern in the present context is more with the situation within which the mechanics operated - how it came to be that the project could be jettisoned.

Lazarsfeld and his colleagues had good reason to consider that the 'rug was being pulled out from underneath the project' since, in the trade paper 'Variety' (May 9, 1952) the front page headline read 'Ford Foundation Maps 1,000,000 Dollars Survey To Cure The Woes Of TV'. No authorization had been given for such a release; in fact, so far as can be gathered, it remained a mystery just who had leaked the information. The release caused consternation among those who were committed to the project since, in the early days of negotiations, such publicity could be detrimental. However, too much must not be read into this incident, except to note that it could have been responsible for the expressed feelings that the project was being sabotaged. There is no evidence that the

project was actually 'torpedoed', and it is reasonable to suppose that, because of the prestige of the groups involved, plus the size of the enquiry, and the foundation's obvious commitment to it, any machinations powerful enough to sink the project would somewhere become visible. That is not to discount behind-the-scenes manoeuvring, and it may well be that, in the final demise of the project, personal antagonisms did come into play - they were not, however, responsible for it. There is other evidence available to account for its failure.

Hutchins was a moving force behind the whole project, but at the same time Henry Ford opposed it on the grounds that it was too controversial. From the start, Henry Ford had insisted on the inclusion of the media industries in the project as a protection against criticism, despite early resolutions to exclude them on the grounds of 'vested interests'. It is true that in the early days of the Committee, certain sectors of the media industry were hostile, but they were placated by Lazarsfeld's diplomacy and became supporters. As Lazarsfeld informed the President of the foundation:

"I am very eager indeed to talk with you about the progress of the preparatory committee on the T.V. Commission. We have made good progress intellectually as well as in our efforts to secure the support of the industry." (Lazarsfeld 15:6:53)

Indeed, in conversation with the writer Lazarsfeld underscored this point:

IM : I know that Ford insisted that the project should only go ahead if there were representation from the media industry involved, and, if my reading of the foundation's files is correct, the media people seemed quite responsive to the idea.

PL : Of course. Look, I had all sorts - I remember big station managers from the mid-West, and I was fairly skilled to reconcile them. The list of station managers is very impressive - the station representatives liked the idea. At least, they never created any trouble. It was one of the strangest episodes. (Lazarsfeld 15:6:73)

Unfortunately for the future of the project the support of the media personnel came too late. Ford's uneasiness probably stemmed from the fact that he had phoned Frank Stanton of CBS in the early days of setting up the committee to ask him to take part in it. Stanton agreed, and was then criticised by the industry. This got back to Ford, and made him unsure about the wisdom of such work, given the atmosphere of the times. Further evidence of the existence of 'nervousness' in relation to the project is the insistence that an outside group approach the foundation for support for the project when, in fact, the proposal had originated within the confines of

the foundation itself. A certain amount of subterfuge followed to cover this deception.

The death knell of the proposed commission was sounded by two events - the first was the departure of Hutchins from the foundation to help establish the Fund for the Republic; the second was the establishment by Reece of his Congressional Enquiry into foundations. Both of these events occurred in 1953, the year that Lazarsfeld submitted the proposal for the Television Commission. Hence, not only was the already tense atmosphere<sup>1</sup> within the foundation further reinforced by the proposed Reece Enquiry but, with Hutchins' departure, the project's principal supporter was removed. Once this lock-gate was opened, the waters of caution flooded in. However, the proposed Television Commission was submerged and not actually drowned, a fact that, to some extent, accounts for the absence of communication to Lazarsfeld regarding the foundation's disposition towards his submitted report. They could not decide what to do with it. It floated for some time in uncertainty, and even looked as if it might surface, but finally, excuses were given as to why it should not be supported. These excuses look very much like post hoc reasoning for dropping the proposal. They included the lack of financial support from the media industry and from citizens' groups, and the fact that the original release of money for Lazarsfeld's Advisory Committee had made no mention of guaranteeing support for any proposals made. Both of these

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1 This point was brought out in an interview with a high ranking official of the Ford Foundation who was active in the foundation during that period. (Interview in New York on 12 July, 1973)

can be discounted as serious reasons for ignoring the proposal, since alternative financial support had never been part of the committee's brief in the first place, and it is doubtful that the Ford Foundation actually needed the financial contributions that a citizens' group could make - unless, once more, it wished to spread responsibility for the commission. In any case, such reasons were never communicated to Lazarsfeld as excuses for the refusal to support the commission. With regard to the fact that there was never any promise to support suggestions that the committee produced, this is not true, in the sense that Lazarsfeld and his colleagues were never in any doubt that their report was going to be accepted. Although it is true that the official release of the original money for the Advisory Committee contained no explicit promise of support for the proposals produced, there was nonetheless a definite tacit understanding. Lazarsfeld is adamant on this point,<sup>1</sup> and other evidence leaves no room for doubt. Such an understanding was not a mis-reading of the situation by Lazarsfeld. Indeed, as Lazarsfeld repeatedly states, "it was a peculiar episode", reflective in many ways of the peculiarity of the times and the uncertainty existing within the foundations. But it would be a mistake to consider it as an isolated incident. Indeed, in order to understand the meaning of this particular episode, it is necessary to situate it

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1 This point was rechecked in particular during a recent conversation with Lazarsfeld - 28 March 1974 in Cambridge, England.

within the kind of analysis of the foundations' social position and their development over time outlined in this chapter.

#### Wider Implications for the Production of Knowledge

In looking at the foundations' role in the production of knowledge, one can adopt varying points of entry, depending upon the level under consideration. For example, one can see them as "brokers of ideas" (Coser, 1970: 337) or as "standards of excellence" to which scholars can appeal for definitions of their work (Berelson, 1960). One can even widen the angle of vision to include foundations within the productive process itself, rather than as part of the apparatus of support. Indeed, it is difficult to draw the line between the 'creator of knowledge' and the 'support structure' necessary for this production, for both are part of the same process of knowledge production. It would be false to treat the academic world as the active partner and the foundations as somehow passive, merely giving over funds to 'worthwhile' studies, for both interrelate, shaping and informing the other. It is not sufficient to consider the foundation as 'out there', to be finally considered when all other aspects of the situation have been attended to and worked out. The foundations exist as part of the social world that the scholar must operate in and on. Their existence is an aspect of reality that must be confronted, an aspect that has its own qualities which need to be taken into account.

Given the rapid expansion of empirical work in American social science, and its institutionalisation in research bureaux, research centres and institutes, the role of the foundations is of paramount importance. As mentioned previously, many of these 'institutes', although linked to universities in a variety of ways, have little sound financial basis and have therefore been obliged to seek external sources of funds to support what is basically expensive work. The relationships between such research institutes and foundations are therefore firmly cemented in the histories of both, and although one is not suggesting that foundations have been responsible for empiricism in any genitive sense, they have greatly facilitated its adoption and spread through their support of the institutional forms necessary for its advanced development. The relationship is a reciprocal one, however, for just as the organisation of empirical research required the financial support of the foundations, so the foundations needed such organised research to help dispense their funds. The allocation of a small grant costs relatively more than the disbursement of a large grant (cf. Whyte, 1955b: 216). Furthermore, handing over large funds to research organisations relieves the foundation of an administrative headache. For instance, by allocating large funds to research institutes or departments attached to universities, they know that their money is 'safe' and consequently, constant scrutiny for the possibility of misuse of funds is unnecessary. They can rest

assured that, within reason, the money will be used for the purposes stated in the grant application. Its proper use is underwritten by the organisational structure within which the research is conducted. The money has been given from one bureaucracy to another with all the empathy of confidence that this implies. But, if the relationship is symbiotic it may, as argued above, have possible disadvantages for the production of knowledge. The interlocking nature of the relationship means that attacks upon the foundations leave the production of knowledge in a balance over which the producers have little control.

In light of this some possible solutions can be suggested for strengthening the position of knowledge production. As far as foundations are concerned, the main problem is finding ways to improve their position so as to lessen their vulnerability to social pressure. As argued above, one of the principal sources of the foundations' vulnerability lies in their lack of legitimation, and whereas one would normally expect an institution to legitimate itself by its actions or some other practices, such a path is denied the foundations by their continued operation within the controversial fields of social science. It must also be remembered that foundations are not in any sense created by the state, or meaningfully ratified, but rather acquiesced to, and therefore cannot draw their legitimation from a higher

authority.<sup>1</sup> Hence, they are consequently obliged to justify themselves by appeals to their own actions.

If fully-fledged legitimation is not possible so long as a foundation operates within the social sciences, it may nevertheless be possible to develop support structures around them. This could be achieved on one level by the foundations themselves giving greater support and energy to the development of the Foundation Council. Secondly, the universities could offer more support when foundations find themselves under pressure. For, when called upon in the recent controversy surrounding the 1969 Tax Reform Act, not all universities lent their support. Of course, it may be that the academic world does not wish to see the continuation of such institutions, but if that is so, and this applies to other agencies as well, then alternative forms of research finance must be arranged.

Apart from improving the position of foundations therefore, one is still left with the question of how to strengthen the financial position of research centres themselves, and so reduce their dependence upon external funding agencies. One solution lies in integrating them more fully into the university administrative structure, to the extent of the parent university accepting responsibility for a substantial part of their operating costs. In other words, one returns to the model of research centres which Lazarsfeld fought for during the Cheatham epoch.

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1 Whitaker (1974: 235-242) in his very recent book on foundations questions the future of foundations after the inroads made by the 1969 Tax Reform Act.

One is not suggesting that the universities accept total responsibility for funding since, as previously stressed, the searching for contracts is not a totally negative intellectual exercise. As well as broadening the base of academic concern, much contract work fits easily with academic advancement and interests. Further, such funds can alleviate the competition for scarce resources within the university. Nevertheless, it is desirable to free research centres from the need to generate their own funds. Not only would they then be relieved of the pressures which have resulted in many of their less attractive practices, but more importantly, they would be able to enquire into socially sensitive areas where external funding agents were reluctant to venture. The universities' provision of a basic but substantial sum for research would mean that key research decisions would rest not upon the vagaries of patronage, but upon the intellectual and political commitments of those directly engaged in the production process. This, in turn, would offer the possibility of greater intellectual freedom.

**APPENDIX A**

**Formal List of Individuals and Places in America  
visited by Lazarsfeld while a Rockefeller Fellow\***

10.6.33 - 10.25.33	Columbia University (Department of Psychology) with Professors Goodwin Watson, Lynd and Gardner Murphy.
10.26.33 - 11.17.33	Harvard University (Department of Psychology) with Professor Gordon Allport. Also contacts in Business School.
11.17.33	Dr. Koffma of Smith College.
11.23.33 - 12.16.33	Columbia University.
12.16.33 - 2.12.34	Washington, D.C. - Federal Bureau. Durham, N.C. - Duke University (McDougall). Lexington, Ky. - University of Kentucky (Department of Psychology) with H. Beaumont.
2.13.34 - 5.18.34	New York City - contacts with marketing organizations; Mr. Robert Lynd, Department of Sociology, Columbia University.
5.18.34 - 5.22.34	Washington, D.C.
5.23.34 - 5.27.34	Pittsburgh - David R. Craig, Director, Research Bureau for Retail Training, University of Pitts- burgh.
5.28.34 - 5.31.34	Columbus, Ohio - H.H. Maynard, Department of Business Organisation, Ohio State University.
6.1.34 - 6.17.34	New York City.
6.18.34 - 7.18.34	Pittsburgh - David R. Craig, Director, Research Bureau for Retail Training.
7.19.34 - 7.26.34	New York City - Columbia University at Psychological Corporation and with Dr. J.B. Watson in Business Psychology.
7.27.34 - 8.17.34	Ithaca, N.Y. - Lewin, Spencer, J.G. Jenkins, etc., Department of Psychology, Cornell University.
9.4.34 - 9.20.34	N.Y.
9.20.34 - 10.3.34	Schenectady, N.Y. - with Professor J.G. Jenkins at General Electric Company.
10.3.34 - 10.6.34	Rochester, N.Y. - University of Rochester, Department of Sociology.

\* Taken from Lazarsfeld's Rockefeller Fellowship Docket.

10.6.34 - 10.23.34	N.Y.
10.24.34 - 10.25.34	Rochester, N.Y. - University of Rochester, Department of Sociology.
10.25.34 -	N.Y.
- 12.18.34	Rochester, N.Y. - University of Rochester.
12.19.34 - 1.21.35	N.Y.
1.23.35 - 2.4.35	Chicago - Professor W. Ogburn, Department of Sociology, University of Chicago; Dean Donald Glesinger, Division of Social Science, University of Chicago.
2.4.35 - 2.10.35	Pittsburgh.
2.12.35 - 3.14.35	Washington - FERA (Federal Emergency Relief Administration) and Rochester, N.Y.
2.27.35	New York City.
3.15.35	Visited New Haven, Conn.
3.29.35 - 4.30.35	Chicago (side trips to Detroit, Ann Arbor and Madison),
5.1.35 - 6.24.35	Iowa City. Minneapolis. Rochester, N.Y. New York City.
6.25.35 - 6.30.35	Boston.
7.1.35 - 7.2.35	Pittsburgh.
7.3.35 - 7.5.35	Washington, D.C. New York City.
7.9.35	Sailed on SS. Rex.

**APPENDIX B      Sources of BASR Research Funds: 1944-1960**

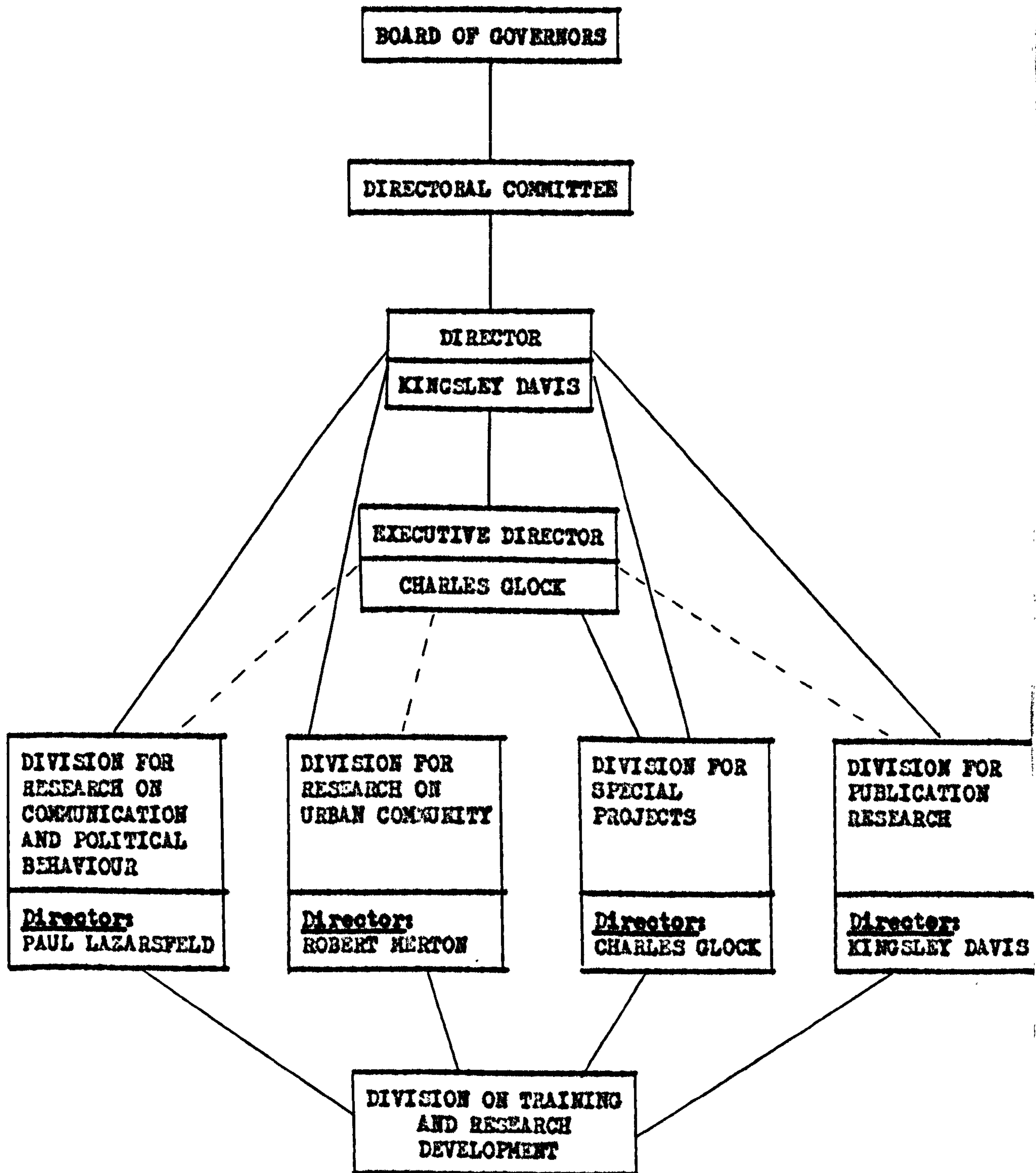
<b><u>Fiscal Year</u></b>	<b><u>Total Income</u></b>	<b><u>Government Contracts</u></b>	<b><u>Grants from Learned Societies</u></b>	<b><u>Grants and Commissions from Business and Industrial Organisations</u></b>	<b><u>Grants and Commissions from Private Non-Profit Organisation</u></b>
1944-45	118,000	-	15%	68%	17%
1945-46	99,000	-	35%	56%	9%
1946-47	90,000	-	58%	34%	8%
1947-48	136,000	-	39%	35%	26%
1948-49	182,000	5%	39%	49%	7%
1949-50	184,000	36%	40%	15%	9%
1950-51	380,000	75%	12%	6%	7%
1951-52	523,000	83%	7%	3%	7%
1952-53	588,000	62%	13%	14%	11%
1953-54	508,000	55%	28%	6%	11%
1954-55	410,000	40%	35%	16%	9%
1955-56	360,000	18%	51%	24%	7%
1956-57	431,000	24%	42%	15%	19%
1957-58	512,000	19%	37%	23%	21%
1958-59	455,000	23%	37%	11%	29%
1959-60 (est.)	589,000	34%	33%	13%	20%

**Source:** UNESCO Study of the Administrative Structure and Working Methods of Selected Social Science Research Institutes.

Made available to the writer by Henning Friis - Research Organiser of the UNESCO Study.

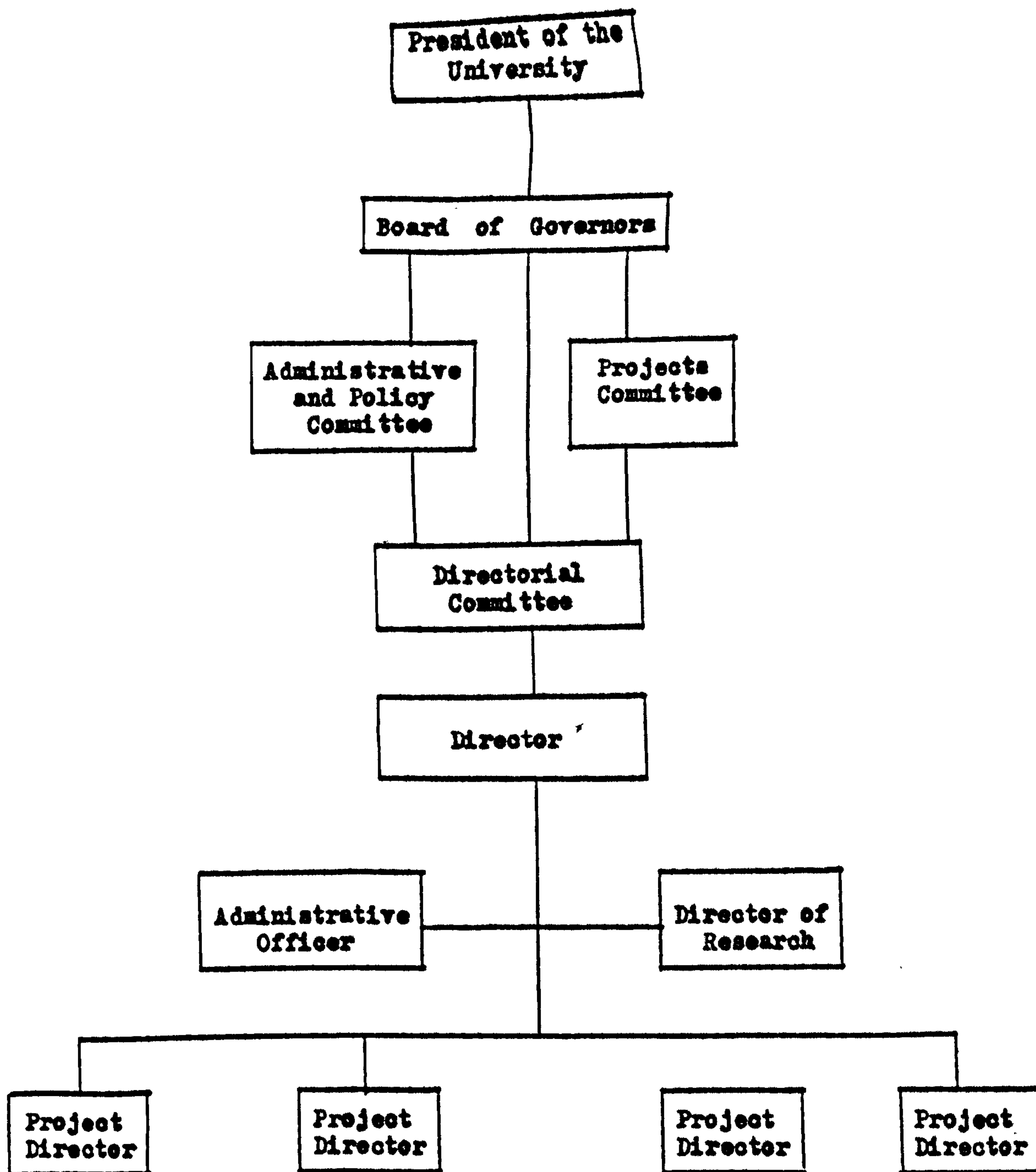
APPENDIX C

Organisational Structure of Bureau of Applied Social Research (BASR) 1951



APPENDIX D

Organisational Structure of the Bureau of Applied Social Research (BASR): 1960



**Source:** UNESCO Study of the Administrative Structure and Working Methods of Selected Social Science Research Institutes.  
Made available to the writer by Henning Friis - Research Organiser of the UNESCO Study.

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**MANLY REPORT****1916**

Commission on Industrial Relations  
Senate Document 415, 64th Congress, 1st  
Session, Congressional Document Serial  
No. 6929.

**MARSHALL, J.****6.11.1936**

Documented interview with F. Stanton  
(Rockefeller Archives R.R.P.).

**May 1937**

Summary of conversation with F. Stanton  
(Rockefeller Archives R.R.P.).

**21.5.1937**

Considerations presented to the Board of  
Trustees in support of Radio Research  
Project Proposal (Rockefeller Archives  
R.R.P.).

**19.8.1937**

Documented interview with F. Stanton and  
P.F. Lazarsfeld (Rockefeller Archives R.R.P.).

**2.3.1938**

Documented interview with H. Cantril and  
F. Stanton (Rockefeller Archives R.R.P.).

**18.1.1939**

Documented interview with Dodds and  
H. Cantril (Rockefeller Archives R.R.P.).

**2.3.1939**

Documented interview with H. Cantril  
(Rockefeller Archives R.R.P.).

**3.3.1939**

Documented interview with P.F. Lazarsfeld  
(Rockefeller Archives R.R.P.).

**16.3.1939**

Telegram to P.F. Lazarsfeld (Lazarsfeld Files)

**16.3.1939**

Letter to D. Foote (Rockefeller Archives  
R.R.P.).

**21.3.1939**

Inter-Office Document (Rockefeller Archives  
R.R.P.).

**22.6.1939**

Letter to H. Odum (Rockefeller Archives  
R.R.P.).

**16.8.1939**

Letter to Professor Richards (Rockefeller  
Archives R.R.P.).

**11.12.1939**

Inter-Office Memo of interview with P.F.  
Lazarsfeld (Rockefeller Archives R.R.P.).

**13.12.1939**

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