INVESTIGATIONS OF PERSONAL TEMPO

by G. L. Davies

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CHAPTER 1:

Orientation

According to Rimoldi (1951), "Among the variables we consider when judging or characterizing an individual, his natural speed occupies a foremost position in our picture" (p. 283). This suggestion is debateable, but it is true that casual observation suggests that there are reliable individual differences in the speeds which individuals spontaneously adopt when performing their everyday activities. We might, for example, be acquainted with someone who appears habitually to speak rather slowly, or we may know someone who walks comparatively quickly, even when he is not under any obvious pressure to do so (as he would be if he were hurrying to catch a train or to keep an appointment).

Casual observations of this kind have prompted numerous psychological investigations of "everyday speed", and these will be discussed in the following chapter. One question to which an answer has been sought in such research concerns <u>reliability</u>: if a man walks quickly on one occasion when no temporal pressure exists (so that he is "free to choose his own pace"), will he tend to do so on later such occasions? It will be seen that, in the majority of cases, and with a wide variety of activities, previous researchers have been able to answer this question in the affirmative.

Another question has been concerned with the <u>generality</u> of "everyday speed". Given that a man is a relatively quick walker, is he also likely to be a relatively quick speaker? In short, is there a unitary "personal tempo", such that high correlations exist between the speeds of performance of a wide variety of everyday activities? It will be seen that, in general, previous findings have suggested that the answer to this question is negative.

Investigations of "everyday speed" in "real-life" situations present methodological difficulties. How is the researcher to ensure that the subject will actually perform the activity which he wishes to investigate? Because of such methodological difficulties, the bulk of the previous research has been confined to the laboratory. Though the details of the experimental arrangements used by different authors vary somewhat, an attempt has usually been made to contrive a situation in which the subject may spontaneously adopt a rate of performance which he finds suitable. In doing so, some investigators have been at pains to make no mention of "speed" in instructions (e.g. Rimoldi, 1951). Others have emphasised that tasks should be performed at a rate which is "convenient or natural" (Harrison, 1941), or have used similar phraseology to indicate that the subject was not required to perform at his maximum rate.

In discussing the diversity of instructions in the literature, Rimoldi (1951) expresses the conviction that "... if we are searching for a study of the individual as he behaves in everyday life, spontaneous speed should be used" (p. 283). "Spontaneous speed" is here contrasted with "maximum speed". The body of research in which subjects have been asked to perform a task, or to make a response, "as quickly as possible" (or where some equivalent phrase has been used) is very voluminous. The reaction-time literature alone is vast. However, the extent to which this type of research is relevant to "everyday speed" is uncertain. If, for example, a person habitually walks comparatively quickly, is he likely to be one of the leaders in a walking-race? That is to say, what is the extent of the correlation between the rate of performance of a task when the subject is left to "choose his own pace" and the rate of performance when he is motivated by some means to perform "as quickly as possible"? This is an empirical question, which should prove amenable to experimental investigation but, in the absence of empirical data, it would appear most prudent to follow Rimoldi's policy in assuming that findings obtained with "maximum rate" instructions are not necessarily relevant to our purpose, though in some cases they clearly will be so. (Harrison, 1941, for example, performed an experiment in which the correlations between "spontaneous rate" and "maximum rate" performance of a number of tasks were investigated, and this will be considered in the following chapter). No attempt will be made, therefore, to review the literature on maximum speed, though specific findings will be cited when they are clearly relevant to the discussion.

If the "maximum rate" literature is not necessarily relevant to the discussion, which literature is relevant? In fact, no hard-and-fast rules can be used in making this decision because, as has been said, different researchers have used techniques which are rather diverse in their details. Nevertheless, there is a body of research with a "family resemblance" and which appears prima facie to be relevant to the question of "everyday speed". In this research, it is found that broadly similar techniques have been used, that similar tasks have been investigated, and that the same references are cited (though they are not often cited by investigators working outside of the field). Most importantly, an attempt has been made to allow the subject to choose his own rate of work, and suggestions of the required tempo of performance have been avoided. It has already been noted that different approaches to the writing of instructions have been adopted, some mentioning "speed" and others not. Moreover, within these two broad categories, numerous variations in the precise wording of instructions exist. Whether such variations in procedure lead to comparable results is, of course, another empirical question, and it is one which should be borne in mind throughout this work. Are the various experiments members of the same family, or are they drawn from different families which happen by coincidence to look alike?

The result of applying the above decision-procedure is a relatively small body of research which is clearly relevant to the question of "everyday speed". However, it will be found necessary to discuss this material in rather more detail than is perhaps customary in works of this kind. There are several reasons for this:

(i). The research is not well-known, and the findings are rarely cited by investigators not directly involved in the field. Moreover, a comprehensive review appears not to be otherwise available. A detailed consideration would, therefore, appear to be particularly appropriate.

(ii). Regrettably, many of the discussions provided by the various authors of their own results tend to be somewhat superficial. The opportunity will often be taken of providing fuller discussion, therefore. Moreover, a frustrating aspect of the previous literature is that researchers have not been forthcoming in stating their hypotheses. We find Rimoldi (1951), for example, stating that certain tasks were included in his investigation "... to test previous hypotheses in the field" (p. 284), but he unfortunately omits to mention <u>which</u> hypotheses. It will sometimes be found useful, therefore, to formulate possible hypotheses when considering the findings of previous workers.

(iii). At many points in the literature, the reader may encounter an investigation in which he is informed that some such task as "counting" was used. However, a closer scrutiny of the literature reveals that, under the generic title, "counting", is classified a number of different procedures, which may or may not lead to comparable results. It would be folly, therefore, to attempt a less detailed review in the style of "... Rimoldi, 1951, found a correlation of x between counting and ...", since this does not provide sufficient information about exactly what task the subjects were performing. Nor is this consideration confined to "counting", of course. It will, therefore, be necessary to provide somewhat fuller descriptions of the tasks if a meaningful discussion of the research is to be possible.

With these points in mind, attention may now be turned towards a consideration of previous psychological investigations into the topic of spontaneous or "everyday" speed.

CHAPTER 2:

Review of the Literature

1. <u>The Reliability and Generality of "Spontaneous Speed"</u>

Tempo of performance was only one of the variables considered by Allport & Vernon (1933) in their extensive investigation, in which 25 male subjects performed a variety of tasks. Subjects attended three experimental sessions, approximately one month apart, and most of the tasks were performed more than once, so that a measure of test-retest reliability could be obtained. Instructions varied somewhat with the different tasks, and they will be noted where appropriate when describing the activities performed.

Where appropriate, Allport & Vernon combined the scores on different tasks to produce composite measures. The following composite measures are included in the matrix of intercorrelations which the authors provide:

(i) Reading. Two different passages of material were read aloud by subjects "in their most natural manner". In one of the sessions, the material was a short story, whilst in the other session, a passage from an introductory text in Psychology was employed.

(ii) Counting. Subjects were asked to count aloud "in their normal way". In the first session, they counted from 1 to 30, and in the third session they counted from 31 to 60.

(iii) Handwriting. Several tasks are included under this heading, including copying a prose passage, writing sentences and signature, writing e's and writing sentences on a piece of paper which was fixed to a blackboard. Allport & Vernon were particularly interested in laying the foundation of a scientific graphology, and this may account for the number of writing-tasks which they included in their battery. For the matrix of intercorrelations between the rates of performance of these tasks, the reader is referred to the original work (p. 89).

(iv) Blackboard writing and Drawing. The writing task required subjects to write the numbers from 1 to 20 (in word form). The authors point out that the area covered by the writing was comparatively large, so that this task involved gross body-movements such as bending and walking, as well as movements of the arm and hand. In the drawing-task, subjects drew three squares, with diagonals, with the right hand.

(v) Drawing Figures, Lines on Paper. The tasks included in this group involved drawing circles, squares and parallel lines with the preferred hand, and in any manner the subject wished. The authors point out here that it was necessary to emphasise to some of the subjects that they were not being tested for speed or accuracy in these tasks.

(vi) Foot-drawing The subject, in a standing position, drew one square (with diagonals) in a sand-box. A screw was strapped to the subject's foot for this purpose. One trial was given with each foot, in different sessions.

(vii) Finger-and Hand-Tapping. The instructions in these tasks (and in stylus-compression and leg-tapping, which are described below) requested the subject to "...tap in a normal way, at his most convenient speed, as if he had to continue for a long time ...". In the finger-tapping task, the forefinger was used, both right and left hands being tested in the same session, and the right hand being retested in a later session. For the hand-tapping, subjects were provided with a sprung stylus, which they held as though for writing. They then tapped with the point of the stylus on to a surface. The right hand only was used in this task.

(viii) Leg-Tapping. The subject adopted a standing position, supporting himself against a table. The whole leg was then repeatedly raised from the floor and lowered. Both legs were tested in the same session.

(ix) Stylus-compression. The subject was asked to hold the above-mentioned stylus between his fingers and to compress and release it "... at a regular and confortable speed". Both hands were tested in one session, and the right hand was retested in a later session.

(x) Walking. Upon completion of both sessions I and II, the subject was timed as he left the laboratory until he reached a point 50 yards away. In addition, the number of steps taken in this distance was recorded. The subjects were aware that they were under observation, but they were asked to ignore the experimenters and to proceed as though unobserved. In the final experimental session, subjects were also asked to walk indoors, but "... as though out of doors".

(xi) Strolling. Subjects were again asked to walk indoors, but "... as if they were walking up and down in their own rooms, meditating". This task was performed in two different locations, and in different sessions.

(xii) Estimating Distances with the Hands. In this task, S sat in front of a surface upon which were inscribed 20 parallel numbered lines. Line 1 was nearest to the subject, and line 20 furthest away. At the beginning of the first trial, S placed his hand on line 1. E then said a number. The subject closed his eyes, and moved his hand to the line of that number, then returned to the home position. He was then permitted to open his eyes. This procedure was continued until a block of 5 such trials had been performed. Following this, a block of five similar trials was performed, but with the subject's hand starting on line 20. The whole procedure was then repeated at a later point in the same session. This 4-block procedure was performed with each hand.

The time recorded in this task was the total for a block of 5 trials. The authors state, however, that the experimenter attempted to keep "... the interval between instructions as constant as possible". Despite this statement, they also state that the length of time occupied by a block of trials reflected individual differences in the speeds of hand-movements in making the estimates.

(xiii) Arranging Cubes. In this task, subjects arranged the weighted cubes from the Binet Test in the appropriate order.

The intercorrelation matrix obtained by these authors is reproduced in Table 2.1. A brief examination of this table is sufficient to reveal that Allport & Vernon's data do not support the view that there is a unitary "personal tempo", since many of the correlations are not statistically significant. This is the conclusion which the authors themselves draw. However, they also point out that their data do not support the view that tempo is specific to each activity and to each occasion. Several clusters of significantly intercorrelated tasks may be found in Table 2.1., and the reliabilities are generally high. Allport & Vernon postulate three "group factors" of tempo:

(1) Verbal speed - comprising reading, counting, handwriting and blackboard writing. The inclusion within this cluster of counting, however, may be challenged on the grounds that only one of the relevant coefficients (counting-handwriting) is statistically significant.

(2) Drawing speed - comprising drawing figures on paper, foot-drawing and blackboard-drawing.

(3) Rhythmic speed - comprising finger- and hand-tapping, leg-tapping and stylus compression.

Allport & Vernon further note that group factors (1) and (2) were moderately correlated (rho=0.61), but that these were independent of rhythmic speed.

TABLE 2.1: Intercorrelations from Allport & Vernon (1933)

	1	2	3	4	5	6	7	8	9	10	11	12	13
1		21	55	41	03	38	-07	-23	-20	-09	-03	20	-06
2			45	25	11	-02	01	04	20	34	16	46	-02
3				58	21	58	09	02	02	20	19	43	-16
4					56	90	31	11	00	-02	-12	24	-24
5						42	53	40	35	-11	-12	25	-17
6							36	14	08	-01	13	15	-11
7								21	70	04	-27	11	05
8									66	07	-24	17	02
9										01	-08	39	33
10											-07	23	01
11												16	38
12													29

Decimal points and leading 0's omitted for clarity.

Underlined coefficients are statistically significant ($p < .05$)

Tasks

1. Reading
2. Counting
3. Handwriting
4. Blackboard writing and drawing
5. Drawing figures, lines
6. Foot drawing
7. Finger, hand tapping
8. Leg tapping
9. Stylus compression
10. Walking
11. Strolling
12. Estimating distances
13. Arranging cubes

Another intercorrelational study of the generality-specificity question was performed by Lauer (1933). Unfortunately, however, the report of the experiment is grossly inadequate, and amounts to no more than an abstract. No information is provided regarding the instructions given to subjects (who were 132 males), but Lauer states that a wide variety of activities was used. Reliability coefficients were high (and this accords with the results of Allport & Vernon), but no information is provided regarding the method by which they were obtained.

Only one correlation is actually cited - that of reading and writing, which had a value of 0.4512. This appears to be broadly in agreement with the results of Allport & Vernon, who reported a correlation of 0.55 between reading and handwriting. However, because of the paucity of detail provided by Lauer, it is uncertain whether the measures which he used are truly comparable with those employed by Allport & Vernon.

Though there were apparently specific examples of relatively high intercorrelations, the average intercorrelation of Lauer's measures was very low. Thus, the author concludes that there is no support in his data for the generality hypothesis.

Harrison & Dorcus (1938) also performed an intercorrelational study to provide data on the generality and reliability of tempo-meadures. Their battery consisted of eight tasks:

(i) Turning a crank-handle whilst reading;

(ii) Finger-tapping whilst reading;

(iii) The Johnson-Dunlap co-ordination task;

(iv) Drawing a line, as straight as possible, between two points 9" apart;

(v) Body-bend, in which the subject (in a sitting position) bent forward to touch a padded bar with his forehead, and then returned to the upright position;

(vi) Arm-raising, in which the arm was raised from a horizontal position through an angle of 90 degrees to touch a bar, and then lowered again;

(vii) Head-turn. In this task, the subject first sat facing a mirror and then rotated his head through an angle of 45 degrees to fixate a strip of paper "just long enough to retain a mental image" and then returned to look into the mirror again;

(viii) A final measure, of which the subject was unaware, was taken at the end of the session. The rate at which S walked along a corridor upon leaving the laboratory was recorded.

Subjects were 50 male undergraduates. No instructions as to the rate of performance which was required were apparently given, and subjects were told that the experimenter was "interested in muscular co-ordination and the effects of distraction on normal performance". The order of presentation of tasks was the same for all subjects, and all timing apparatus was concealed.

Harrison & Dorcus make an important point:

"Probably the greatest single source of error was the varying Aufgabe or mental set of the individual reactors. There was a variation in the interpretation of the instructions and a certain amount of uncertainty among the subjects as to -what they thought was expected. Many asked if they were to perform as rapidly as possible and were assured differently, but some of the others who did not specifically inquire may have had similar orientations to the tasks." (P. 36).

Whether or not this difficulty was specific to their study, and arose from some aspect of their particular instructions, or of their own behaviour when dealing with subjects cannot be determined. What would be of interest in this context would be to compare the range of speeds obtained with those reported by Frischeisen-Kohler (1933) who stressed that subjects should perform in "the tempo most agreeable" to them which, it might be thought, would result in a greater homogeneity of interpretation of what was expected. Unfortunately, however, Harrison & Dorcus do not provide the data which would permit such a comparison to be made.

The intercorrelational data obtained by these authors are reproduced in Table 2.2. Note that the test-retest correlations of the various measures appear along the diagonal of the table.

Unfortunately, no information is provided regarding the length of the test-retest interval, though it would appear most likely that the two samples of each activity were taken on <u>different</u> experimental sessions, since the logic of the "walking" measure required that the subject <u>leave</u> the laboratory. The significance of these data is, therefore, not entirely clear, though it will be noted that most of the coefficients are moderate or high. The reliability of arm-raising (0.25) is not, however, satisfactory, and head-turn appears to be only a little superior (r=0.42). One possible reason for these low reliabilities lies in the artificial nature of the tasks; it is difficult to imagine subjects adopting a spontaneous style of performance when asked to perform such apparently strange activities. Rather, it would be expected that they would adopt a somewhat deliberate approach, at least on first performing the tasks. It is interesting to note that Rimoldi (1951, P. 293) points to the strange nature of two of his tasks (both involving arm-swinging) in attempting to account for factor-loadings which he finds difficult to interpret otherwise. Another possible reason for these low reliabilities lies in the fact that the times required to perform such movements would be quite short, so that these might be rather unrepresentative samples of behaviour. Both of these "explanations" encounter difficulties, however, on account of the satisfactory reliability of 0.62 obtained with head-turn, which would be expected to be subject to similar factors.

TABLE 2.2: Intercorrelations and Standard Errors from Harrison & Dorcus (1938)

		1	2	3	4	5	6	7	8
1.	Cranking	65	-39	32	08	12	09	13	-10
		05	08	08	09	09	09	09	09
2.	Tapping		84	-36	-06	-01	-24	-14	-07
			03	08	09	09	09	09	09
3.	Arm-raising			25	32	35	00	-16	-30
				09	08	08	09	09	09
4.	Body Bend				42	52	-07	04	-19
					08	07	09	09	09
5.	Head Turn					62	02	16	-22
						06	09	09	09
6.	Co-ordination						64	43	-01
							06	08	09
7.								78	16
								03	09
8.	Walking								56
									07

Decimal points and leading 0's have been omitted for clarity.

Considering next the intercorrelations between the different tasks, it must first be noted that the authors do not state whether these values were calculated from the first sample of the various activities, from the second, or from some form of composite of the two sessions. However, it will be seen that the highest correlations are those between head-turn and body-bend ($r=0.52$) and between "co-ordination" and line-drawing ($r=0.46$). The former correlation is not readily interpretable, because it is greater than the test-retest reliability obtained with the body-bend task. Though some common variance is suggested between these tasks, therefore, it would be inadvisable at present to place much emphasis on this finding.

The remainder of the correlations (as the authors point out) provide no support for the generality hypothesis. It might, in fact, be argued that some of the intercorrelations might have been spuriously inflated by the grossly unsatisfactory procedure of adopting the same order of presentation of tasks with all of the subjects. Equally, however, it might be suggested that interactions between some of the tasks might have reduced some of the correlations. All that can be said with confidence regarding this feature of design, then, is that it greatly reduces the value of the results reported by these investigators.

In a later study, Harrison (1941) attempted to improve upon his earlier investigation. In particular, he writes:

> "In these studies, it is desired either to confirm or refute the previous study where some doubts remained after its completion as to the effects which the varying "Einstellung" or set of the subjects had on the intercorrelations." (P. 344).

Harrison's modification of procedure in the later research was to instruct subjects specifically to perform at "their usual or chosen rate". As in the previous investigation, however, they were not informed of the aims of the experiment.

The following 12 tasks were performed:

(1) Cranking. This task was also used in the previous study, but, in this later investigation, the author does not state that the subjects were simultaneously reading (as was the case in the 1938 investigation). Whether it should be concluded that they were not is unclear.

(2) Card-sorting. An ordinary pack of playing cards was sorted into its four suits.

(3) Finger-tapping. This task was also used previously and, as with cranking, it is not stated on this occasion whether subjects were simultaneously reading.

(4) Head-turn. This task was the same as in the previous study. However, it is now stated that"... the whole procedure was illustrated first and then practised once or twice by the subject...." (P. 350). It is not clear whether this is intended to indicate that the experimenter himself performed the task in illustrating the procedure, but it is clear that this would be a suspect practice, since the experimenter might thereby suggest a particular rate of performance to the subject. Nor is it clear whether it should be assumed that illustration of the movement was given in the earlier study, where no mention of such a procedure is made.

(5) Body-bend. This task was also used in 1938. It is now stated that the movement was practised, but no mention is made of illustration.

(6) Arm-raising. As in 1938, but practice is again mentioned.

(7) Speed of Decision. Subjects were presented with Part 1 of the Downey Will-Temperament Scale. In this test, the subject is presented with a series of pairs of adjectives and he must select the member of each pair which he considers to be the more appropriate in describing himself. No time-limit was set for the task, but "subjects were cautioned not to require too great a length of time in their judgments" (P. 351).

(8) Writing. In this task, subjects were required to copy a 45-word extract from a novel.

(9) Reading. A 225-word passage was read aloud. The reading was repeated to assess reliability, but no information is provided as to the length of the test-retest interval.

(10) Counting. "Subjects counted to fifty to twos and then backward to two again". This measure was also repeated for reliability.

(11) Patting. This consisted of 20 repetitions of a routine in which "subjects clapped their hands twice and then struck or patted the appropriate knee with the appropriate hand". The task was illustrated and practised and was also repeated for reliability.

(12) Walking. Subjects walked "as nearly as possible in their everyday manner" along a path marked by a chalk-line. Illustration was given, and the task was repeated for reliability.

In addition to the question of the generality of "spontaneous speed", another major focus of attention in this study was the question of the generality of "maximal speed", and its relationship with "spontaneous speed". To this end, on completion of the "spontaneous rate" tasks, 12 tasks were performed at the "maximal rate", with the experimenter attempting to maintain motivation by frequently stressing that performance should be as fast as possible, and with timing-apparatus conspicuously on display. The results of this phase of the investigation will be discussed in the following section of this chapter.

The intercorrelation matrix obtained with the "spontaneous rate" performances is reproduced in Table 2.3. As Harrison points out, there is scant evidence here for the generality hypothesis. Nevertheless, there are some significant correlations (all of which are positive), and these may be further considered.

TABLE 2.3: Intercorrelations and their Probable Errors from Harrison (1941)

	2	3	4	5	6	7	8	9	10	11	12
1	46	45	04	11	05	16	-06	30	-05	15	25
	08	08	10	09	10	09	10	09	10	09	09
2		61	05	25	13	33	25	44	-16	-12	15
		06	10	09	09	08	09	08	09	09	09
3			11	35	15	20	19	49	09	13	52
			09	08	09	09	09	07	09	09	07
4				29	16	04	-03	27	15	58	18
				09	09	10	10	09	09	06	09
5					10	18	33	52	07	12	36
					09	09	08	07	09	09	08
6						07	-26	28	08	12	19
						09	09	09	09	09	09
7							46	52	13	12	20
							08	07	09	09	09
8								34	25	-01	18
								08	09	10	09
9									02	12	47
									10	09	07
10										29	07
										09	09
11											27
											09

1=Head Turn
2=Arm Raise
3=Body Bend
4=Counting
5=Decision speed
6=Walking
7=Cranking
8=Card Sorting
9=Tapping
10=Writing
11=Reading
12=Patting

The following correlations are listed as significant by Harrison:

Arm-raising and body-bend

Counting and reading

Body-bend and patting

Tapping and cranking

Tapping and speed of decision

Tapping and body-bend

Tapping and patting

Cranking and card-sorting

Head-turn and arm-raising

Head-turn and body-bend

Tapping and arm-raising

Speed of decision and patting

Speed of decision and body-bend

Tapping and card-sorting

Arm-raising and cranking

Speed of decision and card-sorting

It will be recalled that Allport & Vernon (1933) postulated three group factors of tempo: "verbal", "drawing" and "rhythmic". "Rhythmic" was used by these authors in the sense of "repetitive" and there are three tests in Harrison's battery (tapping, cranking and patting) which might be thus classified. The intercorrelations between these tasks, as presented in Table 2.3., provide some support for Allport & Vernon's interpretation, but the patting-cranking correlation ($r = 0.20$) is not statistically significant. In addition, there are some significant correlations which would appear to provide difficulties for the "rhythmic" hypothesis, notably those between the "rhythmic" tasks and the "once-off" tasks (head-turn, arm-raising and body-bend). The correlation of tapping and cranking with card-sorting might be accounted for if it is hypothesised that subjects adopted a regular and repetitive mode of performance in the sorting task (a suggestion which is by no means implausible), but the correlation of tapping and patting with "speed of decision" is not easily accounted for under such a hypothesis.

The correlational pattern obtained with the "once-off" tasks suggests a possible alternative interpretation. These are all "motor" tasks, and it might be suggested that each individual has a stable and characteristic "motor tempo" for a given muscle-group. It might be further supposed that the motor tempi of different muscle-groups are intercorrelated (perhaps because of intercorrelations between the lengths of limbs, for example). The rate of finger-tapping might then be regarded as a reflection of the "finger-tempo", which would, therefore, be correlated with the rate of performance of other motor activities.

The interpretation of such tasks as tapping in terms of a "motor tempo" may be contrasted with the interpretation in terms of a "rhythmic tempo". The latter account suggests that it is the repetitive element common to tapping, patting and cranking which results in their correlation. (The rhythm produced when performing these tasks might, for example, be synchronised with the "ticking" of some "internal clock"). Thus, tasks which are not repetitive (so that a rhythm is not produced when they are performed) would not be expected to be correlated with tapping under this hypothesis. The "motor" hypothesis, on the other hand, suggests that it is not the repetitive element of tapping, patting and cranking which accounts for their intercorrelations, but the common involvement of the motor system. Under such a hypothesis, correlations between repetitive and non-repetitive motor tasks would be expected.

However, doubts are cast on the interpretation of repetitive tasks as reflecting a "motor tempo" by several aspects of previous literature. Stetson (1905), for example, provides three relevant observations. Firstly, he noted that, in the performance of rhythmic tasks (eg., swinging a conductor's baton) at relatively low speeds, the limb in question is actually stationary for considerable periods between the beat-stroke and the back-stroke. Secondly, even when an examination is made of the periods during which the limb <u>is</u> moving, there is no evidence of a stable "motor tempo", for: "...the velocity of the beat-stroke is always two or three times greater than the velocity of the back-stroke" (P. 259). Finally, "the duration of the beat-stroke is strikingly uniform, and it is independent of either the tempo of the rhythm or the length of the stroke" (P. 261). Rimoldi (1951), in a paper which will be discussed later in this section, notes several items of evidence which appear to refute the hypothesis of a "motor tempo" (most of them demonstrating invariance of the number of objects drawn or letters written with considerable variations in size and, therefore, distance moved). A similar conclusion is suggested by the results of Jordan (1970), which will be discussed in a later section of this chapter.

Despite these doubts concerning the plausibility of the "motor" hypothesis, it was nevertheless asserted that this hypothesis would lead to the prediction of correlations between repetitive and non-repetitive motor tasks, whilst the "rhythmic" hypothesis would not. Yet these are the results which Harrison obtained. Is the "rhythmic" hypothesis thereby refuted?

The answer to this question is not necessarily affirmative. It will be recalled, for example, that the "once-off" tasks were practised before the trial which was actually recorded. The question then arises as to how this practice was accomplished. Were the practice trials performed sequentially without interruption or pauses, so that a rhythmic style of performance developed, or were the trials interspersed with intervals of randomly varying length, thus precluding the development of a rhythm? Was there a cessation of practice before the movement which was actually measured, or did the investigator ask the subject to begin repetitively performing the movement and then record one of the repetitions? Unfortunately, Harrison answers none of these questions.

This discussion of the possibly repetitive nature of practice brings to mind another interesting point. Had tasks such as body-bend actually been performed repetitively, and had the scores been in terms of the mean time per movement over a number of repetitions, then these tasks would without hesitation have been classified as "rhythmic". The correlation between body-bend and tapping could then have been interpreted with ease by means of the "rhythmic" hypothesis. But would the mean time obtained in this way be unrelated to the single time which would be obtained if the task were performed in a "once-off" fashion? In short, is it plausible to argue that a single movement might itself be executed in synchrony with the ticking of an internal clock?

Clearly, this argument is speculative, and may be said to have taken on an "ad hoc" character. However, in the absence of information concerning the exact manner in which the tasks were performed, its plausibility cannot be properly assessed. What should be noted is that Harrison's results with "once-off" tasks cannot be regarded as a definite refutation of the "rhythmic" hypothesis.

The failure of walking to correlate with "once-off" or repetitive tasks might be regarded as surprising. Unfortunately, however, Harrison does not state which aspect of walking was recorded. Clearly it would be possible to record either the time taken to cover a certain distance, or the number of paces taken per unit of time, but it is by no means certain that these two methods would result in identical correlational structures. In the absence of further information, then, Harrison's result is not readily interpretable.

Allport & Vernon's "verbal composite" is also relevant to a discussion of Harrison's results. Harrison points out that, if by "verbal" is meant "the manipulation of words", then writing, reading and "speed of decision" might be thus classified. Allport & Vernon also include counting in this group, but it was argued above that its inclusion might not be warranted. One aspect of Harrison's results which provides difficulties for the hypothesis of a "verbal" factor is the non-significant correlation between writing and reading ($r = 0.29$). This conflicts with the correlation of 0.55 which Allport & Vernon reported between reading and "handwriting" and also with the correlation of 0.4512 which Lauer (1933) reported between the same activities.

Another interesting facet of Harrison's investigation is that, upon completion of the "spontaneous rate" tasks, subjects rated themselves as "fast", "medium", or "slow", as shown by their everyday activity". These judgments were then correlated with the objective tempi of performance of the laboratory tasks, and Harrison concludes as a result of this analysis that subjects are able to rate themselves with better than chance accuracy. However, the distribution of ratings was skewed, since more subjects rated themselves as "fast" than rated themselves as "slow". Harrison suggests that speed is a positively valued trait in American society.

A further experiment is reported in Harrison's paper, in which the hypothesis was tested that repetition of the experimental tasks would result in a greater generality of tempo. Two groups of subjects, each consisting of 10 males and 10 females, performed six repetitions of the following tasks: cranking, tapping, head-turn, arm-raising, body-bend, writing, reading, counting, patting and walking. The first group attended for testing on six consecutive days, whilst the second group attended on the same day of the week for six consecutive weeks. The latter group did not provide data which supported the hypothesis. In the case of the "daily" group, however, an increase was observed in the average intercorrelation between tasks. The increase was not consistent, however, since not all of the intercorrelations increased, and those increases which did occur were comparatively small. In view of the relatively small number of subjects involved, Harrison concludes that these small variations might be attributable to chance factors.

A final result which emerges from Harrison's second experiment is that no sex-differences in tempo were observed, except in walking, where Harrison suggests that differences in footwear used by the two sexes may account for the difference. The topic of sex-differences will be covered in a later section of this chapter.

The most ambitious of these studies of the generality-specificity question was reported by Rimoldi (1951), whose investigation involved 91 subjects and 59 tasks. Instructions directed the subjects to perform the tasks in the "most natural congenial way". Test-retest reliability data were obtained, since 17 of the subjects returned for retest at between two and four weeks after the original session. Though, as the author points out, this is a relatively small number (presumably due to difficulties of recruitment in view of the fact that the experimental session was approximately three hours in duration), the test-retest correlations which he reports are very satisfactory.

Nine oblique factors resulted from Rimoldi's analysis, and these will be discussed in turn. This discussion will be hampered by two shortcomings in Rimoldi's report, however: the matrix of intercorrelations is not included, and the tasks are not adequately described. Once more, it appears necessary to make suppositions about the procedural details of an experiment.

Factor A

Test 1	Swinging right arm	0.54
Test 2	Swinging left arm	0.59
Test 3	Symmetrical movement arms	0.70
Test 4	Parallel movement arms	0.73
Test 5	Swinging right leg	0.67
Test 6	Swinging left leg	0.60
Test 7	Symmetrical movement legs	0.57
Test 8	Parallel movement legs	0.66
Test 17	Ergograph	0.31
Test 20	Finger to nose	0.51
Test 21	Bending body	0.63
Test 18	Counting	0.24

In interpreting this factor, Rimoldi suggests that it is related to the "spontaneous speed with which large movements are performed", and also that most of the movements saturated in the factor are involved in gait. As he notes, however, the rate of walking itself does not appear on the factor, though it is suggested that this may be due to difficulties he experienced in standardising the conditions under which that task was performed.

An interesting question concerns whether a correlation might exist between limb-length and the rates of performance of arm- and leg-swinging tasks. In these conditions, the limbs may be acting rather as pendulums, and, were they allowed passively to swing, would do so at a frequency which was dependent upon their length. It might be suggested that this frequency of swinging would represent a condition of "least effort" on the part of the subject since, once the oscillation was begun, only a small input of energy would be required on each swing for the motion to be maintained. Rimoldi does not, unfortunately, provide the mean rates of performance of these tasks, so that the plausibility of the suggestion that subjects were swinging their limbs at a rate which could be described by the classical physical equation for calculating the frequency of a pendulum cannot be determined. Clearly, however, this is an interesting question, and it may have important theoretical consequences for our conceptualisation of some tempo-measures.

Factor B

Test 13	Tapping right arm	0.75
Test 14	Tapping left arm	0.71
Test 15	Tapping right fingers	0.70
Test 16	Tapping left fingers	0.70
Test 12	Tapping left toes	0.69
Test 11	Tapping right toes	0.67
Test 10	Tapping left heel	0.56
Test 9	Tapping right heel	0.55
Test 18	Counting	0.39
Test 30	Copying	0.24
Test 35	Reading Science	0.24
Test 36	Reading news	0.22

The first point to note here is that, in a second-order analysis, it was determined that the correlation between Factors A and B was 0.51 "which qualifies the possibility of interpreting these factors as completely independent and suggests some sort of generality of speed for many varied muscular activities" (P. 291). The intercorrelations between the tasks in the two clusters were also high, and this is of interest in view of the question regarding the effects of limb-length on limb-swinging tasks.

As was the case with Factor A, the "rhythmic" interpretation might be applied to most of the tasks in this group. Counting may be performed "rhythmically", but it must be pointed out that in neither the study of Allport & Vernon (1933), not that of Harrison (1941) were any significant correlations reported between "rhythmic" tasks and rate of counting. Copying and reading cannot be adequately accounted for under the "rhythmic" hypothesis, however. Once again, neither Allport & Vernon (1933) nor Harrison (1941) report significant correlations between reading and copying on the one hand, and "rhythmic" tasks on the other. The loadings of these tasks on Factor B are, in any event, low, and, in the absence of Rimoldi's intercorrelations, it is not clear how much weight should be placed upon them in interpreting the factor.

Factor E

Test 25	Right foot circles	0.77
Test 26	Right foot squares	0.76
Test 29	Left foot squares	0.74
Test 28	Left foot circles	0.73
Test 27	Left foot lines	0.66
Test 24	Right foot lines	0.66
Test 56	Left hand circles	0.29
Test 57	Left hand squares	0.27
Test 32	Cancellation	0.25
Test 53	Right hand circles	0.24
Test 54	Right hand squares	0.22
Test 1	Swinging right arm	0.22
Test 5	Swinging right leg	0.22
Test 6	Swinging left leg	0.21
Test 2	Swinging left arm	0.20
Test 7	Symmetrical movement legs	0.20
Test 17	Ergograph	0.20

The tasks most heavily loaded in this factor involve drawing with the feet. Rimoldi accounts for the loading of the limb-swinging tasks on this factor by suggesting that the drawing-tasks require large movements, with relatively little fine co-ordination. The loadings are quite low, however, so that there is not a great deal to account for. Nevertheless, these loadings might be taken to favour the "motor", rather than the rhythmic" hypothesis, since, though there is considerable involvement of the motor system in the drawing-tasks, they do not appear to be "rhythmic" in the same sense as, for example, tapping or limb-swinging. In considering Factor H, however, it will be seen that there is a possible counter to this argument.

Factor H

Test 52	Right hand lines	0.74
Test 53	Right hand circles	0.64
Test 54	Right hand squares	0.64
Test 55	Left hand lines	0.62
Test 56	Left hand circles	0.53
Test 57	Left hand squares	0.50
Test 31	Writing e's	0.32
Test 30	Copying	0.30
Test 1	Swinging right arm	-0.22
Test 2	Swinging left arm	-0.22
Test 42	Number	-0.20

Here again is a "drawing" factor, but in this case the highest loadings involve tasks in which the hands are used. Though hand-drawing and foot-drawing appear on two separate factors, Rimoldi states that the average intercorrelation between these two types of drawing was 0.36; Allport & Vernon (1933) reported a correlation of 0.42 between foot-drawing and drawing (presumably with the hands) on paper.

Of considerable interest is the fact reported by Rimoldi that, though the number of drawings made in a given time displayed remarkable intra-subject consistency (from the first to the second trial), there was considerable variety in the sizes of the drawings which were produced. In this connection, Rimoldi notes the finding of Freeman (1914), as cited by Hartson (1939), to the effect that the duration of a movement in rapid writing is not necessarily increased when the extent of the movement is increased. It appears, then, that, in Rimoldi's drawing-tasks, the velocity of the body-segment involved is not constant from one trial to the next. This casts serious doubts on the plausibility of the "motor" hypothesis.

The negative loadings of tasks 1, 2 and 42 on Factor H are interesting. Rimoldi suggests that the loadings of tasks 1 and 2 might be accounted for because they were the first to be performed, and many of the subjects expressed surprise at being requested to perform such tasks in a psychological experiment. This highlights a gross methodological flaw in Rimoldi's study: an invariant order of presentation of tasks appears to have been used.

Test 42 is the "Number" subtest from Thurstone's Primary Mental Abilities (PMA) test. Rimoldi is able to offer no explanation of its negative loadings.

Factor C

Test 36	Reading news	0.67
Test 35	Reading Science	0.66
Test 34	Reading literature	0.66
Test 23	Marbles left	0.52
Test 39	Verbal meaning	0.50
Test 22	Marbles right	0.49
Test 19	Sorting cards	0.48
Test 41	Reasoning	0.34
Test 42	Number	0.47

Test 33	Number of things	0.30
Test 48	Recognition	0.27
Test 56	Left hand circles	0.25
Test 32	Cancellation	0.24
Test 57	Left hand squares	0.23
Test 38	Speed of walking	0.20

This Factor is difficult to interpret. Rimoldi rejects its possible interpretation as "verbal", arguing that a more fundamental function, "speed of perception", appears to be at work. The present writer, however, is not convinced of the plausibility of the suggestion that a sufficiently large proportion of the variance of such tasks as the "Number" subtest of Thurstone's PMA can be accounted for by individual differences in the rate of reading of the instructions, as Rimoldi suggests. This argument surely implies that there are no individual differences in the "computing-time" required to calculate the problems, or that such individual differences are positively correlated with differences in reading-rate. Neither of these suggestions appears very plausible.

In the "marbles" task, subjects had to transfer marbles from a tray on one side of them to a tray on the other side, and "right" and "left" refer to the hand which was used to perform the task. Unfortunately, Rimoldi provides no information regarding the details of the arrangement of the task. In particular, he does not quote the distance between the two trays, so that it is not clear whether both were in the subjects' stationary visual field, or whether eye- or head-movements were required when performing the task. If the 1 latter were the case, then it is possible that rapid perceptual processes were involved when the subject was directing his hand towards the tray from which the marbles were to be transported. If no eye-movements were required, however, it is a little difficult to see how "perceptual speed" could have played a sufficiently important role in determining the rate of performance of the task. However, if Rimoldi's "perceptual speed" interpretation encounters difficulties with this task, "verbal speed" interpretations would obviously fare even less well.

A further point which should be made in connection with the PMA tests is that subjects may possibly have been motivated to perform them at their "maximal rate", even though they were not instructed to do so. It will be recalled that Harrison & Dorcus (1938) found a "set for speed" to be widespread amongst their student-subjects, and it might intuitively be thought that such a set would be particularly likely to be active in the performance of intelligence-test items, both because students in higher education are accustomed to the performance of tests and examinations under conditions of limited time, and also because they may regard a test of this type as a challenge, and something in which they should give a good account of themselves.

More information is required on these matters; it may be, for example, that "spontaneous rate" and "maximum rate" are not different for this type of task, the most "natural and congenial" way might be the fastest way. On the other hand, it might be found that subjects will perform at a rate below their maximum if it is emphasised that maximum speed is not required. These questions are fundamental to the concept of "personal tempo" when applied to this type of task, and, therefore, they warrant further investigation.

Factor G

Test 45	Reaction time to light	0.74
Test 46	Discrimination	0.68
Test 47	Discrimination and choice	0.62
Test 44	Reaction time to sound	0.44
Test 17	Ergograph	0.27

Test 43	Word fluency	0.25
Test 51	Faces	0.23
Test 3	Parallel movement arms	0.21
Test 4	Symmetrical movement arms	0.20
Test 30	Copying	0.26

The simple and choice reaction-time tasks which receive the heaviest loadings in this Factor were "maximum rate" performances; subjects were instructed to respond as quickly as possible. Task 51 "involved affective judgements on Brunswik's faces", and a voice-key was used to record the response-time. Rimoldi appears somewhat perplexed by the loadings of tests 17, 3, 4 and 30, but it is questionable, in view of their low loadings, whether much weight should be placed on them in interpreting the factor.

Factor I

Test 48	Recognition	0.45
Test 50	Free association	0.45
Test 49	Equality and difference	0.43
Test 40	Space	0.43
Test 41	Reasoning	0.41
Test 55	Left hand lines	0.40
Test 52	Right hand lines	0.36
Test 51	Faces	0.34
Test 33	Number of things	0.28
Test 42	Number	0.21
Test 47	Discrimination and choice	0.20
Test 13	Counting	0.21

Tests 48-50 were performed "as quickly as possible". In task 48, the subject was required to identify simple geometric figures, and in task 49, the same figures were presented in pairs, and S had to say whether the members of each pair were identical, or whether two different figures had been presented. Task 51 was one of the PMA subtests.

Rimoldi favours interpretation of "cognitive speed" for this Factor. Clearly, however, the loadings of tests 52 and 55 are difficult to reconcile with this interpretation. The fact that we are dealing with a mixture of "maximal rate" and "spontaneous rate" measures also provides difficulties in interpretation. It will be seen shortly that some of the correlations reported by Harrison (1941) between performance of the same task under these two instruction-conditions were quite low, so that it is conceivable that a mixture of "spontaneous" and "maximal" measures might produce a different intercorrelational structure from that which would arise if the same tasks had all been performed under the same instruction-conditions. (In this case, it is difficult to envisage how the maximal-rate tasks could have been meaningfully performed as "spontaneous" tasks, but the difficulty in interpretation still arises.)

Factor D

Test 22	Marbles right	0.62
Test 23	Marbles left	0.61
Test 20	Finger to nose	0.34
Test 37	Pulse	0.30
Test 39	Verbal meaning	0.27
Test 42	Number	0.26
Test 50	Free association	0.21
Test 21	Bending body	0.20
Test 56	Left hand lines	0.20

As Rimaldi points out, this Factor is essentially a doublet, with the major loadings associated with the "marbles" task. As has been seen, these tests also receive significant loadings on Factor C, which was interpreted by Rimoldi as a "speed of perception" factor. However, the loadings of tests 20 and 21, and the fact that Factor D is positively correlated with Factors A and B lead Rimoldi to "... favour the possibility of interpreting (the factor) as some kind of motor activity...".

The role of the "marbles" task is clearly rather complex; it may be the case that there is no overlap between the variance accounted for by Factor C and that accounted for by Factor D, and that both "perceptual speed" and some element of "motor speed" are compromised in determining the rate at which the task is performed, but it is impossible to be specific on this matter in the absence of the intercorrelations between tasks.

Rimoldi himself remarks that "It is interesting to note that none of the tests of tapping has any appreciable loading in Factor D." It appears, then, that he regards the "marbles" task as in some ways similar to the tapping-tasks, though he makes no attempt himself to reconcile this conception with his interpretation of Factor C.

The loadings of pulse-rate on this Factor might be interesting had the author provided adequate details concerning the methods by which the measure was taken; heart-rate is variable according to the conditions under which it is measured, and Rimoldi unfortunately does not state whether the subjects were seated, standing or lying down, or whether measurement was continued until a baseline level of pulse was reached. Though the possibility of physiological correlates of behavioural tempi is worthy of study, there is little to be gained from making such investigations if inadequate methodology is employed. Because of the paucity of detail in Rimoldi's report, there is uncertainty as to the exact significance of his "pulse"measure.

Factor F

Test 59	Metronome second	0.68
Test 58	Metronome first	0.65
Test 17	Ergograph	0.27
Test 30	Copying	0.26
Test 4	Parallel movement arms	0.21
Test 43	Word fluency	0.21
Test 3	Symmetrical movement arms	0.20
Test 49	Equality and difference	0.20
Test 55	Left hand lines	-0.21

This Factor is also a doublet. Rimoldi cites Frsicheisen-Kohler's (1933) study, in which both tapping rate and a preferred metronome-rate were measured. Though Frischeisen-Kohler did not use the technique of correlation, an Index of Variability calculated from the combined metronome and tapping measures suggested a fairly close correspondence between the two speeds. It is interesting to note therefore, that none of the tapping-tasks is loaded on Factor F. One possible reason for this discrepancy is that Frischeisen-Kohler's procedure produced a spurious correspondence between the rates: these were the only tasks performed by her subjects, and it is possible that the subjects thought that the experimenter expected such a correspondence. Furthermore, with such a limited battery it may be comparatively easy for subjects to remember their tapping-rate when they are making their judgements on the metronome-rates. Rimoldi's procedure, on the other hand, would minimise the role of memory, because, whilst the tapping-tasks were performed relatively early in his experimental sessions, the metronome-task is the last in his list. Thus, there may be a great deal of interference from the interpolated tasks.

Just as the above argument regarding interpolated tasks might be used to suggest that Frischeisen-Kohler's result showed a spuriously high relationship between tapping-rate and preferred metronome-rate, so it might also be argued that Rimoldi's vast battery of tests resulted in spurious relationships because the length of the experimental sessions may have been so great that "spontaneous" performance could not be maintained over the entire period. We shall return to this when formulating our general conclusions on Rimoldi's work.

Rimoldi finally performed a second-order analysis, and the oblique Factor-matrix which emerged from this is reproduced in Table 2.4.

Factor alpha is interpreted as "motor", though the author admits that the low loading of primary Factor H, comprising mainly tasks involving drawing with the hands, is difficult to understand. In interpreting Factor Beta, Rimoldi argued that the loadings of primary Factors D and F are not readily interpretable, since they are both primarily doublets. It is interesting to note the negative loading of Factor D, however, in view of the fact that the "marbles" tasks (of which it primarily consists) produced difficulties when interpreting Factor C (which receives a high positive laoding on Factor Beta) because of their positive loadings on that Factor. This state-of-affairs highlights the difficulty of interpreting such data as these. Rimoldi's final interpretation of second-order Factor Beta is rather a negation than a positive interpretation: "Factor Beta could be understood as a kind of

TABLE 2.4: Second-order Factor Matrix from Rimoldi (1951)

	Alpha	Beta	Gamma	Delta
A	59	05	08	-06
B	68	16	-13	15
C	-02	68	-04	47
D	36	-46	-02	-07
E	63	-05	02	49
F	03	46	23	00
G	03	11	20	40
H	05	01	-44	18
I	-05	04	65	10

Decimal points and leading 0's have been omitted for clarity.

fundamental speed activity, independent of motor speed and other non-motor functions". (P. 297).

Factor Gamma is interpreted as "intellectual speed", though the difficulty of accounting for the negative loading of Factor H is admitted. "Factor Delta seems to be mainly related to the speed of reaction-time ...." (P. 298) though how this may be reconciled with the loading of primary Factor E (foot-drawing, etc.) is not made clear. It will be noted, in fact, that the loading of Factor E is greater than that of the reaction-time primary factor (G) itself, and that of the so-called "speed of perception" factor.

In summary, Rimoldi has provided evidence of highly satisfactory test-retest reliabilities of tempo-measures and his results, like those of Allport & Vernon (1933), Harrison & Dorcus (1938) and Harrison (1941) have provided no support for the hypothesis of a general "personal tempo". Equally, however, the hypothesis of extreme specificity is at odds with his findings.

In discussing his factors, interpretation has sometimes been difficult, because one or more loadings have apparently falsified hypotheses which might have been suggested. Part of the reason for such difficulties lies in the lack of detail in Rimoldi's report which has, on occasions, rendered it difficult to determine what task the subjects were actually performing. With such a large battery of tests, the possibility should also be considered that some of the loadings which have proved difficult to interpret might represent Type I errors.

Another feature of the experiment which arises out of the large number of functions investigated by Rimoldi is the extreme length of the experimental sessions. It is difficult to imagine that subjects were able to maintain a "spontaneous" style of performance over such a period. It would seem likely, in fact, that fatigue and boredom would be considerable in such a situation. Any effects of fatigue and boredom (or practice) would constitute a serious difficulty with Rimoldi's design because he apparently employed an invariant order of presentation of tasks. Moreover, it is possible that this facet of the design affected the intercorrelational structure which Rimoldi obtained, due to interactions between tasks which were placed in adjacent positions in the order. Such interactions might sometimes lead to spuriously high correlations, or sometimes to spuriously low correlations. This aspect of the design in particular, then, severely limits the value of the results reported by this investigator.

The most recent studies in the field of "spontaneous speed" are those of Smoll (1975a, 1975b). The first of these papers will be considered in a later section of this chapter, but it is appropriate to consider the second at this point, because it concerns the question of reliability.

The one task which was investigated in both of these experiments was a swinging movement involving the preferred arm. The subject adopted a standing position and gripped a lever which was connected to recording apparatus. From a starting position with the arm hanging vertically, the subject was required repetitively to raise the arm so that a sight on the lever was aligned with a target. The instructions requested the subject to repeat the movement "... at a chosen tempo that was most comfortable for him ...", and it was emphasised "not to perform at the fastest tempo, but at that tempo which was most convenient and natural" (1975a: P. 441, 1975b: P. 732).

In the second (1975b) experiment, Smoll investigated the test-retest reliability of adopted tempi over two consecutive days. 60 subjects served in the investigation, and the procedure involved the performance of 36 repetitions of the movement. On both days, an asymptotic increase in speed was observed with successive repetitions. The asymptote was reached earlier on the second day (by repetition 9) than on the first (by repetition 15).

Test-retest correlations are presented both for mean movement-time and for the variability in movement-time. For the former variable, a correlation of 0.94 ($p < .01$) was obtained, and Smoll reports a value of 0.77 ($p < .01$) for the second. However, the two variables were correlated, so that Smoll considers it more meaningful to report a partial correlation for each variable, with the effect of the second held constant. The obtained coefficients were: for mean movement-time, 0.79 ($p < .01$) and for variability, 0.32 ($p < .05$).

Thus, Smoll's results are broadly in line with previous investigations of the reliability of "spontaneous tempo". The reliability of within-subject variability, though statistically significant, is not very high, however. Factors which affect within-subject variability may prove to be an interesting topic for future research.

2. The Relationship between "Spontaneous" and "Maximum" speeds.

Following the question of the reliability and generality of "spontaneous speed", the next most obvious question concerns its relationship with "maximum speed". As Smoll (1975a) points out, the number of psychological investigations in which "maximum speed" has been required is large. However, the number in which the relationship of "maximum speed" with self-paced or "spontaneous" tempo has been studied is rather limited. Three such investigations will be considered under this heading.

The earliest of these studies was reported by Harrison (1941), and this experiment has already been discussed in part in the previous section of this chapter. A detailed description of tasks and experimental situations will not be required here, therefore. As with "spontaneous" speed, Harrison was interested in the generality of tempo under "maximum rate" instructions. Tapping, cranking and patting were all significantly correlated (as they were in the "spontaneous rate" condition) and tapping was also significantly correlated with a test of finger-dexterity, though the amount of common variance suggested by the correlation ($r = 0.34$) is not very great. In the "maximum rate" condition (in contrast with the "spontaneous rate" condition) reading and writing were significantly correlated.

What is of more interest in the present context is the relationship between the "spontaneous" and "maximum" rates of performance of the six tasks which were performed under both instruction-conditions. These correlations are reproduced in Table 2.5, together with their standard errors. It will be seen that the correlation obtained with reading is high, that the value

TABLE 2.5: "Spontaneous"-"Maximum" Correlations from Harrison (1941)

Task	r	PE
Card Sorting	17	09
Tapping	35	08
Writing	46	08
Reading	70	08
Cranking	09	09
Patting	36	08

TABLE 2.6: Correlations between Instruction Conditions from Mishima (1968)

	Maximal	Minimal
Congenial	-137	351
Maximal		-265

In both tables, decimal points and leading 0's have been omitted for clarity.

for writing is moderate, and that those for patting and tapping are low (though they are statistically significant by the criterion of being more than four times their standard error). The values for cranking and for card-sorting do not attain significance by this criterion. What emerges from these results, then, is that it is not possible to make statements concerning the relationship between "spontaneous" rate and "maximum" rate in general; some tasks "behave" differently from others. One very interesting question for future research would be to investigate why such differences occur. It would also be interesting to know whether tasks which form clusters in intercorrelational studies also "behave" in the same way in this regard. If one task displays a high "spontaneous" - "maximum" correlation, do all those with which its "spontaneous" rate is correlated do so?

The second study to be considered unded the present heading is the first of a series of recent investigations which have been performed in Japan, and which will be discussed at numerous points in this chapter. Though the English translations of these papers are often very poor, this work is of great interest here because it appears to have been carried out in ignorance of the Western research, and also because the journal in which it was published is not widely available here.

The first of these Japanese investigations, then was reported by Mishima (1968), who compared tapping-rates elicited by the instructions to tap "with the speed at which you feel most natural and easy to tap" with those elicited by "maximal way" and "minimal way" instructions. One interesting outcome of this study is stated thus by the investigator:

"Furthermore, the mental tempo by congenial way was proper to each individual person, but there was little difference by both maximal and minimal ways among subjects of the same age" (P. 58).

In analysing his data, Mishima first divided his subjects into a "faster' group and a "slower" group, according to whether they fell above or below the mean in the "congenial way" condition. He then examined the mean rates of performance of these two groups in the other two instruction-conditions and found that, whilst they were not significantly different in the "maximal way" condition, the "faster" group was also significantly quicker in the "minimal way" condition.

Mishima's intercorrelations are presented in Table 2.6. He dismisses all of these correlations except the "congenial" - "minimal" value, but the "minimal" - "maximal" value is also significant ($p < .01$) and the "maximal" - "congenial" correlation is also probably significant at that level (Garrett, 1966: P. 201). The amount of common variance represented by these coefficients is very low, but the significant negative correlation between "maximal" and "congenial" conditions is certainly at variance with Harrison's (1941) low <u>positive</u> correlation between these conditions.

Fujita (1970) reported a series of experiments which are of interest in that they constitute an attempt to relate the study of "personal tempo" with that of skilled performance in general.

In the first experiment, Fujita studied a task which we have not encountered in the literature reviewed hitherto: mirror-drawing. 20 University students acted as subjects, and the principal part of thd experiment involved the performance of 100 practice trials of mirror-drawing, using a star-shaped track.

The instructions required subjects to "...trace within the boundaries as fast as possible...".

Immediately prior to the training-trials, 5 "maximal-rate" trials were performed without the mirror. Two months after the training-trials, a further 3 trials were performed without the mirror, but on this occasion, the subjects were instructed to "trace within the boundaries of the path at the speed which you feel most natural and easy". One week after the "congenial way" trials, a further 10 trials of mirror-drawing were performed, in order to assess the reliability of the measure obtained during the 100-trial practice period.

In discussing Fujita's results, it will be convenient to use the following abbreviations: the five maximal-way" trials without the mirror which preceeded the practice-trials will be referred to as the "maximal way" (MW) condition; the practice-trials are referred to by Fujita as the "skilled level" measure, and the three "congenial way" trials performed two months after the training trials are referred to as the (CW) condition. It should also be noted that the 20 subjects were subdivided into two groups of 10, and that one of the groups performed under a "massed practice" condition, having all 100 training-trials in one day, whilst the other performed 25 trials on each of four consecutive days.

The correlation between "maximal" and "skilled" rate was far from significant in both groups ("massed" group, 0.077; "distributed" group, 0.078). Unfortunately, we are not told which correlation coefficient was used. For the "skill"-CW comparison, the "massed" group produced a correlation of 0.420 (df = 8; $p < 0.05$), and the "distributed" group a correlation of 0.673 ($p < 0.05$). The author describes these correlations as "very high", but this is clearly not the case if our understanding of his ill-described procedure is correct, for they were obtained with groups of only 10 subjects. Also described as "very high" are the MW-CW correlations, for which Fujita reports a value of 0.886 ($p < 0.01$) for the "massed" group, and 0.561 ($p < 0.05$) for the "distributed" group.

The author also reports the results of statistical analysis (by t-tests) on the mean rates of performance under the three conditions. The MW condition produced significantly quicker performance than was observed either in the CW or the "skill" conditions, but "skill" and CW were not significantly different. This pattern was observed with both groups.

It is curious to note that one 't' value of 3.02 ("massed" group, MW-"skill") is reported as significant at the 0.01 level of confidence. Since Fujita's experiment was of the repeated-measures design, the 't' test for correlated means would, of course, be appropriate and, with 10 subjects in each group, would provide 9 degrees of freedom. With that number of degrees of freedom, however, a 't' value of 3.25 is required for significance at the 0.01 level of conficence, and it may be, therefore, that Fujita used the formula for independent means, which would provide 18 degrees of freedom. A 't' value of 2.878 is required for significance at the 0.01 level with 18 degrees of freedom, and this would seem to explain the significance-levels which Fujita quotes. It is a strong possibility, therefore, that Fujita used an inappropriate formula in his analysis. In fact, the use of the independent-means formula on correlated data provides a conservative test, and it is doubtful whether its inappropriate use here invalidates Fujita's findings. However, it naturally produces doubts as to the statistical competence of the author.

One forther point which should be made is that, as we have regrettably found to be common in experiements on "personal tempo", the order of presentation of the various conditions was not varied across subjects. Varying the order of presentation would have lent greater generality to the results.

A replication of the above study is reported by Fujita, in which only the "massed practice" condition was employed, since the previous investigation had not revealed significant differences between the two procedures. One group of 20 subjects was employed, but the order of presentation of conditions was again not varied. The results of the 't' test analysis were in accord with those of the previous investigation (there is no reservation about the degrees of freedom in this case, since the 't' values obtained were sufficiently high not to be affected by the incorrect procedure). The intercorrelations were as follows: "skill"-CW, 0.433; CW-MW, 0.350; MW-"skill", 0.196. For df = 18, a value of 0.444 is required for significance at the 0.05 level of confidence.

On the basis of these two experiments, Fujita concludes that there is a "common tendency" between mirror-drawing and "congenial way" drawing. As has been seen, it is possible to challenge his interpretation of his data, but it appears that this is a line of enquiry which might usefully be pursued further. One factor which may have moderated any correlation which might exist between these two measures, for example, is that the "congenial way" measure was taken two months after the 100 training-trials. Since it is known from previous research that the test-retest reliability of many tempo-measures is high but not perfect, this procedure may have resulted in spuriously low correlations. Furthermore, in the first experiment, some of the correlations between CW performance and individual trials during the 100-trial training-period <u>are</u> very high. As an exploratory study, therefore, Fujita's investigation is of interest, despite its flaws.

A third investigation is reported by Fujita, in which a more complex cognitive task, addition, was studied. The design was similar to that employed in the previous experiments, except that the "congenial way" trials were performed before, and not after, the training-trials. 20 student-subjects were employed, and these were randomly selected from those who took part in the mirror-drawing experiments. This enabled the author to examine the relationship between speed of addition and tapping-rates and "preferred" metronome-rates, which had been determined for the subjects at the time of those experiments. In fact, these measures were not found to be significantly correlated with addition-rate, and this is probably in accordance with what we would expect in the light of the research which has been previously discussed. However, tapping-rate and preferred metronome-rate were found to be significantly correlated. (It is again not stated which correlation coefficient was used, but a value of 0.898 is reported). This appears to confirm Frischeisen-Kohler's (1933) result, but to be in conflict with Rimoldi's (1951) findings, where tapping-speed and preferred metronome-rate were not loaded on the same factor. Since neither Fujita nor Rimoldi describes the way in which the metronome-rate was determined, it is difficult to suggest reasons for this discrepancy. However, this is clearly an issue which merits further investigation; were Fujita's results confirmed, for example, it would be plausible to suggest that, when a subject spontaneously adopts a certain tapping-rate, he does so because the sound produced by that rate is in some way pleasing to him. Clearly, there is no point in enumerating more detailed hypotheses in the absence of the empirical data which might relate to them, but it would obviously be valuable to conduct research into this question.

The design of the experiment called for 5 "congenial way" trials ("Do the test accurately at the speed you find most natural and easy"), followed by 200 practice-trials with the instructions: "Do the test accurately as fast as possible". These instructions were again given two months later, when a further 5 "maximal way" trials were performed.

The results of the experiment revealed a non-significant difference in the mean rates of performance under the MW and CW conditions, and that the correlation between them was 0.854. It will be recalled that Harrison (1941) complained that a "set for speed" was prevalent amongst student subjects, to the extent that he felt obliged to stress that maximum-speed performance was not required in his tasks. A similar "set for speed" may lie behind Fujita's results. Indeed, it might be argued that the use of the word "test" in the instructions would be particularly likely to produce in the subject motivation to perform as quickly as he could. Clearly, this is an important finding, because it casts doubt on the validity of the concept of a "spontaneous" speed, distinct from the maximum rate of performance, in the case of these cognitive tasks. (It will be recalled, however, that Fujita _did_ obtain a higher rate of performance in the MW than in the CW condition in the mirror-drawing experiments). Again, it appears that different tasks "behave" in different ways, and it would be interesting to enquire into the reasons for such differences.

A rather different argument might be put forward, to the effect that the addition-task may have produced a high degree of boredom, and that, when faded with a boring task, subjects work quickly, following the principle that, the more quickly one works, the shorter the time for which the task has to be performed. It might further be suggested that such an attitude would be particularly likely to occur in a situation (as was the case with Fujita's study) where a fixed amount of work must be done, and the time required to complete the task is recorded. It would be interesting to discover whether similar results might be obtained in situations where a fixed work-period is set, and the amount of work completed in that period is recorded. (In such a situation, however, it might happen that subjects would choose to do little or nothing). Clearly, a comparison of the two situations would provide useful information.

It has been implicit in both the above arguments that the rate of performance which Fujita observed in his CW condition was, in fact, close to the "maximal-rate" performance. It is equally plausible, however, to argue from the opposite direction; the MW rate was, in fact, the "congenial" rate of performance because, for some reason, subjects were not motivated in Fujita's experiment to perform at a rate higher than that which they found congenial, even when the instructions specifically required them to perform "as fast as possible".

Evidently, fundamental questions are raised by these findings, and much more information will be required before they can be answered. Why, for example, was there no significant difference between the MW and CW conditions with the addition-task, but a significant difference with the mirror-drawing task? Is the addition task unique in this respect, or are there other such activities where the two types of instructions lead to the same speed of performance? Or were the results due to some feature of Fujita's design?

Two months after the 200-trial practice session in addition (and, therefore, presumably at the same time as the "maximal way" performances in addition) three further tasks were performed by the subjects, using the same test-material. They were:

(i) Cancellation. "Each subject must cross out the figures printed on the paper of the Uchida-Kraepelin Test without fail".

(ii) Comparison of two figures. "Each subject must compare the two figures which are next to each other and write $<$ or $>$ between them."

(iii) Mental arithmetic. "Each subject must add the two figures which are next to each other and say the answer out loud."

All of these tasks were performed 5 times in the "congenial way" and 5 times in the "maximal way" and the order of presentation was not varied. We are not told whether the tasks were performed in blocks, so that five trials of cancellation were followed by five trials of comparison of figures, etc., or whether each task was pprformed once in turn, and then the procedure repeated four further times. Nor is it clear whether or not *exactly* the sameparts of the test were used for the different tasks.

The intercorrelations of the three new tasks and addition in the CW condition were significant at the 0.05 level of confidence, except for the coefficients relating addition and cancellation (0.362) and mental arithmetic and cancellation (0.442), though the latter was not far short of the value required for significance (0.444) at the 0.05 level with 18 degrees of freedom.

In the MW condition, two significant correlations were obtained: comparison-addition (0.696) and addition-mental arithmetic (0.902).

It appears, then, that Fujita has produced evidence of the clustering of "arithmetic tasks", though the correlation of cancellation with one of these tasks is difficult to interpret if by "arithmetic" is meant tasks which actually require *computation*, rather than tasks which merely involve numerical material. It is not possible to suggest any detailed hypotheses regarding this cancellation task, however, because insufficient detail is provided about it. It is not clear from the instructions which were quoted above, for example, whether subjects were required to cancel *all* numerals, or all occurrences of specific numerals.

Whether some form of "numerical" tempo exists, however, is a question which cannot be answered at present, because we do not have adequate information concerning the correlation of such tasks with other cognitive activities. Such information would reveal whether there were sub-groups of activities, or whether the concept of a unitary "cognitive speed" was more compatible with experimental findings. It will be recalled that, in Rimoldi's (1951) investigation, three of Thurstone's PMA subtests ("Number", "Reasoning" and "Verbal Meaning") were significantly loaded on the same factor, and this finding might be taken to support the concept of a unitary "cognitive tempo", but the loadings of some of the other tasks on the same factor would not appear to be compatible with that interpretation, and Rimoldi himself does not favour it. More research is required here.

The data regarding the comparison between the MW and the CW conditions are, regrettably, not statistically analysed by the author. Nor is it possible from the means and standard deviations which he reports to calculate the 't' test for correlated means which is required for such an analysis. The mean times to complete the various tasks in the MW condition <u>are</u> all shorter than those required in the CW condition, but they may not be significantly smaller. The data do <u>suggest</u> that it is meaningful when discussing these tasks to speak of a "congenial rate", as separate from the "maximal rate", but more adequately-analysed data are still required on this point.

A final phase of investigation is reported by Fujita on the relationship between writing-speeds obtained in "maximal way", "minimal way" and "congenial way" conditions. Two groups of 30 subjects each served in this experiment; one group consisted of students, aged 18-23 years, randomly selected from the subjects of the previous experiments, and the other comprised women, residents of a home for the elderly, aged from 60 to 85 years.

The writing-material consisted of three Japanese words, and these had to be written on squared paper, one character per square. The order of presentation of instruction-conditions was as follows: (a). congenial; (b). minimal; (c). Maximal; (d). minimal (e). congenial. This design enabled a measure of test-retest reliability to be taken with the congenial and minimal conditions. The resulting correlations were satisfactorily high, though the coefficient of 0.553 obtained with the shortest word and the "aged" group is rather low. The "aged" group was significantly slower than the students in both the "maximal" and "congenial" conditions ($p < 0.01$), and faster in the "minimal" condition (though this difference was not statistically significant). The results with the CW and "maximal" conditions may, of course, be the result of physical factors, such as arthritic joints, and it would also appear probable that the student-group would write more in their everyday lives.

In this instance, adequate statistical testing of the mean rates of performance under the three instruction-conditions is performed. The mean rate of performance under the CW condition was intermediate between the "maximal" and "minimal" rates, and was significantly different ($p < 0.01$) from both of them.

Finally, attention may be turned towards the correlations between the speeds observed in the three instruction-conditions. Before doing so, however, it must be pointed out that there is apparently a misprint in the relevant table which, for each of the words used in the experiment, gives two entries for the first CW trial, but no entry for the second in the case of the students, and no entries for the first CW trial for the "aged" group, but two entries for the second trial. The most likely interpretation of the table (P. 60) would appear to be that, for each of the words, the upper row contains the entries for the student group far the two CW trials, whilst the lower row contains the entries for the two CW trials of the "aged" group.

If this interpretation of the table is correct, then there is a marked difference in the correlational patterns produced by the two groups. For the "aged" sample, every one of the correlations relating the three instruction-conditions is significant at beyond the 0.01 level of confidence, whilst for the students, only three of the correlations achieve significance at the 0.05 level (actually, these coefficients are also significant beyond the 0.01 level). These are: for the 5-letter word, CW (both trials)-"maximal" , and, for the 8-letter word, CW (first trial only)-"maximal". Harrison (1941) also reported a significant correlation between "spontaneous" and "maximal" writing-speeds ($r = 0.46$), which result appears to be in accord with Fujita's findings.

Fujita does not discuss the possible reasons for this discrepancy in correlational pattern, but it is possible that the elderly subjects set themselves a more stringent criterion of neatness than did the students, and that this led to a greater similarity in speeds under the three conditions with the former group than with the latter sample. We have already reported the fact that the mean rates of performance in the three conditions were closer together in the "aged" group than in the student group, and this may be thought to corroborate this suggestion. However, at the time these data were quoted, it was also suggested that such physical factors as arthritic joints might have led to the results, and it is also quite plausible to suggest that such a factor may lead to a greater correlation between the rates of performance under the three conditions, as well as a greater similarity in the group means. Once again, insufficiency of empirical data lead to the position where we cannot formulate detailed hypotheses, but the striking inter-group differences which Fujita reports are certainly worthy of further investigation.

3. Hereditary Factors and "Spontaneous Speed".

Frischeisen-Kohler (1933) viewed the study of "personal tempo" as a means of investigating the inheritance of personality, the tempo being regarded as an "elementary psychic character". (P. 301). She offers a definition of "personal tempo".

> "With William Stern we understand by tempo the time or pace corresponding to a series of musical tones - that is, the time which will express the inner content of a melody to its best aesthetic advantage. This involves the fact that the tempo is not merely an objective melodic tempo, but is at the same time a subjective tempo of perception. This, however, applies not only to musical impressions, but to every psychic transaction unfolding itself in time. As apprehending individuals we know precisely - or at all events in most cases - whether we feel the speed of any happening to which we are paying attention - for example of a speech - to be suitable, natural, sympathetic. And, in the same way, when we actively intervene in any happening, when we speak, or walk, or perform any deliberately willed action, we choose, quite spontaneously, a congenial tempo for the transaction in question - a tempo which is natural to us. It is true that every action has a suitable tempo of its own - corresponding to the objective musical tempo - which I might call the individual tempo of the action; but in addition to this there is an individual tempo of the personality, the personal tempo, which is expressed more or less markedly in all our doings, in our acts of perception and our volitional processes. The personal tempo adheres to the individuality as a whole; the integral psyche, as

a unity, abhors one tempo as unsympathetic to it, or recognizes another as sympathetic." (P. 301-302).

The meaning of this passage is by no means unambigupus, but the following points appear to emerge from it:

(i). The organism has definite "preferences", both for the rates of unfolding of external events ("...we know precisely... whether we feel the speed of any external happening to which we are paying attention...to be suitable, natural, sympathetic.") and also for the rates of performance of its own activities ("...there is an individual tempo of the personality, the personal tempo, which is expressed more or less markedly in all our actions..."). "Preference" is definitely implied, since ...the integral psyche, as a unity, abhors one tempo as unsympathetic to it, or recognizes another as synpathetic.".

(ii). A unitary "speed-trait' appears to be suggested, since "The personal tempo adheres to the individuality as a whole; the integral psyche, as a unity, abhors...". (Harrison, 1941, also ascribes a belief in a unitary personal tempo to Frischeisen-Kohler). There is, however, a qualification to be made, since "It is true that every action has a suitable tempo of its own...". We are not told precisely how the "personal tempo" and the "individual tempo of the action" relate to one another, but the author may be implying that though (for example) the repetitive lifting of a heavy weight must necessarily be carried on more slowly than the lifting of a light weight, there is a correlation between the rates of performance of the two tasks, despite the physical constraints.

The language of this passage, then, is insufficiently precise for this to constitute a satisfactory definition, though this may in part be due to difficulties in translation. Nor does Frischeisen-Kohler offer any empirical evidence to corroborate her assertions. However, the passage may yet prove useful as a source of testable hypotheses for further investigation.

The report is unfortunately inadequate in respect of procedural and statistical details, but it is stated that two basic tasks, tapping and the selection of a preferred metronome-rate, were used in the investigation. Three variants of the tapping-task were used:

(i). "...tapping against or upon the table with a spontaneously chosen position of the hand or the fingers;"

(ii). "...tapping on the floor with the foot;"

(iii). "...tapping the edge of the table with the outstretched forefinger."

Subjects were apparently assigned to one only of these tapping-tasks. ("...as a rule, of course, only single experiments were made") and each subject was given instructions "...to tap in the tempo most agreeable to him or her, and this direction was always especially emphasized." A ten-second sampling-period was adopted.

In the metronome-task, subjects were presented with "several tempi" (no information is provided regarding the number of different tempi, the values of the tempi, or the scheme for determining the order of presentation) and were requested to indicate "in the case of each tempo whether it was too slow, too quick, or precisely agreeable". When (as apparently frequently happened) two different rates were described as "precisely agreeable", a forced choice was made between them and thus eventually a single preferred rate was arrived at.

A large sample (1000) of subjects took part in the study, ranging in age from 8 to 80 years, and from diverse social backgrounds. No information is provided regarding how much the subjects knew about the aims of the investigation.

Frischeisen-Kohler first presents data on the reliabilities and distributions of her measures. Reliability of the tapping-tasks was assessed by the performance of 4 trials, on different days, and with an interval of one or two days between the second and third trials in most cases. A similar procedure was adopted with the metronome-task, but with the addition of a second block of 4 trials after an interval of 2 months, corresponding with the summer vacation. Any effects of circadian rhythms in tempo were controlled for by administering the trials at "all different times of the day", and the author also points out that the weather, which she suggests has a considerable influence upon the mood of some people, was also variable.

Statistical analysis involved the calculation for each subject of an Index of Variability, obtained by expressing the mean variation in speed across the 4-trial blocks as a percentage of the mean rate as calculated from those 4 trials.

The author states that the variabilities thus obtained were extremely low, and reports a mean Index of Variability of the order of 4.5 for the three tapping-task variants, and of 3.76 and 2.64 for the first and second blocks respectively of the metronome-task. Though these data suggest that the variability was smaller with the metronome than with the tapping-tasks, it is not possible to calculate the significance of this difference from the published data.

An Index of Variability was also calculated for the combined tapping and metronome tempi, and is reported as 12.41. This would appear to indicate a reasonably close correspondence between the rates chosen in the two tasks by each subject and, therefore, a correlation between the measures. This statement requires some modification, however, because the chosen metronome-rates were, on the whole, a little slower than the tapping-rates. The mean rates were:

(i). Tapping - males: 23.06; females: 23.78

(ii). Metronome - males: 18.7; females: 19.6 expressed in beats per 10 seconds.

Neither this difference between the tasks, nor the sex-difference revealed by these data, was statistically tested at the time of preparation of the report. Nor had any statistical treatment of the data been directed at the examination of age-differences in tempo.

As far as inter-individual variability is concerned, it is stated that approximately the same ranges in tempo were obtained in both sexes, and at all ages. Nor were different ranges observed in different social classes.

As has been noted, the major aim of Frischeisen-Kohler's study was to investigate heredity and "personal tempo". To this end, 118 pairs of twins, 53 monozygotic (MZ), 49 dizygotic (DZ) twins of the same sex, and 16 pairs of opposite sex (PT) were studied. The statistical treatment involved the calculation for each pair of twins of the Index of Variability, as a measure of the "likeness" of the members of the pair. Examining the mean Indices of the various groups, the author concludes that:

(i). MZ twins are more alike than DZ twins;

(ii). DZ twins are no more alike than ordinary siblings (of different ages);

(iii). both DZ twins and siblings are more alike than unrelated individuals.

Frischeisen-Kohler discounts the idea that environmental factors may have led to such a pattern of results on the grounds of her previous negative findings with respect to the effects of age, sex and social background upon tempo. This argument is rather weak, however, because there are many environmental factors which "cut across" these variables, and which might operate to produce such data. It might, for example, be hypothesised that people develop similar tempi in proportion to the amount of time which they spend in close contact with one another, and that MZ twins spend more of their time together than do DZ twins or siblings. It would certainly be expected that these latter groups would spend more of their time together than unrelated individuals. In short, this twin-study suffers from the serious defect that no separated twins were included in the sample (at least, we are not informed that any such twins were studied). On these grounds, therefore, Frischeisen-Kohler's data are not adequate to the establishment of heredity as a major determinant of "personal tempo".

Frischeisen-Kohler finally presents results which demonstrate a considerable similarity between the tempi of parents and their children. This is once more interpreted in terms of genetic transmission, environmental determination being discounted on the grounds of the results from the twin-study. It is clear, however, that such results are also compatible with the hypothesis that people develop similar tempi as a result of close personal contact.

In summary, Frischeisen-Kohler's report is rather inadequate; procedural and statistical details are lacking, and it is not clear whether the subjects studied in the phase of the investigation concerned with the inheritance of tempo were actually included in the 1000 on whom reliability and score-distribution data were based, or whether these constituted new samples. It is also clear that the twin-study suffers from a serious shortcoming in design.

Though only a small sample of behaviour-measures was taken in this experiment, being limited to two tasks, the paper nevertheless provides valuable normative data on the rates of performance of these tasks, and the reliabilities were highly satisfactory. In addition, a correlation between the two tasks appears to be indicated.

4. Geographical Correlates of "Spontaneous Speed".

One not infrequently hears such statements as that "the tempo of life is faster in the city than in the country", so that the possibility of geographical correlates of "spontaneous speed" would appear to be an interesting topic for research. Researchers both in the West and in Japan have been attracted by this problem.

The first study to be considered under this heading was reported by Lowin et al (1971), and is exceptional in that it was not a piece of laboratory research (such as those which have been reviewed so far), but an attempt at a form of ethological observation of everyday situations.

The comparison which was made by Lowin et al was between metropolitan city areas and small towns of less than 8,000 inhabitants. The subjects of the observations were not instructed to perform set tasks, but in most cases one of the investigators masqueraded as an ordinary citizen. The following activities were studied:

(1) Post office: the time taken by a clerk to fill in a standard order for the observer.

(2) Filling station: the waiting time between the arrival of a car at the pump and the attendant's enquiry of the driver.

(3) Currency exchange: the time taken by the bank clerk to change 10 5-dollar bills to 50 1's, or vice versa.

(4) Walking speed: the time for any person leaving a bank to walk 100 feet down the street in either direction.

(5) Cigarette purchase: the time elapsed while a standard request for a standard brand and size was answered.

The results indicated that, in accordance with the popular stereotype, the tempo was higher in the city areas than in the country.

This was generally true for all of the measures except the currency exchange, which was often done more slowly in the city than the country. The authors suggest that one factor which may slow down this type of transaction in the city is that the bank clerks there tend to be less trusting than their rural counterparts, and are therefore more likely to count incoming money several times.

Despite this one exception, however, these authors have clearly demonstrated a difference in the tempo of everyday activities in town and country. Equally as interesting as their findings is their method of investigation. It would seem intuitively that, whatever precautions a laboratory investigator might take in order to prevent self-consciousness (or an awareness of being observed) in subjects, he would be unlikely to be as completely successful as an investigator employing the methods of Lowin et al. Though such methods might not be appropriate to all research-projects in the area, this is certainly an interesting approach, and one which would merit consideration in the planning stages of future investigations.

The Japanese study of geographical factors was reported by Nagaski (1972). He performed a laboratory study of the development of regional differences in tempo in two areas: Metropolitan Tokyo and Akita Prefecture. No details are provided, however, regarding geographical, economic or social differences between these areas.

The two tempo measures which were used were forefinger-tapping ("Each of the subjects was required to make finger-tapping for 10 seconds at the most comfortable speed by the method of free expression, after the tapping at the greatest and the slowest speed") and a metronome-task in which "Each of the subjects heard tapping sound of a universal metronome making various kinds of speed according to the method of adjustment by experimenters, and chose the most comfortable speed among them by himself".

The first finding in this experiment is the developmental trend that, in both regions, the primary school group exhibited the fastest "mental tempo", both in the tapping and in the metronome measure. Though it is not entirely clear from Nagasaki's description whether the "fastest" and "slowest" rates were considered (see the instructions quoted above), or whether this finding relates only to the "most comfortable" tempo, the latter would appear to be the more plausible interpretation of Nagasaki's description.

't' tests revealed that, in Metropolitan Tokyo, the primary schoolchildren were faster than junior- or senior-high school students and college students ($p < .01$) whilst these three older groups did not differ significantly from one another. (These results are for the tapping measure). In the metronome task, primary schoolchildren were significantly faster than college students, but none of the remaining comparisons was statistically significant.

In Akita Prefecture, exactly the same pattern of results, with the same levels of significance, were obtained in the case of the tapping-task. With the metronome measure, however, the difference between the primary schoolchildren and college students only reached the 0.05 level of confidence, but, in addition, the former were also significantly faster than senior-high-school students ($p < 0.01$).

In both regions, the group data indicate that the primary schoolchildren tapped at a rate which was significantly faster than their chosen metronome-rate, whilst, in Metropolitan Tokyo, the older groups chose tapping-rates and metronome-rates which were not significantly different. In Akita Prefecture, the mean tapping-rate of the three older groups was slower than the mean chosen metronome-rate.

Nagasaki concludes that, in the younger children, "maximal way" and "congenial way" are not different, and that the process of maturation of the "mental tempo" consists in the differentiation of these two modes of operation. This suggestion should prove to be a fruitful source of further research.

The Tokyo subjects were also significantly faster in tempo than the Akita subjects (except for the tapping-rates of the primary schoolchildren, where the difference was not significant). Without details of the differences between the two areas, this finding cannot be readily interpreted, however. It _may_ indicate that the tempo is higher in a large city, but what variables associated with large cities promote this difference is not clear. In introducing his study, Nagasaki notes findings by Mishima, who concluded that the density of the population led to the difference, but we have no details of these investigations, and it is not possible, therefore to comment upon them.

5. Sex Differences.

Comparatively little evidence is available in the literature regarding sex-differences in tempo. Some of the classic studies (eg., Allport & Vernon, 1933; Rimoldi, 1951) have only used male subjects. Though some of the writers whose investigations employed both males and females have compared the tempi of the sexes in passing, the only investigator in the field whose major interest was in sex-differences appears to have been Hoffman (1969).

Hoffman's task was finger-tapping, in which he employed a trial-length of 10 seconds (the period during which a signal light was illuminated). The subject was given the instructions: "tap in whatever manner feels most comfortable to you at the moment". 119 males and 201 females served in the investigation. The mean for males was 3.76 taps per second, and that for females was 2.98 taps per second. This difference was significant at the .01 level of confidence.

Subjects in the principal part of Harrison's (1941) study were all males, but the second part (concerning the effects of repetition upon the generality of tempo) employed both males and females. Comparing the results of the 20 males with those of the 20 females, Harrison obtained non-significant differences in all measures (including finger-tapping) except walking. Smoll (1975a) noted no sex-differences with his arm-swinging task. Frischeisen-Köhler (1933) did not perform a statistical test on the slight sex-differences she obtained with tapping and preferred metronome-rates, but these were, in any case, in the direction opposite to that reported by Hoffman.

Hoffman's study appears to stand alone, then, in reporting sex-differences in tempo (except for Harrison's result with walking, which the author attributed to differences in footwear).Though Hoffman is not prepared to rule out the possibility of biological factors, he suggests that his obtained difference may reflect "social conditioning".

6. "Spontaneous Speed" and Time-judgments.

Since the earliest days of scientific Psychology, investigations have been performed into the estimation of intervals of time, and the literature is now vast (see Doob, 1971, for an extensive review). However, comparatively little work has been performed in order to ascertain whether there might be a relationship between the perceived rate of passage of time and the rate of Performance of everyday activities. For example, does someone who perceives time to be passing relatively quickly also perform his everyday activities quickly?

Gooddy (1958) provided a discussion of the "time sense", in which he suggested that organisms contained many "clock-like" mechanisms which might subserve temporal judgments. In addition to numerous rhythmic physiological processes such as the pulse and the alpha rhythm, he suggested that locomotor activity might provide one such clock:

"The positions of the limb points are the hands of the locomotor clock upon the face of the earth" (P. 1142).

Though this is not a precise formulation of a hypothesis, it may be that Gooddy wished to echo the writing of Wundt, who suggested in the "Elements of Physiological Psychology" that there might be a relationship between the "indifference interval" and the time taken for the swing of one leg "when we are walking quickly". This suggestion is further elaborated by Fraisse (1963), who argues that:

"Walking, heartbeats, movements effected at a spontaneous tempo and perceptions all follow on at intervals of about 0.70 second, which we consider to be the optimum interval for the functioning of the nervous centres because it is the most economical" (P. 128).

Kastenbaum (1959) criticised Gooddy's paper on the grounds that it neglects the wealth of psychological research, being written from the standpoint of a neurologist. Kastenbaum provided a brief report of some data which he obtained in an attempt to gather empirical evidence of a relationship between "behavioural pace" and the perceived rate of passage of time. In this study, 136 high school students served as subjects, and they were given three tasks. The first involved making verbal estimates of three short intervals of time. In the second, subjects made estimates of the rates at which they would perform certain common activities, such as writing a letter to a friend. In the third test, more general judgments were elicited concerning the rate at which time seemed to be passing and the rate at which they felt events to be occurring in their everyday lives.

Kastenbaum did not find any relationship between these three tests. There is certainly no evidence here, then, of a relationship between everyday tempo and the perceived rate of passage of time. However, it must be borne in mind that this study involved only self-ratings of everyday speed, and not objective measures of subjects' behavioural tempo. Harrison (1941) reported that such ratings achieved better than chance accuracy, but that they were not perfect. It is interesting, therefore, to consider further studies which have involved time-judgments and tempo measures. Though these do not all permit a correlation of tempi with the perceived rate of passage of time, it is nevertheless interesting to consider them.

Craik and Sarbin (1963) reported a study in which "personal tempo", as such, was not the primary focus of attention, but in which tempo-measures were made in the course of testing a model of time-judgments. It will not be necessary here to describe the model in detail, but suffice it to say that a prediction derived from it suggested that exposing subjects to a clock whose rate had been covertly altered would lead to changes in tempo.

The clock in question had been specially modified so that it could be made to run either at the correct rate, or at two erroneous rates: twice the correct speed, or half the correct speed. The instrument was made to look exactly like the wall-clocks which were provided throughout the building in which the research was performed, and subjects were not informed of the deception.

The prediction derived from the model was that, faced with an apparent contradiction between the rate of passage of time and the rate of sensory input, the subject would adjust that aspect of the rate of sensory input which was directly under his own control - his own rate of movement, or "personal tempo". Thus, when the clock-rate was covertly increased, the subject would increase his tempo and when it was covertly decreased, he would decrease his tempo.

Two groups of subjects, one of 14 naval ratings, and the other of 15 ratings, were employed in this experiment. The former group was exposed to the clock running at twice its correct rate, and will henceforward be termed the "fast" group. The latter will be referred to as the "slow" group, since the clock was made to run at half the correct rate in their case.

In the first part of the experiment (which will be referred to as the "control" part), both groups of subjects were treated alike, and performed five tasks. After each task, the subject was required to estimate the length of time for which he had been working. Feedback was then provided by asking the subject to check his estimate against the wall-clock. In the second part of the experiment (which may be termed the "treatment" part), the clock-rate was covertly adjusted, and a further nine tasks were performed. Subjects were again required to make estimates and to check these estimates with the clock (which was now providing erroneous feedback, of course).

The results of the experiment demonstrated firstly that subjects did adapt to the new temporal system: those in the "fast" group made significantly larger estimates in the treatment trials than in the control trials, whilst the "slow" group made smaller estimates.

Though a variety of tasks was performed, those which were analysed under the rubric of "personal tempo" numbered only two, and consisted of "tapping" and "dotting". In the former "S's made continual rows of dashes with their pencils at their most comfortable and agreeable pace". In the latter task, "S's made three pencil dots in each of many rows of circles. They were required to count to themselves as they dotted, 123, 123, 123, and to work as fast as they could".

The results demonstrated that both the "fast" and the "slow" groups were significantly faster in the treatment trials than they had been in the control trials. Examining the difference in the mean rates of the two groups, however, a significant effect of the experimental manipulation was found, since the "fast" group increased their tempi to a greater extent than the "slow" group. This was true both for the "spontaneous rate" tapping ($p < .05$) and for the "maximum rate" dotting ($p < .01$).

The implications of these results for individual differences in tempo are not clear. It would certainly not follow from Craik and Sarbin's model that a correlation would exist between time-judgments and tempo, because the authors stress the role of learning which takes place when temporal information is obtained under a variety of conditions of sensory input. However, their conceptualisation of tempo as an aspect of the sensory environment which is under the organism's control is illuminating, and suggests a further line of enquiry. Welford (1968), in discussing the personality dimension of extraversion/introversion, suggests that "People whose normal level of arousal is relatively high will be sensitive and need to keep their levels of external stimulation down if they are not to become over-aroused" (P. 320). The question may now be put as to whether one aspect of external stimulation which such people "keep down" is the one which is under their own control - their rate of movement.

An obvious difficulty with this argument is the fact that previous researchers have not found evidence of the existence of a unitary "personal tempo". However, it is possible that one of the clusters of intercorrelated measures which have been reported in the literature might act in this capacity. Further research into this issue would be of great interest.

A curious feature of Craik and Sarbin's design is that the trials upon which the comparisons between control and treatment conditions were based were not of the same length. In the control condition, for example, the dotting task was performed for 90 seconds, whilst the trial in the treatment condition was 60 seconds in duration. Should the rate of dotting decrease with time (for example, with the onset of fatigue), this would result in a lower rate of performance over the longer trial than over the shorter trial. This feature of the design may, then, account for the fact that both groups of subjects performed significantly more quickly in the treatment trials than in the control trials. A similar argument applies to the tapping task, in which the control trial was 180 seconds in length, whilst the treatment trial was only 120 seconds in length. It is true that Rimoldi (1951) observed no within-trial variations in tempo, but his trials were apparently only 30 seconds in duration, so that his findings do not necessarily refute the above argument.

Another study involving both "personal tempo" and time judgments was reported by Denner et al (1963). In a preliminary phase of their investigation, 84 subjects were requested to tap on a Morse key for 60 seconds. Each subject was instructed to tap "...at a rate which seemed comfortable to him". (P. 288). From these 84 subjects, the 18 whose tapping-rates fell in the middle of the obtained range were selected for the major part of the experiment.

The subject's basic task in the experiment was to estimate an interval of 70 seconds by the method of Reproduction. During both the standard and the variable intervals, the subject was required to tap in synchrony with a flashing light, which was driven by an electronic metronome. Three different metronome-rates were used:

(i). the subject's own "preferred" tapping-rate;

(ii). a rate 1.3 taps per second above the subject's own "preferred" speed;

(iii). a rate 1.3 taps per second below the subject's own "preferred" speed.

All nine permutations of tapping-rate (slow-slow, slow-"preferred" etc.) were presented to each subject, according to a 9 x 9 Latin Square design. One male and one female subject were assigned to each row of the square, so that there were 18 subjects in all.

Subjects were successful in adopting the imposed rates, both in the sense that the observed rates of tapping with the three different rates were significantly different from one another, and in the sense that the observed rates were not significantly different from the rates of operation of the metronome, with the one exception of the fast rate presented during the variable interval, where the tapping-rates fell below the rate at which the lamp was flashing.

The major dependent variable in this study was the length of the reproductive estimate produced by the subject after each presentation of the standard interval. Analysis of Variance of these data revealed that the effect of the imposed tapping-rate, both in the standard interval, and in the variable interval, was significant well beyond the 0.01 level of confidence. Collapsing across conditions during the variable interval, there was a strong tendency for reproductive estimates to be smaller the faster the rate of tapping which had prevailed during the standard interval. Collapsing across conditions during the standard interval, there was a strong tendency for the reproductions to increase with increasing tapping-rate during the variable interval. The interaction between standard-interval conditions and variable-interval conditions was also significant at the 0.01 level of confidence.

The most economical way of describing the interpretation which the authors make of these data is by use of the "internal clock" analogy. We may then simply say that the faster the imposed tapping-rate, the slower the internal clock. Thus, when a fast rate is presented during the standard interval, the "internal clock" "registers" a shorter elapsed time than it would have done if a slow rate had been imposed, and hence the reproduction of that interval will be relatively short. When a fast rate is presented during the variable interval, the "internal clock" takes longer (as measured by the objective clock) to reach the appropriate "reading", and so the reproduction will be relatively long. The interaction of the two variables is also easily accounted for in this way.

As with Craik & Sarbin's (1963) results, we are not entitled by these data to formulate conclusions regarding a relationship between individual differences in tapping-tempo and individual differences in time-judgments; it would not be legitimate, for example, to argue that people who habitually tap quickly are people who have a "chronically slow internal clock", and who would habitually produce comparatively small verbal estimates and comparatively long productive estimates in time-estimation tasks. Such a relationship could only be revealed by intercorrelational data, and these would not have been appropriate in the study at present under consideration because individual differences in "internal clocks" are not revealed by the method of Reproduction, since it is the same "clock" which measures the standard and the variable intervals.

Finally, we may note that these findings have an important bearing upon theories of time-estimation. They would appear, for example, to be contrary to Ornstein's (1969) hypothesis, since a faster rate of tapping should contain more information than a slower one, and this would lead to a relationship in the opposite direction to that which Denner et al have reported.

Mochizuki (1968) reports a series of investigations into time-estimation and the "mental tempo". In these experiments, subjects were required to compare two time-intervals (which were, in fact, equal). The intervals were "filled" with the ticking of a metronome, and three different metronome-rates were used: the "preferred rate" ("mental tempo"), as determined by the method of adjustment, a faster rate ("mental tempo" x 1.6), and a slower rate ("mental tempo" x 0.4). Time-intervals of 5, 10, 15, 30 and 60 seconds were presented. Two methods of presentation of the intervals were employed: one in which a 15-second pause was interposed between them, and one in which there was no pause.

The results showed that the "mental tempo" intervals were judged to be longer than either the fast or the slower tempo intervals, except for the 15-second "no pause" condition, where the tendency did not reach statistical significance. When both intervals were filled with the <u>same</u> tempo, the second was generally judged to be longer than the first. When the faster and the slower tempi were compared, the faster was judged to be longer than the slower.

Ornstein (1969) proposed a model of time-estimation which suggested that the "storage space" occupied by the events which took place in a given interval determined the apparent duration of that interval. The findings with respect to the comparison between the faster and the slower tempo, and between the first and the second interval when both were filled with the same metronome-rate may be said to be compatible with this model. A faster rate consists of a greater number of events (beats) and would be expected, therefore, to occupy more storage space. A more recent period will be better recalled, and will require more storage space than a less recent one, some of the "contents" of which will have dropped out of store. However, the fact that the interval filled with the "mental tempo" was generally judged longer than the interval filled with the faster rate appears at first sight to be difficult to reconcile with Ornstein's hypothesis. It might be argued that the "mental tempo" is better recalled than other rates, and, therefore, that it occupies more storage space. In fact, this suggestion will be discussed in a later chapter (Chapter 6), and an experiment will be described which set out to compare the accuracy of recall of the "mental tempo" (to use Mochizuki's term) and other rates.

A paper in which a direct comparison of individual differences in time-estimation and in tempo of performance is reported is that of Newman (1972). Her investigation was logically similar to that of Denner et al (1963) in that the effect upon time-estimation of various imposed rates of performance (above and below the "preferred" rate) was studied. In Newman's study, however, the task was not key-tapping, but walking, and the method of Production was used to elicit time-judgments.

Newman's prediction was that increasing the rate of walking would "increase the rate of the internal clock", so leading to shorter productive estimates, and that the opposite effect would occur when the rate of walking was decreased. It will be noted that this predicted effect is in the opposite direction to that which Denner et al found to be operative in the case of finger-tapping.

Newman's procedure involved first the calculation of a "preferred" tempo of walking for each of her 52 male subjects. This was achieved by requiring subjects to walk in their "...most comfortable, natural manner..." around a track. The variable of principal interest was not the speed (in miles per hour), but the cadence (in steps per second), though the speed was also calculated for the purposes of later determining the required imposed rates of walking.

Rates above and below the "preferred" tempo were imposed by means of a motor-driven treadmill. Subjects were first practised on this apparatus until their cadence levelled off to the previously-determined "preferred" rate, and then the experimental trials began, in which subjects made productive estimates of 40 seconds whilst walking on the treadmill at one of three tempi: "preferred", 30% below "preferred", and 30% above "preferred". Three estimates were made under each tempo-condition, and the conditions were presented in different orders to the different subjects.

The data were subjected to Analysis of Variance, and the resulting F-ratio was less than unity, indicating no effect of tempo upon productive time-estimates. However, Newman concluded on the basis of comments made by her subjects that any effect of tempo on time-judgments may have been masked by conscious compensation. One case is cited, for example, of a subject who felt that time was passing more quickly when his rate of walking was increased, but who consciously compensated for this effect when making his judgments, in order to produce accurate estimates.

Of particular interest in the present context is the fact that Newman reports the correlations between time-estimates and walking-rates. For the fast, "preferred" and slow conditions respectively, the obtained Product-moment correlations were: -0.29 ($p < 0.05$), -0.23 ($p < 0.10$) and -0.06 (N.S.), indicating a very slight tendency for subjects with faster walking-tempi to produce shorter productive estimates (to have "faster internal clocks"). Though these correlations are small, reflecting only a small proportion of common variance, they are nevertheless interesting, and suggest the value of further research into this question. It would be particularly interesting, for example, to investigate the correlation between productive time-estimates and "preferred" tapping-rates, especially in view of the fact that Newman predicted a relationship in the opposite direction to that which Denner et al found.

In a somewhat different vein is the investigation of Cohen, Cooper and Ono (1963) into the "tau-movement effect". In this study, subjects made estimates of the distances covered during two parts of a short journey. In some of the journeys the subject walked during the first part and ran during the second. In others, the subject ran in the first part and walked in the second. A number of "control" journeys was also included, in which the subject either walked in both parts or ran in both parts.

Subjects' estimates of the distances covered in the two parts were elicited by two methods, which the authors describe as "estimation" and "production" by analogy with studies of the judgment of duration. In fact, the second method corresponds with what Doob (1971) designates "reproduction". Since Doob's convention has been adopted at other points in this work, Cohen et al's second method will henceforward be referred to as "reproduction".

In the estimation trials, both parts of the journey were of objectively equal length, and the subject made verbal estimates of the two distances after completing the journey. In the reproduction trials, the experimenter designated the length of the first part of the journey, and the subject's task was then to walk or run until he estimated that he had covered an equal distance.

When the mean estimates were examined, it appeared that the tau effect had occurred: that portion of the journey was considered to be of a greater distance which had taken the longer time. However, the authors suggest that this general statement obfuscates the results. Though the overall tendency was towards the tau effect, there was nevertheless a considerable number of subjects who displayed the <u>opposite</u> (anti-tau) effect, whereby that distance was judged greater which had been traversed at a greater speed (and had, therefore, taken the shorter time). In fact, these effects were about equally frequent in the estimation trials, but the tau effect was stronger when it did occur.

The authors postulate two "types" in accounting for their results. Those subjects who displayed the tau effect, it is argued, are primarily sensitive to the passage of time, and are "scarcely aware of speed as such" (P. 390). These are referred to as the "Tortoise" type. It is further suggested that subjects who displayed the anti-tau effect are primarily sensitive to speed, rather than to the passage of time, and these are referred to as the "Hare" type.

The tantalizing suggestion is then made that:

"There are certainly great individual variations in sensitivity to the passage of time, which may be related to metabolism and tempo...." (P. 392).

The authors themselves do not elaborate on this suggestion, but some of the data which they report (Table 2, P. 391) may provide the basis for a hypothesis. In the estimation trials, subjects not only estimated the distances they had travelled in the two parts of the journey, but also the times taken in completing the parts. these data indicate that the "Tortoises" made estimates which were approximately four times the objective duration in both parts of the journey. The "Hares", on the other hand, made estimates which were approximately twice the objective time in the part of the journey when they were running, but only slightly greater than the objective time when they were walking. Overall, then, the verbal estimates of the "Tortoises" were considerably greater than those of the "Hares".

From a "common sense" point of view, it might be suggested that someone who made relatively large verbal estimates would, in his everyday life, frequently find himself in the situation where (for example) he thought that an hour had elapsed since a particular event but, on consulting a clock, discovered that some shorter interval (for example, half an hour) had, in fact, elapsed. It might thus be conjectured that such a person would frequently remark (if only to himself) that "time passes slowly". Such reasoning underlies Doob's (1971) suggestion that "...if time really does seem to pass more rapidly with increasing age and if temporal tendencies generalize, then it must follow that youth will tend to overestimate verbally, and the aged to underestimate verbally, intervals of any duration" (P. 241).

This analysis, then, would suggest that the Tortoises would perceive time to be passing relatively slowly. It might further be suggested that, if a person perceives time to be passing relatively slowly, then it will appear to him that there is no need to hurry his everyday affairs. In this way, it might be predicted that subjects displaying the tau effect would adopt a relatively slow tempo in some of their everyday activities (since previous research has shown that there is not a unitary "personal tempo", the qualification, "some of" is unfortunately necessary). It appears, then, that the Tortoises are appropriately named, since they have low tempi.

The results of Newman (1972) must be borne in mind, however. She, it will be recalled, obtained a very slight tendency for subjects with a high gait-tempo to make comparatively short productive estimates. Unfortunately, this relationship is in the opposite direction to that suggested by the above analysis. Short productive estimates are associated with long verbal estimates (Siegman, 1962), so the above analysis would suggest that those subjects with low gait-tempo should make short productive estimates, and not the rapid walkers. Perhaps Newman's result should be ignored, since the correlation which she reported was only significant at the .10 level, which most researchers would consider to be insufficient to warrant rejection of the Null hypothesis. This course of action might be appropriate, but for the results obtained in another programme of research.

Knapp and Garbutt (1958) developed a method of directly tapping an individual's general notion as to the rate of passage of time. They devised a "Time Metaphor Test" consisting of "...25 phrases which might be employed by a poet or a writer to symbolize his sense of time". The phrases differed in the rapidity of movement which they suggested, and ranged from "swift" metaphors such as "a galloping horseman" to "static" ones, such as "a quiet, motionless ocean". The subject's task was to rate the metaphors according to how well they described his own conception of time. Knapp and Garbutt demonstrated a tendency for subjects who had a high need for achievement (as assessed from TAT protocols) to prefer the swift rather than the static metaphors. Thus, it appears that subjects high in the need for achievement perceive time to be passing rapidly.

The foregoing analysis encounters difficulties when Knapp and Garbutt's finding is considered together with findings reported by the same research-group. Knapp (1958) had demonstrated what Doob (1971, P. 229) justifiably refers to as an "incredible relation" between the need for achievement (again assessed from TAT responses) and preferences for Scottish Tartans. Persons high in the need for achievement tended to prefer blue-green (as opposed to red-yellow) designs.

This last finding permitted Green and Knapp (1959) to employ the "Tartan Test" as a measure of the need for achievement. One of the tasks in this study required subjects to signal when they thought that the level of water in a tube had reached a certain point. The rising water level was observed for a short period, until it was obscured by a mask, which prevented the subject from viewing the water level in the final stages of its journey to the designated level. Subjects choosing the blue-green tartans (ie, those high in achievement motivation) tended to "anticipate" the arrival of the water level to a greater extent than those who chose the red-yellow designs. Doob (1971, P. 266) interprets the greater anticipation of the "high-need" subjects as representing "a greater awareness of the rapid passage of time".

It might be suggested that Green and Knapp's "Tube Test" is a version of the Method of Production. When the water level disappeared behind the mask, the subject had to produce an interval equal to the time which the level would require to reach the designated mark. Considering again the results of Knapp and Garbutt (1958), then, it would appear that there is some association between a tendency towards comparatively short productive estimates and preference for swift metaphors. Subjects who perceive time to be passing rapidly, then, make short productive estimates. Yet short productive estimates are associated with long verbal estimates. It may be, then, that Tortoises (who make long verbal estimates) perceive time to be passing rapidly. If a person perceives time to be passing rapidly, he may conclude that it is necessary to conduct his affairs at a high tempo, in order that something might be accomplished before time "runs out" for him. We may thus be forced to the almost unutterable conclusion that the Tortoises might have a higher tempo than the Hares.

It is obvious that this argument has been rather tortuous. There are too many "missing links" in the data, and only further research will provide them. What has been attempted, however, is to show that there are good grounds for suggesting that there might be a relationship between tempo, the perceived rate of passage of time and the direction of the tau effect. It was further suggested that there are grounds for suggesting that the relationship between tempo and the tau effect might be in either direction - that the Tortoises might be either fast or slow. Only further research will provide the answer to this fascinating question.

7. "Spontaneous Speed" as a Personality Trait

To the extent that an individual's "spontaneous rate" of performance of a given task is a reliable characteristic, it may be regarded as a simple personality trait or "elementary psychic character", to use Frischeisen-Kohler's (1933) terminology. This assumption also appears to underlie the quotation from Rimoldi (1951) with which the present work began. It is perhaps surprising, therefore, that comparatively little work has been done relating the study of "personal tempo" to the study of personality in general.

One such study was reported by Baxter (1927). Noting the longevity of the doctrine of "four humours", she set out to investigate the possibility that there might be four "types" of individuals: slow-weak, slow-strong, quick-weak and quick-strong. The speed measures which she used were derived from tasks performed under a variety of instructional sets "...ranging from tapping and the learning of nonsense syllables in as few trials as possible, where speed was the only element of interest, through experiments such as the cancellation of words, where the effort was to be divided between speed and accuracy, to those where the rate was purely voluntary" (P. 61).

The three tasks which were performed at a "purely voluntary" rate and which, therefore, are the most relevant to the present work, were: reading aloud, a task involving the judging of weights, and one in which the subject was required to draw loops on paper. This last task was also performed at the "maximum rate".

Baxter's results did not support the hypothesis of four "types": though the distribution of her various "strength" scores appeared to be bimodal, this was not the case with the distribution of speed scores. Though she suggests that there is some evidence of a "common rate factor" in the tasks performed at a "purely voluntary" tempo, the intercorrelations are nevertheless very low (though they are all positive). However, a correlation of 0.715 ($p < .01$) is reported between "speeded" and "voluntary" rates of performance of the loop-drawing task.

One interesting feature of Baxter's study is that three physiological measures - pulse-rate, blood pressure and temperature - were also taken. An interesting relationship was observed between pulse-rate and the speed measures since, though the overall correlation was only 0.098, there was a tendency for subjects who were at the upper extreme in the speed distribution to have pulse-rates which were below the group average, whilst those at the lower end of the speed distribution tended to have pulse-rates which were above average for the group. Baxter suggests that this tendency might be accounted for if it is supposed that a relatively high pulse-rate is indicative of an excited emotional state, which might have interfered with performance of the tasks. Since many of the speed measures were obtained from tasks in which there was at least some pressure in the direction of rapid performance, this suggestion appears plausible.

A recent study by McCutcheon (1974) was addressed directly to the question of the relationship between a measure of "spontaneous rate" and a standardised personality inventory. The 16 PF inventory (Cattell et al, 1950) was administered to two groups of students ($N = 21$ and $N = 49$), who also copied prose passages "at their normal rate of speed". None of the correlations between tempo and the 16 PF scales attained significance at the .01 level, but the correlation between writing-speed and the scale "humble-assertive" was significant at the .05 level for the smaller group ($r = -0.51$).

Since this was the only coefficient of the 32 to reach statistical significance, it may very plausibly be regarded as due to chance factors.

Combining the data from the two groups (which had been treated differently only in that the material to be copied was presented by projector to the smaller group but on duplicated sheets to the larger) none of the correlations was statistically significant, and the largest obtained value was -0.27.

One aspect of this experiment which gives rise to concern is that the subjects knew that their writing-speed was being recorded. Moreover, after the first minute of performance the experimenter called out the elapsed time at five-second intervals, so that subjects could record their own writing speeds. The author does point out that this was done in a soft tone of voice in order to avoid giving rise to a sense of urgency, but it is certainly possible that such a procedure might have produced some pressure to work quickly. Another feature of the experiment which could have produced such pressure is the group-testing situation. This method was presumably adopted for convenience, but it might be that a slower writer, seeing that a large proportion of the group had already completed the task, may feel pressured to accelerate towards the end of the writing. Actually, the effects of group working on the tempo of individuals might provide an interesting topic for future research, but, in the absence of information regarding possible group effects, McCutcheon's experimental design must be regarded with some suspicion. It would be interesting to perform further work on this theme, using individual testing. A wider variety of personality tests might also be considered.

8. Physiological Factors and "Spontaneous Tempo"

Rimoldi (1951) suggested that it would be interesting to investigate the possibility of relationships between behavioural tempi and physiological factors, but it is true to this day that comparatively little work has been done on this topic. One such investigation was reported by Rashkis and Rashkis (1962), in which the effects of physiological and physical environmental factors upon "preferred" metronome-rate were observed over a period of 20 days, using only the two authors as subjects.

For one of the subjects, there was a significant correlation ($p < .05$) between tempo and both body temperature and barometric pressure. For the other subject, tempo and body temperature were uncorrelated, but tempo was again correlated ($p < .01$) with barometric pressure. Neither subject displayed a relationship between tempo and atmospheric temperature or relative humidity. The authors conclude that their results are consistent with Hoagland's (1933) "chemical clock" model of time-estimation.

The very small sample of subjects used in this study must give rise to concern. Moreover, no information is provided regarding the "blindness" of the scoring. Was the subject aware of the barometric pressure, etc., when he was selecting the metronome-rate? If this were the case, this would cast grave doubts on the validity of the results.

This investigation, then, can only be regarded as suggestive. In view of the positive results, it would certainly be of interest to pursue this topic further, but it would be necessary to use more subjects and blind scoring techniques.

Another study which may appropriately be considered under the present heading was reported by Mochizuki (1969). Mochizuki's interest was in the relationship between the "mental tempo" and body-type. Two "spontaneous rate" tasks (tapping and counting) and one "maximum rate" task (which called for subjects to "step side to side") were investigated. An Index of body type was calculated, such that:

$$\text{Index} = \frac{\text{weight (kg.)}}{\text{height (metres)}} \times 100$$

A correlation of -0.253 was obtained between the Index and the rate of counting. With 247 subjects, this correlation is significant ($p < .01$).

Three "types" were then defined: an obese type, a medium type and a thin type. In the "spontaneous rate" tasks, the obese subjects were slowest, but the medium and thin subjects were not significantly different from one another. In the "maximum rate" task, the thin subjects were the quickest, but the medium and obese subjects were not significantly different from one another.

The results obtained with the "maximum rate" task might be accounted for in terms of superior mobility in the case of the thin subjects, though it is somewhat surprising that no difference was found between the medium and the obese subjects. Why the obese subjects should be the slowest in the "spontaneous rate" tasks, however, is a little more difficult to answer, because it would not be thought that physical mobility would play a great part in them, and particularly not in the counting task. It might be argued that the obese subjects would be slower (through their lesser mobility) in many everyday tasks and that this slower rate generalizes to activities where their body-type would, in fact, introduce no restrictions. Alternatively, it might be argued that a low rate of performance of the "spontaneous rate" tasks reflects a low level of general spontaneous activity, and that a low level of activity leads to the development of an obese body type. At present, there is insufficient information to support more detailed hypotheses, but Mochizuki's results are certainly suggestive of further research.

A study by Jammes and Rosenberger (1971) may also be considered here, though thd interest of these authors was not in the general question of "everyday speed", but in a specific clinical phenomenon: spontaneous rocking behaviour in mentally retarded patients.

Jammes and Rosenberger measured spontaneous rocking frequency and heart-rate both before and after administration of a drug. The drugs which were used were epinephrine hydrochloride and ouabain, and the subjects of the experiment were 101 patients in a variety of diagnostic categories.

The authors comment that "an excellent correlation between heart-rate and rocking frequency was observed...." (P. 58). Unfortunately, however, no correlation coefficient is quoted in the paper. It is interesting that, in 80 of the patients, the ratio of heart/rocking frequency was "in the range of 1:1". In 16 of the patients, the ratio was of the order of 2:1.

Both of the drugs had the expected effects on heart-rate: the epinephrine resulted in an increase, and the ouabain resulted in a decrease. The rocking frequency was also observed to change after the administration of the drugs, and in each case the change was in the same direction as the change in heart-rate.

Though the authors do not quote their coefficient of correlation, this would appear to be the most impressive evidence of a relationship between a physiological variable and the spontaneous rate of movement. Whether this is confined to the particular clinical phenomenon in question, and what is the exact mechanism of the relationship are questions which might be answered by future researchers.

8. "Spontaneous Tempo" and Rhythm

Wallin (1911) performed an investigation of "preferred tempi" both in metronome beats and in music. In the first part of his study, 20 subjects gave a measure of their "preferred" metronome-rate, which was ascertained by means of a paired comparisons procedure. Having heard a pair of metronome rates, the subject was required to state "...which tempo was preferred, or which was felt to be the more agreeable" (P. 204). The rates to be compared gradually became closer in value as the procedure continued, until a point was reached at which several successive trials were given in which the tempi were objectively equal. Finally, the subject was exposed to a longer period of the chosen tempo, and asked whether he was completely satisfied with his choice.

Wallin reports that the mean chosen inter-beat interval was 0.519 seconds. The range was from 0.305 to 1.370 seconds, which covers the whole of the range available on the instrument which he used. As was the case with Frischeisen-Kohler's (1933) study, then, a wide range of individual differences was obtained.

An interesting aspect of this study is that introspective reports revealed a wide variety of imagery in response to the metronome-rates, and Wallin suggests that the imagery may have affected the choices which subjects made. For example, one subject reported that the faster rates suggested annoying machinery, pile-drivers and steam engines, whilst the slower rates suggested a swinging pendulum. Wallin suggests that the latter was probably a more pleasing image, and notes that the subject in question chose a slow rate as "preferred".

Wallin obtained no systematic relationship between musical ability and chosen rates, but it is stated that those who reported that they enjoyed the melody and harmony of music tended to choose slow or medium metronome rates, whilst those who reported that they chiefly enjoyed the rhythm of music tended to choose faster rates.

The second part of Wallin's study is unusual, in that an attempt was made to make "naturalistic" observations of subjects who were completely unaware that they were the subjects of investigation. Observations were made of spontaneous rhythmic responses which members of theatre audiences made to music of various types. Wallin's reasoning was that the tempo which produced the most vigorous response could be considered to be the "preferred" tempo.

Two aspects of the rhythmic responses were noted - the loudness (for example, of foot-tapping) and the number of members of the audience making such responses. Wallin reports that the tempo which produced the most vigorous response was exactly equal to the mean "preferred" metronome rate which was obtained in the first part of his investigation. He also notes, however, that tempo was not the sole determinant of the vigour of the responses. Such factors as the familiarity of the music and the appeal of its melodic content were also relevant.

The two aspects of Wallin's study which are perhaps most interesting (because they are unusual) are the introspective reports concerning imagery, and the covert observations made in the second part of the investigation. Further research into the former aspect might focus on the factors which determine individual differences in the images which are suggested, and the reliability of the imagery: does a given subject always report the same image to a given tempo, or is the imagery susceptible to change? The second part perhaps gives some grounds for concern, however. It appears from Wallin's report that the investigator himself was the sole observer, and he presumably knew the results which had been obtained in the first part of the experiment. The type of observations which were made (judgments of the vigour of rhythmic responses) must have been to some extent subjective, and the possibilities of observer bias cannot be ruled out. It would be interesting, therefore, to perform further research of this type, perhaps using more observers. In addition, superior technical facilities have become available since Wallin made his investigation, and it may now prove possible to increase the objectivity of the measures (for example, by using electronic instrumentation to measure the loudness of foot-tapping).

Another interesting question is whether a correlational approach might be possible: could the same subjects be observed both in a metronome experiment and when listening to music? It may not prove possible to perform such an investigation without listeners being aware that they were the subjects of an experiment, but it would be comparatively simple to arrange the situation so that they did not know what the interests of the investigator were. For example, subjects might be told that the experiment was concerned with the effects of prior exposure to music on the performance of some subsequent ("bogus") task. The subject might thus be led to believe that his performance on the task was all that was bing measured, and not his responses while the music was actually being played.

A final point which is worthy of note,is that the results which are obtained in a group situation might not be exactly the same as those which would be obtained if the same subjects were all tested individually. The responses of other members of the audience may have a considerable effect upon the responses made by any particular individual. From this point of view, therefore, individual testing might be considered more satisfactory, and it would be interesting to attempt to replicate Wallin's findings using such a technique.

Miles (1937) reported an investigation into "the temporal features of groups in rhythmic performance". The task involved was key-tapping, and in most parts of the investigation subjects were instructed to maintain regular spaces between taps: groups could only be formed, therefore, by emphasising one element of the group, by counting, or by using both of these techniques.

One of the questions which were of interest to Miles was whether the temporal length of a group or the length of the inter-tap interval was the more dominant variable in performance. Were the times taken to produce groups invariant with the size of the groups (subjects varying the inter-tap intervals), or was the inter-tap interval held constant, so that groups containing large numbers of elements (ie. taps) were longer in duration than those containing small numbers?

In the first part of the experiment, Miles investigated "free tapping" (in which subjects were not asked to form groups). "Spontaneous rate" tapping was elicited by instructions to tap "...at any rate you find most satisfactory". Two additional rates were elicited, one which the subject considered to be a little slower than that which he found most satisfactory, and one which was a little faster than the most satisfactory rate. 10 subjects attended for testing on a number of days, and on each day two "most satisfactory", one "slower" and one "faster" rates were obtained.

An interesting question concerns the effects of a "slower" of "faster" rate upon a succeeding "most satisfactory" rate. Does the preceeding task affect the rate which is obtained? Miles reports that no general statement is possible on this point, because an effect was observed in some of the subjects but not in others. However, since definite effects were observed in some cases, it would appear advisable to avoid experimental designs in which "spontaneous rate" performance follows upon instructions requiring different rates of performance.

As in Wallin's experiment, verbal reports were elicited from the subjects who took part in Miles' investigation. One subject considered that it was very doubtful that any rate was really more satisfactory than another. The subject in question was actually one of the most consistent (from day to day) of those tested. Another subject stated that it was definitely more pleasant to tap at the "most satisfactory" rate. The situation is made rather more uncertain, however, by the fact that on another occasion he reported that any rate tended to become satisfactory after a certain time. When he found this happening with a rate which was supposed to be "faster" or "slower", he would modify the rate so that it became unsatisfactory again. Only one of Miles' subjects was able to state <u>why</u> a particular rate was considered most satisfactory.

In the remainder of his investigation, Miles instructed his subjects to form "rhythmic groups" of various sizes (from 2 to 8 taps per group) in order to ascertain whether there was a tendency towards higher tapping-rates as the number of elements per group increased. In this part of the experiment subjects performed both at "spontaneous rate" and at a rate which "seemed fast". No overall tendency was observed for the rate of tapping to increase with the size of the groups and, indeed, the order of presentation of the various group-sizes appeared to have a considerable effect upon the results. However, in a section of the experiment where subjects were instructed to count the groups (rather than counting the individual taps) a tendency was observed for tapping-rate to increase with the number of elements per group.

Perhaps more directly relevant to the present topic than Miles' investigation is a study by Temperley (1963), in which the relationship between "personal tempo" and "subjective accentuation" was investigated. "Subjective accentuation" in this context refers to the tendency for individuals to hear accents in series of metronome-beats which are objectively identical. Temperley suggested that this tendency might be related to "personal tempo" because it might be argued that, when subjective accentuation occurs, the subject is <u>modifying</u> the metronome-rate which is presented. A secondary aim of his enquiry was to investigate the hypothesis of Fraisse (1942) that the "preferred rate" might be that rate which is "pendular" for the limb which is performing a task.

31 subjects took part in Temperley's experiment, and each of these attended for two sessions. In each session, the subjects heard a 33-minute tape on which were recorded 66 series of metronome-beats. Six different metronome-rates were used, and these were arranged in 11 different presentation-orders on the tape. The beats were pure tones of constant length, with inter-tone intervals which depended on the tempo of the metronome.

The subject's task was to beat time to the recorded metronome in any manner which seemed "most easy and natural". It was explained that it was permissible to tap once every "nth" metronome beat, or to interpose taps between beats. Two types of tapping were used: one in which the base of the hand rested on a microphone-box (finger-tapping) and one in which the elbow rested on a table, and the lower arm was moved up and down (arm-tapping). Scoring of responses was achieved both by stationing an observer in the laboratory and by making a tape recording of the proceedings.

In the second stage of the experiment, 20 of the subjects performed a free-tapping task, using the same microphone box as before. Each subject attended for six sessions on different days, and in each session they performed first at the "spontaneous rate" and then at the "maximum rate". In three of the sessions finger-tapping was used, whilst arm-tapping was used in the remaining sessions.

Temperley's hypothesis was that, for a given metronome-rate, the subject would adopt a subjective accentuation (revealed by his tapping-rate) which was that multiple of the metronome-rate which was closest to the tempo of free tapping. Thus, if the subject were presented with a metronome operating at his own "preferred tempo", he would tap once per beat. When the metronome was beating more slowly than this, he would tend to tap more quickly than the instrument (ie., interpose extra beats). When the metronome-rate was above the "personal tempo", the subject would tend to tap at a lower rate.

Temperley performed a mathematical analysis of his data which permitted him to calculate for each subject the metronome-rate which would be most likely to result in a tapping-rate of one tap per beat. This rate, he argues, may be regarded as "...the rate at which the subject would 'like' to tap when he is hearing a series" (P. 285). It is very interesting to note, therefore, that the median value of this rate was very close to the median free-tapping rate. However, Temperley did not obtain a significant correlation between the two rates, so that he rejects his original hypothesis.

Another interesting finding was that there was no significant difference between the rates of finger-tapping and arm-tapping. This result, argues Temperley, tends to cast doubt on the "pendular" hypothesis of Fraisse. This conclusion can be seriously challenged, however, on the grounds that the arm and hand in Temperley's investigation were not given the opportunity to act as pendulums. A pendulum is a free-swinging body. When at rest, it hangs vertically and, if it is deflected from its rest position and then released, damped oscillation will be observed, with the pendulum swinging on either side of the rest position. This is not the situation which existed in Temperley's experiment, however, since the arm was resting on a desk (at the base of the hand in finger-tapping and at the elbow in arm-tapping). Its rest position would then presumably be approximately horizontal, and it would only be free to move on one side of this position. An inanimate "pendulum" arranged in this way, if deflected and then released, would not swing, but would drop back to its rest position and remain there until deflected by a new input of energy. Temperley's experiment is certainly not an adequate test of the "pendular" hypothesis, therefore. Some such task as the arm-swinging activity used by Rimoldi (1951) would be necessary to establish or refute the hypothesis.

Another study pertaining to rhythm in self-paced performance was reported by Jordan (1970), and this appears to provide more evidence in opposition to the "motor" hypothesis which was suggested in the first section of the present chapter. Jordan's subjects moved their hand "at a rate which felt comfortable to them" through a channel which formed the perimeter of an equilateral triangle. The investigator's principal interest lay in the proportions of the total tracing-time spent on the three sides of the triangle. In fact, these proportions were apparently "nearly uniform".

As Jordan points out, the uniformity of these proportions might be attributed to one of two causes: that the subject was imposing a rhythm on performance, or that the distances moved when tracing the three sides were almost equal. It will be recognized that these would be the interpretations of the "rhythmic" and "motor" hypotheses respectively.

In order to test between these alternatives, Jordan next investigated the tracing of an isosceles triangle (in which the sides were not, of course, all equal). The "rhythmic" hypothesis would predict that the proportions spent on the three sides should still be equal. The "motor" hypothesis would predict that the times spent on the three sides would be in proportion to their lengths.

Jordan's results are, in fact, more compatible with the "rhythmic" than with the "motor" hypothesis, since the proportions with the isosceles triangle were again close to one another.

It is interesting to note that Jordan performed a further study in which subjects performed the tracing task simultaneously with a distracting activity (reading). In this case, the results appeared to represent a compromise between what would be predicted by the "rhythmic" and "motor" hypotheses.

On the whole, then, Jordan's results appear to provide support for the "rhythmic" rather than the "motor" hypothesis. However, his investigations can only be regarded as suggestive, since no statistical analysis is offered, and only four subjects were used in the research. This is certainly an interesting method, however, and it would appear to merit a larger-scale study.

Smoll's (1975b) study of the spontaneous tempo of arm-swinging has already been described in the first section of this chapter. In another study published earlier in the same year (Smoll, 1975a), he used the same task, and his aim was to demonstrate the existence of a "preferred rhythm" of performance.

Smoll's recording system was such that he was able to record the time taken to perform each individual swing. Thus, not only was he able to calculate the subject's speed of performance (in terms of the mean time per movement), but also his variability (in terms of the standard deviation of the individual movement-times about the mean movement-time). This within-subject variability was then compared with the between-subject variability obtained from the 75 males and 75 females involved in the experiment.

Smoll reports that the between-subject variability was 72 times greater than the within-subject variability: marked intra-individual consistency was contrasted with marked inter-individual differences.

He concludes that this result indicates that "...individuals have personal or "natural" preferences of voluntary movement tempo which differ from those of other individuals" (P. 442). He suggests that "...in situations involving repetitive motor responses to externally imposed rhythmic stimuli, whether in the laboratory or gymnasium or on the playing field, consideration should be given to the tempo at which individual performers prefer to move" (P. 442).

Unfortunately, this conclusion is not warranted by the data which Smoll reports. It may be, for example, that the subject in this type of experiment "arbitrarily" chooses a rate (having no "preference" for any particular one) and that, once this is established, he is able accurately to reproduce the required inter-movement interval. In short, the data may be indicative of accurate time-estimation, but they are certainly not sufficient to establish that subjects have "Preference" for the rate which they adopt, or that this rate should be considered in situations where an externally imposed rhythm normally exists. Such a conclusion might have been defensible had Smoll included a "control" condition in which subjects were paced at <u>randomly-assigned</u> rates, and had he demonstrated that intra-individual variability in this condition was <u>not</u> substantially smaller than inter-individual differences.

Though Smoll's conclusions are not warranted by his data, his suggestion that the "preferred rate" should be given consideration in the laboratory, playing field or gymnasium (and presumably the factory) is of great interest. This possible practical application of work in this area will be a recurrent theme in later stages of the present work.

<u>Summary and Conclusions</u>

As Rimoldi (1951) points out, "An exact evaluation of the existing bibliography as a whole is quite difficult" (P. 283). However, it is possible to make some summary statements regarding the literature reviewed here, and the best course of action would appear to be to take the subsections of the review in turn.

(1) Whenever even a moderate variety of tasks has been studied, the hypothesis that there is a unitary "personal tempo" has had to be rejected. However, the hypothesis of extreme specificity has also been seen to be at odds with the findings, since clusters of intercorrelated tasks have often been reported. The significant intercorrelations have always been positive. It must be said, however, that many of these correlations have related tasks which were logically very similar: for example, tapping with different parts of the body.

In general, test-retest reliabilities have been high, though occasional instances are to be found in the literature of meaures which have not displayed satisfactory reliability. Whether such failures represent intrinsic variability in the spontaneously-chosen speed of performance of the activities in question, or whether they represent deficiencies in experimental technique, it is not possible to say at present. It is probably advisable, however, for any new researcher entering the field to begin by testing the reliability of his technique by the performance of a test-retest study with the measures which he intends to use.

(2) The results of the research into the question of the relationship between "spontaneous" and "maximum" speeds are by no means unequivocal. There is a hint from

Fujita's (1970) investigation that, with some cognitive tasks, there might be no difference between the tempi adopted under "spontaneous" and "maximum" conditions. It is interesting also that Harrison (1941) obtained his highest correlations with the more cognitive tasks of reading and writing, though unfortunately the mean speeds are not given in his report. With simpler motor tasks, the results hitherto suggest that there is very little common variance between "spontaneous" and "maximum" tempi, though it must be noted that there is here some conflict between the results of Harrison, who obtained a non-significant positive correlation with tapping, and those of Mishima (1968), who obtained a low but significant negative correlation with the same task. The possibility cannot be ruled out, of course, that this discrepancy may represent a cultural difference. Are the connotations attached to such phrases as "congenial way" and "maximal way" similar in the two languages of English and Japanese, and do people in the two cultures have similar preconceptions concerning what is expected of them in psychological experiments ?

(3) Only one study has been performed to investigate possible hereditary factors in the determination of tempo. Frischeisen-Kohler's (1933) study was criticised on methodological grounds, but she did demonstrate a relationship between proximity of familial relationship and similarity of tempo. Further work must be carried out on the development of tempo before the plausibility of the "genetic" hypothesis can be assessed, but it is clearly a possibility which cannot be ruled out at this stage.

(4) Work on the interesting topic of the

geographical correlates of tempo has hardly yet begun, but preliminary results are encouraging. Particularly fascinating is the "ethological" approach of Lowin et al (1971), though Nagasaki (1972) has also obtained positive results in the more artificial setting of the laboratory. That regional differences exist within a single country strongly suggests that differences might also be found between different countries and cultures, and there is clearly plenty of scope for research in this area.

(5) In general, the research suggests that sex-differences in tempo are minimal. Harrison (1941) obtained a significant difference in walking-speed, but he plausibly attributes this to differences in footwear, rather than to biological factors associated with sex. Hoffman's (1969) study stands alone in revealing sex-differences in a simple motor task (tapping). It will be seen in the following chapter that Hoffman's study also conflicts with previous findings in respect of the obtained mean rates of performance. It is certainly possible, therefore, that some feature of his study introduced a spurious set, and that this may account for his discrepant findings with regard to sex.

(6) Research into the relationship between tempo and temporal judgments is another field of enquiry which is as yet in its infancy. There appears to be a conflict between the findings of Denner et al (1963) and Newman (1972), but differences between the methods used in these two studies left "missing links" in the argument, which need to be supplied by further research. However, it was seen in discussing the results of Cohen et al (1963) that it is

certainly reasonable to expect a relationship to exist between tempo and the perceived rate of passage of time, and there is much interesting work to be done in this area. Such research might include not only traditional laboratory methods of time-estimation, but also methods such as Knapp & Garbutt's (1958) Time Metaphor Test.

(7) Very little work has been addressed to the question of the relationship between "spontaneous tempo" and personality. The one study involving a measure of tempo and a standardised measure of personality (McCutcheon, 1974) produced negative results; her one significant correlation may be easily attributed to chance. The present writer is of the view that an unsystematic approach to this question (for example, a large-scale correlational or factor-analytic study involving large batteries of both personality and tempo measures) would not be particularly fruitful, but a more systematic approach would certainly be feasible. In discussing Craik & Sarbin's (1963) results, for example, it was noted that there are grounds for predicting a relationship between tempo and the personality dimension of extraversion-introversion, since the tempo may be regarded as an aspect of the sensory environment which is under the individual's direct control. There are no doubt many other specific predictions which could be made in this field, and it is this guided approach which appears to the present writer to be the most likely to produce readily interpretable results.

(8) The possibility of physiological correlates of tempo has hardly yet been tackled. The preliminary results

of Mochizuki (1968), indicating a tendency for those of "thinner" body-type to have higher tempi than those of a more "obese" type, are promising and suggest the value of further research into this question. Rashkis & Rashkis (1962) reported data which suggest a relationship between preference for metronome-rates and body-temperature, but the two authors themselves were the only two subjects in this study, so that these results can only be regarded as suggestive. Perhaps the most exciting of the findings in this field was that reported by Jammes & Rosenberger (1971), where many of the subjects tested spontaneously rocked at a frequency equal to their heart-rate. The possibility exists, of course, that this relationship might be confined to the particular abnormal phenomenon which these authors were investigating, but only research with other behaviour will demonstrate whether this is, in fact, the case.

(9) Several authors have provided data which are relevant to the question as to whether the "spontaneous" rate of performance of repetitive motor activities can be regarded as representing a "preferred rhtyhm". Temperley (1963) assumed that subjective accentuation represented an attempt by the subject to modify an imposed rate of work and used this phenomenon to calculate the rate at which the subject would "like to tap". There was no correlation between these calculated rates and the speeds which the subjects adopted in free-tapping, but these negative results may indicate that Temperley's assumption was incorrect, rather than that the "spontaneous rate" does not reflect a "preferred rhythm". Smoll (1975a) claimed to

have obtained evidence that the "spontaneous" rate of arm-swinging is a "preferred" rate, but it was seen that his evidence was not, in fact, sufficient to establish such a conclusion. Nevertheless, his suggestion that studies of "spontaneous speed" might have practical implications for situations in which the rate of work is normally imposed from without is worthy of further attention, and this will be one of the themes of later chapters of the present work.

CHAPTER 3:

Experiment I

<u>Introduction</u>

One fact which clearly emerges from the literature reviewed in Chapter 2 is the inappropriateness of the hypothesis that there exists a unitary "personal tempo". However, clusters of intercorrelated measures have often been reported, and it is reasonable to ask further questions about such clusters. What factors "set" the speed which the subject adopts in a given group of tasks, for example ? Is this tempo "preferred" in the sense that the subject eschews other tempi ? These are some of the questions which will be asked (with reference to one of the previously-obtained clusters of tempo-measures) in the present work.

A suitable cluster would appear to be the one which Allport & Vernon (1933) termed a "rhythmic composite", and which consisted basically of various types of tapping-task. It was seen in Chapter 2 that several other authors have reported correlations between tapping-tasks, so that the clustering of measures of this type is certainly a robust phenomenon. Moreover, tapping has been one of the most commonly-used measures in the literature, so that further investigations of such activities would be relevant to an interpretation of much of the existing research. Finally, such tasks are obviously convenient to work with in the laboratory.

It was suggested in Chapter 2, however, that any new researcher entering this field would be well-advised to begin his programme with a test-retest study, in order to assess the reliability of his own procedures, which will almost certainly differ to some extent from those of previous workers. (At least, since previous writers have in general provided relatively undetailed reports, it is impossible to ascertain the extent of the correspondance between any proposed procedures and those which have been used in the literature). It was the purpose of the present experiment, therefore, to replicate previous findings with regard to reliability and intercorrelational structure, and thereby to test apparatus and procedures which the writer proposed to use in later stages of this research programme.

Method

Selection and Description of Tasks. Though it had been decided to investigate further tapping-tasks, it was considered undesirable to design an experiment consisting solely of this type of task. On the one hand, such a procedure might lead to spuriously high intercorrelations. (For example, there may be "carry-over" of the rate of performance from one task to the next.) On the other hand, a battery of very similar tasks would be extremely monotonous from the point of view of the subject. It was deemed necessary, therefore, to include a variety of different tasks in the battery.

Convenience suggested such tasks as reading aloud and counting, but it will also be recalled that previous research left several questions concerning these tasks unanswered. Rimoldi (1951), for example, commented upon the need for further research into the relationship between tapping-rate and speed of reading. Thus, the inclusion of tasks other than tapping also carries with it the advantage that further information may be gathered concerning such activities as reading aloud and counting.

Table 3.1 presents brief descriptions of the tasks included in the battery, together with the abbreviations which will be used in the following pages. Full descriptions and the instructions which were used are presented in Appendix I.

In tasks 1 - 4, the number of cycles completed in the specified period was recorded. In practice, a cycle was defined as the period between two successive "clicks" emitted by the apparatus (see below). Hence if the subject (for example) produced 41 such slicks in the 30-second period, a score of

TABLE 3.1: Tasks Performed in Experiment I

1	FL	Tapping left fingers, 30 seconds
2	FR	Tapping right fingers, 30 seconds
3	TL	Tapping left toes, 30 seconds
4	TR	Tapping right toes, 30 seconds
5	CA	Cancelling occurrences of the letter "e"
6	COU	Counting to 30 (aloud)
7	F	Reading aloud (fictional material)
8	Sc	Reading aloud (scientific material)
9	CD	Circle-dotting (reciprocal tapping), 30 seconds
10	E	Writing "e's", 30 seconds
11	CPY	Copying a prose passage

40 cycles was registered. A similar procedure was adopted in scoring the circle-dotting task. In each of these cases, timing was begun from the first "click" produced by the apparatus.

Task E was scored in terms of the number of letters written in 30 seconds. For the remainder of the battery, the time (in seconds) taken to complete the task was recorded.

Reliability. A test-retest design was employed in this investigation. The interval between the two sessions was set at two weeks.

Order of Presentation of Tasks. It was seen earlier that many of the investigators in this area have used an invariant order of presentation of tasks in their experiments. Such a procedure is unsatisfactory, however, and it was decided in this study to vary the order in which tasks were presented to the subjects. There were two constraints upon this variation:

(1). A "warm-up" task (drawing squares on paper for 30 seconds) was introduced in order to permit the subjects a period in which to become accustomed to the experimental situation, and this was necessarily always performed first.

(2). It was deemed undesirable for very similar tasks to lie adjacent in the order. (This point was also made by Allport & Vernon, 1933). A perfectly random system of variation might, for example, permit the juxtaposition of several tapping-tasks, and it was thought that this might lead to a rapid decrease in the subject's motivation. In addition, the possibility of "carry-over" was again envisaged.

In order to prevent such juxtapositions, the experimental session was first divided into two halves, separated by a rest-interval of approximately five minutes. One member of each of the three pairs of tasks, FL-FR, TL-TR and F-Sc was then assigned to each half of the session. (Which member was assigned to which half was determined by the toss of a coin.) This procedure prevented the juxtapositioning of two finger-tapping tasks, or of two toe-tapping tasks, or of two reading-tasks. However, the juxtapositioning of a finger-tapping task with a toe-tapping task, or of either of these with circle-dotting, could still occur. This was prevented in the following way: the position of the rest-interval was first determined. (Since the number of tasks in the battery was 11, the interval could be placed either following the fifth or following the sixth task.) This was again determined by the toss of a coin. The remainder of the tasks (CA, COU, CD, E and CPY) were then arranged to "fill" the two halves by drawing lots. Within each half, the order of the tasks was then fully randomised. In the event of an unwanted juxtaposition, the second member of the offending pair then exchanged positions with the task next in the order for that half. If the second member of the offending pair was the last task in that half, it exchanged positions with the first task in that half. Should the order still contain an unwanted juxtaposition, it was scrapped, and the procedure was begun again.

Obtaining a "Spontaneous" Response. Two principal problems may be considered under this heading:

(1). preventing the subject from becoming self-conscious or ill-at-ease whilst performing the tasks;

(2). avoiding any suggestion of a speed at which the tasks should be performed.

To a certain extent, these two requirements may be incompatible, since the experimenter-subject interaction which would appear necessary to promote good rapport and self-assurance also introduces the possibility of suggestion by experimenter-subject empathy.

In order to counter problem (1), the experimenter engaged the subject in friendly, informal conversation during the walk from the prearranged meeting-place to the laboratory and during the interval between the two halves of the session. During performance of the experimental tasks, the experimenter remained inconspicuouslby sitting behind, and out of sight of, the subject. The television camera (see below) was placed behind a one-way mirror which was almost completely concealed by hardboard screens in order to convey the impression that it was not in use.

It was thought possible that fluctuations in the experimenter's behaviour (for example, when reading instructions) might increase error-variance, or even that he might inadvertantly bias the results by behaving in different ways with particular subjects or tasks. Though this problem appears not to have been mentioned by previous workers, it was decided in this study to minimise the investigator's part in the procedure. To this end, all instructions were presented to the subject in typewritten form, assembled in a loose leaf book in the order determined by the method described above. The experimenter then had only to instruct the subjects when to begin and end the various tasks.

It was decided that subjects would be given no information regarding the aims of the investigation until after completion of the second (retest) session. In addition, they were requested not to divulge the nature of the experiment to their colleagues, who might be recruited as subjects at a later date.

Rimoldi (1951) argues that no mention of "speed" should be made in the instructions given to subjects. It was noted earlier, however, that Harrison (1941) discovered a prevalent "set for speed" amongst student-subjects, and that this fact led him to emphasise that subjects were not expected to perform the tasks at their fastest rate. It was thought possible that subjects might come to this experiment with such a set, and so the introductory instructions which were given to them (see Appendix I) stressed that the investigator was not interested in how quickly or how well they could perform the tasks.

Related to this issue is the necessity of concealing all timing apparatus since, if the subject were to discover that his behaviour was being timed, he may then perceive the experiment as a "testing situation" in which his optimum rate of performance was of interest. This problem was overcome in this case by making a video recording of the entire session. The experimenter was then able to time the behaviour at his leisure, when testing had been completed for the day. In tasks where performance was for a specified period (30 seconds), the experimenter was able to issue instructions at appropriate times by surreptitious glances at his wristwatch.

Time of Day. In order to control for possible diurnal rhythms in "spontaneous speed", each subject's retest session was, if possible, arranged for the same time of day as his original session. The use of a restricted range of testing-times (1400 - 1630) ensured that, when exact matching was not possible, the discrepancy between the two times remained small.

Subjects. Subjects were 25 male undergraduates, whose ages ranged from 18 to 26 years. 19 of the subjects returned for the retest session.

Apparatus. Sound and vision were recorded by means of a closed-circuit television system and an Amplex video tape-recorder. For tasks FL and FR a Morse key was provided and for tasks TL and TR a foot-switch was constructed. This consisted of a spring-leaded wooden pedal operating a microswitch. Both of these pieces of apparatus were connected to "bogus instrumentation" (an event-recorder) since it was thought that the subject might otherwise become suspicious as to how data were being collected, and hence that the face-validity of the experiment would be decreased. The recordings obtained from this instrument were not used in the data-analysis, however, because the highest drum-speed available was insufficient to permit accurate measurements to be made. The sounds produced by the Morse key and the foot-switch (as recorded on the video tape) proved to be perfectly adequate for this purpose.

Paper and pencil were provided for the subject, together with a sheet of paper carrying the two targets for the circle-dotting task (see Appendix I for a reproduction). The circles were one inch in diameter, and were separated by a distance of three inches (centre-to-centre).

Procedure. Before the subject's arrival, the experimenter prepared the instruction-book and placed it on the subject's desk, together with the other materials which would be required during the course of the session. All electrical equipment was then switched on and tested, and the video-recorder was prepared so that recording could be initiated by the press of a switch. The experimenter then left the laboratory and went to await the subject at a prearranged meeting-place.

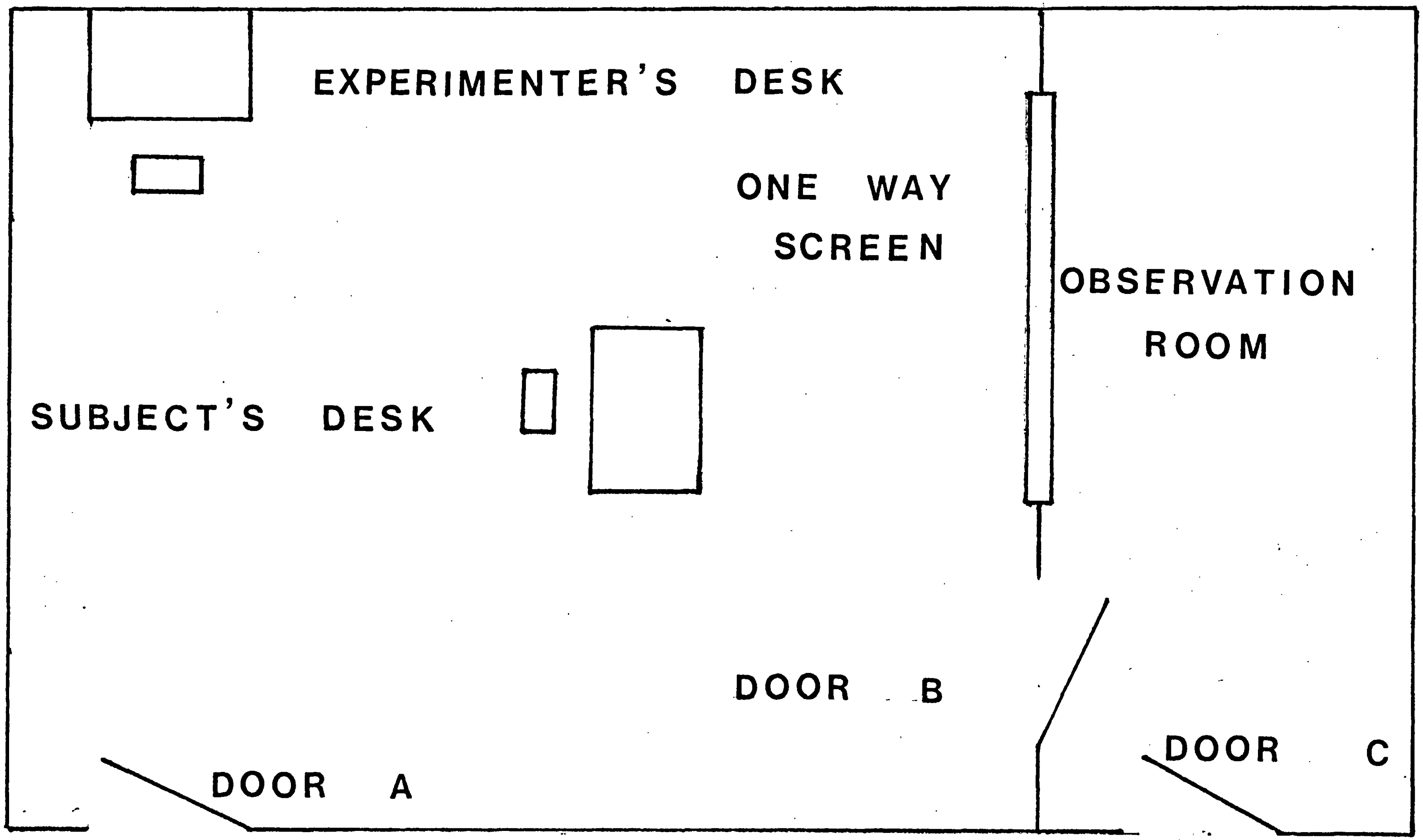
When the subject arrived, the experimenter conducted him to the laboratory, which they entered through door A (see Figure 3.1). Having shown the subject to his seat, the experimenter pointed to the microphone (which was suspended above the subject's desk) and said:

"I'm recording the sound, so I'm just going to switch on my tape-recorder."

He then entered the Observation Room, switched on the video-recorder, and returned to the laboratory, closing door B behind him. As he walked to his seat, he asked the subject to begin reading the introductory instructions, after which the session proceeded, with the experimenter directing the subject to begin and end tasks as appropriate.

Upon completion of the experimental session (which generally required approximately 25 minutes) the experimenter either arranged the time of the retest session (in the case of the first session) or divulged the aims of the investigation (in the case of the retest session). The subject was then thanked for his participation and asked not to communicate to anyone else any information regarding the experiment.

FIG. 3.1 LABORATORY PLAN



Results

Before passing to the statistical analysis of the data obtained in this experiment, it should be noted that, of the 19 subjects who presented themselves for the retest session, one (S 1.19) was discarded from the analysis of Session 2 because of a failure to comply with instructions.

All statistical analysis was performed by computer programmes written by the investigator and executed on a Digital Equipment Corporation PDP Lab 8/E machine. Listings of these programmes may be found in Appendix III.

For the purposes of correlation, raw data (which will be found in Appendix II) were converted into ranks. This procedure permitted straightforward correction for the two different methods of measuring speed which were used in this experiment. Data for some of the tasks (FL, FR, TL, TR, CD and E) were in terms of the amount of work done in a fixed period of time, whilst those for the remaining tasks were in terms of the time required to complete a fixed amount of work. As a result of these logically opposite ways of measuring speed, a tendency (for example) for subjects who tapped quickly also to read quickly would produce a negative correlation between these tasks. In order to avoid confusion, therefore, the ranking routine was provided with a reverse-ranking factility. Whilst the smallest scores (times) for tasks CA, COU, F, Sc. and CPY were assigned the rank of 1, the largest scores (amounts of work) for the remaining tasks were assigned the rank of 1.

Means and variances obtained for the 11 tasks in Sessions 1 and 2 are set out in Tables 3.2 and 3.3 respectively. Data for tasks CA, COU, F. Sc. and CPY are in seconds. Those for FL, FR, TL, TR and

TABLE 3.2: Descriptive Statistics, Session 1

Task	Mean	SD
FL	57.44	24.08
FR	60.94	33.15
TL	44.08	20.40
TR	45.60	21.29
CA	143.12	29.79
COU	20.76	4.05
F	120.48	14.15
Sc	132.64	14.19
CD	56.88	13.80
E	38.96	8.94
CPY	372.04	48.90

TABLE 3.3: Descriptive Statistics, Session 2

Task	Mean	SD
FL	60.11	25.17
FR	60.00	27.19
TL	48.00	18.11
TR	49.83	25.14
CA	138.00	24.57
COU	20.22	3.66
F	116.44	13.85
Sc	127.55	12.67
CD	63.22	13.61
E	40.39	8.05
CPY	185.72	24.42

TABLE 3.4: Test-retest Correlations (Spearman)

FL	FR	TL	TR	CA	COU	F	Sc	CD	E	CPY
832*	860*	843*	821*	792*	665*	829*	717*	819*	443	800

Note

Decimal points have been omitted for clarity of presentation

* $p < .01$

and CD are in terms of the number of cycles completed in 30 seconds. The reader's attention is drawn to the apparent discrepancy between the speed of copying in the two sessions. This arose because, during the course of Session 1, it became clear that the material which had been selected for this task was inconveniently long, so that it was replaced with a shorter passage from the same source in the retest session.

In order to assess the stability of the mean rates of performance over the period of the retest interval, a series of 't' tests for correlated means was claculated. Task CPY was omitted from this analysis, on account of the above-mentioned change of material. Four of the resulting values of 't' were significant, and all of these inducated more rapid performance in the retest than in the original session. They were associated with the following tasks: reading fiction ($t = 2.94$; $p < .01$), reading science ($t = 2.94$; $p < .01$), circle-dotting ($t = 2.16$; $p < .05$) and writing e's ($t = 2.20$; $p < .05$).

From tables 3.2 and 3.3 it may be seen that, in each session, the rate of finger-tapping was close to the rate of circle-dotting. In order to test these results statistically, for each session the rate of circle-dotting was compared with the rate of finger-tapping, with both the right and the left hands. 't' tests for correlated means were again used, and the results of the analysis were as follows: for Session 1, FL-CD, $t = 0.17$, N.S.; FR-CD, $t = 0.83$, N.S. For Session 2, FL-CD, $t = 0.44$, N.S.; FR-CD, $t = 1.24$, N.S. (df = 24 for Session 1 and 17 for Session 2). Thus the rate of finger-tapping was not significantly different from the rate of circle-dotting.

Reference to Tables 3.2 and 3.3 also reveals that, in both sessions, the rate of finger-tapping was higher than the rate of toe-tapping. 't' tests for correlated means indicated that these differences were all significant at the .01 level (session 1: FL-TL, t = 4.85, FR-TR, t = 4.46. Session 2: FL-TL, t = 4.12, FR-TR, t = 4.25).

Test-retest correlations (Spearman rank-order) are presented in Table 3.4. All correlations but one (task E) are significant at the 0.1 level of confidence. The value of 0.443 associated with task E is not statistically significant. The correlation of 0.80 obtained with task CPY is particularly satisfactory in view of the change in material which took place between the two sessions.

The intercorrelations for Sessions 1 and 2 are set out in Tables 3.5 and 3.6 respectively. In interpreting these data, the .01 level of confidence would appear to be appropriate, though correlations which reach the .05 level in both sessions may be considered reliable. Greater weight should be given to the results from Session 1, since the sample-size is greater than that for Session 2.

As expected in the light of the results of previous investigators, the data support neither the view of generality nor the view of extreme specificity of "spontaneous speed". Though there are many non-significant correlations, there are, nevertheless, clusters of intercorrelated measures.

One such cluster, which will be designated the "T-cluster", consists of the four tapping-tasks, FL, FR, TL and TR, together with CD. There is also evidence in Table 3.5 of some degree of common variance between these measures and counting. None of the relevant correlations reaches the .05 level of confidence in Table 3.6,

TABLE 3.5: Experiment I Intercorrelations (Session 1)

	FR	TL	TR	CA	COU	F	Sc	CD	E	CPY
FL	892	735	865	230	548	-219	-225	825	394	-036
FR		696	813	226	421	-230	-203	861	274	106
TL			839	173	390	-017	-144	685	451	210
TR				315	483	-164	-207	842	307	035
CA					457	-278	-184	199	-175	155
COU						-081	-105	445	112	058
F							828	011	247	487
Sc								026	133	495
CD									227	087
E										131

For df = 23, a value of 0.396 is required for significance at the .05 level, and a value of 0.505 is required for significance at the .01 level.

Decimal points and leading 0's have been omitted for clarity.

TABLE 3.6: Experiment I Intercorrelations (Session 2)

	FR	TL	TR	CA	COU	F	Sc	CD	E	CPY
FL	934	893	890	148	300	-153	-070	805	170	188
FR		892	857	148	156	-133	-111	750	105	110
TL			936	160	271	-028	-032	772	312	202
TR				236	312	-105	-048	735	224	263
CA					585	-024	224	226	-109	403
COU						055	113	233	-130	094
F							878	076	317	329
Sc								142	167	517
CD									349	115
E										329

For df = 16, a value of 0.468 is required for significance at the .05 level, and a value of 0.590 is required for significance at the .01 level.

Decimal points and leading 0's have been omitted for clarity.

however. It will also be seen that CA and COU are significantly correlated ($p < .05$) in both sessions, though CA is not significantly correlated with any of the tapping-tasks, or with CD.

Tasks F and Sc. are significantly correlated in both sessions, and the correlation between copying and the reading of scientific material reaches the Q05 level in both sessions. Though the correlation between F and CPY in the retest session is not significant, it would not appear inappropriate at the present stage to regard rasks F, Sc. and CPY as forming a second cluster, which will be designated the "R-cluster".

A final feature of Tables 3.5 and 3.6 which is worthy of note concerns the correlations between finger- and toe-tapping and the two reading-tasks. All eight of the coefficients relating reading to tapping are negative. However, none of these reaches statistical significance, and the correlations relating reading to circle-dotting are low and positive.

Discussion

It was noted in the previous chapter that comparatively few data are provided in the literature concerning the distribution of rates of performance obtained in this type of experiment. One report which does include such information, however, is that of Frischeisen-Kohler (1933), though even in this case comparison with the present results is rendered difficult by her pooling of data obtained with the hands and the feet, between which a significant difference has been obtained here.

Since the sampling-period in Frischeisen-Kohler's tapping-tasks was only ten seconds in length (as distinct from the 30 seconds used here) the data must be converted into taps per second for purposes of comparison. Expressed in this way, her results indicate a mean rate of 2.306 taps per second for males and 2.378 taps per second for females. The present data result in means for Session 1 of 1.91, 2.03, 1.46 and 1.52 taps per second for tasks FL, FR, TL and TR respectively, and of 2.00, 2.00, 1.60 and 1.66 taps per second for the same tasks in Session 2.

Frischeisen-Kohler does not indicate whether the mean rate of finger-tapping was higher than the mean rate of foot-tapping in her study but, if this were the case, then it would follow that the mean rate of finger-tapping which she obtained was even higher than the means which she quotes. Assuming for the present, however, that the two rates were not different (a possible reason for the difference in the present results will be suggested later), it would still appear that Frischeisen-Kohler's subjects tapped more quickly than those used here. From the data which she provides, it is not possible to calculate the significance of this difference, but, since the standard errors for the mean rates of FL and FR were 0116 and 0.22 respectively, it would not appear unreasonable to suggest that chance factors of sampling may account for it.

Another report which provides information on the distribution of speeds is that of Nagasaki (1972) who, like Frischeisen-Kohler, used a 10-second sampling-period. Neglecting the data which he obtained with primary school children (who performed particularly quickly) the data indicate means of 1.33 and 1.45 taps per second for the Akita subjects and of 1.81 and 1.92 taps per second for the subjects from Metropolitan Tokyo. The Akita means are lower than those obtained in this study and, from the standard deviations which Nagasaki provides, it would appear probable that the difference is significant. It has already been noted that the Akita subjects were, in fact, slower than those from Tokyo, but Nagasaki does not provide the geographical information which would indicate whether the Akita or the Tokyo subjects were more comparable with those employed in the present investigation. It is, however, possible that the difference between the Akita subjects and the present sample may also be attributable to regional factors.

Hoffman (1969) also reports mean finger-tapping rates obtained during 10-second trials. For males, he obtained a rate of 3.76 taps per second, and females were significantly slower with a mean rate of 2.98 taps per second. It is notable that these rates are considerably greater than those obtained in this investigation, and that Hoffman's subjects also performed more quickly than those used by Frischeisen-Kohler or Nagasaki. It would appear, then, that Hoffman's data do not fall into line with those reported by other investigators. There is nothing in

Hoffman's report which points to an explanation of this discrepancy.

Considering next information regarding the dispersion of the distributions of speed, Frischeisen-Kohler reported a range of 7 to 58 taps per ten seconds. Assuming that each subject would have maintained the same rate over a 30-second trial, this would have resulted in a range of 21 to 174 taps in such a period. This is close to the range of 16 to 175 taps which is obtained by pooling the data from tasks FL and FR of the present study.

In comparing the present data with those of Nagasaki, it would appear most appropriate to consider his Tokyo subjects, since their mean speed was closer to the means obtained here than was the case with the Akita subjects. Nagasaki reports standard deviation of 6.02 to 7.61 taps per 10 seconds, which would indicate SD's of 18.06 to 22.03 taps per 30 seconds, assuming that subjects would have maintained the same speed over this longer period. These values are somewhat lower than those obtained in this study.

In summary, the distributions obtained here were broadly similar to those reported by previous workers. There is a considerable discrepancy between the present data and those which were obtained by Hoffman, but his results appear to be exceptional.

It was seen in Chapter 2 that several previous authors have reported high test-retest reliabilities for measures of "spontaneous speed", and the present results largely replicate these findings. One point of variance between Rimoldi's (1951) study and the present one, however, lies in the low correlation of 0.443 obtained with task E. Rimoldi reports a correlation of 0.89 for this measure, and there is no obvious explanation for such a discrepancy. It might be hypothesised that the low reliability in this experiment may have arisen from taking too small a sample of the behaviour (subjects performed for 30 seconds as against Rimoldi's 60). However, it is unlikely that this provides a satisfactory explanation, since tasks FL, FR, TL, TR and CD were also performed for only 30 seconds, and the reliabilities associated with these measures are high, and certainly comparable with those reported by Rimoldi.

The test-retest correlations demonstrate that subjects' rates of performance relative to one another are, in all but one case, satisfactorily stable over a period of two weeks. Another interesting question concerns the stability of the group-means over the test-retest interval. It has been shown that tasks F, Sc, CD and E were performed significantly more quickly in the retest than in the original session. Familiarity with the material may well account for the increase in reading-speed. It is also possible that lack of familiarity with the tasks may have led to the adoption of a hesitant approach in the first session. It has been described how, in an effort to avoid any suggestion of a speed at which the subject should perform the tasks, the part played by the experimenter was minimised. Thus, no form of encouragement could be given when the subject began to perform a task, and demonstrations by the investigator were considered inappropriate. Thus, subjects often appeared somewhat hesitant when beginning a new task, as though uncertain whether they had correctly interpreted the typed instructions. It appeared, however, that there was a tendency for them to accelerate as it became clear that they were, in fact, performing the task correctly (since the experimenter had not interrupted to correct them). Upon encountering the tasks in the retest session, the subject would presumably recall what he had done in the previous session, so that uncertainties regarding the interpretation of the instructions would not arise, and the subject could begin confidently and without a hesitant approach.

However, one problem associated with such an "explanation" lies in the fact that not all of the tasks displayed such an increase in speed between the sessions. In the case of the finger- and toe-tapping tasks, this might be accounted for by arguing that they are very similar tasks and that, once the subject had successfully performed the first of them to occur, he was confident that he knew how to perform the remaining three when he encountered them. Since the order of presentation was not constant, the net result over the group would be that the hypothetical effect of hesitancy would be evenly divided amongst the four tapping-tasks and, therefore, that it would be smaller with any one of them.

Though this account is attractive, it remains to be explained why, if such processes were at work, there was no increase in speed between the sessions with tasks CA and COU. Clearly, such inter-session variations in tempo should be subjected to further research.

Two clusters of intercorrelated measures have been obtained in this experiment, and these may now be compared with the results reported by previous workers in the field.

The T-cluster, consisting of the tapping-tasks (and perhaps of counting), appears to be broadly in line with the literature reviewed in Chapter 2. Allport & Vernon (1933) obtained a "rhythmic speed composite" consisting of finger-, hand- and leg-tapping and the repetitive compression of a stylus, but statistically non-significant correlations were obtained between these measures and counting. Rimoldi (1951) extracted a factor (B) on which finger-, arm-, toe- and heel-tapping were all heavily loaded and on which counting also received a (statistically significant) loading of 0.39. In Harrison's (1941) study, tapping was correlated to the extent of 0.47 with "patting", but neither of these measures was significantly correlated with the rate of counting. In summary, though these studies all provide clear evidence of the clustering of various types of tapping (and other repetitive activities such as Harrison's "patting"), a correlation between such tasks and the speed of counting has not always been obtained. Counting will be considered again later in this discussion.

Another cluster, comprising reading aloud and copying, has also been suggested by the results of the present study. In Rimoldi's study, copying did not receive a significant loading on Factor C (on which the reading tasks were most heavily loaded) and Harrison reported a non-significant correlation of 0.29 between reading and "writing" (copying a passage from a book). These negative findings must, however, be set against those of Allport & Vernon and Lauer (1933). In both of these studies, significant correlations were obtained between rates of reading and of writing. It will be recalled that Allport & Vernon postulated a "verbal speed composite" on the basis of their results.

Another parallel between the present results and those reported by Allport & Vernon lies in the tendency towards negative correlations between reading and tapping. Finger- and hand-tapping, leg-tapping and stylus-compression were all negatively correlated with reading in their study (though the correlation between reading and finger- and hand-tapping was very low) and the tendency in the present results has already been noted. In neither investigation, however, was any of the relevant coefficients statistically significant. Rimoldi's Factor C was negatively correlated with Factor B in his second-order analysis, but he reports that the average intercorrelation between reading and tapping was positive ($r = 0.36$).

It is now convenient to turn to a consideration of the possible sources of common variance within each cluster, and particularly within the T-cluster, which forms the principal interest of the present investigation.

The most obvious hypothesis would appear to be Allport & Vernon's (1933) implicit suggestion that the "rhythmic" or repetitive nature of such tasks may account for their common variance. It was seen in Chapter 2 that such an account might be said to encounter difficulties with Harrison's (1941) results on account of the significant correlations which he obtained between tapping and two non-repetitive (or "once-off") tasks, but a possible method of reconciling the hypothesis with these results was also suggested. It would now be of interest to consider the hypothesis in greater detail, and in the light of the present results.

Hitherto, the hypothesis has not been stated in detail; it has merely been suggested that the "rhythmic" character of tasks such as tapping might account for their clustering in intercorrelational studies. Many more specific suggestions are possible, but it would appear to be sensible at this juncture to consider the simplest possible version of the "rhythmic" hypothesis which would account for the experimental findings.

The simplest possible suggestion would appear to be that each subject has a single "preferred rhythm" which he reliably adopts whenever the conditions of the task permit him to do so. In particular, it could be argued that each subject adopted his "preferred rhythm" in both finger-tapping and circle-dotting in the present study, and the data obtained with these tasks suggest that the mean value of this rhythm is of the order of 2 beats per second. (It is true that the rate of toe-tapping, which was correlated with both of these, was lower than this value, but a possible reason for this will be discussed below).

Thus, the "rhythmic" hypothesis would suggest that, in a "spontaneous-rate" experiment, the speed of reciprocal tapping (circle-dotting) is determined by the subject's "preferred rhythm". This may be contrasted with an alternative account, which would suggest that the speed adopted is limited or determined by the difficulty of the targets. For example, the tempo adopted by a given subject may be determined by the standard of accuracy which he sets for himself.

The classic work on the relationship between speed and accuracy in reciprocal tapping was reported by Fitts (1954). His task was slightly different from the version used here since the targets were rectangular, rather than circular. In addition, they were metallic, and were connected to electrical recording apparatus. A more important difference may lie in the form of the instructions given to the subjects ("score as many hits as you can"). Though they were also told to "stress accuracy rather than speed", Fitts himself describes his investigation as one in which "...the S is required to work at his maximum rate...." (P. 390).

Fitts suggested that the difficulty of a given target was proportional to the ratio between the target-separation (A) and the target-width (W). Specifically, he proposed an Index of Difficulty (ID), such that:

$$ID = \log_2(2A/W) \text{ bits per movement} \quad ......(3.1)$$

and suggested that the speed of performance would be related to ID by the equation:

$$MT = a + b(ID) \qquad .....(3.2)$$

where MT (movement-time) is the mean time taken to move the pencil or stylus from one target to the other.

Fitts reported data which provide a good fit to Equation 3.2 except for the case where ID was equal to unity, where performance was somewhat slower than predicted. (This may be due to a physiological limit on the maximum rate of movement). Increases in speed were observed with every decrease in task-difficulty, so that it would appear that task-difficulty does limit speed of performance in the "maximum-rate" condition. Should it be inferred, then, that task-difficulty was limiting the rate of circle-dotting in the present study?

Such a suggestion would appear to provide a less parsimonious account of the results than would the "rhythmic" hypothesis. If the rate of reciprocal tapping is determined by the criterion of accuracy (degree of freedom from errors) which the subject demands, then why should this rate be correlated with the speed of finger-tapping, in which accuracy and errors are not involved? Moreover, if it is to be maintained that task-difficulty determined the speed of circle-dotting, then a remarkable coincidence must be postulated: by chance alone, the experimenter chose targets of just that level of difficulty which would result in a speed of dotting which was not significantly different from the obtained rate of finger-tapping. Though such a coincidence is not impossible, it would certainly appear unlikely. Finally, an examination of the distributions of dots produced by the subjects revealed that they did not use the whole of the available target-width. Rather, the dots were more closely distributed about the centres than was strictly necessary for errorless performance. There is no obvious evidence here, then, that the difficulty of the task was limiting speed. It might, indeed, be argued that subjects could have maintained a somewhat higher speed without a significant increase in errors, or conversely that they could have maintained almost errorless performance at the same speed on somewhat narrower targets (with the same target-separation).

The "rhythmic" hypothesis, then, would appear to provide a more elegant account of the data with finger-tapping and circle-dotting than would the suggestion that task-difficulty determines the spontaneous rate of performance of the latter. Another aspect of the results which may be thought to have a bearing on the "rhythmic" hypothesis is the suggestion from Session 1 that counting might be included in the T-cluster. A proponent of the hypothesis might attempt to account for this finding by suggesting that counting is also a "rhythmic" task if subjects adopt a regularly-timed pattern of enunciation (and, indeed, this appeared to be the case from casual observation). However, two additional points should be made here. On the one hand, it was seen that the validity of including counting in the T-cluster could be questioned on the grounds of the results obtained in Session 2. On the other hand, it should be noted that the mean rate of finger-tapping (and of circle-dotting) was approximately 2 taps per second. Yet the mean rate of counting was approximately 1.5 numeral per second. If the subject has a "preferred rhythm" and if he performs both finger-tapping and counting in a "rhythmic" manner, then why are the speeds not equal?

Two different types of answer may be given to this question. In the first place, it might be suggested that "taps per second" and "numerals per second" are not, in any case, equivalent measures.

Many of the numerals involved in the task used here were bisyllabic, for example. It might be suggested that the rate of counting should, therefore, be considered in syllables per second. However, if this is done, there remains a discrepancy between the two rates. The second type of answer would assume that "taps per second" and "numerals per second" are directly comparable, but would suggest that there is a "harmonic" relationship between the rhythms adopted here in the two tasks. More specifically, it might be argued that subjects were performing in both tasks at multiples of some more fundamental rhythm, such that they were counting at three times the rate of that rhythm, and performing finger-tapping at four times the rate.

It may be, then, that the simple hypothesis of a single "preferred rhythm" requires modification by the addition of the concept of "harmonics". This is a suggestion which will be more fully discussed later in this work (see Chapter 6).

The significant difference which has been obtained here between finger-tapping and toe-tapping is also clearly relevant to this discussion. Once again, if subjects have a "preferred rhythm", and if it is this which accounts for the high correlation between the tasks, then why are the means different ? A proponent of the hypothesis might again have recourse to the concept of "harmonics", but this additional assumption may not be necessary, because it can be argued that the foot-switch used in toe-tapping gave rise to the discrepancy. The microswitch incorporated into this device was "double acting" in the sense that it emitted a "click" both when the contacts made and when they broke. Thus, a click was heard both when the pedal was pressed and when it was released. By contrast, the telegraph key would produce an audible click only when depressed to its full extent.

Subjects' responses to the double-acting foot switch appeared to be variable. Some operated the switch with a sharp ballistic movement so that the two clicks were barely discriminable from one another. Others appeared to adopt a more deliberate approach, so that the two clicks were clearly differentiated. It is by no means implausible to suggest, therefore, that the construction of the foot-switch may have led in part to the discrepancy between the rates of finger-tapping and toe-tapping.

Another aspect of the operation of the foot-switch which may have contaminated the results is the fact that there were numerous occurrences of "contact bounce" when several clicks were heard from the apparatus in rapid succession. (This effect may have resulted from tremor in the subject's foot). Though it is not clear what effect (if any) this characteristic of the foot-switch may have had on the mean rate of toe-tapping, it is certainly possible that it contaminated the results. For example, the foot-switch was ostensibly connected to a pen-recorder, and contact bounce would have led to the registration of spurious responses had these recordings actually been used in the analysis of the behaviour. Subjects may, therefore, have attempted to minimise this bounce in order to avoid spurious recording. One way in which they may have attempted to achieve this is by adopting a slower and more deliberate style of tapping.

These suggestions concerning the possible effects of the foot-switch are somewhat speculative, but it is clear that it would be of interest to conduct further investigations into the relationship between the rates of tapping with the fingers and the toes. For the present, it would appear prudent to regard with suspicion the data obtained here with the toe-tapping task.

Considering next the R-cluster, it is immediately obvious that, in contrast with the relatively simple motor activities of the T-cluster, these are more complex "cognitive" tasks. One possible hypothesis, therefore, is that there may exist a general "cognitive tempo". The borderline between "cognitive" and "non-cognitive" would, of course, have to be determined empirically.

Empirical support for this hypothesis might be claimed from Rimoldi's Factor C, on which reading aloud (three different types of material) and three of Thurstone's PMA subtests (Verbal Meaning, Reasoning and Number) all received significant loadings. It will be recalled, however, that Rimoldi himself rejected this interpretation, preferring to regard the factor as a dimension of "preceptual speed". It was seen, in fact, that the loading of one task on this factor ("marbles") provided particular difficulties for the "cognitive" hypothesis.

It has been seen that Allport & Vernon postulated a "verbal" dimension on the basis of their results, which included reading, counting, handwriting and blackboard-writing. The inclusion in the cluster of counting was challenged, but the significant correlation between counting and handwriting, and the non-significant counting-tapping correlation both appear to be in conflict with the present findings. Moreover, Harrison (1941) reported a correlation of 0.58 between reading and counting, in addition to a non-significant tapping-counting correlation.

It will be recalled that, in the case of Harrison's experiment, the counting task was somewhat different from that used by Allport & Vernon or the present investigator since, having counted to 50 by twos, the subject then counted backwards to two again. It is possible that counting in the reverse direction would constitute a more difficult task than counting forwards, and that subjects would have to concentrate more in order to produce the correct sequence of numerals. Counting forwards, on the other hand, is a highly-practised activity (particularly in childhood), and would be thought to be relatively automatic. It may be that a highly automatic task would be performed in a "rhythmic" style, but that such a method could not be adopted in a task which required more concentration (processing capacity). This may account for the discrepancy between the present results and those which Harrison reported.

However, this argument does not account for the discrepancy between the present results and those obtained by Allport & Vernon, since their task was very similar to the one employed here except that, at a later stage in the experiment, subjects also counted from 31 to 60. This task would, of course, include a greater proportion of predominantly three-syllable numerals (twenty-three, forty-six, etc.) than would the one used here (an argument which also applies to Harrison's version) and it is possible that this feature may have influenced the results. Clearly, a more detailed investigation of different versions of the counting-task would be in order. In counting from one to twenty, for example, is the mean speed (in numerals per second) lower with the predominantly bisyllabic 'teens than with the numbers below eleven (most of which are monosyllabic) or do subjects interpose pauses between the latter, in order to maintain an approximately constant rate of counting? Until detailed information is available on the manner in which people perform various versions of counting, we cannot be confident that they produce equivalent results.

The "cognitive" interpretation, then, would appear to encounter difficulties with Rimoldi's findings, Allport & Vernon's "verbal" interpretation would also be applicable to the R-cluster tasks, but would also meet with difficulties on account of Rimoldi's "marbles" task, However, it was earlier argued that Rimoldi's own interpretation of his Factor C is suspect, and the paucity of information provided by that author regarding the task in question makes interpretation of his data difficult.

Summary and Conclusions

1. The results of this experiment were in large part compatible with those of previous investigators in the field. Test-retest reliabilities were generally highly satisfactory, though one particular exception was noted. The intercorrelational data do not provide support for the hypothesis that there is a unitary "personal tempo", but neither is the hypothesis of extreme specificity compatible with the findings, since several significant correlations were obtained.

2. Two clusters of intercorrelated measures were discussed. The first comprised the tapping-tasks (including circle-dotting), and was designated the "T-cluster". The second consisted of the reading tasks and was designated the "R-cluster". Copying was also tentatively included in this cluster.

3. Possible hypotheses as to the sources of common variance within the clusters were discussed. It was argued that the most parsimonious account of the T-cluster was to suggest that each individual has a "preferred rhythm", which he adopts whenever the conditions of the task permit him to do so. It was suggested that the R-cluster might be interpreted as "cognitive" or "verbal", but neither of these suggestions appeared satisfactory. Further research is required here.

CHAPTER 4:

Experiment II

<u>Introduction</u>

The question of the relationship between "spontaneous" and "maximum" rates of performance is of considerable importance, because any such relationship should cast some light on the significance of the "spontaneous rate" measures which have been taken in experiments in this field. The "maximum" rate might be regarded as representing a physiological limit on the speed of performance, and any correlation between this rate and the "spontaneous" rate would demonstrate that one of the factors "setting" the latter is the physiological capacity of the relevant part of the body. Moreover, the present writer has on several occasions been in discussion with colleagues who have expressed serious doubts as to whether "spontaneous" and "maximum" instructions would lead to any differences at all in behaviour. It was seen in Chapter 2 that Fujita's (1970) investigation provides good grounds for such doubts with an "addition" task.

Two studies were reviewed in Chapter 2 in which the relationship between "spontaneous" and "maximum" rates of tapping had been studied. Har son's (1941) investigation cannot be regarded as wholly satisfactory because he employed an invariant order of presentation, and no mean speeds are reported. Mishima (1968) does report means, and these do suggest a higher tempo under the "maximal" than under the "congenial" instructions. However, there is always the difficulty that cross-cultural differences might exist, so that Mishima's results need to be supplemented by research in our own society. Moreover, Mishima's correlation is significant and negative. A significant negative correlation is a highly unusual finding in this field of enquiry, and it would, therefore, be very interesting if such a result could be replicated here. The following experiment was designed, therefore, in order to investigate the relationship between "spontaneous" and "maximum" rates of tapping.

Method

Design. In certain respects, the ideal design for this type of experiment would be one employing a completely random order of presentation of tasks. In such a design, "spontaneous rate" and "maximum rate" tasks would be intermixed, as was the case in Mishima's (1968) study. However, once the subject has been asked to perform a task at the "maximum rate", he is no longer completely naive with respect to the purposes of the task; he is aware that the tempo of performance is of interest to the investigator. It was thought that this awareness might affect subsequent "spontaneous rate" performance and that it would, therefore, amount to a contaminating variable. Hence, the experiment was divided into two halves, separated by a rest-interval of two to three minutes, with all of the "spontaneous rate" tasks being performed in the first half, and all of the "maximum rate" tasks being performed in the second half. Within each half, the order of presentation was completely randomised.

Because of the similarity of the tasks to be investigated, it was decided to introduce a battery of "buffer tasks" of various types in order to reduce the risks of "carry-over" effects, and to relieve monotony. Each experimental task was followed by one "buffer task" and, in addition, each half of the experiment began with such a task (which acted, therefore, as a "warm-up item). Order of presentation within the "buffer" battery was also randomised.

Experimental Tasks.

1. Finger-tapping (FL and FR). These tasks have already been encountered in Experiment I. In the present case, however, the sampling-period was 20 seconds (as distinct from the 30 seconds used previously). The technical factors which led to the adoption of this

shorter sampling-period will be explained when describing the computing system.

2. Tapping-whilst-reading (FLR, FRR). These were also finger-tapping tests, but the subject was required simultaneously to read aloud. The tasks were included as a check upon the procedure of Harrison & Dorcus (1938), whose measure of tapping-rate was obtained while subjects were simultaneously reading. It was desired to know whether tapping-rates observed under such conditions were comparable with those obtained in tasks FL and FR.

Four short passages were selected for the reading material. These were assigned at random to the four tapping-whilst-reading tasks (FLR and FRR were performed both in the "spontaneous" and "maximum" conditions). In the "maximum rate" condition, subjects were only required to tap as quickly as possible: reading was to be at the normal rate.

3. Alternate tapping (AT). In this task, the subject used both hands, and depressed them alternately (left, right, left, right, etc.). A separate response-button was provided for each hand.

4. Unison tapping (UT). In this task, both hands were again used, but in such a way that the two response-buttons were pressed and released simultaneously,

"Buffer" Tasks.

1. Block Designs (eight tasks). Eight designs, each consisting of 12 wooden blocks, were devised by the experimenter. Coloured diagrams of the designs were prepared and mounted on to typing paper size A4. These could then be included as pages of the loose-leaf instruction book.

2. Reading aloud (R1 and R2). Two passages were selected for reading and were presented in typewritten form to the subject.

Passage R1 was identical to that used in Task Sc of the preceeding investigation. Passage R2 was taken from a well-known psychological work (Miller, Galanter and Pribaum, 1960). Unlike the remaining "buffer tasks", speeds of performance of R1 and R2 were recorded, since it was desired to know whether the negative reading-tapping correlations obtained in the previous study could be replicated.

3. Copying sentences (four tasks). Four ten-word statistical approximations to English, two of the first order, and two of the fifth, were selected for these tasks. They were presented on appropriate pages of the instruction-book, and subjects were provided with paper and pencil with which to copy them.

Assignment of Tasks to the Two Parts of the Experiment. Each of the Experimental tasks was presented once in the "spontaneous rate" part and once in the "maximum rate" part. For purposes of randomisation, the "buffer" battery was treated as a whole, and the first seven tasks to occur in the randomisation procedure were assigned to the first half of the session, whilst the remainder were assigned to the second half.

Subjects. Subjects were 10 male and 14 female students studying a variety of subjects at the University of Leicester. All were naive with respect to the aims of the investigation.

Apparatus and Materials. Two response-buttons of microswitch type were mounted on to a stiff board, which measured approximately 2ft. by 2ft. 6 ins. The buttons were placed towards the rear of the board, one on the left and one on the right, so that they were in convenient positions for operation by the two hands.

Also placed on the board was the subject's communication apparatus. This consisted of a 500 Hz. tone-generator and a Morse key. These were wired in parallel with identical apparatus at the experimenter's station, and the system provided two-way communication between experimenter and subject. Finally, a microphone was attached to the board, and this was connected to an audio cassette recorder at the experimenter's station. The board was then placed on top of the subject's desk.

In a monitoring room adjacent to the subject's cubicle was placed a television camera, which was connected to a monitor at the experimenter's station. The purpose of this apparatus was to permit visual monitoring of the progress of the subject, and to permit the experimenter to see when problems had arisen. The lens of the camera was clearly visible from the subject's position, and the purpose of the system was explained before the experiment began.

Computing System. The principal component of the hardware system was a Digital Equipment Corporation PDP Lab 8/E computer, with 8k of store, real-time clock and analogue-to-digital converter (ADC) and a magnetic tape (DECtape) back-up storage system.

All programmes of the software system were written by the investigator in various versions of FOCAL (the interactive programming language of the PDP 8 family of computers). These programmes will be briefly described here, and full listings will be found in Appendix III.

1. RANGEN (RANdom number GENerator). The purpose of this programme was to randomise the order of presentation of both experimental and "buffer" tasks, and to output the numbers in the alternating scheme, whereby each experimental task was followed by one "buffer" task. Computer generation of the presentation orders was adopted because of the relatively large number of tasks which were used in this experiment. There was the additional advantage that, if the randomised numbers were output on paper tape, a second programme could be written which would translate the output of

RANGEN into easily-understood abbreviations to represent the various tasks. Thus, the normally tedious procedure of determining the order of presentation could be almost completely performed by machine.

2. INTRAN (INTerpreter of RANdom numbers). This was the programme which translated the output of RANGEN into letter-abbreviations. The abbreviations were then typed on the teleprinter. All of the presentation orders which would be required in this investigation were determined with a single run of RANGEN and INTRAN before the testing of subjects began.

3. WRIDEX (WRIter of inDEX). The programme which controlled the computer during the experimental session (see below) required for its proper functioning to "know" the order in which each subject was to perform the experimental tasks. From the programming point of view, the simplest method of providing the programme with this information would be to arrange for the experimenter to type in characters representing the tasks at the beginning of each experimental session. The disadvantage with such a method, however, is that errors may be made, particularly when the late departure of a previous subject has put testing behind schedule. It was decided, therefore, to arrange the system so that the computer could automatically load the presentation order for each subject, thus avoiding the possibility of human error. In order to do this, all of the orders produced by RANGEN were loaded on to an area of magnetic tape which was designated the "Index". Also stored in the Index was a register which kept a record of the number of subjects who had been run (and, therefore, the number of presentation orders which had already been used). At the beginning of an experimental session, the run-time programme first operated to

read from the Index the next presentation order. This was then automatically loaded into core, so that the information was available to the programme when required.

The purpose of the WRIDEX programme was to initialise the Index before the testing of subjects began. The initialisation consisted mainly of the loading of presentation orders, but the programme also served to zero the various registers which were also stored in the Index.

4. RUNTIM (RUNTIMe programme). This was the programme which controlled the computer during the experimental sessions. Simple electronic circuitry was arranged to switch the voltage output from the response buttons from 1.5 Volts (button at rest) to 0 Volts (button depressed). These voltages were fed into the analogue-to-digital converter, where they were sampled at a rate of 100 samples per second. In tasks AT and UT, where both response buttons were used, multiplexed sampling was performed, with the right channel being sampled approximately 70 microseconds after the left.

Data were stored in digital form, with the "button at rest" condition being represented by a value of 511_{10}, and the "button depressed" condition being stored as zero. Values between 0 and 511 were occasionally recorded because of the finite transit-time of the microswitches, but values below (and including) 250_{10} were arbitrarily defined as "button pressed" readings.

The configuration of the computing system was such that, at the sampling-rates used here, continuous recording of one channel could be maintained for a maximum of 20.48 seconds, whilst multiplexed recording of two channels could be continued for a maximum of 10.24 seconds. It was decided, therefore, that the sampling-period for the tapping-tasks would be 20 seconds (as distinct from 30 seconds used in Experiment I). In tasks AT and UT, this was accomplished by taking two ten-second "burst-samples". Following each sample, a period of approximately 0.5 seconds was required for the data to be "dumped" on magnetic tape. During this period, sampling was suspended.

One factor which must be taken into account when designing computer-controlled experiments in Psychology is that the subject might make errors, and this possibility must be catered for by flexibility in programming. In the present case, for example, the subject might inadvertantly press one of the response-buttons (for example, by resting the instruction book on it) at a time when he was not actually performing a tapper-task. Had the runtime programme been arranged so that it "waited" for a button to be pressed and then recorded the voltages for the specified period, such spurious pressing of the button would lead to the recording of a false trial. In order to circumvent this difficulty, it was arranged that the experimenter should signal to the computer when the subject was about to begin a tapping-task. This presented no difficulties, because the television monitor placed in the computer room provided the experimenter with an unrestricted view of the subject's desk. The beginning of a tapping-task was signalled to the computer by pressing the space-bar on the teleprinter.

It was observed during Experiment I, particularly during the initial tapping task, that subjects would often press the key once and then pause to re-read the instructions. Such a pattern of response presented no difficulties with the aural methods of recording which were used in that investigation, but would have led to spurious results with the computer recording system had allowances not been made for it. For example, had the programme been written so that, once the experimenter depressed the space-bar, the computer "waited" for a response-button to be pressed and then proceeded with a burst-sample, any such hesitation would result in the recording of a spuriously low tapping-rate during that sample.

To obviate this difficulty, it was arranged that there would be a 5-second delay between the initial pressing of a response-button and the beginning of the burst-sample. Thus, if the subject performed the task without any initial hesitation, the length of a tapping-trial would be approximately 25 seconds, because the initial 5 seconds of tapping would not be recorded.

A further provision for the possibility of errors was that the runtime programme was so written that the experimenter could resume control when necessary. The various registers involved in the running of the programme could be reset, so that a trial could be repeated if this was necessary.

5. COM 1 (COMpressor stage 1). As explained above, the data were originally stored in digital form on magnetic tape. The "original form" column of Table 4.1 is a typical section of such a recording. "Runs" of "511" represent periods during which the button was continuously in the "at rest" position, whilst runs of "0" represent periods during which the button was continuously depressed. Though this form of storage is convenient in the short term, it will be seen that it is nevertheless highly redundant. Had the data from this experiment been permanently stored in this form, a very large amount of magnetic tape would have been required. The compressor programmes were designed, therefore, to store the data in a more economical form. The mode of operation of COM 1 is depicted in Table 4.1. It will be seen that each run of "511" or "0" was replaced by a single number,, representing the number of readings in the run. (Since the original

TABLE 4.1: Action of the Compressor Programme (COM1)

Original Form	Compressed Form	
0	4	
0	3	
0	5	
0	3	
511	511	
511	511	
511	511	Wasted
0	0	
0	0	
0	0	
0	0	
0	0	Space
511	511	
511	511	
511	511	
etc.	etc.	

sampling rate was at a rate of 100 per second, these run-lengths may be directly read as times in hundredths of a second.)

6. COM 2 (COMpressor stage 2). Though COM 1 translated the original data into a more economical form, it did not in itself result in a saving in the amount of tape required, because sections carrying the translated data were separated by long sections which still contained the (now no longer required) original data. (This is labelled "wasted" in Table 4.1.) The function of COM 2 was to move the sections of translated data closer together, so as to achieve the maximum economy in the utilisation of tape.

7. EXPAN (EXPerimental ANalysis programme). The aim of this programme was to compute, for each task performed by each subject, the mean inter-tap interval and the standard deviation of the successive intervals about that mean. These data were printed on the teletype unit, but the means were also output on paper tape, so that they could be directly input to the intercorrelational programme.

Procedure. Before the subject's arrival, the experimenter prepared the instruction-books. (A separate book was used for each half of the experiment, and the second was not supplied until it was required, in order to prevent the subject from prematurely seeing the "maximum rate" instructions for Part 2). The instructions and materials which would be required in the first part were then placed on the subject's desk, and audio and video equipment was prepared and checked. The computing system was then assembled, and the runtime programme loaded and started (so that the presentation order was read). The experimenter then went to await the subject's arrival at a prearranged meeting place.

When the subject arrived, he was conducted to the experimental cubicle, where the television camera was pointed out, and its purpose explained. It was also explained that the experimenter would not remain in the cubicle during the session, but that his presence was required in the computer room. Any questions were then answered, and the experimenter then went to the computer room, from where the proceedings were controlled by means of the communication system which was described above.

Upon completion of the first half of the session, a rest interval of two to three minutes was given before the experimenter entered the subject's cubicle with the instructions for the second half. Upon completion of the second half, the purposes of the investigation were explained, and any further questions were answered. The subject was then thanked for his co-operation and asked not to divulge any information regarding the experiment to his fellows. Following the subject's departure, the compressor programmes were run and preparations were made for the succeeding subject.

Results

Mean and standard deviations of rates of performance are presented in Table 4.2. Data for the tapping-tasks are in cycles per second, where "cycles" for the single-handed tasks has the same meaning as that employed in Experiment I. For tasks AT and UT, a cycle was defined as the period between successive pressings of the left-hand response button. Data for the buffer tasks, R1 and R2, are in terms of the time (in seconds) required to complete the task.

It will be seen from Table 4.2 that the mean rates of performance were considerably faster under the "maximum rate" instructions than under "spontaneous rate" instructions. These differences were tested by means of t-tests for correlated means, and the results of this analysis are also presented in Table 4.2. It will be seen that all of the resulting values of "t" are highly; significant.

It is interesting to compare the rates of tapping obtained here in tasks FL and FR with those reported in Experiment 1. Table 4.3 presents this comparison, with the data from Experiment I converted into taps per second to achieve comparability with the 20-second trials used in the present study. It will be seen that the present means are somewhat lower than those obtained previously. These differences were also tested by means of t-tests (for independent samples). Performance was faster in Experiment I than in the present study both for left ($t = 2.97$, $df = 37$, $p < .01$) and right ($t = 2.71$, $df = 37$, $p < .01$) hands.

It is interesting to note that the number of cycles of AT (both in the "maximum" and in the "spontaneous" condition) is considerably lower than the number of cycles in the single-handed tasks. Since, by definition, a cycle of AT consisted of two taps (one with each

TABLE 4.2: Experiment II, Means and Standard Deviations

	FL	FLR	FR	FRR	AT	UT	R1	R2
Spontaneous								
Mean	1.28	1.28	1.31	1.29	0.83	1.30	130.37	115.50
SD	0.69	0.74	0.70	0.76	0.37	0.68	20.06	13.78
Maximum								
Mean	4.00	3.69	4.26	3.96	2.42	3.59		
SD	1.21	1.27	1.45	1.36	0.90	1.39		
t	12.76	10.20	11.57	10.60	9.27	8.95		

Data for the tapping tasks are in taps per second.

Data for the reading tasks are in seconds.

hand, it is interesting to enquire whether the number of <u>taps</u> produced in AT was equal to the number of taps produced in the single-handed tasks. In order to answer this question, each subject's score in AT was first converted into taps per second by doubling. For both "spontaneous" and "maximum" conditions, the transformed scores in AT were then compared (by means of t-tests for correlated means) with the scores on the single-handed tasks. For the "spontaneous" condition, the number of taps produced in AT was not significantly different from the number produced in either FL or FR ($t = 0.35$ and 0.16 respectively). For the "maximum rate" condition, however, the number of taps produced in AT was significantly greater than either the number produced in FL ($t = 2.95$, $df = 23$, $p < .01$) or the number produced in FR ($t = 3.21$, $df = 23$, $p < .01$).

Table 4.4 presents a matrix of the intercorrelations of the tempo-measures which were taken in this experiment. It will be seen that, within instruction-conditions, there are high positive correlations between tapping-tasks, including the measures of tapping-rate which were taken when subjects were simultaneously reading. However, none of the coefficients relating "spontaneous" to "maximum" performance is statistically significant, though they are all positive.

The predominance of negative correlations between rates of tapping and of reading has not been replicated here. Indeed, it is noteworthy that the correlations relating reading rate to the "spontaneous" performance of FR are both positive and statistically significant ($p < .05$).

During the course of the experiment, casual observation suggested some degree of interference between reading and tapping in tasks FLR and FRR. The tapping appeared less regular, and the

TABLE 4.3: Comparison between Experiments I and II

Experiment I	FL	FR
	1.9147	2.0313
Experiment II	SFL	SFR
	1.2777	1.3105

TABLE 4.4: Experiment III, Matrix of Intercorrelations

	SFLR	SFR	SFRR	SAT	SUT	MFL	MFLR	MFR	MFRR	MAT	MUT	R1	R2
SFL	834	892	888	815	762	386	276	347	330	382	332	224	239
SFLR		783	907	763	719	347	183	262	260	249	250	208	231
SFR			870	777	656	244	213	338	294	320	286	444	449
SFRR				725	628	285	183	287	235	236	188	218	289
SAT					703	311	103	203	195	269	333	263	291
SUT						325	111	159	150	346	330	288	206
MFL							743	581	648	691	688	-007	-050
MFLR								765	901	834	681	187	117
MFR									758	825	861	070	-003
MFRR										776	666	162	107
MAT											838	304	246
MUT												128	-038
R1													828

For DF = 22, a value of 0.404 is required for significance at the .05 level, and a value of 0.515 for significance at the .01 level

Decimal points and leading 0's have been omitted for clarity reading-voice was often monotonous. Though it is not possible in the present experiment to quantify the latter variable, it is possible to investigate the regularity of tapping, because the computer recorded every inter-tap interval. It was decided, therefore, to compare the standard deviations of inter-tap interval when simultaneously reading and when not reading.

The analysis was performed for sides and instruction-conditions separately. Thus, the SD's obtained in "spontaneous" performance of FL were compared with those obtained in "spontaneous" performance of FLR, and so on. The analysis consisted of counting the number of subjects who registered a greater SD when reading than when not reading. The significance of these results was then assessed by means of the Binomial test.

The results of this analysis are set out in Table 4.5, from which it may be seen that simultaneous reading led to less regular tapping in the "spontaneous" condition for both right and left hands. For "maximum rate" tapping, the effect of reading was in the same direction, but it was not statistically significant.

TABLE 4.5: Numbers of Subjects having Greater Inter-tap Variances when Reading than when not Reading (N = 24)

Hand	"Spontaneous"	"Maximum"
Left	21 ($p < .01$)	17 ($p > .05$)
Right	23 ($p < .01$)	15 ($p > .05$)

Discussion

It will be interesting first to consider the two points of discrepancy between the present study and Experiment I. The first of these concerns the mean rates of finger-tapping. It will be recalled that the mean rate obtained here under the "spontaneous rate" instructions was lower than that observed in the preceeding investigation. Several possible reasons might be suggested for this discrepancy.

One clear possibility is that the discrepancy might be due to the fact that two independent groups of subjects were used in the two investigations. Though this possibility cannot be ruled out, it would be interesting to consider whether there might be any differences in the procedures and materials used which might be thought responsible for the discrepancy.

One plausible suggestion is that the different types of response-key may be responsible. In Experiment I, subjects tapped with a Morse key, whilst those in the present study used a micro-switch similar to the one employed in the foot-switch of Experiment I. It is interesting, therefore, to recall that the latter type of switch was cited as a possible cause of the fact that, in Experiment I, subjects tapped at a lower rate with the toes than with the fingers. For example, it may be that what the subject "prefers" in these tasks is not the actual rate of depressing of the key, but the rate of auditory feedback. The microswitches produced two audible clicks per tapping-cycle, whereas the Morse key produced only one. It may be, then, that subjects in the present experiment maintained the same "preferred rhythm" of auditory feedback by depressing the "double-acting" switches less frequently.

One obvious objection to this argument is that it would appear to imply that the rates of key-depression which were obtained here should have been approximately half the rates obtained in Experiment I. It would be possible, however, to counter this objection by means of the casual observation that (as was the case in the toe-tapping tasks of Experiment I) subjects' reactions to the double-acting switches differed somewhat. Some, for example, tapped on the buttons with a ballistic movement, so that the two clicks were barely discerbable. Others pressed and released the buttons in a more deliberate style, clearly producing two clicks per cycle of tapping. The effect of this variety of ways of operating the buttons would be that, if subjects were maintaining a "preferred rhythm" of auditory feedback, the rate of performance obtained in this study (in terms of key-pressings per unit of time) would be at some point between the tempo observed in Experiment I and a rate of half that value.

The discrepancy between Experiments I and II cannot be regarded as giving firm support to the view that the rate of auditory feedback is "preferred", however, because there are other differences between the two types of response-buttonwwhich might account for the discrepancy. The spring-loading, for example, is most unlikely to be the same, and the hand-position may also be different. (Many of the subjects in Experiment I appeared to grip the knob of the Morse key between the fingers, but the microswitch button was too small to permit it to be gripped). It must be said, however, that the importance of such factors as hand-position and spring-loading appears to be minimal, because of the relationship which was obtained in Experiment I between key-tapping and circle-dotting, between which tasks there are many differences of detail.

Further research would be required to establish the validity of the "auditory feedback" hypothesis. One method which might prove useful would be to employ a single response-key which could be connected to control electronically generated feedback. The relationship between key-movement and feedback could then be manipulated (perhaps with the aid of a computer), to produce a variety of numbers of clicks per movement (or movements per click). The feedback could be delivered through headphones, with masking to prevent the subject from hearing direct auditory feedback produced by the movements of the key.

The second discrepancy between Experiments I and II lies in the correlations between the rates of tapping and reading. The predominance of negative correaltions has not been repeated here, and it was seen above that the positive correlations between reading and the "spontaneous" performance of FR were actually statistically significant. This latter result may be of interest from the point of view of cerebral dominance, since both language and movements of the right hand side of the body are largely controlled by the left cerebral hemisphere. However, Roberts and Gregory (1973) have suggested that "rhythmic tapping" with either hand might be controlled by the right hemisphere. It should be pointed out, however, that the task on which their conclusion was based was somewhat different from FR, since subjects were instructed to tap in a particular pattern ("four taps followed by two") and also to perform the task "fairly quickly". It is not certain, therefore, whether their conclusion would also be applicable to the regular "spontaneous rate" tapping which has been performed in these studies.

It is clear from the mean rates of tapping set out in Table 4.2 that the two types of instructions do lead to different tempi of performance. From the intercorrelations set out in Table 4.4, it would appear that there is very little common variance between the factors which "set" the tempo in the two cases.

The intercorrelational results, then, correspond to Harrison's (1941) data, rather than to Mishima's (1968) findings. Several factors may be suggested in order to account for the discrepancy between Mishima's findings and the results obtained in the Western investigations. Cultural differences cannot be ruled out, especially in view of the positive findings on regional differences which were reported in Chapter 2. In addition, Mishima's subjects were children, whereas both Harrison and the present investigator employed adult subjects. Finally, different trial-lengths were used in the three investigations (Harrison, 30 seconds; Mishima, 10 seconds; present investigator, approximately 25 seconds). In respect of this factor, the design of the present investigation is closer to Harrison's than to Mishima's.

It is interesting to compare the means obtained under the "maximum rate" instructions with the values reported by Bryan (1892) when reviewing early studies on tapping-speed. Though the values varied somewhat with different investigators, the obtained means were generally higher than those reported here. In one study, for example, a mean of approximately six taps per second was obtained. In this case, 300 taps were performed, so that the trial-length was on average approximately twice the length used here. It would not be appropriate, therefore, to argue that the "maximum rate" obtained here was comparatively low because the trials were comparatively long. A further study reported by Bryan resulted in mean rates of performance of a rhythmic task of 10 - 11 repetitions per second after practice.

Inasmuch as the present results may be compared with the early work, then, it would appear reasonable to suggest that the subjects used here may not have been performing at a "maximum rate" which was limited by physiological constraints. If this is true of other experiments of this type using "maximum rate" instructions, then one aspect of the results of Craik and Sarbin (1963) might be more readily interpreted. It will be recalled that, in their study, covert manipulation of the clock-rate affected tempo of performance, both in a self-paced and in a "maximum rate" task. It is very difficult to envisage how increasing the clock rate could increase a physiological "maximum rate", but it is not difficult to envisage how such an increase might be brought about if subjects in the "maximum rate" condition were merely performing at a speed which was higher than the "spontaneous" rate, but still short of a physiological limit.

Considering next the tasks of FLR and FRR, it will be seen from Table 4.4 that, as far as intercorrelational structure is concerned, the results obtained with these tasks are comparable with those obtained with tasks not requiring simultaneous reading. However, as was noted earlier, a certain degree of interference between the tasks was suggested.

The suggestion that the reading-voice was made more monotonous by the necessity to tap remains a casual observation, unaided by objective empirical data. It would, indeed, be difficult to quantify such a variable, except perhaps by some such method as judges' ratings. Such a method would not be appropriate with the recordings obtained in this study, however, because the sounds of tapping were clearly audible on the cassette recordings of FLR and FRR. It would not, therefore, be possible to employ the necessary blind sooring techniques. Nevertheless, such a method might fruitfully be employed in an investigation expressly designed to study the effects of tapping on reading.

It was seen above that the casual observation of a decreased regularity of tapping in FLR and FRR was corroborated by the data regarding the inter-tap intervals. That some difference was shown in Table 4.5 between "spontaneous" and "maximum" conditions might be taken to suggest that the neural organisation of tapping differs in these two instruction-conditions. For example, it might be argued that the decreased regularity of "spontaneous rate" tapping suggests that, in this case, reading and tapping are competing for central processing capacity. In the case of "maximum rate" tapping, however, it would be said either that this is not the case, or that it is true to a lesser extent. Thus, it might be that the "spontaneous rate" tapping requires monitoring at a relatively high level of the nervous system, whilst the "maximum rate" tapping does not.

This interpretation is somewhat speculative, and the present investigation was not, in any case, designed to cast light on such a possibility. Nevertheless, this suggestion warrants further investigation. It would be valuable in such research to attempt to devise an interfering task which was more easily quantifiable than reading, however, or recording procedures would have to be designed in such a way as to permit blind scoring techniques to be adopted.

A final point which is worthy of note is the pattern of results which has been obtained with task AT. At the "spontaneous" tempo, the number of taps produced in this task was statistically equal to the number of taps produced in the single-handed tasks though, in order to achieve this, each hand would have to be working at half the rate in AT as in the single-handed tasks. This result appears to invalidate one possible suggestion, to the effect that the factors or mechanisms which "set" the rate of key-tapping under these conditions are local to the hand or arm concerned. (Though many specific hypotheses could be put forward, one possibility might be that the tapping-rate is governed by the build-up of fatigue in the limb.) Such "local" hypotheses would appear no longer to be appropriate in the light of these results, which suggest that the activity of the two arms is integrated at some higher level of the nervous system.

Summary and Conclusions

1. In this experiment, "spontaneous" and "maximum" rates of finger-tapping were investigated. The two types of instructions did lead to significant differences in performance. Though all of the correlations were positive, none of them was statistically significant, suggesting that there is little or no common variance between the factors which "set" tempo in the two cases.

2. It was noted that the mean rates obtained here were considerably lower than those reported by early workers in the field. It would appear that, at least in this experiment, "maximum rate" performance cannot be regarded as representing a physiological maximum. This reinforces the importance of quoting mean tempi in studies of this type.

3. The predominance of negative correlations between reading and tapping which was obtained in Experiment I has not been repeated here.

4. Mean rates of finger-tapping obtained in this experiment were significantly lower than those reported in the previous study. The possibility was suggested that the response-buttons used here may have been responsible for the discrepancy.

5. As far as intercorrelational structure is concerned, tasks requiring simultaneous reading with tapping are comparable with those not requiring reading. However,

there is some interference between the two activities in the "spontaneous rate" condition. That no significant effect was obtained in the "maximum rate" condition suggests that the neural organisation of the behaviour may be somewhat different in the two cases. It was suggested that further research might be carried out to investigate this possibility.

6. Results with the alternate tapping task suggest that the factors which "set" tapping rate are not local to the limb in question. Some integration of behaviour appears to be taking place at a higher level in the nervous system.

CHAPTER 5:

Experiments III & IV

Experiment III

Introduction

In discussing the results obtained in Experiment I with finger-tapping and circle-dotting, it was argued that the most parsimonious interpretation would be that each subject has a "preferred rhythm", and that he adopted this rhythm in both tasks. This account was contrasted with the suggestion that the rate of reciprocal tapping may have been determined or limited by task-difficulty, as Fitts (1954) has shown to be the case with "maximum-rate" performance. It was argued, however, that such a suggestion could not provide as parsimonious an account as the "rhythmic" hypothesis.

One of the aspects of the results which led to this conclusion was the fact that subjects did not use the whole of the available target-width, but distributed their dots more closely about the centres than was strictly necessary for errorless performance. As Welford (1968) pointed out, the consequence of such under-use is that the nominal Index of Difficulty calculated from Equation 3.1 is an underestimate of the amount of information actually transmitted by the subjects; in effect, they were aiming at targets which were narrower than those specified by the experimenter. Welford proposed an effective Index of Difficulty (ID') such that:

$$ID' = \log_2 (2A'/W') \text{ bits per response} \qquad \text{..... (5.1)}$$

where A' and W' are parameters obtained empirically by actually measuring the distributions of dots produced by the subjects. (The exact method of measurement will be described when reporting the results of Experiment III).

It should be noted at this stage that Welford proposed several modifications of Fitts' equations, but concluded that Fitts' original version was "right in principle". More recently, Bainbridge & Saunders (1972) have reported data which supports Fitts' formulation rather than Welford's. However, the exact mathematical form of the relationship between speed and accuracy is not important in the present context, so that equations of the form originally suggested by Fitts will be used in the interests of simplicity.

When Welford's effective Index (from Equation 5.1) was calculated for the target-sheets used by the subjects in Experiment I, it was found to have values of 3.69 and 3.65 bits per movement for Sessions 1 and 2 respectively. These values are to be contrasted with the nominal difficulty of 2.585, and they indicate that subjects were using only approximately half of the available target-width whilst performing the circle-dotting task.

If subjects were effectively transmitting 3.6 to 3.7 bits per movement in performing the circle-dotting task at the "preferred rhythm", then it might be argued that they should be able to perform at that rhythm on targets having a nominal difficulty of that order, without a significant increase in errors. (The dot-distribution obtained in Experiment I would "fit" into such targets.) For example, they should be able to perform at the "preferred rhythm" on targets having a nominal ID of 3.585, However, the results of Experiment I would suggest that any further appreciable increase in difficulty (eg., to ID = 4.585) would be accompanied by a significant increase in errors at the "preferred rhythm". (The dot-distribution obtained in Experiment I would "spill out" of such targets.) Empirical investigations would be required to determine the response of subjects to such targets, but one clear possibility is that they would choose to perform at a rate below the "preferred rhythm" in order to maintain a reasonable standard of accuracy. The actual outcome would depend on the relative pay-offs of speed and accuracy.

If it is argued that targets of ID = 2.585 are sufficiently low in difficulty tp permit the "preferred rhythm" to be adopted, then it would follow that targets of a lesser degree of difficulty (eg., ID = 1.585) would also be sufficiently low in difficulty for that rhythm to be adopted. Thus, the same rate of performance (ie., the "preferred rhythm") should be observed at ID = 1.585 as at ID = 2.585.

The present interpretation, then, leads to predictions that, in "spontaneous-rate" performance, the same speed (the "preferred rhythm) will be observed at all levels of difficulty below ID = 3.6 to 3.7. This clearly contrasts with the results obtained by Fitts with "maximum rate" instructions, where increases in speed were obtained with every decrease in difficulty. It was considered worthwhile, therefore, to test these predictions in the following experiment, by observing prrformance on targets at various levels of difficulty.

Method

Design. In discussing the design of this experiment, it will be helpful to provide an outline of Fitts' design. He investigated four values of A (16", 8", 4" and 2") and four values of W (2", 1", $\frac{1}{2}$" and $\frac{1}{4}$"). These provided 16 target-combinations and values of ID from 1 to 7. The order of presentation of the combinations was determined by constructing a 16 x 16 Latin Square and by assigning one subject to each row of the square.

Lack of technical facilities at the time of performance of the present study led to the adoption of a paper-and-pencil version of the task, rather than Fitts' electrical apparatus. (Welford, 1968, actually prefers the paper-and-pencil version since it provides information on the distribution of hits, which was not available in Fitts' study). In the interests of consistency of production, it was decided that the target-sheets should be duplicated and this decision imposed an upper limit on the value of A which could be investigated, since the largest duplicating paper available was of size A4. Three values of A were chosen: 8", 4" and 2".

Considerations of ease and consistency of production led to the adoption of "pseudo-rectangular" targets (pairs of parallel lines), which were made by means of the underscoring key of a typewriter. The inter-line spacing of this machine then determined the values of W which were available. These were: 1.33", .66", .33" and .16". Thus, the experiment provided 12 combinations of A and W and values of ID from 1.585 to 6.585. The order of presentation of the combinations was randomised for each subject.

It will be recalled that task CD of Experiment I was performed significantly more quickly in the retest than in the original session. It was suggested that uncertainty on the part of the subjects as to whether they had correctly interpreted the instructions may have led to the adoption of a hesitant approach in the first session. It was decided, therefore, that subjects would be given the opportunity to perform the task (and thereby to assure themselves that they knew how to perform it) before the experimental trials began. Two one-minute "practice" trials were provided for this purpose. The experimental trials were each 30 seconds long.

Eight target-combinations were used for the practice trials, employing the two extreme values of A (8" and 2") and the four values of W. Each subject performed in both practice trials with the same target-combination (though two separate duplicated sheets were provided). Two subjects were randomly assigned to each of the eight target-combinations used for practice.

The experiment, therefore, had a 4 (practice W) x 2 (practice A) x 4 (experimental trials W) x 3 (experimental trials A) design. Each subject performed with each of the 1112 target-combinations in the experimental trials, so that there were repeated measures over the last two factors of the design.

In order to maintain motivation and to reduce "carry-over" effects between adjacent tasks, it was decided to interpose an irrelevant activity between trials of the dotting task. Following each trial (including the practice trials, but excluding the final experimental trial), the subject was presented with a pair of cart oon-jokes, and his task was to indicate (on a slip of paper provided for the purpose) which of the two he found the more amusing. It was not deemed necessary to vary the order in which the cartoons were presented.

<u>Apparatus and Materials.</u> 2 practice target-sheets, clearly labelled, and 12 experimental target-sheets (also labelled) were

placed on the subject's desk, in the order in which they would be required. A pencil was also provided. Instructions (see Appendix I) were typewritten and assembled in a loose-leaf book, together with the cartoons to be judged.

A concealed audio cassette recorder was used to record the sounds made by the subjects' dotting. Speed of performance was ascertained from the recordings when the subject had departed.

Subjects. 16 undergraduate students, 9 male and 7 female, served as subjects in this investigation. They were enrolled in a nine-week course in introductory Psychology which was being taught by the investigator at the University of Keele. All subjects were naive with respect to the aims of the investigation.

Procedure. Limited laboratory facilities necessitated that the experimental sessions be carried out in the investigator's office at the University of Keele. The subject sat at a desk, facing a wall. Throughout the session, the experimenter sat at his desk behind (and out of sight of) the subject, and at a distance of approximately eight feet. The subject was told to ignore the experimenter, who would be "just reading". He was then asked to read the introductory instructions, and the directions for the first trial of reciprocal tapping.

Before commencement of the first trial, the subject was asked to demonstrate the way in which he was to perform the task, so that it could be determined whether or not he had correctly interpreted the instructions. If this proved not to be the case, further oral instructions were given, but the experimenter never demonstrated the task. When the investigator was satisfied that the subject understood the task, the session began.

Upon completion of the experimental session, the aims of the investigation were divulged, and the subject was sworn to secrecy and thanked for his co-operation

Results

The raw data obtained in this experiment (see Appendix II) were subjected to a 4 x 2 x 4 x 3 Analysis of Variance, with repeated measures over the last two factors. The factors and their levels are summarised in Table 5.1.

Table 5.2 presepts the treatment-means for each of the factors, summed over the remaining three factors. The Analysis of Variance is summarised in Table 5.3. Four of the F-ratios are statistically significant:

(1). Factor 1: $F = 4.13$; df = 3.8; $p < .05$.

(2). Factor 3: $F = 21.09$; df = 3.24; $p < .01$.

(3). Factor 4: $F = 26.00$; df = 2.16; $p < .01$.

(4). 1 x 2 x 3 interaction: $F = 3.71$; df = 9.24; $p < .01$.

The 1 x 2 x 3 interaction is depicted graphically in Figure 5.1. On inspection, it will be seen that the major difference between 5.1(a) and 5.1(b) lies in the position of the line representing Level 1 of Factor 1. Since each of these lines is based upon two subjects only, and since there were no repeated measures over Factor 2, it would not appear unreasonable to ascribe this interaction to the chance assignment of two particularly slow subjects to the practice-condition involving Level 1 of Factor 1 and Level 2 of Factor 2.

The means for Factor 1 (Table 5.2), which differ at the .05 level of confidence, appear to represent a tendency for th e narrower practice-targets to result in slower performance in the experimental trials. The mean associated with Level 2 of Factor 1, however, is difficult to interpret; it is by no means obvious why that particular width of practice-target (.66") should result in especially rapid subsequent performance. In view of the fact that there were no

TABLE 5.1: the Factors and their Levels, Experiment III

Factor	Level 1	2	3	4
1 - Practice W	1.33	0.66	0.33	0.16
2 - Practice A	8.00	2.00		
3 - Exp. trials W	1.33	0.66	0.33	0.16
4 - Exp. trials A	8.00	4.00	2.00	

Note: the measurements are in inches

TABLE 5.2: Experiment III, Treatment Means

Factor	Level 1	2	3	4
1	57.90	80.52	53.73	44.02
2	60.84	57.24		
3	70.71	62.77	55.87	46.81
4	50.34	59.27	67.52	

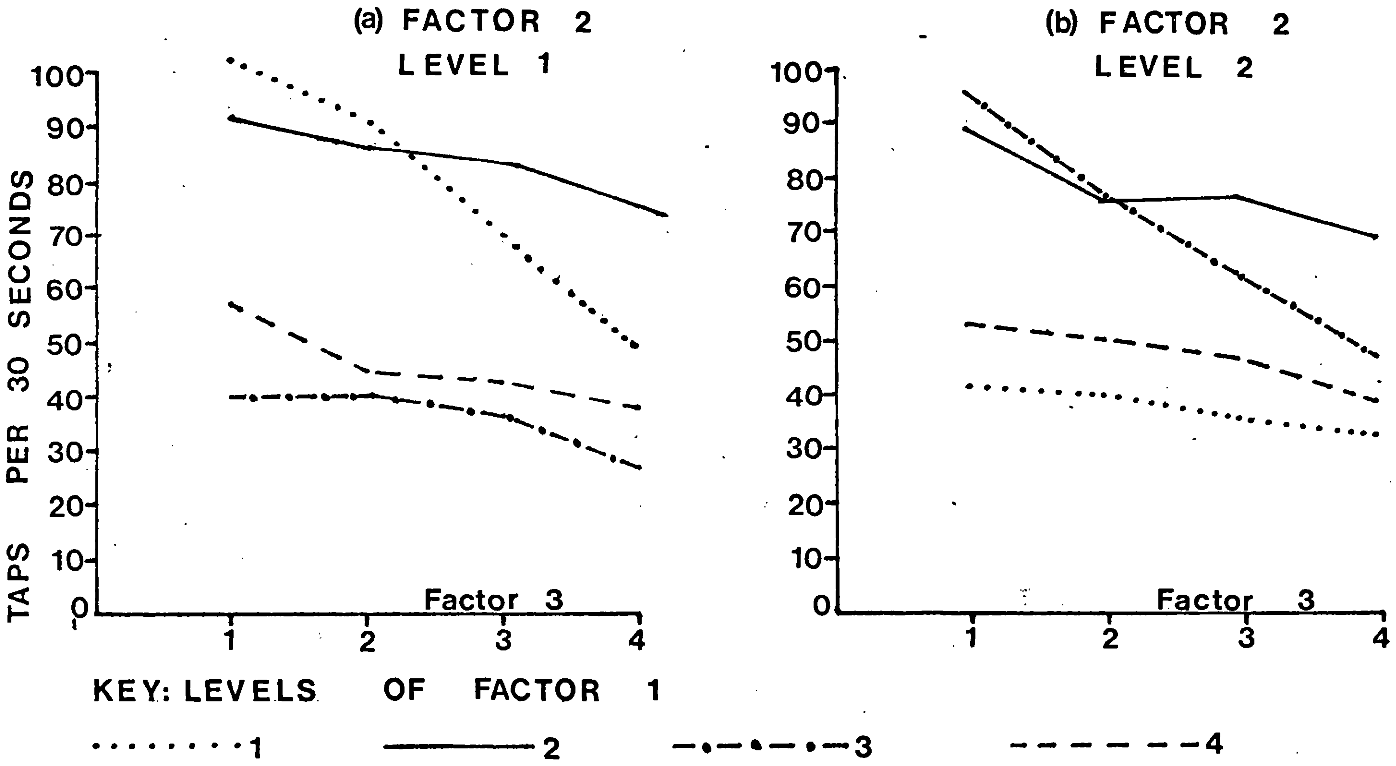
Note: data are in dots per 30 seconds

TABLE 5.3: Experiment III. Summary of ANOVA

Source	SS	DF	MS	F
Between people	88415.33	15		
1	34392.75	3	11464.25	4.13*
2	623.52	1	623.52	0.22
1 x 2	31218.23	3	10406.08	3.75
Subj. w. gr.	22180.83	8	2772.60	
Within people	50066.33	176		
3	14860.71	3	4953.57	21.09**
1 x 3	896.04	9	99.56	0.42
2 x 3	137.19	3	45.73	0.19
1 x 2 x 3	7342.23	9	871.36	3.71**
3 x subj. w. gr.	5636.83	24	234.87	
4	9440.76	2	4720.38	26.00**
1 x 4	576.29	6	96.05	0.53
2 x 4	58.64	2	29.32	0.16
1 x 2 x 4	1775.74	6	295.96	1.63
4 x subj. w. gr.	2904.92	16	181.56	
3 x 4	447.70	6	74.62	1.19
2 x 3 x 4	156.16	6	26.03	0.42
1 x 3 x 4	1080.43	18	60.02	0.96
1 x 2 x 3 x 4	1245.30	18	69.18	1.10
3 x 4 x subj. w. gr.	3007.42	48	62.65	
TOTAL	138481.67	191		

** $p < .01$ 1 $p < .05$

FIG. 5.1 EXPERIMENT III 1x2x3 INTERACTION



Data Plotted in Fig. 5.1

2:1 - Part (a)				
	3:1	3:2	3:3	3:4
1:1	101.83	90.83	69.83	49.67
1:2	91.00	87.67	85.83	76.33
1:3	40.00	40.50	36.67	37.17
1:4	56.00	44.17	39.17	26.83

2:2 - Part (b)				
	3:1	3:2	3:3	3:4
1:1	40.17	39.67	36.33	34.87
1:2	88.50	74.83	74.33	65.87
1:3	95.33	75.17	60.17	44.83
1:4	52.83	49.33	44.67	39.17

repeated measures over Factor 1, individual differences might again be invoked, but this argument is a little less plausible in this case than it was when applied to the 1 x 2 x 3 interaction, since each of the means in the first row of Table 5.2 is based on data obtained from four (as distinct from two) subjects.

The main effects of Factors 3 and 4 are not difficult to interpret; the rate of performance decreases with decreasing target-width and increasing target-separation. This result is highly significant.

Table 5.4 presents the data which are of particular relevance to the hypothesis under test in this experiment. These are the mean rates of performance obtained during the experimental trials with the 12 combinations of A and W. Also set out in this table are the relevant values of the Index of Difficulty and the percentage of errors. An error was defined as a dot whose centre lay outside of the target area (including the width of the boundary-line).

It is interesting to compare the obtained rates of performance on different targets having the same Index of Difficulty. It will be seen from Table 5.4 that four such sets of comparisons are possible, since there are two targets each at ID = 2.585 and ID = 5.585, and three targets each at ID = 3.585 and ID = 4.585.

The Newman-Keuls method was used to make these comparisons, using the error term for the 3 x 4 interaction in the denominator of the formula for the statistic (see Winer, 1970). The results of this analysis are set out in Table 5.5.

It will be seen that none of the resulting comparisons was statistically significant. For a given level of difficulty, the rate of performance is constant with variations in A and W. This

TABLE 5.4: 3 x 4 Interaction, Mean Speeds and Errors

Target Separation		8"	4"	2"
Target Width				
1.33"	Target	B	F	J
	Speed	60.69	72.13	79.31
	% Errors	0.31	0.26	0.00
	ID	3.585	2.585	1.585
0.66"	Target	C	G	K
	Speed	52.44	62.88	73.25
	% Errors	1.67	1.49	0.51
	ID	4.585	3.585	2.585
0.33"	Target	D	H	L
	Speed	46.94	56.13	64.56
	% Errors	8.12	6.24	2.03
	ID	5.585	4.585	3.585
0.16"	Target	E	I	M
	Speed	41.44	46.19	52.94
	% Errors	14.18	11.18	9.80
	ID	6.585	5.585	4.585

TABLE 5.5: Comparison of Speeds on Targets having Equal Levels of Difficulty

Target	Target	ID	q
F	K	2.585	0.57
B	G	3.585	1.11
B	L	3.585	1.96
G	L	3.585	0.85
C	H	4.585	1.86
C	M	4.585	0.25
H	M	4.585	1.61
D	I	5.585	0.38

None of the values presented in this table is statistically significant.

is true without exception, and these results replicate Fitts' findings with "maximum-rate" instructions.

Of greater interest is the extent to which speed varied with difficulty. A brief inspection of Table 5.4 suggests that the rate of performance decreased with each increase in the Index of Difficulty from 1.585 to 6.585. In order to test this tendency statistically, each target at each level of difficulty was compared with each target at the adjacent (higher) level of difficulty. Thes comparisons were also made by means of the Newman-Keuls method, and the results of this analysis are summarised in Table 5.6.

From Table 5.6, it will be seen that Target J produced significantly more rapid performance than either of the targets (F and K) at ID = 2.585. Target F produced significantly quicker performance than any of the targets at ID = 3.585, and the same is true of Target K. In general, performance at ID = 3.585 was more rapid than performance at ID = 4.585, but the comparisons between Targets B and H and between Targets G and H were not significant. In comparing performance at ID = 4.585 with that at ID = 5.585, only the two comparisons involving Target H were significant. Neither of the two targets at ID = 5.585 produced significantly more rapid performance than was observed with Target E (ID = 6.585).

It should be bourne in mind that these comparisons might be considered to be somewhat conservative; one-tailed tests might arguably be appropriate in view of Fitts' (1954) previous results with "maximum-rate" performance. However, the results indicate a clear tendency for speed to decrease significantly with increasing difficulty. A "ceiling effect" appears to be indicated towards the upper and of the range of levels of difficulty used here.

An inspection of the error-rates displayed in Table 5.4 reveals

TABLE 5.6: Comparison of Speeds at Adjacent Levels of the Index of Difficulty

Target	ID	Target	ID	q
J	1.585	F	2.585	3.63*
J	1.585	K	2.585	3.06*
F	2.585	B	3.585	5.78**
F	2.585	G	3.585	4.67**
F	2.585	L	3.585	3.77*
K	2.585	B	3.585	6.35**
K	2.585	G	3.585	5.24**
K	2.585	L	3.585	4.39**
B	3.585	C	4.585	4.17*
B	3.585	H	4.585	2.30
B	3.585	M	4.585	3.92*
G	3.585	C	4.585	5.27**
G	3.585	H	4.585	3.41
G	3.585	M	4.585	5.02**
L	3.585	C	4.585	6.12**
L	3.585	H	4.585	4.26*
L	3.585	M	4.585	5.87**
C	4.585	D	5.585	2.78
C	4.585	I	5.585	3.16
H	4.585	D	5.585	4.64*
H	4.585	I	5.585	5.02**
M	4.585	D	5.585	3.03
M	4.585	I	5.585	3.41
D	5.585	E	6.585	2.78
I	5.585	E	6.585	2.40

* $p < .05$ ** $p < .01$

that they, too, are related to the Index of Difficulty, with the more difficult targets producing greater numbers of errors than the less difficult targets. Subjects were apparently performing too quickly on the difficult targets (and/or too slowly on the easy targets) to maintain a constant standard of accuracy.

In Table 5.7, the mean rate of performance for the 12 target-combinations are expressed in terms of the number of movements per second. This table also contains the values of ID and the mean rates of transmission of information (ID x speed in movements per second) in bits per second (see Fitts, 1954). The transmission-rates are depicted graphically in Figure 5.2 together with Fitts' data. In each of these curves, values of ID represented by more than one target-combination have been plotted by taking the mean of the means for the relevant combinations. It will be seen that the transmission-rates obtained here are considerably lower than those reported by Fitts and that, though a straight line might represent the best fit for his data, the present data fall on a negatively accelerated curve.

The two targets (F and K) having an Index of Difficulty of 2.585 permit a comparison with the circle-dotting task of Experiment I, in which the targets also had an A:W ratio of 3:1 and an ID of 2.585. From Tables 3.2 and 3.3 it may be seen that the rates of performance of task CD were 56.88 and 63.22 taps per 30 seconds in Sessions 1 and 2 respectively. Performance on Target F of the present experiment was significantly faster ($t = 2.05$, $df = 35$, $p < .05$) than the first session performance of task CD, but was not significantly different from performance of that task in the retest session ($t = 1.02$, N.S.). A similar pattern emerges with Target K, where performance was significantly quicker than

TABLE 5.7: Experiment III Information Rates

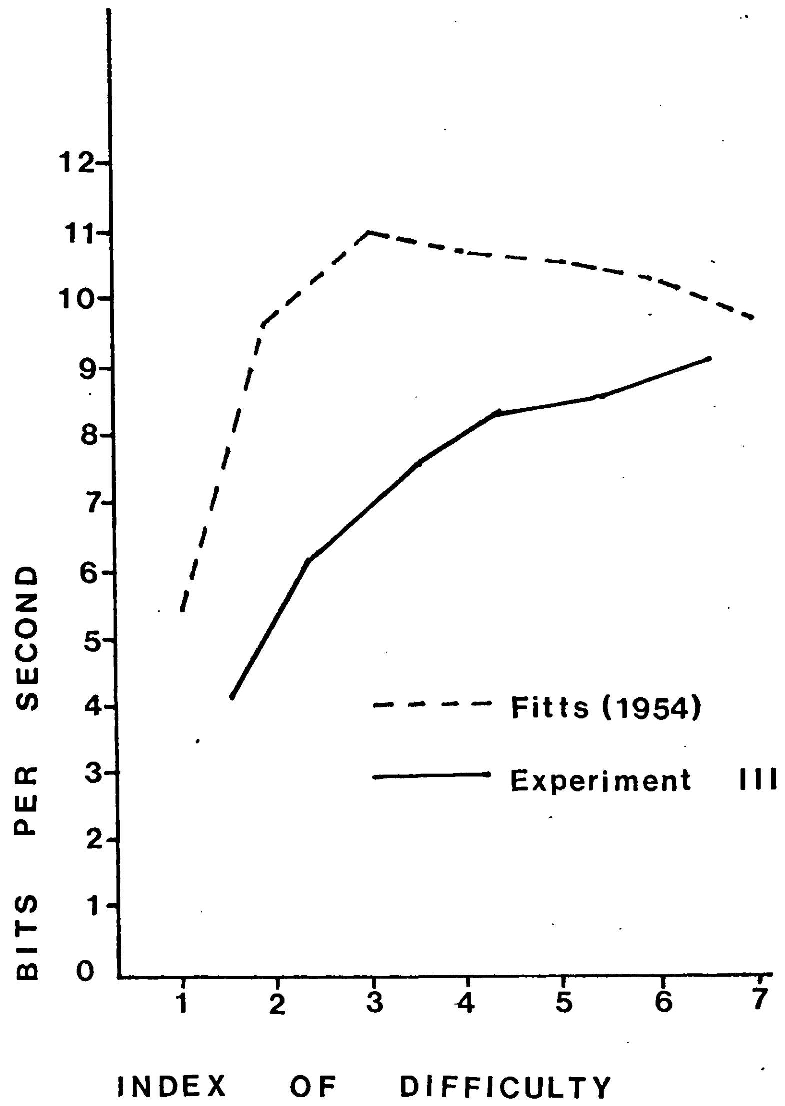
Target	A	W	ID	MPS	BPS
B	8"	1.33"	3.585	2.02	7.24
C	8"	0.66"	4.585	1.75	8.02
D	8"	0.33"	5.585	1.56	8.71
E	8"	0.16"	6.585	1.38	9.09
F	4"	1.33"	2.585	2.40	6.20
G	4"	0.66"	3.585	2.10	7.53
H	4"	0.33"	4.585	1.87	8.57
I	4"	0.16"	5.585	1.54	8.60
J	2"	1.33"	1.585	2.64	4.18
K	2"	0.66"	2.585	2.44	6.31
L	2"	0.33"	3.585	2.15	7.71
M	2"	0.16"	4.585	1.75	8.02

Note

MPS = Mean speed in movements per second

BPS = Mean transmission rate in bits per second

FIG. 5.2 INFORMATION TRANSMISSION RATES



Data Plotted in Fig. 5.2

Fitts (1954)

ID	Mean
1	5.56
2	9.64
3	10.99
4	10.62
5	10.46
6	10.20
7	9.58

Experiment III

ID	Mean
1.585	4.18
2.585	6.26
3.585	7.49
4.585	8.20
5.585	8.66
6.585	9.09

that obtained with CD in the first session of Experiment I ($t = 2.22$, $df = 35$, $p < .05$) but not significantly different from that which was observed in the retest session ($t = 1.16$, N.S.).

The analysis of information transmission rates which was presented above is not completely satisfactory because it does not take into account errors, which were not of equal frequency with the various target-combinations. It was noted in the Introduction that the effect of a high error-rate is that the effective target-width is greater than the nominal width (the width of the printed targets). Thus, when the error-rate is high, the value of ID which is obtained from Equation 3.1 is an overestimate of the true amount of information transmitted per movement.

An inspection of the target-sheets used by the subjects revealed that correction was also necessary for the easier targets, where the error-rates were very low. Subjects again tended not to use the whole available target-width, but concentrated their dots more closely about the centres than was strictly necessary for errorless performance. In these cases, the value of ID (and, therefore, the information transmission rate derived from it) underestimates the true value.

Welford (1968) describes one method of correcting for errors which was originally suggested by Crossman (1957). Crossman's method rests on the assumption that the distribution of dots is approximately normal, and makes use of the amount of information contained in such a distribution. However, Welford argues that the assumption of normality may be incorrect, and it should also be noted that Crossman's method does not provide correction for situations in which the error-rate is zero, and the distribution of dots is narrower than the target which is provided. Moreover, when the error-rate is low, any calculation which makes use of it may be based on a very small number of dots and, therefore, may be unreliable.

It was decided, therefore, not to use Crossman's method, but the technique which was briefly described in the Introduction to this experiment, and which involved the calculation of effective values of A and W from direct measurement of the dot-distributions produced by the subjects.

Each of the 12 target-sheets used by each subject was treated separately in this analysis. Beginning with the left target, the distance between the extreme dots on either side was measured (in millimetres). The procedure was repeated for the right target, and then the arithmetic mean of these two widths was used as the value of W' in Equation 5.1. The distance between the rightmost dot aimed at the left target and the leftmost dot aimed at the right target was them measured. This was then added to W' to produce the value of A' to be substituted into Equation 5.1.

The corrected information transmission rate for each target-sheet was then obtained by multiplying ID' by the mean number of movements per second which the subject had produced when using that target. The mean corrected rates of transmission are set out in Table 5.8. It will be seen that the rates are nearly constant, though there is some evidence of a fall-off with Target J.

TABLE 5.8: Experiment III. Corrected Information Rates

Target Separation		8"	4"	2"
Target Width				
1.33"	Target	B	F	J
	BR	8.78	8.31	6.31
	ID	3.585	2.585	1.585
0.66"	Target	C	G	K
	BR	8.96	8.59	7.92
	ID	4.585	3.585	2.585
0.33"	Target	D	H	L
	BR	8.53	8.51	8.35
	ID	5.595	4.585	3.585
0.16"	Target	E	I	M
	BR	8.17	7.87	7.29
	ID	6.585	5.585	4.585

Note

BR = Information transmission rate (bit-rate)

Discussion

Though subjects maintained the same rhythm of performance on targets having the same Index of Difficulty, it has been shown that significant increases in speed were obtained with every decrease in difficulty, except for the step from ID = 6.585 to ID = 5.585. Yet it was predicted from the hypothesis of a "preferred rhythm" that the same rate of performance (the "preferred rhythm") would be observed on all targets at or below ID - 3.585. Has the hypothesis been refuted by these results?

In answering this question, it should first be noted that there is one possible flaw in the argumnnt which was set out in the Introduction to this Experiment. Though subjects in Experiment I did not use the whole of the available target-width, it may be that they prefer to operate with some "safety margin", and that any substantial increase in difficulty would force them to decelerate because they were not prepared to operate with a reduced margin. It can be argued, therefore, that it is dangerous to extrapolate from the results of Experiment I to the type of data which would be expected on targets of a higher degree of difficulty.

However, if it is argued (on the basis of the relationship which was obtained in Experiment I between key-tapping and reciprocal tapping) that targets at ID = 2.585 are sufficiently low in difficulty to permit subjects to adopt the "preferred rhythm", then it follows that targets at ID = 1.585 will also be sufficiently low in difficulty to permit this rhythm to be adopted. Thus, performance on Target J of the present study should have been at the same rate as performance on Targets F and K. It has been shown, however, that performance on Target J was significantly faster than performance on Targets F and K. Now, though it appears very reasonable to suggest that increasing the difficulty of the targets might force subjects to operate at a rate below the "preferred rhythm", it would appear to be far less reasonable to suggest that decreasing the difficulty of the targets would force them to operate at a rate above the "preferred rhythm". Does this aspect of the results constitute a firm refutation of the hypothesis of a "preferred rhythm"?

There is one aspect of the present results which suggests that this question may be answered negatively. It has been shown that performance on Targets F and K was significantly more rapid than the Session 1 performance of circle-dotting in Experiment I, even though the quoted values of the Index of Difficulty were identical. Clearly, the discrepancy must be accounted for, because it may signify that some aspect of the present experiment was "disturbing" the behaviour and, therefore, that the results may not be wholly reliable. There are several possible ways of accounting for the discrepancy.

One possible suggestion is that the discrepancy may be due to the fact that different samples of subjects were used in the two experiments. Thus, by chance factors of sampling, it may be that the subjects selected for the present experiment had "preferred rhythms" which were somewhat higher in speed than those of the previous subjects. Such an account might "rescue" the hypothesis of a "preferred rhythm" because it could be argued that the "preferred rates" of the present subjects were so high that targets of ID = 2.585 did limit the rate of performance here, even though they apparently did not in Experiment I.

This possibility cannot be ruled out, but a casual observation on the part of the experimenter casts some doubt upon it, and suggests that an alternative type of account should at least be considered. The argument regarding sampling-differences would suggest that subjects in the present experiment were faster from the beginning than those used in Experiment I. However, many of them appeared rather to accelerate during the course of the practice-trials (and may have continued to do so during the experimental trials, but any acceleration then would not be as obvious, because it would be confounded with treatment-effects). Unfortunately, the practice-trials were not recorded so that this casual observation cannot be checked objectively, but it would be worthwhile to consider what factors could have led to any such acceleration.

One possibility is that perceptual-motor learning took place during the course of the relatively large number (14) of trials in Experiment III, so that the subject became able to perform the task more quickly whilst maintaining the same degree of accuracy. Since there were only two trials of reciprocal tapping in Experiment I, the opportunity for such learning to take place would be much more limited (and, in any case, performance in the present study was significantly faster than that in Session 1 of Experiment I, but not significantly faster than the retest performance). This argument appears to undermine the "rhythmic" hypothesis. Though the subject might become more skilled, why should this fact force him to operate at a speed above his "preferred rhythm"? If there is a speed at which the subject "prefers" to perform the task, then it would be expected that any such perceptual-motor learning would be reflected in a narrowing of the dot-distribution, rather than in an increase in speed. Moreover, it is implicit in this argument that the rate of circle-dotting in Session 1 of Experiment I was limited by target-difficulty. The plausibility of this suggestion has been questioned above.

Another possibility is that Experiment III established a different set in the subjects from that which was in operation in Experiment I. For example, the present experiment involved a relatively large number of targets of obviously varying levels of difficulty, whilst only one type of target was used in Experiment I. It may be that, in thus making obvious the variable of task-difficulty, the experimental design led subjects to perceive the investigation as a "testing situation" in which they should attempt to achieve relatively high scores (by performing quickly). Moreover, two of the trials were designated "practice trials", and involved targets labelled "practice sheets". Since it is well known that practice leads to more rapid performance in the vast majority of activities, it may be that the subjects concluded that an increase in speed was <u>expected</u>, and that they acted accordingly, forsaking their "preferred rhythm" to perform at a higher rate (which would be determined by such factors as the standard of accuracy which they set themselves, the amount of effort they were prepared to expend, etc). This argument, then, would suggest that the present results do not refute the hypothesis of a "preferred rhythm" because a contaminating variable in the design established a "set for speed" in the subjects.

Another possibility will be explained by means of an analogy. Under given physical conditions, a violin string has a certain "resonant frequency". Though it may be forced mechanically to vibrate at a wide range of frequencies, it will do so only with a much smaller amplitude than is observed at the resonant frequency. In this way, the string may be said (very anthropomorphically) to have a "preferred" frequency of vibration.

If certain physical conditions change, the resonant frequency also changes. For example, if the tension on the string is increased, the resonant frequency increases. Yet the string still has a "preferred" frequency of vibration. It will vibrate at other rates (including its former resonant frequency) only with much reduced amplitude.

The hypothesised increase in speed during Experiment III may be analogous to what occurs when the tension on a violin string is increased. That is to say, the subject may have a definite "preference" for one particular tempo at any point in the experiment, but "preference" may be expressed for different rates at different times during the session. For example, Craik & Sarbin (1963) suggested that one way in which the "personal tempo" might be conceptualised is as one aspect of the sensory environment which is under the control of the nervous system. Thus, the nervous system can regulate the rate of sensory input by regulating the "personal tempo". This type of effect may account for the hypothesised acceleration during the present experiment. For example, the large number of trials (which were rather homogenous in character) may have produced an experimental session which was monotonous from the subject's point of view. It may be that, in monotonous conditions, the nervous system increases the "personal tempo", as though to compensate for the paucity of simulation offered by the situation.

The above account would also "rescue" the hypothesis of a "preferred rhythm" because it could be argued that conditions so increased the tempo of the rhythm that targets of ID = 2.585 began to impose a limit on the rate of performance. Hence the relationship between speed and difficulty.

A final possible suggestion is that there is not really any discrepancy between Experiments I and III. Hitherto, it has been stated that the nominal Index of Difficulty of the targets used in Experiment I was 2.585 bits per movement. However, Equation 3.1

(from which this value was calculated) may be said to underestimate the degree of difficulty of circular targets because they restrict movement in <u>two</u> dimensions, whilst long rectangular targets impose restrictions in one dimension only. If this underestimation were of the order of 1 bit per movement, for example, then the appropriate comparison between the two experiments would be at ID = 3.585, rather than at ID = 2.585. A brief inspection of Table 5.4 reveals that the rates of performance obtained with Targets B, G and L (ID = 3.585) were very close to those obtained in Experiment I. The possibility that the discrepancy between Experiments I and III is only apparent, and that it is due to an underestimation of the degree of difficulty of the targets used in the former, is certainly worthy of consideration, therefore.

This is an account of the discrepancy which would undermine the "rhythmic" hypothesis. If it is argued that the results of Experiment I demonstrate that it is possible to perform at the "preferred rhythm" on targets of ID = 3.585, then it should obviously be possible also to perform at that rhythm on targets of ID = 2.585 and ID = 1.585. However, it was seen that decreasing task-difficulty from ID = 3.585 led to significant increases in speed, both in stepping from 3.585 to 2.585 and in stepping from 2.585 to 1.585.

The plausibility of this account can be questioned, however, because an inspection of the distributions of dots produced in the present experiment revealed a complicating factor. Many of the subjects appear to have begun a trial with the pencil at the distant; end of the targets, and to have progressively moved the hand towards the body whilst tapping, as though to produce a column of dots. The results of this would be that, for any one dot, the <u>effective target-length</u> would be smaller than the actual length of the printed targets, because the subject would be aiming at a fairly restricted area. Thus, Equation 3.1 may in this case underestimate the information transmitted even with the rectangular targets.

Of these possible interpretations of the discrepancy between Experiments I and III, three ("learning", "change of set" and "violin string effect") suggest that the repeated-measures design may have been responsible, either by dint of the large number of trials involved, or because of the obvious variation in task-difficulty. (Though there was the additional suggestion under the "change of set" possibility that the use of the word "practice" may have been responsible.) The remaining accounts ("sampling differences" and the possible underestimation of the degree of difficulty of circular targets) do not implicate the repeated-measures design.

In fact, these last two suggestions appear to be amongst the weakest. Doubts were cast on the argument concerning the additional difficulty of circular targets because of the columns of dots which many subjects produced in this experiment. The argument concerning sampling-differences has an "ad hoc" character, and it does not conform with the experimenter's casual observation of an acceleration during the course of the practice trials. The "learning" account is also of doubtful validity, because it would appear to imply that performance at ID = 2.585 is limited by task difficulty.

The two possibilities which appear to be the most plausible, then, both suggest that the discrepancy may have resulted from the use of a within-subjects design in this investigation. Later in this chapter, an experiment will be reported which represents an attempt to avoid the possible disadvantages of this type of design.

Experiment IV

Introduction

It was suggested earlier that Experiment III may not have provided an adequate test of the hypothesis of a "preferred rhythm" on account of a discrepancy between the results obtained and those which were reported in Experiment I. It was considered that the most plausible accounts of this discrepancy were those which suggested that the within-subjects design was responsible. Another aspect of the experiment which may plausibly be regarded as a possible source of the discrepancy is the fact that the word "practice" was used. It was thought desirable at this stage to conduct a further investigation of "spontaneous-rate" reciprocal tapping, but avoiding these features which were thought to be responsible for the discrepancy between Experiments I and III.

Omission of the word "practice" would present no difficulties, of course. However, the obvious alternative to an experiment having repeated measures would be one in which a separate group of subjects was assigned to each target-combination (so that each subject performed with only one target). However, to investigate the number of target-combinations included in Experiment III with an independent-groups design would be inconvenient, because a large number of subjects would be required. More importantly, the wide range of individual differences outained in this type of experiment renders between-subjects comparisons somewhat insensitive, so that there is an uncomfortably high probability of committing the Type II error (and this is why a between-subjects design was not selected for Experiment III).

Two unsatisfactory alternatives appeared to exist, therefore.

Either the results of Experiment III must stand, despite the doubts which they raise, or a new experiment of independent groups design must be performed, with the risk of committing the Type II error. Upon further reflection, however, it was concluded that some form of compromise might be struck, which would permit a more satisfactory test of the hypothesis of a "preferred rhythm".

In essence, the hypothesis might be tested by comparing rates of performance on just <u>two</u> targets, one at ID = 2.585, and one at ID = 1.585. Now, whilst it may be argued that the number of trials (14) in Experiment III may have made it difficult to test the "preferred rhythm" hypothesis, such an argument would appear less convincing when applied to a two-trial experiment. Furthermore, by the use of a counterbalanced design, any changes in speed with trials could easily be analysed.

It was considered worthwhile, therefore, to conduct a further study with the reciprocal tapping-task, in which each subject would be asked to perform one trial on each of two targets. The "preferred rhythm" hypothesis formed the null hypothesis of the experiment, and this was tested against the <u>directional</u> alternative that more rapid performance would be observed on the less difficult target.

Method

Design. The two target-combinations selected from those used in Experiment III were:

(i). J: A = 2", W = 1.33", ID = 1.585

(ii). K: A = 2", W = .66", ID = 2.585

The order of presentation of these targets was counterbalanced, half of the subjects performing first with target J, and half performing first with target K. Thus, the design was a 2 (presentation order) x 2 (target-combination), with repeated measures over the second factor.

An experiment consisting solely of two trials of reciprocal tapping would not, of course, be satisfactory. On the one hand, it was deemed desirable once again to include a "warm-up" task to permit subjects a period during which they could become accustomed to the experimental situation. On the other hand, there was once more the need to minimise "carry-over" effects by interposing an irrelevant activity between the trials of reciprocal tapping.

A battery of four irrelevant "buffer tasks" was prepared, therefore, and two of these were performed before the first trial of tapping, and two were performed between the two trials of tapping. Two of the "buffer" tasks involved the construction of a Koh's Blocks design. In each of the remaining "buffer" tasks, the subject was presented with a pair of cartoon-jokes, and had to indicate which of the two he found the more amusing. The order of presentation of the tasks within the "buffer" battery was determined by the use of tables of random numbers.

Apparatus and Materials. The targets were identical to targets J and K of the previous experiment, except that, on this occasion photocopies (rather than duplicated copies) were used. Two pairs of cartoons were selected, and were glued to A4 typing paper for insertion into the loose-leaf instruction-book. Coloured diagrams of the Koh's Blocks designs were similarly presented.

The instruction-book, a set of 12 coloured wooden blocks, two clearly labelled target-sheets, a slip of paper on which to record cartoon-ratings and a felt-tipped pen were provided on the subject's desk, which was positioned so that he sat facing a plain wall.

A concealed audio cassette recorder was used to record the sounds produced by the subject's tapping.

Subjects. Subjects were 14 male and 6 female volunteers from a course in introductory Psychology which was being taught by the investigator. All were naive to the aims of the investigation.

Procedure. Experimental sessions were held in the investigator's office in the University of Keele. Subjects sat facing a wall, whilst the experimenter sat directly behind them at a distance of approximately eight feet.

Upon S's arrival, he was shown to his seat, and it was explained that the instructions for the experimant were contained in the loose-leaf book. It was pointed out that the experimenter's part in the procedure was minimal, but that:

"there's one task which, although it's very easy to do, it's not very easy to describe on paper, so you may not understand the instructions I've written. Just before you start this task, then, I'll intervene, and check that you've got the right idea."

A check on whether or not the subject had understood the instructions for the reciprocal tapping-task was considered desirable because some subjects had experienced difficulties in the previous investigation.

Having made these comments, the experimenter said that the subject should ignore him and that he was, in any event, "only reading". E then returned to his desk, where he simulated reading during the remainder of the session.

At an appropriate point, E went to check that the subject had fully understood the instructions for the tapping-task. When necessary, further verbal directions were given; no demonstrations of any kind were ever given. In fact, in only one case did the subject claim that he could not understand the instructions, and even then only minimal prompting was required before he was able to perform the task satisfactorily.

The cassette recorder was started before the subject commenced the first trial of reciprocal tapping, and the noise produced by the operation of the switch was masked by a "bogus" cough. The 30-second trials were timed by consulting a wristwatch.

Upon completion of the session, the subject was thanked for his co-operation. The aims of the investigation were not divulged at this point, but the experimenter devoted part of the following week's lecture to a short exposition of the purposes of the research and of the results obtained.

Results

The mean rates of performance obtained with the two target-combinations are set out in Table 5.9. These data are broken down according to presentation-order in Table 5.10. Group 1 performed first with Target J, and Group II performed first with Target K. The means presented in Table 5.10 were analysed by means of a 2 x 2 analysis of Variance (with repeated measures over the second factor), and this is summarised in Table 5.11.

The difference between Groups I and II was not statistically significant ($p > .05$, two-tailed). However, the effect of target-difficulty was significant ($p < .025$, one-tailed), with more rapid performance on Target J. The main effect, however, must be considered in the light of the significant ($p < .01$) presentation-order by target-difficulty interaction. It will be seen from Table 5.10 that, though Group II performed more quickly on the easier target, Group I actually performed more quickly on the more difficult target. The within-group target-effects wrere tested by means of the Newman-Keuls method, which reiealed that there was no significant target-effect in the case of Group I ($q = 0.79$, N. S.), but that Group II performed significantly more quickly on Target J than on Target K ($q = 6.33$, $p < .01$).

TABLE 5.9: Experiment IV, Mean Rates of Performance for Groups I & II Combined (dots per 30 seconds)

Target J	Target K
65.65	61.10

TABLE 5.10: Experiment IV, Mean Rates of Performance for Groups I & II Separately (dots per 30 seconds)

	Target J	Target K
Group I	56.10	57.40
Group II	75.20	64.80

TABLE 5.11: Experiment IV. Summary of ANOVA

Source	SS	DF	MS	F
Between people	12391.88	19		
A	1755.63	1	1755.63	2.97
Subj. w. gr.	10636.25	18	590.90	
Within people	1035.50	20		
B	207.03	1	207.03	7.66*
A x B	342.22	1	342.22	12.67**
B x subj. w. gr.	486.25	18	27.01	
Total	13427.38	39		

Note

Factor A = Presentation order

Factor B = Target Difficulty

** $p < .01$

* $p < .05$

Discussion

It would appear that the hypothesis of a "preferred rhythm", as postulated earlier, has not been borne out in this experiment, since subjects performed significantly more quickly on Target J than on Target K. Yet it was predicted that the same "preferred rhythm" would be observed on both targets.

This conclusion must be qualified, however, in the light of the significant interaction which has been obtained. Whilst the above statement is certainly true of subjects in Group II, it is not applicable to subjects in Group I, who performed at (statistically) the same speed on the two targets. Clearly, it is important to arrive at a satisfactory interpretation of this interaction.

Perhaps the most immediately obvious suggestion is that the results reflect the operation of two factors, which were in opposition in the case of Group I but assisted one another in the case of Group II. The "two factors" might be termed a "target effect" (whereby more rapid performance is obtained with the less difficult target) and an "order effect" (whereby less rapid performance is obtained with a given target when it is performed first than would have been observed had it been performed second). A proponent of this interpretation might claim support for the hypothesis of an "order effect" from the results of Experiment I, where more rapid circle-dotting was obtained in Session 2 than in Session 1.

This interpretation implied that performance on Target K (ID = 2.585) was limited by task-difficulty (though this limitation was completely cancelled out by the "order effect" in the case of Group I). Such a possibility was admitted in discussing the previous experiment because the high rates of performance (in comparison with Experiment I) suggested that a "set for speed" might have been in operation. However, the means obtained at ID = 2.585 in the present study were quite close to those obtained in Experiment I. If it is argued that task-difficulty limited the speed of performance on Target K of the present investigation, therefore, it must be inferred that this is also likely to have been the case in Experiment I. Yet the plausibility of this suggestion was questioned on the grounds of the obtained relationship between the tempi of circle-dotting and key-tapping.

A second difficulty with this interpretation is that it implies that a given target will be performed more quickly by those subjects who receive it first.than by those who receive it second. This is certainly true of Target J, but the difference is in the opposite direction to that predicted in the case of Target K. Because the overall effect of Factor A is not significant it would not be valid to perform a posteriori statistical test on these comparisons, but it should certainly be noted that there is no indication of the proposed "order effect" with Target K.

It is possible, then, to challenge both of the postulates ("target effect" and "order effect") of this interpretation. It would be instructive, therefore, to consider whether it might be possible to suggest an alternative interpretation which was free of difficulties. One possibility is that a change of set took place, even in this two-trial experiment.

It could be suggested that both groups of subjects performed at the "preferred rhythm" on the first target. A crucial event is assumed to occur, however, when the subject notes at the beginning of the second trial of reciprocal tapping that the targets are not of the same level of difficulty as those which were presented for the first trial. It would again be argued that, in thus making obvious the variable of task difficulty, the design had induced a "set for speed" in the subjects.

This suggestion alone is clearly not adequate, because subjects in Group I did not perform more quickly in the second trial of reciprocal tapping than they had in the first. It might be suggested that the hypothesised set for speed was superimposed on a target effect, and that the two effects cancelled in the case of Group I. However, the suggestion of a target effect leads once more to the assumption that task-difficulty was limiting; the rate of performance in this experiment, and this assumption has been called into question in the light of the results of Experiment I.

An alternative suggestion might be that both groups of subjects changed their perception of the demands of the experiment when they observed the differences between the two types of target, but that this resulted in overt changes of behaviour only in the case of Group II. At the beginning of the second trial of tapping, these subjects were presented with targets which were <u>less</u> difficult than those which they had previously used, whereas subjects in Group I were presented with <u>more</u> difficult targets. It might, therefore, be suggested that subjects in the latter group had "missed their chance" to perform quickly, having already used the less difficult targets, and having performed on them at the "preferred rhythm". Subjects in Group II, on the other hand, had not yet used these targets and, upon noting the variable of task-difficulty, might have proceeded to work quickly on them.

This argument has an unsatisfactory "ad hoc" flavour. Moreover, the suggestion that subjects in Group I had "missed their chance" to perform quickly might be taken to imply that they could not have performed more quickly on the more difficult targets (ie., that tempo was limited on those targets). It is certainly not possible to rule out the suggestion that changing from a more to a less difficult target might have different motivational consequences from a change in the reverse direction, but this suggests that complex and subtle influences might be at work in these experiments with reciprocal tapping. Such influences would warrant further investigation.

The research reported in this chapter has been instructive, but inconclusive. Either it must now be concluded that the results of Experiment I were not, after all, indicative of a "preferred rhythm", or it must be concluded that "order effects" (be they due to changes of set or the "violin string effect", etc.) render repeated-measures experiments on reciprocal tapping unsuitable as a test of the hypothesis of a "preferred rhythm", even when only two trials are involved.

The intruiging question is whether the same pattern of results would have been obtained in Experiment I had the targets been of ID = 1.585, rather than 2.585. It might be suggested that this question could be answered by repeating the experiment, but with the less difficult targets. The resulting comparisons between the experiments would be insensitive, however, because it would be a between-subjects comparison. The same might be said for the more elegant procedure of performing a modified version of Experiment IV, but using a between-subjects design. In order to provide the necessary power a relatively large number of subjects would be required. Is this latter experiment the only alternative available?

This question can be answered in the negative. The attempt which has been made in this chapter to provide evidence of a "preferred rhythm" has been somewhat indirect. The experiments represent attempts to set up situations in which (it has been assumed) subjects are given the opportunity to adopt the "preferred rhythm". The failure to obtain results consistent with predictions does not constitute a firm refutation of the hypothesis, because it could be argued that (for one reason or another) the subjects did not choose to take the opportunity which was offered to them.

The time is perhaps now right for the adoption of a more direct approach. Smoll (1975a) is one of the previous researchers who have postulated the existence of a "preferred tempo", and he suggested that this rhythm should be taken into account in situations where a tempo of performance is normally imposed from without. This suggests that a more direct approach would be to observe what happens when subjects are paced at rates other than that which they spontaneously adopt. It is to this more direct approach that these investigations will turn in the following chapter.

CHAPTER 6:

Experiments V, VI & VII

Experiment V

Introduction

In the previous chapter, brief note was made of a more direct approach to the question of the existence of a "preferred tempo". The method would involve first measuring the "spontaneous rate" and then pacing the subject (for example, by metronome) at various tempi above and below this. The effects of being paced at "non-preferred" tempi would then be observed.

This experiment was proposed in discussing results obtained with reciprocal tapping, so that this task might appear to be the obvious choice for such a study. However, the results of the previous two experiments suggest that "order effects" may be of considerable magnitude with reciprocal tapping. The eaact nature of such "order effects" was not determined but on the one hand, it was thought possible that the "spontaneous rate" might change during a session, whilst on the other it was considered possible that changes of set might result in the situation where subjects were no longer performing at the "spontaneous rate". If the latter were the case, then multi-trial experiments using the reciprocal tapping task would clearly not be suitable for present purposes. Even if the former were the case, difficulties would arise in the proposed pacing experiment. If the logic of this experiment demands that the "spontaneous rate" be measured at the beginning of a session, then changes in this rate would result in the situation where paced rates presented towards the end of the session (having been determined by some transformation of the "spontaneous rate" at the beginning of the session) bore an unknown relationship to the "spontaneous rate" which would be elicited at the time of their presentation.

It might be suggested that the solution to this difficulty would be to measure the "spontaneous rate" at several points in the experiment in order to obtain information regarding the time-course of "order-effects". However, such a procedure would not be satisfactory because, once having been paced, the subject is no longer naive in the way which he was before pacing. It is not clear how this would affect "spontaneous rate" performance.

A more satisfactory approach would be to select a task which was known not to be subject to "order effects". It will be recalled that, though reciprocal tapping displayed a significant increase in tempo between the two sessions of Experiment I, there was no significant difference between the sessions in the tempo of key-tapping. This latter task, therefore, might appear to be a more appropriate choice for the proposed pacing experiment.

However, this information regarding the stability of the rate of key-tapping was obtained by comparing two different sessions, separated by an interval of two weeks. It is by no means implausible to suggest, however, that, though "order effects" may not be present in that situation, they may nevertheless occur when key-tapping is performed several times within a single session (as would be the case in the proposed pacing experiment). It was considered of interest, therefore, to conduct a pilot investigation into the within-session stability of key-tapping, in which the subject would be required to perform a number of trials of the task.

Method

Design. This experiment was of single-factor repeated-measure design. Each subject performed 10 30-second trials of finter-tapping.

A battery of "buffer tasks" was also prepared. In each of these, the subject was presented with a pair of cartoons, and required to state which of the pair he found the more amusing. One of these cartoon-ratings acted as a "warm-up" task, the remainder, being interspersed between the trials of tapping. The order of presentation of the cartoon-pairs was randomised for each subject.

Subjects. Subjects were 4 male and 11 female undergraduates enrolled in a course in elementary Psychology, which was being taught be the investigator. All were naive with respect to the aims of the investigation.

Apparatus and Materials. Subjects were provided with the usual loose-leaf book of typed instructions, into which were also inserted the cartoons to be judged. A slip of paper, clearly labelled, was provided for the purpose of registering the judgements. A Morse key was used in the tapping-tasks, and the sound produced by this device was recorded on a hidden cassette recorder.

Procedure. All experimental sessions were held in the experimenter's office at the University of Keele. Subjects sat at a desk which was facing a wall, and the experimenter sat at his desk, approximately eight feet behind (and out of sight of) S. On S's arrival, he was shown the instruction-book, and it was pointed out that the experimenter had a minimal part to play in the procedure, and that he would be "just reading". The session then began, with E directing S to terminate each trial of key-tapping when appropriate. On completion of the session, the subject was presented with a short written account of the aims of the research. He was then thanked for his co-operation and sworn to secrecy.

Results.

The data obtained in this experiment are set out in Table 6.1. Mean rates of performance for the 10 trials are presented in Table 6.2, and the Analysis of Variance is summarised in Table 6.3. It will be seen from Table 6.3 that the variation between the means is not greater than that which could be attributed to chance.

If Table 6.2 is examined, it will be seen that the largest difference between means is that between trials 1 and 2. Since the overall F-ratio was not statistically significant, however, an individual contrast between these maans would not be appropriate. Moreover, inspection of Table 6.1 reveals that a considerable proportion of this difference is contributed by a single subject (S 5.8), whose rate on trial 2 was twice the rate which she had adopted in trial 1.

TABLE 6.1: Experiment V, Raw Data (in taps per 30 seconds)

	Trial									
S	1	2	3	4	5	6	7	8	9	10
5.1	81	98	93	99	90	92	99	99	96	100
5.2	74	71	71	86	79	82	91	85	82	88
5.3	33	37	38	38	40	41	40	41	41	41
5.4	29	25	26	26	24	26	25	26	26	28
5.5	29	28	29	27	28	29	30	32	30	35
5.6	43	43	44	44	44	44	45	44	45	45
5.7	49	50	24	49	36	13	66	42	27	65
5.8	48	100	94	91	96	95	95	97	98	91
5.9	48	55	56	55	59	46	51	54	54	58
5.10	43	46	46	45	46	46	47	47	47	47
5.11	110	137	152	160	157	158	162	166	159	162
5.12	114	110	90	94	87	79	70	69	69	72
5.13	80	89	90	86	88	89	93	94	88	90
5.14	26	30	32	31	32	32	33	32	31	31
5.15	49	50	50	50	52	50	51	52	51	51

TABLE 6.2: Experiment V Trial

Means (taps per 30 seconds)	
1	57.07
2	64.60
3	62.33
4	65.40
5	63.87
6	61.47
7	66.53
8	65.33
9	62.93
10	66.93

TABLE 6.3: Experiment V, Summary of ANOVA

Source	SS	DF	MS	F
Between people	166076.57	14		
Within people	11083.70	135		
Trials	1144.41	9	127.16	1.61
Residual	9939.29	126	78.88	
Total	177160.27	149		

Discussion

It would appear from the results that have been obtained that the "spontaneous" rate of key-tapping is satisfactorily stable when several trials are performed with a single session. These results would suggest, therefore, that the proposed "pacing experiment" is a practical proposition from that point of view.

It is clear that this generalisation may not be applicable to all subjects, however. S.5.7, for example, displayed considerable variation in tempo between the different trials. Such exceptions provide no difficulties, however, since the overall trend which was obtained in this experiment was one of stability of chosen tempi.

The greater stability of the spontaneous tempo of key-tapping, when compared with the rate of reciprocal tapping, suggests that the "order-effects" which have emerged with the latter task are due to some specific characteristic of the task, rather than to a common feature of tapping-tasks in general. Such factors as changes in set have been suggested, but further research would be required to investigate these possibilities.

It is interesting to examine the data of individual subjects (Table 6.1). Subject 5.7, for example, displayed a marked variability in tempo across trials. This fact in itself is of no great interest, but what is remarkable is the relationship between the different tempi which she adopted. To a considerable extent, the various speeds appear to be multiples of what will, for the purposes of discussion, be termed a "basal rate" of approximately 12 taps per 30 seconds. Thus, 13 taps were recorded on trial 6, 24 on trial 3, 36 on trial 5, 49 in trials 1 and 4, and 50 on trial 2. The remainder of the trials do not fit exactly into this pattern, though an improvement in fit may be obtained by hypothesising a basal rate of 6 taps per 30 seconds.

It is, of course, by no means difficult to observe <u>some</u> form of pattern in a set of results by such "post hoc" methods. It would, therefore, be inadvisable to attach a great deal of significance to this result. However, the interest of the data lies in the conceptual questions which they raise. One question which has almost been avoided hitherto, for example, concerns the effects upon chosen tempo of physical constraints imposed by the task. It may be quite practical for the subject to tap on a Morse key at a rate of 60 taps per 30 seconds, but it is doubtful whether such a high rate of performance would be practical with some other forms of "rhythmic" task which have been found to be correlated with key-tapping. It would seem likely, for example, that Rimoldi's (1951) body-bending task would have been performed at a lower rate than this, though the author himself unfortunately provides no data on the question. But if the rates were different, what, if any, was the relationship between them? One possibility which is suggested by the results obtained with S 5.7 is that one may have been a multiple of the other, or that they may both have been multiples of some yet slower basal rate. That is to say, the possibility exists that, in a given task, the subject may select that multiple of the basal rate which best suits the physical constraints imposed by the task. Clearly, this suggestion may provide a fruitful avenue for future research.

Doubts may be raised, however, concerning the feasibility of such research. One obvious difficulty is that only one of the 15 subjects actually display any evidence of "harmonic" relationships between the speeds adopted in different trials. It is true that S 5.8 proceeded in trial 2 at almost exactly twice the rate which she had adopted in the first trial, but the remainder of her data, in common with those of the remaining subjects, displayed a comparatively high degree of stability. The question naturally arises, therefore, as to whether the results obtained from S 5.7 may be indicative of a process which is present in all subjects, though for some reason not in evidence in this particular experiment, or whether they represent some phenomenon specific to that particular subject and this particular experiment.

A second difficulty with the hypothesis that subjects adopt a multiple of some basal rhythm which best suits the constraints imposed by a particular task is that it carries the potential of becoming an irrefutable hypothesis, since ever better fits can be obtained by hypothesising progressively lower basal rates. In Table 6.1, for example, the data can be fitted very well by the postulation of a basal rate of approximately 3 taps per 30 seconds. The best fit of all can be obtained by postulating a basal rate of 1 tap per 30 seconds, but it is clear that at this point the hypothesis has become meaningless. Evidently, some lower limit would have to be imposed on what was acceptable as a meaningful basal rate, but it is not clear by what criteria such a limit would be set.

Summary and Conclusions

1. In this experiment, 15 subjects performed 10 30-second trials of key-tapping within a single session. There was no significant tendency for speed to vary over the trials. It would appear, therefore, that the spontaneous rate of key-tapping is quite stable over time.

2. Despite the overall stability of tapping-rates, one subject displayed an interesting pattern of variation in speed. Many of the different rates were multiples of what was termed a "basal rate" of approximately 12 taps per 30 seconds. The possibility was discussed that, for a given task, the subject might select that harmonic of the "basal rate" which best suits the physical constraints of the activity.

Experiment VI

Introduction

The aim of the following investigation was to cast light on the question as to whether the "spontaneous" tapping-rate exhibited by subjects in these experiments is in any meaningful sense a "preferred" rate. A necessary prerequisite, therefore, would appear to be a definition of the word, "preferred".

The most satisfactory course would appear to be to adopt the definitions used by previous writers who have employed this adjective (e.g., Smoll, 1975a, 1975b). However, a brief description of the literature is sufficient to reveal that none of the previous investigators has provided any definition. Indeed, it will be seen that the present writer has also used the word without definition, though always within quotes to signify its undefined status. It might perhaps be argued that a definition is not actually necessary, since all native speakers of English will already be familiar with its meaning. However, its use in the present context is perhaps somewhat unusual, and it would appear to advisable to consider a definition, since this will be of assistance in ennumerating the kinds of evidence which could be considered as support for the view that subjects have "preferences" for particular tapping-rates.

According to the "Concise Oxford Dictionary", appropriate synonyms for "prefer" are "chose rather" and "like better". What must be done next is to consider the kinds of empirical data which could be considered as evidence that the subject "chooses rather" or "likes better" his "spontaneous" tapping-rate, to ask whether such evidence exists in the literature, and to consider ways in which the evidence might be supplemented if it should prove to be deficient.

Generally, there are two broadly different ways in which "prefer" (or its dictionary synonyms) might be intended. On the one hand, conscious preferring, choosing or liking might be implied. For example, given a series of tapping-rates, the subject might actually be able to tell the experimenter that the one amongst them which he prefers is his own "spontaneous" rate. To date, no experiments appear to have been performed which use this method to determine whether the "spontaneous" tapping-rate is a "preferred" rate. It is true that a rather similar method has been used (eg., by Frischeisen-Kohler, 1933) to determine a "preferred" metronome-rate, but the experimenter in this case appears to have started with the assumption that subjects would hava a "preferred" rate. The subject was not so much asked whether he found one of the presented rates more congenial than the others, but which one he found the most congenial. In fact, Frischeisen-Kohler notes that subjects often judged several rates to be agreeable, and a forced-choice procedure was then adopted to arrive at a single measure. Unfortunately, she does not provide information regarding the distribution of presented rates. If the different rates which were judged agreeable were very close in value, this is perhaps what would be expected: it would seem somewhat implausible to suggest that the subject might have an extremely specific preference for only one frequency. If, however, the rates which were judged agreeable were distributed fairly widely, then this equivocation on the part of subjects would appear to cast considerable doubts on the validity of the concept of a "preferred" metronome-rate.

The possibility of conscious preferences for particular rates has not, then, been adequately explored in the literature. In the following experiment, therefore, it was decided that the subject would be presented with a number of rates of tapping (including his own "spontaneous" rate) and that he would be asked to make judgements on certain evaluative Semantic Differential scales in order to determine whether he had any preferences amongst them.

The second sense in which "preferred" and its synonyms might be intended is one which does not imply that the subject is consciously aware that he has "preference" for a particular rate. (Perhaps the word should always be used within quotes when intended in this sense, since it is unusual for "conscious" not to be connoted.) It would be quite reasonable, for example, to suggest that the "preferred" tapping-rate represents some natural tendency of the organism to operate at that particular rate due to mechanisms of which the subject was not consciously aware. For example, it was seen that Craik & Sarbin (1963) conceptualised the "personal tempo" as a means used by the central nervous system to regulate its level of sensory input, and it is quite plausible to suggest that the subject himself might not be aware of such processes.

Clearly, unconscious "preferences" of this type would not be likely to be revealed in Semantic Differential judgements, but in objective measures of speed such as those which have been taken in all of the experiments in the existing literature. What results have been obtained which might be counted as evidence of "preferences"?

It was seen in Chapter 2 that the only author who has apparently set out with the aim of providing evidence of "preference" is Smoll (1975a, 1975b). In the first of these papers, it was shown that marked within-subject consistency in tempo was contrasted with marked between-subject differnnces. However, Smoll's conclusions were challenged on the grounds that the data were not sufficient to support them. In short, Smoll did not employ the obvious control of comparing the "spontaneous" rate with rates randomly assigned to the subject. In his second paper, Smoll presented evidence of high reliability of tempo, and it was seen in Chapter 2 that this finding is in agreement with the vast majority of previous data on reliability. The significance of reliability will be discussed shortly, but for the present it should be noted that there is an almost complete lack in the literature of cases in which an investigator has put forward evidence in support of his use of the word "preferred". What appears to be necessary at this point, therefore, is a consideration of what aspects of objective tempo-measures could reasonably be considered as evidence of "choosing rather" or "liking better".

In a sense, the fact that the subject performs the task at one particular rate at all might be said to be a case of "choosing rather". After all, there is no specification of a required tempo of performance; the subject is merely told to tap at a congenial rate. It might be said, therefore, that the subject has chosen the observed rate rather than (or in preference to) the other possible rates of tapping.

Few people would be satisfied with this argument, however. An obvious question would be: "what happens if the subject is asked to perform the task on several occasions? Does he always perform at a similar speed, or does he adopt different tempi every time?" Of course, if the subject did adopt a completely different speed on each occasion, then it could be argued that each of these tempi was the "preferred" tempo at the time it was adopted (simply because it was chosen, rather than another tempo), but this would clearly be a thoroughly vaccuous use of the term. Certainly, if a "preferred" tempo is to be of the kind of practical significance suggested by Smoll (1975a), then a fairly high degree of generality would be required. The reliability which has often been reported in the literature, therefore, is not only compatible with the hypothesis of a "preference", but it is also a sine qua non of any meaningful version of the hypothesis.

Does the reliability of tempo prove the hypothesis of "preference", however? Or is it possible to account for such data without recourse to "preferred" or its synonyms? One possible alternative stems from a consideration of the choice open to the subject performing a tapping-task in an experiment of the type reported here. It is true, as was stated above, that the subject has in principle a wide range of speeds which are permissable, but the fact is that on any particular trial he has to select one particular rate, simply because he cannot tap at every possible speed simultaneously. It may be, therefore, that he "arbitrarily" selects one speed from amongst the possible rates. What evidence has anyone for suggesting that the subject would not have been equally "content" to perform at any other tempo?

Clearly, the reliability data might be cited in response to this question. If the subject would have been equally "content" to tap at other speeds, why does he not take the opportunity to do so when repeating the task two weeks later? Or why does he not take the opportunity to perform at different rates with the two hands within an experimental session?

These questions may be answered by an appeal to the subject's perception of the expectations of the experimenter. In a test-retest study such as Experiment I of the present series, the experimenter may be said in a sense to be promoting reliability by the controlled similarity of the two sessions. Seeing that everything about the two sessions is almost exactly identical, it not be surprising if the subject were to conclude that the experimenter expected (or wanted) him to perform at the same rate in the retest as in the original session. All that is required is that, at the time of the retest session, he can still accurately recall the tempo which he adopted in the previous session.

This last requirement might be considered to be the weak point of the above argument. It might be regarded as unlikely that a subject could accurately recall a tapping-rate over such a period unless there were something "special" about that rate. Thus, a proponent of the hypothesis of "preference" might suggest that it is only the "preferred" rate (a "special" rate) which is reliable, and that, if the subject were randomly assigned a rate and asked to reproduce it two weeks later, he would not be able to do so. Perhaps surprisingly, this experiment has not been previously performed. In the same way that Smoll's (1975a) experiment might be criticised on the grounds that he did not use the control of comparing the "spontaneous" rate with randomly-assigned rates, so might the research on reliability. What is of interest is not whether the "spontaneous" rate is reliable, but whether it is more reliable than rates randomly assigned to subjects by the experimenter. The final investigation in this chapter (Experiment VII) will be directed towards this question.

Considering again Smoll's (1975a) suggestion that the "preferred tempo" should be taken into account in situations where a rhythm is normally imposed from without, it would presumably be suggested that some deleterious effects would be observable when the performer was paced at "non-preferred" rates. The most elementary question concerns whether the subject is actually capable of performing at tempi other than that which he spontaneously adopts. As would perhaps be expected, the answer to this question is affirmative, and was provided by Denner et al (1963). In their study, subjects tapped in synchrony with a metronome which was in operation throughout the period of tapping. However, an interesting question concerns what effects would have been observed had the metronome been stopped at some point, and the subject left to continue tapping. Would he take the opportunity (wittingly or unwittingly) to drift back or "regress" in tempo towards his "spontaneous"rate, as would be expected if he "chose it rather" or "liked it better" or would he be "content" to continue to tap at the originally imposed tempo? Since this question is readily testable experimentally, and since its testing would require an experimental design similar to that which would be required by the Semantic Differential study, it was decided that the search for "regression" towards the "spontaneous" rate would form the second element of Experiment VI.

Method

Design and Procedure. The rationale of this investigation dictated that a measure of the subject's spontaneous rate of key-tapping should first be obtained, and that he should, in subsequent trials, be paced by means of a metronome at numerous faster and slower rates. The experiment thus fell into two phases: a preliminary stage (termed "Trial 0"), during which the subject performed at a spontaneously adopted rate, and a subsequent stage during which a number of different rates of performance were specified by metronome (Trials 1 - 7).

In Trial 0, the subject tapped for one minute at a spontaneously chosen rate. The termination of this period of work was indicated by means of a 500 Hz. tone-generator, similar to the one used in Experiment II. Following the period of tapping, the subject completed a form carrying ten Semantic Differential scales, which were to be marked "according to how well you think they describe the rate of tapping which you have just been using".

Instructions for Trial 0 were typewritten. The instructions for key-tapping were as follows:

> "On the desk in front of you, there is a key labelled "Key A". Your task is to tap on this key, using whichever hand you prefer. Tap regularly, so that the intervals between successive taps are more or less equal. Do the task in your most natural and congenial way. Begin as soon as you like, and continue until you hear the communication-tone."

Two Semantic Differential scales were selected as appropriate to this experiment; good-bad and pleasurable-painful. These were included in a battery of ten scales, which are set out in Table 6.4. The same scales were also employed in the paced stage of the experiment. Both scale order and polarity were randomised for each subject.

It was decided that, during the second stage of the experiment,

TABLE 6.4: Experiment VI, Semantic Differential Scales

Good	Bad
Pleasurable	Painful
Active	Passive
Graceful	Awkward
Excitable	Calm
Fast	Slow
Spacious	Constricted
Beautiful	Ugly
Interesting	Boring
Stable	Changeable

TABLE 6.5: Experiment VI, Means for Factor A

Pacing Factor	Mean (%)
0.55	98.68
0.70	99.48
0.85	101.97
1.00	101.71
1.15	100.61
1.30	99.10
1.45	103.12

each subject would be presented with seven paced rates of performance, three of which would be faster than the rate which he had adopted in Trial 0, three of which would be slower, and the remainder would act as a control, being exactly equal to the rate which had been observed in Trial 0. The subject was given no information regarding the relationship between the various rates and his rate on Trial 0. The order of presentation of the seven rates was randomised for each subject.

The paced trials (Trials 1-7) were identical to one another in design except, of course, that the metronome rate was different in each case. The design of the trials will be explained with the aid of Figure 6.1.

The beginning of a trial was determined by the subject, who depressed a "Start Key" when he was ready. (Event 1). Immediately, a series of 30 metronome-beats were delivered at the specified rate. During this period (Phase A), the subject's task was to tap in unison with the metronome. When the metronome stopped (Event 2), the subject's task during Phase B was to continue (without any hesitation) to tap at the tempo which the metronome had specified. One minute after the cessation of the metronome pacing, the communication-tone sounded (Event 3) to indicate that the subject should cease tapping.

During the subsequent period of one minute (Phase C), the subject filled in the Semantic Differential scales, "according to how well they describe the rate of tapping which you have just been using". If the scales had been completed before the end of Phase C, the subject was required to fill the remainder of the period by adding columns of figures which had been taken from tables of random numbers. The subject was instructed that,should

Figure 6.1: Experiment VI, Design of Trials 1-7

Event	1	2	3	4	5
	S presses Start key	Metronome stops	Communication tone sounds	Communication tone sounds	Communication tone sounds
Phase	**A**	**B**	**C**	**D**	
Period	Variable - dependent on metronome rate	1 minute	1 minute	1 minute	
Task	Tapping in unison with metronome	Tapping without metronome	Completing rating scales - solving arithmetic	Tapping without metronome	

the communication-tone signal the end of Phase C before he had completed the scales, the priority was to complete them before the beginning of the task of Phase D. During Phase D, the subject was required to reproduce the previous metronome-rate for a further period of one minute. As may be seen from Figure 6.1, the metronome-rate was <u>not</u> repeated at the beginning of Phase D, but the subject was required to reproduce the rate from memory.

One minute after the subject had begun tapping in Phase D, the communication-tone signalled the end of the trial. The subject was then free to rest for a period if required, before initiating the following trial.

Because of the complexity of the experimental design, it was deemed necessary to depart from previous practice by presenting instructions for the paced section of the experiment orally, and in a somewhat less formal manner. The great difficulty with written instructions would be that their length and complexity would carry the risk of confusing the subject. It was decided that oral instructions would be superior in this respect, because the experimenter would be able to ask whether the subject understood as the exposition of the procedure was in progress. Following the initial exposition of the procedure, a summary was given, and the subject was asked if he understood what was required. If this was the case, the paced stage of the experiment was begun; otherwise, further clarification was given.

In determining the metronome rate for each trial, the mean inter-tap interval during the "spontaneous rate" trial was multiplied by a "pacing factor". The seven pacing factors were: 0.55, 0.70, 0.85, 1.00, 1.15, 1.30 and 1.45. This multiplication then produced the inter-tap interval which was to be produced by the metronome for that trial.

Computing System. It will be seen from the above description that the procedure of this experiment was comparatively complex; measurement of the speed adopted in Trial 0, calculation of the required metronome-rate for each of the paced trials, and setting the metronome to the appropriate rate all had to be accomplished "in real time". It was because such complexity introduced the possibility of error that it was decided to employ full automation of the procedure, with the aid of a laboratory computer and a system of programmes written by the investigator. An exhaustive description of the system will not be attempted in this section, but the salient features of the various elements will be briefly discussed. Full listings of the programmes and overlays will be found in Appendix 111.

The machine which was used was the same PDP 8/E which was employed in Experiment II, but the facilities available had been extended by the time of execution of the present study. In particular, a buffered digital input/output module had been added, and this device permitted full computer control, requiring no interventien of any kind on the part of the experimenter. Should the subject commit an error, however, it was possible for the experimenter temporarily to resume control until the error had been rectified.

Signals from the morse key used for tapping were fed into the analogue-to-digital converter, as in Experiment II, and the treatment of these data followed the principles adopted in the earlier investigation. The sampling-rate was again 100 per second. The one-minute trials used in this experiment were recorded by means of 3 20-second "bursts", following each of which a period of approximately .5 seconds was required for the stored data to be transferred to magnetic DECtape. During this delay, recording was suspended.

The "start" key (which was mentioned in the previous section) was connected to the digital input, where it was serviced by an overlay to LABFOCAL (the programming language) which was devised by the experimenter. Two more overlays of the investigator's authorship were used to control the digital output. These permitted the computer to control the communication-tone and also the "computerised metronome".

The "computerised metronome" consisted of a simple circuit arrangement whereby a series of short electrical pulses could be delivered at a specified rate to a pair of headphones. The subject was required to wear these headphones throughout the paced stage of the experiment.

As in Experiment II, it was decided that an Index would be set up on the data storage tape. This was loaded before the beginning of experimentation with all of the randomised presentation-orders which would be required. During the execution of the experiment, a register in the Index kept a count of the number of subjects who had been run, so that, at the beginning of a session, the next presentation-order in the list was automatically loaded, without the need for the experimenter to type in any parameters.

In view of the large volume of data which would be generated during this investigation, it was decided that raw data would bot be permanently stored on magnetic tape. Instead, at the end of an experimental session, the subject's data were output in a compressed form (without loss of information) on paper tape. In addition, certain numerical summaries of the data were printed on the teleprinter.

The Semantic Differential forms were also prepared by computer.

The relevant programme was designed to randomise both the scale order and polarities, and to print the scales. Forms for all of the subjects were produced at a single run of the programme, and the resulting roll of paper was cut into sections as required. Each subject was provided with eight separate sections, one for each trial (0 - 7). Trial-numbers were printed on the sections. An example of the Semantic Differantial forms is reproduced in Appendix III.

Subjects. Subjects were 17 unpaid volunteers, recruited from a variety of departments within the University of Leicester. All were naive with respect to the aims of the investigation.

Additional Materials. For Trial 0, typed instructions were provided, together with the appropriate Semantic Differential form and a pencil. Forms for the remaining trials were provided when the procedure was explained, together with a sheet of random numbers which were to be added in the circumstances explained above.

Results.

In analysing the data, the mean inter-tap interval for each 20-second burst-sample was first expressed as a percentage of the metronome inter-beat interval which the subject was attempting to reproduce at the time of the sample. Thus, a mean tapping-rate which exactly matched the metronome-rate would be indicated by a score of 100. When the subject's tapping-rate was higher than the metronome-rate, a score below 100 would be registered. A score above 100 would indicate that the subject's tapping-rate was below the metronome-rate. Both raw data and the resulting percentage scores are set out in Appendix II.

The hypothesis of the experiment (that the "spontaneous" rate is a "preferred" rate, and that "regression" would occur when subjects were paced at "non-preferred" rates) would predict drifts towards scores below 100 with pacing-factors above 1.00, and drifts towards scores above 100 with pacing-factors below 1.00. The control condition (pacing-factor 1.00) would be expected to result in scores not significantly different from 100.

The initial phase of the analysis comprised a 7 (pacing-factors: Factor A) x 3 (20-second bursts within a 1-minute tapping period: Factor B) x 2 (Phase B vs. Phase D: Factor C) x 17 (subjects: Factor D) Analysis of Variance.

The mean scores for Factors A, B and C are presented in Tables 6.5 - 6.7. Table 6.8 presents the A x B x C interaction, whihh is also depicted in Figures 6.2 - 6.8. The Analysis of Variance is summarised in Table 6.9, from which it can be seen that none of the resulting values of F attained significance at the .05 level of confidence. In view of this, further analysis of trends (drifts) would not have been warranted.

TABLE 6.6: Experiment VI. Means for Factor B

	Burst	
1	2	3
101.42	100.14	100.43

TABLE 6.7: Experiment VI. Means for Factor C

Phase B	Phase D
99.99	101.32

TABLE 6.8: Experiment VI. Means in A x B x C Interaction

		B1	B2	B3
A1	C1	99.67	98.86	98.34
	C2	98.73	97.78	98.51
A2	C1	99.68	98.29	98.47
	C2	102.11	99.53	98.82
A3	C1	101.13	100.41	100.07
	C2	105.67	103.36	101.16
A4	C1	100.16	101.03	100.84
	C2	105.68	100.96	101.60
A5	C1	98.61	98.73	99.14
	C2	102.41	102.04	102.71
A6	C1	99.79	98.59	100.16
	C2	99.07	97.14	99.85
A7	C1	103.38	102.39	102.23
	C2	103.74	102.88	104.07

FIG. 6.2 ABC INTERACTION

AT A1

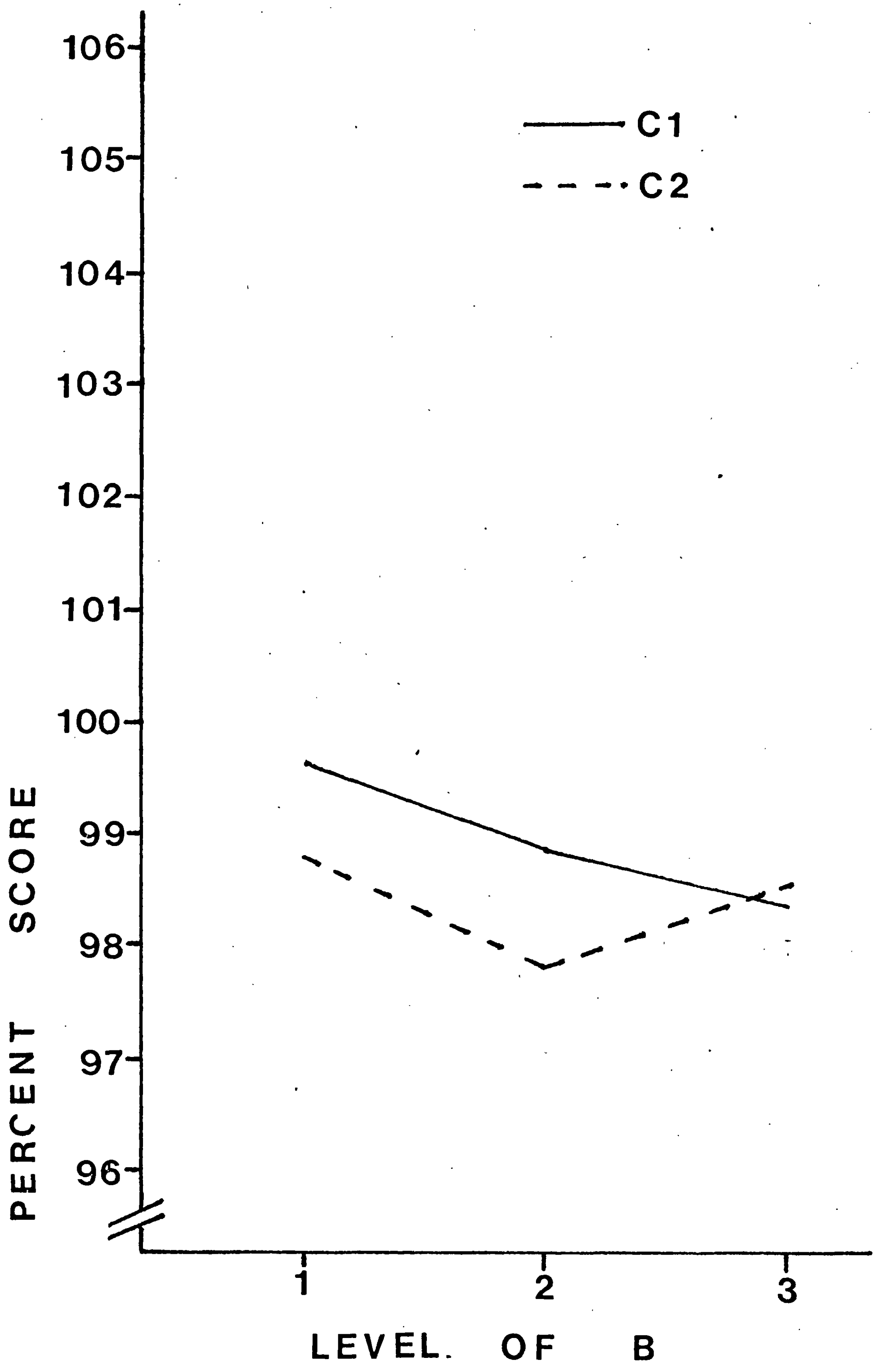


FIG. 6.3 ABC INTERACTION

AT A2

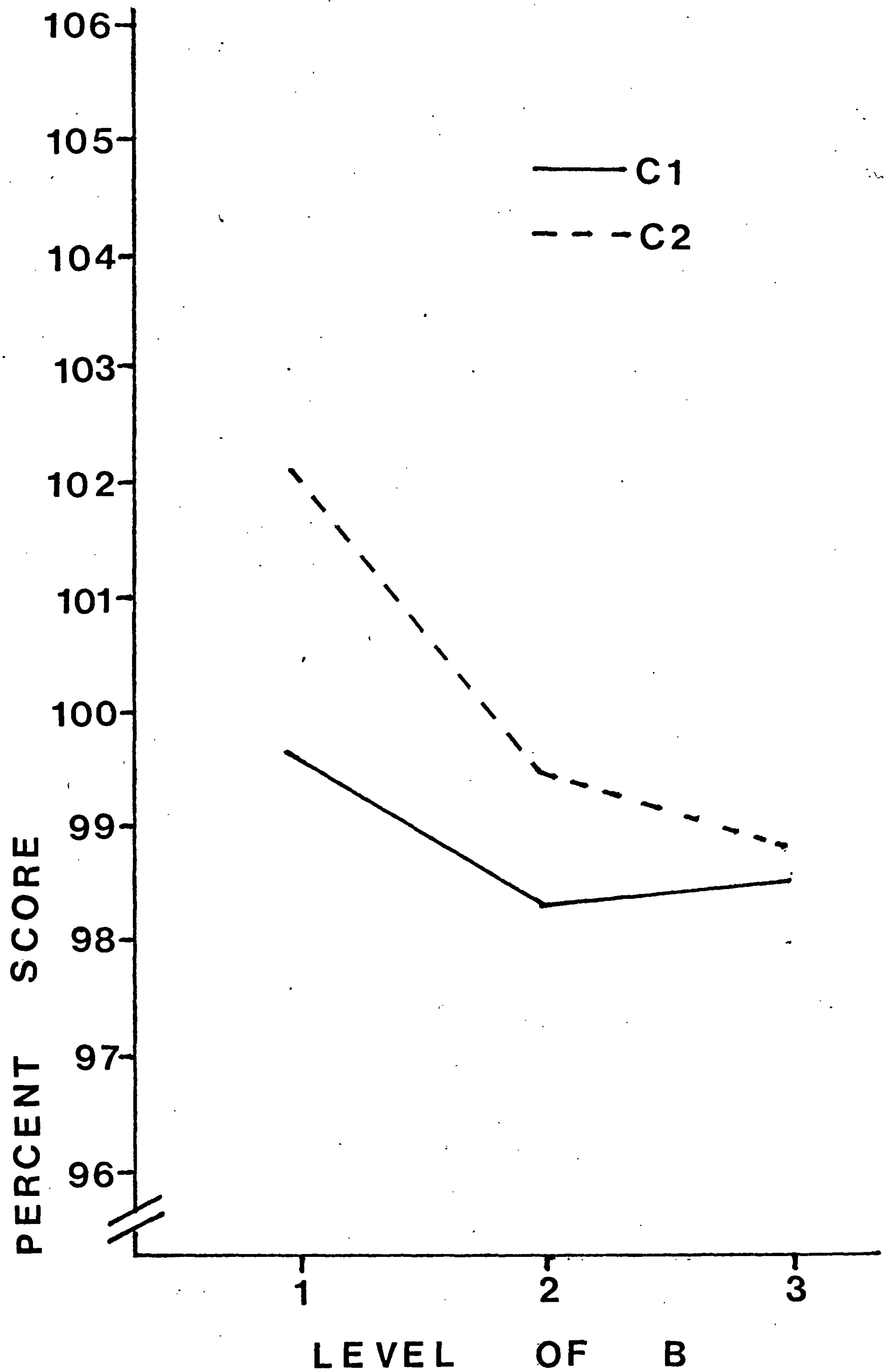


FIG. 6.4 ABC INTERACTION

AT A3

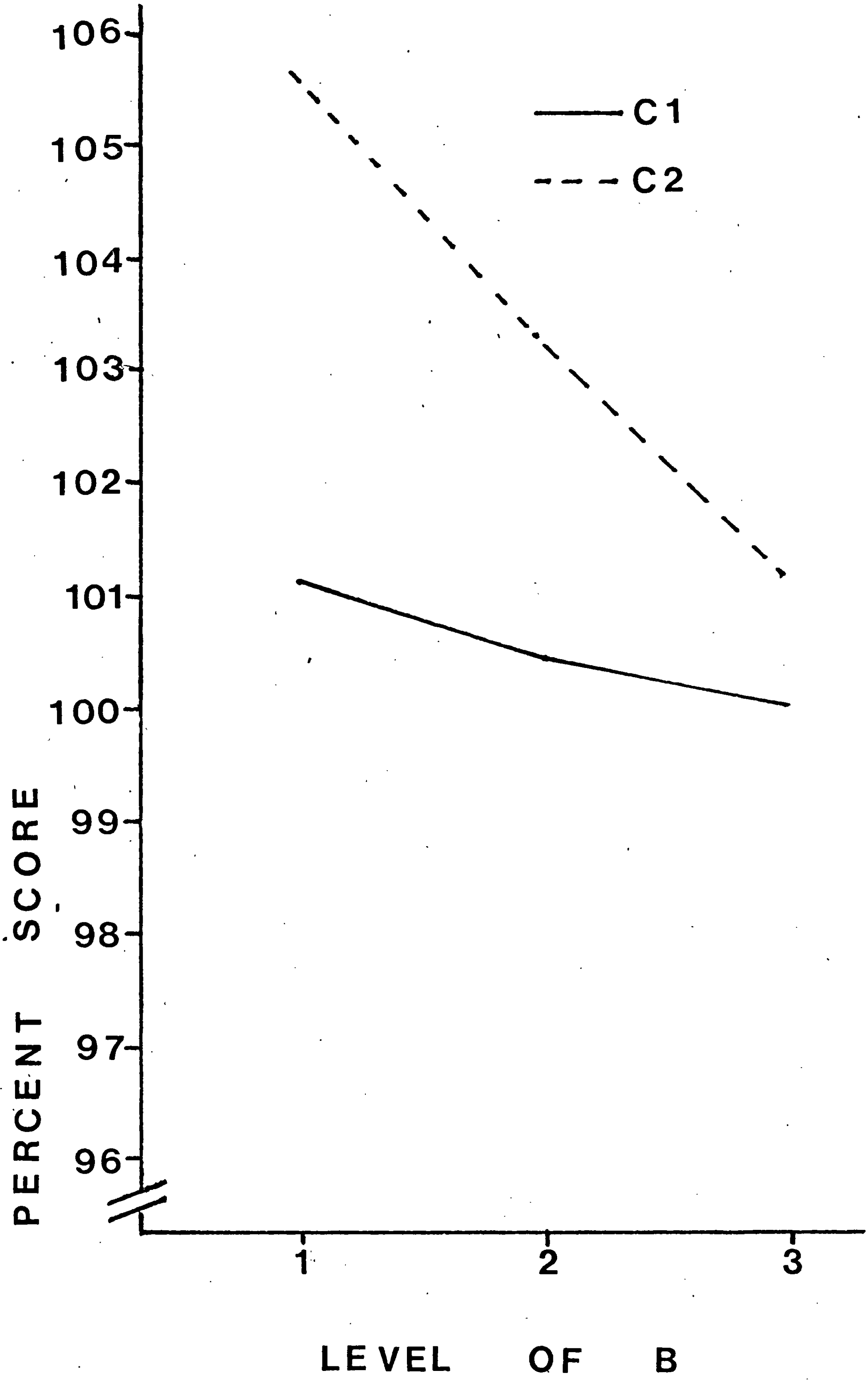


FIG. 6.5 ABC INTERACTION

AT A4

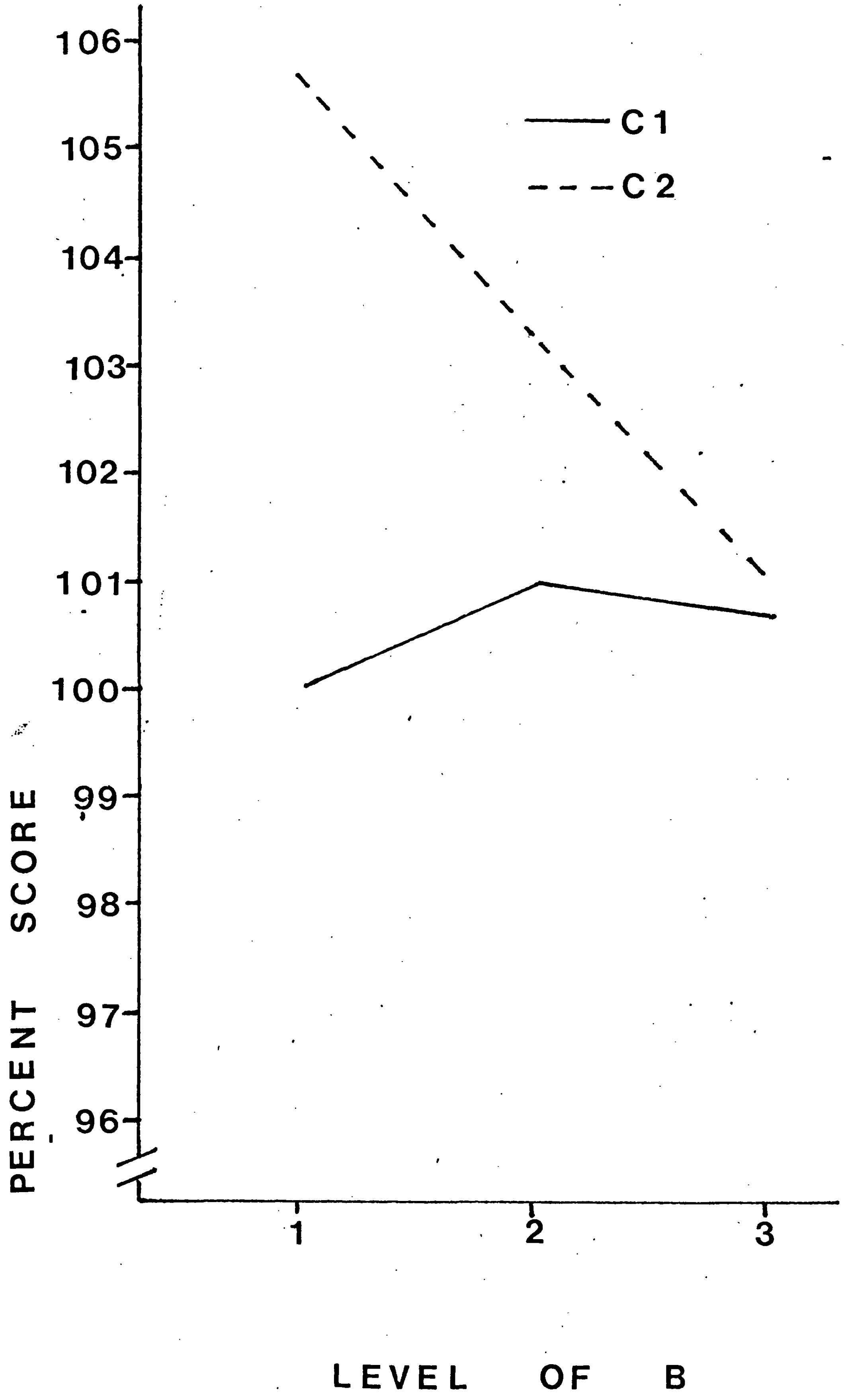


FIG. 6.6 ABC INTERACTION
AT A5

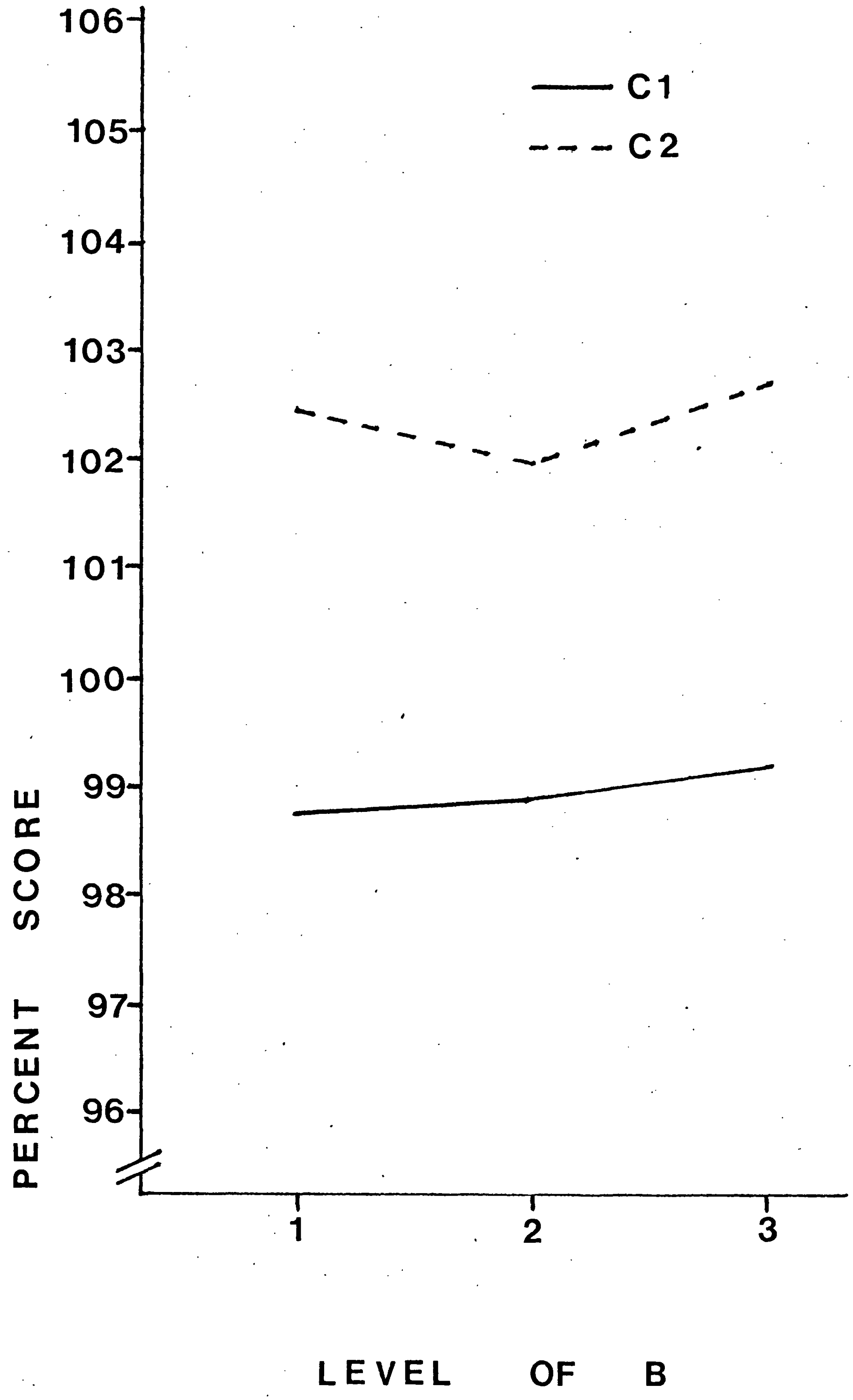


FIG. 6.7 ABC INTERACTION AT A6

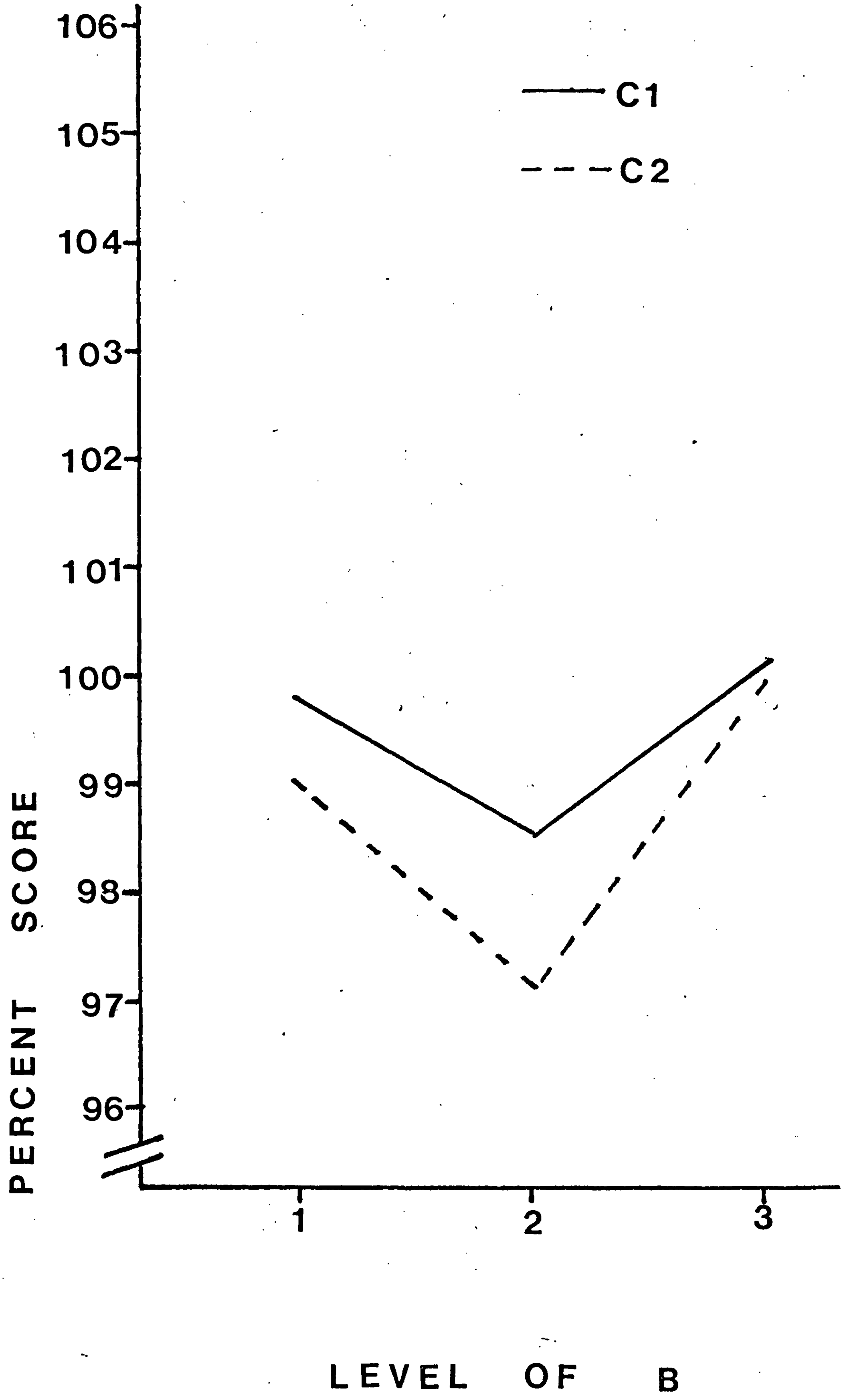


FIG. 6.8 ABC INTERACTION AT A7

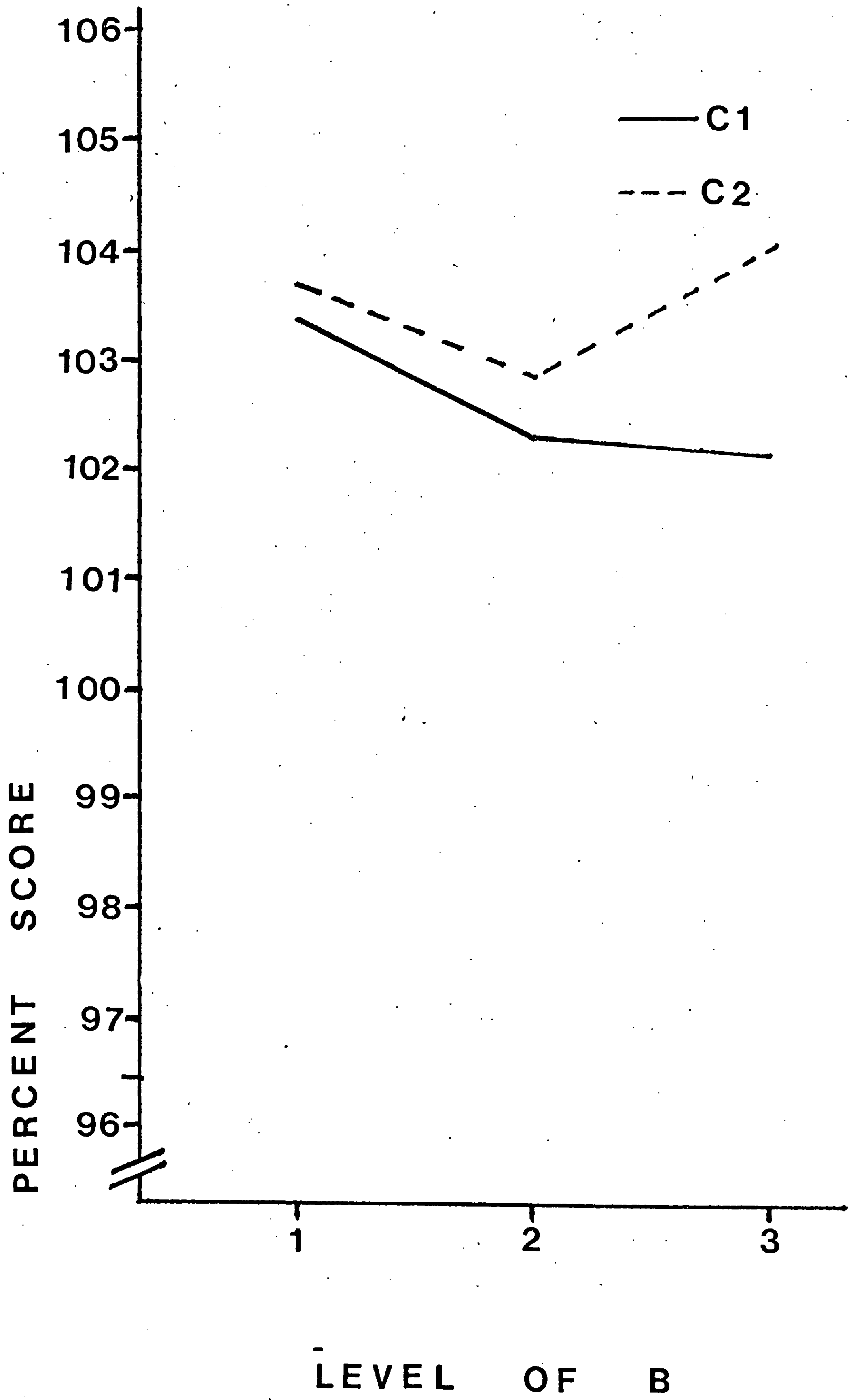


TABLE 6.9: Experiment VI, Summary of ANOVA

Source	SS	DF	MS	F
A	1704.87	6	284.14	0.77
B	212.83	2	106.42	3.14
C	314.13	1	314.13	0.69
D	47369.18	16		
AB	216.52	12	18.04	1.04
AC	420.57	6	70.10	0.63
AD	35553.04	96	370.34	
BC	62.29	2	31.14	2.29
BD	1084.14	32	33.88	
CD	7240.93	16	452.56	
ABC	188.14	12	15.68	1.30
ABD	3332.43	192	17.36	
ACD	10748.39	96	111.96	
BCD	435.08	32	13.60	
ABCD	2320.76	192	12.09	
Total	111197.31	713		

Table 6.10 presents the mean scores obtained with a pacing-factor of 1.00, at both levels of Factor C, but summed across the levels of Factor B. The differences between these means and hypothesised values of 100 were tested by means of x/SD scores. For C1, x/SD = 0.10; $p > .05$, and for C2, x/SD = 0.24; $p > .05$. The mean tapping-rates in the control condition were not, therefore, significantly different from the mean metronome-rates.

A second control datum was available in this experiment, since the mean rate of tapping during Trial 0 may be used to compare these results with previous relevant data. The most appropriate comparison would appear to be with task FR of Experiment I. In the present study, the mean inter-tap interval during Trial 0 was 0.8885 seconds. This is compared with a value of 0.6076 for task FR in Session 1 of Experiment 1. The difference between these two means proved to be significant by t-test for independent samples ($t = 3.22$; $df = 40$; $p < .01$).

It might be suggested that this discrepancy between Experiments I and VI could be atrributed to the fact that a longer period of tapping was required in the latter study. It may be, for example, that some factor such as fatigue led the subjects in Experiment VI to decelerate during the one-minute Trial 0, thus resulting in a lower mean rate of performance. In order to test this suggestion, the mean inter-tap intervals for the three 20-second burst-samples of Trial 0 were compared. These means are set out in Table 6.11. It is clear from this table, however, that the trend was towards acceleration rather than deceleration, since the successive mean intervals decrease. The significance of the differences between the means of Table 6.11 was tested by a single-factor repeated-measures Analysis of Variance, which is set out in Table 6.12. It will be seen that the resulting F is far

TABLE 6.10: Mean % Scores with Pacing Factor = 1.00

Phase B		Phase D	
Mean	SD	Mean	SD
100.68	6.59	102.74	11.47

TABLE 6.11: Trial 0 Burst Means (Hundredths of sec.)

1	2	3
90.43	88.24	87.89

TABLE 6.12: Trial 0, Summary of ANOVA

Source	SS	DF	MS	F
B. people	33632.16	16		
W. people	4510.10	34		
Bursts	64.46	2	32.23	0.23
Res.	4445.64	32	138.93	
Total	38142.26	50		

from significant.

In analysing the data from the two Semantic Differential scales which are of interest here, each subject's ratings were assigned numerical values from 1 to 7 inclusive. The scale-poles "bad" and "painful" were assigned the value, 1. The mean ratings obtained with these scales are set out in Tables 6.13 and 6.14, and depicted graphically in Figures 6.9 and 6.10.

The analysis of these data comprised two separate single-factor repeated measures ANOVA's, and these are summarised in Tables 6.15 and 6.16. It will be seen that ratings on the scale "Pleasurable-Painful" varied significantly with pacing-factor. However, the means presented in Table 6.14 do not follow the pattern which would have been expected under the hypothesis that the "spontaneous" rato is a "preferred" rate. This hypothesis would predict that the highest ("most pleasurable") mean would be associated with the pacing factor of 1.00, and that the mean ratings would decrease in value on either side of this factor. This is clearly not so, and the mean rating associated with the pacing factor of 1.00 is only third in descending order of magnitude.

TABLE 6.13: Mean Semantic Differential Ratings, Good/Bad

Pacing Factor						
0.55	0.70	0.85	1.00	1.15	1.30	1.45
4.29	4.59	4.00	4.41	4.00	3.71	4.06

TABLE 6.14: Mean Semantic Differential Ratings, Pleasure/Pain

Pacing Factor						
0.55	0.70	0.85	1.00	1.15	1.30	1.45
4.47	4.59	4.12	4.29	4.00	3.71	4.12

FIG. 6.9 MEAN RATINGS ON GOOD-BAD

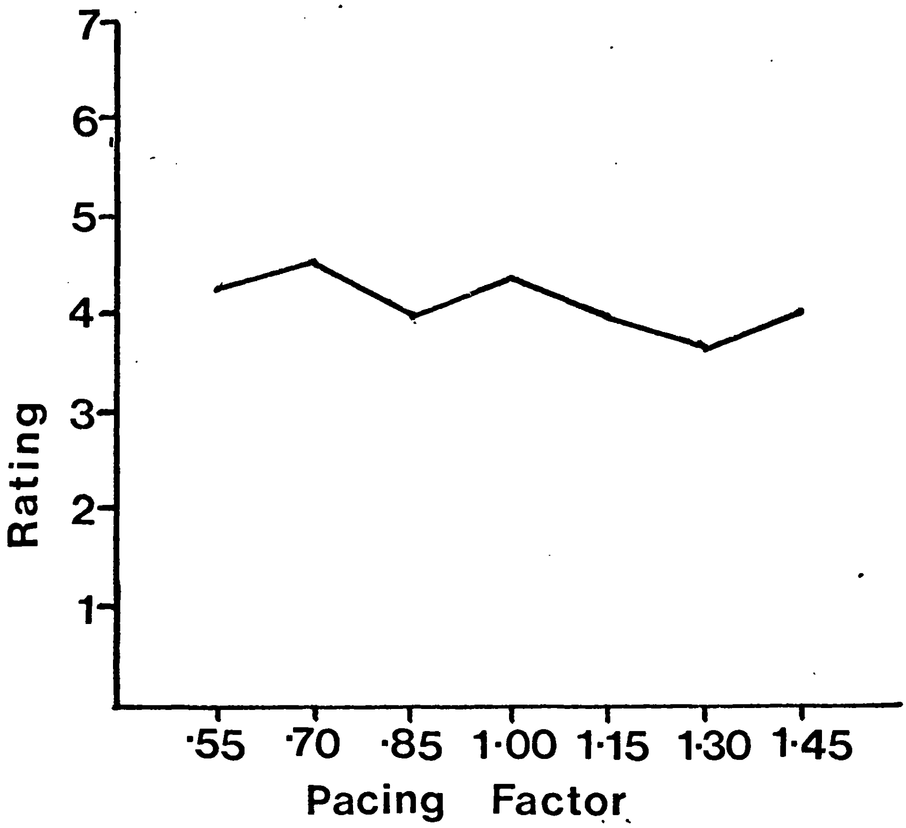


FIG. 6.10 MEAN RATINGS ON PLEASURABLE-PAINFUL

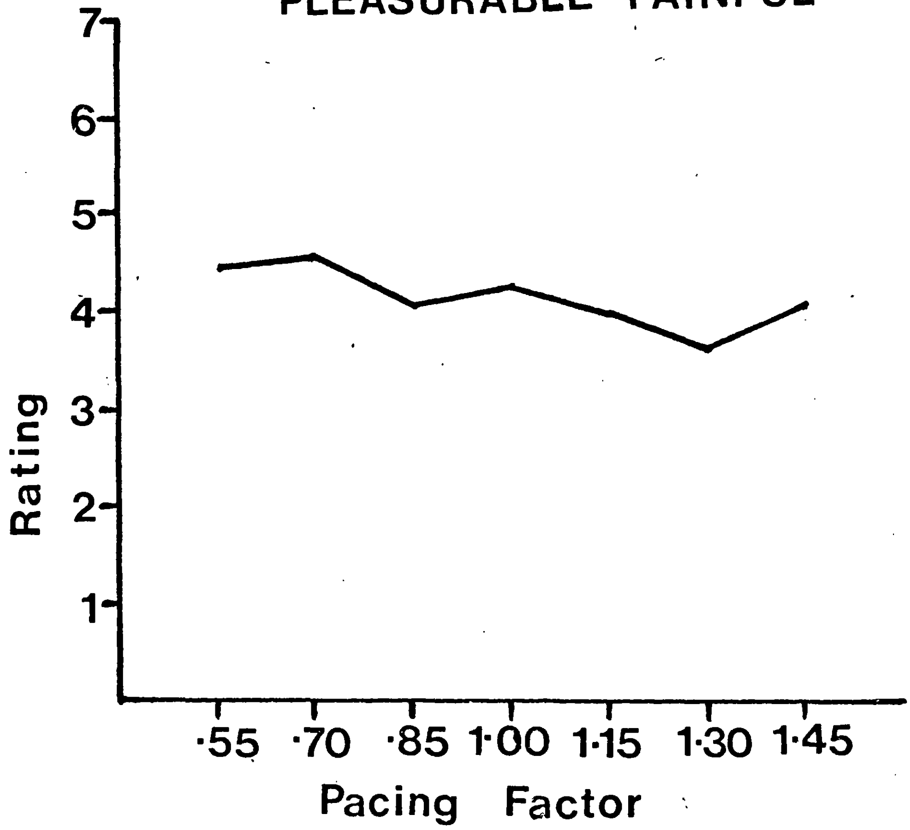


TABLE 6.15: Summary of ANOVA, Good/Bad

Source	SS	DF	MS	F
B. people	14.42	16		
W. people	79.86	102		
Speeds	9.04	6	1.50	2.04
Res.	70.82	96	0.74	
Total	94.28	118		

TABLE 6.16: Summary of ANOVA, Pleasurable/Painful

Source	SS	DF	MS	F
B. people	24.79	16		
W. people	67.14	102		
Speeds	8.99	6	1.50	2.47*
Res.	58.15	96	1.61	
Total	91.93	118		

$^*p < .05$

Discussion

"Regression" from an imposed rate towards the "spontaneous" rate would be expected to be revealed in three of the F-ratios of the four-factor analysis. If subjects gradually decelerate when paced at rates above the "spontaneous rate", and accelerate when paced at rates below it, then the means at the various levels of Factor A should differ significantly. Specifically, for pacing-factors below 1.00, scores would tend to be greater than 100, whilst for pacing-factors above 1.00, scores would tend to be smaller than 100. It was seen however, that there was no significant variation in the means at the various levels of Factor A.

It might be suggested that Factor A provides a comparatively insensitive test for "regression", because any such tendency might not establish itself until comparatively late in the period of tapping. A drift might be revealed by comparing the three 20-second "bursts" within a tapping-period (Factor B) or when comparing the first tapping-period in a trial (Phase B) with the second (Phase D). The appropriate tests on these possibilities are not the main effects of Factors B and C, however, because the drifts, being in opposite directions on either side of the pacing-factor of 1.00, would cancel out. It is the A x B and the A x C interactions which are important, therefore. As was seen, neither of these interactions was statistically significant.

There appears, then, to be no evidence in these data that the subject "choses rather" or "likes better" his "spontaneous" rate. Nor can any support for this hypothesis be derived from the Semantic Differential scores. No significant variation was obtained with the scale "Good-Bad", and the significant variation which was obtained with the scale "Pleasurable-Painful" is not readily interpretable, and is perhaps best attributed to chance factors. In any event, the pattern which was obtained would need to be replicated before an attempt at interpretation could be considered worthwhile.

The failure of the Semantic Differential scales to provide any evidence of preference does not in itself refute the hypothesis that the "spontaneous" rate is a "preferred" rate, because, as was pointed out in the Introduction to this experiment, it is meaningful to speak of "preferences" of which the subject is not consciously aware. How much damage is done to the hypothesis by the failure to obtain "regression"?

It might be argued on several grounds that little damage is done by these results. One argument, for example, might be that the "preference" is not for a <u>single</u> rate, but for a <u>band</u> of rates, and that all of the pacing-factors used in this experiment fell within that band. Such an argument is not very convincing, however, since the range of pacing-factors used here was considerable. If the subjects are "indifferent" over such a wide range of tempi, then any "preferred band of rates" would not be likely to be of great practical significance in the way suggested by Smoll (1975a).

A second objection might be that the experiment was not sufficiently sensitive, since the tapping-periods involved were comparatively short, and did not permit "regression" to become established. Though deleterious effects were not observed when subjects performed at a "non-preferred" rate for these short intervals, it might be that such effects would have been observed had the tapping-period been as long as five minutes, for example. After all, in any practical situation in which the "preferred" tempo may be of significance (such as at work), the period for which the task was performed would be likely to be longer than those used here.

This is a more compelling objection than the first, but it may prove difficult to design a study which was more satisfactory in this respect than the present experiment. With the existing trial-length of three minutes, including two one-minute periods of uninterrupted tapping, several of the subjects spontaneously commented that, though they found the aims of the research very interesting, they had nevertheless found the experiment both taxing and monotonous. (A considerable incidence of sighs and groans also testified to this.) The experimenter is very sceptical, therefore, of the feasibility of extending the tapping-period much beyond the length used here.

A more satisfactory procedure might be to increase the length of Phase C of the trial, during which the subject was not actually tapping. If this were done, memory factors might bring about "regression" in the form of an A x C interaction. In effect, the next experiment to be reported will adopt this suggestion, since, as will be seen, it employed an interval of approximately two weeks between the first period of tapping at the paced rate and the second period of work.

Another objection might be that subjects in this experiment were, after all, instructed to maintain the various pacing-rates. In not displaying "regression", therefore, they were merely complying with instructions, so that this experiment cannot be regarded as a refutation of the view that the "spontaneous" rate is a "preferred" rate.

Though this argument is attractive, the results do, nevertheless, cast doubts on Smoll's (1975a) suggestion that the "preferred" rhythm might have important practical consequences.

What Smoll suggests is that the rhythm should be taken into account in situations where an externally imposed rhythm normally exists - that is, in situations where the subject is told in some way to perform at a speed specified without reference to his hypothetical "preferred" speed. What this experiment has shown is that, when people are told to perform at "non-preferred" rates, they will do so, without drifting back towards their "spontaneous" speed, or expressing any dislike of the imposed rate. What, then, is to be gained from taking into account their "spontaneous" rate?

Another objection to the present study is that "regression" is but one way in which a "preference" might reveal itself. It may be, for example, that the quality of work is at its optimum when the task is performed at the "preferred" tempo. It is difficult, however, to see how the concept of quality could be applied to tasks such as tapping. This is certainly a suggestion which should be investigated, however, and this could be done with more complex tasks where a measure of quality would be available. Another possibility which might be investigated with tapping is that the "spontaneous" rate is one which requires the minimum of information-processing in the nervous system. It might be, for example, that if subjects were simultaneously tapping and performing some other task, tapping at the "spontaneous" rate might interfere less with the other task than tapping at rates other than the "spontaneous" rate.

An interesting point is the discrepancy which has been found between the mean rate of tapping in Trial 0 and the mean rate of tapping in an ostensibly identical task in Experiment I. It was seen that the most obvious explanation - that the difference was due to the different lengths of time for which the task was performed - did not appear to be compatible with the data, so that this result remains to be accounted for.

It will be recalled that a similar discrepancy was obtained between the first computer-controlled experiment (Experiment II) and Experiment I. At the time, it appeared plausible to account for this discrepancy in terms of the differences in response-keys which had been used in the two studies. Such an account will not work in the present case, however, because the key used in Experiment VI was identical to that used in Experiment I.

Subjects in Experiments I and V adopted similar tapping-rates, and the rates in Experiments II and VI were also close to one another. We have, then, a discrepancy between Experiments I and V on the one hand, and Experiments II and VI on the other. The major difference between the experiments would appear to be that the second pair (II and VI) were computer-controlled, whilst Experiments I and V were not. Is it possible, then, that the computer-control might lie at the root of the discrepancy?

Such a possibility is certainly not inconceivable. For example, in both of the computer-controlled experiments it was deemed necessary to warn subjects that, once they had begun tapping, they should not hesitate or pause until told to stop. The reason for this was that any pause during burst-sampling would naturally lead to mean inter-tap intervals which were misleading. In emphasising in this way that "computers can be pedantic", it may be that the impression was conveyed to the subject that the recording system was comparatively crude, and that careful (slow?) tapping was necessary if spurious or misleading data were to be avoided. (In fact, of course, the recording system was capable of accurately recording tapping-rates far in excess of those which could be accommodated by the aural methods used in Experiments I and V.)

In conclusion, then, no evidence has been found here of a "preference" for the "spontaneous" rate, either in the objective measures of tempo, or in the Semantic Differential judgements. However, it would be unwise for several reasons to state categorically that there is no "preferred" rhythm.

As far as the Semantic Differential study is concerned, it could be argued that inappropriate scales were used and that, had more appropriate scales been used, patterns of judgements would have been obtained which revealed "preference" for the "spontaneous" rate. This is certainly possible, but it should be pointed out that the scales which were used here would be thought, prima facie, to be particularly likely to reveal any "preference". Nevertheless, future researchers might consider the possibility of conducting a larger-scale Semantic Differential study. Such a study would need to include safeguards to control the Type 1 error.

As far as the objective tempi are concerned, several suggestions have been made in this discussion. For example, it is clear that "regression" is not the only way in which a "preference" might be revealed. Another obvious index which was mentioned is the quality of output. It was suggested that this index might be studied by the use of more complex tasks than finger-tapping. Another suggestion which might best be followed up with the use of more complex and realistic tasks is that the work-period should be increased. It may be that difficulties would arise concerning the face-validity of an experiment requiring protracted periods of performance of such an elementary task as tapping. The possible use of more realistic tasks will be discussed more fully in the following chapter.

A final suggestion was that the pause between the initial performance of a task at a paced rate and the attempt at reproducing the rate (without the metronome) should be increased. This suggestion will, in fact, be followed up in Experiment VII, which is shortly to be reported.

Finally, it would probably never be possible to state categorically that there was no "preferred" tempo. This is precisely because different authors would probably attach different connotations to the word. All that can be done is to accumulate data on the behaviour of the "spontaneous" rate and then to leave the decision as to the use of words to the individual researcher. What can be said unequivocally is that, when subjects are paced for short periods at tapping-rates other than that which they spontaneously adopt, they display no tendency to modify the rate of performance, and no tendency to make more positive judgements of the "spontaneous" rate on certain evaluative scales. One of the tasks of future research will be to discover whether this conclusion is also applicable to different tasks and longer work-periods, or whether other indices can be found which suggest that the "spontaneous" rate is in some way "special".

Summary and Conclusions

1. In this experiment, it was hoped to cast light on ways in which the "spontaneous" rate of finger-tapping might be regarded as a "preferred" rate. Subjects first Performed the task at a spontaneously adopted rate, and were then paced by a metronome in subsequent trials at a number of different rates. For each subject, three of the metronome-rates were higher than his "spontaneous" rate, three were lower, and one was a "control" rate, precisely equal to the "spontaneous" rate. Subjects judged all of the tapping-rates on evaluative Semantic Differential scales.

2. With the scale "Good-Bad", no significant variation in the evaluations of the different rates was obtained. With the scale "Pleasurable-Painful", significant variation was obtained, but it was not of such a pattern as would be compatible with the hypothesis that the "spontaneous" rate is a "preferred" rate.

3. During period of work when the metronome was not in operation, the subject didplayed no tendency to modify the rate of work so that it became closer to the "spontaneous" rate.

4. The mean tapping-rate in the "control" condition was not significantly different from the mean "spontaneous" rate.

5. The mean "spontaneous" rate was significantly lower than that which was obtained in Experiment I. Though no definite explanation of this discrepancy is available, it was suggested that the computer recording system may have led subjects to adopt a deliberate style of tapping in order to avoid the possible registration of spurious data.

Experiment VII

Introduction

Earlier in this chapter, it was suggested that there was a shortcoming in the previous literature on the reliability of "spontaneous" speed, whose effect was that the undoubted reliability of such measures could not in itself be taken as evidence of "preference". It was argued that the crucial question was not whether spontaneously adopted tempo was highly reliable, but whether it was more reliable than the reproduction of tempi which were randomly assigned to subjects by the experimenter, and which, therefore, would not necessarily be "personal" or "natural" to the individual in question. Since no-one in the previous literature has compared self-paced and assigned tempi in this way, it was decided to remedy this situation in Experiment VII. The basic aim was to compare the reliability)over an interval of two weeks) of a self-paced tempo with the reliability of a tempo chosen for the subject by the experimenter. In so doing, a more sensitive test for "regression" was provided than was available from Experiment VI. (In effect, the length of Phase C of a trial had been increased from one minute to two weeks.)

Method

Design. Since data on the reliability of "spontaneous" speed are already available from Experiment I, the simplest method of accomplishing the aims of this investigation would appear to be to design an experiment which was similar to the first, but in which the tapping-rates were chosen for the subject (and presented by metronome), rather than by the subject. A comparison could then be made of the reliabilities obtained in the two experiments.

The question then arises as to the scheme by which the pacing-rates should be calculated. One suggestion might be that the rates could be determined in some way by the use of tables of random numbers. However, it would appear more satisfactory to ensure that the distribution of paced rates was approximately equal to the type of distribution of spontaneous tapping-rates which is obtained in these experiments. A simple method of achieving this would be to assign to each subject in the new experiment the rate which had been spontaneously adopted by one subject in Experiment I.

The design as laid out above is clearly not entirely satisfactory, however. The major difficulty is that any effects of the experimental manipulation (paced vs. "spontaneous") would be completely confounded with differences between the experiments. There might, for example, be differences in the mean test-retest intervals, because the actual test-retest interval for a particular subject will depend on when it is convenient for him to come for retest. Moreover, by the time that Experiment VII was being planned, it would have been impossible to reproduce exactly the conditions of Experiment I because the investigator had moved to another University, and different laboratory settings and apparatus would have to be employed. For these reasons, it was obvious that fresh data on the reliability of "spontaneous" speed would have to be collected.

The next question to be resolved was whether the comparison between paced and "spontaneous" tapping should be made between subjects or within subjects. A within-subjects design would clearly be attractive in view of the greater degree of control which it would afford, but it was thought that it might be difficult to implement this design. In such a design, of course, the subject would be required to tap twice in each session, once at a spontaneously chosen rate, and once at a paced rate. The difficulty was envisaged, therefore, that he may not be able to discriminate between the two rates. When asked in the retest session to perform the task, he may be able to recall both rates, but may not be able to remember which rate he had adopted in which condition.

Discriminability between the two instruction-conditions might be improved by arranging that the subject perform two different tasks, one in each condition. For example, a given subject might be required to perform finger-tapping at a spontaneously chosen rate and toe-tapping at a paced rate. It will be recalled that, in Experiment I, both of these activities produced high reliability and were also performed at significantly different rates. Clearly, then, discriminability was adequate with the two tasks; in the retest session, the subject was able to"attach" the appropriate rate to the appropriate task. It was decided, therefore, to employ these two tasks in this study, so that half of the subjects performed "spontaneous" finger-tapping and paced toe-tapping, and half performed "spontaneous" toe-tapping and paced finger-tapping. Subjects were assigned to these two combinations alternatively, as they arrived at the laboratory. It was, as usual, deemed undesirable to elicit a "spontaneous" rate after the subject had performed at a paced rate, so that all of the subjects in this experiment performed forst in the "spontaneous" condition and later at a paced rate.

Once more a battery of "buffer tasks" was compiled. These occupied positions 1, 2, 4 and 5 in the experimental order, with the "spontaneous-rate" tapping position 3, and the paced tapping in position 6. Two of the buffer tasks were Koh's Blocks designs, and in each of the remaining two the subject was presented with a pair of cartoon jokes, and asked to state which of the two he found the more amusing. The order of presentation of the buffer tasks was randomised for each subject. The buffer battery used in Session 1 was identical to that used in Experiment IV. A different battery of similar composition was used in Session 2.

As stated earlier, it was deemed desirable to administer paced rates whose distribution was typical of the distributions of "spontaneous" rates which are observed in these experiments. The simple method which was used to achieve this was to pace each subject at the rate which the previous subject had spontaneously adopted. The alternating scheme of assignment of subjects to conditions ensured that finger-tapping was always paced by a rate which had been obtained with finger-tapping, etc. The "spontaneous" toe-tapping rate which was used to pace the first subject was obtained from an initial subject who performed just that task. The instructions given to this subject were identical to those used in the remainder of the experiment to elicit "spontaneous-rate" finger-tapping.

Subjects. 12 male and 14 female undergraduates, enrolled in a course in introductory Psychology, attended for Session 1. Two of the females failed to return for retest. All subjects were naive with respect to the aims of the investigation.

Apparatus. A Morse key was provided for finger-tapping. For toe-tapping, the investigator constructed a wooden pedal. The pedal was not hinged or sprung, so that it offered solid resistance to the foot. The primary purpose of the pedal was to provide "bogus" instrumentation, but it also acted as a sounding-board, thereby facilitating the recording of subjects' tapping. From both pieces of apparatus wires were led away to a position out of sight of the subject. These were not, in fact, connected to recording apparatus, but were also included to provide "bogus" instrumentation.

Two audio cassette recorders were employed, and these will be referred to as the "data recorder" and the "metronome recorder". The former was used in both sessions to record the sounds produced by the subjects' tapping. It was connected to a remote microphone which was concealed under the subject's seat. The metronome recorder was used in the first session only, and provided the rate at which the subject was required to tap in the "paced" condition. Both recorders were situated on the experimenter's desk in a position where they were not visible from the subject's seat.

The recording which was played to the subject by means of the metronome recorder was originally derived from an electronic metronome of continuously variable speed. The method of use of this instrument will be described below.

In addition to the above apparatus, subjects were provided with a loose-leaf book of instructions of the usual type, a set of coloured wooden blocks, a pen, and a slip of paper on which to record their judgments of the cartoons.

Procedure: Session 1. All experimentation was carried out in the investigator's office at the University of Keele. The subject sat facing a wall in one corner of the room. The experimenter sat at his desk behind and out of sight of the subject, and a filing cabinet was interposed between subject and experimenter.

Upon arrival, the subject was shown to his seat, and the purpose of the instruction-book was explained. The experimenter then returned to his seat, where he simulated reading. At an appropriate point, the data-recorder was started, and the sound produced by this operation was masked by a "bogus" cough.

The procedure continued under the control of the typed instructions until the subject was about to begin Task 6 (the paced tapping). It was considered that oral instructions would be more suitable at this point, so that the appropriate page in the instruction book did not give directions on how to perform the task, but simply requested S to ask the experimenter for instructions. When this request was made, the experimenter left his seat and went to explain to the subject the procedure which would be used. The metronome recorder was started, and the subject was required to tap in synchrony with the recording. 30 metronome beats were delivered, and the recording was then stopped without warning. (The subject was not informed of the exact number of beats which would be delivered, but was told that the metronome would stop "after a while"). When the recording stopped, the subject's task was to continue tapping, without hesitation, until requested to stop by the experimenter. Though the subject was not informed in advance of the period for which he would be required to continue tapping, he was, in fact, stopped 30 seconds after cessation of the metronome recording.

Upon completion of the session, a time was arranged for the retest session. Subjects were reminded by letter of the appointment approximately 2 days before the arranged date.

Before the arrival of the succeeding subject, the experimenter made a new metronome recording, based on the "spontaneous rate" which had been adopted by the subject who had just been tested. The appropriate point in the data recording was first located, and the electronic metronome was switched on. A match was then made by ear between the recorded tapping and the metronome. This method provided only an approximate match but this was nevertheless all that was required. When the experimenter was satisfied that a good match had been achieved, and when the data recorder had been switched off, a new recording was made on the metronome recorder.

When the new recording had been made, the instruction book and other materials were assembled in readiness for the following subject.

Procedure: Session 2. Typed instructions were used throughout Session 2. In both of the tapping-tasks, the subject was instructed to reproduce the rate which had been adopted in the appropriate condition in Session 1. The order of presentation of the tasks in Session 2 was counterbalanced: it was planned that half of the subjects should perform the "spontaneous-rate" task first, and that half of them should perform the "paced" task first. Of the former group, half had been paced with finger-tapping and half with toe-tapping. The same was true of the latter group.

Such a design would require that the number of subjects be a multiple of four. It was decided that all subjects who returned for retest would be tested, but that data would be randomly discarded if necessary in order to achieve perfect counterbalancing.

Upon completion of the retest session, the aims of the research were described to the subject. Any questions were then discussed, and he was thanked for his co-operation.

Results

Though the number of subjects (24) who returned for retest was a multiple of 4, it was nevertheless necessary to discard the results from four of these in order to balance the design. The data which were discarded were determined by drawing lots. This procedure was carried out by a colleague of the experimenter, who had not seen the data which had been collected. The discarded data are included in the appropriate table in Appendix II.

In analysing the results from the 20 remaining subjects, the difference (regardless of sign) between the numbers of taps produced in the two sessions by each individual was first calculated. This was done for the "spontaneous" and "paced" conditions separately, so that each subject was represented by a "deviation score" for the "spontaneous" condition and one for the "paced" condition. Each deviation score was then converted into a percentage deviation score by expressing it as a percentage of the rate which the subject had adopted in the appropriate task in Session 1. The mean percentage deviation score for each condition is set out in Table 6.17. Under the hypothesis that the "spontaneous" rate is more reliable than randomly assigned rates, it would be expected that the percentage deviation score would be smaller in the case of the former than in the case of the latter. It will be seen from Table 6.17 that the difference is, in fact, in the opposite direction. Since the distributions of the percentage deviation scores were not normal, the Wilcoxon Test was used to assess the significance of the results. This analysis revealed that there was no significant difference in the accuracy of

TABLE 6.17: Experiment VII, Mean Percent Deviations

Spontaneous	Paced
21.2465	20.3065

TABLE 6.18: Experiment VII, Test-retest Correlations (Spearman)

Spontaneous	Paced
0.74**	0.73**

**$p < .01$

reproduction of the "spontaneous" and "paced" rates ($T = 113$; N. S.).

Table 6.18 presents the test-retest correlations (Spearman) for both conditions. These coefficients are very similar, and they are both significant ($p < .01$).

Discussion

In this experiment, no significant difference has been observed in the reliability with which the subject reproduced "spontaneous" and randomly assigned rates of tapping over a period of two weeks. It is important to note, however, that both rates were highly reliable. This result is important because, had the reliability of the "spontaneous" rate not been high, then the present results would have been suspect.

In discussing the previous experiment, it was suggested that a subject has a relatively broad "band of indifference" with respect to tapping tempo. It was further suggested that, when a subject is asked to tap in an experiment of this type, he might randomly choose a tempo from within that band, and that the often-reported reliability of "spontaneous" rates could be accounted for by arguing that the subject is able to recall the rate which he adopted over a period of time. The present results are certainly compatible with this model. The data do not, of course, demonstrate that the model is correct, but it is certainly more parsimonious than the suggestion that a subject has a "preferred", "personal" or "natural" (or otherwise "special") tapping-rate.

It might, of course, be objected that the failure to obtain a difference in reliability between "spontaneous" and "paced" rates was due to insensitivity in the present experiment. Specifically, it might be argued that the present test-retest interval was insufficiently long. Though this objection cannot be firmly dismissed, it should be noted that the interval used here was not unduly short, and that it is comparable with the test-retest intervals used by several previous authors. Moreover, any forgetting of a tapping-rate would be thought intuitively to proceed quickly at first, and gradually to decelerate. Thus, the test-retest interval would have to be extended to a considerable degree if this objection were to be put to the empirical test.

A more subtle objection might be that the subject does, indee , have a "preferred" rate which is highly reliable, and that any randomly assigned rate is calibrated against the standard of the "preferred" rate, and is then encoded in some reliable way in memory. For example, the encoding might be in verbal form ("20% faster than the 'preferred' rate", for instance).

It should first be noted that it would be extremely difficult to devise an empirical test of such a model. Secondly, the suggestion that the "spontaneous rate" is a highly stable standard is in need of qualification. Though, as Table 6.18 reveals, high test-retest correlations have again been obtained here, it can nevertheless be seen from Table 6.17 that there was a considerable degree of inaccuracy in the absolute values of the tempi which subjects adopted in the "spontaneous" condition in the two sessions. This degree of variability which has been revealed by the present data-analysis surely casts doubt on the feasibility of matching externally-imposed rates of work to the individual's "preferred" rate of movement, in the manner suggested by Smoll (1975a).

A third objection may be raised against the "calibration" model: though it is plausible to argue that the use of a highly reliable internal "standard" ("preferred" rate) may permit the reproduction of a wide range of assigned rates, it would appear intuitively unlikely that such a system would result in a reliability of reproduction of assigned rates which was as high as the reliability of the "standard" itself (and it is assumed that the "standard" and the "spontaneous rate" are identical). This is because the reproduction of an assigned rate would appear to necessitate more processes than the reproduction of the "spontaneous" rate, and any one of these additional processes could reasonably be expected to introduce further unreliability.

More explicitly, the implication of the "calibration" model is that, when the subject is asked to tap under "spontaneous rate" instructions, he simply outputs the "standard". When later asked to reproduce the rate which he previously spontaneously adopted, he again outputs the "standard". Any variability in the "standard" rate will, therefore, be directly reflected in the test-retest reliability of the "spontaneous" rate.

The situation when the subject is assigned a rate and subsequently attempts to reproduce it is, according to this model, somewhat more complex. In particular, three important processes would appear to be implied:

(1) when the rate is presented, a comparison is made with the "standard" rate;

(2) the result of this comparison is stored;

(3) in the retest session, the result of the comparison is recalled and is used to produce a transformation of the "standard" rate. This produces the rate which is output.

Any variability in the "standard" rate would once again be directly reflected in the test-retest reliability of reproduction of the assigned rate. However, each one of the above processes would also appear likely to introduce unreliability, and this would be additional to that which arose from variability in the "standard". In addition to these three potential sources of error, the <u>resolution</u> of the comparison process is also important. Would this system be able to discriminate, for example, between a rate 25% above the "standard" and one 26% above the "standard" ? If not - if, for example, rates between 22% and 28% above the standard were encoded as "+25%" and subsequently output as such - then this would constitute a further source of unreliability of reproduction of assigned rates.

It might, of course, be argued that the above development of the "calibration" conception is but one interpretation, so that it cannot be regarded as a refutation of the approach in general. However, it should serve to clarify the fact that a model which postulates that the assigned rates were reproduced by calibrating them against the hypothesised" preferred" rate would imply that more complex processes are involved in the reproduction of assigned rates than are involved in the reproduction of the "spontaneous" rate. It is a feature of systems in general that, unless component processes or device- are perfectly reliable, then greater complexity leads to lesser reliability. Yet there is a wealth of research demonstrating that human information-processing is not characterised by perfect reliability. Since the test-retest reliabilities in the two conditions of this experiment were very similar indeed, any suggestion that more complex processes of reproduction were required in one than in the other must, therefore, be considered highly suspect.

Summary and Conclusions

1. In this experiment, the test-retest reliabilities of spontaneously adopted and randomly assigned rates were investigated over a test-retest interval of two weeks.

2. Reliabilities in both conditions were high, but there was no significant difference between them.

3. It was concluded that the results are compatible with the suggestion that, upon being asked to tap under "spontaneous rate" conditions, the subject randomly selects a rate from within his "band of indifference" and that the proved test-retest reliability of measures of "spontaneous rate" may be accounted for if it is supposed that the subject *assumes* in the retest session that he is to work at the same rate as in the original session. All that is required is that the subject be able to recall the original rate which, according to the present results, he is indeed able to do.

CHAPTER 7:

Conclusions and Prospects

CHAPTER 7.

It is not the aim of the present chapter to provide a detailed summary of the results which have been obtained here; summaries have been provided at appropriate points in Chapters 3-6. Rather, a more general appraisal will be given of the progress which has been made both here and elsewhere, and possible directions for future research will be suggested.

The literature which was reviewed in Chapter 2 was curiously sporadic; though research has been performed in the field since the early days of experimental psychology, the topic has never been "in vogue", and no one investigator appears to have published a sustained programme of research in the area. Rather, the topic of "personal tempo" appears to be one with which researchers have briefly flirted before passing on to other interests.

It is true that this state of affairs may be in the process of changing at present; several relevant papers have been published in recent years by various writers at Waseda University in Japan, and the two recent papers of Smoll (1975a, 1975b) may mark the beginning of a coherent programme of investigations on his part. Whether this recent upsurge of interest will eventually prove to have been merely a temporary phenomenon remains to be seen.

One consequence of the lack of a sustained research programme in the past is that the literature is somewhat disorganised and characterised by a certain duplication of effort. Several investigators, for example, performed intercorrelational studies on the question of specificity-generality (Allport & Vernon, 1933; Lauer, 1933; Harrison & Dorcus, 1938; Harrison, 1941; Rimoldi, 1951). These studies all warrant the same conclusion: that the hypothesis of a unitary speed-trait is not compatible with experimental results.

Having demonstrated this, however, the investigators remained silent, as though no further questions remained to be asked.

Apparent duplication of effort is not in itself undesirable, provided that later investigations represent methodological and conceptual advances on the earlier work. However, it was seen that there have been unfortunate traditions in this field of elementary flaws of experimental design. In the main, the later writers merely continued these traditions. From this point of view, therefore, the best of these studies remains that of Allport & Vernon (1933).

Not all of the literature, however, was concerned with the issue of specificity-generality. Numerous studies exist in which an attempt has been made to relate spontaneous tempo to some other variable. Examples of such work include the research of Mishima (1969) on body-type and of Lowin et al (1971) and Nagasaki (1972) on geographical factors. These investigations, however, are like isolated outposts in an as yet unexplored territory; there is no background of knowledge to facilitate interpretation of the findings.

There is a lack of organisation and sense of direction in the literature, then. In bringing about a degree of organisation, a review and a clear statement of the important questions would be invaluable. It is hoped that this work will have gone some way towards this aim.

The spirit underlying this work was that, even though the issue of specificity-generality had been resolved, valid and interesting questions remained to be asked. For example, there was the suggestion of Smoll (1975a) that the "preferred rhythm" should be taken into account in such situations as the gymnasium or the playing field (and presumably the factory). Smoll attempted to reinforce his suggestion with data obtained with a simple laboratory task. The adequacy for this purpose of the data which he provided has been questioned here, and one of the aims of the present research was to provide more satisfactory evidence that people have "preferred" tempi of performance of one simple type of laboratory task.

In the light of the results of Experiments III, IV, VI and VII, some might be tempted to conclude that there is, after all, nothing "preferred", "personal" or "natural" about the tapping rate which the subject spontaneously adopts in experiments of this type. An alternative model of the situation was briefly outlined in the Introduction to Experiment VI, and it would appear appropriate at this point to provide a more elaborate model, taking into account results published by previous authors, as well as those presented here.

The first tenet of the model would be that the subject has a relatively broad "band of indifference" with respect to tapping rates in these studies. This postulate was suggested by the results of Experiment VI. It appeared from that investigation that there was a range of tapping rates within which no one rate offered any advantage to the subject. Common sense would suggest that there would be a physiological upper limit to the tapping-rate, and it appeared reasonable in the light of the results of Experiment II and of those obtained by Craik and Sarbin (1963) to suggest that, at least in some experiments, there may be a rate above which the subject may not be motivated to perform, but which is not a physiological maximum tempo. Both Experiment II and the previously published research would suggest that there is little or no common variance between the "spontaneous" tempo and the rate elicited by "maximum rate" instructions.

When the subject first encounters "spontaneous rate" instructions, he has to select one tempo from within the band of indifference. In the simplest possible version of the model, it might be supposed that this selection is made on a purely random basis. The tempo which is actually selected is then encoded in memory (in a way which has not yet been determined) so that, if the subject is required to repeat the task at a later date, he is able to reproduce the original tempo, having concluded that the experimenter expects the same rate of performance. This is not an unreasonable assumption because investigators performing test-retest studies usually attempt to make conditions in the different sessions as near identical as possible. That the phenomenon of reliability does not necessitate the postulate that there is something "special" about the "spontaneous" rate was demonstrated in Experiment VII.

What of the intercorrelations which have sometimes been reported? The subject's conception of the experimenter's expectations might again be invoked. If tasks appear similar to the subject, then he might reasonably conclude that a similar tempo is appropriate. When it is borne in mind that most of the clusters which have been reported have consisted of very similar tasks (involving tapping with different limbs, or different positions of the same limb, for example), then perhaps the intercorrelations also become easy to understand.

The model,appears, then, to be compatible with the results which have been obtained here, but the previous published work must be taken into account. A consideration of the literature reveals one type of finding which may not be compatible with that part of the model which suggests that the initial selection of a rate is entirely random. Numerous instances were cited in Chapter 2 of relationships between tapping tempo (or the speed of performance of some other simple laboratory task) and some variable existing outside of the laboratory, such as geographical background. Clearly, if variables of this type are related to the laboratory tempo, then they in part determine that tempo. Its determination is not, therefore, entirely random. Can the model be modified to accommodate results of this kind, or must it be abandoned? If an enduring variable such as the subject's geographical background affects his laboratory tapping-rate, then does it follow that his tapping rate is also an enduring feature which, therefore, merits the title "preferred", "personal" or "natural" tempo?

Perhaps not. One point which must be borne in mind is that subjects in this type of study have been faced with a very unstructured situation. In the present series, for example, a minimum of instructions was given, the possibility of suggestion has (hopefully) been minimised, and no demonstrations were ever provided. The result is that the subject has been given a highly open-ended task. (What does the experimenter actually mean when he writes "most natural and congenial way"?) Hence, the subject has had to interpret what was required of him. It might then be argued that people from different geographical backgrounds arrive at different interpretations of the experimenter's requirements. Nagasaki (1972) does not provide details of the geographical characteristics of the two regions (Metropolitan Tokyo and Akita Prefecture) which he investigated, but it would appear reasonable to suppose that his result (a higher tempo in the former than in the latter) represents an urban-rural difference (or at least a difference between highly urbanized and less urbanized areas) on the grounds that it is difficult to conceive of a region more strongly urban than Metropolitan Tokyo. What might be suggested, then, is that town dwellers arrive at different interpretations of the task from country dwellers. But why?

The answer to this question may be provided by Lowin et al (1971). They demonstrated that, in accordance with papular views, city dwellers conduct at least some of their everyday affairs at a higher tempo than country dwellers. Perhaps, then, the following argument might be put forward. People in these laboratory experiments decide to tap at what they consider to be a "medium" speed. Concepts of what constitutes a "medium" speed might be moulded by the pace at which everyday affairs are conducted in one's environment. If subjects in experiments on "spontaneous tempo" do elect to tap at a "medium" rate, then, those from a highly urban background might be expected to exhibit a higher rate than those from more rural surroundings.

The above argument might be thought to have undermined itself. Are not Lowin et al's results sufficient justification for the view that people do have "preferred tempi" (for example, of walking down the street) ? It certainly appears somewhat implausible to argue that the differences which these authors obtained are due to differences in interpretations of an unstructured task. May it not be, then, that those who have lived in the country for several years have a "preferred tempo" of walking, etc., in the sense that, when they go to live in a large city they find the "pace of life" (temporarily or permanently) excessively high for them ?

It certainly is possible that such "preferences" exist. What is being suggested here, however, is that there is no need for the postulate of "preference" in the case of laboratory tapping-rates. It was clearly stated in discussing Experiment VI that it would be for future researchers to investigate whether such a conclusion was generalizable to other activities. The statement that "there is no need for..." is the firmest conclusion which can be drawn from these results. It cannot be concluded that there is definitely no "preferred" tapping rate because, as has repeatedly been stressed, not all of the senses in which "preferred" might be intended have been covered in this work. Moreover, it is always dangerous to build strong theoretical edifices on the foundations of negative results. But when the literature is considered, there appears to be no positive evidence for the positive assertion that there is a "preferred" tapping rate. The onus of proof should surely lie upon those who make positive assertions, rather than those who suggest that the available evidence does not warrant the assertion in the first place. It is suggested, therefore, that the present model is more parsimonious and that it accords with the evidence, provided that the random initial selection is replaced by a selection determined by the subject's interpretation of the requirements of the experiment.

What, then, of Smoll's (1975a) suggestion as to a possible practical application of work on the "spontaneous" tempo? It certainly cannot be dismissed on the grounds of the evidence reported here. What might be suggested is that the time could now be right for those interested in this suggestion to embark upon a study of activities which are actually performed in the gymnasium or on the playing field or in the factory. It would not appear implausible to suggest, for example, that "preferences" would be more likely to exist in activities which the individual actually performs frequently in his everyday life than in one which he has probably never performed before instructed to do so by a psychologist in a laboratory. Moreover, when the literature is considered as a whole, it is seen that there has been an almost complete neglect of the possibility of studying "everyday speed" in real everyday situations. Lowin et al (1971) have demonstrated that such studies are possible and one of the tasks for future researchers will be to redress the balance, and to challenge the almost complete monopoly of laboratory studies in this field. Further consideration will shortly be given to this possibility.

However, a broadening of horizons from the traditional laboratory studies is but one possible direction for future researchers. Within the laboratory there are many interesting questions which remain to be answered. It has been suggested here that what determines the tempo at which a subject taps is his interpretation of what is required by the experimenter. It is clear that numerous factors might affect the interpretation which is arrived at by a given subject, and one interesting possibility would be to investigate these factors. Such investigations would certainly further our understanding of the significance of the simple measures which have been taken in this type of experiment, so that it would appear worthwhile here to consider some of the possibilities which might be investigated.

One possible approach would be to attempt to relate tempo to time-judgments. It was seen that Newman (1972) has already begun work in this area, and numerous reasons were suggested in Chapter 2 why it might be supposed that a relationship would exist between "everyday speed" and the perceived rate of passage of time. In Newman's study, a very low negative correlation was obtained between productive estimates and gait-tempo. This result was not particularly impressive, but Newman employed but one method of eliciting time-judgments. Such methods do not correlate perfectly, as the literature reviewed by Doob (1971) demonstrates. It would also be of interest to investigate the relationship between the spontaneous rate of finger-tapping and time-judgments. One possible hypothesis, for example, might be that subjects in experiments such as those performed here set out to tap at an estimated rate (for example, of one tap per second). Individual differences in rate (which, under these circumstances would effectively be repeated productive estimates of one second) might then be related to individual differences in productive time estimates, as measured by the traditional laboratory technique.

This is a simplified example, used only for purposes of illustration. In practice, any relationship between tempo and time-judgments would almost certainly be more complex. For example, if subjects were setting out to tap at an estimated rate in this way, it would be unlikely that they would all attempt the same rate. Though some might attempt one tap per second, others might attempt two taps per second, or one tap per two seconds. A method which might prove useful, therefore, would be to ask subjects after a period of tapping to estimate the rate at which they had been working.

In the writer's experience, one of the issues most frequently mentioned to the researcher into spontaneous tempo is the possibility of physiologica correlates. Rimoldi (1951) also made a plea for more research into this question, and he himself used one physiological measure in his study (the pulse-rate). One reason for the popularity of the suggestion that physiological factors could be relevant may be the fact that many of the tempo-measures which have been used in the past (and particularly those used by the present investigator) have been rhythmic in character, and many physiological rhythms are known to exist. The heart-rate measure which Rimoldi used is an obvious example.

However, it is clear that simplistic hypotheses, such as that the subject chooses to tap at a rate equal to his pulse, will not suffice. A glance at the distributions of tapping-tempi obtained in Experiments I and V will demonstrate the inadequacy of this suggestion, for the mean rates were of the order of 60 taps per 30 seconds. It would appear unlikely, however, that the mean pulse-rate during these experiments would have been as high as 120 beats per minute.

However, it is necessary here to make a point which is similar to the one which was made above in connection with time-judgments: it might be that some subjects choose to tap at the same rate as their pulse, but that others tap at a <u>multiple</u> of their pulse-rate. It is interesting in this connection to note the study by Jammes &

Rosenberger (1972). These authors investigated the relationship between heart-rate and spontaneous rocking-frequency in mentally retarded patients. The majority of the patients rocked at a rate which was equal to their pulse-rate, but some of them rocked at half their pulse-rate.

Just as any investigation into a possible relationship between tempo and heart-rate would have to face the possibility that not all subjects exhibited the same ratio between the two rates, so would studies of physiological correlates in general have to face the possibility that not all subjects "chose" the same physiological correlate. While some may tap at a rate which was related to their pulse-rate, others may tap at a rate which was related to some other physiological rhythm. The study of possible physiological correlates, then, would not be without its difficulties, but this no doubt represents a field with ample scope for future research.

Another major possibility for further research is concerned with personality variables. For example, as Craik & Sarbin (1963) point out, one way in which spontaneous tapping-rates may be conceptualised is as an aspect of sensory input which is under the individual's own control. Evidence is available (eg., Bartol & Martin, 1974) which suggests that there are individual differences in the preferred level of sensory input, and that these are related to such personality dimensions as introversion-extraversion. Specifically, these authors demonstrated that extraverts preferred more complex abstract designs than did introverts. They concluded, therefore, that extraverts preferred a higher level of sensory input than did introverts. One possibility, then, is that the hypothesised preference for higher levels of sensory input on the part of extraverts might manifest itself in higher rates of spontaneous finger-tapping, which might be said to produce a higher level of stimulation.

These, then, are some of the possible lines of enquiry into the factors which "set" the tapping-rate which the subject adopts in these traditional laboratory studies of self-paced tempo, and such investigations would certainly further understanding of the significance of the measures which have been made using such a situation. However, it was argued above that one shortcoming in the literature was the almost complete monopoly of laboratory investigations. To borrow from Rimoldi (1951), "...if we are searching for a study of the individual as he behaves in ordinary life..." then perhaps we should study activities which he actually performs in ordinary life. It might be argued that many of the measures which have been used in the laboratory studies do not fit readily into this category. Moreover, the effects of placing the subject in a laboratory setting and then instructing him to perform certain tasks (when he knows that he is under observation) are unknown. As a result of observing a large number of subjects in these studies, the writer has become aware that the precautions which were taken here to avoid self-consciousness do not always work. Not infrequently subjects have appeared perplexed, embarrassed or sceptical when being asked to perform such activities as counting aloud, foot-tapping, etc. Rimoldi (1951) also notes that many of his subjects expressed surprise upon encountering his first two tasks (arm-swinging in a standing position). In the case of at least some of the subjects, then, the accuracy of the adjective, "spontaneous" may be doubted (and it is for this reason that the word has been enclosed within quotes in this work). The laboratory studies might be said to be somewhat artificial, but is there any practical alternative?

The study of Lowin et al (1971) might be cited as an example of an alternative approach which should abolish self-consciousness in the subjects. It will be recalled that these authors used a more "naturalistic" method, in which people were covertly observed as they went about their everyday affairs. In comparison with their study, the laboratory investigations do, indeed, appear artificial and contrived. Why, then, has such a method not been used more frequently in the literature?

One of the major reasons for the unpopularity of this method amongst researchers may be the practical difficulties which may arise from its use. In the study by Lowin et al, for example, several researchers (working in areas with different geographical characteristics) were required, and not all researchers will be in a position to summon such support. Moreover, "naturalistic" observation techniques may be almost inapplicable to questions which have been asked by some of the laboratory researchers. The question of generality, for example, had to be answered by means of observations on a number of different activities. In a laboratory study, it is comparatively easy to ensure that the subject will perform whatever activity the experimenter wishes, and for a period of time which is considered sufficient for reliable measurement. It is clear, however, that the researcher using techniques of covert observation may have to observe a subject for a very long period before a specific piece of behaviour was spontaneously emitted, and even when the subject did spontaneously perform the activity of interest, there is no guarantee that he would continue to do so for a sufficient period to provide a reliable measure.

In the study performed by Lowin et al, subjects were only observed on one occasion. In some types of investigation, however, it may be necessary to observe the same subject on more than one occasion. It is comparatively easy, for example, to perform a test-retest study of reliability in the laboratory (though the subjects do not always return for retest !), but such a study would be fraught with difficulties if covert observations were used.

It may be that such a study could only be performed using subjects who were known to the experimenter and whom he met on a regular basis. In that event, it may prove difficult to prevent the subject from becoming aware that an investigation of some form was in progress.

In short, then, the attractions of a more "naturalistic" approach are balanced against the fact that the experimenter must relinquish a great deal of the control which is possible with a laboratory investigation. In terms of time, personnel and (perhaps) recording apparatus, covert observation techniques will not be economically practical for many investigators. Nevertheless, Lowin et al have shown that it is possible to conduct such investigations for those who have the resources to do so. It will be interesting, therefore, to consider some of the research which might be conducted.

One of the measures used by Lowin et al was the rate of walking in the street, and several interesting questions are raised by the result which they obtained - that the rate of walking in the city areas was in general higher than that in country areas. One question, for example, concerns what happens when individuals migrate from one type of area to the other. Do former rural inhabitants increase their walking-rate when they go to live in the city? If so, how quickly does the adaptation take place? Do all adapt, or only those who were formerly slow even by rural standards, for example? If adjustment occurs, does it do so in both those migrating from the country to the city and in those migrating in the reverse direction, or is the effect unidirectional? What other activities display a difference between town and country? Most fundamentally, what are the factors which bring about such differences?

Questions might also be asked concerning the effects of discrepancies between the spontaneous tempi of individuals who co-operate in performing an activity. What happens, for example, when a slow walker and a rapid walker walk together? Does one of them dictate the pace, or is a compromise struck? Does the answer to this last question depend on the relationship between the individuals, their personalities and aspects of the situation? If one of the walkers is obliged to relinquish his normal pace, what adverse effects can be observed in him? Similar questions might also be asked regarding other everyday activities, such as conversations.

Whilst discussing tasks which are more typical of everyday life than those which have been used in the traditional laboratory studies, it should be noted that it was suggested earlier that more realistic tasks might be used in the future by researchers interested in Smoll's (1975a) suggestion of a possible practical application of hypothetical "preferred" tempi. This suggestion was made because of misgivings regarding the face-validity of experiments requiring extended periods of performance of such simple tasks as the finger-tapping which was used in Experiment VI. An interesting question concerns whether it would be possible to perform a "pacing experiment" using a task which is actually performed in some specific industrial situation. A requirement of such a task would be that no initial period of learning was required; otherwise, there may be an increase during the initial period in the tempo at which the subject spontaneously performs the task.

One task which might be used is one typical of those performed in packing or sorting departments in factories, where objects are presented to subjects by means of a conveyor belt. Such a task might be used in laboratory situations, with the consequent advantages of control which such methods provide, but the experiment may have greater face-validity than one using the more traditional type of tasks.

For example, an apparatus could be constructed which consisted of a variable-speed conveyor belt, which could be used to present various types of objects to the subject. There are numerous types of task which could be performed, but possibly the simplest would be an elementary sorting-task in which, for example, two different types of object were presented in random order, and the subject was required to sort the objects into two separate containers. If the discrimination-task were sufficiently simple, then little or no learning would be required.

If the apparatus were so designed that the subject had access to the speed-control of the belt, then a measure of the "spontaneous" rate of work could be obtained. In a "pacing experiment", the speed of the belt would then be set in turn at several rates above and below the "spontaneous" tempo. Measures of the quality of output might then be obtained from error-rates and after a period of work the subject might be permitted to reset the speed of the belt. If the "spontaneous" rate is "preferred", then it would be expected that he would have a tendency to reset the speed of the conveyor so that it was nearer to his "spontaneous" rate.

Though the study of simple tasks requiring little practice would be the most convenient, the role of skill might later be investigated by the use of tasks requiring more complex discriminations, or an element of manual dexterity might be introduced by incorporating a simple assembly operation in the task. An interesting question in this context concerns whether individual differences in unpractised tempo might persist into practised performance.

Another question which might be investigated using this method would be that of co-operation between subjects with different "spontaneous" rates. If two people are working on the same belt, for example, is it desirable that they should be matched for "spontaneous" rate?

Finally, in the working environment, the factors which "set" the rate of work which the subject would choose are far more complex than those which operate in the laboratory. For example, there are incentives and other pressures. Research into tempi which are free from such factors does not, therefore, present a complete account. However, it may prove possible to simulate such factors in the laboratory by means of incentives of various types, including actual payment. There is clearly ample scope for future researchers here.

The field of "personal tempo", then, is one which will furnish ideas for research-projects for a long time to come. Understanding of the significance of the measures taken in the traditional type of laboratory study is still at an elementary level, and the more "naturalistic" approach exemplified in the paper of Lowin et al (1971) has hardly yet begun. Smoll's (1975) suggestion as to a possible practical application of "preferred" tempi is deserving of further investigation. Whilst it appears obvious to "common sense" that it is possible to pace individuals at a rate which is too <u>high</u> for them, the possibility that it is also possible to pace them at an excessively <u>slow</u> rate receives less attention. Only further research will reveal whether or not people do have "preferences of voluntary movement tempo" which should be taken into account under conditions where the tempo of work is normally dictated from without.

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APPENDIX I:

Instructions and Materials

SECTION A:

Experiment I

1. Instructions to Subjects

(a) Introduction to Session 1

In this experiment, we are interested in certain aspects of some simple activities. It is not necessary for you at the moment to know the purposes of the investigation, so do not concern yourself with trying to guess the aims behind it. And we are not interested in how fast you can perform the activities, so just do them in your most natural, congenial way.

When you have finished reading this Introduction, tell the experimenter, and, if you have any questions, ask them at this stage. He will then ask you to pass on to the first task, the instructions for which you will find on the next page. Never pass on to the instructions for the next task until the experimenter asks you to do so.

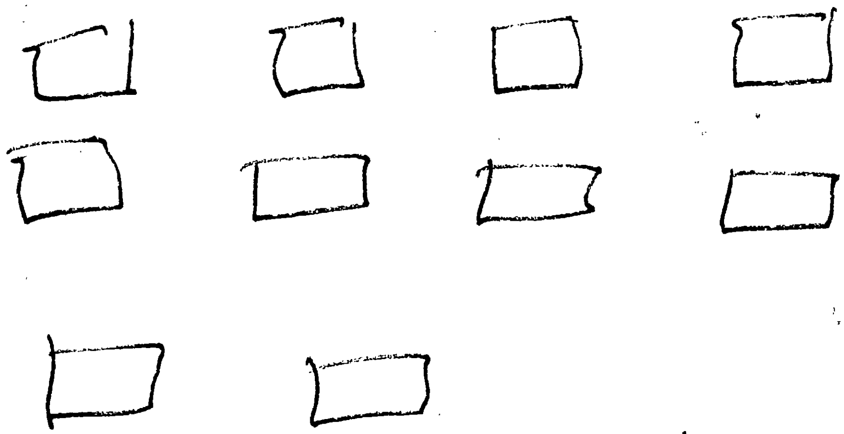
(b) Introduction to Session 2

This session consists of a repeat of the activities done in the first session, so that the measures taken then can be checked. As in the first session, we want you to perform the tasks in your most natural, congenial way.

(c) "Warm-up" Task

The first task involves drawing squares.

When the experimenter says "go", begin drawing rough squares, as follows:



.....etc.

Use one of the pieces of writing paper provided, and continue until the experimenter says "stop".

(d) <u>Task 1: FL</u>

On the desk in front of you, there is a tapper-key. When the experimenter says "go", begin tapping, <u>using the left hand</u>. Tap regularly, so that there are more or less equal intervals between successive taps. Continue until the experimenter says "stop".

Using the tapper-key on the desk again, tap as before, but this time using the left hand. Again tap regularly, so that the intervals between successive taps are more or less equal. Begin when the experimenter says "go" and continue until he says "stop".

(d) <u>Task 2: FR</u>

On the desk in front of you, there is a tapper-key. When the experimenter says "go", begin tapping, <u>using the right hand</u>. Tap regularly, so that there are more or less equal intervals between successive taps. Continue until the experimenter says "stop".

Using the tapper-key on the desk again, tap as before, but this time using the right hand. Again tap regularly, so that the intervals between successive taps are more or less equal. Begin when the experimenter says "go" and continue until he says "stop".

(e) <u>Task 3: TL</u>

On the floor beneath the desk, there is a foot-switch. The task is to depress the switch regularly and repeatedly, so that there are more or less equal intervals between successive presses. <u>Use the left foot</u>. Begin when the experimenter says "go" and continue until he says "stop".

Using the foot-switch on the floor again, press repeatedly as before, but this time using the left foot. Again, press regularly, so that the intervals between successive presses are more or less equal. Begin when the experimenter says "go" and continue until he says "stop".

(f) Task 4: TR

On the floor beneath the desk, there is a foot-switch. The task is to depress the switch regularly and repeatedly, so that there are more or less equal intervals between successive presses. Use the right foot. Begin when the experimenter says "go" and continue until he says "stop".

Using the foot-switch on the floor again, press repeatedly as before, but this time using the right foot. Again, press regularly, so that the intervals between successive presses are more or less equal. Begin when the experimenter says "go" and continue until he says "stop".

(g) Task 5: CA

Cross out all of the e's in the following passage. Begin as soon as you like. (The passage is reproduced under "Experimental Materials").

(h) Task 6: COU

Count to 30 (aloud). Begin as soon as you like.

(i) Task 7: Reading aloud (Fiction)

Read the following passage aloud. Begin as soon as you like. (The passage is reproduced under "Experimental Materials").

(j) Task 8: Reading aloud (science)

Read the following passage aloud. Begin as soon as you like. (The passage is reproduced under "Experimental Materials").

(k) Task 9: CD

On the desk, you will find a piece of paper with two circles drawn on it. The task is to tap alternately in these two circles, using the pencil provided. Begin with the pencil in the left-hand circle, tap in the right circle, then in the left, etc. Begin when the experimenter says "go" and continue until he says "stop".

(l) Task 10: E

This task involves writing e's. When the experimenter says "go", begin writing e's as follows:

e e e e

e e e e

e e e .....etc.

Use one of the pieces of writing paper provided, and continue until the experimenter says "stop".

(m) Task 11: CPY

Copy the following passage on to one of the sheets of writing paper provided. Begin as soon as you like. (The passages are reproduced under "Experimental Materials").

2. Experimental Materials

(a) Passage for Cancellation

The progress of science is the work of creative minds. Every creative mind that contributes to scientific advance works, however, within two limitations. It is limited first by ignorance, for one discovery waits upon that other which opens the way to it. Discovery and its acceptance are, however, limited also by the habits of thought that pertain to the culture of any region or period, that is to say, by the Zeitgeist; an idea too strange or preposterous to be thought in one period of western civilization may be readily accepted as true only a century or two later. Slow change is the rule - at least for the basic ideas. On the other hand, the more superficial fashions as to what is important, what is worth doing and talking about, change much more rapidly, depending partly on discovery and partly on the social interaction of the wise men most concerned with the particular matter in hand - the cross-stimulation of leaders and their followers, of protagonists and their antagonists. (Boring, 1950; P. 3).

(b) Fictional Passage for Reading Aloud

Something has happened to me; I can't doubt that any more. It came as an illness does, not like an ordinary certainty, not like anything obvious. It installed itself cunningly, little by little; I felt a little strange, a little awkward, and that was all. Once it was established, it didn't move any more, it lay low and I was able to persuade myself that there was nothing wrong with me, that it was a false alarm. And now it has started blossoming.

I don't think the profession of historian fits a man for psychological analysis. In our work, we have to deal only with simple feelings to which we give generic names such as Ambition and Interest. Yet if I had an idea of self-knowledge, now is the time when I ought to use it.

There is something new, for example, about my hands, a certain way of picking up my pipe or my fork. Or else it is the fork which now has a certain way of getting itself picked up, I don't know. Just now, when I was on the point of coming into my room, I stopped short because I felt in my hand a cold object which attracted my attention by means of a sort of personality. I opened my hand and looked; I was simply holding the doorknob. This morning, at the library, when the Autodidact came to say good morning to me, it took me ten seconds to recognise him. I saw an unknown face which was barely a face. And then there was his hand, like a fat maggot in my hand. I let go of it straight away and the arm fell back limply.

In the street too there are a great many suspicious noises to be heard.

So a change has taken place in the course of these last few weeks. But where ? It's an abstract change which settles on nothing. Is it I who has changed ? If it isn't I, then it's this room, this town, this street; I must decide. (Sartre, 1965; Pp. 13-14).

(c) Scientific Passage for Reading Aloud

By far the greatest stride forward in the reduction of jet noise has been made with the introduction of the turbo-fan engine, such as the Rolls-Royce RB 211 powering the Lockheed Tristar airliner. This engine has what is known as a high by-pass ratio. The first stage of the compressor is greatly enlarged to be more of a multi-bladed fan than a compressor stage. This is surrounded by a large cowl. Then follows the rest of the compressor, which is smaller in diameter, as is the body of the engine. A large proportion of the air handled by the first stage is spilled out over the body of the engine through the back of the large cowl. Only a small amount of air enters the engine. The result is that although a large mass of air is accelerated by the engine, only a small part of it passes through the combustion chambers and is emitted as a high-speed jet. Most of the air passes at ambient temperature along the outside of the engine and creates a "buffer-zone" between the turbulent jet and the atmosphere when it reaches the rear of the nozzle. For a start, the air which has been by-passed is not as turbulent as the jet, therefore reducing noise, but also the mixing of the gases of the jet with the cold and atmospheric air is made smoother because of the intermediate layer of by-passed air.

Things would certainly be simpler if jet noise were the only source of noise from a turbo-jet (or turbo-fan) engine, but it is not. There is also the disturbance caused by the interaction of compressor blades and, to a lesser extent, turbine blades. The latest generation of Rolls-Royce engines embodies a major innovation on this core, because the normal inlet guide vanes have been entirely omitted, eliminating an important disturbance caused by each blade suddenly meeting a drop in air velocity every time it passes behind a stationary vane. Noise from rotor/stator interaction, where it does occur, has been reduced by using wider spacing between rotors and stators, and by incorporating ideal ratios between the total number of blades of each. (Taylor, 1970; P. 194).

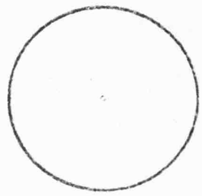
(d) <u>Passage for Copying, Session 1</u>

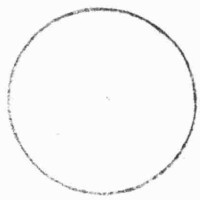
Consider how an ordinary day is put together. You awake, and as you lie in bed, or perhaps as you move slowly about in a protective shell of morning habits, you think about what the day will be like - it will be hot, it will be cold; there is too much to do, there is nothing to fill the time; you promised to see him, she may be there again today. If you are compulsive, you may worry about fitting it all in, you may make a list of all the things you have to do. Or you may launch yourself into the day with no clear notion of what you are going to do or how long it will take. But, whether it is crowded or empty, novel or routine, uniform or varied, your day has a structure of its own - it fits into the texture of your life. And as you think what your day will hold, you construct a plan to meet it. What you expect to happen foreshadows what you expect to do. (Miller, Galanter and Pribram, 1960; P. 5).

(e) <u>Passage for Copying, Session 2</u>

I often wondered who it was whom Maria really loved. I think she loved the young Pablo of the saxophonewith his melancholy black eyes and his long, white, distinguished, melancholy hands. I should have thought Pablo a somewhat sleepy lover, spoilt and passive, but Maria assured me that though it took a long time to wake him up he was then more sensuous and forward and virile than any prize fighter or riding master. (Hesse, 1965).

(f). Speciment Targets for Circle-dotting





SECTION B:

Experiment II

1. Instructions to Subjects

(a) Introduction

In this experiment, we are interested in various aspects of some simple activities. It is not necessary for you to know the exact aims of the experiment at the moment, but they will be fully divulged to you at the end of this session.

The experiment is largely under your own control, and most of the data are being recorded by the Departmental computer. The instructions for the various tasks you will be asked to perform are typed on the following pages. When you have finished one task, but not before, you may pass on to the instructions for the next. Please do not write on any of the typed sheets; writing paper is provided for those activities for which it is necessary.

In some of the instructions, you will be asked to "start as soon as you like, and continue until you hear the tone". You may now familiarise yourself with the sound of this tone by pressing the Morse key on the desk. If no mention is made of the tone in the instructions for a particular activity, this is because it is then obvious to you when you have completed the task, and it will not be sounded.

Please be sure that you fully understand the instructions for a task before you attempt it.

Read them more than once if you wish.
Sometimes, you will be asked to use button A or button B. Please be sure that you are using the correct one and, once you have started, don't stop until the end of the activity is signalled, or you will confuse the computer. If there is anything in any of the instructions which you cannot understand, you may summon the Experimenter by pressing the Morse key three times, but only do this as a last resort.

It is very important to remember that you are in no way being tested. We are not interested in how quickly or how well you can perform the tasks; just do them in your most natural, congenial way.

The experiment is in two parts, with a short rest interval between them. If you have no difficulties, you may now pass on to the instructions for the first task.

(b) "Spontaneous rate" Finger Tapping, SFL, SFR

Tap on button A (B), using the middle finger of the left (right) hand. (Rest your wrist on the desk). Tap regularly, so that there are more or less equal intervals between successive taps. Begin as soon as you like, and continue until you hear the tone.

(c) "Spontaneous rate" Tapping with Reading, SFLR, SFRR

Next is an activity which involves tapping whilst reading aloud. Overleaf, you will find a prose passage. Read it aloud, and at the same time tap on button A (B), using the middle finger of the left (right) hand. (Rest your wrist on the desk). Tap regularly, so that there are more or less equal intervals between successive taps, and begin tapping and reading at the same time. Begin as soon as you like, and continue until you hear the tone.

(The prose passages used in these tasks are reproduced under "Experimental Materials).

(d) "Spontaneous rate" Alternate Tapping, SAT

This task involves tapping alternately on buttons A and B. Use the middle finger of the left hand for button A and the middle finger of the right hand for button B, and proceed by tapping left, right, left, right, etc. (Rest your wrists on the desk). Tap regularly, so that there are more or less equal intervals between successive taps. Begin as soon as you like, and continue until you hear the tone.

(e) "Spontaneous rate" Unison Tapping, SUT

The next task involves tapping simultaneously on buttons A and B. Use the middle finger of the left hand for button A and the middle finger of the right hand for button B, with both hands moving in unison (both up, both down, etc.). Rest your wrists on the desk. Tap regularly, so that there are more or less equal intervals between successive taps. Begin as soon as you like, and continue until you hear the tone.

(f) Introduction to Part 2

In some of the instructions of Part 2, you will be asked to perform as quickly as you can. Please try not to take short-cuts to achieve this, however; your maximum rate of performance of the whole task, not of part of it, is of interest. If you find yourself becoming tired, you may take a rest at the end of an activity. Please don't rest in the middle of one, however; remember that the computer is stupid.

Note that some of the instructions do not ask you to perform as quickly as you can; you should then perform those activities in your most natural and congenial way. As with Part 1, be sure that you understand the instructions before you do anything, and always use the buttons specified in the instructions.

(g) "Maximum rate" Finger Tapping, MFL, MFR

Tap as quickly as possible on button A (B), using the middle finger of the left (right) hand. (Rest your wrist on the desk). Tap regularly, so that there are more or less equal intervals between successive taps. Begin as soon as you like, and continue until you hear the tone.

(h) "Maximum rate" Tapping with Reading, MFLR, MFRR

Next, we have tapping and reading aloud again. Overleaf, there is a prose passage. Read it aloud, and at the same time tap on button A (B), using the middle finger of the left (right) hand. (rest your wrist on the desk). This time, however, tap as quickly as possible (though read at your normal speed). Tap regularly, so that there are more or less equal intervals between successive taps. Begin as soon as you like, and continue until you hear the tone.

(i) "Maximum rate" Alternate Tapping, MAT

The next task involves tapping alternately (left, right, left, right, etc.) on buttons A and B, but this time tapping as quickly as possible. Use the middle finger of the left hand for button A and the middle finger of the right hand for button B. (Rest your wrists on the desk). Tap regularly, so that there are more or less equal intervals between successive taps. Begin as soon as you like, and continue until you hear the tone.

(j) "Maximum rate" Unison Tapping, MUT

The next task involves tapping simultaneously on buttons A and B, but this time performing as quicklty as possible. Use the middle finger of the left hand for button A and the middle finger of the right hand for button B. (rest your wrists on the desk). Remember tha~~t~~, in simultaneous tapping, both hands move in unison (both up, both down, etc.). Tap regularly, so that there are more or less equal intervals between successive taps. Begin as soon as you like, and continue until you hear the tone.

(k) Reading Aloud, R1, R2

On the following page, you will find a prose passage. The task is simply to read it aloud

(The passages which were used in these tasks are reproduced under "Experimental Materials").

(l) Wooden Blocks Designs

Construct the following design with the wooden blocks:

(A coloured plan view of the design was inserted here)

Replace the blocks into the container when you are satisfied that you have completed the design.

(m) <u>Copying Sentences (statistical approximations)</u>

Copy the following sentence on to the writing paper provided:

(The sentence was typed here).

2. Experimental Materials

(a) Passage for Reading Aloud, R1

(As for task Sc. of Experiment I. See Appendix I/A/2, Section (c).

(b) Passage for Reading Aloud, R2

Consider for a moment the family record player. It is a machine with a routine, or program, which it follows whenever it is properly triggered. The machine has a routine for changing records. Whenever the appropriate stimulus conditions are present - for example, when the arm is near enough to the spindle, or when a particular button is pressed - the routine for changing the record is executed. There is even a "sense organ" that discriminates between ten-inch and twelve-inch records and there are effectors which push the next record into place and lower the tone arm gently into the groove of the record. The entire performance is obviously voluntary, for no matter how we curse the machine for failing to play the record we want, we shall not alter its sequence of operations. The routine for changing records is built into the machine, locked in, and it never guides the actions of any other machine.

However, the record which is played by the machine is also a programme. It is a programme that controls the small movements of the stylus and, simultaneously, the larger and audible movements of the diaphragm of the loudspeaker. But the record is a communicable programme. It can be played on any one of a large class of machines. Machines that can use communicable programmes, that can share them with other similar machines, are obviously more flexible than those that cannot. The fixed cycle of the record changer makes it less flexible than the phonograph stylus, which can follow an indefinite number of different patterns of movement.

Communicability is an extremely important property that a programme - or a plan - can have. Communicable programmes are not limited to the mechanical world; the chromosome is an example of a communicable programme in biological form. At the behavioural level, of course, communicable Plans play the central role in our educational process. (Miller, Galanter and Pribram, 1960).

(c) Passages for Reading with Tapping

1. What can we do to the exhaust or intake of any internal combustion engine ? We are all very familiar with that rusty tube which carries the exhaust from a car engine, and all too often we get a glimpse inside a silencer as a final flake of rusty metal drops away and all the world knows

that another silencer has gone. A car silencer sawn open is a rather boring shell of a thing with a few tubes here and there. How does it work ?

It can, of course, use the principle of the small circular lined duct (a straight-through silencer). However, exhaust (and intake) noise contain most of their energy at low frequency, particularly at the combustion rate of the engine, and when, as usual, there are cylinder-to-cylinder and stroke-to-stroke variations in combustion, this can cause sub-harmonics of even lower frequency.

We know that absorption silencers are not at their best at low frequency, and it is just as well that it is possible to do the same job by a totally different method and achieve good low frequency attenuation, even if it is at the expense of some back-pressure. This type of silencer works on reactive, as opposed to absorption, principles. The most fundamental type of reactive silencer is the simple expansion chamber, containing nothing at all but air. Once again, it is a question of impedance mis-match, but, rather than draw an electrical analogy, it may help to explain what happens by comparing the system to an anti-vibration mounting arrangement.

2. However, if you remember that what we are enclosing is a diesel alternator, there are going to be some problems, of which cooling can be quite a serious one. It may be necessary to pass several thousand cubic metres of air per hour in and out of the enclosure if the engine radiator is part of the set. How can you let air in and out without letting sound out ?

The first necessity is to cut an inlet and an outlet aperture in the wall. This will cope with the air and let out a blast of noise. As usual, the hole can be regarded as a sound-source in its own right and it will radiate sound hemispherically. Interference effects will come into play with increasing frequency, causing the hole to radiate most high-frequency noise directly in front of it. What can be done about it ? What can ever be done to silence noise travelling in a gas stream along a pipe or a duct ?

If a duct is fitted to the aperture, sound will then travel along the duct in two ways. Some sound waves will enter the duct obliquely; they will travel along it by being reflected from side to side. Other waves will travel straight down the duct as plane waves without touching the sides. Obviously, if the walls of the duct are not reflective - that is, they are absorbant - the first type of wave will not get very far. How far they will get will depend on their angle, the duct width and the absorption coefficient of the duct lining. With an average type of lining, the waves incident at an angle of greater than 30 degrees will be reduced to the level of the plane waves within about four duct widths.

3. Next time you examine the tyres on your car, or before they wear out, look at the pattern of the tread, particularly at the edges. You will see that the transverse cuts between the sections of rubber are by no means regularly spaced, but that the distance alternates progressively between narrow and wide. This is done to prevent the tyre from "whistling", instead of which it produces more of a random hiss.

A section of the M10 motorway was given an experimental surface which was basically concrete with transverse slits to drain water away. Unfortunately, the slits gave rise to loud whistling noises when driven over, and anxious drivers were to be seen on the hard shoulder peering at their back axles. Another section was tried with simple alternate spacing between slits, but, as would be expected, this still produced pure tones. Only if a completely random or a carefully calculated progressive alternation between narrow and wide spaces is used can this problem be overcome. The latter method has the effect of "warbling" the note fast enough for it to sound more like broad-band noise, while both methods distribute the sound energy over a wider frequency range. The problem is not, as was thought, one of tyre resonance.

This introduces the interesting possibility of providing talking road signs by serrating the surface of the road in a manner similar to the undulations in the groove of a gramophone record. The voice from the road would unfortunately vary from Paul Robeson at slow speeds to Donald Duck at high speeds.

4. What technical developments can we expect in the future ? What will the noise from machines be like ? The answer is that some will be noisier, some quieter, and some the same. The aero-engine industry will probably just about keep pace with the rising size of jet engines and prevent large increases in noise. Vertical take-off and landing will dramatically alter the noise contours around airports, increasing the noise in the immediate neighbourhood, but as a greater altitude can be reached in a shorter distance, inhabitants in the medium field will benefit, those in the far field will probably find little difference.

Diesel engines are in grave danger of becoming much noisier as casting methods improve and unstressed parts of the engine become thinner and thinner. When the walls of a crank-case become as thin as five millimetres, noise radiation starts to go up at a greatly increased rate. Much of the necessary research on engine noise has already been done, and it has long been possible to silence exhausts very satisfactorily. It is simply a matter of cost, and unless consumer demand or the law forces engine manufacturers to adopt silencing measures, it would be suicide for them to add cost when they can sell as many noisy engines as quiet ones.

We can certainly expect some improvement in some corners from the use of new or different materials. Plastics have much better inherent damping than steel, and we have already seen the use of carbon fibre in the fan-blades of aero-engines. We shall see more and more plastic motor cars, and it is partly because of the money tied up in metal-working plant that we do not see more now. However, plastic has less mass than steel, and if external damping is applied to the latter, it becomes a good insulator.

SECTION C:

Experiment III

1. Instructions to Subjects

(a) Introduction

In this experiment, you will be asked to perform a few very simple tasks. There is no need at present for you to know the purposes of the investigation, but they will be explained to you at the end of the experimental session.

The experiment is largely under your own control; instructions for the various tasks are typewritten on the following pages. In some cases, the instructions ask to to begin as soon as you like, and the Experimenter will say "stop" when you have finished. In others, it will be obvious to you anyway when you have finished. In either case, when you have finished a task, you may turn over the page and begin the next one.

If there are any questions, ask the Experimenter now. If not, turn over and start the experiment.

(b) Reciprocal Tapping

The next task is called the reciprocal tapping task. On the desk you will find a piece of paper labelled "Practice Sheet 1", with two pairs of parallel lines printed on it. These oairs of lines will be referred to as "targets". Place the paper on the desk so that one of the targets is to your left and the other is to your right. The task is simply to dot alternately in the two targets (left, right, left, right, etc.) using the pencil provided.

Hold the pencil as if for writing and dot once inside the left target. Dot smartly, so that the pencil leaves a mark and also makes a noise. Then dot in the right target, then in the left, back to the right, etc. You will probably find it most convenient if you hold your arm free - don't rest it on the desk. Perform the task in your most natural, congenial way. Begin as soon as you like, and continue until the experimenter says "stop".

Using "Practice Sheet 2", practise the reciprocal tapping task again in your most natural and congenial way. Begin as soon as you like, and continue until the experimenter says "stop".

Using Target Sheet 1 (etc.) perform the reciprocal tapping task in your most natural, congenial way. Begin as soon as you like, and continue until the experimenter says "stop".

(The target-sheets used in this experiment are reproduced under "Experimental Materials").

(c) Cartoon Ratings

On the following page, you will find two cartoons. Your task is simply to decide which one of the two you find the more amusing. We really want your first impressions, so don't linger over a decision. When you have decided, indicate on the paper entitled "Cartoon Ratings" which of the pair you prefer by writing the letter associated with it (A or B) by the appropriate number (note that the pairs are numbered on the top right hand corner of the page on which they appear).

You will occasionally come across other such pairs of cartoons and though these instructions will not be repeated, your task will be the same. Whenever you find a pair of cartoons, write "A" or "B" by the appropriate number, depending on which you find more amusing on first impressions.

2. Experimental Materials

(a) Targets for Reciprocal Tapping

The target-sheets used are reproduced on the following 12 pages. The reproductions are full-size, except that the boundary lines have here been shortened by approximately 45mm.

The identification letter of each target appears in the bottom right corner of the page on which it is printed.

B

D

E

E

F

G

H

I

J

K

SECTION D:

Experiment IV

1. Instructions to Subjects

(a) Introduction

In this experiment, you will be asked to perform a small number of very simple tasks. It is not necessary for you at the moment to concern yourself with the aims behind the investigation, but they will be fully explained to you later.

The experiment is largely under your own control; Instructions for the various tasks are typewritten on the following pages. In two of the six tasks to be performed the instructions direct you to begin as soon as you are ready, and the experimenter will indicate to you when you have finished by saying "OK". In the remaining tasks, it will be obvious to you anyway when you have finished, so he will not say anything. In either case, once you have finished a task, you may turn over the page and proceed to the next activity. If you wish to take a short interval between tasks, you are quite free to do so.

It is important to realise that there is no sense in which this experiment is a test; we are not interested in how quickly or how well you can perform the tasks, so just perform them in your most natural and congenial way.

If you have any questions at this point, the experimenter will answer them. If not, then you may turn over the page, and you will find the instructions for the first task.

(b) Reciprocal Tapping

The next task is called the Reciprocal Tapping task. On the desk you will find a piece of paper (labelled "Sheet 1") with two pairs of parallel lines printed on it. These pairs of lines will be referred to as "targets". Place the paper on the desk so that one of the targets is to your left and the other is to your right (ie, so that the word "TOP" is the right way up). The task is simply to dot alternately in the two targets (left, right, left, right, etc.) using the felt-tip pen.

Hold the felt-tip as if for writing and dot once inside the left target. Dot smartly, so that the pen leaves a mark and also makes a noise. Then dot in the right target, then in the left, back to the right, etc. You will probably find it most convenient if you hold your arm free - don't rest it on the desk. Perform the task in your most natural and congenial way. Begin as soon as you like, and continue until the experimenter says "OK".

Perform the Reciprocal Tapping task in your most natural and congenial way, but this time using the paper labelled "Sheet 2". Begin as soon as you like, and continue until the experimenter says "OK".

(c) <u>Blocks Designs</u>

As for Experiment II. (See Appendix I/B/1, Section 1).

(d) <u>Cartoon Ratings</u>

As for Experiment III. (See Appendix I/C/1, Section c).

2. Experimental Materials

(a) Targets for Reciprocal Tapping

These were identical to Targets J and K of Experiment III. (See Appendix I/C/2).

SECTION E:

Experiment V

1. Instructions to Subjects

(a) Introduction

As for Experiment III. (See Appendix I/C/1, Section a).

(b) Finger Tapping

On the desk, you will find a telegraph key. The task is simply to tap on it, using your preferred hand (right if you are right-handed, etc.). Tap regualrly, so that there are more or less equal intervals between successive taps (don't pretend you are sending messages in Morse). Begin as soon as you like, and continue until the experimenter says "stop".

(c) Cartoon Ratings

As for Experiment III. (See Appendix I/C/1, Section c).

SECTION F:

Experiment VI

1. Instructions to Subjects

(a) Introduction

In this experiment, we are interested in various aspects of some simple activities. It is not necessary for you to know the exact purposes of the investigation at the moment, but they will be fully explained to you at the end of this session.

The experiment is largely under your own control; instructions will be found on the following pages and the Experimenter will give you further directions as appropriate. In some of the tasks the computer will signal to you when you should stop by sounding a tone. Please demonstrate this sound to yourself now, by pressing the "Start" button.

There is one important thing to remember; since computers can be rather pedantic, once you have started doing a task, you must continue with it. Make sure you fully understand what you are supposed to do before starting, therefore. (If you do make a mistake, it is possible for the Experimenter to take over control of the computer, so ask for help if you need it).

If you have no questions, you may now start the first task, the instructions for which you will find on the following page.

(b) "Spontaneous rate" Finger Tapping, Trial 0

On the desk in front of you, there is a key labelled "key A". Your task is to tap on this key, using whichever hand you prefer. Tap regularly, so that the intervals between successive taps are more or less equal. Do the task in your most natural and congenial way. Begin as soon as you like, and continue until you hear the communication-tone.

(c) Semantic Differential

On the desk, you will find a piece of paper labelled "Trial 0" (etc.), containing a series of rating-scales. Your task is to fill in these scales, according to how appropriately they describe the rate of tapping which you adopted in the previous task. Fill in the scales as follows:

```
                                   X
BLACK ... ... ... ... ... ... ... WHITE
```

(5th. scale-point from the left indicated).

If you find any of the scales not really appropriate, indicate the central scale-point.

Please do not linger over your judgments; give first impressions.

(An example of the forms is given in Appendix III).

SECTION G:

Experiment VII

1. Instructions to Subjects

(a) Introduction to Session 1

In this experiment, we are interested in certain aspects of some simple activities. The detailed aims behind the investigation will be explained to you at the end of the second part, in about a fortnight. For the moment, don't concern yourself with the purposes of the experiment.

The experiment is largely under your own control, and the instructions you will require are typed on the following pages. In some cases, the experimenter will tell you when you have finished a task, but in most cases it will be obvious to you when you have finished. When you have completed a particular task, you may turn over the page and pass on to the instructions for the next one.

It is important to bear in mind that this experiment is in no sense a test. We are not interested in how quickly or how well you can perform the various activities, so just do them in your <u>most natural and congenial way</u>.

The experimenter's part in the procedure is minimal. However, remember that if you don't understand something, then you can ask him to clarify a point. If you have any questions at this stage, then ask them. Otherwise, you may turn over the page and begin the first task.

(b) <u>"Spontaneous rate" Tapping, Session 1</u>

On the desk, there is a telegraph key. Your next task is simply to tap on this key using the preferred hand (right if you are right-handed, etc.). In performing the task, you will probably find it most comfortable to rest your wrist on the desk, and to grip the knob of the key between the thumb and fingers.

Tap regularly, so that there are more or less equal intervals between successive taps. You may begin as soon as you like, and continue until the experimenter tells you to stop.

On the floor beneath the desk, there is a pedal. Your next task is simply to tap on this pedal, using whichever foot you prefer. Note that the pedal is not hinged or sprung, so that it will not "give" as you tap. In performing the task, you will probably find it most comfortable to rest your heel on the floor, and to tap on the pedal with the ball of the foot.

Tap regularly, so that there are more or less equal intervals between successive taps. You may begin as soon as you like, and continue until the experimenter tells you to stop.

(c) "Paced" Tapping, Session 1

We now come to the final task in the first part of the experiment. The experimenter will give you oral instructions for this, so please tell him now that you want the instructions for the last task.

(d) "Spontaneous rate" Tapping, Session 2

You will remember that last time you performed a finger-tapping task with the telegraph key on the desk. Your next task is to do this again, working at the same speed that you adopted last time. If you think you can't remember the speed you adopted, then guess - it is surprising how well people can, in fact, remember these things.

Begin when you are ready, and continue until the experimenter tells you to stop.

You will remember that last time you performed a foot-tapping task with the pedal on the floor. Your next task is to do this again, working at the same speed which you adopted last time. If you think you can't remember the speed you adopted, then guess - it is surprising how well people can, in fact, remember these things.

Begin when you are ready, and continue until the experimenter tells you to stop.

(e) "Paced" Tapping, Session 2

You will remember that last time you performed a finger-tapping task with the telegraph key on the desk. Your next task is to do this again, working at the same speed at which you were paced by the metronome last time. If you think you can't remember the speed at which you were paced, then guess - it is surprising how well people can, in fact, remember these things.

Begin when you are ready, and continue until the experimenter tells you to stop.

You will remember that last time you performed a foot-tapping task with the pedal on the floor. Your next task is to do this again, working at the same speed at which you were paced by the metronome last time. If you think you can't remember the speed at which you were paced, then guess - it is surprising how well people can, in fact, remember these things.

Begin when you are ready, and continue until the experimenter tells you to stop.

(f) Blocks Designs

As for Experiment II. (See Appendix I/B/1, Section 1).

(g) <u>Cartoon Ratings</u>

As for Experiment III. (See Appendix I/C/1, Section c).

APPENDIX II:

Tables of Raw Data

TABLE A: Experiment I, Session 1, Raw Data

Subject	FL	FR	TL	TR	CA	COU
1.1	52	50	45	36	170	25
1.2	125	175	100	94	165	20
1.3	27	27	24	21	143	21
1.4	70	63	71	67	132	18.5
1.5	64	61	54	48	124	23.5
1.6	39	56	34	36	125	24
1.7	31	43	20	22	136	20.5
1.8	57	46	38	37	139	21
1.9	48	43	57	52	119	12
1.10	16	19.5	15	15	162	24.5
1.11	62	45	44	39	145	18.5
1.12	46	40	31	35	87	18.5
1.13	40	50	38	44	238	25
1.14	88	83	62	53	118	15.5
1.15	53	50	34	28	160	24
1.16	36	36	30	36	136	30
1.17	67	69	29	45	116	19
1.18	26	32	31	27	165	26
1.19	60	52	35	43	163	23
1.20	97	106	60	79	137	21
1.21	60	52	29	34	183	17
1.22	63	63	37	49	128	21
1.23	57	68	58	38	136	19
1.24	69	68	39	60	148	17.5
1.25	83	126	87	102	103	14

Continued overleaf

TABLE A Continued

Subject	F	Sc	CD	E	CPY
1.1	99	141	52	52	360
1.2	139	147	76	43	392
1.3	118	130	34	41	324
1.4	121	128	60	51	409
1.5	111	114	70	50	322
1.6	120	124	54	39	273
1.7	132	134	42	24	394
1.8	153	163	38	41	423
1.9	116	134	54	45	332
1.10	98	110	34	19	362
1.11	114	128	62	35	383
1.12	108	116	52	38	360
1.13	104	113	66	34	360
1.14	122	136	62	34	311
1.15	124	132	46	48	370
1.16	128	144	46	28	482
1.17	140	150	68	31	443
1.18	120	132	38	34	387
1.19	135	148	58	46	438
1.20	107	117	68	53	346
1.21	112	127	58	45	403
1.22	141	151	58	29	357
1.23	118	130	66	34	317
1.24	103	115	70	44	325
1.25	129	152	90	36	428

For units of measurement, see Table B

TABLE B: Experiment I, Session 2, Raw Data

Subject	FL	FR	TL	TR	CA	COU
1.1	43	45	33	27	160	22
1.2	119	138	84	104	161	20
1.3	29	32	24	26	148	27
1.5	66	62	54	54	120	27
1.6	64	53	40	46	106	16
1.7	36	39	29	24	108	15
1.8	54	59	41	38	140	19
1.9	50	36	45	45	134	10
1.12	46	46	41	40	100	18
1.15	50	57	47	32	143	26
1.17	56	47	34	29	146	18.5
1.18	34	35	29	29	172	26
1.20	119	107	83	81	151	19.5
1.21	46	45	32	31	169	20
1.22	51	50	56	51	127	18.5
1.23	72	75	69	95	121	16
1.24	77	71	62	63	137	12.5
1.25	70	83	61	82	108	12.5

Continued overleaf

TABLE B Continued

Subject	F	Sc	CD	E	CPY
1.1	101	110	50	38	191
1.2	124	135	76	47	202
1.3	120	126	46	47	174
1.5	117	123	84	51	173
1.6	122	125	62	42	146
1.7	108	122	56	45	198
1.8	145	155	42	39	184
1.9	112	125	62	55	183
1.12	96	108	52	45	159
1.15	112	128	66	54	190
1.17	137	143	62	42	245
1.18	116	127	50	30	202
1.20	101	116	104	57	171
1.21	108	127	52	54	190
1.22	137	148	54	46	177
1.23	109	125	66	53	178
1.24	102	113	76	44	152
1.25	129	140	78	25	228

Units of Measurement

Tasks FL, FR, TL, TR and CD: Cycles (see text)

Tasks CA, COU, F, Sc and CPY: Seconds

Task E: Number of letters written

TABLE C: Experiment II. Raw Data

S	SFL	SFLR	SFR	SFRR	SAT	SUT
2.1	1.6461	1.7767	1.5752	1.8612	1.0127	1.7544
2.2	0.6867	0.7653	0.9519	0.9179	0.4717	0.4505
2.3	1.1880	0.3501	0.9505	0.9850	0.6448	0.8547
2.4	1.2146	1.9115	1.2416	2.0768	0.7035	1.9737
2.5	1.0230	0.7821	1.2506	0.7136	0.6504	1.5649
2.6	1.0682	1.2215	1.5259	1.4264	0.8599	0.7254
2.7	1.3105	1.2085	1.3790	1.2507	0.7817	1.6096
2.8	1.0209	0.6144	0.9156	0.7333	0.4810	1.0058
2.9	1.7427	1.6717	1.8602	1.5121	0.8649	1.7940
2.10	2.4440	2.6170	2.5875	2.6091	1.7454	2.0608
2.11	1.0121	0.9221	0.8616	0.9404	0.5675	0.9305
2.12	0.6569	0.7183	0.7560	0.6660	0.5227	0.6940
2.13	1.1681	1.4551	1.2607	1.3678	0.7696	0.9283
2.14	1.3685	1.4439	1.3541	1.3033	0.8976	1.3286
2.15	1.0272	1.2183	0.8333	0.9073	1.0521	1.2671
2.16	0.8155	1.0293	0.7603	0.8767	0.3966	0.8034
2.17	1.0886	1.2671	1.0858	1.1646	0.6810	1.1653
2.18	0.5956	0.3741	0.4034	0.3786	0.4427	0.5640
2.19	1.5516	1.4588	1.3747	1.5030	0.9917	1.1029
2.20	1.3026	1.3075	1.3975	1.2723	1.0076	1.2834
2.21	1.0698	0.9804	1.2665	1.1429	0.8825	1.0938
2.22	1.2301	1½3588	1.3333	1.0622	0.9404	1.3430
1.23	3.9039	3½7670	3.8558	3.9454	1.9447	3.7602
1.24	0.5299	0.6036	0.6704	0.4622	0.6407	1.1346

Continued overleaf

TABLE C Continued

S	MFL	MFLR	MFR	MFRR	MAT	MUT
2.1	4.2296	3.7280	4.4746	4.6418	2.7722	4.8744
2.2	2.6620	5.1438	5.5528	5.9208	3.7803	4.1709
2.3	5.0353	4.4657	5.3347	4.4111	2.9965	4.8266
2.4	3.3469	2.1750	3.3770	2.3268	1.7382	2.6790
2.5	4.0900	3.8520	5.1552	4.4244	2.3566	5.1088
2.6	2.6972	2.2200	3.3685	1.7875	1.3742	2.4565
2.7	2.3386	2.4599	2.7638	2.3303	1.9398	2.1543
2.8	2.7425	2.5551	2.4415	2.6276	1.4606	1.7773
2.9	5.1630	4.5091	5.6899	4.1793	3.4902	4.6170
2.10	4.1477	4.3609	4.8049	4.9148	2.9874	3.9414
2.11	2.6197	2.9412	3.2048	2.8042	1.4033	1.7179
2.12	5.7460	5.2951	5.9387	5.5360	3.4691	5.9121
2.13	4.7980	4.1041	4.4657	4.3087	1.9299	3.8013
2.14	5.2314	5.1552	6.6601	5.6727	3.8618	5.2954
2.15	3.2582	2.2556	2.6567	2.1739	1.6029	3.3334
2.16	3.4274	2.8014	3.4309	3.6906	1.5756	2.6571
2.17	5.7703	4.9239	1.4841	4.5501	2.0402	2.0202
2.18	2.3386	1.9588	2.8773	2.5615	1.1740	1.8955
2.19	3.7261	4.2021	5.2738	4.4769	2.0526	3.0397
2.20	4.2090	2.6863	4.3522	4.3675	2.8340	2.9904
2.21	3.0731	2.4403	2.9397	2.6660	1.2632	2.0812
2.22	4.1563	3.5607	4.2254	3.9118	2.8941	4.2510
2.23	6.7942	6.8879	7.3944	7.1394	3.9315	6.4410
2.24	4.3829	3.8403	4.3785	3.6519	3.1186	4.1772

Continued overleaf

TABLE C Continued

S	R1	R2
2.1	137	109
2.2	158	140
2.3	130	121
2.4	143	125
2.5	114	95
2.6	124	114
2.7	118	101
2.8	122	110
2.9	148	130
2.10	124	109
2.11	119	114
2.12	119	104
2.13	134	118
2.14	130	125
2.15	125	113
2.16	120	113
2.17	121	104
2.18	169	140
2.19	115	102
2.20	195	98
2.21	126	110
2.22	109	131
2.23	118	112
2.24	111	104

Units of Measurement

The scores for the 12 tapping tasks are in terms of the mean number of cycles per second over the 20-second periods of sampling. Data for R1 and R2 are in seconds.

TABLE D: Experiment III. Numbers of Dots in 30 secs.

	B	C	D	E	F	G
3.1	60	43	36	35	83	71
3.2	68	61	63	62	79	70
3.3	73	70	68	56	87	79
3.4	29	34	24	33	29	30
3.5	22	22	23	19	24	25
3.6	96	83	80	70	101	94
3.7	89	71	51	39	125	105
3.8	81	61	61	57	101	77
3.9	53	47	39	34	43	47
3.10	68	55	51	39	88	73
3.11	109	77	66	47	123	123
3.12	49	44	38	33	50	46
3.13	40	50	43	40	59	42
3.14	64	47	45	36	89	54
3.15	27	34	24	26	25	26
3.16	43	40	39	37	48	42

Continued overleaf

TABLE D Continued

	H	I	J	K	L	M
3.1	73	34	121	78	56	28
3.2	74	59	89	82	72	74
3.3	70	72	88	94	89	88
3.4	30	33	38	46	40	36
3.5	24	23	26	26	23	22
3.6	96	83	101	106	112	89
3.7	57	51	133	94	93	51
3.8	70	56	113	98	106	86
3.9	47	37	56	59	49	42
3.10	63	48	86	60	64	41
3.11	82	43	137	159	93	80
3.12	44	42	66	53	51	47
3.13	54	49	54	46	39	48
3.14	54	42	72	80	61	50
3.15	24	22	36	40	34	24
3.16	36	43	53	51	51	41

TABLE E: Experiment III, Numbers of Errors

	B	C	D	E	F	G
3.1	0	0	0	2	2	4
3.2	0	0	14	25	0	0
3.3	0	0	9	19	0	1
3.4	0	0	0	1	0	0
3.5	0	0	0	1	0	0
3.6	1	2	8	14	0	3
3.7	0	4	2	7	1	3
3.8	0	0	4	9	0	0
3.9	0	0	0	1	0	0
3.10	0	0	0	3	0	0
3.11	2	9	23	11	0	4
3.12	0	0	0	0	0	0
3.13	0	0	0	0	0	0
3.14	0	0	1	1	0	0
3.15	0	0	0	0	0	0
3.16	0	0	0	0	0	0

Continued overleaf

TABLE E Continued

	H	I	J	K	L	M
3.1	6	2	0	0	1	4
3.2	8	16	0	0	1	21
3.3	10	24	0	1	5	25
3.4	0	0	0	0	0	0
3.5	0	0	0	0	0	0
3.6	13	12	0	0	4	11
3.7	1	3	0	0	3	1
3.8	0	1	0	0	1	3
3.9	0	2	0	0	0	0
3.10	0	2	0	0	0	0
3.11	17	6	0	4	6	16
3.12	0	0	0	0	0	0
3.13	0	1	0	0	0	0
3.14	1	5	0	1	0	2
3.15	0	0	0	0	0	0
3.16	0	0	0	0	0	0

TABLE F: Experiment IV. Numbers of Dots per 30 secs.

	Target J	Target K
Group I		
4.1	46	59
4.3	44	58
4.5	54	56
4.7	41	43
4.9	65	56
4.11	54	56
4.13	69	72
4.15	67	65
4.17	64	58
4.19	57	51
Group II		
4.2	86	84
4.4	119	95
4.6	78	60
4.8	69	65
4.10	61	48
4.12	55	47
4.14	88	77
4½16	95	82
4.18	26	24
4.20	75	66

TABLE G: Experiment V, Raw Data (in taps per 30 seconds)

	Trial									
	1	2	3	4	5	6	7	8	9	10
5.1	81	98	93	99	90	92	99	99	96	100
5.2	74	71	71	86	79	82	91	85	82	88
5.3	33	37	38	38	40	41	40	41	41	41
5.4	29	25	26	26	24	26	25	26	26	28
5.5	29	28	29	27	28	29	30	32	30	35
5.6	43	43	44	44	44	44	45	44	45	45
5.7	49	50	24	49	36	13	66	42	27	65
5.8	48	100	94	91	96	95	95	97	98	91
5.9	48	55	56	55	59	46	51	54	54	58
5.10	43	46	46	45	46	46	47	47	47	47
5.11	110	137	152	160	157	158	162	166	159	162
5.12	114	110	90	94	87	79	70	69	69	72
5.13	80	89	90	86	88	89	93	94	88	90
5.14	26	30	32	31	32	32	33	32	31	31
5.15	49	50	50	50	52	50	51	52	51	51

TABLE H: Experiment VI, Trial 0, Mean Inter-tap Intervals (in hundredths of a second

S	Burst 1	Burst 2	Burst 3
6.1	78.4800	80.2917	82.6666
6.2	51.8158	51.8947	51.9474
6.2	93.0952	86.5217	92.9524
6.4	57.1143	54.6389	53.8649
6.5	122.7500	121.8750	121.3750
6.6	97.4000	96.8000	101.8420
6.7	146.3850	157.0830	146.6150
6.8	114.6470	116.0000	112.0590
6.9	84.8696	81,7083	80.7500
6.10	95.1905	97.2500	101.6320
6.11	64.7667	62.2500	61.7813
6.12	121.9380	118.3120	119.3120
6.13	86.5652	71.5926	70.0000
6.14	82.8750	79.2400	79.2400
6.15	101.5790	92.6190	90.6818
6.16	70.7143	66.3000	63.6452
6.17	67.1379	65.7000	63.7742

TABLE I: Experiment VI. Raw Data (hundredths of sec.)

A1 (Pacing Factor = 0.55)

		C1 (Phase B)			C2 (Phase D)		
S	PI*	B1	B2	B3	B1	B2	B3
1	44	44.1818	42.8696	41.6042	43.8889	41.0000	41.4792
2	28	27.2192	27.3014	26.4133	25.8961	25.2405	25.0000
3	49	49.8500	48.9500	49.0250	49.8000	48.8000	50.5385
4	46	46.0698	45.5581	44.1333	45.0455	44.5000	44.0000
5	67	66.4667	65.8667	65.6333	62.6452	62.3750	60.8750
6	53	52.5263	48.6098	46.1395	51.1538	50.7949	50.6154
7	81	81.9167	83.4783	82.9583	82.4583	82.5000	83.3913
8	62	60.6875	61.1250	58.9394	66.6333	63.2581	69.5357
9	45	45.3409	44.7954	43.9111	44.6364	44.9318	44.3333
10	53	52.4474	52.3421	52.3421	53.2432	51.3947	51.3421
11	34	35.3571	41.2708	41.0417	31.0156	32.0161	30.8594
12	65	66.3000	70.6429	73.7037	78.6800	86.3478	92.9524
13	41	42.3617	36.6667	39.3400	37.5849	36.5556	34.3965
14	44	42.4681	41.7021	41.2292	405306	39.6200	39.6800
15	51	52.2368	52.1842	52.0000	56.5714	56.1143	55.1389
16	36	35.2143	34.1379	33.4237	34.1724	34.0517	34.7895
17	35	33.8644	33.1167	33.3390	34.8421	34.0690	33.3559

*PI - Pacing Interval (the inter-beat interval of the metronome).

Continued overleaf

TABLE I Continued

A2 (Pacing Factor = 0.70)

		C1 (Phase B)			C2 (Phase D)		
S	PI*	B1	B2	B3	B1	B2	B3
1	56	54.5000	53.4865	53.5135	53.6216	52.5789	52.2895
2	35	36.2000	35.7455	35.0000	34.2750	32.7667	32.7377
3	63	66.3333	66.4667	67.1379	68.9310	68.9310	69.8571
4	38	37.2264	36.6296	36.3704	35.2500	34.5263	34.2931
5	85	85.0000	83.9130	82.0416	85.0435	83.1667	81.5417
6	68	59.4848	62.5161	57.9706	80.4167	72.4444	67.5862
7	104	101.1580	102.9470	103.4440	106.2780	106.7220	106.5560
8	79	73.7778	71.7407	74.1538	65.3000	64.4839	64.2903
9	57	57.0571	55.5278	55.8000	54.6667	55.0833	54.0270
10	67	67.9399	63.4516	64.5333	63.1290	62.0313	61.0938
11	43	43.1087	43.3696	43.1739	44.0889	44.6364	44.7273
12	83	87.3182	95.0952	97.6000	109.3890	111.0000	111.5290
13	52	51.2564	48.6098	45.2045	46.2791	43.5333	43.4667
14	56	55.3333	54.8889	54.8055	56.7143	55.8286	55.0833
15	65	69.6071	70.4286	78.1600	82.0833	72.1111	72.7407
16	46	43.1739	41.6596	40.3061	38.7451	37.6981	36.7963
17	45	42.7609	42.6739	43.2826	50.2564	50.6154	51.5000

*PI - Pacing Interval (the inter-beat interval of the metronome)

Continued overleaf

TABLE I Continued

A3 (Pacing Factor = 0.85)

S	PI*	C1 (Phase B) B1	B2	B3	C2 (Phase D) B1	B2	B3
1	68	68.0345	66.2333	65.4667	65.0333	62.9677	63.1935
2	43	44.2444	44.7500	44.3556	49.7500	48.1951	47.6585
3	76	79.1200	81.1666	82.4583	86.2609	86.7391	84.6956
4	46	46.0698	45.5581	44.1333	45.0455	44.5000	44.0000
5	103	101.8420	105.1588	105.9440	103.3160	103.9470	105.8890
6	83	83.2500	84.0869	71.6667	88.6818	84.0000	78.0400
7	126	134.1430	138.886	140.6430	159.7500	142.7690	141.2140
8	96	93.6190	96.1500	93.6190	92.2857	94.0000	91.4762
9	69	70.1429	69.8571	69.1429	71.2857	69.4643	68.4828
10	82	81.5833	82.9167	83.0833	75.3461	73.7778	73.8518
11	52	54.2222	48.2195	50.4872	49.2250	49.1250	48.9750
12	101	99.2000	107.7780	110.2780	116.9410	113.5290	110.3330
13	63	66.6333	63.1290	56.8000	67.7586	67.5862	64.3871
14	68	65.5517	60.4375	64.9667	79.0800	78.3600	77.6400
15	79	88.3182	84.5217	96.6000	98.6000	93.3333	88.0909
16	56	53.8378	52.9459	51.9211	57.8823	56.5714	54.6944
17	55	52.2895	51.3158	50.0513	50.7436	49.0000	47.5000

*PI - Pacing Interval (the inter-beat interval of the metronome)

Continued overleaf

TABLE I Continued

A4 (Pacing Factor = 1.00)

S	PI*	C1 (Phase B) B1	B2	B3	C2 (Phase D) B1	B2	B3
1	80	82.2917	75.7308	75.5000	82.5833	75.9231	76.8846
2	51	53.4865	53.4595	53.8649	57.4412	55.7714	56.0571
3	90	94.4286	96.1000	96.5500	104.4210	109.2780	108.3890
4	55	55.2222	55.0000	55.2778	55.7714	56.8857	56.6286
5	122	121.8120	121.9380	119.6250	125.7330	122.0620	124.8130
6	98	95.0000	100.7890	105.0530	98.3000	92.9048	90.5909
7	149	149.7690	156.5830	152.2310	153.0770	153.6920	152.1540
8	114	110.2220	112.4120	105.1580	118.3750	112.5880	107.6670
9	82	81.6250	80.2917	79.2800	77.4800	74.7308	74.6923
10	97	94.9047	103.8420	105.3330	105.0000	93.9048	104.3160
11	62	60.7500	57.9118	54.5278	58.1176	56.6000	55.2778
12	119	122.9380	122.5000	124.0000	155.5000	135.3570	133.7860
13	75	71.1481	78.2400	78.9200	77.0400	73.0741	74.6923
14	80	78.7200	78.7200	78.4000	77.7200	74.7308	77.7600
15	94	108.5000	108.1110	114.8240	131.2000	109.4440	117.1760
16	66	64.8000	66.9310	66.7931	69.4286	73.8461	70.2143
17	65	58.5588	54.7778	53.8649	52.9730	50.6154	52.5263

*PI - Pacing Interval (the inter-beat interval of the metronome)

Continued overleaf

TABLE I Continued

A5 (Pacing Factor = 1.15)

		C1 (Phase B)			C2 (Phase D)		
S	PI*	B1	B2	B3	B1	B2	B3
1	92	92.8571	95.0476	92.6667	95.2500	92.0000	92.9048
2	58	57.4412	58.4706	58.7879	59.7576	58.2941	57.2647
3	103	104.1050	100.5260	105.3890	105.1050	103.8950	107.6110
4	63	61.1875	60.2424	60.5454	62.9677	62.6452	61.5312
5	140	144.3080	139.7140	136.1430	135.7860	135.7860	135.2140
6	112	106.2780	104.2630	111.9410	106.7220	110.7780	120.9380
7	171	168.6178	201.2220	216.1110	246.0000	268.4290	275.7140
8	131	140.8570	136.7140	134.0710	128.5330	125.2670	126.0670
9	94	87.5000	85.4348	84.0435	76.1538	75.9231	76.8800
10	111	110.4440	110.2780	103.0000	108.8330	107.2780	105.5000
11	71	63.4839	64.3871	61.9375	60.0303	57.7879	57.6765
12	136	132.9330	131.8000	128.3330	143.9230	128.4670	128.2000
13	86	70.9259	75.6538	75.0385	91.1428	75.3461	80.7500
14	92	91.3810	92.0952	91.1905	93.2857	90.8095	90.8571
15	108	138.9290	141.6430	152.6150	176.2730	211.3330	197.9000
16	75	67.2414	60.9375	57.7647	54.8333	51.5789	51.5000
17	74	69.7143	65.0667	65.7667	62.9677	60.1818	63.1613

*PI - Pacing Interval (the inter-beat interval of the metronome)

Continued overleaf

TABLE I Continued

A6 (Pacing Factor = 1.30)

S	PI*	C1 (Phase B) B1	B2	B3	C2 (Phase D) B1	B2	B3
1	104	99.9500	96.5500	96.4500	80.8333	75.6538	77.8800
2	66	66.5000	67.6207	68.6552	67.9655	67.5172	69.0714
3	117	117.4120	121.5000	120.3750	119.4380	125.5330	126.3330
4	71	70.7500	70.9643	69.4286	70.8929	69.6786	70.2500
5	158	163.7500	177.0000	171.8180	156.4170	150.0770	164.5000
6	127	122.0000	110.9440	112.4120	104.4740	100.7370	105.7780
7	193	202.0000	198.5560	213.5560	199.2000	218.0000	220.0000
8	148	159.5000	154.5830	173.7270	155.4170	158.6670	169.0910
9	106	106.0000	102.2630	104.7780	103.3680	99.2500	100.4210
10	126	128.2000	123.9370	124.0000	115.4710	117.0000	122.0000
11	80	80.3333	69.2143	79.8800	70.8929	70.5357	74.4231
12	154	146.3080	160.4170	160.2500	179.6360	171.3640	174.0000
13	97	95.5500	92.3333	88.3182	93.0952	93.3809	88.7500
14	104	101.8420	98.7000	98.3000	95.6500	95.5500	95.9000
15	122	128.1330	136.6430	139.0710	169.6360	159.5000	170.7270
16	85	83.2500	80.6250	80.2083	92.5238	80.1666	80.8750
17	84	76.3077	73.7407	71.3214	68.7586	64.8667	66.3333

*PI ?Pacing Interval (the inter-
beat interval of the metronome)

Continued overleaf

TABLE I Continued

A7 (Pacing Factor = 1½45)

		C1 (Phase B)			C2 (Phase D)		
S	PI*	B1	B2	B3	B1	B2	B3
1	116	110.5560	115.2940	115.6470	95.2500	107.9440	107.4440
2	73	70.2857	68.9310	69.8214	75.5385	70.9643	71.5926
3	130	133.0000	136.0000	129.8670	132.8000	131.4000	131.8000
4	79	81.0417	79.9200	78.5600	78.2800	81.2917	82.3333
5	176	198.0000	190.3000	208.4440	158.7500	176.3640	179.5450
6	142	158.1670	133.0000	128.4670	131.7330	130.2000	127.8670
7	216	230.1250	237.2500	241.6250	243.8750	248.2500	276.1430
8	165	171.0000	175.9090	179.6360	171.9090	166.6360	171.8180
9	118	119.4380	116.4710	106.0560	108.5000	107.5560	104.8950
10	140	134.7140	130.9330	128.0000	115.7650	104.2110	120.8120
11	89	89.6818	89.5454	90.0454	84.6956	77.6000	85.7826
12	172	183.1000	193.8000	200.5560	218.6670	221.1110	218.6670
13	108	115.8240	116.3530	116.4710	149.0770	140.6430	127.5330
14	116	114.5290	114.8820	111.7060	113.4120	110.2220	111.9410
15	136	166.0910	165.8330	169.5450	190.3000	199.0000	195.0000
16	95	95.0952	92.8571	91.7619	96.7500	91.8095	90.1818
17	94	88.6818	85.4348	83.6956	96.0500	91.5238	92.5714

*PI - Pacing Interval (the inter-beat interval of the metronome)

TABLE J: Experiment VI, Percentage Scores

A1 (Pacing Factor = 0.55)

	C1 (Phase B)			C2 (Phase D)		
	B1	B2	B3	B1	B2	B3
6.1	100.4090	97.4318	94.5454	99.7500	93.1818	94.2727
6.2	92.5000	90.1428	89.2857	97.2142	97.5000	94.3214
6.3	101.7346	99.8979	100.0408	101.6326	99.5918	103.1428
6.4	95.1000	89.7666	90.2666	84.2333	81.5333	80.3666
6.5	99.2089	98.3134	97.9552	93.4925	93.0895	90.8507
6.6	99.1132	91.7169	87.0566	96.5094	95.8301	95.4905
6.7	101.1358	103.0617	102.4197	101.8024	101.8518	102.9506
6.8	97.8870	98.5806	95.0645	107.4677	102.0322	112.1612
6.9	100.7555	99.5333	97.5777	99.2000	99.8444	98.5111
6.10	98.9622	98.7547	98.7547	100.4528	96.9622	96.8679
6.11	104.0000	121.3823	120.7058	91.2352	94.1764	90.7647
6.12	102.0000	108.6769	113.3846	121.0461	132.8461	143.0000
6.13	103.3170	89.4390	95.9512	91.6585	89.1707	83.9024
6.14	96.5227	94.7727	93.7045	92.1136	90.0454	90.1818
6.15	102.4313	102.3137	101.9607	110.9215	110.0196	108.1176
6.16	97.8055	94.8333	92,8333	94,9166	94.5833	96.6388
6.17	96.7428	94.6285	95.2571	99.5428	97.3428	98.1714

Continued overleaf

TABLE J Continued

A2 (pacing Factor = 0.70)

	C1 (Phase B)			C2 (Phase D)		
	B1	B2	B3	B1	B2	B3
6½1	97.3214	93.7321	95.5535	95.7500	93.8928	93.3750
6.2	103.4285	102.1142	100.0000	97.9428	93.6285	93.5428
6.3	105.2857	105.5079	106.5714	109.4126	109.4126	110.8888
6.4	97.9736	96.3947	95.7105	92.7631	90.8684	90.2368
6.5	100.0000	98.7176	96.5176	100.0470	97.8470	95.9294
6.6	87.4705	91.9411	85.2500	118.2647	106.5294	99.3970
6.7	97.2692	98.9903	99.4615	102.1923	102.6153	102.4615
6.8	93.3924	90.8101	93.8607	82.6582	81.6202	81.3797
6.9	100.1052	97.4210	97.8947	95.9122	96.6315	94.7894
6.10	101½4029	94.7014	96.3134	94.2238	92.5820	91.1791
6.11	100.2558	100.8604	100.3953	102.5348	103.8139	104.0232
6.12	117.2530	114.5662	117.5903	131.7951	133.7349	134.3734
6.13	98.5769	93.4807	86.9230	89.0000	93.7115	83.5961
6.14	98.8035	98.0178	97.8571	101.2678	99.6964	98.3571
6.15	107.0923	108.3538	120.2461	126.2769	110.9384	111.9076
6.16	93.8478	90.5652	87.6304	84.2173	81.9565	80.0000
6.17	95.0222	94.8222	96.1777	111.6888	112.4666	114.4444

Continued overleaf

TABLE J Continued

A3 (Pacing Factor = 0.85)

	C1 (Phase B)			C2 (Phase D)		
	B1	B2	B3	B1	B2	B3
6.1	100.0441	97.3970	96.2794	95.6323	92.6029	92.9264
6.2	102.8837	104.0697	103.1627	115.6376	112.0697	110.8372
6.3	104.1052	106.8026	108.5000	113.5000	114.1315	111.4473
6.4	100.1521	99.0434	95.9347	97.9130	96.7391	95.6521
6.5	98.8737	102.0970	102.8543	100.3106	100.9223	102.8058
6.6	100.3012	101.3132	86.3493	106.8433	101.2048	94.0240
6.7	106.4603	109.7539	111.6190	120.4365	113.3095	112.0714
6.8	97.5208	100.7812	97.5208	96.1354	97.9166	95.2916
6.9	101.6521	101.2463	100.2028	103.3188	100.6666	99.2463
6.10	99.4878	101.1219	101.3170	91.8902	89.9756	90.0609
6.11	104.2692	92.7307	97.0961	94.6538	94.4615	94.1730
6.12	98.2178	106.7128	109.1881	115.7821	112.4059	109.2376
6.13	105.7619	100.2063	90.1587	107.5555	107.2857	102.2063
6.14	96.3970	88.8823	95.5441	116.2941	115.2352	114.1764
6.15	111.7974	106.9873	122.2784	124.8101	118.1392	111.5063
6.16	96.1428	94.5535	92.1785	103.3571	101.0178	97.6607
6.17	95.0727	93.3090	91.0000	92.2545	89.0909	86.3636

Continued overleaf

TABLE J Continued

A4 (Pacing Factor = 1.00)

	C1 (Phase B)			C2 (Phase D)		
	B1	B2	B3	B1	B2	B3
6.1	102.8625	94.6625	94.3750	103.2250	94.9000	96.1000
6.2	104.8823	104.8325	105.6078	112.6274	109.3529	109.9215
6.3	104.9222	106.7777	107.2777	116.0222	121.4222	120.4333
6.4	100.4000	100.0000	100.5090	101.4000	103.4000	102.9636
6.5	99.8442	99.9508	98.0491	103.0573	100.6229	99.8688
6.6	96.9387	102.8469	107.1938	100.3061	94.7959	92.4387
6.7	100.5167	105.0872	102.1677	102.7382	103.1476	102.1140
6.8	96.6842	98.6502	92.2456	103.8333	98.7631	94.4473
6.9	99.5365	97.9146	96.6829	94.4878	91.1341	91.0853
6.10	97.8350	107.0515	108.5876	108.2474	96.8041	107.5463
6.11	97.9838	93.4032	87.9516	93.7419	91.2903	89.1612
6.12	103.3109	102.9411	104.2016	130.6722	113.7478	112.4285
6.13	94.8666	104.3200	105.2266	102.7200	97.4266	99.5866
6.14	98.4000	98.4000	98.0000	97.1500	93.4125	97.2000
6.15	115.4255	115.0106	122.4189	139.5744	116.4255	124.6595
6.16	98.1818	101.4090	101.1969	139.5744	116.4255	124.6595
6.17	90.0923	84.2769	82.8615	81.4923	77.8615	80.8153

Continued overleaf

TABLE J Continued

A5 (Pacing Factor = 1.15)

	C1 (Phase B)			C2 (Phase D)		
	B1	B2	B3	B1	B2	B3
6.1	100.9347	103.3152	100.7282	103.5326	100.0000	100.9782
6.2	99.0344	100.8103	101.3620	103.0344	100.5000	98.7241
6.3	101.0679	97.6019	102.3203	102.0388	100.8640	104.4757
6.4	97.1269	95.6190	96.0952	99.9523	99.4285	97.6666
6.5	103.0785	99.7928	97.2428	96.9928	96.9928	96.5785
6.6	94.8928	93.0892	99.9464	95.2857	98.9107	107.9821
6.7	98.6069	117.6725	126.3801	143.8596	156.9766	161.2339
6.8	107.5267	104.3587	103.8702	98.1145	95.6259	96.2366
6.9	93.0851	90.8829	89.4042	81.0106	80.7659	81.7872
6.10	99.4954	99.3513	92.7927	98.0450	96.6486	95.0450
6.11	89.4084	90.6901	87.2394	84.5492	81.3943	81.2394
6.12	97.7426	96.9117	94.3602	105.8235	94.4632	94.2647
6.13	82.4767	87.9651	87.2558	105.9767	87.6162	93.8953
6.14	99.3260	100.0978	99.1195	101.4021	98.7065	98.7608
6.15	128.6574	131.1481	141.3055	163.2129	195.6759	183.2407
6.16	89.6533	81.2533	77.0133	73.1066	68.7733	68.6666
6.17	94.2027	87.9324	88.8783	85.0945	81.3243	85.3513

Continued overleaf

TABLE J Continued

A6 (Pacing Factor = 1.30)

	C1 (Phase B)			C2 (Phase D)		
	B1	B2	B3	B1	B2	B3
6.1	96.1057	92.8365	92.7403	77.7211	72.7403	74.8846
6.2	100.7575	102.4545	104.0151	102.9696	102.3030	104.6515
6.3	100.3504	103.8461	102.8803	102.0854	107.2905	107.9743
6.4	99.6478	99.9436	97.7887	99.8450	98.1408	98.9436
6.5	103.6392	112.0253	108.7468	99.0000	94.9873	104.1139
6.6	96.0629	87.3453	88.5118	82.2598	79.3228	83.2913
6.7	104.6632	102.8808	110.6528	103.2124	112.9533	113.9896
6.8	107.7702	104.4459	117.3851	105.0135	107.2094	114.2500
6.9	100.0000	96.4716	98.8490	97.5188	93.6320	94.7358
6.10	101.7460	98.3650	98.4126	91.6428	92.8571	96.8253
6.11	100.4125	86.5125	99.8500	88.6125	88.1750	93.0250
6.12	95.0064	104.1688	104.0584	116.6493	111.2727	112.9870
6.13	98.5051	95.1855	91.0515	95.9690	96.2680	91.4948
6.14	97.9230	94.9038	94.5192	91.9711	91.8750	92.2115
6.15	105.0245	112.0000	113.9918	139.0491	130.7377	139.9426
6.16	97.9411	94.8470	94.3647	108.8470	94.3176	95.1411
6.17	90.8452	87.7857	84.9047	81.8571	77.2261	78.9642

Continued overleaf

TABLE J Continued

A7 (Pacing Factor = 1.45)

	C1 (Phase B)			C2 (Phase D)		
	B1	B2	B3	B1	B2	B3
6.1	95.3103	99.3879	99.6982	82.1120	93.0517	92.6206
6.2	96.2876	94.4246	95.6438	103.4794	97.2054	98.0684
6.3	102.3076	104.6153	99.9000	102.1538	101.0769	101.3846
6.4	102.5822	101.1645	99.4430	99.0886	102.8987	104.2152
6.5	112.5000	108.1250	118.4318	90.1988	100.2145	102.0113
6.6	111.3873	93.6619	90.4718	92.7676	91.6901	90.0492
6.7	106.5370	109.8379	111.8611	112.9027	114.9305	127.8425
6.8	103.4363	106.6121	108.8727	104.1878	100.9939	104.1333
6.9	101.2203	98.7033	89.8813	91.9491	91.1525	88.8898
6.10	96.2214	93.5214	91.4285	82.6857	74.4357	86.2928
6.11	100.7640	100.6067	101.1685	95.1685	87.1910	96.3820
6.12	106.4534	112.6744	116.6046	127.1337	128.5523	127.1337
6.13	107.2407	107.7314	107.8425	138.0370	130.2222	118.0833
6.14	98.7327	99.0344	96.3017	97.7672	95.0172	96.5000
6.15	122.1250	121.9338	124.6617	139.9264	146.3235	143.3823
6.16	100.0947	97.7473	96.5894	101.8421	96.6421	94.9263
6.17	94.3404	90.8829	89.0425	102.1808	97.3617	97.3617

Continued overleaf

TABLE K: Experiment VI, Scale Good-Bad

	Pacing Factor						
S	0.55	0.70	0.85	1.00	1.15	1.30	1.45
6.1	6	6	5	6	1	1	6
6.2	5	5	2	4	5	2	3
6.3	4	4	4	4	4	4	4
6.4	4	4	4	4	4	5	5
6.5	4	6	5	6	5	5	5
6.6	3	4	2	4	4	4	3
6.7	4	5	4	4	4	4	4
6.8	4	5	5	5	3	4	4
6.9	4	4	4	4	4	4	4
6.10	4	4	4	4	4	4	4
6.11	4	4	4	4	4	4	4
6.12	5	4	4	5	4	3	4
6.13	3	5	5	4	6	5	2
6.14	4	4	4	4	4	4	4
6.15	6	5	5	4	3	3	4
6.16	5	5	3	5	5	3	5
6.17	4	4	4	4	4	4	4

Scale Poles

Good = 7

Bad = 1

TABLE L: Experiment VI, Scale Pleasurable-Painful

S	Pacing Factor 0.55	0.70	0.85	1.00	1.15	1.30	1.45
6.1	6	6	3	5	3	2	6
6.2	5	5	2	4	4	3	3
6.3	5	2	5	4	4	4	3
6.4	5	5	4	5	5	5	5
6.5	6	6	6	5	6	4	5
6.6	4	5	4	4	4	4	4
6.7	4	4	4	4	4	4	4
6.8	4	5	4	4	4	4	4
6.9	3	5	4	4	5	3	3
6.10	4	4	4	4	4	4	4
6.11	4	4	4	4	4	4	4
6.12	5	4	5	6	3	3	4
6.13	3	4	4	3	2	4	4
6.14	4	4	4	4	4	4	4
6.15	5	5	5	5	3	3	4
6.16	6	6	4	4	5	4	4
6.17	3	4	4	4	4	4	5

Scale Poles

Pleasurable = 7

Painful = 1

TABLE M: Experiment VII, Raw Data (taps per 30 secs.)

	Session 1		Session 2	
S	Spont.	Paced	Spont.	Paced
7.1*	27	52	18	34
7.2	31	27	36	24
7.3	32	34	39	45
7.4	40	32	51	39
7.5	64	43	66	55
7.6*	21	63	28	56
7.7	36	19	41	18
7.8	48	37	61	62
7.9	54	51	52	60
7.10	60	56	59	56
7.11	84	65	63	63
7.12	29	89	46	99
7.13	39	32	41	39
7.14*	44	47	39	39
7.15	58	47	58	43
7.16	60	62	55	52
7.17	49	63	63	93
7.18	43	55	47	94
7.21*	29	50	30	56
7.22	119	30	138	37
7.23	27	117	59	119
7.24	54	26	62	25
7.25	37	56	45	60
7.26	49	44	47	44

*Randomly discarded from the analysis (see text)

APPENDIX III:

Computer Programmes

SECTION A:

Statistical Programmes

1. F/IT 1: t-TEST FOR INDEPENDENT MEANS

Language: LABFOCAL

Input: Parameters NA and NB (the numbers of subjects in each group) input from the Teletype. Data input on High-speed paper tape reader.

Output: Teletype.

2. F/PT 1: t-TEST FOR CORRELATED MEANS

Language: LABFOCAL

Input: Parameter N (number of pairs of observations) input from Teletype. Data input on high-speed paper tape reader.

Output: Teletype.

1. F/IT 1

```
Ø1.Ø1 E
Ø1.Ø5 A ? NA NB ?,!
Ø1.Ø6 *
Ø1.1Ø F J=1,NA;A X;S SW=SW+X;S SX=SX+X↑2
Ø1.11 F J=1,NB;A X;S SY=SY+X;S SZ=SZ+X↑2
Ø1.15 S LA=NA*SX-SW↑2;S LB=NB*SZ-SY↑2
Ø1.16 T FSQT(((NA+NB-2)*(NB*SW-NA*SY)↑2)/((NA+NB)*(NB*LA+NA*LB))),!
Ø1.17 *;Q
```

2. F/PT 1

```
Ø1.Ø1 E
Ø1.Ø5 A ? N ?,!
Ø1.Ø6 *
Ø1.1Ø F J=1,N;A A(J)
Ø1.11 F J=1,N;A X;S DD=DD+A(J)-X;S DS=DS+(A(J)-X)↑2
Ø1.15 T (DD/N)/FSQT(((N*DS-DD↑2)/(N*(N-1)))/N),!
Ø1.2Ø *;Q
```

3. F/RO 4: Matrix of Intercorrelations (Spearman)

Language: LABFOCAL

Input: Parameters BLOCKS (number of tests to be intercorrelated) and N (number of subjects per block) from Teletype. Also, the number of blocks to be reverse-ranked (see below). Data from high-speed paper tape reader.

Reverse-ranking: in ranking the scores in a block, the score which is numerically highest is normally assigned the rank of 1. If the user wishes that some of the blocks be ranked so that the smallest score is assigned the rank of 1, the reverse-ranking facility is used.

Capacity: the programme incorporates two features which result in the ability to handle large numbers of blocks and subjects. Ranks are stored on magnetic DECtape, and provision is made for the matrix to be output in sections should the width of the matrix exceed the width of the Teletype paper. Approximate limits to BLOCKS and N are 720 and 250 respectively.

Output: Teletype.

3. F/RO 4

```
Ø1.Ø1 E
Ø1.Ø5 A ? BLOCKS N ?,!
Ø1.Ø6 T "HOW MANY BLOCKS TO BE REVERSE-RANKED ?";A OP,!
Ø1.Ø7 I (OP) Ø1.1Ø,Ø1.1Ø,Ø1.Ø8
Ø1.Ø8 T "WHICH BLOCKS ?";F H=1,OP;A O(H)
Ø1.1Ø *;S CO=1;F G=1,BL;D 2
Ø1.12 G Ø6.Ø1

Ø2.Ø1 F H=1,N;A A(H)
Ø2.Ø2 I (OP) Ø2.1Ø,Ø2.1Ø,Ø2.Ø3
Ø2.Ø3 I (O(CO)-(G)) Ø2.1Ø,Ø2.Ø4,Ø2.1Ø
Ø2.Ø4 S CO=CO+1;S CF=Ø;F H=1,N;D Ø2.15;S RA=RR;D 3
Ø2.Ø5 G Ø2.11
Ø2.1Ø S CF=Ø;F H=1,N;D Ø2.15;S RA=RF;D 3
Ø2.11 S X=FP(CF*1ØØ,N+1);S X=FDTA(G,Ø);R
Ø2.15 S RF=1;S RR=1;S TI=1;S SU=Ø;F I=1,N;D 4
Ø2.16 R

Ø3.Ø1 I (1-TI)Ø3.Ø6,Ø3.Ø7,Ø3.Ø7
Ø3.Ø6 S RA=RA+(TI-1)/2;S CF=CF+(TI↑3-TI)/12/TI
Ø3.Ø7 S X=FP(RA*1Ø,H)

Ø4.Ø1 I (A(I)-A(H)) Ø4.1Ø,Ø4.11,Ø4.12
Ø4.1Ø S RR=RR+1;R
Ø4.11 I (H-I) Ø4.13,Ø4.2Ø,Ø4.13
Ø4.12 S RF=RF+1;R
Ø4.13 S TI=TI+1
Ø4.2Ø R

Ø6.Ø1 S OV=Ø
Ø6.Ø3 T !!!!;S G=OV+2
Ø6.Ø5 T "      ";F J=OV+1,OV+8;D Ø6.Ø7
Ø6.Ø6 T !!;G Ø6.12
Ø6.Ø7 I (J-BL)Ø6.Ø8;R
Ø6.Ø8 T % Ø3.ØØ,J,"    "
Ø6.12 I (BL-G)Ø6.4Ø
Ø6.13 T % Ø3.ØØ,G;S X=FDTA(G,1);S I=OV+1;F J=1,N;S A(J)=FB(J)/1Ø
Ø6.14 S A(N+1)=FB(N+1)/1ØØ
Ø6.15 I ((G-1)-I)Ø6.3Ø;I ((OV+8)-I)Ø6.3Ø;D 8;G Ø6.15
Ø6.3Ø T !!;S G=G+1;G Ø6.12
Ø6.4Ø T !!;I (I-BL)Ø6.41;G 1Ø.Ø1
Ø6.41 S OV=OV+8;G Ø6.Ø3

Ø8.Ø2 S X=FDTA(I,1);S SS=Ø
Ø8.Ø5 F H=1,N;S X=FB(H);S SS=SS+(A(H)-X/1Ø)↑2
Ø8.Ø7 S XS=(N↑3-N)/12-A(N+1);S X=FB(N+1);S YS=(N↑3-N)/12-(X/1ØØ)
Ø8.Ø9 S RO=(XS+YS-SS)/(2*FSQT(XS*YS))
Ø8.15 T % Ø6.Ø3,RO
Ø8.2Ø S I=I+1

1Ø.Ø1 *;Q
```

Sample Output from F/RO 4

```
 N 18 BLOCKS 11
HOW MANY BLOCKS TO BE REVERSE-RANKED ?5

WHICH BLOCKS ?5 6 7 8 11

        Ø.934  Ø.893  Ø.890  Ø.148  Ø.300 -Ø.153 -Ø.070  Ø.805  Ø.170

               Ø.892  Ø.857  Ø.148  Ø.156 -Ø.133 -Ø.111  Ø.750  Ø.105

                      Ø.936  Ø.160  Ø.271 -Ø.028 -Ø.032  Ø.772  Ø.312

                             Ø.236  Ø.312 -Ø.105 -Ø.048  Ø.735  Ø.224

                                    Ø.585 -Ø.024  Ø.224  Ø.226 -Ø.109

                                           Ø.055  Ø.113  Ø.233 -Ø.130

                                                  Ø.878  Ø.076  Ø.317

                                                         Ø.142  Ø.167

                                                                Ø.349


  Ø.188

  Ø.110

  Ø.202

  Ø.263

  Ø.403

  Ø.094

  Ø.329

  Ø.517

  Ø.115

  Ø.329
```

SECTION B:

Experiment II Software

1. RANGEN: Random Number Generator

Language: P/FOCAL

This programme produced the order of presentation of tasks, subject to the constraints imposed by the experimental design (see Chapter 4). The orders were output on paper tape for input into INTRAN (see below).

2. INTRAN: Interpreter of Random Numbers

Language: P/FOCAL

The experimenter required a list of the orders of presentation of tasks, for purposes of assembling the instruction books and operating items of apparatus. Such a list could have been assembled from the numerical output of RANGEN with the aid of a reference key, but such a procedure would have been subject to the possibility of human error. INTRAN was written so that the output of RANGEN could be translated into easily understood abbreviations of the task-names. These were output on the Teletype.

3. WRIDEX: Writer of Index

Language: LABFOCAL

This programme loaded the output of RANGEN on to the Index of the magnetic DECtape, from which position they could be read automatically by the Runtime programme. The necessity to input parameters from the Teletype to control RUNTIM was thereby avoided.

1. RANGEN: Random Number Generator

```
Ø1.Ø1 E
Ø1.Ø4 A ? CYCLES ?,"STARTING SUBJECT ",SS,!
Ø1.Ø5 T % Ø2.ØØ,!;O O PTP:
Ø1.Ø6 F H=1,CY;D 2
Ø1.1Ø O C;Q

Ø2.Ø1 F J=1,22;S RF(J)=Ø;S G=Ø
Ø2.Ø5 F I=1,2;D 3
Ø2.Ø6 F J=17,3Ø;D 5
Ø2.Ø8 T SS+H-1
Ø2.1Ø F J=1,7;D 6
Ø2.11 T OR(8),OR(16)

Ø3.Ø4 F J=1,8;S RF(J)=Ø
Ø3.Ø5 S X=FRAN()*2;I (X-1)Ø3.Ø6,Ø3.Ø6;S OR(G+1)=7;S OR(G+8)=8;G Ø3.1Ø
Ø3.Ø6 S OR(G+1)=8;S OR(G+8)=7
Ø3.1Ø F J=(G+2),(G+7);D 4
Ø3.11 S G=8

Ø4.Ø5 S X=FITR(FRAN()*6+1);I (X-7)Ø4.Ø8,Ø4.Ø5,Ø4.Ø5
Ø4.Ø8 I (Ø-RF(X))Ø4.Ø5
Ø4.1Ø S RF(X)=1;S OR(J)=X;R

Ø5.Ø5 S X=FITR(FRAN()*14+9);I (X-23)Ø5.Ø8,Ø5.Ø5,Ø5.Ø5
Ø5.Ø8 I (Ø-RF(X))Ø5.Ø5
Ø5.1Ø S RF(X)=1;S OR(J)=X;R

Ø6.Ø6 T OR(J),OR(J+8),OR(J+16),OR(J+23)
```

2. INTRAN: Interpreter of Random Numbers

```
Ø1.Ø1 E
Ø1.Ø4 A ? CYCLES ?,!
Ø1.Ø6 O I DTA1: TC.PF
Ø1.Ø7 F J=1,66;A TC(J)
Ø1.Ø8 I C;O I PTR:
Ø1.Ø9 F H=1,CY;D 2
Ø1.1Ø I C;O I TTY:,E;Q

Ø2.Ø4 T !!;A SU;T % Ø3.ØØ, "SUBJECT ",SU,!!
Ø2.Ø5 F J=1,15;D 3
Ø2.Ø8 T !!

Ø3.Ø5 A OR;F I=(OR-1)*3+1,(OR-1)*3+3;S X=FOUT(TC(I))
Ø3.Ø6 T "    ";D Ø3.Ø5;T !!
```

3. WRIDEX: Writer of DECtape Index

```
Ø1.Ø1 E
Ø1.Ø2 A ?SUBJECTS ?,"STARTING SUBJECT ",SS,!
Ø1.Ø3 *
Ø1.Ø4 A NS;I (SS-NS)Ø1.25,Ø1.1Ø,Ø3.Ø5
Ø1.1Ø S X=FDTA(1,1);F H=1,SU;D 2
Ø1.12 S X=FDTA(1,Ø);*;Q
Ø1.25 T "PAPERTAPE TOO FAR ADVANCED ",!;*;Q

Ø2.Ø4 S X=FP(NS,FB(199)*13+2ØØ)
Ø2.Ø6 A X;A X
Ø2.Ø7 F J=1,6;D 4
Ø2.Ø8 S X=FP(FB(199)+1,199);D Ø2.Ø6;D Ø2.Ø6;A NS

Ø3.Ø5 F J-1,3Ø;A X
Ø3.Ø6 G Ø1.Ø4

Ø4.Ø5 D Ø2.Ø6
Ø4.Ø6 F I=Ø,1;A Y;S X=FP(FITR(Y/2-Ø.5),(FB(199)*13+2ØØ)+(J+(I*6)))

1Ø.Ø1 F J=1,2Ø47;S X=FP(Ø,J)
1Ø.Ø2 S X=FP(2,12);S X=FDTA(1,Ø);G Ø1.Ø1
```

4. RUNTIM: Runtime Programme

Language: LABFOCAL

This programme was written to control the computer recording of tapping tasks. Manual overide facilities were provided so that the Experimenter could assume control in the event of an error on the part of the subject. Tapping data were stored on magnetic DECtape.

4. RUNTIM: Runtime Programme

```
Ø1.Ø1 E
Ø1.Ø3 S X=FCLK(1Ø,3);S X=FDTA(1,1);S BL=FB(FB(1Ø)*3+12)
Ø1.Ø5 I (9Ø-(BL+15))15.Ø1;S NT=12;S SU=FB(FB(1Ø)*13+2ØØ)
Ø1.Ø7 S CO=1;F J=1,12;S A(J)=FB(FB(1Ø)*13+2ØØ+J)
Ø1.Ø9 T % Ø3.ØØ, "SUBJECT",SU," STARTING",BL,!;S X=FDTA(BL,1)
Ø1.11 I (NT-CO)1Ø.Ø1
Ø1.13 T !,"%",!;A C,!;I (Ø-C)Ø6.Ø1;I (1-A(CO))Ø3.Ø5,Ø2.Ø5,Ø2.Ø5

Ø2.Ø5 S X=FADC(A(CO));I (25Ø-X)Ø2.1Ø;G Ø2.Ø5
Ø2.1Ø S X=FSAM(Ø,55Ø,A(CO),1)
Ø2.11 S X=FADC(A(CO));I (X-25Ø)Ø2.15;G Ø2.11
Ø2.15 S X=FADC(A(CO));I (25Ø-X)Ø2.2Ø;G Ø2.15
Ø2.2Ø S X=FSAM(Ø,2ØØØ,A(CO),1);S X=FP(A(CO),2ØØ1);S X=FDTA(BL+BC,Ø)
Ø2.21 S BC=BC+1;S CO=CO+1;G Ø1.11

Ø3.Ø5 S X=FADC(Ø);I (25Ø-X)Ø3.2Ø;S X=FADC(1);I (25Ø-X)Ø3.21;G Ø3.Ø5
Ø3.2Ø S H=Ø;G Ø3.25
Ø3.21 S H=1
Ø3.25 S X=FSAM(Ø,5ØØ,Ø,1)

Ø4.Ø6 S X=FADC(H);I (X-25Ø) Ø4.1Ø;G Ø4.Ø6
Ø4.1Ø S X=FADC(H);I (25Ø-X) Ø4.15;G Ø4.1Ø
Ø4.15 S X=FSAM(Ø,1ØØØ,Ø,2);S X=FP(A(CO),2ØØ1);S X=FP(H,2ØØ2)
Ø4.16 S X=FDTA(BL+BC,Ø);S BC=BC+1

Ø5.Ø1 D 4;S CO=CO+1;G Ø1.11

Ø6.Ø1 T "RERUN",!;I (1-A(CO-1))Ø6.Ø3;S CO=CO-1;S BC=BC-1;G Ø1.13
Ø6.Ø3 S CO=CO-1;S BC=BC-2;G Ø1.13

1Ø.Ø1 T % Ø2.ØØ, !,"ENDS",(BL+BC)-1,!
1Ø.Ø2 S X=FDTA(1,1);S X=FP(SU,FB(1Ø)*3+11);S X=FP(BL,FB(1Ø)*3+12)
1Ø.Ø3 S X=FP(BL+BC-1,FB(1Ø)*3+13);S X=FP(FB(1Ø)+1,1Ø)
1Ø.Ø4 S X=FP(BL+BC,FB(1Ø)*3+12);S X=FDTA(1,Ø);Q

15.Ø1 T "INSUFFICIENT SPACE ON DECTAPE ",!;Q
```

5. 6. COM1, COM2: Compressor Stages 1 and 2

Language: LABFOCAL

The action of these programmes was described fully in the text. Their function was to convert the data recorded by RUNTIM into a more economical form for longer term storage.

5. COM1: Data Compressor Stage 1

```
Ø1.Ø1 E
Ø1.Ø3 S X=FDTA(1,1);I (FB(1Ø)-FB(8))2Ø.Ø1,Ø1.25,Ø1.Ø5
Ø1.Ø5 S SU=FB(FB(8)*3+11);S BL=FB(FB(8)*3+12)
Ø1.Ø7 T % Ø3.ØØ,"SUBJECT ",SU," STARTING AT ",BL,!
Ø1.Ø9 F J=Ø,15;S X=FDTA(BL+J,1);S X=FRB(Ø);S X=FDTA(BL+J,Ø)
Ø1.1Ø S X=FDTA(1,1);S X=FP(FB(8)+1,8);S X=FDTA(1,Ø);Q
Ø1.25 T "ALL ANALYSED",!;Q

1Ø.Ø1 T "IMPOSSIBLE ERROR",!;Q
```

6. COM2: Data Compressor Stage 2

```
Ø1.Ø1 E
Ø1.Ø2 S X=FDTA(8,1);I (FB(1Ø)-FB(9))2Ø.Ø1,Ø1.25,Ø1.Ø5
Ø1.Ø5 S  BL(1)=FB(FB(9)*3+12);S BL(2)=BL(1)*8
Ø1.Ø6 I (15-CO)1Ø.Ø1;S X=FDTA(BL(2)+(CO*8),1);S G=FB(2);G Ø1.1Ø
Ø1.Ø9 S X=FDTA(BL(2)+(CO*8)+LC,1)
Ø1.1Ø S X=FDTA(BL(2)+AF,Ø);S AF=AF+1
Ø1.12 I (256+(LC*256)-(G+6))Ø1.15;S CO=CO+1;S LC=Ø;G Ø1.Ø6
Ø1.15 S LC=LC+1;G Ø1.Ø9
Ø1.25 T "ALL ANALYSED",!;Q

1Ø.Ø1 S X=FDTA(8,1);S X=FP(BL(1)+FITR(AF/8),FB(9)*3+13)
1Ø.Ø2 S X=FP(FB(9)+1,9);S X=FP(BL(1)+FITR(AF/8)+1,FB(9)*3+12)
1Ø.Ø3 S X=FDTA(8,Ø);Q

2Ø.Ø1 T "IMPOSSIBLE ERROR",!;Q
```

7. EXPAN: Experimental Data Analysis Programme

Language: LABFOCAL.

This programme operated on the compressed data to produce, for each trial, the mean and standard deviation of the inter-tap intervals. These statistics were output on the Teletype, but the means were also output on paper tape for input into F/RO 4 and other statistical programmes.

7. EXPAN: Experimental Data Analysis Programme

```
Ø1.Ø1 E
Ø1.Ø2 A ?SUBJECTS ?,"FIRST SUBJECT ? ",SF,!;*;T % Ø8.Ø4,!
Ø1.Ø8 I (SU-SC)2Ø.Ø1,2Ø.Ø1,Ø1.Ø9
Ø1.Ø9 S GC=0;G Ø7.Ø5
Ø1.1Ø S X=FDTA(1,1);S AR=FB((SC+GC)*3+12);S TC=Ø
Ø1.11 T "SUBJECT ",FB((SC+GC)*3+11),!
Ø1.12 S X=FDTA(AR,1);S SN=Ø
Ø1.2Ø I (TC-(6+(HC*6)))Ø1.24;G 15.Ø1
Ø1.24 S SM=Ø;S SS=Ø
Ø1.25 S ST=7+(256*SN);I (2Ø47-ST)Ø1.28
Ø1.26 I (256-FB(ST-5))Ø1.3Ø
Ø1.27 I (4-A(TC+1))Ø4.Ø3;G Ø2.Ø5
Ø1.28 S AR=AR+1;D Ø1.12;G Ø1.25
Ø1.3Ø S SN=SN+1;G Ø1.27

Ø2.Ø5 I (FITR(FB(ST-5)/2)-FB(ST-5)/2)Ø2.1Ø
Ø2.Ø8 S N=FB(ST-5)-2;G Ø2.11
Ø2.1Ø S N=FB(ST-5)-1
Ø2.11 F J=ST,2,ST+(N-1);D Ø2.4Ø
Ø2.12 T N/2,(SS-SM↑2/(N/2))/(N/2-1),!
Ø2.15 S M(A(TC+1)+HC*6)=(N/2)/SM
Ø2.25 S TC=TC+1;S SN=SN+1;G Ø1.2Ø
Ø2.4Ø S SM=SM+FB(J)/1ØØ+FB(J+1)/1ØØ;S SS=SS+(FB(J)/1ØØ+FB(J+1)/1ØØ)↑2

Ø4.Ø3 S RG=RG*(-1);S TA=TA*((RG+1)/2)
Ø4.Ø4 S TI=Ø;S J=Ø;I (FB(ST-1)-5ØØ)Ø4.Ø5;S RE=1;G Ø4.Ø7
Ø4.Ø5 S RE=Ø
Ø4.Ø7 S J=J+1;S TI=TI+FB(J+(ST-1));I (TI-1ØØØ)Ø4.Ø7,Ø4.1Ø,Ø4.12
Ø4.1Ø S LE=J;S RS=J+1;I (RE)25.Ø1,Ø4.2Ø,Ø4.3Ø
Ø4.12 S LE=J;S RS=J;I (RE)2Ø.Ø1,Ø4.13,Ø4.14
Ø4.13 S X=FP(TI-1ØØØ,RS+(ST-1));G Ø4.2Ø
Ø4.14 S X=FP(FB(J+ST-1)-(TI-1ØØØ),ST+(LE-1));G Ø4.3Ø
Ø4.2Ø I (FSGN(FITR(((FB(ST-5)-RS)+1)/2)-((FB(ST-5)-RS)+1)/2))4.21;G 4.22
Ø4.21 S N=FB(ST-5)-RS;G Ø4.25
Ø4.22 S N=FB(ST-5)-RS-1
Ø4.25 F J=(ST-1)+RS,2,(ST-2)+RS+N;D Ø2.4Ø
Ø4.26 G Ø4.34
Ø4.3Ø I (FSGN(FITR(LE/2)-LE/2))Ø4.31;G Ø4.32
Ø4.31 S N=LE-1;G Ø4.33
Ø4.32 S N=LE-2
Ø4.33 F J=ST,2,ST+(N-1);D Ø2.4Ø
Ø4.34 S SN=SN+1;S TA=TA+(N/2);S TC=TC+(1*((RG+1)/2));I (RG)Ø1.25;G Ø4.4Ø
Ø4.4Ø T TA,(SS-SM↑2/TA)/(TA-1),!
Ø4.41 S M(A(TC)+HC*6)=TA/SM;G Ø1.2Ø

Ø6.Ø1 A ?STARTS ?,!
Ø6.Ø2 S J=Ø;S TI=Ø
Ø6.Ø3 S J=J+1;S TI=TI+FB(J+(ST-1));I (TI-1ØØØ)Ø6.Ø3,Ø6.Ø4,Ø6.Ø5
Ø6.Ø4 T "CLEAN ",J,!;Q
Ø6.Ø5 T "SPLIT ",J,!;Q

Ø7.Ø5 S HC=Ø;S RG=1;I (SC)25.Ø1,Ø7.2Ø;F J=1,3;A X
Ø7.Ø6 F J=1,6;A X;A X;F I=Ø,1;A A(J+(I*6))
Ø7.12 F J=1,4;A X
Ø7.15 G Ø1.1Ø
Ø7.2Ø A X;I (SF-X)Ø7.3Ø,Ø7.21,Ø7.25
Ø7.21 A X;A X;G Ø7.Ø6
Ø7.25 S GC=GC+1;F J=1,3Ø;A X
Ø7.26 G Ø7.2Ø
Ø7.3Ø T "PAPERTAPE ADVANCED ",!;*;Q

15.Ø1 I (HC-1)15.Ø2;G 15.Ø4
15.Ø2 S HC=HC+1;G  Ø1.24
15.Ø4 T !;F J=1,1ØØØ;S X=X+1
15.Ø5 S X=FPTP(1);F J=1,12;T M(J),!
15.Ø6 F J=1,1Ø;S X=X+1
15.Ø7 S X=FPTP(Ø)
15.Ø9 T !
15.1Ø S SC=SC+1;G Ø1.Ø8

2Ø.Ø1 T "END",!;*;Q
```

SECTION C:

Experiment VI Software

1. Overlay FDI: Digital Input Function

Language: PAL-8

This overlay permitted the control of the Digital Input module by LABFOCAL. This was used in Experiment VI to enable the Runtime programme to "sense" the pressing of the "Start" key.

1. Overlay FDI: Services Digital Input

```
/****OVERLAY TO LABFOC; FDI()      PAL8-V7             PAGE 1

               /****OVERLAY TO LABFOC; FDI()
               /OVERLAYS FPO
        4062   *4062
04062   1121   1121
        4242   *4242
04242   7300           CLA CLL
04243   3276           DCA COUNT         /CLEAR COUNTER
04244   7040           CMA
04245   6503           DBCI              /CLEAR INPUT REGISTER
04246   7300           CLA CLL
04247   6502           DBSK              /INPUT EVENT YET ?
04250   5247           JMP .-1           /NO - TRY AGAIN
04251   6504           DBRI              /YES - READ IT
04252   3275           DCA WORD          /STORE EVENT
04253   1275           TAD WORD
04254   0274   AGAIN,  AND MASK
04255   7440           SZA
04256   5265           JMP ROTATE
04257   1276           TAD COUNT
04260   3046           DCA 46
04261   3045           DCA 45
04262   1277           TAD P27
04263   3044           DCA 44
04264   5536           JMP I EFUN31      /FUNCTION RETURN
04265   7300   ROTATE, CLA CLL
04266   1275           TAD WORD          /FETCH EVENT WORD
04267   7004           RAL               /ROTATE LEFT ONCE
04270   3275           DCA WORD          /STORE ROTATED VERSION
04271   1275           TAD WORD
04272   2276           ISZ COUNT         /INCREMENT COUNT
04273   5254           JMP AGAIN         /MASK AGAIN
04274   3777   MASK,   3777
04275   0000   WORD,   0
04276   0000   COUNT,  0
04277   0027   P27,    27
               /DEFINITIONS
        6503   DBCI=6503
        6502   DBSK=6502
        6504   DBRI=6504
        0136   EFUN31=136
               $
```

2. Overlay FDO: Digital Output Function

Language: PAL-8

This overlay permitted LABFOCAL to service the Digital Output module. It was used in conjunction with the FCLK (clock) function of LABFOCAL, and was used in Experiment VI to permit computer control of the communication tone.

2. Overlay FDO: Services Digital Output

```
/*****OVERLAY TO LABFOC: FDO()     PAL8-V7              PAGE 1

                /*****OVERLAY TO LABFOC: FDO()
                /CLOCK RATE MUST BE SET BY FCLK
                /OVERLAYS FPI()
        4061    *4061
04061   1127    1127
        4310    *4310
04310   7300            CLA CLL
04311   4453            JMS I INTEGER   /GET ARG.
04312   7040            CMA
04313   3342            DCA ARG
04314   1343            TAD WORD1
04315   2342    RET,    ISZ ARG
04316   5340            JMP ROTATE
04317   3344            DCA WORD
04320   6134    CHECK,  CLEN            /READ ANABLE REGISTER
04321   7650            SNA CLA         /CLOCK RUNNING?
04322   4566            ERROR4          /NO - ERROR
04323   7300            CLA CLL         /YES
04324   1344            TAD WORD        /FETCH DB OUTPUT WORD
04325   6506            DBSO            /OUTPUT
04326   7300            CLA CLL
04327   6135            CLSA            /CLEAR FLAG
04330   7300            CLA CLL
04331   1745            TAD I PNTR      /FETCH PRESET VALUE
04332   6133            CLAB            /LOAD PRESET
04333   6131            CLSK            /OVERFLOW?
04334   5333            JMP .-1         /NO - TRY AGAIN
04335   7240            CLA CMA
04336   6505            DBCO            /YES - CLEAR OUTPUT WORD
04337   5536            JMP I EFUN31    /RETURN TO FOCAL
04340   7010    ROTATE, RAR
04341   5315            JMP RET
04342   0000    ARG,    0
04343   4000    WORD1,  4000
04344   0000    WORD,   0
04345   3522    PNTR,   3522
                /DEFINITIONS
        6134    CLEN=6134
        4566    ERROR4=4566
        6135    CLSA=6135
        6506    DBSO=6506
        6133    CLAB=6133
        6131    CLSK=6131
        6505    DBCO=6505
        0136    EFUN31=136
        0053    INTEGER=53
                $
```

3. SEMDIF: Semantic Differential Printer

Language: P/FOCAL

This programme was used to randomise the order and polarity of the Semantic Differential scales for each trial. The forms which would be required during the experiment were then printed on the Teletype. A specimen output is included.

3. SEMDIF: Semantic Differential Preparation Programme

```
Ø1.Ø1 E
Ø1.Ø2 A ?SUBJECTS?;T !!!!!!!!!!;F K=1,SU;D 2
Ø1.1Ø Q

Ø2.Ø3 T % Ø3.ØØ,"SUBJECT",K,!!!!!
Ø2.Ø5 F TR=Ø,7;D 3

Ø3.Ø4 T % Ø3.ØØ,"TRIAL",TR,!!!!
Ø3.Ø5 F M=1,1Ø;S A(M)=Ø
Ø3.1Ø F M=1,1Ø;D 1Ø
Ø3.15 T !!!!!,"**********",!!!!

1Ø.Ø5 S X=FITR(FRAN()*1Ø+1);I (1Ø-X) 1Ø.Ø5
1Ø.Ø7 I (Ø-A(X)) 1Ø.Ø5
1Ø.1Ø S A(X)=1
1Ø.11 S Y=FITR(FRAN()*2+1);I (2-Y) 1Ø.11
1Ø.2Ø I (X-1Ø)1Ø.23;I (Y-2)1Ø.22
1Ø.21 D 2Ø.37;D 2Ø.Ø1;D 2Ø.21;D 2Ø.Ø2;D 2Ø.22;R
1Ø.22 D 2Ø.38;D 2Ø.Ø2;D 2Ø.21;D 2Ø.Ø1;D 2Ø.22;R
1Ø.23 I (X-9)1Ø.26;I (Y-2)1Ø.25
1Ø.24 D 2Ø.33;D 2Ø.Ø3;D 2Ø.21;D 2Ø.Ø4;D 2Ø.22;R
1Ø.25 D 2Ø.34;D 2Ø.Ø4;D 2Ø.21;D 2Ø.Ø3;D 2Ø.22;R
1Ø.26 I (X-8)1Ø.29;I (Y-2)1Ø.28
1Ø.27 D 2Ø.Ø5;D 2Ø.21;D 2Ø.Ø6;D 2Ø.22;R
1Ø.28 D 2Ø.34;D 2Ø.Ø6;D 2Ø.21;D 2Ø.Ø5;D 2Ø.22;R
1Ø.29 I (X-7) 1Ø.32;I (Y-2)1Ø.31
1Ø.3Ø D 2Ø.32;D 2Ø.Ø7;D 2Ø.21;D 2Ø.Ø8;D 2Ø.22;R
1Ø.31 D 2Ø.37;D 2Ø.Ø8;D 2Ø.21;D 2Ø.Ø7;D 2Ø.22;R
1Ø.32 I (X-6) 1Ø.35; I (Y-2) 1Ø.34
1Ø.33 D 2Ø.Ø9;D 2Ø.21;D 2Ø.1Ø;D 2Ø.22;R
1Ø.34 D 2Ø.33;D 2Ø.1Ø;D 2Ø.21;D 2Ø.Ø9;D 2Ø.22;R
1Ø.35 I (X-5) 1Ø.38;I (Y-2)1Ø.37
1Ø.36 D 2Ø.35;D 2Ø.11;D 2Ø.21;D 2Ø.12;D 2Ø.22;R
1Ø.37 D 2Ø.34;D 2Ø.12;D 2Ø.21;D 2Ø.11;D 2Ø.22;R
1Ø.38 I (X-4)1Ø.41;I (Y-2)1Ø.4Ø
1Ø.39 D 2Ø.32;D 2Ø.13;D 2Ø.21;D 2Ø.14;D 2Ø.22;R
1Ø.4Ø D 2Ø.37;D 2Ø.14;D 2Ø.21;D 2Ø.13;D 2Ø.22;R
1Ø.41 I (X-3) 1Ø.44;I (Y-2) 1Ø.43
1Ø.42 D 2Ø.37;D 2Ø.15;D 2Ø.21;D 2Ø.16;D 2Ø.22;R
1Ø.43 D 2Ø.37;D 2Ø.16;D 2Ø.21;D 2Ø.15;D 2Ø.22;R
1Ø.44 I (X-2) 1Ø.47;I (Y-2)1Ø.46
1Ø.45 D 2Ø.35;D 2Ø.17;D 2Ø.21;D 2Ø.18;D 2Ø.22;R
1
 Ø.46 D 2Ø.31;D 2Ø.18;D 2Ø.21;D 2Ø.17;D 2Ø.22;R
1Ø.47 I (Y-2)1Ø.49
1Ø.48 D 2Ø.19;D 2Ø.21;D 2Ø.2Ø;D 2Ø.22;R
1Ø.49 D 2Ø.35;D 2Ø.2Ø;D 2Ø.21;D 2Ø.19;D 2Ø.22;R

2Ø.Ø1 T "GOOD"
2Ø.Ø2 T "BAD"
2Ø.Ø3 T "GRACEFUL"
2Ø.Ø4 T "AWKWARD"
2Ø.Ø5 T "PLEASURABLE"
2Ø.Ø6 T "PAINFUL"
2Ø.Ø7 T "BEAUTIFUL"
2Ø.Ø8 T "UGLY"
2Ø.Ø9 T "CONSTRICTED"
2Ø.1Ø T "SPACIOUS"
2Ø.11 T "ACTIVE"
2Ø.12 T "PASSIVE"
2Ø.13 T "EXCITABLE"
2Ø.14 T "CALM"
2Ø.15 T "FAST"
2Ø.16 T "SLOW"
2Ø.17 T "STABLE"
2Ø.18 T "CHANGEABLE"
2Ø.19 T "INTERESTING"
2Ø.2Ø T "BORING"
2Ø.21 T "   ";F J=1,7;T "...    "
2Ø.22 T !!
2Ø.31 T " "
2Ø.32 T "  "
2Ø.33 T "   "
2Ø.34 T "    "
2Ø.35 T "     "
2Ø.37 T "       "
2Ø.38 T "        "
```

Specimen Output from SEMDIF

TRIAL 6

PAINFUL	...	...	...	...	...	...	...	PLEASURABLE
BEAUTIFUL	...	...	...	...	...	...	...	UGLY
SLOW	...	...	...	...	...	...	...	FAST
GOOD	...	...	...	...	...	...	...	BAD
BORING	...	...	...	...	...	...	...	INTERE
STABLE	...	...	...	...	...	...	...	CHANGEABLE
GRACEFUL	...	...	...	...	...	...	...	AWKWARD
CONSTRICTED	...	...	...	...	...	...	...	SPACIOUS
CALM	...	...	...	...	...	...	...	EXCITABLE
ACTIVE	...	...	...	...	...	...	...	PASSIVE

TRIAL 7

BEAUTIFUL	...	...	...	...	...	...	...	UGLY
GOOD	...	...	...	...	...	...	...	BAD
SLOW	...	...	...	...	...	...	...	FAST
GRACEFUL	...	...	...	...	...	...	...	AWKWARD
PASSIVE	...	...	...	...	...	...	...	ACTIVE
CHANGEABLE	...	...	...	...	...	...	...	STABLE
CALM	...	...	...	...	...	...	...	EXCITABLE
CONSTRICTED	...	...	...	...	...	...	...	SPACIOUS
BORING	...	...	...	...	...	...	...	INTERE
PLEASURABLE	...	...	...	...	...	...	...	PAINFUL

*

4. EXRUN6: Experiment VI Runtime Programme

Language: LABFOCAL

This programme controlled the computer recording of the tapping tasks. Data were output in a compressed form on paper tape. Manual overide facilities were provided so that the Experimenter could assume control in the event of an error on the part of the subject.

4. EXRUN6: Experiment VI Runtime Programme

```
Ø1.Ø1 E
Ø1.Ø2 S X=FDTA(1,1);F J=1,7;D Ø1.Ø4
Ø1.Ø3 T % Ø2.ØØ, "SUBJECT",FB(1Ø)+1,!,!,!;G Ø1.Ø5
Ø1.Ø4 S MU(J)=FB((1Ø+J)+1Ø*FB(1Ø))/1ØØ
Ø1.Ø5 S TR=Ø
Ø1.Ø6 S X=FDI();S X=FCLK(75,2);S X=FDO(1Ø)
Ø1.Ø7 D 2
Ø1.Ø8 S X=FDTA(6,1)
Ø1.15 S TR=TR+1;I (7-(TR))Ø1.4Ø
Ø1.19 S X=FDI()
Ø1.2Ø S B(TR)= FITR(CL*MU(TR));S X=FCLK(B(TR),2);S X=FMET(3Ø)
Ø1.21 S K=Ø;D 1Ø;S X=FCLK(3,2);S X=FSAM(Ø,2ØØØ,Ø,1)
Ø1.22 D 1Ø.Ø2;S K=1;D 1Ø;G Ø1.15
Ø1.4Ø T % Ø3.ØØ, !,"PREFERRED-",CL,!,!,!,!
Ø1.49 S TR=Ø;S K=Ø;F J=1,3;D Ø1.52
Ø1.5Ø F TR=1,7;D Ø1.85
Ø1.51 G Ø1.6Ø
Ø1.52 S NT=Ø;S TA=Ø;D 3;T % Ø8.Ø4,(2ØØØ-TA)/NT,!
Ø1.6Ø A X;S TR=Ø;F J=1,3;D 16
Ø1.7Ø S TR=TR+1;I (7-TR) Ø1.8Ø;G 15.Ø1
Ø1.8Ø S X=FDTA(1,1);S X=FP(FB(1Ø)+1,1Ø);S X=FDTA(1,Ø);Q
Ø1.85 T % Ø4.ØØ,!!!,MU(TR)*1ØØ,!,B(TR),!!;F K=Ø,1;F J=1,3;D Ø1.52

Ø2.Ø2 S NT=Ø;S TA=Ø;S K=Ø;D 1Ø;F J=1,3;D 3
Ø2.1Ø S CL=FITR((6ØØØ-TA)/NT)

Ø3.Ø5 S X=FDTA((TR*6)+(K*3)+(J+1),1)
Ø3.1Ø I (FSGN(FITR(FB(2)/2)-FB(2)/2))Ø3.15
Ø3.11 S TA=TA+FB(FB(2)+5)+FB(FB(2)+6);S X=FP(FB(2)-2,2);S NT=NT+FB(2)/2
Ø3.12 G Ø3.5Ø
Ø3.15 S TA=TA+FB(FB(2)+6);S X=FP(FB(2)-1,2);S NT=NT+FB(2)/2
Ø3.5Ø R

1Ø.Ø1 S X=FCLK(1,2);F J=1,3;D 11
1Ø.Ø2 S X=FCLK(75,2);S X=FDO(1Ø)

11.Ø1 S X=FADC(Ø)
11.Ø2 I (X-25Ø) 11.Ø3,11.Ø3,11.Ø4
11.Ø3 D 11.Ø1;I (25Ø-X) 11.Ø4,11.Ø4,11.Ø3
11.Ø4 D 11.Ø1;I (X-25Ø)11.Ø5,11.Ø5,11.Ø4
11.Ø5 S X=FSAM(Ø,2ØØØ,Ø,1);S X=FRB();S X=FDTA((TR*6)+(K*3)+(J+1),Ø)

15.Ø1 S J=Ø
15.Ø2 S J=J+1;I (6-J)Ø1.7Ø
15.Ø3 I (FADC(1)-5ØØ) 15.Ø3
15.Ø5 S X=FDTA(TR*6+(J+1),1)
15.Ø6 C HOLD-UP
15.Ø7 S X=FPTP(2)
15.Ø9 T FB(2),!
15.1Ø F K=7,FB(2)+6;T % Ø3.ØØ, FB(K),!
15.12 C HOLD-UP
15.2Ø S X=FPTP(Ø)
15.21 C HOLD-UP
15.3Ø G 15.Ø2

16.Ø5 S X=FDTA(TR*6+(J+1),1);
16.Ø6 C HOLD-UP
16.Ø7 S X=FPTP(2)
16.Ø9 T FB(2),!
16.1Ø F K=7,FB(2)+6;T % Ø3.ØØ,FB(K),!
16.12 C HOLD-UP
16.2Ø S X=FPTP(Ø)
```

G.L. DAVIES Ph.D. THESIS 1976

Abstract

A number of previous authors have investigated individual differences in the tempi at which people perform everyday activities such as walking, speaking and writing. The present work consists of seven studies of the tempi which subjects spontaneously adopt when performing simple laboratory tasks, particularly repetitive motor activities such as tapping.

In the first experiment, intercorrelational data were reported which contradicted the view that there is a unitary "personal tempo", though clusters of intercorrelated activities were obtained. In the second study, "spontaneous" and "maximum" tempi were compared, previous work on this question being considered methodologically unsound. It was concluded that there is little or no common variance between speeds elicited by "spontaneous" and "maximum" tempo instructions.

Later experiments were concerned with the question as to whether there might exist a "preferred rhythm" - a rhythm of performance which the subject adopts whenever the conditions of the task permit him to do so. Evidence in favour of this suggestion was obtained in the first experiment, but the third and fourth experiments, which were designed to test a prediction derived from it, failed to support the hypothesis. This negative conclusion was further supported in a study in which subjects were paced at rates other than that which they spontaneously adopted, and in which they displayed no tendency to depart from the imposed ("non-preferred")

rate when presented with an opportunity to do so. Finally, an experiment was performed which demonstrated no significant difference between the test-retest reliabilities of the speed which the subject spontaneously adopts and speeds arbitrarily imposed by the experimenter. It was concluded that there was no need for the hypothesis of a "special" or "preferred" rhythm with these tasks.