# STELLAR EVOLUTION AND SPECTRAL CLASSIFICATION (1860 - 1910)

by

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To My Parents

D.H.DEVORKIN





#### ABSTRACT

The interdependence of schemes of spectral classification and theories of stellar evolution is examined and discussed in the context of the work of major 19th and early 20th century astronomers, including A. Secchi, H. Vogel, W. Huggins, N. Lockyer, A. Ritter, F. McClean, W.H.S. Monck, A. Maury, W.P. Fleming, E.C. Pickering, E. Hertzsprung, and H.N. Russell. The nineteenth century concept of evolution through gravitational contraction is identified as a dominant theme, and is analysed within the historical context of the establishmen and exploitation of the energy conservation laws during the midnineteenth and early twentieth centuries. The factors affecting studies of stellar evolution in the late nineteenth and early twentieth centuries, including developing techniques in astronomical spectroscopy, stellar kinematics, laboratory spectroscopy, astrometry, photography, and the application of physical theory to astronomical problems, form a necessary background to this theme. Finally, the early development of the Hertzsprung-Russell Diagram is critically examined.

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# AN ASTRONOMICAL SYMBIOSIS: STELLAR EVOLUTION AND SPECTRAL CLASSIFICATION

# (1860 - 1910)

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#### INTRODUCTION

A central theme in the development of modern astrophysics during the 19th and 20th centuries was the classification and interpretation of stellar spectra. A provocative element in this line of work was the proclivity to discuss schemes of classification in terms of the idea that stars would change their spectra with time. Thus, the basic methods and aims of classification - the identification of similarities and morphological continuity from class to class - became the basic ingredients through which the process of stellar evolution might be revealed. As Agnes Clerke commented at the turn of the century, "Modes of classifying the stars have come to be equivalent to theories of their evolution."<sup>1</sup> On the other hand, we might ask, to what degree did theories of evolution influence classification?

It is the aim of this study to examine the degree of influence evolution had upon classification, as both developed during the period 1860 to 1910. The beginning of this interval is defined by the virtually simultaneous appearance of works by Kirchhoff and Darwin, when both schemes of classification and a strong interest in evolutionary interpretation began to appear. It ends with the first decade of the present century, when, after over forty years of classification, attempts failed to provide a consensus, and an international group of astronomers sat down together to attempt to clarify the issue. This period also included many technological advances in instrumentation and technique, as well as developments in radiation theory and the interpretation of atomic spectroscopy which have had a strong and lasting influence upon astrophysics.

We will begin by discussing speculative attempts to understand stars as physical structures with finite lifetimes and limited sources of energy. Though such attempts became controversial in the latter part

of the 19th century, when they were found to be irreconcilable with the age requirements of uniformitarian geologists and Darwinians,<sup>2</sup> we will be concerned with the subject only insofar as sources of stellar and solar energy were used as a basis for determining how the physical structure of a star would respond to adjustments in its store and source of heat. In other words, how did the observed characteristics of a star - the amount and character of radiation (both dependent upon the radius and temperature of the star), and the mass - change with expenditure of energy?

To provide an historical context for the solution to these questions, many established and emerging concepts and techniques in astronomy and astrophysics will be reviewed. The development of techniques in laboratory spectroscopy for the interpretation of stellar and solar spectra was a primary activity. Lockyer and Huggins, the early initiators of these experimental studies, depended upon contacts with chemists and physicists familiar with, and interested in, such problems. Gradually, the crude visual observations of stellar spectra became replaced by photographic techniques, allowing for higher dispersion and far greater resolution. Spectrum-photography also enabled transient phenomena, such as the flash spectrum of the Sun, to be recorded as unambiguously analysed, to say nothing of greatly increasing the objectivity of line identification and line structure. Though the application of photography in the observatory and the ever more sophisticated and broad-ranging techniques for the duplication of stellar spectra in the laboratory greatly aided the physical interpretation of stellar spectra, there was still little agreement amongst astronomers concerning the nature of that interpretation. Thus, the interpretation of observed spectral differences amongst stars ranged widely, and was often influenced by, and had great influence upon, preferred theories of stellar evolution. Finally, when

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Hale began his own laboratory studies, and Alfred Fowler meticulously continued studies initiated by Lockyer, some general agreement began to appear, making it possible to arrive at some physical understanding of stellar spectra. But this interval of time spanned some thirty years. through which there were also important advances in radiation theory. particularly concerning the relation between the colour and temperature of a radiating body; and advances in observing techniques which allowed for larger samples of stars to be studied for apparent brightness, parallax and spectrum. When processed, the first two yielded the absolute energy outputs of the stars, and the third, through the laboratory techniques just mentioned, yielded both an estimate of the chemical composition of the stars and their temperature. By the turn of the century, established techniques for determining both the proper motions and radial velocities of the stars began to yield samples sufficiently large that it became possible to discuss empirically their kinematic properties, when compared with brightness and spectrum.

Our first chapter will show how the direction of evolution of the structure of a star, established since the time of Newton as one of general contraction, was used to answer the important question of stellar lifetimes; that of the Sun in particular. Three continuing theories of the source of stellar heat will be examined: meteoritic bombardment (or the mechanical conversion of motion into heat through collisional friction), simple cooling through radiation, and the conversion of the gravitational potential of the contracting star into heat. We will look closely at studies of the first and third mechanisms, for in many cases they will be connected, and it will be here that we first encounter the possibility that a star might be heating and/or expanding with age.

The most important question, involving not only stellar

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lifetimes but the nature of the evolutionary paths, will be how their luminosities and temperatures change with time. The first chapter will have as its primary responsibility the clarification of how these physical properties of stars do change with time, according to the theories of individuals such as Helmholtz, Kelvin, Lane and Ritter. In addition, the relative acceptance of the various theories presented by those named above by the astronomical community will be determined, which will then serve as a foundation for discussions in subsequent chapters.

The second chapter will review the development of major schemes of spectral classification of stars from Secchi to the Harvard Classification. At every stage in our discussion, we will identify the degree to which either the common theory of stellar evolution accepted by the majority, or the peculiar theory of the individual classifier, aided the development of the system. In this chapter, we establish the fact that the two activities were interdependent to a high degree.

Chapter three reviews major attempts at the end of the nineteenth and beginning of the twentieth centuries to arrive empirically at an understanding of the relative brightnesses and spatial frequencies of the different classes of stars. Difficulties encountered by Kapteyn and others in this line of work will be seen to be interpreted by Hertzsprung as due to the existence of a rare class of stars of high luminosity, but with spectra and colours deceptively similar to normal stars.

Hertzsprung's detection of these two classes of stars, now called giants and dwarfs, was duplicated by Russell, working directly from parallaxes and spectra. Russell's development of an evolutionary scheme based upon the existence of these two classes of stars comprises the bulk of chapter four, where we will look closely at influences upon him from the earlier work of Lockyer, and the supportive enterprise of

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#### E.C. Pickering.

Chapter five, in several respects, is a continuation of chapter two, in that it reviews the critical International Solar Union meeting held in 1910, which for the first time established, by committee, guidelines and procedures for the further organisation of stellar spectra into systems of classification. The testimony of astronomers queried by this committee identifies the degree to which theories of evolution influenced schemes of spectral classification, and also helps to illustrate which theories were in favour, and how astronomers felt in general about the state of understanding of stellar evolution. Since the major participants here were largely those people examined in previous chapters, this discussion serves as a culmination to the timeperiod examined in this work. Within this last chapter we will also discuss the origin of the technique of spectroscopic parallaxes, an empirical method of obtaining absolute luminosities of stars. It is included here to carry to a logical conclusion several studies in the late 19th and early 20th centuries by Lockyer, Huggins, Maury and Hertzsprung that anticipated the technique. It is also included because it provides the strongest evidence in favour of the reality of giants and dwarfs, independent of Russell's work.

Our story thus ends with the establishment of the existence of giants and dwarfs. The author has elsewhere discussed aspects of this particular episode in early astrophysics, which culminated in the direct interferometric measurement of the diameter of a giant star in 1920,<sup>3</sup> and so a rediscussion of the observation need not be made here. Ending at 1910 is also realistic in that it marks the end of a period of evolutionary thought where all stars were believed to fit on a continuous sequence of brightness and temperature. The new period, commencing approximately in 1910 and lasting at least until World War II, might best

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be characterised as one where the role of giants in evolution was a primary problem, and where the solution came from advances in atomic and nuclear physics, and a better appreciation of the behaviour of gases under extreme conditions.

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# CHAPTER 1

# The Direction of Stellar Evolution, 1850 to 1900

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#### CHAPTER 1

#### The Direction of Stellar Evolution, 1850 to 1900

#### Introduction

In this chapter we will be concerned with nineteenth-century attempts to understand the structure of the Sun and stars. We will also examine theories which provided estimates for the ages and lifetimes of the Sun and stars. At every stage, we will see that theories of stellar structure and ranges of stellar lifetimes were closely associated, and that a central question throughout the period was whether the temperatures and luminosities of stars rose, or fell, with time. This last discussion will then provide background for our central theme in later chapters: the interdependence of spectroscopic classification of the stars, and theories of the character of their evolutionary process.

By the beginning of the century, gravity as the cause of the formation of stars from nebulae was well established, and described in the works of Newton, Laplace and William Herschel.<sup>1</sup> Even by mid-century, with the observations of Lord Rosse suggesting that all nebulae were in fact unresolved clusters of stars, the existence of true nebulae was still entertained by some<sup>2</sup> though the concept had certainly been weakened. The problem as posed here of course was that, without nebulous matter, out of what do stars form? As we shall see, during the fifties, meteoritic theories became temporarily popular, not only as the source of heat for the stars, but also as the medium out of which they formed. By the 1860's with the spectroscopic studies of nebulae by Huggins, the existence of true nebulae became accepted, and though meteoritic theories survived for some time, theories of contraction of gas spheres grew steadily in popularity.

Another important background condition - to place our study in

proper context - was that by 1800, and certainly by the 1830s, the studies by William Herschel of the forms of nebulae and their classification (identifying them chronologically as stars still in a pre-stellar stage) had introduced the concept of change, or process, into astronomy.<sup>3</sup> This was not the old revelation that with time, celestial objects might change in spatial position. Here we see the element of time emerge as part of an evolutionary process, where, to paraphrase A.O. Lovejoy, the 'inventory' of nature (the classification of forms presumed static throughout all time) became the 'programme' of nature "... which is being carried out gradually and exceedingly slowly in the cosmic history ..."<sup>4</sup>

Even though John Herschel did not sympathise with his father's evolutionary concept, still, he continued his father's vast classification of nebulae; extending the work to the southern hemisphere. Ironically, John Herschel's work caused G.B. Airy to remark in 1836 that the various forms of nebulae seen stimulated "... the idea of change ....<sup>5</sup> in the heavens, and that the old universe which remained static with time had been replaced by one where processes of structural change were everywhere evident.

But enormous difficulties remained. What was the source of stellar heat? Were nebulae initially hot, out of which stars would form and cool? Or were nebulae initially cold: the source of heat for stars coming from another, yet unknown, mechanism, such as combustion or electrical action? Gravity was merely thought to be the cause of the process of contraction prior to the mid-century, and was not considered as an agent in the production of stellar heat.<sup>6</sup> Further, in William Herschel's view of the structure of the Sum as a solid surrounded by a luminous atmosphere (a model he derived from observations and interpretations of the structure of sunspots by Alexander Wilson in the late 18th century) we find that the process of change through contraction of

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a nebulae into a star would cease once the stellar stage appeared; though Herschel did consider briefly the continued accretion of material onto the surfaces of already formed stars.

Without a clear mechanism for the production of stellar heat, nebulae were thus thought of as being very hot, out of which stars would cool and condense. Once a star was actually formed, further physical change was thought to be cooling alone.

A major shift in thinking occurred at mid-century, with the establishment of the concept of the mechanical equivalent of heat through the work of Mayer and Joule. This new concept, together with the law of energy conservation it stimulated, was quickly applied to the source and maintenance of solar heat. The first part of our chapter will discuss the contributions of two major figures in this application -Hermann von Helmholtz, and William Thomson.

#### Helmholtz - "On the Interaction of Natural Forces" 1854

The original presentation of H. Helmholtz's paper was as a popular discourse in Konigsberg, in February, 1854. It was reprinted in the <u>Philosophical Magazine</u> in 1856, and it is from this source that we review his ideas.

In laying the groundwork for his eventual discussion of the source of solar heat Helmholtz remarked:

In the collision and friction of bodies against each other, the mechanics of former years assumed simply that living force was lost. But I have already stated that each collision and each act of friction generates heat; and, moreover, Joule has established by experiment the important law, that for every foot-pound of force which is lost a definite quantity of heat is always generated, and that when work is performed by the consumption of heat, for each foot-pound thus gained a definite quantity of heat disappears.<sup>7</sup>

Extending his commentary from the terrestrial to the celestial realm, he

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used the observation that the same gravitational forces present at the surface of the Earth were also at work in the heavens in the form of the nature of planetary motions and double stars. By analogy then, he was able to state

> ... that therefore the light and heat of terrestrial bodies do not in any way differ essentially from those of the sun, or of the most distant fixed star; that the meteoric stones which sometimes fall from external space upon the earth are composed of exactly the same simple chemical substances as those with which we are acquainted ...<sup>8</sup>

Nothing new was needed, therefore, to examine the store of force present in the celestial realm - the store of force "capable of action".

With these preliminaries, Helmholtz turned to the nebular hypothesis of Laplace to establish his model of the formation of stars and planets from nebulae. Helmholtz was one of those alluded to before who was not particularly affected by the apparent resolution of the Orion Nebula by Rosse; for he firmly stated his belief in the present existence of true nebulae, representative of the one which gave birth to the Sun and planets long ago.

The mechanism of a contracting nebula provided Helmholtz with his stellar source of light and heat, even though the original nebular mass might have been cold.

When the original nebular mass began to contract it possessed, "... in accordance with our new law, the whole store of force which at one time must unfold therein its wealth of actions ..."<sup>9</sup> Chemical forces had to remain dormant at this time, and await considerable condensation before they could take any hold.

Helmholtz regarded the original potential (or store of mechanical forces) of the nebular cloud to be such that:

... there is no necessity whatever to take refuge in the idea of a store of these originally existing.<sup>10</sup>

Evidently, the original read a bit different, as the unknown translator footnoted this remark with: "... No necessity for a Firemist". Thus the shining fluid of Herschel could be cold and

> When through condensation of the masses their particles came into collision and clung to each other, the <u>vis viva</u> of their motion would be thereby annihilated, and must reappear as heat.<sup>11</sup>

From this, Helmholtz proceeded to calculate the store of heat potentially available through this process of collision. He found the potential difference between an infinitely extended solar mass of negligible density, and that of the Sum in its present state. The amount of heat energy generated thus far (of which barely one part in 454 remains according to Helmholtz) was about 3500 times greater than what could be derived from an amount of coal equal in mass to the entire solar system. The great bulk of this heat energy must have been radiated quickly away, and it was quite some time, in Helmholtz's mind, before bodies like the planets could form as solids. Evidence from polar flattening suggested to him a previous state of fluidity for the terrestrial, and especially, the jovian planets.

But what of the store of force that is left to the system? Helmholtz saw here a vast store still held in planetary motions, and in the motions of meteoritic material. Helmholtz examined the conversion of the kinetic energy of meteoritic motion into heat through friction as the small bodies enter the Earth's atmosphere, and also discussed the heat derived by direct collisions of planetary bodies with the Sun, but did not come to a direct statement which could be classed as a meteoritic theory of the Sun's heat. He got quite close though:

Thus has the falling of the meteoric stone, the minute remnant of processes which seem to have played an important part in the formation of the heavenly bodies, conducted us to the present time, where we pass from the darkness of hypothetical views to the brightness of knowledge.<sup>12</sup>

The only hypothetical element in his scenario was the "... assumption of Kant and Laplace, that the masses of our system were once distributed as nebulae in space ..."<sup>13</sup>

If Helmholtz ended his discussion here, there would not have been much to distinguish it from the earlier theories of Laplace and Herschel, save for the mechanical equivalent of heat. But Helmholtz went a step further, and began to ask about the nature of the Sum's continuing source of heat. Tossing the possibility of chemical combustion aside, he considered the gravitational potential of the Sun itself, though it will be noted that he did not explicitly state here that the Sun was indeed capable of further contraction:

> If ... we adopt the very probable view, that the remarkably small density of so large a body is caused by its high temperature, and may become greater in time, it may be calculated that if the diameter of the sun were diminished only the tenthousandth part of its present length, by this act a sufficient quantity of heat would be generated to cover the total emission for 2100 years. Such a small change besides it would be difficult to 14

The obvious impossibility of detecting a change in the angular diameter of the Sun by some 18/100ths of a second of arc in 2100 years must have been a comfort to Helmholtz. Arguments of this type were to be used later by almost everyone who followed this general concept. The rates of contraction were to vary for one reason or another, but, in every case, the rate was beyond detectability.

As to the amount of time since the contraction of the nebulous solar cloud to its present state, and the amount of time left for the Sun to continue to shine, Helmholtz could say nothing. He did conclude that

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both were vastly larger than the total record of human history - of the order of hundreds of millions of years. As we shall see, Thomson was not to be so generous.

To sum up Helmholtz's ideas and to place them totally within the realm of the concept of gravitational contraction would be an oversimplification. It is evident that he was quite impressed with the idea of meteoric impact - the mechanism of collision, and could not bring himself to state explicitly that the Sun was indeed capable of contraction. Until this was possible, the meteoric impact source would remain attractive to many, the alternative being a confession of total ignorance on the subject.

#### The Meteoritic Theory of William Thomson

At a meeting of the British Association in 1853, William Thomson heard and became influenced by a paper given by J.J. Waterston on the meteoritic theory of the Sun's heat. James Joule, several years prior, had also put forth this view, and even though he and Thomson were friendly, Thomson apparently first heard of the idea at the 1853 meeting.<sup>15</sup>

Thomson's first major statement<sup>16</sup> was far more directed and focussed than was the presentation of Helmholtz. Of course, their audiences were quite different. While Helmholtz approached the subject from his discipline, physiology, and linked all forms of force, Thomson paid attention to the solar system, and was far more convinced of the meteoric origin of solar heat. At the outset of his paper he wished to consider three possibilities: (1) that the Sun is a heated body losing heat; (2) that its heat is due to chemical combustion; (3) that its source is meteoric. He clearly settled upon the third.

Further, Thomson's Sun was a "melted mass" capable of convection, where interior heat could be transferred to the surface for continued

radiative loss. Though he stated this as his opinion, he did not develop the idea of convective transport at the time. Nor did he seriously consider the possibility of the generation of heat due to gravitational compression.

After a short introduction reviewing Joule's earlier discussion of the amount of heat generated by a meteoritic body passing into the Earth's atmosphere<sup>17</sup> and Waterston's more recent application of this source of light and heat to the Sun, Thomson stated that:

> It is in fact not only proved to exist as a cause of solar heat, but it is the only one of all conceivable causes which we know to exist from independent evidence.<sup>18</sup>

He then calculated the rate of infall of meteoric matter needed to sustain the energy output of the Sun, based, with Helmholtz, upon Pouillet's 1838 value for the solar constant. Thomson calculated that from Waterston's model, where the meteoritic stones were supposed to hit the surface of the Sun directly, the solar surface would "... be covered to a depth of thirty feet in a year, if the density of the deposit is the same as that of water, which is a little less than the mean density of the Sun ....<sup>19</sup> In order to arrive at this calculation, Thomson merely determined the velocity a meteoritic mass would have if it fell from infinity to the Sun's surface. The velocity at the point of impact would be a measure of the kinetic energy, which was directly transferrable into a measure of heat energy.

Thomson's meteoritic source was within the Earth's orbit, and travelled in slowly decaying spiral orbits into the solar atmosphere. The actual conversion of kinetic to thermal energy came as the meteoric stones entered the solar atmosphere and vapourised. This slightly less efficient model required that Thomson suppose a meteoric influx twice the rate of Waterston's, thus causing the surface of the Sun to be

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covered to a depth of sixty feet per year.

From this, the Sun would increase in diameter by one mile in 88 years, and it would take 4000 years for the Sun to grow by a tenth of a second of arc.

Both Helmholtz and Thomson had a slowly cooling Earth and Sun, and both produced changes in the Sun which, over historical time, were not observable. While both utilised gravitation, and so produced "gravitational theories of solar heat" (as Thomson classed his own), they differed in their choice of the mechanism for conversion of gravitational potential into heat and light.

Though Thomson concluded that the "... source of energy from which solar heat is derived is undoubtedly meteoric  $\dots$ "<sup>20</sup> he still felt obliged to show the inadequacy of chemical sources. After an extensive discussion to this end, he stated, as his closing description of the energy source of the Sun -

> The store of energy for future sunlight is at present partly dynamical, that of the motions of these bodies round the Sun; and partly potential, that of their gravitation towards the Sun. This latter is gradually being spent, half against the resisting medium, and half in causing a continuous increase of the former.<sup>21</sup>

This ended his central text. In it is incorporated his concept of the proportion of potential available for heat and light, though at the time it was expressed in purely dynamical terms. Eventually, as it became modified to Helmholtz's mechanism, it would come to be known as Kelvin Contraction, and would survive as the modern description of a portion of a star's very early evolutionary life.

From the present amount of rotational energy possessed by the Sun, and an estimate of the amount of spiralling meteoritic impact that would be necessary to produce this rotation, the amount of time necessary to produce the present rotation was estimated, assuming that the rate of infall was sufficient to sustain the Sun's luminosity. The value came to be 25,000 years, and with various corrections, it increased to 32,000 years. Thomson estimated that this process could sustain the Sun for 300,000 years.

These were absurdly short time scales for the needs of uniformitarians like Lyell, who, in influencing others like George Scrope, a volcanologist, "... felt that he had heard Nature's refrain of 'Time!-Time!-Time!' echoing from all his geological observations ...."<sup>22</sup>

In the 1850s, this great disparity seemed not to be a loudly contested problem. Burchfield has noted that Thomson's 1854 papers attracted little attention<sup>23</sup> and so it would be presumed did Helmholtz's, even though the latter provided a vastly greater time scale at first.

In the late fifties, Thomson's attention was directed to other matters. But in 1859, an apparent confirmation of his meteoritic theory by Leverrier had been announced. Leverrier had been able to detect the precession of the perihelion of Mercury's orbit. Today, we see this as a predicted result of Einstein's general theory of relativity. But to Leverrier, something consisting of ponderable matter had to be the cause of the perturbation. It was either due to an intra-Mercurial planet (the legendary Vulcan) or to streaming meteoric matter passing across Mercury's orbit changing the mass distribution in that region of the solar system. Thomson seized upon the latter possibility as an argument for his hypothesis. "His elation, however, was short-lived", noted Burchfield. "The following year further calculations showed that a mass of meteors sufficient to supply the Sun's heat would create a much greater perturbation of Mercury's orbit than the one Leverrier discovered".<sup>24</sup>

In the next year or two, Thomson found time to rethink his theory, and by 1861 produced a revised form, which appeared in abstract in the <u>Reports of the British Association</u>.<sup>25</sup> In the transition from the

fifties to the sixties, Darwin's <u>Origin of Species</u> appeared, as did the work by Kirchhoff on spectrum analysis - specifically his application to the study of the Sun.

Darwin's significance partly lies in the realm of time. It greatly intensified the need for vast amounts of geological time, and hence, solar time. Thomson later directly faced this question of time, not in the direction of Lyell, but of Darwin. Kirchhoff's work demonstrated for the first time that the interior of the Sun could not be cold and dark, as the Wilson-Herschel sumspot model suggested. This eventually led to work by Faye and Lane on the consideration of the gaseous, and hence compressible nature of the solar interior.

Even though Kirchhoff's work led others to the conclusion that the interior of the Sun might be gaseous, Kirchhoff himself felt that it must be either a solid or liquid, since it produced a continuous spectrum. Later in the decade, however, Plücker, Frankland, Wüllner and others showed that highly compressed gases could also produce continuous emission.<sup>26</sup> To Agnes Clerke, Kirchhoff's work finally established that the interior of the Sun was hotter than its atmosphere.

We might well conclude that, by the time Thomson revised his meteoric theory in 1861-62, the general view of the internal constitution of the Sun had altered significantly, but the realisation that it contained a compressible gas had not developed into general thinking.

#### Thomson's 1862 Revision - "On the Age of the Sun's Heat"

Thomson's revised meteoric theory shifted the period of meteoric activity into the past, leaving as the heat source of the present Sum only its present store, to be dissipated by convection from the interior, followed by radiation into space. This process of cooling, which we saw in his earlier model and in the models of Helmholtz, became

the predominant direction of evolutionary thinking for at least thirty years.

After a short discussion of how Leverrier's observations of the advance of the perihelion of Mercury had caused him to rethink his model, Thomson summarised his new revision:

> All things considered, there seems little probability in the hypothesis that solar radiation is at present compensated, to any appreciable degree, by heat generated by meteors falling in; and, as it can be shown that no chemical theory is tenable, it must be concluded as most probable that the sun is at present merely an incandescent liquid mass cooling.<sup>27</sup>

Thomson then entered into a long discussion of the determination of the mean specific heat of the Sun. This, of course, depended upon the types of materials found there.<sup>28</sup> Thomson decided to place the mean specific heat of the Sun at a value somewhat less than water, but made most of his calculations of the cooling rate of the Sun on the assumption that it equalled that of water, as a convenient maximum limit that would minimise the cooling rate, and hence maximise the future lifetime of the Sun.

Thomson was to come to Helmholtz's conversion of potential into heat, but still saw the contraction of the solar globe <u>as a consequence</u> of cooling. Thus, he calculated that with a mean specific heat as that of water, "... there would be in 860 years a contraction of 1 per cent on the sum's diameter, which could scarcely have escaped detection by astronomical observation ..."<sup>29</sup> He reasoned that this amount of contraction had not occurred, because a 1 per cent contraction would have resulted in an apparent diameter for the Sun of 18 seconds of arc less since 1000 A.D. But the very act of contraction itself would produce heat, and thus alter the Sun's output of energy:

> Mutual gravitation between the different parts of the sun's contracting mass must do an amount of work, which cannot be calculated with

certainty, only because the law of the sun's interior density is not known.<sup>30</sup>

Now, following Helmholtz, Thomson stated that the work done in the contraction by one tenth per cent of a homogeneous solar sphere "... would, as Helmholtz showed, be equal to 20,000 times the mechanical equivalent of the amount of heat which Pouillet estimated to be radiated from the sun in a year ....<sup>31</sup>

But this was speculation without knowledge of the distribution of density within the interior of the Sun. Thomson was even unwilling to place definite upper and lower limits:

> We cannot, therefore, say whether the work actually done by mutual gravitation during a contraction of one-tenth per cent of the diameter, would be more or less than the equivalent of 20,000 years' heat; but we may regard it as most probably not many times more or less than this amount.<sup>32</sup>

Thomson felt that the work done by contraction could not increase its mechanical energy output, in response to its cooling. From experimental evidence, he concluded that mechanical energy always diminished, and "... that the sun always radiates away in heat something more than the Joule-equivalent of the work done on his contracting mass, by mutual gravitation of its parts ..."<sup>33</sup> Thus Thomson concluded that the Sum must give out more, "... or not greatly less ..." than the equivalent of 20,000 years of heat in contracting by 1.8 seconds of arc in that time - an amount certainly well below detectability. There was no question then that the Sum was cooling.

Turning to the possible process of cooling employed by the Sun, Thomson noted:

The interior temperature of the sun is probably far higher than that at his surface, because direct conduction can play no sensible part in the transference of heat between the inner and outer portions of his mass, and there must be an approximate convective equilibrium of heat throughout the whole, if the whole is fluid. That is to say, the temperatures, at different distances from the centre, must be approximately those which any portion of the substance, if carried from the centre to the surface, would acquire by expansion without loss or gain of heat.<sup>34</sup>

This statement can be regarded as his strongest yet in support of the presence of convection. He had already considered convection in 1854, but now, quite possibly bolstered by further evidence (from Kirchhoff) that the interior of the Sun was hotter than its exterior, the liquid model was settled upon, and hence convection became the dominant transport mechanism. Chandrasekhar<sup>35</sup> has noted that this discussion by Thomson in 1862 "... may properly be described as the real forerunner of the subsequent studies by Lane, Ritter, and Emden". Chandrasekhar also noted that "It is somewhat surprising that twentyfive years should have elapsed before Lord Kelvin applied his idea of convective equilibrium to the study of gaseous configurations".<sup>36</sup>

To Thomson then, the Sun was an incandescent liquid losing heat. His meteoric impact mechanism acted to a significant degree only in the past. Since Thomson spent considerable effort at reconciling this model with uniformitarian thinking, we might see it as a form of actualism following M.J.S. Rudwick's classification of variations of uniformitarian thought.<sup>37</sup> Here, a process seen today was far more active in the past.

As to time scales, of course Thomson was highly restrictive. He felt that "... There is no difficulty in accounting for 20,000,000 years' heat by the meteoric theory ..."<sup>38</sup> and placed 10,000,000 as a lower limit, and between 50 and 100,000,000 as an upper limit. But 500,000,000 was out of the question.

For quite some time, Thomson continued to favour his revised meteoric form, over the possibility that the Sun's chief source lay in its conversion of potential energy into heat. Quite possibly, his eventual conversion to Helmholtz's view was in part due to the growing belief that the Sum was gaseous. Spencer had suggested this as early as 1858, but Spencer could hardly have excited Thomson, or influenced him at the time. In the sixties, due to the work of Faye, and then Lane, the idea began to win adherents.

### Work in the 1860s on the Internal Constitution of the Sun

H. Faye<sup>39</sup> seems to be credited more than others with the first extensive discussion of the gaseous nature of the solar interior, though we have already noted that Spencer had this idea in 1858. Clerke notes that Faye's was the first "... coherent scheme of the solar constitution covering the whole range of new discovery ...."<sup>40</sup> The new discoveries were spectrum analysis and Carrington's observations of the differential rotation of the Sun.

Faye discussed how gases under great pressure and temperature could remain in the gaseous state, based upon the physical experiments by Cagniard de la Tour in 1822 which showed that under such conditions, the density of the gas could rise, but it would still behave as a gas. This conclusion was later greatly aided by Andrews' study of the critical point which had commenced earlier in the decade, and drew much attention from Spencer. Andrews' work culminated in 1869.

Faye saw the life history of the Sun as a cooling process. Clerke noted this, and Spencer, too, had this opinion about Faye's concept. As Spencer understood Faye, "... he looks forward to the future formation of a liquid film a the surface of a star7 as an event that will rapidly be followed by extinction ..."<sup>41</sup> Spencer, however, thought that this thin liquid 'crust', formed when sufficient heat had been extracted from the solar interior by convection and surface radiation,

would not be strong enough to contain what heat remained. Spencer thus modified Faye's model, after noting his own priority in the idea, to have the liquid film form earlier in solar life somewhere beneath the visible photosphere, and stated that:

> ... extinction cannot result until, in the course of further aggregation, the gaseous nucleus has become so much further reduced, and the shell so much thickened, that the escape of heat generated is greatly retarded. 42

Spencer clearly regarded the future state of the Sun as an "... advanced state of concentration  $\dots^{43}$  and he prefers his own speculation on the simple basis that his thicker shell would be more likely to contain the residual solar heat. This might suggest a model not unlike that thought of for the cooling of planetary bodies over a shorter time-scale.

We now turn to the work of J. Homer Lane, who had a developing interest in the problem of the solar temperature and constitution in the sixties, and was influenced by Faye.

#### J. Homer Lane

Jonathan Homer Lane, a quiet frequenter of the scientific circle in Washington D.C., (centred around Newcomb, Joseph Henry and Benjamin Pierce) had a long interest in the application of thermodynamic principles to the attainment of extreme cold.

Lane was clearly stimulated by the ideas of Joule, Mayer, and Thomson in the late forties and fifties, and was much influenced by the lectures of Espy on convection in the Earth's atmosphere.<sup>44</sup> Lane's letters at Yale and at the National Archives attest to his long interest in astronomical problems. At first, these were observations of meteor trails (a natural for an undergraduate at Yale during mid-century) and aurorae, and calculating orbits. His later interests in astronomy seem to be derived from his interests in the behaviour of gases under compression and rarefaction. In 1857, he left his position at the Patent Office after a political upheaval of its staff, and during the next decade, his life remains obscure, though it is evident from his papers that he was violently opposed to the sessionists and, at the same time, attempted unsuccessfully to design and market devices for making ice and transmitting military signals. Lane's evident frustration and poverty might have caused him to return to his pure interests in astronomy during this period. In the late fifties, he daydreamed of the construction of mammoth telescopes, sent aloft into the Earth's atmosphere borne by huge balloons.

The first indication of his application of convection theory to the determination of the constitution of the Sun is in 'Item 70' of his National Archives collection. This small collection of notes and computations has the following as a heading in Lane's hand:

... The within formulae were written down about the year 1863 (perhaps earlier) in considering the credibility of the sun being a gaseous body, sustaining its heat by the descent of its mass in cooling, and keeping up by its circulation a continual precipitation of  $\sqrt{\text{carbon?}}$  vapour in the photosphere, and the continual re-vaporization of the carbon  $\sqrt{?}$  in the interior, after the philosophy of terrestrial storms as explained by Espy.

<u>Conclusion</u>: it seemed evident the Sun's gaseous constitution could not be credibly referred to the known  $\boxed{?7}$  laws of the gases so far as they are known.

J.H. Lane, May, 1867

The few legible and readable passages in the notes contained within this material indicate that Lane considered the Sun as a gaseous sphere sustaining heat by contraction and proceeded almost exactly as he did in his later publication on the matter in 1869-70, except for changes in notation and a few sketchy additions. In both he considered the equilibrium between the forces of gravitation and differential elasticity upon a thin layer of gas within the Sum. His object of study was the determination of the degree of, or divergence from, equilibrium at each point within the Sum as a function of composition, density, and temperature - the primary factors which entered into the determination of what he called 'elasticity' which we would today, utilising kinetic theory (Lane still used 'caloric'), discuss as gas pressure.

We can see what Lane's conclusions were in his first reconnaissance of the problem, which we will date at 1863. He assumed very high temperatures and low densities in the solar photosphere, as he later explained in his published account. We now turn to this account and review the influences upon him which culminated in his 1869 discussion. We will then review his conclusions, and their effect upon future work by Thomson and others.

First, the various dates are important to note. 1863 was after Thomson's revision of his meteoric theory and after Kirchhoff, but before Faye. 1867 was clearly after Faye's 1865 discussions in the <u>Comptes Rendus</u>, and their announcements (or popularisations) in <u>The Reader</u>. This date was also recalled by Newcomb in his <u>Reminiscences</u>, who had an acquaintance with Lane:

> Among the attendants at the meetings of the Scientific Club <u>for</u> Washington, DC7 was an odd-looking and odd-mannered little man, rather intellectual in appearance, who listened attentively to what others said, but who, so far as I noticed, never said a word himself. Up to the time of which I am speaking, I did not even know his name, as there was nothing but his oddity to excite any interest in him.<sup>45</sup>

Newcomb continued, however, with an extensive recollection of a meeting in 1867 where he, Lane, and a Mr. W.B. Taylor (later of the Smithsonian) walked near the Smithsonian grounds and discussed a recent article on the constitution of the Sun which had appeared in <u>The Reader</u>. Newcomb recalled:

The view maintained was that the sun was not a molten liquid, as had generally been supposed up to that time, but a mass of incandescent gas, perhaps condensed at its outer surface, so as to form a sort of immense bubble. I had never before heard of the theory, but it was so plausible that there could be no difficulty in accepting it. So, as we wended our way through the Smithsonian grounds, I explained the theory to my companions in that ex cathedra style which one is apt to assume in setting forth a new idea to people who know little or nothing of the subject. My talk was mainly designed for Mr. Taylor, because I did not suppose the little man would take any interest in I was, therefore, much astonished when, at a it. certain point, he challenged, in quite a decisive tone, the correctness of one of my propositions. In a rather more modest way, I tried to maintain my ground, but was quite silenced by the little man informing us that he had investigated the whole subject, and found so and so - different from what I had been laying down. 46

It is clear from what follows that Lane's interest was stimulated by Faye's work, and that his opening paragraphs to his 1869 address before the National Academy of Sciences meeting in Washington (April 13-16) give some glimpse to Lane's recollections of his walk through the Smithsonian grounds with Newcomb and Taylor.

Lane's descriptive introduction to his paper recalled Helmholtz's and Thomson's original hypotheses and Thomson's revision, and noted that he and Benjamin Pierce had previously used Helmholtz's hypothesis "... to account for the renewal of the heat radiated from the sum, by means of the mechanical power of the sum's mass descending toward his center ...  $\langle which 7 \rangle$  ... have shown that this provides a supply of heat far greater than it is possible to attribute to the meteoric theory of Prof. Wm. Thomson ... "<sup>47</sup> Lane also recalled that this work set him thinking about the possibility that the solar interior was transparent, and that the photosphere was a region of condensation of something like carbon, which, upon condensation, sank into the interior to revapourise - a circulatory process akin to that proposed by Espy. We see here the similarity with his comments in 1867. In his paper, he noted that Espy's ideas had been delivered in a lecture prior to 1850 which he had attended, but which had been generally neglected by the scientific community. His knowledge of Espy's discussion of convective transport provided the reason why "... in a time when the constitution of the sun was exciting so much discussion, that the above suggestions /convection in the sun7 should have occurred to myself before I became aware of the very similar, and in the main identical, views of Prof. Faye, put forth in the <u>Comptes</u> <u>Rendus</u> ....<sup>48</sup>

Lane's first calculations assumed a temperature and density for the solar photosphere, and then proceeded to determine if such a configuration behaved according to the gas laws. As noted, it appeared not to do so. Not totally dejected by this apparent failure, Lane still mentioned his work to Joseph Henry, secretary of the Smithsonian, who referred him to the <u>Comptes Rendus</u> and Faye's article.

Lane then noted how, after reading Faye, he felt that the theory was inadequate, and was not directly comparable to observation. He did not, however, try to modify Faye's work, until another acquaintance, Dr. Craig in the Surgeon General's office, independently came up with the same idea, also without knowledge of Faye's work. At this point, Lane again took up the problem, discussing it with Newcomb, the end of which (along with Newcomb's interest in the Nebular Hypothesis) was to revitalise his interest. Lane remarked:

> Not any longer relying on my first rough estimate based upon assumed high temperatures at the photosphere, the question was now inverted. Assuming the gaseous constitution, and assuming the laws expressed in Poisson's formulae, known to govern the constitution of gases at common temperatures and densities, what shall we find to be the temperatures and densities corresponding to the observed volume of the sun supposing it were composed of some known gas such as hydrogen, or supposing it to be composed of such a mixture of gases as would be represented by common air.<sup>49</sup>

In Lane's collected papers, the only discussion<sup>50</sup> within which he examined such situations did not indicate what assumptions were made originally concerning the temperature and density of the solar photosphere. In his revision, he constructed what today would be called a solar model; assuming a particular physical constitution, inserting appropriate physical variables, and testing its validity against observable surface conditions.

The formalism discussed by Lane has been reviewed by Chandrasekhar<sup>51</sup> who noted that "... when the crudeness of the then available data is considered, Lane's success in estimating  $\rho$  and T /The density and temperature at the solar surface7 at the surface of the sun is a remarkable achievement. It may thus be said that his paper has made him the author not only of the first investigation of the physical conditions in the solar atmosphere but also the first investigations on stellar interiors though the latter was not his primary concern ..."

But what of 'Lane's Law'? Here we must again turn to Newcomb, who, like ourselves, was awaiting a quantitative discussion of this law, as Lane had apparently described it to him in the Smithsonian garden:

> After the paper in question appeared, I called Mr. Lane's attention to the fact that I did not find any statement of the theorem which he had mentioned to me to be contained in it. He admitted that it was contained in it only impliedly, and proceeded to give me a very brief and simple demonstration.<sup>52</sup>

Actually, Newcomb admitted that he could not recall if the theorem was really discussed on that day in 1867, or whether he learned about it later, as might be implied from Lane's remarks. In any event, Chandrasekhar discussed how one might derive Lane's law from the text of his 1869 paper:

Lane must have derived the law associated with his name essentially from an argument involving the homology invariance of the equilibrium configurations built on the law  $P_{\alpha \ \rho} = \frac{1+(1/n).^{53}}{P_{\alpha \ \rho}}$ 

Lane's Law, and his general discussion of convective equilibrium, most certainly required homology, or the restriction that with any general expansion or contraction of the body, the physical parameters density, temperature, and pressure - may change their absolute values, but not their <u>relative</u> distributions. This is the same as saying that the mass distribution within the gaseous sphere remains constant.

The problem, as Lane attacked it, was to take the solar mass and radius, with an assigned ratio of specific heats,  $\gamma$ , and to calculate the temperature, pressure and density, as a function of either mass or, in his case, radius. This fundamental technique was retained for over sixty years, being regarded by Eddington in 1926 as the usual mode for the solution of models describing stellar interiors in convective equilibrium.<sup>54</sup>

Today, we find that the ratio of specific heats favoured by Lane ( $\gamma = 5/3$ ) described the condition where half the gravitational energy of contraction is turned into radiation and the other half into heat. This is known as 'Kelvin-contraction', and is still believed to be at least partially valid in some pre-main sequence phases of stellar evolution.

Though there seems little question that Lane actually did derive the equations that today bear his name, it is interesting to examine his notes, collected in the National Archives, for such evidence. Since these notes are little better than fragmentary, it is difficult to determine the page order, except for a defining page discussing terminology and the basic theme. On a subsequent page, we find:

$$p = C s^{(k/k-1)}; \quad dp = (k/k-1) C s^{(1/k-1)} ds$$
$$ds = (k-1)/k \cdot C^{(-1)} s^{(-1/k-1)} dp$$
and
$$ds = (-1)(k-1/k) \cdot R^2/M \cdot C^{(-1)} \cdot n/r^2 \cdot s^{(-1/k-1)} dr$$
<sup>55</sup>

(after various illegible substitutions for the variation of pressure with depth), where r is the radius vector of any point within the Sun; m is the mass of all matter within that point, r; s is the temperature of the same point; p is the 'elastic pressure' at the same point, r; and k is the ratio of specific heats. If the point in question is at r = R, then the variables r, R, m, M drop out. He thus began from the equation of a polytrope, with arbitrary constant C, and progressed to a point where he was able to relate change in temperature to change in radius; or, as it is commonly described:  $r \cdot T(r) = constant$ .

Newcomb noted that after Lane presented his paper, and its subsequent publication, nothing happened:

So the matter stood, until the centennial year, 1876, when Sir William Thomson paid a visit to this country. I passed a very pleasant evening with him at the Smithsonian Institution, engaged in a discussion, some points of which he afterwards mentioned in an address to the British Association. Among other matters, I mentioned this law, originating with Mr. J. Homer Lane. He did not think it could be well founded, and when I attempted to reproduce Mr. Lane's verbal demonstration, I found myself unable to do so. I told him I felt quite sure about the matter, and would write to him on the subject. When I again met Mr. Lane, I told him of my difficulty, and asked him to repeat the demonstration. He did so at once, and I sent it off to Sir William. The latter immediately accepted the result, and published a paper on the subject, in which the theorem was made public for the first time.56

Thomson's first explicit proof of the homology theorem did not appear until 1887.<sup>57</sup> The immediacy recalled by Newcomb might have been a slip (as we shall see in discussing the 1887 paper), or a contraction. In any event, during the period between 1870 and 1887, Newcomb discussed the apparently 'paradoxical' results of Lane's law, without mentioning Lane explicitly.

> It may seem a paradoxical conclusion that the cooling of a body may cause it to become hotter. This indeed is true only when we suppose the interior to be gaseous, and not solid or liquid. It is, however, proved by theory that this law holds for gaseous masses.

He then proceeded to provide, in small print, a description of Lane's Law (still without acknowledgment):

> If a spherical mass of gas be condensed to one half the primitive diameter, the central attraction upon any part of its mass will be increased fourfold, while the surfaces subjected to this attraction will be reduced to one-fourth. Hence the pressure per unit of surface will be augmented sixteen times, while the density will be increased but eight times. If the elastic and the gravitating forces were in equilibrium in the original condition of the mass, the temperature must be doubled in order that they may still be in equilibrium when the diameter is reduced to one-half.

If, however, the primitive body is originally solid or liquid, or if, in the course of time, it becomes so, then this law ceases to hold, and radiation of heat produces a lowering of the temperature of the body, which progressively continues until it is finally reduced to the temperature of surrounding space.<sup>59</sup>

Newcomb could say nothing of the present state of the solar interior, but he put an upper limit on the present conditions of heat of some 10,000,000 years. A total age estimate for the Sum to contract from 'infinity' to a point where it cooled in a solid state to the temperature of space was 18,000,000 years, though he noted that these values were crude estimates only.

### State of Awareness in the 1870s and 1880s of Contraction as Source of Solar Heat

We are already aware of Newcomb's opinion. C.A. Young, in an early edition of The Sun (which was written in the 1870s) noted that:

> Two different theories have been proposed, which are probably both true to some extent. One of them finds the chief source of the solar heat in the impact of meteoric matter, the other in the slow contraction of the sun. $^{60}$

Young felt that the meteoric theory quite possibly could account for at least some of the solar heat, and felt that the chief problem with the contraction theory was that "... there is as yet no direct evidence whatever that the sun is really shrinking ..."
Young expressed the idea of homologous contraction in the following way:

In this process of contraction, each particle at the surface moves inward by an amount equal to the whole diminution of the solar radius, while a particle below the surface moves less, and under a diminished gravitating force; but every particle in the whole mass of the sun, excepting only that at the exact center of the globe, contributes something to the evolution of heat.<sup>61</sup>

Young emphasised his concern that the Helmholtz contraction rate was so slight that it rendered the contraction unobservable. Helmholtz's rate was sufficient to maintain the solar temperature, but Young wondered what would happen if the rate exceeded this amount. He thus turned to Lane, and examined what would happen if the Sun were "wholly gaseous". Young considered Lane's ideas curious and, at first sight, paradoxical, but gave them an adequate descriptive treatment. To Young, if the Sun was indeed gaseous, it would contract and heat until "... the density becomes so great that the ordinary laws of gaseous expansion reach their limit and condensation into the liquid form begins ..."<sup>62</sup> He continued:

> The sun seems to have arrived at this point, if indeed it were ever wholly gaseous, which is questionable. At any rate, so far as we can now make out, the exterior portion - i.e., the photosphere - appears to be a shell of cloudy matter, precipitated from the vapors which make up the principal mass, and the progressive contraction, if it is indeed a fact, must result in a continual thickening of this shell and the increase of the cloud-like portion of the solar mass.

Young's opinion was quite common at the time: that, if Lane's criterion held at some point in the life history of the Sun, it was in its past history. In the same year (1883), Ritter was to come to this conclusion, and eventually both Lockyer and Huggins were also to come to this, though from characteristically different lines of reasoning. It is significant then that the supposed heating phase, either through meteoric bombardment or gravitational contraction, was believed to be a past event or phase, and not applicable to the present Sun.

In 1879 J. Janssen reviewed the state of knowledge about the Sun in a paper entitled "Notes on Recent Progress in Solar Physics".<sup>63</sup> Janssen referred indirectly to the ideas of Helmholtz and Lane:

> A law which is based on the fundamental properties of matter states that the entire mass of the Sun can be called upon to support its radiating power.<sup>64</sup>

To Janssen, the solar interior was gaseous (for he was a friend of Faye) and nebulae were cold. Thus he stated, after noting that "... an increasing amount of evidence points to the nebular origin of the Sun ...":

How can one conceive of a nebula, having incandescent gases, which is able to create suns with cold nuclei? The condensation can only cause an increase in heat, and cannot diminish it.<sup>65</sup>

While there is some acknowledged ambiguity to this statement, it seems to be consistent with Lane's idea.

In 1883, both Arthur Schuster and August Ritter contributed important discussions. Schuster discussed the internal constitution of the Sun, on the basis that it is a gaseous body noting:

> It could not be otherwise, for the interior of the Sun cannot be permanently at a lower temperature than the surface, which we know to be sufficiently hot to volitalize some highly refractory metals.<sup>66</sup>

Schuster's Sun was in convective equilibrium and followed adiabatic conditions. This, believed Schuster, must be true to account for the Sun's low mean density, but, thus far, he believed, though "... Opinions such as these have been independently expressed in different parts of the world ... no one, the author thinks, has subjected them to the test of calculation ..."<sup>67</sup> Schuster, then, was not aware of Lane and Ritter. Further, he noted that it was "... rather curious that, as far as he is aware, the problem of the internal equilibrium of a gaseous gravitating mass has not as yet been discussed ..." Of great interest to our discussion was his closing statement:

One more interesting question, which he will mention only in this place, can be easily discussed by means of our equations. It is that which refers to the change of temperature and size of a gaseous body owing to the loss of heat by radiation.

It is enormously frustrating that he left it there, without any qualitative conclusion. Schuster did eventually discuss these evolutionary questions, in a reply to a paper by Lockyer in 1897.

#### The Work of August Ritter - 1883

In contrast to Schuster's sketch, Ritter provided a highly detailed discussion of the problem of a gas sphere in convective equilibrium under its own gravitation for a range of ratios of specific heats, and also paid considerable attention to the future of the Sun. His main period of work on this problem spanned the years 1878 to 1883 under the general title "On the Constitution of Gaseous Celestial Bodies" and was printed in <u>Wiedemann's Annalen der Physik</u>. Chandrasekhar has reviewed the main results of the more important of the 18 papers that comprised this great work, and our discussion will follow his review, except for the sixteenth paper in the series, which was first published in 1883, and republished (and translated) in the <u>Astrophysical Journal</u> in 1898.

It is Chandrasekhar's opinion that Ritter was unaware of Lane's work, and we have no doubt that this is true. Chandrasekhar discussed the relative significance of the two workers' contributions:

> Unlike Lane, Ritter was primarily interested in the equilibrium of stellar configurations, and his contribution to the formal mathematical theory is so great that such aspects of the theory of gaseous configurations built on the law  $P \alpha \rho (1+1/n)$ , as are commonly known are almost entirely due to Ritter.<sup>68</sup>

In the first nine papers, published between 1878 and 1880, Ritter

#### established the following:

- 1. "Lane's law" (independent of Lane) explicitly proved.
- 2. Derivation of the "Kelvin-Helmholtz" contraction time scale.
- 3. The adiabatic expansion and contraction of a gas sphere, providing an explicit mechanism for the variability found in stars. Here Ritter finds that the period of oscillation is inversely proportional to the mean density of the gas sphere.<sup>69</sup>
- 4. Detailed polytropic models calculated for n from 1 to 5.
- 5. The examination of what Chandrasekhar calls "composite configurations": models with incompressible cores and gaseous envelopes. Point source models, with singularities at their origins were also considered.

This last study, in his ninth paper in 1880, led directly to his sixteenth and seventeenth papers, which considered what is today referred to as the "giant-dwarf" theory of stellar evolution, later discussed by Lockyer and Russell. We now turn to the sixteenth paper.

The reprinted paper in the <u>Ap.J.</u> in 1898 had a long editorial forward discussing its significance. It was chosen because of "... its bearing on the question of stellar classification ..." - a most important statement considering our ultimate theme.<sup>70</sup> The editorial also noted:

> It will be seen that the author, reasoning on purely physical grounds and assumptions, arrives at a classification which includes stars with rising temperatures as well as those with falling temperatures. Thus he infers that the red stars of class IIIa have not reached the acme of their brilliance, and that those of class IIIb (Secchi's fourth type) are nearing extinction after having run their course of stellar evolution.

The note continued by introducing Ritter's basic concepts, including his use of the term "Zustandslinie" (translated as "condition line") expressing, for a body in convective equilibrium, the conditions under which it behaves as a perfect gas.

Ritter's intention was to determine the nature of the adiabatic

.....

curve that represents the ideal gas condition. Using Stefan's recent law for heat radiation emitted as a function of temperature, Ritter compared the temperatures for various models with different ratios of specific heats, plotted against decreasing radius, and also plotted the heat radiation per unit surface and the total quantities of heat radiated per unit time.

He concluded that a decrease in total luminosity for a star could occur while both the interior and surface of the star heated. Turning directly to the question of the Sun, he added:

> The Sun is at present in a condition to elude an answer to the inquiry whether its mean temperature is still increasing or already decreasing. In any case, however, the Sun has already attained a condition of density such that the fulfillment of the above assumption of a comparatively slight depth of the radiating surface layer is hardly to be doubted. From the results of the above investigation we may therefore reason with a high degree of probability that under any circumstances - whether the Sun's temperature be rising or decreasing - the total yearly heat radiation from the Sun is at present already on the decline, therefore that the Sun is to be considered a star that has already entered the stage of diminishing light intensity.<sup>71</sup>

Ritter discussed time scales for contraction, establishing the potential of a gas sphere in adiabatic equilibrium, and the potential derived as a result of the mechanical work done by gravitation in bringing the gas sphere from an infinite radius to that of the present Sun. With various ratios of specific heats (from 1.14 to 5/3) he came out with contraction times between 5.5 and 6.5 million years for the time to contract from a sphere the size of the Earth's orbit to its present radius. He knew that these values were approximations only, since no account of the effect of increased absorption with condensation had been made. Also, at the time, the value of the solar constant was in question since Pouillet's classic work was being retested by Langley after an extensive study atop Nount Whitney in 1881. Langley's larger value

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reduced the contraction time (for  $\gamma = 5/3$ ) to 4.3 million years.

Ritter was painfully aware of the many assumptions that were being made. These included spherical symmetry and the gaseous condition throughout all contraction phases, the lack of rotation, and the lack of the effect of gaseous condensation upon the emissivity and temperature of the radiating layer. Nevertheless, he was convinced enough of the general value derived to state in conclusion:

... it seems permissible to conclude from the above investigation that the actual age of the Earth must be far less than the estimates of some geologists, who place it at hundreds of millions of years  $\dots$ <sup>72</sup>

# The Extension of Ritter's Discussion to the White and Red Stars

Ritter was the first to extend these theoretical discussions beyond the case of the Sun. Assuming absorption independent of temperature, he derived the following proposition from a comparative study of two models:

## The surface temperatures of two stars of equal densities are to each other nearly as the square roots of their masses.<sup>73</sup>

This proposition was then used to explore the question: "Under what circumstances can the surface temperature of a star exceed the present surface temperature of the Sun?" He assumed similar physical and chemical properties for the material composing the Sum and stars.

Ritter found that a small surface temperature rise in the Sun would require a very large increase in its central density "... beyond the range of probability ..."

> It is therefore decidedly improbable that the surface temperature of the Sun will increase significantly in the future. We may perhaps with greater probability assume that it is already on a slow decline.<sup>74</sup>

Raising his conclusion to the level of a proposition, Ritter stated:

# The surface temperature of the Sun was never very much higher, and in future can never be very much higher than it is at present.

From these two propositions, Ritter noted that for a star's temperature to be higher than that of the Sun, its mass would have to be greater. He acknowledged that some stars of one solar mass might not be as evolved, and might therefore have slightly higher temperatures. But this could not be the only reason why white stars, with decidedly higher temperatures, were observed as frequently as they seemed to be. He thus concluded that on the whole, "The masses of the white stars are greater than the mass of the Sun".<sup>75</sup> This relationship remains correct today.

Turning to the red stars, he admitted quite another situation. First, taking the solar mass and expanding it into a sphere 100,000 times the solar value, and assuming adiabatic equilibrium, the density would be extremely low, and a corresponding low degree of absorption would be present. In a gas sphere such as this, contraction would increase interior heat enough to cause the radiation of the sphere to increase. At some point in the contraction process, a central nucleus of sufficient density to impede radiation will occur and the mean density of the gas sphere will eventually come to a point where deviations from the gas laws become important enough to make the heat output of the star decrease. Ritter thus identified a period of increasing luminosity and temperature, and a period of decreasing luminosity and temperature and specified that "... it follows that at some intermediate point the quantity of heat radiated per unit of time must have reached a maximum".<sup>76</sup>

Ritter then referred to a portion of his paper that for some reason was not present in the translation, where he showed that "... a star always reaches the culmination of its heat radiation earlier than the culmination of its surface temperature ....<sup>77</sup> He placed the Sun past the first point (which possibly occurred when the Sum was still approximately the size of the Earth's orbit) and possibly at or close to the second culmination point of maximum temperature. Examining the possible combinations of colour and brightness during the life cycle of a star, Ritter then noted:

> A star which emits bluish-white light at the culmination of its surface temperature must have appeared red at an earlier epoch, when its surface temperature was far below its maximum value; and similarly it will necessarily appear red once more at a later time, when after passing its maximum the surface temperature has again fallen considerably.<sup>78</sup>

Thus, there were two classes of red stars, and quite possibly, for those which were able to achieve the white stage, the maximum period of heat radiation (or luminosity) was when they were still red in colour. Ritter assigned his evolutionary sequence various positions according to the spectral sequence identified by H.C. Vogel in 1874:

> If we divide the stars into three classes according to their surface temperatures, assigning with Vogel the white stars to the first class, the yellow to the second, and the red stars to the third class, we should have to distinguish between two groups or subdivisions within the third class. According to the theory here advanced, we should put in the first subdivision those red stars whose surface temperatures have not yet reached the culmination point; in the second all those red stars whose surface temperatures have already passed their culmination. Since we may assume ... that a star always reaches its luminous culmination much earlier than that of its surface temperature, the stars belonging to the first subdivision will necessarily possess predominently slight density and high luminous intensity, while those of the second subclass will have great density and slight luminous intensity.<sup>79</sup>

Ritter continued this idea using Vogel's IIIa and IIIb classifications. He believed that with condensation and an increase in density, increasingly complex combinations of chemical elements and compounds would occur. The anomalous banded spectra of Vogel's Class IIIb might well be identical to his older and denser class of red stars. That no stars of this subclass brighter than fifth magnitude had been detected seemed to be significant evidence for his supposition, since many stars of class IIIa were brighter. He did not make any definite statements regarding this possible confirmation, and preferred to leave confirmation open for further spectrum analysis, which was eventually to come, of course, in Antonia Maury's classification scheme, as interpreted by Hertzsprung.

He also believed that a completely definitive system should account for subclassifications of the 1st and 2nd types. As to how to distinguish between the two subclassifications, he again provided no answer but noted:

> In any case we may infer that the nature of the spectrum of a star depends not only upon the temperature but also upon the density of its surface layer, which increases with age; and that the spectra of two stars of the same surface temperature can be different if their surface layers have unequal densities.<sup>80</sup>

Ritter also saw a mass and age dependence for spectra, but left the entire issue open to further spectroscopic research.

As his last topic in this paper, he considered the apparent lack of red stars in the sky. To do this, he adjusted the integration limits on his equations expressing contraction times, to determine the relative amounts of time a contracting gas sphere spent in its various stages. He had already discussed briefly the fact that for a gas sphere of solar mass and dimensions equal to the Earth's orbit, the amount of time needed to contract to twice the present solar diameter was less than the amount of time needed to contract the rest of the way. Taking r as the present solar radius, he found that to go from 215r to 2r would take only 1.4 million years (where 215r represents the size of the Earth's orbit) while to go from 2r to r would be about 5.1 million, and from 2r to  $\frac{1}{2}$ r (half the present solar radius) would be some 28.5 million

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years. Obviously, the amount of time spent in the early red phase was very small compared to the amount of time spent in stages not too dissimilar from the present Sun. From this he concluded:

> The point of culmination of heat radiation of a star represents a state of relatively rapid transition; the point of culmination of surface temperature, however, represents a state in which the star remains for a comparatively long time.<sup>81</sup>

This was based simply upon the fact that the culmination point of heat comes well before the culmination point of temperature. Thus, we see many bright white and bluish-white stars, and comparatively many bright yellows, but very few bright reds.

Ritter actually classed stars into three groups based upon relative age. In the first, evolution proceeded slowly. This was the nebular state. Once contraction began, evolution would become rapid the star would rise to maximum brightness and then to maximum temperature. This was the second phase. The third phase began when high densities were reached and both luminosity and temperature decreased. This was a slow phase also. Ritter called these classes A, B and C, for want of any better names. It was tacitly assumed that stars in class A were not observable as such, but that, from the law of probability, one would expect to see fewer B class stars than C, and that B would be small compared to the whole. His B class included Vogel's IIIa stars and C comprised I, II, IIIb on Vogel's system.

The paucity of bright red stars was explained in this manner. But Ritter also believed that a star of small mass evolved more rapidly than a star of high mass. With this, if the stellar system was still young, only stars with the smallest masses would have evolved to the IIIb stage. Ritter does not specify what fraction of all stars were of "low" mass however.

Within this paper we see the basis for Ritter's concept that

not all stars follow the same evolutionary path. From his proposition that the Sun could never have been much hotter than it is at present, one might infer that not all stars were massive enough to reach the whitestar stage. Though he did not explicitly discuss this "distillation" agent for his version of a double valued temperature arch, it is still quite clear that one could easily come to it from his stated proposition, which, we might add, was revived by Russell after 1910 to account for the distribution of mass observed down the Main Sequence.

In a later part of our discussion it will be shown that Huggins, partly to spite Lockyer in the mid-nineties, became quite anxious that Ritter's work be appreciated so that Lockyer's ideas might be seen to be less than original. To this end he drew Hale's attention to these papers by Ritter, and in consequence, the sixteenth in the series, the one we have been able to review in detail, was translated and reprinted in the <u>Astrophysical Journal</u>.

As for nineteenth century citations to Ritter's work, in addition to Huggins' zealous remarks and Hale's obvious interest on the part of the reprint, we find no mention in the first (1885) and second (1887) editions of Agnes Clerke's <u>History</u>. She apparently was not aware of Lane either. C.A. Young's <u>The Sun</u> (1883 edition) discusses Lane but not Ritter. His 1896 edition of <u>The Sun</u> gave an identical treatment. Clerke's <u>System of the Stars</u> (1890) mentions neither Lane, Ritter <u>nor</u> Kelvin (Wm. Thomson) but does discuss the double valued temperature line of evolution. Frost's translation of Scheiner, <u>Astronomical Spectroscopy</u> (1898) is equally silent. Through 1895, the editions of Young's <u>General</u> <u>Astronomy</u> adhered to Helmholtz's theory, treating Lane's ideas as an interesting anomaly, with no mention of Ritter. Newcomb and Holden, of course, cited Lane as did R.S. Ball in <u>The Story of the Heavens</u> in 1886.

George Darwin was well aware of Ritter's work in 1889 and was

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well disposed to its applicability, utilising what today is referred to as "Kelvin-contraction" to describe a contracting mass of perfectly elastic meteors in order to reconcile Lockyer's meteoritic hypothesis with the nebular hypothesis.<sup>82</sup> This discussion will be continued later, after we discuss Thomson's revived interest in 1887.

### Thomson's 1887 Address: "On the Sun's Heat"

After preliminary comments on the constancy of the Sun's heat, Thomson remarked that his hypothesis, in the form presented by Helmholtz "... may be accepted as having the highest degree of scientific probability that can be assigned to any assumption regarding actions of prehistoric times".<sup>83</sup> The "essential principle" of this meteoritic hypothesis was:

> ... at some period of time, long past, the Sun's initial heat was generated by the collisions of pieces of matter gravitationally attracted together from distant space to build up his present mass; and shrinkage due to cooling gives, through the work done by the mutual gravitation of all parts of the shrinking mass, the vast heat-storage capacity in virtue of which the cooling has been, and continues to be, so slow.

Thomson pursued Lane's apparently "paradoxical" result that as a result of contraction, the Sun was actually heating, and not cooling, and footnoted his discussion here to say:

> The 'paradox' referred to here, is, as I now find, merely a misstatement (faulty and manifestly paradoxical through the omission of an essential condition) of an astonishing and most important conclusion of a paper by J. Homer Lane.

Apparently, at the time of this note (February 21, 1887) Thomson had not yet actually read Lane's paper, for he referred to Newcomb's <u>Popular Astronomy</u> and R.S. Ball's <u>Story of the Heavens</u> for the resolution of the apparent paradox of a body gaining heat through radiation. The paradox, as Thomson originally saw it, was caused by not stating that an essential condition of this heating process was that the gas sphere behave ideally. By inserting a phrase to account for this, Thomson considered the paradox resolved. He thus proceeded to discuss as did Ball and Newcomb, that the Sun is cooling:

> The truth is, that it is because the sun is becoming less hot <u>in places of equal density</u>, that his mass is allowed to yield gradually under the condensing tendency of gravity; and thus from age to age cooling and condensation go on together.<sup>84</sup>

It is interesting to recall Newcomb's recollection of how he and Thomson discussed Lane's ideas. Newcomb could not reproduce Lane's derivations in 1876 and Thomson remained unconvinced until he received from Lane (supposedly through Newcomb) the full argument. But in another paper by Thomson, written on the heels of his Royal Institution address, Thomson noted that one day after he provided the footnote about Lane's work quoted above, he received another letter from Newcomb with what was obviously a copy of Lane's paper. Having read the paper, Thomson had to note that "... precisely the same problem as that of my article is very powerfully dealt with, mathematically and practically ....<sup>85</sup> Very strangely, Thomson also noted that this letter from Newcomb "called his attention" to the paper. He certainly had been aware of its existence before, in 1876 if we are to believe Newcomb's Reminiscences. Most definitely, from Thomson's own 21 February note in his Royal Institution Address, we know that he was aware of Lane's work from Newcomb's and Ball's popular books. When Thomson actually did become aware of Lane's work thus remains an open point. Either Newcomb's "Reminiscences" were poor, or it took Newcomb some 11 years to send Lane's paper to Thomson!

In any event, by 1887, Thomson had re-discovered Lane's discussion of gas spheres in convective equilibrium and had proven that the radial distribution of the state variables could be represented by a differential equation which was soluble for mass, density and temperature, what we today would call a polytrope. His paper in the <u>Philosophical</u> Magazine, stimulated by an examination question created by his friend and associate P.G. Tait, was developed to back up his address to the Royal Institution.<sup>86</sup>

Thomson, unlike Newcomb, Young, Ball and a few others, was at first reluctant to discuss the possibility that some stars in the sky might still satisfy the gas laws, and thus be heating. Ball had noted that this was quite possible, and that, in the Sun, partial heating could very well be counteracting the cooling process, thus causing the temperature of the Sun to remain static. But as we shall now see, Thomson did come to some conclusion about the earlier history of the Sun, and past changes in its temperature.

Thomson provided two propositions as explanation for the light and heat of the Sun. The first was the condition of convection which replenished the surface layers of the Sun cooled by radiation. The second was that the chief source of energy for the Sun was gravitational contraction. He discussed time scales based upon the second proposition alone. The main limitations on past and future time scales was the assignment of density. In past history, 15,000,000 years of time was available for the contraction of the Sun from four times its present radius to its present value. In future history, 20,000,000 years of solar heat (based upon its present rate) radiation would cause a contraction of the solar disc to half its present value. Thouson considered this an upper limit since at that point the mean solar density would be over 11 times that of water, "... or just about the density of lead, /which/ ... is probably too great to allow the free shrinkage ... of a cooling gas to be still continued without obstruction through overcrowding of the molecule ....<sup>87</sup>

With Ritter and Lane, Thomson quite clearly felt hampered by the lack of reasonable knowledge of the distribution of density within the Sun, the value of the solar constant, and the correct assignment of a ratio of specific heats to the solar interior. He also studied models with values ranging from 1.4 to 5/3.

With the ratio equal to 1.41<sup>88</sup> Thomson found that the central density of the Sun was less than 31 times that of water, and considerably greater than its mean density of 1.4 times that of water. But he also knew that a variation in the ratio of specific heats could greatly alter the central density, which made it very difficult to determine whether the Sun was becoming hotter or colder.

This question clearly was at the centre of his interest, and that of many who followed such problems. A portion of Thomson's address was reprinted in America under the title "Is the Sun becoming Hotter or Colder".<sup>89</sup> Considering the possible density range of matter in the solar interior, Thomson could not directly answer this question, but stated:

> The question, Is the sun becoming colder or hotter? is an extremely complicated one, and in fact, either to put it or to answer it is a paradox, unless we define exactly where the temperature is to be reckoned.<sup>90</sup>

Thomson wished to define the temperature change as that reckoned at a point within the Sun where the density was some specified value. Thus, with contraction, one would have to find the new temperature at the "homologous" point within the star that had the same density as its counterpart in the star examined at the earlier epoch. These homologous points of density existed at different absolute values of the new and old radius of the star, which allowed Thomson to conclude:

> ... the distance inwards from the surface at which a constant density is to be found diminishes with shrinkage, and thus it may be that at constant depths inwards from the bounding surface the temperature is becoming higher and higher.

This could lead to a rise in temperature, even with a general loss of heat due to radiation from all the Sun's parts. It was this

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condition of homology that was missing from Thomson's first encounter with Lane's theory via Newcomb.

Thomson then answered the question posed concerning the future temperature of the Sun. He began by discussing the history of the Sun's central temperature.

Most certainly, millions of years ago, the Sun was "... wholly gaseous to the centre" and its temperature was therefore rising. Also, at some distant point in time, condensation might continue to the point where the centre of the Sun becomes solid, and hence, conduction being inadequate to allow for the dissipation of the heat build-up from gravitational shrinkage, the central temperature would also be increasing. But somewhere between these extremes is most probably where the Sun is at now, and that during this period,

> ... when the central parts have become so much condensed as to resist further condensation greatly more than according to the gaseous law of simple proportions, it seems to me certain that the early process of becoming warmer, which has been demonstrated by Lane, Newcombe sic and Ball, must cease, and that the central temperature must begin to diminish on account of the cooling by radiation from the surface, and the mixing of the cooled fluid throughout the interior.<sup>91</sup>

It is difficult to believe that Thomson had not read Ritter's work. Lane did not explicitly discuss homologous evolution of a convective gas sphere and Ritter did; the <u>Annalen</u> of Wiedemann were certainly known both in America and Britain, and it was common to see British papers in the journal. In fact, Ritter's sixteenth paper appeared just after one written by William Siemens in 1883. It was easy to acknowledge Lane, for he produced only one paper on the subject and left the field wide open. But Ritter surveyed almost every aspect of the field, and, as we have seen, created "... almost the entire foundation for the mathematical theory of stellar structure ..."<sup>92</sup> If anyone else wished to enter the field along those lines, it would be difficult to acknowledge Ritter and then claim any degree of priority or originality.

In Thomson's work we also see the embryonic temperature arch of Lockyer. Of course, Thomson did not emphasise observables, and was only speculating on the Sun. But his further discussion leads us closer and closer to Lockyer.

"Now we come to the most interesting part of our subject" continued Thomson, "the early history of the Sun".<sup>93</sup> Here he asked what was the original state of the matter comprising the Sun "... before it came together and became hot ..."<sup>94</sup> He considered Croll's mechanism of two massive colliding objects with velocities greater than what would be expected from a simple fall from infinity. Thomson provided immediate criticism and rejection on the basis of probabilities, and then progressed to his own scheme:

> Suppose now, still choosing a particular case to fix the ideas, that twenty-nine million cold, solid globes, each of about the same mass as the moon, and amounting in all to a total mass equal to the sun's, are scattered as uniformly as possible on a spherical surface of radius equal to one hundred times the radius of the earth's orbit, and that they are left absolutely at rest in that position.<sup>95</sup>

These masses would gravitate to their common centre, and through friction and the expenditure of mechanical energy in collision all become melted and incandescent producing a vastly heated mass "... of a few hundred thousand or a million degrees centigrade ..." which will not remain stable due to the great heat but explode outwards again into a sphere comparable to, but less than a radius of, some one-hundred times the radius of the Earth's orbit. Contraction will eventually dominate, after a period of instability, "... and the incandescent globe thus contracting and expanding alternately, in the course it may be of three or four hundred years, will settle to a radius of forty times the

radius of the earth's orbit ...,<sup>96</sup> Thomson noted here parenthetically that this value of the radius was "... 40 per cent of the radius of a spheric surface from which its ingredients must fall to their actual positions in the nebula to have the same kinetic energy as the nebula has ..." He was thus thinking of nebulae, and quite possibly of novae as their precursors.

This vastly extended globular mass was of extreme low density but would have, by his calculation, a central temperature of some 50,000 degrees C. After several million years, the mass would shrink and become our Sun, as a result of the radiation of heat. It would, however, possess no rotation, and Thomson did not specify the surface temperature of the original mass, or the direction of the temperature progression with shrinkage.

To account for the present rotation rate of the Sun, and, implicitly on the nebular hypothesis the present amount of angular momentum in the solar system, Thomson then revised his meteoritic model by giving each of his twenty-nine million moons a small degree of motion, "... making up in all an amount of moment of momentum about a certain axis, equal to the moment of momentum of the solar system ... or considerably greater than this, to allow for the effect of resisting medium ....<sup>97</sup>

This time, the initial collapse time would be the same, some 250 years, but now, well before all the masses reach their common centre, collisions will begin to occur through the increase of the individual random motions. This will first prolong the initial explosive heating phase, and second, will generally impart a rotation term to the collapsing cloud of melting moonlets. This rotational momentum is conserved, so, after the subsequent explosion of the cloud and its eventual settling to 40 A.U., the cloud will flatten and possibly eject rings of material,

in keeping with Laplace's hypothesis.

What do we see in Thomson's address that is similar to Lockyer's Meteoritic Hypothesis which was under development at the time? The meteoric origins were common: collisional heating appears in both. An initial explosive phase is more apparent in Thomson's scheme, as it is in Croll's. Further, while Thomson did not directly favour a double-valued temperature arch, and also dodged the question of the present trend of the solar temperature, it is evident that he considered the former quite seriously. As to the latter, he felt that the Sun was presently cooling. These are not significantly different from Lockyer's ideas, as presented simultaneously.

Another similar element of Thomson's work, better compared to that of George Darwin's of 1889, was that he saw the collisional process as being perfectly elastic. Darwin, in his attempt to reconcile Lockyer's hypothesis with the nebular hypothesis, did this too.

As to the differences, Thomson's scheme was clearly meteoritic only in its origins; while Lockyer's was chiefly meteoric until the attainment of maximum temperature around the B star stage (using present notation). Thomson's initial quick collapse of his meteoric cloud caused the total vaporisation of the bodies, and it is assumed that the resultant nebula was gaseous throughout.

Finally, Lockyer does not publicly acknowledge or discuss Thomson's meteoritic theory, but uses the meteoritic mechanism as an explanation of the spectra of stars and nebulae based upon his laboratory experimentation on meteoritic material. Lockyer clearly wanted to argue from the experimental-empirical side, though in correspondence circa 1893 with the editors of the <u>Philosophical Transactions</u>, (in responses to referees' comments on his papers), he did acknowledge Ritter's work, but felt that it must not be overly applied without better knowledge. By 1890 then, we might well conclude that almost all discussions of the evolution of the Sun's store of heat involved at some stage a meteoritic/collisional phase. Thomson, G. Darwin, Tait, Croll, Lockyer, Proctor, and many others must be included here. Lockyer, however, must be distinguished as providing a theory which employed meteoritic phenomena to a much greater degree than the others. Through the nineties, Ritter's work (which was not meteoric at all) became much better known as both his and Thomson's ideas were republished in American journals. The last part of our discussion then will follow events that transpired through the turn of the century, and our discussion will end approximately with Schuster's 1903 paper on stellar evolution.

## Arguments Arising from Further Consideration of the Lane/Ritter/Kelvin Model - 1890-1903

In 1891, William Huggins delivered the Presidential Address to the British Association at Cardiff<sup>98</sup> and his paper, "Celestial Spectroscopy" included mention of the work of Ritter, in connection with problems in stellar evolution. Huggins, through this period, was asking his friends, notably C.A. Young, for their opinions of the work of Ritter and Lane. In his address, however, he did not lay any emphasis upon the theory, but merely presented it as one amongst many for the course of evolution. While he mentioned Ritter by name, and provided a description of his view of the course of evolution which had a star passing through the red stage twice, first quite rapidly while on the rise, and the second for a more prolonged period of time, he added that "... Recently a similar evolutional order has been suggested, which is based upon the hypothesis that the nebulae and stars consist of colliding meteoric stones in different stages of condensation ..." without mentioning Lockyer by name. Later in his address, he mentioned Lane's law, but its inclusion appeared to be awkward and not in context.

Possibly, at the time, Huggins didn't exactly know what to do with this law. But by the late nineties, in preparing his <u>Atlas</u>, he was able to incorporate the law into an evolutionary scheme which had the stars <u>heating</u> with age from <u>blue to red</u>!

From the standpoint of Huggins' review, and descriptions given in texts already noted, it is safe to say that the general ideas put forth by Helmholtz, Thomson (now Lord Kelvin), Lane and Ritter were in common knowledge. In the latter part of the decade, however, a situation arose which caused many to ask for a general clarification of this line of work.

In the late nineties, Helmholtz's basic idea had found its way into lectures given at the University of Chicago, which prompted one of the students, Anne Sewell Young, to write a short note for <u>Popular</u> <u>Astronomy</u> deriving the relations that were needed to see how the Sun's heat output could be replenished by its gravitational potential.<sup>100</sup>

Six months later T.J.J. See, at the time in Cambridge, Massachusetts, but who had been at Chicago, published a note in the <u>Astronomical Journal</u> entitled "Note on a new law of Temperature for Gaseous Celestial Bodies".<sup>101</sup> His short announcement read:

> While occupied with some researches on the heat of the sun, and on the cause of the darkness of the companion of <u>Sirius</u>, in May, 1898, I proved that for every gaseous celestial body the law of temperature will be expressed by the following remarkable formula:

### T = K/R

where T is the absolute temperature, R the radius, and K a constant, different for each body ... This law, governing the development of stars from nebulae, is of the utmost generality, and during the past eight months I have drawn from it some conclusions regarding the relative ages of the stars and nebulae, which will, I think, settle the much debated question of the proper classification of the stars of different spectral types. These conclusions, drawn from the above fundamental law of nature, were made known to several distinguished astronomers as much as six months ago, and were announced more fully in a public lecture at the Lowell Institute, Boston, Jan 10, 1899.

In a following article put out later in January, See gave a fuller mathematical account of his ideas, which began to draw some criticism from readers of that journal. At first, criticism<sup>102</sup> was over small technical points, but by this time, several writers, including C.A. Young, were alerted to the fact that it was quite possible that what See was claiming as new was really a restatement of the laws of Helmholtz, Lane and Kelvin.

See had no doubt anticipated this, for in his second <u>Astronomical</u> <u>Journal</u> paper, reprinted in <u>Popular Astronomy</u><sup>103</sup> he provided a long "Historical Statement" on how he was led to this discovery from 1895 studies at the University of Chicago on the source of the Sum's heat. He admitted, at least, to following the course set by Helmholtz, and then noted that, after he had completed his derivation, he had sent it to "... some fifteen of the most distinguished astronomers in the United States ... and ... to an illustrious English friend, who of all men would presumably know of such a law if any had been discovered by previous investigators ..."<sup>104</sup> The reply from the Americans offered no new insights, and the Englishman merely said:

> The only investigation which I can remember which goes mathematically into similar questions though whether such a law is definitely stated I do not recollect - is the series of papers at some time intervals by Ritter, about ten years ago in Wiedermann's Annalen.

See noted that among the Americans consulted were several on the <u>Ap.J.</u> board, and that his illustrious English friend became so interested in the subject that he wrote a letter to the Editor of the <u>Ap.J.</u> (George Ellery Hale) suggesting that Ritter's long neglected work be reprinted. From this description, it is clear that See's friend was

Huggins, whom we have just noted was well aware of Lane's work, and who wrote the letter to Hale in October 1898.

Huggins' letter, stimulated by Hale's announcement that he had observed bright lines in Vogel's IIIb stars, was a cautionary statement against an association with Lockyer, which would be implied by a link between IIIa or IIIb stars and nebulae:

> In case these bright lines, (if real) induce you to speculate a little in respect of spectral classification, I would ask you not to overlook Ritter's important papers, especially the one on a curve of temperature founded on Lane's law. Ritter was the great author of a classification including a rise and fall of temperature. He placed IIIa stars as rising and IIIb stars as falling. This was nearly ten years before the more recent quite similar arrangement founded on the meteoric hypothesis. I hope that Frost will give Ritter his due in his new edition of his translation. You would be doing good service if you were to give a translation of Ritter's paper in the Journal, and so show that a temperature classification is really Ritter's, and should be called by his name; and has no necessary connection with the meteoric hypothesis. I mentioned Ritter in my Cardiff Address, and Schuster, last year, called attention to Ritter's classification  $\dots$ <sup>105</sup>

The reference to Schuster will be covered here in our discussion of Lockyer's work on the chemistry of the hottest stars in 1897. Schuster's paper was commentary upon Lockyer's work. Even though there was no question that Huggins was discussing Lockyer's Meteoritic Hypothesis, we see his inability to mention the man's name, even to Hale. Huggins added a postscript:

> I am not expressing any opinion as to whether your IIIb stars are rising or falling. It is important that the rise & fall of temperature <u>/is</u>/ due to the <u>second principle of Lane's law</u>, and any classification founded thereon, should be <u>kept</u> <u>quite distinct from the meteoric hypothesis</u>. If you will look at Schuster's remarks ... you will see how clear he is on this point. No doubt the rise and fall on Lane's law must come in the future, and it would be a hindrance to the advance of truth, if this were regarded as, even in the remotest way connected with the meteoric hypothesis.

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Of course, this letter to Hale was after the alleged letter to See dated August 12, 1898, but, as we have seen, Huggins was well aware of Lane in the early nineties, and also knew that Schuster, by 1897, had called attention to Ritter's ideas and classification. Huggins' chief interest in writing to Hale was, of course, to argue that if the laws of Lane and Ritter were to become better known and applicable to stellar evolution in the future, "... <u>it would be a hindrance to the</u> advance of truth, if this were regarded as, even in the remotest way connected with the meteoric hypothesis ..."

Why Huggins apparently failed to mention Lane to See, or whether he actually did mention Lane's work to See, is not known at this time. The importance lies elsewhere, in See's evaluation of earlier work, and in the reactions to See's claims to priority.

See's historical statement continued to recall that in December 1899, while on a visit to Yerkes, he was shown a paper by Ritter in volume 13 of <u>Wiedemann's Annalen</u> which, though it had stated the same law discussed by See, was stated only "in language" without any mathematical formalism. This particular paper was not one of Ritter's major statements, and See makes no mention of the fact that Ritter produced some eighteen papers on the subject. The paper which appeared in Volume 13 actually was referring to one of his own earlier papers ( $\neq$  8) in Volume 11 of the <u>Annalen</u> where Ritter established the mathematical formalism. See most certainly was conversant in German, since he completed his dissertation earlier in the decade at Göttingen. He therefore did not read the Volume 13 paper carefully, or curiously decided to omit any reference to earlier papers.

See also claimed that if he had not written to Huggins about the work he was doing, Ritter's papers would have continued to be unknown. But Huggins certainly knew of them previously (his 1891 address

discusses them) and wrote to Hale more incensed about Lockyer's statements than See's.

See noted that Lane's 1870 paper did not actually contain the law, which is correct. He recalled that he had asked Cleveland Abbe to search out the papers and manuscript materials left by Lane to see if such a law indeed was derived or discussed. According to See, Abbe was unsuccessful. Presumably, Abbe, who had written Lane's obituary for the National Academy, overlooked the material I discussed in my previous section on Lane in this chapter. How carefully he looked, and his qualifications to do so, might remain in question, as, according to N. Reingold,<sup>106</sup> Abbe's obituary of Lane contained many errors and doubtful assertions.

After See's derivation of his law he applied it to a classification of stars and a general course of future evolution for the Sun, which on his scheme was to turn blue with increasing age.

See's application of the law required that nebulae be cold, close to absolute zero. Following Vogel's classification, the white stars, of Vogel's first class, were the most condensed and the hottest. See correctly inferred that this made the white stars, the "Sirians", very bright. Further, he envisioned the Sirians to be as far removed from nebulae as possible, in stage of evolution, and noted that their apparent association in the sky (and similarity of spectrum) had to be explained in some other way.

In discussing solar stars of Vogel's second class, See noted that their temperatures were lower, and concluded that since the Sun exhibited a well mixed atmosphere, which did not stratify elements by their specific weights, it could not have passed yet through the Sirian stage. The gravitational force at the surface of Sirian stars was sufficient to cause such a differentiation resulting in the strong

hydrogen spectra characteristic of the class. Thus, solar type stars had rising temperatures, and the Sum would eventually become a blue Sirian type star through continued contraction and heating. His course from red to blue had its origins in a rather curious recollection from ancient history. See and others in the early nineties found that Ptolemy had noted that Sirius was a red star. This started See on a campaign to create a theory of evolution to account for this. He became aware of this historical note in 1892<sup>107</sup> and, during the decade, engaged in various debates and arguments in defence of the correctness of the Sirian colour in ancient times. Actually, Stoney as early as 1868 noted this passage from Ptolemy, and the fascinating question had appeared in the literature from time to time well into the twentieth century. Nobody took it as seriously as See did though. Still, the story warrants telling, and would be an interesting anecdote for future study.

The main thread of our story lies in the direction of the effect of See's work upon the astronomical community. The effect was primarily a reaction to See's claims of priority. In that regard, it affords a good opportunity to examine the extent to which the earlier work was known.

C.A. Young responded to See's papers in April, 1899, in an article entitled "Lane's Law of Increase of Temperature in a Gaseous Sphere Contracting from the Loss of Heat". Young, in the early nineties, had been a sympathetic listener to See's ideas (in correspondence) and here expressed surprise "... that Professor See should lay so much stress on his re-discovery of a law that has been known for thirty years, and for more than twenty has been given in the text-books used in our schools and colleges; and still more surprised that he should attach so much importance to deductions from it, as if it could be assumed to represent the actual facts of stellar temperature". 108

Young noted that when Lane's paper was originally read and published (in what was a widely read American journal devoted to general discussions in science) it caused a great deal of excitement because of the apparent paradox presented:

> I remember very well my first acquaintance with it in 1870, derived from a conversation with Professor Benjamin Peirce, who was much interested in it, and subsequently published two or three papers on the subject in the Proceedings of the American Academy (Boston).

I have unfortunately not been able to locate these papers by Peirce mentioned by Young. However, Young's next references are verified. He noted that the first appearance of the law in its present form where its implications were explicitly discussed was in Newcomb's <u>Popular Astronomy</u> (1878 ed. p.520) and, since that date, the law had appeared in many places, notably (as we have seen) in Newcomb and Holden's <u>Astronomy</u>, Young's <u>The Sun</u> and <u>General Astronomy</u>, Langley's <u>New Astronomy</u>, Proctor's <u>New and Old Astronomy</u>, in the "Concise Knowledge Library" edition of <u>Astronomy</u> (in an article by Agnes Clerke), and

> ... in fact in nearly all the textbooks that have appeared in this country for the last twenty years; though of course not in the shape of a <u>formula</u> introducing the idea and symbol of absolute temperature.

Of course, this was See's point, and later he stuck to it vehemently. It should be noted that during the nineties, See had not yet gained the notoriety he was later to acquire, which resulted in his banishment from the astronomical world, and a general boycott of his work by every journal except the <u>Astronomische Nachrichten</u>. Incidents like the one under review here helped to gain See his reputation.

The bulk of Young's attention was to the applicability of Lane's law to stars like the Sun. He felt that if it had been generally assumed that the law was indeed applicable, then "... It would doubtless ...

have taken a much more conspicuous place in astronomical discussion and speculation ... "But terrestrial evidence led to the conclusion that stars like the Sun could not be perfect gases. There was also

some speculation that nebulae weren't since it was believed that they contain considerable amounts of "non-luminous solid or liquid material ...."<sup>109</sup>

Thus, Young concluded:

It is unsafe therefore to adopt this law as expressing quantitatively and exactly the relation between the diameter and temperature of a shrinking nebula, especially after it has become a "star", possessing a photosphere like the Sun's, an envelope of "cloud", made up of solid or liquid particles mingled with gas still uncondensed. The law only serves to indicate in a general way what may be expected to happen in a contracting nebula; first, a rise of temperature reaching its maximum sometime after a photosphere has been formed; followed by a probably long period of approximate constancy; with, finally, a pretty rapid fall after the photospheric envelope becomes too thick, or dense, or viscous to allow free convection currents between the interior and the surface.

Young was, of course, an early strong influence upon Henry Norris Russell, who, in 1899 was in residence at Princeton and in close contact. The above qualitative review of the probable course of evolution must have been of great interest to the young Russell who was at the time keenly interested in the densities of stars and, in eleven more years, was to bring the theory out again, based entirely upon observational evidence.

Young added that in all probability, Lane's law would not provide accurate numerical results for the temperature distribution in the Sum.

See had decided that the radiation from the Sun was proportional to its temperature. In a discussion of the past amount of heat received by the Earth, See noted that the amount received was dependent upon the angular size of the Sun (or proportional to the square of its radius) and the temperature of the historical Sun, which, on his law, was inversely proportional to radius. The implication was made that the larger historical Sun therefore provided more heat to the early Earth, which implied a linear relationship with heat output and temperature. Young observed:

> Now, while we do not yet know certainly the exact law which connects the absolute temperature of a surface like that of the Sun with its radiating power, there is no question that the latter increases much faster than the temperature: - according to Stefan the radiating power is proportional to the <u>fourth</u> power of the temperature, while Rosetti makes it more nearly proportional to the <u>third</u>.<sup>110</sup>

Either of these laws, observed Young, would then cause the early Earth to have received less heat than the present Earth, on See's hypothesis (which Young preferred to leave as "Lane's law of temperature").

Aside from our specific interests in noting the degree of acceptance of the applicability or non-applicability of Lane's law to the Sun and stars, as provided by See's claims and reactions to it, we now have a clear statement as to the applicability of one of the most important radiation laws - that of Stefan - to the study of stars at the turn of the century.

Lockyer also responded to See's claims, by asking John Perry to discuss, for <u>Nature</u>, his opinions of the various studies made on the subject of "The Life of a Star" from the direction of theory.

Perry was already well acquainted with the problem, as he had been a student of Kelvin's, and had produced a number of studies on convective equilibrium. By the early nineties, Perry had become a critic of his teacher's arguments for the rate of cooling of the Earth, and its subsequent age.<sup>111</sup>

Perry's response to Lockyer was published in <u>Nature</u> in the

form of an extensive note with what amounted to a complete re-derivation of the equilibrium configuration for a gaseous star, on the assumption that the perfect gas laws were adhered to until the central density became 0.1 that of ordinary water.

Before he entered into his own discussion, he dealt with See in a rather harsh manner, noting that many of See's assumptions were "metaphysical" and "Thus it is impossible for a mathematical physicist to get to Mr. See's point of view".<sup>113</sup> Perry was particularly annoyed with See's assignment of temperature, without any specification of where this temperature was to be taken - at the solar surface, or a mean temperature, etc. Further, See assigned a finite gravitational pressure for the surface of the Sun, which to Perry was a physical impossibility, in the absence of matter outside that point bearing down upon the "surface".

Perry's chief criticisms of Ritter and Lane also centred around their assignment of temperature, the former using Stefan's law and the latter, the earlier law of Dulong and Petit:

> ... It seems to me that we know too little about the phenomenon of radiation from layers of gas with denser and hotter layers below and rarer and colder layers above to allow of any weight being placed upon these assumptions of Ritter or of Homer Lane.<sup>114</sup>

Perry did not directly criticise Lane's or Ritter's procedures, only their physical assumptions, based upon extrapolated laboratory work. Perry proceeded to obtain formulae for the distribution of physical variables within a gas sphere representing the Sun for two cases. First, he assumed the uniform temperature of past history promoted by Newcomb and others. Then he altered his model so that the past history of the Earth's temperature was not uniform, caused by a non-uniform heat output from the Sun. This second solar model was the one preferred. Perry believed that there were periods of millions of years where the Sun radiated less than one-third or even one-tenth of its present rate.

Perry was confident that during some phase of the lifetime of any star, the gas laws were adhered to but he was not sure of what would happen once the density of the star reached that limit where it began to cease behaving like a perfect gas. Of course, he also felt that the nebular stages of stars, when they were composed of meteoritic material, or a combination of solid and gaseous material (evidently from Kelvin and Lockyer's influence), were mere speculation too, and the later stages, comprising most of the stellar lifetime, were unknown completely in character. He did speculate that with advancing age, and contraction, the "radiating layer" of the star became less and less submerged, and rose nearer the "surface" of the star. Perry elaborated:

> It seems to me that this is an important thing. Α young star, a truly gaseous star, has great depth of radiating layer. I mean it is probably only at great depths from the free surface that we find the layer from which a continuous spectrum comes. I take it that it is only during collision of molecules that a continuous spectrum is given out; in the free-path state of a molecule it radiates its own light only. Great density and high temperature conduce to the giving out of the continuous spectrum. In old stars, like our sun, the layer of stuff capable of giving out white light is comparatively near the surface of the star. I can imagine a comparatively young star long before its heat energy is at a maximum, not radiating energy very fast, but rather giving out bright line spectra light from the greater part of its area; in fact from all but its central parts.<sup>115</sup>

Perry was willing to examine the role of collisional effects causing the continuum. Even though it had long been known that a high pressure gas could produce continuous spectra, the usual theory was that the continuum arose from a region of condensation (to account for a sharp solar limb). His belief in the migration of the "radiating surface" with age causing the depth of the atmosphere of the star to effectively decrease was actually quite similar to Huggins' view,

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expressed at about the same time. His association of young stars with bright-line spectra was not unfavourable to either Lockyer's, or the established course of evolution. He did envision a period of maximum temperature however, if "heat energy" can be so construed. Of interest here is that at this point, the star evidently is not too luminous. Perry is quite concerned that his view - that in the past the Sun was less luminous than at present - be aired. In order to do this, he examined "Non-Uniformitarian Assumptions",<sup>116</sup> which assumed that at any time in past history, the rate of radiation of the Sun was proportional to the "intrinsic thermodynamic energy" contained within it. This nongravitational energy was assumed to be maximum when the Sun's radius was four times its present value. This was well within the limits he had previously set for the range of radii of the early Sun when it could still behave as a perfect gas.

Perry's study ended on a generally inconclusive note. Obviously, without some way of knowing what the internal composition of the Sun or stars was, or having a known value for the mean specific heat ratio, or the actual range of conditions under which gases behave perfectly, there were simply too many unknowns to allow for any definite quantitative conclusions.

In the years following 1900, See continued to examine the problem, though now "banished" to comparative isolation at Mare Island in California, an outpost of the U.S. Naval Observatory. By this time, the papers by R. Emden had begun to appear, which, after several years, were to culminate in his famous book "Gaskugeln". By 1905, See had begun to incorporate some of Emden's ideas, notably that rotating gas spheres could not remain homogeneous. While Emden introduced the idea to promote his conception of a stratified series of convective layers within the Sun which became more discontinuous "... during the cooling

of the rotating sun from without ..."<sup>117</sup> See used it to attack the studies of Darwin and Poincare, who required homogeneous spheres in their examination of figures of equilibrium of rotating masses of fluid. See noted:

If in accordance with these views the heavenly bodies develop from nebulae made up first of compound gases, afterwards becoming monatomic when high temperature develops, it follows that condensing masses are always heterogeneous to a very considerable degree. The condensation of the matter towards the centre is diminished by the high temperature developed, when the mass becomes monatomic, but the central density remains six times the mean.<sup>118</sup>

See's suggestion of dissociation (in this case molecular dissociation) in the stellar interior is not as unique as he would like to suggest. He noted that the assumptions of all the others (Lane, Ritter, Kelvin, Perry, etc.) included only compound and homogeneous mixtures. This is not quite true, as we have seen, but seems not to have been a major point at the time. Later, after Eddington's work in 1916, the effect of ionization on the stellar interior (upon its compressibility primarily) did become an extremely important question. Further, See's consideration of heterogeneous models at the time was an interesting forerunner of models, created in the thirties by Strömgren, Öpik and others, which had convective cores and radiative envelopes and which were intended to describe the structure of giant stars. There evidently is no link between See's work and this later work, however.

#### Conclusions

Relating work here to other chapters in this study, it seems that we might classify thought concerning the concept of contraction into two primary phases. First, pre-1860 speculation was limited to the pre-stellar stage alone, any subsequent evolution being cooling of a static surface (with the possibility of minimal contraction due to

cooling only). The post-1860 era seems to bring a broader interpretation of the contraction phase from nebular stage, through the onset of stellar stages, and well on to the extinction of the star itself as an incompressible cooling mass. While Helmholtz did consider this as early as 1854, it was not until the Sun could be conceived of as gaseous (and therefore more compressible than if it was solid), that we see directed attempts to study it in terms of an unambiguous contracting model. Throughout this latter phase, ignorance of the true behaviour of gases under conditions assumed to exist in the stellar interior severely limited the assignment of the Sun to a definite future course of evolution of rising or falling temperature, though most felt it had to be cooling. Chief among these unknowns was the value of the ratio of specific heats, whose behaviour was only empirically understood at the time and without a well-defined atomic-molecular theory of matter was impossible to predict for the stellar interior. Still, with these unknowns as constraints, it is quite clear that qualitatively at least, most who considered the general course of evolution of stars from theoretical considerations at one time or another seriously considered the idea that stars pass through the red stage twice, arising initially out of nebulae.

The mutual effect of this evolutionary concept upon schemes of spectral classification will be examined in the following chapters.

#### References

 Newton, as early as 1692, had implied this in a letter to the Rev. Dr. Bentley (H.W. Turnbull, <u>The Correspondence of Sir</u> <u>Isaac Newton, III</u> (Cambridge, 1961), p.234) but William Herschel put the idea on a firm observational footing (E.S. Holden, <u>Sir William Herschel</u> (Scribner's, 1881), pp. 202-204; M. Hoskin, <u>William Herschel</u> (Sched and Ward,1959), p.30; M. Hoskin, <u>Vistas in Astronomy</u>, <u>9</u> (1967), p.80.).
 Hoskin has pointed out correctly (<u>Ibid.</u>, 1967) that Rosse's colleagues were more anxious to claim the ultimate resolvability of all nebulae than he was himself. Rosse's most ardent spokeman, J.P. Nichol, claimed that the possibility of true nebulae existing had been "... for ever ... hopelessly

destroyed ..." (<u>Ibid.</u>, p.81) though in Nichol's other books, far less conclusive statements were made. John Herschel also kept from such strong statements. It is curious that through the many editions of his <u>Outlines</u> (between 1849 and 1872), one finds the following statement unchanged:

Although, therefore, nebulae do exist, which even in this /Rosse's 6-foot reflector/ powerful telescope appear as nebulae, without any sign of resolution, it may very reasonably be doubted whether there be really any essential physical distinction between nebulae and clusters of stars, at least in the nature of which they consist ... (John Herschel, Outlines of Astronomy (1872), p.639).

The negative opinion regarding the existence of nebulae was not held by everyone. As we shall see, Helmholtz believed in them through the fifties. Further, from the recent work of Ronald Numbers, <u>Creation by Natural Law - Laplace's Nebular Hypothesis</u> <u>in American Thought</u> (Univ. of Washington, 1977) we find that many Americans, notably the Princeton professor Stephan Alexander, continued to discuss the nebular hypothesis during the fifties. Other adherents during the fifties, according to Numbers, included Benjamin Peirce, B.A. Gould, O.M. Mitchel, Elias Loomis, Daniel Kirkwood, Joseph Henry and Asa Gray (<u>Ibid.</u>, p.64). Further, Numbers discusses the detractors of the nebular hypothesis, and nowhere is there to be found an argument based upon the non-existence of true nebulae. See also <u>Ibid.</u>, pp. 30-31.

- 3. <u>Op. cit.</u>, Hoskin (1959), p.59.
- 4. A.O. Lovejoy, The Great Chain of Being (Harper, 1960), p.244.
- 5. G.B. Airy, <u>Mem. R.A.S.</u> 9 (1836), p.168.
- W.W. Campbell, "Newton's Influence upon Astrophysics", in <u>Sir</u>
  <u>Isaac Newton</u> (Baltimore, 1928), pp. 83-84.
- 7. H. von Helmholtz, Phil. Mag. 4th S. XI (1856), p.500.
- 8. <u>Ibid.</u>, p.503.
- 9. <u>Ibid.</u>, p.505.
- 10. <u>Ibid</u>.
- 11. <u>Ibid</u>.
- 12. Ibid., p.507.

13. <u>Ibid</u>.

- 14. <u>Ibid.</u>, p.514.
- 15. J.D. Burchfield, Lord Kelvin and the Age of the Earth (Science History Publications, 1975), p.23.
- 16. William Thomson, "On the Mechanical Energies of the Solar System", <u>Trans. R.S. Edinb.</u> 21 pt. 1., (1853-4); read April 17, 1854.
- 17. J. Joule, Phil. Mag. (May 1848).
- 18. Op. cit., Thomson (1854), p.66.
- 19. Ibid.
20. <u>Ibid.</u>, p.71.

21. <u>Ibid</u>.

22. M.J.S. Rudwick, <u>The Meaning of Fossils</u>, 2nd Ed. (Science History Publications, 1976), p.171.

23. <u>Op. cit.</u>, Burchfield (1975), p.26.

24. <u>Ibid.</u>, pp. 26-27.

- 25. From its Manchester meetings, September, 1861. Reprinted in: <u>Phil. Mag. 23</u> (1862), p.158; later in full form as a popular lecture, in <u>MacMillan's Magazine</u>, March, 1862.
- A. Clerke, <u>History of Astronomy</u>, 2nd Ed. (A&C Black, 1887),
   p.189, footnote.
- 27. William Thomson, "On the Age of the Sun's Heat", <u>MacMillan's</u> <u>Magazine</u> (March, 1862); reprinted in: <u>Popular Lectures and</u> Addresses (MacMillan, London, 1891), pp. 360-361.
- 28. Thomson was quick to recall the teachings of Stokes, noting that Bunsen and Kirchhoff had made a recent and admirable application of Stokes' principles and theory. Thomson thus took his well documented side in the priority dispute involving Stokes, Balfour Stewart and many others. Cf. D. Siegel, <u>Isis</u> <u>67</u> (1976), p.565; W. McGucken, <u>Nineteenth Century Spectroscopy</u> (Johns Hopkins, 1969), pp. 14-22. It is also worthy of note that through the sixties, this was not the only major priority dispute between the Germans and British, for they were also arguing about possession of the conservation laws, siding either with Joule or Mayer.

29. <u>Op. cit.</u>, Thomson (1862), p.364.

30. Ibid.

31. <u>Ibid.</u>, p.365.

32. Ibid.

33. <u>Ibid.</u>, pp. 365-366.

34. <u>Ibid.</u>, pp. 369-370.

- 35. S. Chandrasekhar, <u>The Study of Stellar Structure</u> (Chicago, 1939), p. 176.
- 36. <u>Ibid.</u>, p.179.
- 37. M.J.S. Rudwick, "Uniformity and Progression: Reflections on the Structure of Geological Theory in the Age of Lyell", in: Roller, R. (Ed.), <u>Perspectives in the History of Science and</u> <u>Technology</u> (Oklahoma, 1971).
- 38. <u>Op. cit.</u>, ref. 34, p.373.
- 39. H. Faye, <u>Comptes Rendus</u> <u>60</u> (1865), pp. 89, 138, 595.
- 40. <u>Op. cit.</u>, ref. 26, pp. 189-190.
- 41. H. Spencer, <u>Recent Discussions in Science</u> (D. Appleton, N.Y., 1884), p.304.
- 42. Ibid.
- 43. <u>Ibid.</u>, p.305.
- 44. Cf. Letters in Lane Collection, #66, #67, National Archives.

45. S. Newcomb, <u>Reminiscences of an Astronomer</u> (Houghton-Mifflin, 1903), p.246.

- 46. <u>Ibid.</u>, pp. 246-247.
- 47. J. Homer Lane, "On the Theoretical Temperature of the Sun; under the Hypothesis of a Gaseous Mass Maintaining its Volume by its Internal Heat, and depending on the Laws of Gases as known to Terrestrial Experiment", <u>American J. of Science 50</u> (1870), p.57.
- 48. <u>Ibid.</u>, p.58.
- 49. <u>Ibid.</u>, pp. 58-59. The Poisson relation for adiabatic processes would today be called a polytrope.
- 50. Item #70, National Archives Collection of Lane Papers.
- 51. <u>Op. cit.</u>, Chandrasekhar, ref. 35, pp. 176-177.

52. <u>Op. cit.</u>, Newcomb, ref. 45, p.248.

53. <u>Op. cit.</u>, Chandrasekhar, ref. 35, p.177. The formula, as provided by Chandrasekhar, when obeyed, describes a gaseous distribution which is polytropic of the general form:

$$\mathbf{P} = \mathbf{A} \cdot \boldsymbol{\rho}^{\Upsilon}$$

where P = pressure,  $\rho = density$ , and A and  $\gamma$  are numerical constants which describe the relative behaviour of the pressure and density of the gas with temperature. Chandrasekhar's exponent (1 + 1/n) defines n, the polytropic index, where  $\gamma$ , in our equation is:

$$\gamma = 1 + 1/n$$

By varying n, and hence  $\gamma$ , various temperature distributions can be examined, one of which will approximate the actual, or desired solution. Lane, in varying  $\gamma$  was able to choose the one which best fit a reasonable mixing length at a point where T equalled what he believed to be the correct solar surface temperature.

54. A.S. Eddington, <u>Internal Constitution of the Stars</u> (Dover reprint, c. 1926), p.83.

55. Fragmentary notes, Lane Collection, National Archives.

56. Op. cit., Newcomb, ref. 45, p.248.

- 57. William Thomson, "On the Equilibrium of a Gas under its own Gravitation Only", Phil. Mag. 23 (1887), pp. 287-292.
- 58. Newcomb and Holden, Astronomy (H. Holt, 1879), p.307.
- 59. <u>Ibid.</u>, pp. 307-308.
- 60. C.A. Young, The Sun (D. Appleton, New York, 1883), pp. 270-271.
- 61. <u>Ibid.</u>, p.274.

62. <u>Ibid.</u>, p.275.

63. J. Janssen, <u>Annuaire du Bureau des Longitudes</u> (1879), p.623;

Cambridge, 1926

Cf. A.J. Meadows, Early Solar Physics (Pergamon, 1970), p.135 ff.
64. <u>Ibid.</u>, p.168.

- 65. Ibid.
- 66. A. Schuster, <u>Report of the British Association</u> (1883), p.427.
  67. <u>Ibid.</u>, p.428.
- 68. <u>Op. cit.</u>, Chandrasekhar, ref. 35, p.178.
- 69. This aspect of Ritter's study is of interest, as it appears to be a singular event in the study of the theory of variable stars. It is well known that variability was largely discussed as some form of eclipsing mechanism through this period and well into the 20th century, being seriously discussed as pulsation by Shapley and others only by 1916. Further, the consideration that a star might be able to expand against its own gravitational potential seems to be unique for this period. It is without question that a closer study of Ritter's work and the influences upon him must be made, but this is a study for the future, possibly for someone with better access to the language and literature of his time.
- 70. Editorial Remarks prior to Ritter's translated paper, Astrophysical Journal 8 (1898), p.293.
- 71. <u>Ibid.</u>, Ritter, p.299.
- 72. <u>Ibid.</u>, p.305.
- 73. <u>Ibid.</u>, p.307.
- 74. <u>Ibid.</u>, p.308. Thus far, Ritter had assumed that the Sun obeyed "Mariotte's law" which was the European expression for Boyle's law of the compressibility of gases: "The temperatures remaining the same, the volume of a given quantity of gas is inversely as the pressure which it bears", (Ganot, <u>Physics</u>, 8th Ed. (New York, 1877)). Ritter felt that this assumption was not really

valid, and that "... the Sun long ago passed the stage of development at which the approximate validity of Mariotte's law ceases in consequence of the gradual increase of the density".

- 75. <u>Ibid.</u>, p.309.
- 76. <u>Ibid.</u>, p.310.
- 77. <u>Ibid.</u> Ritter referred to paragraph 65 which did appear in the original version of his work (<u>Annalen der Physik und Chemie</u> <u>20</u> (1883), p.142) under the title "Hypothesis of the law of contraction of the Sun" where he was able to establish the time rate of change of the work done by contraction and compare it to the potential still available in the Sun at that point (with given state variables of temperature, pressure and radiant output). At each point in time, therefore, he was able to determine if the radiation was greater or less than the work done in contracting.
- 78. <u>Ibid.</u>, p.311.
- 79. <u>Ibid</u>.
- 80. <u>Ibid.</u>, p.312.
- 81. <u>Ibid.</u>, p.313.
- 82. G. Darwin, Phil. Trans. 180 (1889), Series A, pp. 1-69.
- William Thomson, <u>Popular Lectures and Addresses I</u>, p.379.
   This was his Royal Institution Address of 1887.
- 84. <u>Ibid.</u>, p.380.
- 85. <u>Op. cit.</u>, ref. 57, p.287, note.
- 86. Ibid.
- 87. <u>Op. cit.</u>, ref. 83, p.394.
- 88. <u>Op. cit.</u>, ref. 57, p.289.
- 89. Publications of the Astronomical Society of the Pacific 4

(1892), p.105.

- 90. <u>Op. cit.</u>, ref. 83, p.408.
- 91. <u>Ibid.</u>, pp. 408-409.
- 92. <u>Op. cit.</u>, ref. 35, p.179.
- 93. <u>Op. cit.</u>, ref. 83, p.410.
- 94. <u>Ibid.</u>, p.411.
- 95. <u>Ibid.</u>, pp. 417-418.
- 96. <u>Ibid.</u>, pp. 418-419.
- 97. <u>Ibid</u>.
- 98. Huggins, <u>British Association Reports</u>, <u>LXI</u> (1891), pp. 3-37; reprinted in <u>Smithsonian Report</u> for 1891 (Smithsonian, 1893).
- 99. <u>Ibid.</u>, (1893), p.80.
- 100. A.S. Young, Popular Astronomy 6 (1898), p.145.
- 101. T.J.J. See, <u>Astronomical Journal</u> <u>19</u>, ≠453, p.169.
- 102. <u>Astronomical Journal 19</u>, ≠456 (1899).
- 103. T.J.J. See, Popular Astronomy 7 (1899), p.129.
- 104. <u>Ibid.</u>, p.130.
- 105. Letter, Huggins to Hale (30 October, 1898). Hale Papers Microfilm.
- 106. N. Reingold, "Jonathan Homer Lane", <u>Dictionary of Scientific</u> <u>Biography 8</u> (1973), p.3.
- 107. Letter, T.J.J. See to C.A. Young (30 April, 1892), Dartmouth University Library.
- 108. C.A. Young, Popular Astronomy 7 (1899), p.225.
- 109. Ibid., p.226.
- 110. <u>Ibid.</u>, p.227.
- 111. <u>Op. cit.</u>, ref. 15, pp. 134-135.
- 112. J. Perry, "The Life of a Star", <u>Nature 60</u> (1899), p.247.

# 113. Ibid.

- 114. <u>Ibid</u>.
- 115. <u>Ibid.</u>, p.249.
- 116. <u>Ibid.</u>, p.251.
- 117. R. Emden, Astrophysical Journal 15 (1902), p.52.
- 118. See, Astronomische Nachrichten 169 (1905), p.362.

# CHAPTER 2

# Stellar Classification and Stellar Evolution 1860-1900

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#### CHAPTER 2

#### Stellar Classification and Stellar Evolution 1860-1900

#### Introduction

In Chapter 1, the direction of evolution was identified in 19th century thinking as a cooling process, with a persistent secondary concept similar to Lockyer's temperature arch appearing in several places after mid-century. In Ritter's scheme, the heating phase was very short relative to the cooling phase, thus making bright red stars rare in the sky. Thomson, too, had a short heating phase built into his scheme, and, as we shall see later in this chapter, one of the most influential German spectroscopists, H. Vogel, did not reject the possibility that an early heating phase existed in the lifetimes of stars. But while heating was an accepted phase for the early history of a star's life, very few other than Lockyer felt that it was of sufficient duration to allow a significant fraction of a sampling of stars seen in the sky to be stars in heating stages.

How did 19th century schemes of spectral classification reflect or influence theories of stellar evolution? Commencing in the sixties with Secchi, Rutherfurd, Donati and Carpenter, and continuing in the seventies with Vogel, Huggins and Lockyer through the turn of the century with Pickering's staff and others, we will identify the mutual interdependence of classification and evolution. We will also bring in other allied work, such as nebular research and stellar statistics, which interacted strongly with evolutionary ideas, and thereby we will set the stage for our ultimate discussion of the evolutionary origins of the H-R Diagram.

Prior to 1860, and the work of Kirchhoff, it was known that the spectra of stars varied with colour. William Herschel, in 1798,

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observed (using a prism placed in front of his eyepiece) that Betelgeuse seemed to be more intense in the red than did Sirius. Though these differences were noted, Herschel felt that in the main, a high degree of sameness predominated which implied "... The similarity of the general constitution of the sun, and stars, and the planets ...."

Between 1817 and 1823, Fraunhofer studied solar and stellar spectra, first with a small slit spectroscope and later with a more efficient objective prism telescope. He was able to show that stellar spectra differed greatly from solar spectra, but that there were many similar lines found in both. Still it was not a simple task for Fraunhofer in 1823, or others such as John Herschel, David Brewster, or Fox Talbot in following decades, to come to the conclusion that the lines they were seeing in the solar spectrum were not somehow due to terrestrial absorption. Of greater difficulty was the persistence of sodium, the famous problem which kept the identity rule of "one element, one spectrum" out of reach until the time of Kirchhoff. As the various aspects of the significance of Kirchhoff's accomplishments have been extensively reviewed, we will not repeat the discussion here. It is sufficient to say that, by mid-century, detailed studies of flame spectra in combination with observations of solar spectra seen through varying air masses, rendered the solar origin for most of the lines an accepted reality.

To compare the bright line spectra of laboratory flames to the dark line spectra of the Sun, however, involved the assumption that a body absorbs the same colours that it emits. McGucken's discussion<sup>3</sup> has shown that this identity was not generally accepted until the time of Kirchhoff and Bunsen though more recent studies by M.A. Sutton<sup>4</sup> and D.M. Siegel<sup>5</sup> have shown that the laws were obtainable at a somewhat earlier date, and were most certainly "in the air". When Kirchhoff's work did appear, a priority dispute between the Germans and British therefore arose, involving Stokes, Thomson, Stewart, Angstrom, Kirchhoff and others. Whittaker<sup>6</sup> and McGucken<sup>7</sup> also note the early work of Foucault in 1849, which came close to Kirchhoff's conclusions.

Whatever the outcome of the priority dispute over the ownership of Kirchhoff's laws, Kirchhoff's role is marked for our special interest because he showed how his conclusions could be applied to the Sum and stars. Of course, even here he was anticipated to some extent by Stokes and others, but he extended the analysis to more lines in the solar spectrum, and showed more line coincidences, than had been done previously by anyone. Through the early sixties he identified iron, magnesium, calcium and chromium, and eventually produced a standard map of the solar spectrum containing some 600 lines; of these, 138 were identified with known elements, including almost 60 due to iron alone.

Kirchhoff also applied his laws to the determination of the structure of the Sun, as deduced from its dark line spectrum:

> The sun consists of a luminous nucleus, which would by itself produce a continuous spectrum, and of an incandescent gaseous atmosphere, which by itself would produce a spectrum consisting of an immense number of bright lines characteristic of the numerous substances which it contains.<sup>8</sup>

Thus, while Kirchhoff was willing to state that the solar atmosphere was gaseous, he left the actual state of the interior open to question. To him, since it gave a continuous spectrum, it had to be either solid or liquid.

It is of critical interest to note that whatever priority Stokes and others might have had in the analysis of spectra, it was not until Kirchhoff's studies that we see the beginnings of stellar spectral classification. Indeed, in the very year of Kirchhoff's work, stellar classification began. Our analysis will proceed then with a discussion of these systems, emphasising the role of evolutionary theories as a central theme.

### Spectral Classification in the 1860s

In this section, as in subsequent ones, we will rely upon the comprehensive review of classification systems provided by R.H. Curtiss in 1931.<sup>9</sup> Table 1 reviews their basic nomenclature and characteristics, and also allows for intercomparison. Thus, within our text, the emphasis will be upon discussions of evolution, and not upon the varied details of the classification systems themselves, unless they bear directly upon evolution.

#### •• Donati

In 1860, G.B. Donati began to examine a small sample of stellar spectra: his conclusions were published in 1862.<sup>10</sup> Clerke<sup>11</sup> has commented that his system was a failure in that it involved only 15 of the brighter stars in the sky, but from a historical standpoint, we can see its significance. He clearly associated spectrum with colour, and, hence, set the stage for the further concept of stellar aging via a temperature change.<sup>12</sup> Donati had his detractors in the sixties, too. d'Arrest in Copenhagen felt that an association between colour and stage of development of a star was suspect.<sup>13</sup>

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#### Rutherfurd and Carpenter

In 1862-63 two workers, one English and the other American, contributed to the still small list of spectral classification schemes. Lewis Morris Rutherfurd of New York City used a slit spectroscope. From an observing list of 23 stars, he proposed a crude classification<sup>14</sup> utilising three groups; corresponding roughly to the Sun, Sirius, and white stars showing no lines (Rigel). At Greenwich, J. Carpenter<sup>15</sup> attempted a classification with a novel spectroscope described by Airy.<sup>16</sup>

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		. <b>.</b>	Table 1		• • • •	•
Donati (1860-61)	White		Yellow	Oran	Red	
Rutherfurd (1862)	Group 3 Spice Rigel	Group 2 Sirius			Group 1 (lines and bands)	
Carpenter (1863)	Group 2		Group 3		Group 1	
Secchi (July 1863)	Class 2 White stars Sirius		-		Class 1 (dark bands)	•
Seochi (Aug. 1866)	Class 3 (wide Hydrog lines)-Siriu	ren 15	Class 2 (narrow lin Arcturus	•=)	Class 1 (wide bands) Betelgeuse	
<b>Secchi (Oct. 1866)</b>	Type I Vega		Type III Sum		Type II Betelgeuse	
Secchi (1867)	Type I Vega		Type III Sum	•	Type II Alpha Herc.	
Secchi (1867-68)	Type I Vega		Type II Arcturus		Type III Alpha Herc.	Type IV 152 Schj.
¥og+l (1874)	Class Ia Sirius Vega	Class Ib (Orion St) Class Ic (Beta Lyrae	Class IIa Capella Arcturus Aldebaran	Class IIb Wolf-Rayet	Class IIIa Betelgeuse Alpha Herc.	Class IIIb (all faint red stars in Schj.
		Gan. Cass)			•	catalogue)
Lockyer (1874)	۵		ß		Y	
Buggins (1880)	Sirius Voga Ą Ursa Kaj & Virg.		& Aquilae Rigel & Cygni Capella Run		Arcturus Aldebaran Betelgeuse	
Ritter (1883)	с	•	c		B	C A(Neb)
Lockyer (1888)	Group IV only hottes	t stars	Group V Group III	· .	Group VI Group II	Group VII Group I
•	VII./10 2					
Pickering (1891)			-			۲
Pickering/ Fleming (1890-92)	A B (incl. Or C D	ion stars)	E F Q	•	×	o x q Q
	• •	•.	J K L			•
KoClean	Division I Division II Division II	I .	Division I Division I Division I Division I	I II V	Division V	•
Boack (1892-98)	Orion Sirian		Capellan Arcturian		Antarian	•
Vogel (1895)	Class Ini Class In2 Class In3	Class Ib Class Ici Class Ic2	Class IIa Class IIb		Class IIIa	Class IIIb
Lockyer (1897) Genera	Karkabian Sirian Cygnian Procyonian	Alnitamian Crucian Taurian Rigelian	Polarian Arcturian Argonian		Antarian	Piscian
Pickering/ Plening (1897)	A B	• • • •	T B G E	•	X	H O P Q
Maury (1897)	11 11-11	XII	XIII to		XVII-XX	XI 1,11,1V,V
			<b>A</b> 74	•	. •	IIXI
Buggins (1899)	Bellatrix Rigel g Cygni Regulus Vega	•	Castor Altair Procyon Y Cygni Capella		Arcturus Betelgeuse	
	Sirius		• -	-		
Cannon (1901)	В А	T	đ	E	<b>X</b>	N 0

Tabulation of all spectroscopic classification systems discussed within Chapters 2 and 3 showing interrelationships. In cases where stars or numerical designations are listed vertically within a class (based upon Secchi (1887-68))the order is intended to read down the list, and will continue in the mext column, i.e. for Huggins (1899) Castor comes after Sirius. Rutherfurd did express what might have been a general belief at the time concerning the implications of the variation of spectra amongst stars:

> One thought I cannot forbear suggesting: we have long known that 'one stars differeth from another in glory;' we now have the strongest evidence that they also differ in constituent materials, - some of them perhaps having no elements to be found in some other. What then becomes of that homogeneity of original diffuse matter which is almost a logical necessity of the nebular hypothesis?<sup>17</sup>

Spectral differences interpreted as composition differences amongst stars will be a persistent non-evolutionary theme which we will see continue through the remainder of the century.

## \*\* <u>Secchi</u>

The first major system to evolve in the sixties was that of Father Angelo Secchi, director of the Observatory of the Roman College (now the Vatican Observatory). Secchi had repeated some of Fraunhofer's spectroscopic observations as early as 1855,<sup>18</sup> but did not begin systematic work until stimulated by Kirchhoff's discoveries, by Donati's classification attempts,<sup>19</sup> and the opportunity to gain assistance in observing and instrumentation from J. Janssen, who was to provide considerable aid in the preparation of Secchi's monumental <u>Le Soleil</u>.<sup>20</sup>

Secchi's first system in 1863 included only two classes, divided into "coloured" and "white". In reporting his first work, he noted:

> The study of the spectra of heavenly bodies has a two-fold significance: first, to establish the existence and nature of their atmospheres, and second, to be able to answer certain questions about the very interesting order in the universe, questions above all pertaining to the motions of the stars.<sup>21</sup>

It has been the opinion of his reviewers that one of the questions "of the very interesting order in the universe" not asked by Secchi was whether any system of classification could reflect the progress

of stellar development. McCarthy has suggested that "... while Fr. Secchi's classification did not claim to be a genetic theory of the development of stars from class to class, nevertheless it did recognise the different physical conditions which obtained in the different classes ...<sup>22</sup> While it appears to be true that Secchi at no time proposed a specific scheme of classification based upon evolution, some observations of nebulae in 1865 tend to show that he, indeed, had this study in mind to some extent. In a letter from Secchi to De La Rue<sup>23</sup> in 1865, and published in translation in the <u>Monthly Notices</u>, Secchi discussed the spectrum of the nebula in Orion and his own observations of it, which seemed to be similar to those recently made by Huggins on planetary nebulae. Within this note, written, as it seems, indirectly as a series of questions to Huggins, Secchi indicated the existence of three distinct lines in the visual and near blue region.

One of these lines coincided with the strong Fraunhofer F line, but the others were not identified. Secchi noted that their positions were similar to those found by Huggins in planetary nebulae, but their relative intensities were different. The similarity here, and the additional observation that Huggins had noted what Secchi thought was a bright line at F in the spectrum of Betelgeuse, caused Secchi to wonder:

> ... Can this star be a body intermediate between the perfectly formed stars and the nebulae? It is here that the study of the question is important. I do not know whether Mr. Huggins has done anything on this kind of work.<sup>24</sup>

Huggins had been involved<sup>25</sup> with nebular spectra, but not so much with its evolutionary implications. Since he interpreted his own observations somewhat differently, he responded to Secchi's question:

> I may be allowed to say that I cannot agree with the ingenious conjecture of Secchi that the star  $\underline{\alpha}$ Orionis may be a body intermediate between a nebula

and a fully-formed sum, because the dark line of absorption corresponding to F is wanting in its spectrum.<sup>26</sup>

Huggins added that the absence of the F line in the star can lead only to the conclusion that the particular gas which produced this line (hydrogen) is not in the atmosphere of that star. Huggins also provided an account of his recent work on Orion. He reviewed his determination of the physical state of the nebulae, and referred to Herschel's concept of the condensation of stars from them:

> If the gaseous matter of these objects represented the 'nebulous fluid', out of which according to the hypothesis of Sir Wm. Herschel, stars are to be elaborated, we should expect a spectrum in which the groups of bright lines were as numerous as the dark lines due to absorption found in the spectra of the stars.<sup>27</sup>

Huggins qualified the above statement by suggesting that a "progressive" development of nebular spectra into stellar spectra should be looked for, assuming that the three bright lines seen in nebular spectra represent "... matter in its most primary forms ..." Surely, he felt, "... we should expect to find in some of the nebulae, or in some parts of them, indications by a more complex spectrum, of an advance in the formation of the separate elementary bodies which exist in the Sun and in the stars ..." Such a progression was believed to have been observed already in the form of spiral nebulae like the Great Nebula in Andromeda, where a contral nucleus within the nebula produced a continuous spectrum. Huggins utilised this observation to comment:

> It may, therefore, be that nebulae which have little indication of resolvability, and yet give a <u>continuous</u> spectrum, such as the <u>Great</u> <u>Nebula in Andromeda</u>, are not clusters of suns, but gaseous nebulae which, by the gradual loss of heat, or the influence of other forces, have become crowded with more condensed and opaque portions.

Returning to nebulae with purely gaseous spectra, Huggins' note concluded:

So far as my observations extend at present, they suggest the opinion that the nebulae which give a gaseous spectrum are systems possessing a structure, and a relation to the universe, altogether distinct from the great group of cosmical bodies to which our Sun and the fixed stars belong.<sup>28</sup>

Huggins' opinion at the time was against the interpretation that would place nebulae higher (or earlier) on the evolutionary series than stars. He was, of course, to change this opinion completely.

Early in 1866, Secchi published detailed maps of two stars, Sirius and Betelgeuse, typical of his first classification scheme.<sup>29</sup> But within the same year, he changed the methodology of his study away from a careful examination of a small selected list of representative stars to a broad and general study of as many stars as possible. By August, 1866,<sup>30</sup> he had added an additional third class to the first two.

His stars of class III were the most numerous on his list. In the brightest of this class, very fine lines were observed. Within this report, Secchi announced that he was at work upon a general catalogue of spectra for 220 stars. The catalogue appeared in October and in the following months<sup>31</sup> was reprinted in several places. Within this catalogue, Secchi rearranged his classes into "types", and added a fourth subdivision of the white stars to account for the stars in the constellation of Orion (with the exception of Betelgeuse).

In the following year, Secchi published a new catalogue with 316 stars. No significant change in the nature of the system was made, but it was becoming evident to him that intermediate types between I, II and III began to appear in greater numbers as he increased his sample. Later in the year, he reversed types II and III so that they

read: Type I, White stars; Type II, Yellow stars; Type III, Red stars.

This revised order was retained throughout the remainder of Secchi's work on spectral classification, though it quickly became modified.

Secchi drew several conclusions from his first extensive cataloguing efforts. First, he considered the reality of his classification scheme, noting that it must be correct because he was able to classify so many stars into a simple and homogeneous system. He was also able to group statistically his types into different regions of space; those of type I tending to concentrate in constellations associated with the Milky Way. Secchi paid close attention to the stars of the "Orion" type and their association with nebulae. The similar colours of these stars and the nebula surrounding them in this subclass were of great interest. Secchi felt that the similarity was due to the fact that the stars were imbedded within the nebula.

Secchi discussed many instances of variation in line width and intensity in spectra within his types. In particular, he suggested that variations in the width and sharpness of hydrogen lines in his Type I stars might be due to differences in temperature and the abundance of hydrogen in the stellar atmospheres.<sup>33</sup> He also examined the band structure of his Type III red stars, which quickly led him to suspect a fourth type. Details on this were published in August, 1868:

> There is a complete opposition between the third type and this fourth type. The two spectra are not merely modifications of the same type they are due to completely different substances ...

Secchi's fourth type exhibited the Swan bands of carbon, which were correctly identified by him. They have been referred to as stars of class R and N, and were considered by Henry Norris Russell to be a parallel sequence of evolution to the K and M types (after work done by Rufus and Curtiss at Michigan in 1917).

The primary distinction in Types III and IV came in the structure of their spectral bands, and the existence of other apparently bright lines. In the fourth type, the bands fell off in intensity quickly on the blue side and gradually on the red side; while in the third type, the structure was reversed. It must be stated here for clarity that Secchi considered these features as bright, with dark spaces betwen them, an interpretation that persisted until the work of Fowler and Hale.

Secchi's detailed attention to line strengths among his stars of Type II has been interpreted by Hoffleit<sup>35</sup> as possibly the earliest instance where spectral criteria later found to be dependent upon luminosity were identified.

In following years, Secchi continued his observations but his basic scheme remained generally unchanged. At times, stars noted by Secchi as not belonging definitely to any one type were later classified by others as extensions to Secchi's system. In 1891, E.C. Pickering thus proposed a fifth type, consisting of stars of the Wolf-Rayet type, which Secchi had singled out in 1869.<sup>36</sup> Pickering's fifth type<sup>37</sup> included planetary nebulae along with the Wolf-Rayet stars. That it has been called Secchi's fifth type<sup>38</sup> differs greatly from the interpretation of the stars and their apparent association with Type IV by Secchi, which effectively put them at the opposite end of an order envisioned by Pickering.

Except for indirect references, and one instance where by implication stars were related genetically to nebulae, Secchi appears to have not formally considered a system of classification based upon

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an order of evolution.

One of the by-products of Secchi's research was a rudimentary temperature sequence.<sup>39</sup> Secchi was well known for his studies of the temperature of the Sun in the early 1860s, and maintained a keen interest in determining the physical properties of objects from a study of their spectral characteristics. In April, 1867, Secchi pointed out that the D line seen in red stars was very broadened over that found in the solar type stars:

> This broadening indicates that these red stars are enveloped in atmospheres of great absorbing power, whose nature will not be known until the chemists have separated in spectra just what belongs to the nature of the substance and what to its temperature.

From his own laboratory studies, conducted in the following year, Secchi was able to arrive at the conclusion that his types III and IV were at lower temperatures than the solar type stars.

By July, 1870, Secchi had applied his experimental findings to the stars, and thereby created a temperature sequence based not so much on colour as on line structure.<sup>41</sup>

Huggins, however, did not agree with Secchi's temperatures for the red stars. Huggins' own research on nebulae indicated:

That the colours of the stars are owing to the partial absorption by the vapours of their photospheres of some of those colours which are different from, or complimentary to, the colours wherewith they actually shine to us. From this it follows that a red star is not necessarily in a state of lower incandescence than a blue star. $^{42}$ 

Huggins more or less consistently held to this view, which we shall later examine in detail, and in the period 1891-1900 used it to align his concept of the order of stellar evolution with Lane's Law.

\* Huggins - 1866

Huggins' famous 1866 lecture entitled "The Results of Spectrum Analysis Applied to the Heavenly Bodies" posed the developing problem of the identification of the chemical origins of many of the lines seen in stars, but not identified on Earth:

> Some of them are probably due to the vapours of other terrestrial elements, which we have not yet compared with these stars. But may not some of these lines be the signs of primary forms of matter unknown upon the earth? Elements new to us may here show themselves which form large and important series of compounds, and therefore give a special character to the physical conditions of these remote systems.<sup>43</sup>

This non-identification was especially true in the spectra of the nebulae, as we have seen. To Huggins, the suggestion by William Herschel that the nebulae were the spawning grounds for stars greatly increased interest in these "... faint, cometlike masses ..."<sup>44</sup> Huggins compared his spectroscopic observations of the mebulae with the visual characteristics of nebulae resolved by Lord Rosse:

> Half of the nebulae which give a continuous spectrum have been resolved, and about one third more are probably resolvable; while of the gaseous nebulae none have been certainly resolved, according to Lord Rosse.<sup>45</sup>

Huggins posed possible interpretations of gaseous nebulae, considering them the confirmation of Herschel's "primordial matter". If this was not to be the significance of the nebulae, then "... what is the cosmical rank and relation which we ought to assign to them?" As an indirect clue, Huggins noted that the spectra of comets appeared to be similar to nebulae. Then he considered what should be observed if the nebulae truly were stars in the process of formation:

> If physical changes of the magnitude necessary for the conversion of the gaseous bodies into suns are now in progress in the nebulae, surely this process of development would be accompanied by marked changes in the intrinsic brightness of their light, and in their size.<sup>46</sup>

Huggins proposed methods of the measurement of the brightnesses and angular dimensions of the nebulae, specifying that these should be

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made carefully for comparison at a future date.

Huggins ended his address with several conclusions about the structure of stars, the elements found within them, and the origins of their colours, basically reviewing in outline statements made within his text. His final remark, however, regarding any associated theory of stellar evolution, was conservative:

> It may be asked what cosmical theory of the origin and relations of the heavenly bodies do these new facts suggest? It would be easy to speculate, but it appears to me that it would not be philosophical to dogmatize at present on a subject of which we know so very little. Our views of the universe are undergoing important changes; let us wait for more facts, with minds unfettered by any dogmatic theory, and therefore free to receive the obvious teaching, whatever it may be, of new observations.<sup>47</sup>

Few, including Huggins, truly followed this suggestion. Within the text of his lecture, no attempt was made to order the stars in any scheme of classification. Stars were discussed randomly, in the order Capella, Sirius, Vega, Solar type, Betelgeuse. After discussing the identification of known elements in their spectra, and hence in their atmospheres, Huggins turned to stellar colours.

He believed that light coming from the stellar interior was initially white, but became coloured through selective absorption caused by absorption lines. At the time, this caused Huggins to speculate upon the colour of Sirius, a star with few lines:

> This peculiarity, which seems invariably connected with colourless stars, ... invites speculation. May it be a sign of a temperature of extreme fierceness?<sup>48</sup>

As we shall see, by the 1890s Huggins was to answer this question in the negative.

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#### G. Johnstone Stoney -1867

One year after Huggins' address, G. Johnstone Stoney proposed

a theory of evolution that put red stars at the young end.<sup>49</sup> Stoney's hypothesis considered the effect of gravity upon the spectral appearance of a star. In brief, he felt that a redder star resulted either from reduced mass or "... from its being so dilated by heat that its outer parts are further removed from its centre ...,"<sup>50</sup>

Stoney's consideration of the increased radius of cooler stars over the hotter blue stars was the germ of his belief in the progression from red to blue. His colour change mechanism was more a result of the variation of the distillation of elements in the stellar atmosphere than a difference in temperature. Thus, his colour progression from red to white cannot be unambiguously classed as a heating, or as a contraction effect.

Through the late sixties and early seventies, Secchi's classification project continued, Huggins and Draper began to experiment with photographic spectra, and Lockyer, C.A. Young, and others began to discuss the detailed structure of the solar atmosphere. By 1870, "... the crude notion thrown out by Zöllner in 1865 that yellow and red stars are simply white stars in various stages of cooling ..."<sup>51</sup> was no longer accepted without reservation. According to Clerke, D'Arrest and Angstrom<sup>52</sup> both protested against the idea, the latter preferring changes in atmospheric quality and composition as the cause, rather than changes in age and atmospheric temperature. Nevertheless, by the mid seventies, H.C. Vogel followed Zöllner's lead and brought out an extensive classification system which had a strong evolutionary bias. At about the same time, Lockyer also proposed his first crude classification.

## Classification in the 1870s

Lockyer - 1873

Lockyer's Bakerian Lecture before the Royal Society in November, 1873, identified many aspects of his research, including his technique of "long" and "short" lines to detect trace impurities and facilitate line identifications, and his use of dissociation to discuss the structure of the solar atmosphere and the relative temperatures of the stars, the latter being based primarily upon Secchi's observations.<sup>53</sup> In this paper, Lockyer created a simple classification into three types based upon the degree of spectral complexity. Stars with strong blue continua, hydrogen strong, and thin metallic lines were called  $\alpha$ . Solar types stars were  $\beta$ ; and  $\gamma$ were the red stars. The channelled spectra of the red stars suggested to him that a process of "celestial dissociation" 54 was at work. In the red stars, temperatures were low enough to allow for compounds to In hotter stars, compounds would be broken into their constituent form. atoms, and in the hottest, even these atoms, unlike terrestrial experience, would be broken into their fundamental constituents. Lockyer also utilised his concept of dissociation as an indicator of stellar temperature, calibrated by laboratory experiment, but also provided a strong non-evolutionary prediction that composition differences might play a role in spectral differences. 55

To Lockyer, this dissociation mechanism was an additional source of stellar energy, since the heat required by dissociation would be given up as a star cooled and compounds formed. This was his only evolutionary consideration at the time.

## Vogel - 1874

In 1874, H.C. Vogel proposed a scheme of spectral classification similar to Secchi's, but differing in the very important respect that it

was based upon a general theory of stellar evolution. Vogel's basic argument was that:

A rational classification of the stars according to their spectra is probably only to be obtained by proceeding from the standpoint that the phase of development of the particular body is in general mirrored in its spectrum.<sup>56</sup>

Vogel created three "Types" distinguished by colour: white, yellow and red. Vogel's Type III included both Class III and Class IV of Secchi, and were designated IIIa, and IIIb. Vogel acknowledged that these two types were distinct, but that their degree of difference was too small to justify a separate numerical division.<sup>57</sup>

Vogel's rationale was to create a continuous sequence of spectra to represent cooling, from stars of the Sirius, Orion types, and bright line stars, through solar types, and thence to either IIIa or IIIb, depending upon composition.

The combination of Secchi's III and IV classes into two subdivisions of one type was criticised and commented upon by many in the following decades; it clearly represented a wholly different course of evolution from that which Lockyer was to propose. Through the eighties and nineties, the question was to centre around whether stars followed a linear single-valued temperature sequence as they aged, or whether they experienced similar temperatures at widely spaced stages of their development.

Vogel's recourse to evolution itself drew criticism in the seventies. In 1875, d'Arrest<sup>58</sup> doubted that the accepted view of cooling, starting from the white stars, could be correct because nothing was definitely known about how stars arrived initially in the white-hot stage. Another objection came from the still highly selective manner in which spectra were examined. Even though an extension of Secchi's work by d'Arrest resulted in over ten thousand stars being classified by 1875, and the work of Vogel also greatly added to this number, it was still quite certain that many "transition states" had been left unobserved that could very well have some affect on any scheme of classification. By 1880, the detection of these missing links was of great interest, and variations were constantly being suggested. One came from Henry Draper, who, writing to C.A. Young with examples of the spectra of Altair taken with his 28-inch speculum reflector remarked:

> ... It shows the lines in the background of the spectrum fairly ... and that picture clearly shows that there is a finer and (to coin a phrase) a more concealed spectrum of lines than in any star I have yet worked on. Vega does not seem to give the same. I do not want to speculate on the subject yet but it points to a curious transition state ...<sup>59</sup>

Another example, this time for the red stars, helps to illuminate how some classes of stars became of interest. d'Arrest and others had noted the rarity of IIIa and extreme rarity of IIIb classes which seemed to be paradoxical (on the basis that they were grouped together). Their rarity, and the general controversy surrounding their association, was stimulus enough to study them. This can be seen in a letter from E.C. Pickering responding to a letter from E.S. Holden in 1881 requesting further assistance in his studies of the nebulae:

> I hope that in your search for new nebulae, you will not overlook the very red stars. The reddish stars are plenty enough, but those having well masked banded spectra are rare and seem to form a distinct class. They are easily distinguished by a direct vision prism. Doubtless among them you would find some interesting variables.<sup>60</sup>

#### Classification in the 1880s

## \*\* Huggins - 1880

In 1880 Huggins reviewed the progress of his research, which, since 1876, had included the photographic study of stellar spectra. By 1880 he was ready to discuss a tentative scheme of classification

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with weak links to evolution. 61

Huggins' primary spectral criterion was the behaviour of the H and K lines calcium (now known as ionized calcium). In white stars, K was fainter than H, but in a star like Arcturus (classified with the Sun), K was broad and strong relative to H. This variation through the spectral types was continuous, and so Huggins decided that this would be his classification criterion - the visibility of the K line. Starting with near invisibility in the white stars, and progressing through the solar type stars, where it became stronger and started to show wings, the K line would increase in intensity finally to stars like Arcturus, where it far outgrew its neighbour, H.

At the time, Huggins did not know that the relative behaviour of the K and H lines was due to the blending of H with a line in the hydrogen series (H  $\epsilon$ ) which had not been observed as yet. It was unambiguously identified in 1882 only when Huggins photographed it in the spectrum of the Orion Nebula.<sup>62</sup>

The remainder of Huggins' text discussed the spectra of specific stars representing the white stars, solar stars, and provisionally, a redder class, though no specific mention was made of this third type of star existing as a distinct class from his photographic work.

In an addendum to the paper, dated March 10, 1880, Huggins included further observations. Within it, five stars were discussed, including Betelgeuse, as an example of a red star whose spectrum was extremely difficult to photograph.

Of course, at the time, photographic emul tions were insensitive to the red, and so only the brightest of that class could be spectroscopically photographed.

Huggins made a passing reference to evolution, in discussing

Capella:

The great interest of this star in connexion with the researches contained in this paper is that it appears to be a sum in the same stage as that in which our sun is. $^{63}$ 

Huggins thus equated spectrum with stage of development, but expressed considerable caution:

> Whether the order of change from the more simple typical spectrum in which these researches show that the stars may be arranged, also indicates some of the successive stages of their life changes through which they pass is a point on which we know nothing certainly.<sup>64</sup>

This did not prevent Huggins from suggesting an order of development, which started with the white stars Sirius and Vega, progressed through Spica, Altair, Rigel, Deneb, and onto Capella and the Sun, and then to Arcturus, Aldebaran, and Betelgeuse. This order, identical to Vogel's, was a cooling progression. Huggins conceded, however, that it was possible that the order of development could be the inverse from red to white - as put forth by Stoney in 1867.

Huggins clearly was not ready to depend upon evolutionary considerations, but held them definitely in view.

# \*\* Lockyer's Classification 1888-1890

Lockyer's classification was based upon temperature criteria developed from his dissociation theory, and observations of nebulae interpreted as swarms of meteoritic material.

In 1887, he had noted "... that the nebulae are composed of sparse meteorites, the collisions of which bring about a rise in temperature sufficient to render luminous one of their chief constituents magnesium ..."<sup>65</sup> This identification was based upon the examination of the flame spectrum of magnesium found in meteoritic material examined in Lockyer's laboratory. In following years, this identification was disputed by Huggins, Keeler and others associated with Huggins, and by

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the early nineties, seemed to be decided conclusively against Lockyer's identification thus making his theory of meteoritic nebulae unacceptable to most<sup>66</sup> even though many, including George Darwin and William Thomson, had utilised meteoritic phases in discussing the past history of the Sun.

During 1887-1888, the connection between Lockyer's interpretation of the meteoritic nature of nebulae and his growing concept of the course of stellar evolution was clear, but since he had not yet begun his own observations of stellar spectra, statements by him in 1887 needed verification, as he noted to E.C. Pickering, in a letter asking for aid:

> By this time you may have seen a paper which I have sent in to the Royal Society on the spectra of Meteorites & of stars generally. I am anxious to know whether without interfering with any of your proposals as to publication & discussion of your observation you could send me any spectra of stars of the solar type more or less, but with considerable variation with reference to the intensity of H & K. I have thrown out a suggestion in that paper which it is important to verify as soon as possible, & if you would do it yourself so much the better. The suggestion is that we may have many lines in the spectrum of a star on both sides of the maximum of temperature, & I am anxious to see if there is any definite criterion.

Your remark in your last letter that you have already the spectra of 27,000 stars quite makes one's mouth water, & it would be quite worth a pilgrimage to Boston if one could see them when one got there.<sup>67</sup>

In an 1887 paper, Lockyer had already provided a tentative temperature arch<sup>68</sup> and now needed confirmatory observations. Pickering's spectra, however, were not appropriate for the test, for in reply he mentioned that most of his spectra would not show sufficient detail. "They serve to indicate the general class as to which a spectrum belongs rather than the details of the structure ..."<sup>69</sup>

Nevertheless, Lockyer maintained his scheme in 1888, and felt that the connection between his meteoritic nebulae and stars was direct:

... it is ... possible that many stars, instead of being true condensed swarms due to the nebulous development ... are simply appearances produced by the intersection of streams of meteorites.<sup>70</sup>

Turning to the regular forms of nebulae, those which seemed to suggest action toward some centre, Lockyer believed that the increase of brightness with decreasing radius was due to an increase in meteoritic collision, with the end result the formation of a nebulous star. Nebulous stars were represented by bright line spectra, where the condensation had progressed enough to raise the temperature somewhat, but was still in a range comparable to low nebular temperatures. He then turned to laboratory evidence, which showed the temperature and pressure dependence of carbon and hydrogen. Lockyer predicted that the appearance of one or another of these elements, or the non-appearance of either was a measure of the degree of condensation of nebulae. The least condensed would show no hydrogen at all. Then, with condensation, hydrogen would appear, alone at first, and then subsequent condensation would bring the carbon up together with the hydrogen. As an example of this third stage, Lockyer used Betelgeuse, within which bright lines and bands of hydrogen and carbon were believed to exist.

He continued his discussion of objects in various degrees of condensation, passing through Vogel's IIIa types stars, to solar types, and so forth.

Part II of his lecture began with an extensive review of former classifications, which included Rutherfurd, Secchi, and Vogel. In discussing Secchi's work, Lockyer introduced his own early work on the temperature dependence found in the progression of spectral types, and how it compared to Secchi's.

Lockyer then criticised Vogel's decision to include Secchi's types III and IV into one type. Lockyer's arguments here were, of course,

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based upon his feeling that the IIIa and IIIb stars represented almost totally opposite ends of the evolutionary spectrum. By 1888 Lockyer was prepared to discuss subclasses in his various groups - classes that represented transition states and criteria for noting rising and falling temperature. Also by 1888, he had distributed the various Vogel types into seven groups.<sup>71</sup>

These seven groups were compared to Vogel's classes in a "Council Note" in 1889.<sup>72</sup> We have incorporated this comparison into Table 1.

His development of criteria for determining the degree of condensation of a body, and whether it was to be placed on his rising or falling temperature branches, were extensively discussed in his 1888 Bakerian Lecture, and his book, <u>The Meteoritic Hypothesis</u>, which appeared in 1890.

Stars on the descending branch were far more condensed than those on the ascending branch. Thus, the degree of condensation was primarily sought from spectral features, as interpreted on his meteoric model. But Lockyer's model had meteoric characteristics only on the ascending branch, and so criteria for determining the relative degree of meteoric and gaseous cross-sectional area would not work as a differentiating test. He thus resorted to line spectra:

> The general conclusion to be drawn from the observations is that there are several lines in the spectra of stars on the ascending side, of temperature curve, which do not occur in stars with a spectrum resembling that of the Sun, which must lie on the descending side of the curve, as we know it to be cooling.<sup>73</sup>

The fascinating question here is: How did Lockyer ascertain that the Sun is cooling? Lockyer had not publicly acknowledged Lane's law, though he was aware of it at the time (1890). Meadows has discussed<sup>74</sup> the fact that Lockyer's temperature curve was similar to Ritter's, but

Lockyer preferred to resort to temperature criteria based upon dissociation rather than upon physical theory. The gaseous spectrum of the Sun convinced Lockyer that it must be on his cooling branch, a conclusion enforced to some extent by a subsequent discussion of the continuous gradation of colour seen in the groups representing his temperature arch.<sup>75</sup>

Possibly the greatest single difficulty encountered by Lockyer in his classification scheme based upon evolution was to convince others that nebulae condensed into red stars, and not white stars. Central to this was whether the association of the chief nebular line with magnesium, as seen in Lockyer's laboratory and interpreted as a condensing swarm of meteoritic material, was more viable than the arguments supporting association of the white stars with nebulae. The latter association, as we will point out in the next section, was possibly the most persuasive in establishing the ultimate order of the classification system generated by the staff of the Harvard College Observatory.

### Classification in the 1890s

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## The Development of the Harvard System

The origins of the Henry Draper Memorial have been reviewed recently.<sup>76</sup> Our examination of the influence of evolution will begin with the earliest Harvard systems, and end with the culmination of the early phase of that classification, around 1900. In all stages of development of the system, the influence of the Harvard director, E.C. Pickering, will become evident, though it will also be apparent that his female staff held strong interests in using evolution to create classification schemes.

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# The First Classification - 1890

The first extensive catalogue appeared in the Harvard Annals

in 1890<sup>77</sup> with a short introduction, presumably written by Pickering. It comprised over ten thousand stars and included stars north of -25 degrees declination. In the next year, a comprehensive history and discussion of the classification appeared, establishing it in honour of Henry Draper.<sup>78</sup> The general philosophy of the project was established, however, in the 1890 report. First, it was acknowledged in the "Preface" that all previous systems of classification were insufficient to be able to account for the great diversity seen amongst the spectra examined, though when grouped by principal characteristics, the system of Secchi's basic four types still held. Pickering therefore described his scheme in terms of Secchi's.

Pickering created an alphabetical classification, starting with A, which was clearly based upon the appearance of certain groups of lines; notably the H and K lines and the hydrogen series. His search for line continuity was not his only criterion though, for he pointed out in several places the fact that stars with certain spectra seemed to be associated spatially.<sup>79</sup>

Pickering's spatial association of type is interesting to consider. Basically, evolution can be divided into two forms, the growth or evolution of one object - a star, or the evolution of a system of objects, such as a cluster of stars. Direct visual evidence gathered since the time of William Herschel had provided the germ of thought that these systems must be coval, and Pickering's personal work had already aided this concept. In 1886, he did a spectroscopic study of the Pleiades, and showed that the spectra of the stars in this cluster were all quite similar. In a review of his work this similarity was considered "... a circumstance which seems strongly to confirm the idea of a community of origin ...."<sup>80</sup>

Pickering's attention to nebulae throughout the eighties

brought him to the bright-line stars. He continually received advice and encouragement in this direction, from John Herschel in 1883, and from C.A. Young in 1885, who commented about stars that, upon closer examination, turned out to be nebulae.<sup>81</sup> Pickering's association of red stars with nebulae, in his 1881 letter to Holden,<sup>82</sup> was an idea in agreement with Stoney and of course with Lockyer, who was by the late eighties writing to Pickering asking for spectra that might help confirm his views.

In reviewing his general system of classification, Pickering compared it with Secchi's, to which Pickering had added a fifth type, for bright line stars and planetary nebulae:

... A, B, C, and D indicate varieties of the first type, E to L varieties of the second type, M the third type, N the fourth type, and O, P, and Q spectra which do not resemble those of any of the preceding types.<sup>83</sup>

It should be noted that Secchi's IV stars, classed as N by Pickering, did not appear in the catalogue because they were all too faint. Pickering's fifth type, into which the classes O, P and Q were lumped, created a discontinuity in colour, which did not seem to bother him. At the time, then, we might conclude that evolution did not play a visible part in Pickering's classification.

Pickering's first discussion was put together in some haste, and he wished to note repeatedly that fuller commentary would be available soon. He did note that Mrs. Fleming's role in the first classification was extensive, but reviews of the work felt that she deserved a more prominent role, and thus identified the classification as "Pickering-Fleming".<sup>84</sup>

In the following year, <u>Volume 26</u> appeared, wherein Pickering did provide greater exposure for Mrs. Fleming's work. This new volume did not produce any new catalogue entries, but it did serve as a general

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introduction to the Henry Draper Memorial.

The chief statistical inference brought out in this volume was that Secchi's Type I stars appeared to be highly confined to the plane of the Milky Way. Pickering concluded from this that the Milky Way was therefore a body comprised of stars different from our Sun, and that its age and composition was far different from ours. He also found that stars of the Wolf-Rayet class were highly confined to the Milky Way plane. Thus an apparent paradox existed - that stars at opposite ends of his classification were associated in position. Further, since novae normally appeared in the Milky Way plane, they became linked somehow to Type I stars, and also to bright-line stars. We will soon see how these associations caused the Harvard Classification to change.

## Reactions to Pickering's Original Classification

In August, 1893, a "Congress of Astronomy and Astro-Physics" was held in Chicago organised by George Ellery Hale. Pickering had been invited to this Congress by Hale, who asked that Pickering "... present a paper on some subject of your own selection. General conclusions as to stellar constitution and evolution as learned from the work of the Henry Draper Memorial would seem a very suitable subject, but on this point you are best able to judge ..."<sup>85</sup> Pickering replied three days later that he would discuss the work of the observatory, without speculative elaboration. By May of 1893, Pickering had decided that his schedule couldn't permit the trip to Chicago, which disappointed Hale, but Pickering was still going to prepare a paper. The paper eventually read appeared in print later in the year, and provided insight into the growth of the Harvard Classification system, and its physical interpretation. The paper, titled "The Constitution of the Stars" contained a few clues to evolution, even though, by the above

request, Pickering had certainly been given the opportunity to expound on the subject, if he cared to do so. The only direct statement read:

> A careful study has been made by Mrs Fleming of the fainter stars, and of the brighter stars by Miss A.C. Maury. From this it appears that while at first sight many spectra seem to be unlike, nearly all of them can be arranged according to a simple system. It is not proposed in the present paper to consider the cause of these differences. For purposes of description, it will be convenient to treat them as if due to differences in composition only, although there is evidence that the actual variation is rather in the order of growth.<sup>86</sup>

# \*\* Lockyer's Reaction

Several years later, in a general review of Pickering's work, Lockyer felt that, indeed, he had made an explicit evolutionary statement. Admitting that Pickering had made no statements regarding temperature, Lockyer believed that "... he distinctly accepts the idea of evolution, or what he terms 'an order of growth'".<sup>87</sup> Lockyer then quoted Pickering:

> In general, it may be stated that, with a few exceptions, all the stars may be arranged in a sequence, beginning with the planetary nebulae, passing through the bright-line stars to the Orion stars, thence to the first type stars, and by insensible changes to the second and third type stars. The evidence that the same plan governs the construction of all parts of the visible universe is thus conclusive.<sup>88</sup>

This was Pickering's last statement of his paper. It will be noted that in our previous quotation from Pickering, his use of the term "order of growth" was highly restricted, and used only as an example of one possible cause out of many of what appeared to be a simple system. Lockyer did not mention this qualification, and therefore presented Pickering's quotation out of proper context.

Lockyer also did not point to the fact that Pickering's evolutionary scheme did not follow his alphabetic progression. Here,
it would read: P - O - A - B, etc., though Pickering himself preferred to abbreviate his progression by the use of Vogel's or Secchi's types.

### \*\* <u>Maunder's Reaction</u>

Pickering's association of spectral differences with differences in composition could have been stimulated by the opinions of E.W. Maunder, who had several times in the past written to Harvard for spectroscopic data, and who reviewed the question of the association of spectral changes with stellar evolution in 1891.<sup>89</sup> Maunder's rather remarkable paper reviewed Secchi's classification and then commented that "... It was very natural that so soon as this classification was recognised, these several types should be interpreted as representing different successive epochs in the life history of a star ..."<sup>90</sup> The Sirian type was considered to be the youngest and of the highest temperature, as Maunder recalled. From this Maunder stated:

> With this idea, it has been very customary to speak of the Sirian stars as being on the average much larger than stars resembling our sun in spectrum. Thus the late R.A. Proctor, writing in reference to them, says, "the stars belonging to this type are certainly in many cases, and probably in all, very large 'orbs'," and he often spoke of them as 'giant suns', a practice in which many other writers have imitated him.<sup>91</sup>

This fascinating use of "giant" has been searched out in Proctor's books, but as yet, to no avail. And his statement that the term was in common use also bears need of confirmation. But, beyond the significance of the use of the term, Maunder reasoned that if these stars were actually larger, they should be among the most luminous stars (intrinsically) in the sky. From parallaxes derived by Elkin at Yale, and magnitudes from Oxford's "Uranometria", Maunder found the relative brightnesses of 9 Sirian stars and 13 solar stars, on the basis that the Sum's brightness equalled unity. His tabulated brightnesses clearly showed the solar stars to be superior, on the average. This today can easily be understood by the fact that Maunder's list of bright solar stars contained giants like Arcturus, Capella and Aldebaran, though his listing was so small and based upon such meagre data, that we can only regard the result as fortuitous.

Following this apparent refutation of the normal course of evolution, Maunder then used binary star data to show that Sirian stars were the less dense of the two, which would follow if the progression from Sirian to solar types was one of condensation. This brought out the apparent paradox of the less dense stars being also the smaller of the two, which was indeed one of Maunder's conclusions: "... it is the solar which have the better right to be entitled 'giant stars'".<sup>92</sup> Again, Maunder's discussion was fortuitous, for in order to determine relative densities, he assumed that the surface brightnesses of both classes was the same. A similar approximation had, interestingly enough, been used by Pickering in 1880 in an extended discussion of the photometric determinations of stellar diameters.<sup>93</sup> Pickering, however, expressed his belief that the assumption was poor, though Maunder made no mention of this.

Maunder also resorted to several other arguments to state his case. Among them, Pickering's study of the small spread in spectral class in the Pleiades was considered to "most conclusive".

> Forming, as they manifestly do, a real group, and therefore lying all, practically, at the same distance from us, and embracing amongst their number stars of a great range of magnitude, we see they must be of very different sizes. Yet we find practically but one type of spectrum.<sup>94</sup>

From this almost accidental statement of the fundamental character of the upper Main Sequence of the HR Diagram, anticipating Hertzsprung by 15 years, <u>(although at the time hardly convincing due</u> to the poor data and gross assumptions Maunder then concluded: 102.

Is it reasonable to suppose that throughout the group the smaller stars are just so much younger in actual interval of time from their formation than the larger, that smaller size has been exactly balanced by shorter time and that in this way the entire group preserves to us as appearance of uniformity? Is it not much more natural to suppose that they all show the same spectrum, because, forming one group, they contain the same materials and in similar proportions?<sup>95</sup>

Maunder's conclusions applied to stars of the first two Secchi types, and in his opinion were certainly open to revision based upon better data, derived from work like Pickering's. He mentioned that Lockyer's classification could very well account for several objections he had raised, but this was in passing only, without elaboration. Finally, he considered stars of types III and IV too poorly studied at the time to comment upon.

Pickering's mention of composition as the independent variable was to disappear in the next decade.

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### Frost and Scheiner's Reactions

Aside from explicit reactions to the Harvard Classification, it was clear that most spectroscopists welcomed the fact that for the first time, a great number of stars had been examined systematically. Not everyone was optimistic regarding the value of the project. Edwin Frost noted in his translation of Scheiner's <u>Astronomical Spectroscopy</u> that

> Scheiner dissents very strongly from the favorable opinion of the Draper Catalogue expressed by the translator, and declines to accept as conclusive any inferences which may be based upon it.<sup>96</sup>

Frost, of course, was very much in favour of the new catalogue and classification which, when completed for the southern hemisphere would, for the first time, allow for a reasonable discussion of the distribution of the various spectral types in space. Frost provided an extensive discussion of the many implications to be drawn from the

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Draper Catalogue, which we will review eventually.

# \*\* Fowler's Reactions

Alfred Fowler, one of Lockyer's most illustrious assistants, reviewed the Draper Catalogue in 1892, and gave it good marks, except that he wished that more information had been provided about the spectra themselves.<sup>97</sup> Fowler also expressed disappointment that, though Pickering chose to compare his system with several others, no attempt to discuss it in terms of Lockyer's scheme appeared. Fowler, however, expressed gratification on behalf of Lockyer; noting how Pickering's work on Wolf-Rayet spectra helped to associate these stars with planetary nebulae as the first stage of condensation out of them, inferred from the lack of the chief nebular line at 5007A.

... This, Mr. Lockyer explains, is due to increased temperature, and this view is strengthened by the fact that the line was seen only during the later stages of the visibility of Nova Cygni ...<sup>98</sup>

Finally, Fowler pointed out that Pickering's assumption of the association of Type I stars with the Milky Way does not mean that it forms a system excluding the Sun:

> ... The lines in the spectra, so far as we know them, indicate the same substances in each, and the tendency of evidence is to show that the sun is a type of what the stars of the Milky Way will become ...

An assumption like the above might have been too explicit and dependent upon evolution for Pickering. It should be noted, however, that in this case, whether one took the conventional evolutionary direction, or Lockyer's double valued one, the interpretation by Fowler would have come out the same.

By 1891 it had become apparent to Pickering that several classes were of doubtful reality. He therefore suggested tentatively that classes E and G were really identical, as were H, I and K.<sup>99</sup> This

short remark, however, didn't warrant the conclusions drawn by the Council of the R.A.S., who reported in 1893<sup>100</sup> that within <u>Volume 26</u> (they unfortunately left a typographical error in the report that described it as <u>Volume 27</u>) Pickering had simplified his system. Though this was to be done eventually, it hadn't actually transpired at the time of the report, and certainly did not appear in <u>Volume 26</u>, which was meant to be nothing more than an introduction to <u>Volume 27</u>, which, of course, had already appeared with the original classification.

In fact, no simplification of the general system appeared until 1897, when Mrs. Fleming dropped several of the least definite letters after her work on the spectra of clusters.<sup>101</sup> The original order remained, however.

In the same year, a far more significant volume appeared which occupies a central position in our discussion, the work of Antonia Maury on the "Spectra of Bright Stars".<sup>102</sup> This work was of a completely different nature from that of Fleming, being a critical study of fewer stars, utilising higher dispersion, and far greater attention to detail.

### The Classification of Antonia C. Maury - 1897

Fowler's wish for a more detailed classification from better spectra was, to some extent, satisfied by Miss Maury's scheme. She examined detailed line structure of stars whose spectra were produced by three thick objective prisms ganged in front of the Harvard telescopes.

This task had been delegated by Pickering in 1888, and the result, published in 1897, did not meet with full approval from the Director, who noted in the Preface: "... she is alone responsible for the classification contained in Part 1 of this volume ...."<sup>103</sup>

Antonia Maury, of all the women working with Pickering,

maintained a high degree of originality.<sup>104</sup> She was the niece of Henry Draper, and had the advantage of studying chemistry and mathematics at Vassar.

About 4800 photographs of the spectra of 681 bright stars were examined by comparison with chosen standards. Miss Maury found, with the greater dispersion of her spectra, that classification had to involve not only the positions of spectral lines, but their character or appearance on the plate. This second consideration, most certainly a refinement, required closer attention to instrumental effects:

> ... Care was necessary therefore, not to confound appearances due to the want of an accurate focal adjustment with those characteristic of particular stars. When the focal adjustment was unsatisfactory, the edges of the lines are ill defined; but when it is improved, when these edges become more definite, the line itself often remains comparatively indistinct, and having relatively little contrast with the remainder of the spectrum. Such lines, in the course of the present treatise, will be described as 'hazy'.<sup>105</sup>

Here is one element of disagreement between Maury and Pickering. While Maury based a classification on this hazy characteristic, Pickering remained sceptical of its reality, believing that most of the haziness was due to instrumental effects.

In discussing the general philosophy of classification, Maury noted that, as technique advanced, subgroups and transition classes to account for intermediate varieties became needed "... so that the entire number of spectra observed could be arranged in a series, which has usually been regarded as exhibiting more or less distinctly a course or plan of development ....<sup>106</sup> Maury then proceeded to discuss the method of classification:

> As usual, the stars were arranged in an apparently progressive series, which in the present case was made to include twenty-two groups, excluding composite spectra ... and also those in which bright lines were the most important feature. But it also appeared that

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a single series was inadequate to represent the peculiarities which presented themselves in certain cases, and that it would be more satisfactory to assume the existence of collateral series. These are called "Divisions". They pursue parallel courses of development through at least many of the groups employed, as above stated, to represent stages of progress ...<sup>107</sup>

This significant statement, announcing explicitly, on the basis of spectral differences, the existence of two distinct divisions of stars of the same general spectral class, formed the basis of subsequent work that produced the technique of spectroscopic parallaxes. We can, in addition, find ingredients in Miss Maury's divisions that resembled to a degree what Lockyer was trying to say when he developed criteria for differentiating stars of rising temperature from stars of falling temperature.

Maury's chief division was labelled 'a', and included 355 stars out of the 681 studied. These were normal clear-line spectra without evidence of poor continuum/line contrast or haziness as mentioned above. The next division was 'b', where all lines were relatively wide and hazy. Fewer faint lines were visible in these stars, due to the haziness of the lines, but it was important to note that in this division, relative line strengths were the same as in division 'a'.

Division c showed a marked change in characteristics. Hydrogen lines were narrow and well defined, and generally less intense than those found in divisions a and b of the same spectral class. Orion lines were narrow, well defined and "... are still discernible in this division in some groups early in the series from which they have disappeared in Divisions a and b ....<sup>108</sup>

In discussing the manner in which spectral lines changed from group to group, Maury used the phrase "... general course of development ...<sup>109</sup> which might have been meant to imply a course of evolution, though if this was the only reference, the implication would have been weak. Another term implying evolutionary considerations was "early" and "earlier" in reference to the spectra of type I stars relative to types II and III. After a general discussion of the progression of line structure, she commented:

> The nearly constant relation of the decreasing intensity of the lines of hydrogen to the increasing intensity of the lines of calcium and solar lines is probably the most important law in the sequence of stellar types.<sup>110</sup>

There can be no doubt that statements such as this one later provided the most powerful argument for the standardisation of spectral classification schemes on the Harvard system. If this statement was made without any mention of evolutionary considerations, it would certainly stand as a purely empirical system. But such was not the case, as we have already implied, and will now show.

After discussing her twenty-two groups, and the distribution of divisions among them, which was believed to be incomplete due to brightness limitations, Maury then returned to evolution:

> While it will be generally admitted that the series represents successive stages in stellar evolution, it may still be doubted whether the arrangement beginning with the Orion type, and here adopted, is in fact the natural order. It is strongly indicated, however, by the gradual falling off of the more refrangible rays in the successive groups, by the corresponding increase in the less refrangible rays, and by the occurrence of marked absorption at the close of the series. The comparative simplicity of the Orion spectra and the increasing complexity shown throughout the series lend additional weight to the argument. Finally, the prevalence of the Orion type in great nebulous regions, and in Orion and the Pleiades, indicates very emphatically that stars of this type are in an early stage of development.111

Her justification of the direction of evolution is most interesting to consider - the variation of continuum radiation as one advances through the groups. Today, this is believed to be a temperature change. Indeed, the Harvard system has always been linked with a temperature sequence; in some cases, to the extent of believing that the temperature progression was a primary consideration in the development of the system itself.<sup>112</sup> This dependence upon temperature seems to be historically incorrect, as far as the intention of the originators of the system were concerned. The statement by Maury above about the change in continuum radiation is the closest one yet found in her discussion that could be construed to imply temperature considerations. It must be recalled that at the time, great doubt existed regarding the physical state of stars although the correlation of temperature and spectrum always remained likely.<sup>113</sup>

The connection of nebulae to the sequence of Miss Maury's groups was a tentative one at best, especially due to her limitation of the progression of types to Secchi's types I to III only. At the end of her general discussion, however, she commented:

> It is ... a matter of great interest that the bright line stars are found at the beginning and at the end of the series, and that one class of them probably connects the series with the nebulae.<sup>114</sup>

The association of nebulae with the Harvard Sequence was one primary element in the revision of the sequence itself by Cannon and Pickering, to which we now turn as the last stage of the early development of the Harvard system. In the following discussion of Cannon's work, we will also see how Pickering regarded Maury's scheme, and the reality of her "Divisions".

# Annie J. Cannon - 1896-1901

Miss Cannon followed the Pickering-Fleming system, in preference to Maury's. She was assigned to the study of the spectra of all bright stars south of declination - 30 degrees. By 1900, she had classified 1122 stars, and this was published, with discussion primarily written by her, in the following year.

In an "Introductory Note", Pickering reviewed the history of the three major projects within the Henry Draper Memorial: the systems of Fleming, Maury and, now, of Cannon. He noted: "In all three cases, it was deemed best that the observer should place together all stars having similar spectra and thus form an arbitrary classification rather than be hampered by any preconceived theoretical ideas, or by the previous study of visual spectra by other astronomers ...."<sup>115</sup> The truth of this statement is questionable, but its presence certainly must be considered important as representing what Pickering wished to be the case.

After initial comments on line criteria, Cannon discussed Maury's classification subdivisions:

> Partly from the fact that so small a proportion of the total number of stars classified has been photographed with more than one prism, it was found inexpedient to make the divisions "a", "b", and "c" as given in Part I of this volume.<sup>116</sup>

Cannon continued to state that there are "... doubtless great differences in the width and sharpness of the spectral lines ..." and then gave several examples. But, even though she admitted that "great care" had been taken to determine whether these differences were true or instrumental, the Maury divisions were not retained.<sup>117</sup>

The reversal of classes A and B was rationalised in terms of line spectra:

The evidence that the Orion spectra precede the Sirian is as good as that the Sirian precede the solar. The gradual decrease in the intensities of the Orion lines is accompanied by gradual increase in the hydrogen lines, and by the incoming of faint solar lines, so that in spectra of Classes B8A and B9A, solar and Orion lines are comingled. Hence it was necessary either to interchange the letters B and A of the Draper Catalogue or to place the letter B before the letter A. The first alternative would prove confusing. The second presents no real difficulties since the letters are merely symbols to express an observed condition.<sup>118</sup>

While from this we see that the inversion was based upon the behaviour of the 'Orion' lines, the case for the placement of the O Class before the B involved another consideration in addition to line structure: "A few spectra of the Orion type were found which clearly precede those of the class called B ..."<sup>119</sup> It was in these few spectra that Cannon found similarities with the spectra of Pickering's fifth type, those showing bright-line spectra.

The consideration in placing the O stars before the B stars, beyond simple line structure, was that similar lines in the two classes appeared first bright, then dark, as if, following Kirchhoff's law, one were to examine a radiating gas producing a bright line spectrum, and then, while the viewing was in progress, a hotter incandescent source was placed behind the original gas. The implication was strong, linking nebulae to stars by line structure alone, for in those stars in the O class where similarity of line structure was found with dark-lines in B stars, the chief nebular line was also present at 5007. It is of interest that at this time Cannon left the P class out of the general picture, though from time to time, in the future, it was to be placed before the O class.<sup>120</sup> Miss Cannon accepted the usual direction for evolution, but:

> ... The order of the development is not indicated, and the series might proceed from Class Mb to Class Oe, instead of from Class Oe to Class Mb. The latter seems more probable, perhaps owing to its agreement with Laplace's theory of stellar development ...<sup>121</sup>

Evolution played less a part in Cannon's classification than in Maury's,

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as far as an examination of their discussions in the <u>Annals</u> reveals. It is possible that their true feelings are masked by manner of presentation; Maury detailing closely an extremely sophisticated system and Cannon holding closer to the primary aim of the Draper Memorial. It must also be realised that the state of agreement on any one theory of evolution, or, indeed, on the most general characteristics of evolution, such as direction or the nature of the change in physical state, was certainly not constant during the time Maury did her work (1888-1897) and during Cannon's initial period of activity (1896-1900). Indeed, the acceptance of the nebular hypothesis changed greatly during this period, which saw the development of the hypothesis of Chamberlin and Moulton.

In the decades following her completion of the first Catalogue, Miss Cannon continued to classify a vast number of stars on essentially the same system as outlined here. The question of the association of this system with physical considerations, such as evolution or temperature was a central one, especially at several of the early International Solar Union conferences that were held prior to World War I. These discussions, insofar as they deal with evolution, will be reviewed within their chronological and topical context.

#### The Further Development of Lockyer's Scheme in the Nineties

After Lockyer's first classifications in 1874 and 1888, he decided to commence an observing programme to gather spectra of stars using two six-inch astrographs with objective prisms, and also a conventional slit spectrograph in conjunction with his 30-inch reflector. In this section, we will review his refined classification that appeared in 1892, based partly upon these observations, and continue through the nineties with an examination of later revisions and the roles of evolution and dissociation in his work.

During the period 1889-1891, while observations were being collected, Lockyer continued to interpret both his classification scheme and the various orders of celestial phenomena, in terms of the Meteoritic Hypothesis. Almost without exception, his work prior to 1890 was embodied in his book, the <u>Meteoritic Hypothesis</u>.

In late 1892, Lockyer presented an extensive re-discussion of his classification scheme, based now upon his own observations of stellar spectra. This new discussion offered very little new material over his original discussions, and most probably was a vehicle to place both observations and induction under the same heading.

It was his intention, as he stated in his introduction, to see if "... the hypothesis founded on eye observations is also demanded by the photographs  $\dots^{122}$  While he considered the observations thus far collected  $\angle \overline{4}43$  photographs of 171 bright stars 7 to be sufficient to re-examine "... most of the crucial ..." points of his hypothesis, he did not feel that they formed a definitive set. Since his spectra were of relatively high dispersion, the objective prisms being far thicker than those in use at Harvard, Lockyer had in hand a set of detailed spectra showing many lines not seen or examined by him visually.

He examined in detail the line structure found in his types. As opposed to his 1888 <u>Bakerian Lecture</u>, none of this was presented from the standpoint of the meteoritic hypothesis, but on the basis of line structure alone, very much in the spirit of the Harvard classifications.

Lockyer found that, for his early-type stars, "... all of the principal lines of the solar spectrum can be made out ...",<sup>123</sup> but were thinner than those in the Sun. Further, he observed an interesting opposing behaviour of the metallic and hydrogen lines: The metallic lines appear to be stronger when the hydrogen lines are finer and <u>vice</u> <u>versa</u> ...

This significant observation, later to be a prime luminosity criterion in the spectroscopic parallax technique of Adams and Kohlschutter, was not ignored by Lockyer. He used this line characteristic to create (or re-confirm) his double valued temperature arch. Similar distinctions of relative line strengths were also used in his line criteria subdivisions. Another temperature criteron was the presence or absence of continuum absorption in the ultra-violet. In all of his work, he was keen on equating variations in line strengths to what was seen in sources at different temperatures in his laboratory spectra.

Upon completion of his discussion of the observed line intensity differences that identified his subclasses and subdivisions, Lockyer examined their continuity and immediately identified two series of spectra:

> One important fact comes out very clearly, namely, that, whether we take the varying thicknesses of the hydrogen or of the lines of other substances as the basis for the arrangement of the spectra, it is not possible to place all the stars in one line of temperature ...<sup>124</sup>

He thus launched into an extended discussion of these two temperature series, and felt that specific criteria were available for all but the highest temperature stars. The historical fact was that as long as the majority of lines in these hottest stars remained unidentified, no comparisons with laboratory studies could be made. After 1895, and the work of Ramsay, this situation was to change with the identification of terrestrial helium. After his description of the temperature sequence, he examined how his new criteria fitted his Meteoritic Hypothesis, and concluded that in this new study, continuity and consistency with the meteoritic mechanism was preserved. In summary, his 1892 revision retained the basic features of his original work, since most of the determinations of order were still based upon the variations of lines and bands whose origins were still in question. Much of this was to change by 1900.

#### Reactions to Lockyer's "Revised" Classification of 1892-93

Two days after Lockyer had read his paper before the Royal Society, Huggins wrote to Hale outlining suggestions for programmes to be attempted at the new planned Yerkes Observatory. Hale, always sensitive to the experience of his elders, commonly requested such advice, but hardly expected to receive this commentary in Huggins' reply:

> ... But after all what is the use of your working!, there has just come out from S. Kensington a long paper on stellar spectra, which proves absolutely the meteoric hypothesis, the magnesium origin of the nebular lines, and the whole box of old tricks over again!!

**!!!** Surely now all other observatories may close their shutters!<sup>125</sup>

James Keeler's reactions were confined to elements of Lockyer's paper that were "... the subjects of more or less controversy ...."<sup>126</sup> Keeler questioned many of the line criteria used by Lockyer, noting in several places that both the line and continuum criteria he had developed could not be put to use with the spectra he had produced, which were too highly confined to a small photographic region and whose spectral sensitivity was unknown. At the conclusion to his review, he made an important observation:

> It will be observed that the stars considered in the memoir, or rather the stars whose spectra have actually been photographed at Kensington, are those which present the fewest difficulties to other systems of classification ...<sup>127</sup>

This statement was clearly in response to Lockyer's claim of being able to produce almost a continuous and perfect series based

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upon his theory, which he used, of course, to further his cause.

In the 1898 revision of Scheiner's work, both author and translator made only bibliographical reference to this paper by Lockyer, the preferred standard system being Vogel's. Frost did offer some additional discussion of the implications of Vogel's system, which was based upon a single temperature series of descent alone. He mentioned considerations that would agree with Lockyer's views, without mentioning Lockyer by name, but then concluded:

> Nevertheless, Vogel's view that we can observe only the descending branch of the temperature curve of stars appears to be confirmed by many of the more recent spectroscopic discoveries. We may cite the evidence afforded by the gaseous stars, by the intimate connection of nebulae with many stars of the first class, by the fact that the Algol-type variables - apparently young systems - belong to the first class, and by the relatively slight density of the binaries with spectra of Type Ia as compared with those of the solar class.<sup>128</sup>

These statements appeared in a translator's insertion, and therefore reflect Frost's views. We note in particular his recourse to binary stars, and notions as to their degree of development.

After Lockyer's paper was read at the Royal Society, referees' comments were solicited before the paper was accepted for publication. The two referees in this case were W.H. Christie of Greenwich, and G.H. Darwin, the latter already contributing supportive material in his attempt to reconcile the meteoritic and nebular hypotheses.

Christie<sup>129</sup> felt that Lockyer's paper should be published and that representative spectra be published along with the text, though in several areas, especially those where Lockyer had been repetitive in stating his meteoritic hypothesis, the paper would be improved by considerable contraction. Christie expressed a desire for greater explanatory discussion of the method of reductions to wavelength, and lists of wavelengths themselves, "... to enable the reader to judge of

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the evidence for identification ..."

Darwin<sup>130</sup> at first proclaimed ignorance of the principles of spectrum analysis, and noted that this should be considered "... As there have been controversies between the author and other spectroscopists ..." Darwin therefore confined his commentary to Lockyer's statements concerning the nebular hypothesis in his introduction; conditions for the two series; and for the position of the attainment of maximum temperature during the life of a star. Darwin began:

> On page 1 /of the manuscript7 the author maintains that there is a fundamental difference between his meteoric hypothesis and the nebular hypothesis because the latter demands that the highest temperature should occur at the beginning of the line of evolution. Now in this important point I venture to differ from him. August Ritter (and after him I think Lord Kelvin), have sic7 as I understand, shown that the internal temperature of a gaseous star increases as it contracts. It is not necessary here to discuss this apparent paradox that a cooling body should get hotter as it cools. If this be so I fail to see that the evidence adduced in this paper will discriminate between the meteoritic and nebular hypothesis although it may of course be remembered that all the phenomena are consistent with the meteoritic explanation.<sup>131</sup>

Darwin believed that this conclusion affected the whole paper, and pointed expressly to Lockyer's remarks concerning the reality of his temperature arch. Darwin added: "... if Ritter is right the increase of temperature will not cease under the conditions comtemplated by the author ... " which were, of course, that the stars, upon arriving at the descending branch, were perfect gases.

Darwin concluded by recommending publication in the <u>Philosophical Transactions</u>, not on the basis of whether the theory was right or wrong, but on the basis of the work performed in the collection of the data and in the elucidation of the basic theory, in which "... I think that there is a great deal of truth ..." Darwin believed that the value of the paper was that "... it serves as a basis for the coordination of facts ..." He qualified his report at the end by stating that Lockyer should be made aware of his comments if Kelvin agreed with his comments and criticism. No evidence has been found that indicates Kelvin's attitude towards this paper, although Lockyer was "made aware" of Darwin's comments.

Lockyer was shown excerpts from these two referees' reports, and was given a chance to reply in writing, which he did in July, 1893. First, to Darwin's criticism that the meteoritic hypothesis and nebular hypothesis were incorrectly separated, and Lockyer's apparent ignorance of Ritter's work, he responded:

> In the present communication I have not referred to the views of Ritter or any subsequent inquiries because I consider that the time for discussing them has not yet arrived as indicated at the commencement of the paper. The new hypothesis is simply contrasted with that of Laplace. I am perfectly aware that the meteoritic hypothesis must eventually be discussed in relation to the gaseous contraction theory ... but up to the present there is no suggestion offered either as to the chemical or physical characteristics of the gas in question ...<sup>132</sup>

All that Lockyer seemed to infer at the beginning of his paper was that "... The results as yet obtained are not sufficient to permit a discussion of all points bearing upon the hypothesis ..."<sup>133</sup> As this quotation continued by stating that the present data did allow for discussion of all crucial points bearing upon the theory, lockyer apparently did not consider the Lane/Ritter considerations to be "crucial". Today we would tend to agree with Darwin on this point, for even though Ritter held to a double valued temperature curve, from considerations quite different from Lockyer's, it was Ritter's basic argument that Russell eventually used to create a theory of the Main Sequence. Lockyer answered Darwin's second use of Ritter, the discussion pertaining to the point at which maximum temperature occurs in stars, by referring only to his previous answer.

Darwin's opinions and criticism of Lockyer's 1893 paper show that he remained critical of Lockyer's interpretations, though Lockyer himself was justified in reserving judgment concerning the applicability of Ritter's law without better knowledge of the state of the gas within the interior of a star.

Through the 1890s, Lockyer worked on two fronts. First, he continued to travel to solar eclipse events to further study the solar chromosphere and corona, and second, an allied study, he focused attention upon the spectra of the hottest stars. In both cases, his intention was to determine the identifications of many of the still unknown lines in the spectra of the hottest stars. Only in this way could progress be made in coming to an acceptable interpretive model for the causes of changes observed in sequences of stars of high temperature. One of the most significant events that aided the identification of unknowns in the hottest stars came in 1895 with the isolation and identification of Helium as a terrestrial substance. As the D<sub>3</sub> line had already been observed, not only in the spectra of the hottest stars, but in the Orion nebula by Copeland in 1889, Lockyer now had at hand one of the most important keys to the behaviour of stars at the highest temperatures. We note in passing that the observation of D, in nebulae was believed by Lockyer to be due to a relatively small number of head-on meteoritic collisions, which did not represent the collisional energy or low temperature of the general meteoritic swarm. His attention in the mid-nineties, therefore, was restricted to the spectrum of helium as it occurred in the Sun and in hot stars.

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### On the Chemistry of the Hottest Stars - 1897

As he had done in many previous expositions, Lockyer began his discussion with a review of his technique of "long and short lines",<sup>135</sup> which now aided the assignment of temperature based upon the laboratory spectra of calcium, magnesium and iron. Lockyer believed that he could show complete agreement between temperatures derived from the three metals, and from the behaviour of the stellar continuum. But his primary discussion now concerned helium and hydrogen in the hottest stars. He felt that since hydrogen was found in almost all temperature ranges, while helium (or the "cleveite" gas) was to be only found in the hottest stars, it was the latter that was the more reliable for temperature discrimination.

Lockyer found that the lines identified as helium increased in intensity with an increase in temperature. They first appeared in stars like Deneb and increased in strength in hotter stars. Lockyer's observations of these lines then caused him to create a sequence which, when compared to the Harvard sequence, is seen to represent the reversed order "... B, A ..." without recourse to association with nebulae.

It was an interesting and important consideration for Lockyer that the individual characteristics and peculiarities of each element be understood, particularly the varying range of temperature over which they appeared and temperature levels where significant changes took place in their spectra. Calcium and hydrogen had long ranges, magnesium was intermediate, with iron and the cleveite gases shortest but at opposite ends of the range. Lockyer noted that the point where the enhanced spectrum of iron took over from the regular spectrum was in Deneb's temperature range, where nothing seemed to happen to the enhanced calcium spectrum at that point. Here clearly was evidence of \*Spectral lines originating from the halo and core respectively of a source, when imaged by a lens.

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the varying structure of the elements, and that, somehow, the passage of spectra from regular to enhanced was bound up with peculiarities in structure. Lockyer made no comment on this at the time.

Lockyer was able to identify four basic criteria for the degree of condensation of a stellar body, once its mean temperature was determined. These included continuum absorption in the violet, the varying thicknesses of the hydrogen line series and the metallic lines, the varying intensities of these two sets of lines, and the relative thicknesses of the cleveite lines, when they appeared. Of course, most of these criteria were just those revived by Adams and Kohlschutter, and are certainly the fundamental criteria used today in the spectroscopic assignment of absolute luminosity. This is to Lockyer's credit, even though his meteoritic interpretation has long since been abandoned.

Lockyer's meteoritic interpretation accounted for continuum differences by greater amounts of cool gases producing absorption in the meteoritic swarms. The thinness of the hydrogen lines on the ascending branch was caused by absorption produced by gases in the meteoritic interspaces which masked the regions producing the hydrogen absorption, close to points of grazing collisions of meteoritic matter. This masking was believed to be less pronounced in real stars. The varying metallic spectra resulted from greater optical depths in the uncondensed swarms. Variations in line thickness were also interpreted as due to Doppler broadening caused by turbulence in the uncondensed swarms.

Within a section entitled "The Bearing of These Results upon the Dissociation Hypothesis", Lockyer acknowledged that few people believed in dissociation and then tried to imply that this was due to the diverse realms of the astronomer and chemist: The chemist has little interest in an appeal to celestial phenomena, and astronomers do not generally concern themselves with chemistry. The region investigated by the chemist is a low temperature region dominated by monatomic and polyatomic molecules. The region I have chiefly investigated is a high temperature region, in which mercury gives the same phenomena as manganese.<sup>136</sup>

Although Lockyer felt that dissociation had great support, he noted that one element had been abandoned:

> With regard to the basic line part of the inquiry, I think I shall not be going too far in saying that it has been universally rejected, and chiefly on the ground that some lines which appeared coincident at the dispersion I employed appeared double with higher dispersions.<sup>137</sup>

Lockyer pointed out that when these coincidences first became suspect, largely due to the work of C.A. Young on the spectrum of the Sun, he stated that the evidence was not sufficient to discount the concept.<sup>138</sup> Lockyer reserved discussion on this point well through the nineties in the hope "... that some chemist would take up the question of spectroscopic impurities out of which it grew ..."<sup>139</sup> Indeed, Lockyer's own assistants were assigned to the task, and in 1897, the work of Fowler changed Lockyer's regard for the reality of "basic" lines and the fundamental concept of dissociation itself.<sup>140</sup> In Lockyer's own consideration of the "basic" lines in his review paper in 1897, it was evident that, though he felt that the new criteria would somehow make them more important, any discussion of the subject would have to wait.

Lockyer was sensitive to non-evolutionary causes for differences in stellar spectra and gave the matter detailed attention. Clearly, if differences in spectra were due to composition differences, celestial dissociation would be invalidated. He thus entertained this possibility, which we have seen had been a consistently recurring theme in the writings of Maunder and others. But Lockyer used it as a stage to highlight the reality of his dissociation hypothesis, for he rapidly dispensed with the possibility of composition differences, by noting that, although seventy-two different elements were known at the time, stars classed themselves into seven groups chemically. He gave other arguments too, all based upon his concept of uniformity and continuity.

Lockyer proclaimed the reality of celestial dissociation by comparing inorganic evolution with its organic counterpart:

> I claim that each step in the work has demonstrated the truth of that hypothesis more and more, and that we can now acknowledge that the phenomena of the inorganic world are dominated by an evolution not less majestic, although much more simple, than that now universally accepted in the case of organic nature.<sup>141</sup>

So, just as natural selection was a guiding principle in organic evolution, dissociation was the guide in the inorganic world.<sup>142</sup> His experiments through 1899 with larger spark coils had finally allowed him to reproduce the spectrum of Deneb, which he felt settled the question of dissociation. His order of inquiry beyond this point was revealed in a letter to C.A. Young, reporting upon his successes. Lockyer announced:

As this settles dissociation I am now turning my attention to evolution. $^{143}$ 

This was written one year before the date of the author's preface to Inorganic Evolution.

## Schuster's Commentary on Lockyer's "Chemistry of the Hottest Stars"

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Lockyer's paper "On the Chemistry of the Hottest Stars" was followed by a discussion by Arthur Schuster on the chemical constitution of the stars.<sup>144</sup> As the session of 25 March, 1897 of the Royal Society had been convened for the purpose of discussing "The Chemical Constitution of the Stars", Schuster began his note by wishing that Lockyer had confined his remarks to this topic alone. Schuster considered the second half of Lockyer's paper to be a direct challenge to accept the dissociation theory "... in its full generality ..." without consideration of alternatives.

This type of reaction to Lockyer has been noted by Brock. 145 Schuster stated that he concurred with Lockyer's general classification scheme, partly because of Lockyer's reliance upon laboratory experiments. But he disagreed with many of Lockyer's physical interpretations of spectral changes. He considered other possible causes of changes in line ratios; primarily the possibility that with a change in temperature, a star might change from a condition of convective equilibrium to one of thermal equilibrium. This kind of change would greatly affect the spectrum of a star, since in the convective state, mixing takes place which brings masses of gas of greatly differing temperature into contact, or into different layers of the solar atmosphere. In stars dominated by thermal radiation, no mixing effects exist, and the spectrum arises from one temperature alone. Schuster reviewed the observed fact that convection currents exist near the Sun's surface and concluded: "There is in consequence an approach to a uniform distribution of matter and enormous differences of temperature in layers which are comparatively close together ...."<sup>146</sup> Even though Schuster felt that the spectra of hot stars was proof enough for the absence of convection, he felt obliged to discuss why no convection occurred. He believed that diminished surface gravity and density would inhibit convection and promote radiation. He noted that this explanation was similar to one given by Huggins in his Presidential address to the BAAS in 1891, but that, to be fair, difficulties could easily be found with the general discussion, just as they could easily arise from Lockyer's dissociation hypothesis.

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Schuster also criticised Lockyer on the grounds that he apparently had not kept up with the work of Ritter, Lane and Kelvin on the behaviour of gases under contraction. To Schuster, these men had shown that a "... <u>radiating</u> and <u>contracting</u> mass is not necessarily a <u>cooling</u> mass; on the contrary, the interior of our sun is almost certainly rising in temperature at the present moment ...."<sup>147</sup>

Schuster continued, following Ritter, to consider the possibility that not all stars follow the same course of evolution and that "... it would be unwise to push the argument of uniformity too far ..."<sup>148</sup> Here he was referring to mass, but later was to concern himself with composition. Regarding mass, Schuster felt that the Sun could never have achieved the temperature of hydrogen stars, and that it was at about its maximum at the time. This, of course, followed from Ritter's work and is a consideration we still use today. Of course, Schuster's criticism indirectly pointed to Lockyer's placement of solar stars on the descending branch of a continuous curve.

Two final points capped Schuster's commentary. First, he felt that Lockyer's principle of continuity unrealistically demanded uniformity of composition and the uniformity of the ultimate states (or state) of matter. While Schuster was disposed to both these concepts, he argued that differences in spectra ascribed to rising and falling temperature conditions by Lockyer could very well be due to composition effects. Also, he felt that there was "... no reason to believe that the nebulae of the present day resemble our sun's ancestor ..."

He continued:

Some of the stars which are now in an early stage of development may be forming through the condensation of matter which has been left over by others; and it would not be surprising if the youngest star did not agree in constitution with its aged companions.<sup>149</sup>

This statement embodies the present day identification of stellar populations of stars of differing chemical composition, first brought out by Baade in 1944. Schuster's remarks are of interest for they represent an evolutionary view not only of stars, but of stellar systems of different age. In any event, he dropped the consideration at this point, and concluded his remarks with the second final point; that though we are free to consider the question of the reality of the uniformity of ultimate matter, Lockyer's claim that direct proof of the situation can be gleaned from spectroscopic studies of stars in varying states of dissociation is misleading. Here, according to Schuster, one would find not primordial matter, in its original cool state, as in the nebulae, but "... a temporary relapse of our elements into their original state ...." He continued: "That may be so. It is in my opinion a perfectly legitimate hypothesis, one that at present has not been disproved ... " But neither had it been proved; the final proof possible only from direct laboratory identification of traces of ultimate matter.

Lockyer answered Schuster in a very brief and obtuse note which was published in the <u>Proceedings</u><sup>151</sup> in the second person. The only substantive remark seemed to be that Lockyer believed convection was important in hot stars, too, and further that mixing must occur at very different rates on the two sides of the curve. With this, Lockyer attempted to show that mixing did not severely affect the spectroscopic appearance of a star, since only small spectral differences in line structure were noted for stars on both sides of the curve. He made no mention of Schuster's recourse to the work of Lane and Ritter. He simply did not feel that it was applicable to his own, since the constitutions of stars were so poorly known.

An interesting commentary on the proceedings of this meeting

at which Lockyer and Schuster spoke was provided by someone who did not attend - Sir William Huggins - who nevertheless had been careful to be quickly appraised of the event. He reported the meeting to Hale:

> The affair at the R.S. was a discussion in name only. It was got up by L., to give him an opportunity to attempt to rehabilitate his dissociation, and meteoric theories. In papers sent out, he gives at length, and makes much of, what you said about calcium.<sup>152</sup>

Huggins added that the chemist Armstrong, one of the attendees, felt that "... there was nothing antecedently impossible in the notion of the dissociation of the elements ..."<sup>153</sup> but that others, like Schuster, felt that "... all the phenomena could be explained without having recourse to dissociation ..." Hale had been discussing the dissociation of calcium with Huggins during this period, and had somewhat rankled the old spectroscopist by indicating a certain favour with the idea. Huggins also sought out another American favourably disposed to dissociation, C.A. Young, to further discuss the matter, and indicated in a long statement his and Stokes' apparent reservation for accepting dissociation too quickly.<sup>154</sup>

### Lockyer's 'The Sun's Place in Nature' and Inorganic Evolution' - 1897-1899

In the remaining years of the nineteenth century, Lockyer rediscussed his theories of dissociation and evolution several times. His <u>The Sun's Place in Nature</u>, more an extension of his <u>Meteoritic</u> <u>Hypothesis</u> than anything else, was an answer to his most persistent critic, Huggins. The book grew out of a series of lectures given in 1894.<sup>155</sup> His primary discussion involving Huggins concerned the identity of the chief nebular line, but he also provided a rediscussion of his classification system.

Since The Sun's Place in Nature was written shortly after Fowler's work on iron and Lockyer's long paper on the hottest stars (as

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we infer from the date of his preface - July 1897), little was altered here from these discussions before the Royal Society. Even the format of his fourth section, "The Sun's Place Among the Stars" is identical to the format of his March address. There is an important change however, in that Lockyer now felt obliged to discuss his ideas in terms of physical theory. In a discussion of Vogel's classification, equated with Laplace's Nebular Hypothesis as a linear cooling process from a hot nebula, Lockyer ignored explicit arguments modifying Laplace by Darwin, but indirectly used the argument to discuss condensation in general:

> I have already pointed out that in accordance with thermodynamic principles, the temperature must increase with condensation. A nebula condensing, then, must be a nebula getting hotter. We have already seen it demonstrated that the bright-line stars are bodies more condensed than nebulae, 156 consequently they will be hotter than nebulae.

From his use of "thermodynamic principles" in this passage we might infer that Lockyer, during 1897, largely in response to Schuster's remarks and to the remarks of Darwin concerning his 1893 paper submitted to the Royal Society, began to budge in the direction of contemporary physical considerations. He discussed Darwin's demonstration that a swarm of meteorites behaved according to kinetic theory, and concluded from it that, "... in accordance with dynamical theory, the temperature <u>of</u> a swarm must increase with condensation so long as the conditions of a perfect gas hold good, and if we accept that a swarm of meteorites will behave like a perfect gas, then swarms of meteorites will also get hotter by condensation ..."<sup>157</sup>

Lockyer continued in this manner by noting that:

... in all such condensations as we are considering a time must arrive when the loss of heat by radiation will be greater than the gain due to condensation ...

... at which point, of course, cooling would set in. In this commentary,

of course, we can see Ritter's influence, but Lockyer differed with Ritter's belief in the maximum temperature of the Sun in past history. Noting that the Sun was an example of a cooling body, Lockyer added that independent lines of inquiry showed that it was hotter in past times. He does not elaborate here, and presumably one line of "independent" inquiry was the meteoritic hypothesis itself. In like manner, he discussed nowhere the criteria for cooling to set in, since no spectroscopic evidence was available to distinguish a perfectly compressible gas from one that was not perfectly compressible. He couldn't apply the criteria for convective or radiative equilibrium, as Schuster had discussed, because convection appeared on both sides of the temperature arch, though in different proportions, according to Lockyer. The actual change from rising to falling temperature would have had to have been explained while the star, at its hottest, remained in the radiative state alone.

In 1898, Lockyer extended this discussion of the appearances of chemical substances at different stellar temperatures. He began by listing advances in identification and association with temperature range for metals. These included a better understanding of the spectrum of Deneb, from high tension laboratory spark work, the identification of Pickering's new "hydrogenic" series in four more stars in Orion, the discovery of more lines of still unknown identification, and the discovery of oxygen in hot stars by Frank McClean. This new material was then combined with earlier data, and presented in a map,<sup>158</sup> correlating the visibility of 24 substances (metals, proto-metals, unknowns, hydrogen, helium, etc.) against the stars in which they appeared. From this map, Lockyer concluded:

> It will be seen that this more general inquiry entirely justifies the prior statement /Proc. R.S. 61 p.1827 that the metallic lines are thickest in

stars increasing their temperatures, and that the hydrogen lines are thickest in stars decreasing their temperatures; in other words, on the opposite arms of the temperature curve.<sup>159</sup>

This allowed Lockyer to come to the same interpretations he arrived at two years prior using his meteoritic hypothesis and now better established spectral criteria for changing temperatures.

In 1899 Lockyer announced a revised nomenclature for his system of classification - his "Genera".

#### Lockyer's "Genera"

Lockyer felt that the time was right for a revision, which would help to clarify earlier alphabetical and numerical schemes. The clarification, of course, was in the direction of Lockyer's scheme of evolution, and was justified on the basis that a considerable number of lines in stellar spectra had been identified, allowing for the establishment of definite chemical groupings. Following this logic, one would expect Lockyer to create a system of classification based upon chemical names, but this was not considered since the major chemical features appeared on both sides of the temperature curve. Lockyer took up a suggestion by T.G. Bonney in February, 1899, to create a system based upon the place where typical examples of the scheme were found. Bonney, a geologist, applied the prevalent scheme for the assignment of geological epochs to Lockyer's needs.<sup>161</sup>

This scheme must have been very exciting for Lockyer to contemplate, and evidently he made no hesitation in applying it, for it represented just what he had been looking for - a mode of expression to relate inorganic to organic evolution. Lockyer named his genera after the archtypal star itself if it was the brightest in the constellation within which it was found. If the star was not the brightest, the name of the constellation was used as root. Lockyer believed that this new mode of classification allowed for the eventual inclusion of intermediate forms, as they became identified, with a greater ease than any numerical or alphabetical scheme. To emphasise the continuity of his series, he created a schematic table that isolated his genera according to the stellar conditions believed to most clearly characterise his spectra (see Table 2, p.132).

As we have noted, many later techniques of luminosity differentiation have been echoes of Lockyer's fundamental use of line ratios. His scheme did faithfully represent a temperature sequence,<sup>163</sup> but an unambiguous interpretation of line ratios in terms of luminosity never appeared in his writings.

Lockyer's recourse to genera for his classification system was not original. In 1892 W.H.S. Monck suggested their use<sup>164</sup> for classification in a paper on the relationships noted between the proper motions of different groups of stars and their spectra. He used the same "-ian" ending, or "-an" where appropriate, and indicated that his proposal was intended to facilitate identification of class. In this note, he identified <u>Capellan</u>, <u>Arcturian</u>, <u>Sirian</u>, and <u>Antarian</u> classes.

During 1899 and early 1900, Lockyer presented what was to be his last major review of work. Entitled <u>Inorganic Evolution</u>, it was intended not only as an update of his previous books, but also as a review of the material presented in them, and how this work had fared in the following years. The work comprised five chapters or "books", and, as was typical of these works, was in many places a verbatim transcript of material already in print from the <u>Philosophical Transactions</u>, the <u>Royal Society Proceedings</u>, etc. Notably, the last paper discussed here was reproduced in full.

While no truly new material appeared in Inorganic Evolution

# Table 2

# Lockyer's 1899 genera representing both the rising and falling branches of his temperature arch, separated in terms of primary spectral characteristics.<sup>162</sup>

### Classification of Stars

	ł	Highest Temperature			
Gaseous stars		(Proto-hydrogen stars (		(Argonian (Alnitamian	
		( Cleveite-gas stars	(Crucian (Taurian		Achernian Algolian
Proto-metallic stars			(Rigelian (Cygnian (		Markabian  Sirian
Metallic stars			(Polarian (Aldebarian		Procyonian Arcturian
Stars with fluted spectra			Antarian		Piscian

Lowest Temperature

from the standpoint of any revisions in his interpretive schemes, extensive discussions did appear concerning objections to his Dissociation Hypothesis, and his replies. The final part of the book was devoted to the general question of evolution, both organic and inorganic. Our discussion of his book will be limited to this last topic, which began with a discussion of the evolutionary causes of composition differences based upon spatial distribution studies.

By 1900, several statistical studies of the spatial distribution of spectra had been completed by Pickering, Kapteyn, W.H.S. Monck, and F. McClean. Lockyer examined these studies, and became quite interested in the distributions of nebulae, bright line stars, and novae with respect to the plane of the Milky Way. All three seemed to have the same distribution - highly confined to the Milky Way. This was agreeable to Lockyer, who interpreted these phenomena in terms of collisions of meteoritic swarms: collisions which would be maximised in dense regions of space.

Lockyer's primary conclusion from statistical studies was that:

Although this discussion of different types of stellar spectra indicates a collective tendency of some types, it proves at the same time that the chemical substances represented in such types are distinctly not limited to the regions in which they predominate  $\dots 165$ 

Hydrogen, of course, appeared in all but carbon stars, and though helium was common to many, it did prefer the plane of the Milky Way. The distribution of carbon was also thought to be general, existing both in the hottest and coolest stars. But here Lockyer was deceived by his misidentification of the 4686 feature that indeed was not carbon but helium. Discussing the general distributions of iron, magnesium and calcium, Lockyer concluded that no localisation due to direction was apparent. In discussing distance, Lockyer recognised that some differentiation apparently existed, but noted that the statistical spread of the several chemical groups caused many overlaps. If he had considered the possibility of a luminosity function, his conclusions would have been easier to see. From all of the above, he finally concluded that "... the chemistry of all parts of space is the same ..."<sup>166</sup> This then removed what he considered to be the major objection from stellar evidence to uniformity and the dissociation hypothesis, and provided at the same time further support for the meteoritic hypothesis:

> As on the latter hypothesis the stars become hot in consequence of meteoritic collisions, we should expect to find nebulous conditions following suit; seeing that nebulae are masses of meteorites, we should expect to find especially the gaseous nebulae and results depending upon their presence in the region where the hottest stars exist in which dissociation has been studied.<sup>167</sup>

Schuster's continued recourse to the work of Ritter and Lane, and especially his comment that Lockyer made no indication of being aware of their work, was finally answered in <u>Inorganic Evolution</u>. Lockyer did not mention his previous commentary in reply to reviewers' suggestions to consider the work of Ritter and Lane, but provided a similar argument - that he felt that his work was not involved with the question of gaseous masses:

> In my work which has consisted in the discussion of spectroscopic observations, I was at the outset led to the view that it was not a question of gaseous masses at all, originally, and therefore I did not refer to Ritter's conclusions on this point.<sup>168</sup>

It is of interest to compare this published statement with his unpublished reply to Darwin's review in 1893. In the unpublished version, Lockyer stated that the general problem was not yet at a state where Ritter's theoretical interpretations could be applied, and that he was aware that in the future, they would become more important. Just what Lockyer meant, in <u>Inorganic Evolution</u>, that he believed the question was not one of gaseous masses at all, referred to his "original" view - which had, by 1900, been altered. Of course, Lockyer, long dependent upon chemical interpretation, might have been overly reluctant to apply physical considerations he had not long been familiar with, even when they apparently were able to come independently to the same evolutionary model, in its more general characteristics. Some of this might be gleaned from the following commentary by Lockyer:

> Again, I had to face the spectroscopic evidence of a chain of obviously cooling bodies, and it was a detail to consider that "a radiating and contracting mass is not necessarily a cooling mass", because in spite of this truism a time must certainly come when all bodies will find their temperature reduced.<sup>169</sup>

Is it conceivable that Lockyer hadn't critically familiarised himself with Ritter's criteria for rising and falling temperatures? Lockyer had previously mentioned what seemed to be Ritter's exact criterion in <u>The Sun's Place in Nature</u> in 1897 and apparently had by that time become more familiar with the theory, enough at least to show how it supported his own work. Something undoubtedly caused Lockyer to become more conservative in the interval, for, as he continued:

> I am aware that Ritter's conclusions regarding the first rise and subsequent fall of temperature of gaseous bodies, are similar to those supported by the spectroscopic evidence of what I have considered to be condensing swarms of meteorites, but it would not have been fair to claim Ritter's conclusions as supporting my own, because the bases of the phenomena considered by us were so different.<sup>170</sup>

While it is truly rare to see such reserve in Lockyer's attitude towards a theory that in a large degree was supportive of his own, it might be understood better in terms of Lockyer's need to preserve his concept of uniformity, where all stars eventually would pass through the same stages of evolution; a need, according to Schuster, not shared by Ritter's evolutionary scheme. Lockyer did not discuss this difference as a point of objection to Ritter's theory, but did point out other apparent objections. One came from a letter published in <u>Nature</u> and written by John Perry.<sup>171</sup> We have already discussed Perry's comments, which were requested by Lockyer for <u>Nature</u> as an evaluation of the applicability of the laws of Lane, Ritter and See. Lockyer used Perry's mildly negative conclusions to support his contention that the time was not right to depend too heavily upon these theoretical arguments.

The final sections of Lockyer's <u>Inorganic Evolution</u> dealt, as we have mentioned, with the phenomenon of evolution itself, both in the inorganic and organic worlds, and here we see how his thinking was wedded to continuity and the uniformity of Nature. He wanted to change his direction of discussion to a point where spectroscopic phenomena could be interpreted in terms of evolution and not in terms of dissociation. He thus started by discussing the phenomenon of evolution from the organic side, labelling it as "... one of the greatest triumphs of the century just ending ..."<sup>172</sup> and "... the most profound revolution in modern thought which the world has seen ..."

In attempting to maintain perspective between inorganic and organic evolution, Lockyer identified what he believed to be their primary difference. Organic evolution was dependent upon time, during the progression of which, other physical and biological elements must have remained somewhat constant. Inorganic evolution, however, was temperature dependent:

> It is for this reason that in the inorganic evolution which now concerns us the chemical changes brought about by changes of temperature must be our chief guide, and the earliest and simplest forms must be sought in regions where the highest temperature is present.<sup>173</sup>
His guide was dissociation, to which he again turned for the underlying interpretive scheme for his classification system. Recalling his genera, Lockyer professed that "... by means of this recent development of spectrum analysis, we have been able really to do for the various stars what the biologist, a good many years ago, did for the geological strata".<sup>174</sup> In short, Lockyer felt that he was able to answer in the affirmative the question he had asked: "Do the stars show a progression of chemical forms as the geological beds show a progression of organic forms?" In answering in the affirmative, however, Lockyer felt it wise to qualify his conclusion by explaining that at the very high temperatures found in the hottest stars, one cannot think of the normal divisions of solid, liquid and gas. In the hottest stars, the only state is that of a gas. This statement is interesting in that it apparently contradicts Lockyer's earlier concern for the non-applicability of Ritter's work to the stars.

Lockyer's final summary in <u>Inorganic Evolution</u> pointed to three ways for evolution to proceed - by polymerization (of similar chemical molecules), by combination (of dissimilar chemical molecules), or by "... the new physical view ..." of the gradual build-up of complex elements from similar charged particles. Lockyer was most interested with the third, for it offered a return to unity:

> In this last conception we have the material world, up to the highest complex, built up of the same matter under the same laws; as in spectrum analysis there is no special abrupt change between the phenomena presented by the simple and compound bodies of the chemist, so also in the new view there is no break in the order of material evolution from end to end ...<sup>175</sup>

We thus end our discussion of Lockyer's studies in spectral classification during the period 1890-1900. In the following two decades of his life, no comparable work appeared that would provide such

a great synthesis of his views. After the appearance of Russell's work in 1913, Lockyer revived and refined his discussion of the temperature curve in several volumes of the <u>Hill Observatory Bulletin</u>. Of course, his extensive laboratory and observatory projects continued, and within the first four years of the new century he published an extensive list of spectra. At appropriate points in our discussion of later work, we will refer to the continued work of Lockyer, and examine closely the influence he had upon H.N. Russell.

## Huggins - 1891-1900

Our examination of Huggins' work in this section will be bracketed by two publications, his presidential address to the British Association in August, 1891,<sup>176</sup> and the publication of his <u>Atlas of</u> <u>Representative Stellar Spectra</u> in 1899.

While Huggins did not formally create a system of classification, his spectroscopic studies were a great influence on the work of other classifiers. We have already reviewed a spectral sequence he proposed in 1879, which was similar to Vogel's. In his early work, up to 1890, we have seen that he continually expressed reserve in linking any theory of evolution to any scheme of classification, or vice vers@. By 1890, however, the situation changed somewhat, as Lockyer had noticed and had commented on later in Inorganic Evolution.

One of Huggins' chief reversals was his opinion on the role of nebulae. In the 1860s, he considered them to be wholly of a different order than stars. But by 1890, Huggins most definitely was linking nebulae to stars. His own contributions to this change were not minor, for in the intervening years, his identifications of elements in the Sun and stars were a great stimulus in the concept of the unity of matter. In 1879, he was one of the first, with Vogel, to discover the photographic lines of the Balmer series, an observation that, in fact, greatly aided the acceptance of the Balmer Rule itself.<sup>177</sup>

In his presidential address, Huggins expressed caution concerning the application of the results of laboratory spectra to the stars. Since laboratory spectra were emission phenomena, and the majority of stellar spectra were primarily absorption phenomena, most spectroscopists at the time, according to him,<sup>178</sup> remained sceptical of their applicability for the determination of temperature. Huggins also felt that, since the source of radiation in stars and in the laboratory was different, further unknown complications might exist. Line ratios and line widths were also to be suspected as illusory in the acquisition of physical evidence, for it was impossible to be sure where they arose in the stellar atmosphere.

Most of these cautions were, of course, in answer to Lockyer's arguments in the <u>Meteoritic Hypothesis</u>. Nevertheless, they were justified at the time.

After these introductory remarks, Huggins spoke of stellar evolution. He had provided an evolutionary scheme based upon photographic spectra in 1879, which was based upon Vogel's of 1874. The white stars were the youngest, and the progression continued from white to red:

> ... the white stars, which are most numerous, represent the early adult and most persistent stage of stellar life; the solar condition that of full maturity and of commencing age; while in the orange and red stars with banded spectra we see the setting in and advance of old age. But this statement must be taken broadly, and not as asserting that all stars, however different in mass and possibly to some small extent in original constitution, exhibit one invariable succession of spectra.<sup>179</sup>

The concept of continuity in evolution was thus not an overriding concern for Huggins, who sided with Ritter's discussion of the

mass dependence of an evolutionary path. At the outset, Huggins referred to Ritter's series of papers on the behaviour of gaseous spheres undergoing contraction. Noting Ritter's evolutionary scheme, he was careful to represent it as being double valued in colour without mentioning temperature directly. Huggins commented that "... Recently a similar evolutional order has been suggested, which is based upon the hypothesis that the nebulae and stars consist of colliding meteoric stones in different stages of condensation ... "<sup>180</sup> Huggins then passed right on to discuss the possibility that the diversified spectra seen amongst stars could very well be due to composition differences, and not to evolution.

Actually, Huggins' commentary on evolution was more a discussion of the physical constitution of stars. While he was very careful to avoid any inference that his evolutionary ideas were linked with a temperature sequence, he noted without further comment that "... It has been shown by Lane that, so long as a condensing gaseous mass remains subject to the laws of a purely gaseous body its temperature will continue to rise ...."181 This was Huggins' only evolutionary statement invoking temperature, and he made no comment on how this might be incorporated into his scheme. Huggins did, however, concern himself with the problem of determining relative temperatures, but his avoidance of any direct discussion of a temperature sequence might very well have been due to the possibility that at the time he was unsure as to which way the sequence really went. As we shall see towards the end of this section, Huggins eventually came to an order inverted from Vogel's cooling scheme.

His discussion of stellar structure, referred to in part by Schuster in 1897, argued that the Sun and stars "... are generally regarded as consisting of glowing vapours surrounded by a photosphere where condensation is taking place, the temperature of the photospheric layer from which the greater part of the radiation comes being constantly renewed from the hotter matter within ..."<sup>182</sup> At the stellar surface, convective equilibrium was dominant, and the subsequent mixing would not allow the different gases "... to retain the inequality of proportions at different levels due to their vapour densities ..." Huggins believed that the stellar spectrum depended upon the nature of the photospheric layer, and the nature of cooler gases above it. In turn, the photosphere and higher regions were dependent not only upon temperature, but upon the force of gravity in the two regions. As a star condensed, the force of gravity would of course increase, but the range depended upon the mass of the star.

The Sun was believed to have condensed to a point where gravity at its surface was great enough to cause a large density gradient in the surface regions. Huggins believed the temperature gradient at the surface to be similarly large, if the atmosphere was considered free to expand into space. His eventual point here was to reaffirm the fineness of the reversing layer, in opposition to Lockyer.

Starting at the Sun's place in stellar life, Huggins then traced backwards in time:

Passing backward in the star's life, we should find a gradual weakening of gravity at the surface, a reduction of the temperature-gradient so far as it was determined by expansion, and convection currents of less violence producing less interferences with the proportional quantities of gases due to their vapour densities, while the effects of eruptions would be more extensive ...<sup>183</sup>

Tracing this progression back, one would expect to find an ever simplifying spectrum, from the gradual diminution of mixing allowing for the sorting by vapour density to occur. "At last we might come to a state of things in which, if the star were hot enough, only hydrogen might be sufficiently cool relatively to the radiation behind to produce a strong spectrum ....<sup>184</sup>

Huggins felt that this entire subject was obscure as yet, and commented "... we may go wrong in our mode of conceiving of the probable progress of events, but there can be no doubt that in one remarkable instance the white-star spectrum is associated with an early stage of condensation ..."

His last quoted statement came from the density of Sirius, an example of a white star. With a knowledge of its absolute luminosity, which was believed to be between forty and sixty times that of the Sun, and its mass, as derived from its binary orbit, its volume was found to be far greater than the Sun's, to account for its great luminosity. But its relatively smaller mass (twice that of the Sun) meant that it would have to be less dense, or, less condensed. Huggins continued:

> It follows that, unless we attribute to this star an improbably great emissive power, it must be of immense size, and in a much more diffuse and therefore an earlier condition than our sun; though probably at a later stage than those white stars in which the hydrogen lines are bright.

Sirius does possess far greater emissive power per unit surface area than does the Sun, as would be gathered from Stefan's law. But at the time, no general agreement existed over the applicability of such laws. Huggins did consider the possibility, at least, and in the same spirit went on to consider how the relative temperature of stellar photospheres might be determined from a determination of the brightest portions of their continua. Huggins noted that Langley had found an inverse dependence of the wavelength of maximum radiation upon temperature, and that, for the Sun, the maximum was to be found in the blue region of the spectrum.

Turning to historical conceptions of the evolutional role of

nebulae, Huggins admitted that though Herbert Spencer had correctly interpreted their evolutional place as early as 1858, Huggins' own opinions in 1864 were perverted by "... the undue influence of theological opinions then widely prevalent ..."<sup>185</sup> which caused him to consider nebulae as distinct from stars. He recalled that two years later, before the British Association, he presented a more open opinion by asking that "dogmatic theory" be kept away from this realm of study.

Huggins examined the nebular hypothesis, in terms of the conservation of energy, as interpreted by Helmholtz. He chided past workers who had considered meteoric phases in the past history of the solar system, and clearly aimed his remarks ultimately at Lockyer. He stated here for the first time that he believed the mean temperatures of nebulae to be cold, a significant conclusion, for it indicates that from this date, Huggins was formulating his own evolutionary progression of temperature to meet the initially cold requirement of nebulae. Through Lane's law, and his own carefully constructed mechanisms for the determination of relative temperature, Huggins was to argue that stars heat upon contraction, though their colours changed from blue to red.

At the date of his address, the statistical examination of the various spectral classes (in terms of motion, position and brightness) was becoming an important problem for study. Without elaboration, Huggins mused:

> Can we suppose that each luminous point has no relation to the others near it than the accidental neighbourship of grains of sand upon the shore, or of particles of the wind-blown dust of the desert? Surely every star, from Sirius and Vega down to each grain of the light dust of the Milky Way, has its present place in the heavenly pattern from the slow evolving of its past ...<sup>186</sup>

In his address, we have found an outline for Huggins' continued

work through the nineties, which we will now follow, through his own review, as presented in his <u>Atlas</u>.

# \*\* Huggins' Atlas

In an introductory statement, which included a history of his observatory and accomplishments, Huggins admitted that a surviving theme from his early work was the contention that:

> ... The stars were undoubtedly suns after the order of our own sun, though not all at the same evolutional stage, older or younger it may be, in the life-history of bodies of which the vitality is heat  $\dots$ <sup>187</sup>

Much of his discussion in the historical section dealt with evolution, and much of the commentary was identical to that of his Presidential address of 1891. But here we clearly find an explicit rationale based upon evolution:

> ... the character and strength of the ultra-violet region of the spectra of the stars will be of the first importance in any discussion of the classification of the stars founded upon the hypothesis of an evolutional progress ...<sup>188</sup>

Not only is this one of the most direct statements concerning the dependence of classification schemes upon evolution, it is also a direct consideration of the need to classify at all. It hints that the hypothesis of evolution is in fact a justification of the effort of classification. As he indicated in his 1891 address, he depended heavily upon the observed nature and extent of ultra-violet continuum radiation in different stars as an independent measure of relative temperature. Huggins wanted it to be known that his opinions had not changed significantly in the interval since his 1891 address. While it may be correct to agree with this, the statement is misleading, for the similarity of opinion might be considered as an outline only. In detail, the ten year interval saw a significant increase in Huggins' comprehension of the use of physical considerations such as Lane's Law in his schemes. His explicit discussions of temperature sequences also attest to this change, though it may be regarded as a "solidification" of opinion. Of course, the greater editorial freedom allowed by the format of his folio edition <u>Atlas</u> gave Huggins time and more space to express his opinions and conclusions.

Turning directly to evolution, Huggins opened up the possibility of a rise in temperature as the true direction:

> ... Such a sequence /his own7 necessarily takes place along one direction, a continuously downward one; even when, as we shall see, the actual temperature may be rising at the expense of the potential energy of separation of the stellar matter ... 189

Huggins felt that "... Such a sequence of changes, determined directly through loss of energy through radiation, forms a truly natural basis for a classification, depending upon evolution ..." Huggins was clearly ready for this change. By 1899 he had had the council of C.A. Young, and had sensed the general acceptance of the concept, after papers by Kelvin and Schuster had supported it.

As we have seen, in a letter from Huggins to Hale in October, 1898,<sup>190</sup> Huggins was quite capable of accepting a double valued temperature curve, as long as it wasn't associated with a meteoric interpretation. His consideration of a Ritter-type progression became clear in his <u>Atlas</u>, as he dealt at length with cold nebulae and initial conditions. Possibly due to the work of Becquerel in 1896 and the Curies in 1898, Huggins was also able to consider another source for stellar energy. Thus while he believed that the potential energy of a contracting cloud was sufficient to cause its temperature to rise, he added: "... This view does not, however, exclude the possibility of the play of atomic energy also, at the very beginning of the gravitating into nebulae of the extremely diffuse primordial matter ..."<sup>191</sup> Even though Huggins considered an evolutionary scheme similar to a double valued temperature arch, he at no time placed any of his stars on either of the two possible branches. He invoked Lane's Law, but, as Huggins related, this would not account for the entire lifehistory of a star:

> ... A time will come, however, when the expenditure by radiation will at last exceed the energy which can be made up by the shrinkage of the mass; the star then will begin to cool, and when sufficiently condensed will give out less light and exhibit corresponding spectral changes ...<sup>192</sup>

Huggins' constant feeling, that much more than temperature determined both the luminous efficiency and spectral appearance of a star, inhibited his application of the above model to a clearly defined evolutionary spectral sequence. He worried over the effect of increasing acceleration due to gravity at a stellar surface as the star contracted, and how this change affected the distribution and state of the material within the star, "... where the spectral phenomena have their origin ..."<sup>193</sup> In considering the two branched temperature hypothesis, Huggins added:

> ... Concurrently with a rise and then a fall of temperature, other conditions brought about by increasing gravity, especially the potent one of density, will come in, which must modify and may even mask more or less completely, the changes in the spectrum which would follow directly from differences in temperature ...

Variable masking was believed to exist due to the varying density of the photospheric regions of stars in different stages of contraction. The mode of transport of energy in young and old stars (based loosely upon Schuster's 1897 remarks) was thought to differ, where progressive condensation would increase convection and decrease radiative transport, though both modes were to some extent present in every star. The last idea, that both radiative transfer and convection existed in all stars certainly reflects modern thinking. Density changes at the photosphere and just above were thought of as the dominant factor in spectral line interpretation. He went so far as to suggest that the density decrease caused by the rise of the photosphere with contraction was actually greater than its increase, caused by general contraction. Further, the decrease was aided by greater rates of diffusion caused by the greater gravitational force. Thus, photospheric densities in solar stars should be less than those for earlier type stars.

Huggins was emphatic in pursuing the role of density and wrote persuasive letters to friends about it. Hale, of course, was considered a pivotal member of the astronomical community, and therefore received much commentary from Huggins. In this case, Huggins wrote concerning his <u>Atlas</u>:

> ... you will have noticed in the book the emphasis I lay ... on the probability that the conditions brought about by advancing condensation in stars -<u>density, greater & less, distance within the</u> <u>boundary of the spectrum region - convection</u> <u>currents - more rapid gradients, etc.</u> (are) more <u>directly potent influences on the star's spectrum,</u> <u>than temperature alone.</u><sup>194</sup>

Huggins' continued discussion of evolution contained many aspects seen before. He reconsidered non-evolutionary composition effects and, as before, concluded that they could not account for the observed composition differences in stars. He also mused over evolution rates dependent inversely upon mass.<sup>195</sup>

He then turned to spectral classification, repeating his strong conviction that classification must reveal the process of evolution, within which the relative density from stage to stage must be the "guiding principle" <sup>196</sup> of that natural system of classification. His great emphasis on the role of density might very well have stimulated Alexander Roberts and H.N. Russell to independently examine the densities

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of Algol variables at the time. Both papers came out as Huggins was making these remarks.

Huggins relied upon Vogel's classification system and supported its evolutionary significance. Leaving out stars of the Wolf-Rayet type, since they were still so poorly understood, Huggins began with the white stars as the least condensed. The subclass represented by Bellatrix came first, supported by association with nebulae, especially the Trapezium stars in Orion. Here Huggins also used the statistical findings of Frank McClean, which implied that the Helium stars were the most recently formed from nebulae.

Within his discussion of the earliest type stars, Huggins repeated his earlier contention that they did not represent the state of maximum temperature. It was clear enough that Huggins was driving towards a double valued temperature curve, but it is curious why he wished to emphasise it so often in his Atlas, since he was also convinced of the secondary role of temperature. Much of his subsequent discussion dealt with the assignment of maximum temperature, which he was to eventually place at the solar stage, using a Wien-type measurement of the continuum brightnesses of stars of Vogel's classes. To Huggins. the stars with the most extended ultra-violet continua would be the hottest. He was not aware that the Balmer limit strongly depressed the continuum brightness in stars with dominant hydrogen absorption. He therefore finally came to the conclusion that stars of the solar class were the hottest, since they apparently had the strongest ultraviolet continua, measured roughly in the spectral region blueward of 4000 Angstroms.

For stars redder than solar type, Huggins envisioned greater continuum absorption and line absorption, due to ever increasing density. Curiously, Huggins did not consider the possibility that this

increased absorption might be masking even higher temperatures. He commented simply that the shortening of the continuum beyond the Capella stage could be due in part to an actual cooling and subsequent drop in intensity. He makes no argument for why this might be so, which seems strange, for he clearly could have invoked Lane's law to declare that at points beyond the solar stage, the stars were no longer gaseous.<sup>197</sup>

Huggins decided that the Sun was presently at its maximum possible temperature. Of course, the geological record was too short to be able to say anything here, but Huggins did manage to use a statement from C.A. Young's <u>General Astronomy</u> wherein he had concluded that the Sun's temperature had been constant over geological time (mainly from the continued presence of water on Earth). With this from Young, Huggins decided that the only place in its evolutionary life where the solar temperature could appear constant would be during a relatively flat maximum. Huggins also used the radiometric work of Nichols at Yerkes on stars of Vogel's 1st, 2nd and 3rd classes that seemed to show that solar type stars put out the most energy. Nichols had found that Arcturus put out about 2.2 times the radiation of Vega. As we shall soon see, Hale, Nichols' boss at Yerkes, in a review of Huggins' Atlas, felt that Huggins' conclusions at best had to be considered along with other alternative explanations, such as the possibility that the apparent angular diameter of Arcturus surpassed that of Vega.

Even though at the outset of his <u>Atlas</u>, and in his 1891 Presidential Address, Huggins uttered grave warnings against the application of laboratory experimentation to the study of celestial spectra, he himself indulged in the practice happily. To be fair, though he stated that temperature and density were the prime factors

influencing laboratory spectra, he also was quite clear that he found it nearly impossible to separate out the two physical parameters. In so doing, he dispelled any ideas that the correlation of laboratory phenomena with stellar phenomena was a straightforward process - which was to his credit.

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## Huggins' Discussion of Binary Evolution

Huggins felt that the comparison of the spectra of components of double stars was an extremely important element in the study of evolution, since the studies by Poincare, George Darwin, and T.J.J. See had all shown theoretically how such systems originate and evolve. From their work, Huggins believed that double stars arose by the separation, or fission, of an originally nebulous mass into two or more bodies. From this mode of origin, as opposed to capture, the two stars within a double system would necessarily be of the same chronological age, since they should have been at the same evolutionary stage at the moment of separation. In addition, he expressed the belief that they should contain the same composition, although he considered the possibility that due to diffusion in the original nebular mass, the lesser of the two fission products might well have a lighter mean composition.<sup>200</sup>

Huggins felt that, if the components were of unequal mass, then the smaller of the two should evolve quicker. From this, he believed that a spectroscopic examination of double systems would be of great value in determining the primary cause of successive changes in spectra - whether they are due to increasing condensation or to compositional differences. He suggested comparing the spectra of doubles with the spectra of widely spaced and independent stars. If the spectral classes found amongst doubles agreed in every respect with those found

amongst single stars, then the effect of increasing condensation would be the mechanism of spectral change, since components of double stars would usually be expected to have basically the same composition, notwithstanding the mass differences. Huggins concluded in the affirmative for this test, as he was able to find spectra in double star systems that represented all major stages of his spectral classification scheme, and hence all stages of evolution.

Huggins considered the possibility that the relative ages of double star systems since fission might be determined from their spectra. Double systems with early type components (white stars) would have separated recently, whereas red doubles would have been separated at some time in the more distant past. If the prevalent theory of evolution was correct, then one would expect to see red stars in wide systems, and white stars in close systems. This would only work when both components of the system were of approximately equal mass. When component masses differ, Huggins expected to see them differ in their stages of evolution. He qualified this, however, by feeling that a knowledge of the radiation emitted by the white stars as compared with the red stars was as yet too poorly determined to discuss, since the relative effect of a decrease in surface area with age countered what Huggins believed to be an increase in temperature with advancing age and condensation, though this increase was masked by increased atmospheric absorption. As we have seen, he believed in part from Nichols' work that red stars were actually brighter. In this he was correct, since the sample studied included what we would refer to today as red giant stars.

At the time, Huggins and his wife had had difficulty reconciling their views of evolution with several well-known binary systems. In 1897, one of these,  $\beta$  Cygni, or Albireo, gave them

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considerable trouble. Here, the brighter component is yellow and the fainter one is blue.

We have, therefore, to face the apparent anomaly that it is the 'larger' star which is in the more advanced stage of development. It may reasonably be suggested that we really know nothing of the true relative masses of the stars, and that we have no certain ground for assuming that the brighter star is actually the larger one ...<sup>201</sup>

The first sentence above caused Huggins to ask speculatively if greater mass could, indeed, cause a more rapid progress of evolution. But all he did was conjecture: no conclusions were reached. In correspondence with Young in 1897, Huggins went a bit further in explanation:

> The Gordian difficulty which this view seems to raise on See's hypothesis I ventured, some years ago, with more than Alexandrian audacity to cut by the simple supposition that the blue star is much larger, and so still gaseous; and the extreme thinness of K shows that the calcium vapour there is much more tenuous than in the other star. I pointed out in 1891 that the brightness of a star would be affected by the emissive power of the substances mainly present in its photosphere. The yellow star possesses doubtless a more completely formed photosphere, and by Lane's law has become much hotter. In talking the matter over with Stokes some years ago, he seemed to think well of this way of looking at the matter.<sup>202</sup>

By bringing in masking, and Lane's Law, Huggins could argue that the hotter star was not the brighter. It is ironic to know today that the great effort Huggins expended in explaining this star went for nothing, as it is generally concluded today that Alberio is but an optical double, the two "components" being only associated by projection on the celestial sphere.

Huggins' <u>Atlas</u> marks his last major effort in purely astronomical matters. In the first few years of the 20th century, and the last of his life, he became preoccupied with the laboratory study

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of the spectrum of emanations from radium. His <u>Atlas</u> reflects his career achievement, concerning which he was enormously possessive. He was, therefore, keenly interested in reactions to it, notably from George Ellery Hale.

## Reactions to Huggins' Atlas

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Hale's review was quite fair and supportive, but it did provide several alternative explanations for ideas expressed in the <u>Atlas.</u><sup>203</sup> First, in reviewing Huggins' use of the extension and strength of the ultraviolet continuum as a measure of relative temperature, Hale agreed that its use for this purpose was considered a fair criterion, but that mere comparison of equal continuum regions accounting only for relative differences in line absorption was not enough. The redder star could very well appear brighter if it were larger than the blue star.<sup>204</sup>

This view was quite opposed to Huggins' evolutionary scheme, for blue stars were necessarily larger than red stars. Hale did not point this out, but Huggins did oomment about it in correspondence. In only a few scattered publications at the time, primarily J.E. Gore's popular books, is any discussion of this possibility extended.

Hale's second point dealt with Huggins' conclusion, based upon Langley's work, that continuum absorption in the ultraviolet for solar stars was greater than that for blue stars. Hale felt that difficulties in testing this conclusion were not only in understanding the "unequal effect of the increasing general absorption ..." but also from the variation of the position and number of dark lines in the spectra from class to class. Instrumental error and terrestrial atmospheric problems had to be considered also. Hale did not criticise Huggins' conclusions, but only repeated them, though in a different manner, employing the "head of the hydrogen series", or in modern terms, the Balmer decrement:

Just beyond the head of the hydrogen series the spectra of stars of the first class seem to fall off greatly in brightness, while the spectra of solar stars, though they do not extend so far into the ultra-violet, nevertheless appear to be stronger in this region when equally intense in the blue.<sup>205</sup>

Thus while Hale identified the position of decreased continuum absorption in stars of the first class as blueward of the decrement, the lack of understanding of the cause of the diminution (which came only upon the arrival of the Bohr atom), led him to review Huggins' conclusion without comment. Hale, however, wondered about Huggins' use, or interpretation, of Nichols' work on stellar heat radiation:

> If the accuracy of the heat measure could be relied upon, if the important law  $\lambda_{\max} \times T = \text{const.}$  could be considered to hold for both stars, and if the effect of increasing absorption were not most marked in the shorter radiations of the solar star, it might be fair to conclude that the maximum in <u>Arcturus</u> is displaced toward the red, and consequently that its effective temperature is lower than that of <u>Vega</u> - a result opposed to that reached by Sir William and Lady Huggins.

Hale had confidence in Nichols' work, but mentioned that "... while Paschen's law, which applies to the 'black body', is not rigorously true for either star, it is at least safe to say that the maximum moves toward the red with falling temperature ..." Huggins' criteria for the variation of line and continuum absorption with advancing spectral class, based upon his own observations, were regarded by Hale as important, since they were at the time the only observations available on the subject. Thus, Hale felt that "... great weight must attach to their conclusions, even though they differ from commonly accepted views ..."<sup>206</sup> He did feel that further observations, would be needed to settle the issue.

Hale felt that Huggins' section on historical spectra were "... of great service in tracing the course of evolution from star to star ..." and that his discussion of evolution was the most important section of the <u>Atlas</u>. Huggins' evolution commentary was convincing, and "... the strongest evidence is afforded by the gradual change in spectrum from star to star, and the possibility of forming unbroken series beginning and terminating in widely different types ..."<sup>207</sup> This last comment no doubt referred to Lockyer's scheme, where differentiation between the earliest and latest types was difficult.

Huggins' reaction to Hale's discussion was quite mild; though he found the idea of red stars being larger than white quite difficult to swallow, and suggested that evolution itself contradicted the idea:

> The assumption that the solar stars are of larger size is scarcely probable, as it is generally admitted, they are at a more advanced stage, and consequently in a more condensed condition than the white stars.<sup>208</sup>

Huggins added in a footnote, in his letter to Hale, a possible explanation for the apparently discordant Nichols measures in the far red region of the spectrum:

> N.B. There is another way of looking at the bearing of Nichols' ultra-red observations on the relative effective temperatures of solar and blue stars. It is suggested in the <u>Atlas</u> that in the case of the early white stars there may be no true photosphere of solid or liquid particles, but the light may come from a considerable depth within, and chiefly from matter in a gaseous state. In this case Paschen's law would probably not hold and consequently Nichols' observations would give us no certain information of the relative temperatures of the two classes of stars ...

This of course would have been fine for Huggins, who believed that his ultra-violet continua were sufficient criteria for temperature measures. But if he were to apply the above reasoning to Nichols' work, why not to his own? We see here, I believe, an example of the limitations that existed upon understanding the nature and source of continuum radiation, especially at a time five years before the defining works of Schuster and Schwarzschild. Huggins also showed a lack of understanding of Paschen's and Wien's determinations of the character of heat radiation. These laws pointed to an inverse linear relationship between temperature and the maximum point of radiation (i.e. colour) and were most certainly applicable to the gaseous state. 209 It should be noted that though Paschen's law was mentioned in private to Hale, its applicability as discussed in Huggins' Atlas was only implied. Huggins knew that he was not on solid ground. In fairness it must be said that, during the nineties, rival temperature/radiation relationships were common, including several that had the maximum wavelength of radiation varying as the inverse square root of the temperature.<sup>210</sup> Even in 1902, Clerke voiced the opinion of many astronomers of the time by noting that laws such as these, which gave such discordant values for the temperature of the Sun "... cannot be trusted far out of sight ... " and added "... They are not true enough to bear extension into regions beyond experience ...."211

There are many elements in Huggins' discussion which will survive in one form or another in Russell's work. First, it is clear now that he envisioned a double valued temperature curve, but neglected to discuss it explicitly. Next, his interpretation of binary fission was similar to one put forth by Russell, though both Russell and Huggins followed Darwin to a great extent. Third, Huggins' insistence on the importance of density as the guiding evolutionary variable cannot be underrated, since it was not until Russell had determined, with Shapley, that the two classes of red stars differed chiefly in density that we

see Russell firmly presenting his case.

#### H.C. Vogel and other Potsdam Workers: 1895-1900

One of the primary researches at Potsdam, completed later in the decade,  $^{212}$  was an extensive catalogue of photographic stellar spectra which allowed Vogel to restate his classification scheme in  $1895^{213}$  in the same format as 1888, but with greater resolution. As with Lockyer's work, the new observational material came from the identification of the cleveite gas lines in the spectra of the hottest stars.

Vogel's listing of the cleveite lines in the stars of his first class showed a wide variation in the number and strengths of the lines, which allowed him to comment:

> The examination of these numbrous spectra has again strengthened my opinion that only general and far-reaching characteristics should be considered in classifying stars according to their spectra, and that a rational system is conceivable, only on the basis that the different spectra of the stars are indications of different stages of development.<sup>214</sup>

This comment was intended to refer to Pickering's Harvard work, in addition to his own, for he continued:

> ... In my opinion, it is to be regretted that, in the comprehensive spectroscopic Durchmusterung of stars down to the 7th magnitude, which Pickering has undertaken with an object-glass prism, the stars are classified without reference to any general considerations, but are merely divided into sixteen classes, designated by the letters A to Q, according to the appearance of the spectrum, which is frequently liable to misinterpretation in the case of improperlytimes exposures, especially those on the brighter stars.<sup>215</sup>

If Vogel had looked a bit further into Pickering's work, or at least into the explanatory statements by Fleming and others, he might have seen that evolutionary considerations were indeed present, though

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not to the extent found in his own commentary. Vogel felt that the years since his first classification had confirmed the importance of the consideration of a line of evolution in the construction of any classification scheme. Even with the increased facility of photography, however, he still felt that visual techniques were superior to delineate IIIa from IIIb stars since the variations were to be seen only in the red, and for the additional reason that in ...

... the subdivisions <u>a</u> and <u>b</u> of my system, the criterion for deciding which of the two represents the more advanced stage of development is entirely lacking ...  $^{216}$ 

He then continued to justify his combination of Secchi's III and IV stars.

Only this much can be said, that in both subdivisions the atmospheres of the stars have so far cooled that dissociation has come to an end, and chemical combinations can exist. There are consequently no grounds for placing the stars of class IIIb, the absorption bands of which are mainly produced by hydrocarbons, in a special IV class.

The only significant change in Vogel's revised scheme of 1895 was in his first class, which was redefined in terms of the bright cleveite lines, the nature of the continuum, and relative strengths of the hydrogen and helium lines. Even though Vogel knew that his Ic stars should have been first in the series, he declined to change his notational order, to avoid confusion, even though,

> According to the present standpoint, it might seem better to give the first place to the few stars whose spectra contain <u>bright</u> lines, as representing the first stage of development; but since, in my opinion, a final decision of this question is not yet possible, I have retained the order of my former series, on formal grounds, and have again placed these stars together under a third subdivision,  $c.^{217}$

Vogel did not conclude, as did many others (e.g. Lockyer and Huggins), that helium spectra denoted an earlier stage of development

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than hydrogen spectra.<sup>218</sup> This might have been due to the presence of bright cleveite lines in the solar corona (though this is conjecture). Vogel's failure to reorganise his classification drew some mild criticism.<sup>219</sup> We can understand Vogel's caution in the light of Frost's remarks<sup>220</sup> that Vogel had always believed in a short, but invisible, heating phase for stars, prior to their incorporation into his classification. The actual place of Ic relative to those lines characteristic of nebulae had to remain in doubt, therefore; especially since bright-line structure could very well be due to an extended atmosphere, or some nova-like process.

Vogel's system had considerable influence on Scheiner and Dumer, and was found by them to best represent an order of evolution based upon theoretical argument. Scheiner wrote a commentary in the <u>Potsdam Publications</u> based upon it, entitled "Investigations of the Spectra of the Brighter Stars by Photography".<sup>221</sup> N.C. Duner later reviewed Scheiner's remarks,<sup>222</sup> and an extract appeared in translation in 1898<sup>223</sup> in the Ap.J.

Duner's discussion of Scheiner's paper was considered by Hale and Frost "... as representing the latest statement by the highest living authorities, of the theory of stellar evolution, as revealed by the spectroscope ...."<sup>224</sup>

Dumer explained Scheiner's evolutionary process in terms of the changing structure of the stellar interior and atmosphere. In the early stages, the star maintained an extensive hydrogen envelope causing the strong characteristic lines of the Balmer series. The extent of this atmosphere, in terms of the size of the star, was the chief determining factor in producing either a pure dark-line spectrum, a combination of a dark-line and a bright-line spectrum, or a dominant bright-line spectrum. The dark-line hydrogen spectrum indicated that

the region of rarefied hydrogen gas was not much larger than the radius of the star. Here the metallic lines would be very weak, since the absorbing layer was at a very high temperature, as indicated by the blue colour of this stellar class. The rapid radiation of energy that accompanied this stage of evolution, with subsequent contraction "... which balances the loss of heat ..."<sup>225</sup> caused a general cooling of the absorbing regions, so that subsequent stages of development would see the rise of metallic spectra. It would seem that Duner would have noted if Scheiner had, in his original essay, discussed the behaviour of a condensing gas sphere in accordance with the laws of Lane, Ritter or Kelvin. Except for the maintenance of heat energy density through contraction, there seems to be no consideration of such matters:

> On account of rapid radiation there follows a contraction which balances the loss of heat, especially in the interior of the star, but there is a decrease in the volume, not only of the inner sphere of gas but also of the atmosphere. In consequence of the lessened height of the latter, the fall in temperature becomes more rapid and the absorbing layer becomes cooler. Metallic lines, and in some stars helium lines, then show more clearly in the spectrum ...<sup>226</sup>

Lacking here is any direct statement about the mean temperature of the stellar interior and its change upon contraction. Certainly as the star contracts, the partial maintenance of interior heat, along with a steeper temperature gradient from centre to edge could cause a drop in atmospheric temperature at the region of absorption, assumed to be above the level of the photosphere. In consequence of this drop, the metallic lines would strengthen, with a corresponding decrease in the hydrogen lines. Scheiner felt that the decrease in the hydrogen spectrum could be due to escape of the gas from the contracting star [as Schuster later suggested], or a thinning of the hydrogen layer in consequence of the migration of the photosphere. Duner preferred the second mechanism, as a consequence of contraction:

For instance, as the diameter of the star and with it the hydrogen atmosphere, decreases, the previously formed photosphere sinks towards the center. Further condensations occur in the cooler atmosphere above, and thus the upper limit of the photosphere approaches the upper limit of the atmosphere. The absorbing layer of hydrogen therefore becomes thinner ...<sup>227</sup>

Since the photosphere was believed to be a region of condensation, it was temperature dependent, and a general cooling would, of course, cause the photosphere to sink.

Scheiner's concept was quite different from Huggins' view that with advancing evolution, the photosphere rose higher and higher into the stellar atmosphere. As a result, the densities of their absorbing regions were far different. Huggins' rose with age, while Scheiner's dropped.

Scheiner's view of an extremely low density absorbing region above the photosphere was created to support the observation of a well defined solar limb. Schwarzschild later confirmed this low density condition, but invoked radiative equilibrium within a region of both absorption and emission without recourse to a photospheric region of condensation to account for the well defined limb of the Sun and limb darkening in terms of a gaseous model.

With the increase of metallic line spectra, Scheiner's and Duner's evolutionary scheme progressed to stars of Vogel's class IIa. With further cooling, molecular spectra appeared and the star progressed into Class III. In contrast to Lockyer's opinions, and to Huggins' stated concept of the effect of a dense and opaque masking atmosphere, Duner then commented:

> Whether the photospheres of the stars of Classes IIa and IIIa are really cooler than those of Class Ia, must remain unsettled, as it cannot be distinguished whether the weakness of the violet

end of the spectrum arises from a lower degree of incandescence of the photosphere, or a stronger absorption in the atmosphere.<sup>228</sup>

This was, of course, Scheiner's opinion, as repeated by Duner. Duner himself felt that "... the weakness of the violet part of the spectrum in Classes IIa and IIIa, always, or at least generally, arises from absorption in the atmosphere of the star ..." This was in agreement with Huggins, as far as the effect of masking was concerned. But we must recall that Huggins believed that Class II stars were actually brighter in the violet than either class I or class III. Thus while they agreed as to the nature of the effect of masking, the spectra they had chosen as examples, derived from different equipment and photographic techniques, led them to different conclusions. For Huggins, the photospheric layer of a solar star was hotter than that of a Sirian, while in Duner's and Scheiner's more common view, the opposite held.

In the conclusion of this translated review, Duner commented on Vogel's classification:

> The investigations of the author <u>Scheiner</u> furnish a very strong support to Vogel's classification of stellar spectra. It is to be hoped that they may also contribute to the adoption of this only rational classification so that finally the Types <u>of</u> Secchi7, to which astronomers have clung with a persistence worthy of a better cause, may disappear forever from astrophysical works. It is sad to think that a classification is still used, at the best of doubtful accuracy, for which it can only be claimed that it was proposed a few years before that of Vogel, when the science of astrophysics was in its earliest stage, and it was not yet possible to form a rational classification.<sup>229</sup>

While it was certainly reasonable to push for the adoption of any one system of classification, justification in terms of an appeal to a rational basis dependent upon theories of evolution was met with criticism. Frost had made editorial comments to this effect in his translation of Scheiner<sup>230</sup> and his comments were later supported by James Keeler,<sup>231</sup> who went on to outline various problems with a rational classification:

Only the upper portions of star spectra, where photography is applicable, have been adequately studied, and it would be easy to mention special cases in which a classification based upon the appearance of this region would be in error, or at least fail to represent the entire truth. The stars of class Ib are too few in number to be regarded as a general transition form, and no classification except Lockyer's atlempts to trace the connection between Secchi's third and fourth types; while the facts brought out by the researches of Campbell on the Wolf-Rayet stars, the character of the spectra of the nuclei of planetary nebulae, and the association of stars of class Ia and class Ic with extended nebulae show that there is still much doubt as to the position which should be assigned to the various kinds of bright-line stars in a general classification based upon a theory of development.<sup>232</sup>

Keeler had confined his remarks to the earlier classes, probably because these were the only ones Scheiner and Duner dealt with in detail, and further because these had spectra closest to those of nebulae - which were Keeler's main interests. He therefore did not mention the continuing difficulty with the classification of the red stars - whether they formed two distinct classes, or were both variations on one general class. George Ellery Hale, however, was keenly interested in this problem, which was made difficult by the fact that so few red stars of Secchi's Type IIIb or Vogel's IV had been adequately observed. He was also in a good position to attack the problem, having the world's largest refractor and the proper spectroscopic equipment easily to hand.

# G.E. Hale's Study of Secchi's Type III and IV Stars

One of the first projects initiated at Yerkes after its completion and dedication in 1897 was a study of Secchi's Type III and IV stars. Hale hoped to settle if they were similar, as Vogel felt, and if the bright line spectra seen in some of them was real.

Hale stated that the recent work of Vogel and Wilsing, Huggins, Lockyer and McClean had all afforded "... additional material for an extensive study of stellar development ..."<sup>233</sup> As he then went on to discuss difficiencies in the present knowledge of Type IV stars, we can only conclude that he had the evolutionary discussion in mind as a motive. This, however, was never explicitly stated by him.

Hale, working with Ferdinand Ellerman, made a preliminary announcement of their work in 1898,<sup>234</sup> concluding tentatively that fourth type stars indeed possessed bright lines. He reasoned this since they were best visible under high dispersion, had line widths that were independent of the width of the spectrograph slit, and seemed to be brighter than neighbouring continuum regions. Hale also noted that two of the lines were very close to bright lines seen in Wolf-Rayet spectra by Campbell. This association seemed to be further strengthened by J.A. Parkhurst's study of the similar spatial distributions of Type IV stars and Wolf-Rayet stars - both were highly confined to the plane of the Milky Way.

Hale's preliminary study concluded that there were no "... very striking similarities (between Type III and Type IV7, though there seem to be certain resemblances ... which deserve and will receive further study ...." $^{235}$ 

Huggins reacted quickly and predictably to Hale's observation of bright lines and expressed fear that Hale had not adequately accounted for possible contrast effects. After expressing his surprise, and making various suggestions, Huggins warned:

> I think before deciding, you must have some other ground than merely the appearance of the plate.<sup>236</sup>

Hale carried out Huggins' suggestions, taking several spectra of a IVth Class star each with different slit widths, and again came to the same positive conclusion. This time, Huggins congratulated Hale and added:

> ... Do not think that I have any objection to bright lines in IIIb stars; or that I am unduly conservative, and not ready to welcome and rejoice in any new views which are well founded. I was, and am anxious that you should be careful as I know so well the deceitfulness of photographs.<sup>237</sup>

By 1899, Hale and Ellermann had improved their spectroscopic instrumentation (substituting a multi-prism train and faster camera for the original single prism and long-focus camera). Now, Hale was able to identify a sequence within the fourth class noting that "... This grouping presumably represents the normal order of development ..."<sup>238</sup> and further that his examples "... may be taken as fairly representative of well-defined steps in the evolutionary process ..."

In this new work, he re-confirmed the existence of bright lines. Still, at the time, he was not able to find any clear evolutionary transition between the Type IV stars and any of the other types.

As soon as Hale concluded negatively in his search for transition types, Ellerman found some, which resulted in another "bulletin" from Yerkes.<sup>239</sup> He was able to show that the two types were almost identical in the visual range, but diverged somewhat in the red, though many points of comparison still existed. This allowed him to conclude:

> These photographs serve to confirm the common belief in the essential similarity of the two types of red stars, and may perhaps afford material for a study of their development.<sup>240</sup>

By May, 1899, Hale was again ready to discuss his work; but added little new, except for a brief and tentative discussion of the existence of hydrocarbons in Type IV spectra, a combination of carbon and hydrogen expected by Scheiner<sup>241</sup> to exist as stellar atmospheric temperatures fell with advancing age.

Hale's work over the next few years took several directions. His main energy was spent in organising and equipping the new Mount Wilson Solar Observatory. His work on red stars did not cease, however, for, during 1903, he continued to direct Ellerman and Parkhurst (the former was to follow Hale to California, the latter to remain at Yerkes) in the task of compiling an extensive catalogue of Fourth Type Stars. With Walter Sydney Adams at the Solar Observatory, Hale embarked on a new direction: the comparison of the spectra of red stars with sunspot spectra, and the determination of temperature criteria from this comparison. A third direction at the Solar Observatory was the study of laboratory spectroscopy and the examination of the behaviour of metals in the arc and spark. Much of this work was reminiscent of Lockyer, and is of interest to our discussion, as Hale felt his work to be of importance to evolution. 242 We will review in turn each phase of Hale's work, and examine how it was received by Huggins and Lockyer during 1908.

The significant goals of Hale's laboratory investigations were the role of temperature and density in causing spectral changes, and the identification of calcium dissociated in the Sum, a conclusion contested by Huggins in correspondence. Hale's confirmation of the carbon identification of the banded structure in Secchi's Type IV stars and Fowler's identification of the TiO or Ti origin of the bands in Secchi's Type III in 1904 and his subsequent identification of the same bands in sunspot spectra in 1906 (simultaneously with Hale's staff at Mount Wilson) were also important for evolution discussions. These steps formed a link between solar and later type stars, as Hale was

eventually to conclude in his book The Study of Stellar Evolution in 1908.

In early 1904, as Hale became more and more involved in securing funds for his solar observatory in California, his continuing study at Yerkes of red stars was at a stage where an extensive review and discussion was prepared, with the assistance of Ellerman and Parkhurst.

The role of evolution as a stimulus for research into these objects was made immediately evident in their introduction:

The possibility of basing a systematic scheme of stellar evolution on spectroscopic observations is foreshadowed in the work of Fraunhofer  $\dots ^{243}$ 

In Hale's opinion, Secchi's scheme was purely empirical, "... intended to serve only as a convenient means of grouping similar spectra ..." though we have shown that Secchi was not unaware of evolutionary considerations. Hale continued:

> ... But the researches of Huggins and Vogel soon introduced the idea of development, and the changes of spectra from type to type came to be regarded as synonymous with progressive changes in the stars themselves ...

After this came a brief review of the course of evolution, starting with the low density white stars, and continuing to the solar stage and finally to the red stage. But here, the course became blurred, in Hale's opinion:

> It is not clear, however, why there should be two distinct classes of red stars, characterized by widely different banded spectra ...<sup>244</sup>

The problem was not so much with the Type III stars, but with the IVth type, since none were brighter than magnitude 5.3. The obvious advantages of the Yerkes telescope were mentioned, and this was indicated as being the stimulus for the initiation of the research in 1898. Various aspects of the long programme included gathering comparison spectra of other types, a continued study of the reality of the bright lines seen in the Fourth Type spectra, a parallel study of carbon in the solar chromosphere, widened lines in sunspots, and allied studies of the carbon spectrum in the laboratory.

Hale used both photographic and visual observations to carry out his tests for bright lines and requested that Campbell and Keeler at Lick attempt to confirm his own positive findings, which they apparently did, at some time prior to Keeler's death in late 1900.<sup>245</sup> Upon quoting confirmatory remarks from Keeler (in correspondence) Hale commented:

These results are in striking contrast with those obtained by Sir Norman Lockyer, and reported by him in his article, "The Piscian Stars"  $\dots$  <sup>246</sup>

Lockyer's Piscians were at the bottom of his descending temperature branch. He had studied these stars in 1894 and 1895 and had found "... In addition to the carbon bands, numerous lines were seen without much difficulty, but only the more prominent ones could be satisfactorily measured ... No suspicion of bright lines was entertained during these observations. Attempts to photograph the spectra were not sufficiently successful to help matters ...,"<sup>247</sup>

Lockyer would not be expected to want bright-line spectra to appear in these stars, as they were most removed from nebulae. That Hale came to opposite findings was not immediately accepted by Lockyer, from an examination of Hale's published spectra in 1899. Hale felt in 1903 that Lockyer's reaction was understandable as the originals were far more decisive than the imperfectly reproduced spectra. Hale did wonder why Lockyer was not able to see the bright lines in his own spectra, since "... A three-foot reflector should be admirably adapted for the investigation of these stars, whether visually or photographically. And yet the bright lines, which should have been easily visible, were not

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seen, while  $H\beta$  was recorded as a dark line ... As a matter of fact our photographs show no dark  $H\beta$  line in any of these stars".

No attempt is made here to account for the differences seen by Lockyer and Hale, save for mentioning that Lockyer believed Hale's observations to be due to contrast effects. Ironically, those stars in which Lockyer did expect to see bright lines - his Antarians - at the base of the ascending branch and still in a meteoritic state, were soon found to be without the bright features by his own assistant, Fowler, who interpreted the illusion in terms of contrast also. Of course, Fowler's identification of the banded structure in these Antarian stars with titanium, or titanium oxide, as reported in the same paper,<sup>249</sup> also created some embarrassment, as their original identification with carbon by Lockyer was an important element in his Meteoritic Hypothesis.

As to the identification of the bright lines in the fourth type stars, Hale could say nothing certain. They did not coincide with any known series or known materials. The only possible link was that there was some similarity to the bright lines seen in the Wolf-Rayet stars, as deduced from Campbell's work at Lick.<sup>250</sup> Hale repeated many of the identifications of helium and the 'second series of hydrogen' that were made by Campbell for the lines in the Wolf-Rayet spectra, but felt that the differences in position (amounting to several angstroms) between these lines and the ones in the Fourth Type stars rendered any confidence in identification very weak. While he also felt that the similarity between the two types of stars was worth noting, no evolutionary link on the basis of bright-line spectra was going to be made, since the production of such spectra, and their occurrence in so many peculiar spectra rendered it doubtful if they could be used for classification.<sup>251</sup> Hale's comment here is interesting. Lacking any

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believable classification criteria, he felt he was not able to discuss evolution.

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# The identification of dark lines in the Fourth Type Stars

Hale's interest in the dark lines of Fourth Type stars centred about those which were seen most widened in sunspot spectra. After a long tabulation of these lines, from his work, and studies by Keeler of these lines in Type III stars, Hale concluded that the Type IV spectra agreed closely with spot spectra, and to a greater degree than agreement between Types III and IV. However, the degree of disagreement between all three sources was slight, so "... If the lines widened in Sun-spots are to be regarded as characteristic of fourth-type stars, they seem to be equally characteristic of stars of the third type ....<sup>252</sup> This conclusion held evolutionary significance for Hale:

> Sun-spots are presumably to be associated with a late rather than an early stage of solar development, and there is reason to suppose that they may grow more numerous as the Sun continues to cool. On <u>a priori</u> grounds, therefore, they might well be expected to be prominent features of red stars. The strong tendency of these stars to variability, which is even more pronounced in the case of fourth-type than in that of third-type stars, certainly does not lessen the probability that numerous Sun-spots are present.<sup>253</sup>

Lockyer did not agree with Hale's interpretation, and preferred to explain the spectral similarity as due to generally lowered temperatures in the red stars, which was how he was able to say that his Arcturian stars were cooler than Capellans.<sup>254</sup> Hale did have several other arguments supporting lowered temperatures in red stars, but, at the time, his spot-spectra studies were not among them.

In subsequent years, Hale extended the study, directing Walter Sydney Adams to examine the brightest Type III star in the sky -Betelgeuse. He would have preferred to study a fourth type star, but none was bright enough to be studied at high dispersion with the Snow Solar Telescope. In 1906, Hale and Adams repeated their belief that similarity with spot spectra indicated these stars possessed large spots: thus compared with Lockyer's belief in a generally lowered temperature, though the latter was also a reasonable conclusion. Later in the year, Adams used the Snow telescope on Arcturus, which required accumulated exposures of 23 hours duration. This study brought Adams and Hale closer to Lockyer:

... Should it prove, as seems at present very probable, that the differences between the spectrum of the Sun and that of spots are to be accounted for on the basis of a lower temperature in the latter, we must also infer a lower temperature for <u>Arcturus</u> than for the Sun.<sup>255</sup>

Adams then recalled his previous work with Hale on Betelgeuse, which showed a lesser correlation:

> We have, accordingly, in Sun, Arcturus, and  $\alpha$ Orionis, a series arranged in the order of increasing differences for the characteristic spot lines. In the event referred to above namely, that these differences can be accounted for on a temperature basis - this sequence would represent a scale of descending temperatures.<sup>256</sup>

At the end of his paper, Adams repeated comments by Baxandall (an assistant to Lockyer at South Kensington) written to Hale. Frank Baxandall had also been studying Arcturus and had come to similar results, though they had been published under Lockyer's name. Baxandall wrote to Hale expressing surprise that his letter had been mentioned, and cautioned that it would have been better to refer to Lockyer's paper, rather than to Baxandall's earlier letter, for obvious political reasons.<sup>257</sup>

Hale's opinion changed to Lockyer's view later in 1906. As he pointed out in a paper on sunspot spectra with Adams and Gale: Formerly we were inclined to the view that the presence of spot lines in the spectra of red stars indicated the presence of spots like those on the Sun. Our recent work has led us to the opinion that the comparatively low temperature of these stars offers the simplest explanation of the observations.<sup>258</sup>

Recourse to spot spectra as a temperature criterion was thus established, and while the work at Mount Wilson and South Kensington was in harmony here, other aspects of Hale's discussion of Secchi's Fourth Type stars in 1904 were not in agreement at all with Lockyer's views.

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## The Distribution of Fourth Type Stars in Space

By the time of the memoir by Hale, Ellerman and Parkhurst, a considerable advance had been made in the number of Fourth Type stars recognised. In 1898, Parkhurst investigated the distribution of 242 Fourth Type stars with respect to the Milky Way and later extended his work by a comparison of the density distribution of these stars with that of about 10,000 <u>Durchmusterung</u> stars in a similar magnitude range.

Comparing the degree of condensation (number counts) of red stars with that of the general field from the <u>Durchmusterung</u>, Parkhurst found that the red stars tended to concentrate in the plane of the Galaxy to a far greater degree than did the general population. A possible association of this distribution with bright line stars of the Wolf-Rayet type, also highly concentrated in the Milky Way, was then noted.

The remainder of their discussion dealt with the physical condition of the red stars, their classification, and their role in the process of evolution.

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# <u>Conclusions as to the Physical Condition, Classification and</u> <u>Evolution of Fourth Type Stars</u>

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From their work, the authors verified the long-held assumption that these stars exhibited both general and selective absorption to a
greater degree than did solar stars. The greater strengths of the metallic lines indicated a spectrum "... such as would probably result from the cooling of a star like the Sun ...."<sup>259</sup>

As another indication of decreased temperature, Hale used the behaviour of the 4227 line of calcium, which was known to increase in strength with a fall in temperature, and, according to Huggins, with an increase in density. Hale noted that this line was about at the same strength in both third and fourth type stars, but that further study would be needed to determine whether the strength of the 4227 line was a temperature or density effect. He indicated that studies were in progress at Yerkes which "... may permit the effects of temperature to be distinguished from those of density ..."<sup>260</sup>

As would be expected, Hale and Huggins corresponded on this point. Hale wrote to Huggins in June, 1903, discussing the work of Hartmann (who was very sceptical of temperature effects) and noted that \*... I confess that I think one could go too far in denying the effect of temperature changes ... "At the time, Schuster had spent some time as Hale's guest and apparently the two held the same opinion. Hale was cautious not to disregard Huggins' emphatic view regarding density, but it was evident that Hale favoured temperature as the primary variable affecting the 4227 line. Hale wrote extensively to Huggins prior to publication in 1903 to elicit his reaction, and maintain diplomatic relations. Thus, Hale congratulated Huggins on his discovery of the helium spectrum in radium, which was to be an important argument for the presence of some source of radioactive energy in the Sun and stars - beyond gravitational contraction. Huggins' reaction to Hale's letters has not been found, but can be inferred from Hale's later letters. Apparently, Huggins lectured Hale at length about the illusory effects of varying exposures and other instrumental problems. 262

In addition to calcium, the lines of titanium in the region of 4534-4536 were considered important for classification. With the exception of one of these lines, at 4534.14, which was seen also in early Orion type stars, all the lines were strong. The single line seen in Orion type stars, believed to be an enhanced line by Hale, was very weak. This behaviour was appropriate for a reduction in temperature. The line was considered to be an important classification line, as it appeared in the early type stars, increased in strength as one progressed through the Sirians, reached maximum intensity in stars like Alpha Persei, and then decreased through the solar stars and the third-type stars, where it was found to be the weakest of the titanium group. Usually, it was absent entirely in fourth-type spectra. The behaviour of the other titanium lines in the group was just opposite. They were strongest in the fourth, third and solar types respectively.

An important conclusion drawn from this was that the gross dissimilarity of the behaviour of the titanium lines in fourth-type and Wolf-Rayet spectra made it difficult to associate the two types. In spite of the evidence from distribution and from the appearance of bright lines, this was the major factor causing Hale to separate them in terms of stage of development.

Another aspect of Hale's study at this time, with a bearing upon classification and evolution, was his discussion of stars with mixed spectra - including nebulae, Wolf-Rayet, and types III and IV following Kayser's arguments for spectral effects caused by cooling in hot stars.<sup>263</sup>

Kayser, using theoretical arguments (radiation laws of Planck and Wien), showed that in bright line objects, relative line strengths could yield temperatures. From Hale's studies of hydrogen in the Sun, which concluded that the coefficient of absorption decreased with increasing wavelength, Kayser was able to discuss further the effect of temperature upon mixed spectra, and showed that lines in the visible should reverse sooner than lines in the blue. His own arguments indicated that the region of greatest reversal (dark line intensity) was also the region of strongest continuum emission.

Kayser felt that if the absorption coefficient could be effectively modified by the nature of the black body radiation curve, some interesting conclusions would result:

> From the position of that line in the spectrum where the darkness of the reversed lines begins rapidly to increase, some idea of the temperature of the absorbing enclosure could be obtained, the temperature being higher the greater the number of bright lines. I admit that the phenomenon becomes more complicated by Campbell's beautiful observation that the intensity of the hydrogen lines does not necessarily decrease from the first member but may actually increase for the first few members of the series, which seems to me to indicate a very high temperature ...<sup>264</sup>

Thus to Kayser, a bright line spectrum indicated a very high temperature, and a mixture of bright and dark lines indicated that the stellar nucleus did not behave like a solid, and therefore had a density far below other stars:

> If then a star, in which the whole hydrogen series is bright, gradually cools down, the series will by degrees become reversed, beginning at the lowest members and last of all H $\alpha$ . Campbell therefore was perfectly correct ...<sup>265</sup>

Hale followed Kayser's interpretation for the Wolf-Rayet stars, based upon Campbell's observations, and concluded that the Wolf-Rayet stars showed a shift in the maximum intensity of their hydrogen lines to the violet when compared with those in the Sun; and therefore were at a higher temperature, and hence at an earlier stage of evolution, quite separated from the red stars.

An extension of this line of thought led to some trouble, however, for if the maximum of intensity in a bright line spectrum shifted to the blue with increasing temperature as it did for the radiation from a continuous source, both the nebulae and that portion producing the bright line spectrum of Wolf-Rayet and fourth-type stars would have to be at high temperatures. While nothing was said about the supposed temperature of nebulae, it was difficult to suppose that the fourth-type stars possessed regions of high temperature. Hale therefore felt that this study was inconclusive for the moment, and indicated that Schuster was also working on the question of the cause for mixed spectra.

Schuster's famous paper, which "set the stage"<sup>266</sup> for the study of radiative equilibrium in stars, was entitled "Radiation Through a Foggy Atmosphere" and dealt with the role of scattering. While his paper appeared in January, 1905, in the <u>Astrophysical Journal</u>, he had been working on the problem for several years, since a preliminary paper appeared in 1902. Much of what he was working on was of direct interest to the case of line reversals. As he wrote to Hale in 1903:

> I have now refreshed my foggy memory in matters of the foggy atmosphere. The following are the principal results:

- Everything else being equal, the rays of greatest emission power are those most likely to be reversed. This probably accounts for the absence of D<sub>3</sub> among the Fraunhofer lines.
- (2) The scattering being equal for all rays, the least refrangible rays are first reversed.
   This would be the case in the bright line stars.
- (3) If the scattering is due to small particles, like the scattering in our atmosphere, the more refrangible rays are scattered so much more, that the more refrangible rays /become/ more easily reversed ... This seems the case of the (\*illeg.\*) ... /sun?/

The effects (2) and (3) may be present simultaneously, so that it is quite possible for C to be bright, F & H $\gamma$  to be reversed & the ultraviolet lines to be bright again. The appearance of the enhanced lines in the spectrum of the flash suggests that these lines are not conspicuous amongst the Fraunhofer lines because their emission power is too great. In fact in a foggy atmosphere it is not to be expected that the emission spectrum should be the reversal of the absorption spectrum. I shall give a short account of this to the British Ass. & I don't think it will take me long to write out the full paper, which I will send you for publication as soon as possible.<sup>267</sup>

After Schuster completed his formalism in his 1905 paper, he concluded: "... The essential criterion which separates the brightline emission from the dark-line absorption lies in the temperaturegradient of the luminous gas ....<sup>268</sup> He continued:

> The temperature gradient is chiefly regulated by the gravitational force, and a star in the early stages of condensation will therefore be in the condition most favorable for the bright-line emission. If the light is but feebly absorbed, so that we can look into considerable depths of the star, it may be possible that the outer regions contribute bright lines, while the hotter inner portions show absorption lines.

Schuster noted that his discussion was quite similar to that of Kayser. Unlike Kayser, he did not need a high temperature gas for the production of bright lines. All that was needed was a temperature gradient, which, Schuster believed, argued more for low temperature conditions.<sup>269</sup>

This removed an important constraint on the temperature of nebulae, and of bright-line stars of the fourth-type, not to mention Wolf-Rayet stars.

Another important consideration was the order of appearance of lines with increasing condensation, and, hence, increasing temperature gradients. Schuster used the persistent H and K lines as an example. Noting that their prominence might be due to a high value of their absorption coefficients, Schuster remarked that from the experiments of the Hugginses showing the persistence of these lines, "... We are justified in concluding ... that the emissive power of H and K is very great ..."<sup>270</sup> In this manner, he felt that the lines that first become reversed, as a star continues its evolutionary process of condensation, would be those with high values of the absorption coefficient. Unfortunately, for the time, the discussion was moot, since absorption coefficients were almost totally unknown and would not be generally available until the 1920s.

Schuster also examined the effect of scattering on the intensity distribution of the continuum radiation arising from the stellar photosphere. When the radiating layer of a star had cooled to the point where condensation of particles of solid and liquid matter appeared whose dimensions were large compared with the molecular level, the reduction of intensity would not be selective, and would not affect the observed distribution. "There would consequently be no great alterations in the relative intensities of red or blue, and we could obtain a correct idea of the temperature of the radiating body by a . thermal comparison of the intensity of radiation in different parts of the spectrum".<sup>271</sup> On the other hand, when the condensation particles were small, or only of molecular dimension, scattering would be much greater in the blue than in the red and:

> Consequently the radiation emitted by a mass of gas under these conditions would show the violet considerably weakened as compared with the red. This opens out the possibility that with increasing temperature the violet portion of the continuous spectrum of a star may diminish in intensity as compared with the red end ...

This fascinating possibility could very well have been applied by Huggins in his scheme where condensation produced heating but which was masked by a more extensive atmosphere. It is not known whether Huggins ever made mention of this. In later correspondence with Hale in 1907, Huggins seemed to be quite interested in radiative equilibrium, but it was from Schwarzschild's discussion in 1906. No mention of Schuster's work was made by Huggins.

The work of Schuster and Schwarzschild on radiative equilibrium marked the first extensive attempt to explain the observed solar limb-darkening.<sup>272</sup> Their work, however, did not generate much interest, and had to await Eddington's revival ten years later.

Several years prior to this work, Schuster offered an explanation of stellar evolution that Hale later was to discuss with interest in his book, <u>The Study of Stellar Evolution</u>. Schuster's discussion, "The Evolution of Solar Stars" appeared in April, 1903.

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## Schuster's View of Stellar Evolution (1901-1903)

Schuster's theory of solar evolution was originally presented to the Royal Philosophical Society of Glasgow in November, 1901. It was revised and published in the <u>Ap. J.</u> in April, 1903. Pertinent to our major question were his introductory remarks:

> Everyone recognizes that some kind of evolution is clearly indicated by the manner in which star spectra classify themselves into groups which, though distinct, are yet connected with each other by intermediate types.<sup>273</sup>

Schuster felt that most of those involved agreed that some form of evolution was taking place, but that the course of evolution for any one particular star was certainly an open question:

> Does each star or, at any rate, the great majority of them, pass through each of the stages of a uniform evolution? Has, for instance, our Sun at one time given a spectrum identical with that of  $\alpha$  Leonis? Further, are we justified in concluding that all stars are made up of the same chemical elements in the same proportion? And lastly, admitting a uniform evolution, what is the meaning, as the star grows older, of the gradual displacement of the hydrogen in its atmosphere, first by calcium, and ultimately by iron and other metals?

After a brief review of observational and experimental considerations, Schuster turned to theoretical studies of stellar constitution, beginning with the driving force - gravitational contraction. After arguing for convection in the Sun, Schuster then reviewed the theory of convective equilibrium of Lane, Ritter and Kelvin, and the resulting behaviour of a gas sphere contracting, but retaining its ideal gas state. He did not believe that any star with a condensation region (visible photosphere) could be totally in a state of a perfect gas, because this condition would require absolute zero at the "surface" boundary. He did feel, however, that it was probable that below this region of condensation, Lane's Law would hold to a considerable depth.

"Ultimately", Schuster added, "and especially in the case of stars which are already advanced in their condensation, the equations will fail, because the molecules of a gas become as near to each other as they are in liquids, molecular forces come into play, which prevent the gases from behaving in the ideal manner of a perfect gas. The molecular forces diminish the compressibility, and ultimately, the heat which is generated by compression will fail to compensate for the heat lost by radiation. When that period has been reached the star will begin to cool, pass into the liquid state, and soon cease to be luminous".<sup>274</sup>

This lucid description of Lane's and Ritter's theory was then put to the test, using the visible surface of the Sun as proof of the reality of convective equilibrium. He then progressed to a series of stellar and solar models, and discussed in detail density, pressure and temperature gradients within their interiors. At this point, he also wondered about the effects of conduction and radiation. Conduction was felt to be important at the stellar core, which was most certainly a

solid or liquid, while radiation was most important at the stellar surface, and especially significant above the photosphere. If radiation were actually dominant, Schuster reasoned that the high temperature gradients required for convection could be eased. At the time, Schuster could not produce a stable configuration employing radiative equilibrium alone. Direct evidence of solar convection seemed to settle this question.

Schuster then turned to:

... arguments that have convinced the great majority of astronomers of a process of evolution which in the course of time makes each star pass successively through a number of stages, in which the spectrum changes from that of the helium stars to that of the hydrogen stars, and hence to that of stars with prominent calcium lines and of the solar stars.<sup>275</sup>

These returned him to his original questions. The causes of observed spectral differences were crucial, whether dependent upon evolution or not. In either case, spectral differences still had to be accounted for (by temperature, composition or structural differences). But in terms of evolution, they had to be reconciled with the apparent paradox that, while the prevalent concept of the direction of age was that of a cooling process, the theory of Lane "... states that the star should get hotter ...,"<sup>276</sup> He continued:

> The apparent disagreement between theory and observation has been a stumbling-block to astronomers, but it is due in great measure to the want of definiteness in our meaning, when we speak of the 'temperature' of a star.

The internal temperatures of stars are not known through observation, though this is where Lane's law applies. "What we can observe", said Schuster, "is the photosphere and the absorbing layer above it, and the temperature of these portions of the Sun are not touched by Lane's theory ..."

Schuster examined various possible effects (both evolutionary

and non-evolutionary) upon the nature of the photosphere. These included contraction, and hence the increase of surface gravity, and with this, increased convection. Increased convection could bring more energy to the photosphere, and hence render it more luminous, but here, Schuster considered another heating source - meteoritic impact:

> ... we must not assume that the inflow of outside meteoric matter, which not so long ago was considered to be the chief cause of the maintenance of solar heat, is altogether inactive.<sup>277</sup>

Schuster went so far as to speculate that in younger stars, meteoritic influx might be at a higher rate, and may even become the dominant energy source. In this, he was reminiscent of Kelvin.

Another possible heating element in the photosphere was the 'greenhouse effect'. The solar atmosphere might trap infrared radiation from the interior and thus perpetuate its high temperature.

The various heating elements and impact mechanisms posed by Schuster were not meant to be taken very seriously, in order to reconcile Lane's Law with observation. He simply wished to present possibilities, and to illustrate the fact that alternatives were likely to exist.

Of course, the paradox could be solved from the other direction - that stars obeying Lane's Law actually did heat up, and that the many modes of temperature assignment used thus far for the spectral sequence yielded a backwards progression. He thus attacked dissociation, using the argument that the mean density of stars in general was greater than that of hydrogen, making it difficult to suppose that a large amount of stellar material existed in the dissociated state, assuming further that temperature did increase with depth. On the other hand, he did not feel that Huggins' alternative explanations were completely valid - that early type stars did not have much, or any, convective mixing, while later type stars did. He demonstrated this by taking the solar mass, expanding it to Sirian dimensions, and showing that its surface gravity was still in excess of 'g', thus making convection still important.

Still, spectral differences had to be explained. Schuster would not go so far as to invoke composition differences. He did provide one interesting alternative for the disappearance of hydrogen with advancing spectral class: occlusion - the physical entrapment of a gas by a liquid or solid. Here, a contracting star would create internal conditions that would progressively favour occlusion and absorption of hydrogen first, and then the heavier elements in turn.

At this point, he felt that he had speculated enough, and so decided to return to the realm of "facts  $\dots$ "<sup>278</sup>

From the studies of H.N. Russell, W.H.S. Monck, A.W. Roberts and many others, Schuster felt that relative stellar densities had been determined to enough precision to allow for their use in any confirmatory studies of the direction of evolution. But since these studies all were of binaries, utilising surface brightnesses to calculate relative radii, errors most certainly crept in where very dissimilar surface brightnesses were involved, since the relation of emissive power to surface brightness was still not codified. Still, he felt that the errors were smaller than the differences seen between spectral classes, and so came to a most interesting discussion. He stated that the range of density found within each spectral class was "considerably greater" than that found for the differences between the means of different classes. He gave a number of examples.

His first example was  $\gamma$  Leonis, a star similar in spectral type to Arcturus. Assuming equal emissive power area for area with the Sun, the density of this star came out to be 0.0002 that of the solar value. Schuster felt that  $\gamma$  Leonis would require an emissive power 300

times greater than the Sun's to account for this, which was untenable. Other examples included were of the solar type, where n Cassiopeiae was found to have the solar density, but another solar type star system,  $\gamma$  Virginis, was found with a density sixty times smaller.<sup>279</sup> Examples such as this caused Schuster to make the following observation:

> As far as the observations go, the stars which are purely hydrogen stars show smaller variations in density than the solar stars, and have a density which is unmistakably smaller, but great variations are found in the intermediate stages in which the calcium lines are prominent.

It is assumed, of course, that Schuster derived these conclusions from the work of those directly involved in double star work.

Schuster did not draw any conclusions from this significant observation; nothing, at least, as far-reaching as Russell's later identification of giant and dwarf stars. In perspective, Schuster had very few systems from which he could draw any conclusions. Russell, too, in the years 1909-1910, as he completed his early correlative studies of the spectra and luminosities of his selected sample, felt that far more data on stellar densities was needed before the two sequences could be credibly discussed. In addition, the observed spread within each spectral class, while large for solar type stars, was not as enormous as those found from Shapley's eclipsing binary studies in 1910-1913.

Nevertheless, Schuster did uncover the rudimentary density dispersion which later became a fundamental fact; but, at the time, his primary interest was in the run of density with spectral class. Again, the techniques of Russell and Roberts were referred to, as were their results for Algol systems, to verify the generally held belief that the Sirian type Algols were less dense than solar type stars: The spectra of these stars all seem to belong to the pre-solar type, and their low density therefore confirms the results arrived at from the consideration of other binaries, though we should not lose sight of the fact that the average density of the stars of the solar type seems to be considerably less than that of the Sun itself, so that the average density of the <u>Algol</u> variables is not much less than half the average density of the solar stars.<sup>280</sup>

From the above, we immediately see that Schuster did not have the possibility of a collateral series of giants and dwarfs in mind. He was careful to consider the average densities, which, for solar type stars was biased towards lower values due to the inclusion of giants and sub-giants of solar type.

From binary studies, Schuster turned to further evidence from statistical studies, primarily the distribution of spectral types in space. Schuster nevertheless considered these lines of work less fruitful than direct laboratory comparisons with solar conditions.

Schuster's general view as to the present state of knowledge of stellar evolution was that the picture was clear for the early phases of contraction out of nebulae, and through the first and second types of spectra. The picture for the third and fourth types, however, was not as clear: this involved the possibility mentioned by several workers, that somewhere between nebulae and stars of the first type came a period of heating, represented by stars classed within the fourth and, possibly, third types. At the time, Fowler's study of the illusory bright lines in Type III Antarians and Hale's memoir which linked Types III and IV had not appeared.

The last part of Schuster's review was a general statement of his own theory of the progress of stellar evolution. Basically, contraction, heating, and increased occlusion caused the funadmental change from a hydrogen to a metallic line spectrum as a star progressed through the numerical types.

Schuster's 1903 paper continued to a discussion of secular changes expected in his evolutionary scheme. He wished to emphasise that his process allowed for greater individual variations in stellar life-histories:

> The amount of hydrogen, according to my present view, which a star is able to condense depends on its mass, and the amount of hydrogen which happens to be present in the neighborhood.<sup>281</sup>

Schuster's two main mechanisms, in response to contraction as the driving force, were distillation and absorption, with subsequent diffusion. Thus he felt he could account for the main features of the early and middle stages of evolution. He believed, however, that extending his reasoning to later types, the third and fourth, was not yet possible (though he did mention that bright lines in the fourth types did tend to indicate that they were young). In place of any explicit discussion, Schuster provided a more philosophical argument that, it was hoped, might allow for further progress. Basically, he objected to a rigid adherence to the principle of uniformity in discussing evolutionary progress:

> I know that the great principle of uniformity will be quoted against any supposition that a particular class of stars is essentially different in its composition from others, but I believe, on the other hand, that the skies bear ample evidence of real differences in composition.<sup>282</sup>

Schuster felt that the fourth type stars with carbon spectra must not be forced uncritically into an evolutionary line from the third class, on the basis of uniformity alone.

In developing his argument, he called uniformity a "fallacious guide", and warned that "... Examples are plentiful in the history of science where the law of uniformity might have been quoted, and has been quoted, in support of obsolete moribund theories ..."<sup>283</sup> Schuster was actually not against the concept in its present form, but seemed to

be reacting to a more rigid interpretation that very well could have been found in the writings of Lockyer. Thus, Schuster was able to conclude:

> The universe shows law, order, and regularity, but it refuses to be forced from birth to death through a single channel. There is uniformity no doubt, but it is a uniformity which at all times, and in all places, is relieved by endless variety.<sup>284</sup>

Schuster's scheme, then, might be classed as a synthesis of the rational and the empirical. While he considered observed facts as all-important in bringing about a scheme based on contraction under gravity, he felt that these facts must be considered independent of <u>a priori</u> judgments about mass and composition.

In accord with the ideas of the time, he conceived of stars as mechanisms, for he felt that "... by pure reasoning, and without any consideration of imaginary laws,  $\angle$ We are led7 to consider the universe to be in the state of a clockwork which is running down ..."<sup>285</sup> From this last quotation, we also see how his concept of mechanism accorded with the second law of thermodynamics.

Hale was impressed with Schuster's facility at synthesis, and invited him to visit Mount Wilson and Yerkes. As a result, their contact proved to be a significant influence on Hale. Of course, there were many other points of contact between the two, especially in the organisation of solar research on an international scale. Through the years following its foundation in 1904 in St. Louis, Schuster acted as chairman of the executive committee of the "International Union for Co-operation in Solar Research" which eventually became the <u>IAU</u>, and which was very much a result of Hale's organisational zeal.

# Hale's Classification of Fourth-Type Stars and Theory of their place in Evolution - (1904)

Returning now to Hale's memoir with Ellerman and Parkhurst, we will complete the consideration of his involvement in evolution up to the date of publication of his book.

Hale felt that he had established a link between types III and IV, but he was still not happy about the role of bright lines. Regarding Lockyer, Hale wrote:

> So far as the fourth-type stars are concerned, it therefore appears that Lockyer adopts the view held by other investigators, and confirmed by the present research, namely, that they represent the last stage of stellar development. But we do not think that he has given sufficient reasons for separating fourth-type stars from those of the In the first place, we are unable third type. to understand how the spectra of third-type stars can be considered to resemble in any way the spectra of nebulae, or to be evolved from nebular spectra. So far as we are aware, no star showing a spectrum intermediate in character between that of a nebula and the spectrum of a third-type star has hitherto been detected. This seems to us a most serious objection to Lockyer's classification. 286

No mention was made of Lockyer's contention that bright lines occurred in third type spectra, but later in his conclusions Hale noted that, since bright lines appeared in so many peculiar circumstances, and in so many different types of spectra, it was not possible to depend upon them for any classification decision. Schuster, in 1903, felt otherwise, and, presumably, so did Lockyer until Fowler's work in 1906.

Hale felt that any conclusions as to the evolutionary place of the Wolf-Rayet stars was premature, but that if they turned out to be related to IVth types "... it would conflict seriously with current ideas regarding stellar evolution ...."<sup>287</sup>

In conclusion, Hale listed evolutionary steps, which he felt were the best operating conditions at present. The first separated

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Wolf-Rayet stars from fourth types, and the second was that "... Fourth type stars probably develop from stars like the Sun through loss of heat by radiation ...."<sup>288</sup>

We note here with interest that much of Hale's activities coincided with those of Fowler. They both recognised the presence of the line spectra of titanium in late-type stars in 1904, and Fowler, in addition, realised that the fluted spectrum of IIIa stars was also due to titanium, or to its oxide. As a result, they corresponded frequently on many matters of interest.<sup>289</sup>

During the period 1904-1908, temperature criteria were firmly established as important in classification studies. Hale was also constantly mindful of the interdependence of evolution and classification. Possibly nowhere else was his interest in, and concern for, evolution so completely stated as in his book <u>The Study of Stellar</u> <u>Evolution</u>, published in 1908, but in preparation for several years previously.

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## Hale - The Study of Stellar Evolution - 1908

Originally intended as a handbook for the public depicting the Yerkes Observatory, Hale's procrastination and departure from Yerkes brought in many elements of research peculiar to his new solar observatory at Mount Wilson. The original intention of the book was to answer the most persistent questions asked by the public,<sup>290</sup> and to give an account of the goals and methodology of astronomical research. That he chose evolution for his topic and title reveals much about his own interests; so, too, does a public lecture he gave in 1908 on desirable trends in education - which included greater exposure to evolutionary theories in the organic and, supposedly, in the inorganic world.<sup>291</sup> Hale's book was not unlike other popular studies. The title attracted general interest, but the text was an advertisement for the mechanics of astronomical study at Wilson and Yerkes. All aspects of astronomical study were discussed, where they had a bearing on stellar evolution. Hale noted: "I finally adopted the plan of describing a connected series of investigations, laying special stress on the observational methods employed, in the hope of explaining clearly how the problem of stellar evolution is studied ..."<sup>292</sup>

Hale admitted that his coverage was incomplete, as he reviewed mainly what was being done at those observatories with which he was involved. We already have a reasonable idea of what went on at his two observatories, but a good overview of the way Hale did science is also available to us from some comments by J.C. Kapteyn to Frost in 1908. Hale's mode of inquiry was quite distinct from Kapteyn's. Kapteyn was an early visitor at Mount Wilson, and helped to develop various extended projects that were distinctive of his style. With the possible exception of Russell, Kapteyn was one of the most stimulating influences upon the observatory staff in its formative years. Yet, as he noted to Frost, his style was distinctly different from Hale's:

> Of course our lines of work and thought are very different. Prof. Hale, whose work is all devoted toward evolution, looks for special objects which seem to promise <u>results?</u> for his research. In my work, which is mainly statistical, we have to look for average conditions and nothing is worse than working on selected stars - Still, we are coming nearer together and I expect that **so**on after the beginning of the photographic work we will come still nearer.<sup>293</sup>

These comments, written to Frost, are of interest, as Kapteyn was soon to produce an extensive paper on evolution, most obviously influenced by Hale's interests, but highlighting the importance of evolutionary criteria based upon the statistical examination of spectra. Hale's introductory chapter dealt with historical theories of stellar evolution. Spectral classification schemes were immediately recognised as crucial in "... marking definite stages in an orderly process of development ..."<sup>294</sup> His commentary continued in a manner reminiscent of Lockyer:

> We are now in a position to regard the study of evolution as that of a single great problem, beginning with the origin of the stars in the nebulae and culminating in those difficult and complex sciences that endeavor to account, not merely for the phenomena of life, but for the laws which control a society composed of human beings.

Hale's scenario of the progress of research at Mount Wilson left the reader with the distinct impression that all lines of astronomical research eventually led to the study of stellar evolution,<sup>295</sup> and that this particular study probed the deepest and most fundamental questions that man could ask.

Distinctly missing from his text was an exposition of advances from the theoretical side, save for a later discussion of Schuster's review.

After a lengthy review of sunspot studies, Hale voiced his general conclusion as to their role in evolution studies:

... it should follow that if the entire Sun, or a star like the Sun, were cooled in the same degree, its spectrum would resemble that of a sunspot ...

... Our ideas of stellar evolution are based on the belief that stars exist in all stages of development and differing sic? greatly in temperature. If our inference be correct, we should find, among the stars which have passed by continual cooling beyond the solar stage, some in whose spectra spot lines appear ...<sup>296</sup>

Hale's next chapter on stellar temperatures outlined how this type of test might be applied, and followed his studies culminating in his 1904 memoir on stars of Secchi's fourth type. Briefly, Hale remained disposed to accept the common relationship between temperature and colour, but admitted that masking effects by stellar atmospheres kept the study open, and far from consensus, which was a correct assessment. On the other hand, Hale admitted that the reduced temperatures of red stars was almost a "belief" with him, more from evolutionary considerations than from anything else:

> It seems to be true that the older and cooler stars have denser atmospheres than the younger and hotter ones. It is thus probable that the stars whose spectra contain the greater proportion of red light actually are cooler than those in which the violet light is relatively stronger.<sup>297</sup>

Hale's later chapters on various aspects of stellar evolution emphasised the influence he saw from the Nebular Hypothesis, which had recently been severely tested and modified by Moulton and Chamberlin. He felt that their "... searching test ..."<sup>298</sup> did not alter "... the widespread and favorable influence exerted by the hypothesis on the intellectual life of the nineteenth century ..."<sup>299</sup> in much the same way as Darwin's organic evolution would remain "... even if the hypothesis of natural selection were forced from its place by that of mutation ..."

To Hale, stellar evolution and the origin of the solar system were not independent problems. He thus discussed at length Herschel's work that led to the idea that stars evolved out of nebulae. This, in turn, ushered in his discussion of the progress of evolution, as deduced from spectral classification at Harvard and elsewhere. In his discussion, Hale did allow for a brief pre-stellar heating phase in a star's life, by quick recourse to Lane's Law, which held until a star developed a true photosphere.

After a review of the commonly accepted course of evolution, (which had a flavour similar to Schuster's remarks) Hale turned to

another of Schuster's interests - the validity of the doctrine of uniformity. With Schuster, he warned against describing all stars in terms of a standard model with uniform composition pervading all space. The question of mass as a defining variable for evolutionary paths of stars was brought up, and this then brought in the persistent confusion caused by binary systems. In these, the primary and secondary components did not follow normally expected colour/stage of evolution/mass patterns (i.e. fainter components were found to be more advanced, etc.).

Hale noted that studies of the motions of stars could yield important information about evolution. He pointed to obvious examples open clusters assumed to have stars all at the same age - as important for determining the mass dependence upon the rate of evolution. He then reviewed Kapteyn's work, which, with the clusters, was of importance in that "... community of motion may mean organic relationship of stars in a group ..."<sup>300</sup>

Hale's later chapters discussed Lockyer's Meteoritic Hypothesis and the Planetesimal Hypothesis of Chamberlin and Moulton. Hale was most interested in George Darwin's efforts to reconcile Lockyer's theory with the Nebular Hypothesis. But he felt that Lockyer's identification of the chief nebular line, disproved by Keeler in the early 1890s, was a near fatal blow; though the origin of nebular light was conceded to be still a mystery. Hale did discuss Lockyer's temperature arch and general classification, including dissociation, with definite favour. Here we might note that by 1920, writing in <u>The New Heavens</u>, Hale heartily accepted Russell's revision of Lockyer's arch, and gave credit to Lockyer as the pioneer. Regarding Lockyer's classification, Hale, in 1908, noted its problems, but added:

... the classification nevertheless deserves careful consideration, and the most searching tests that can be applied. $^{301}$ 

The remainder of Hale's book dealt with variations in solar heating, construction of large telescopes, opportunities for amateur observers, and other popular topics. Since the book was intended to be general, Hale kept back many problems. Notably, he neglected to mention that, while theories of evolution and schemes of classification were intimately connected in astronomical research (a fact quite evident from a reading of his book), he nowhere indicated what confusion existed in the classification schemes in existence at the time. During the period of writing Stellar Evolution, Hale most certainly was becoming acutely aware of the problems caused by the lack of an agreed classification. As we shall see in Chapter 5, his close associate, Frost, pleaded in 1904 for work that would eventually lead to a consensus, and in so doing set in motion plans for a committee on spectral classification. This first met at the 1910 meeting of the International Solar Union at Mount Wilson - a meeting carefully planned by Hale.

We thus end our discussion of Hale's ideas and of the dependence of nineteenth-century schemes of spectral classification upon evolution. We now return to the 1890s, and pick up an important theme dealing with classification and evolution which will lead us to Hertzsprung and the origins of the HR Diagram - the statistical studies of spectral classes.

#### References

- 1. William Herschel, Philosophical Transactions (1814), p.264.
- 2. William McGucken, <u>Nineteenth Century Spectroscopy</u> (Johns Hopkins Press, 1969).
- 3. <u>Ibid.</u>, p.29 ff.
- 4. M.A. Sutton, "Spectroscopy and the Chemists: A Neglected Opportunity?" <u>Ambix 23</u> (March 1976), p.16.
- D.M. Siegel, "Balfour Stewart and Gustav Kirchhoff: Two Independent Approaches to Kirchhoff's Radiation Law" <u>Isis 67</u> (December 1976), p.565.
- 6. E.T. Whittaker, <u>A History of the Theories of Aether &</u> <u>Electricity. Volume 1</u> (Harper Torchbook, 1960), p.369.
- 7. <u>Op. cit.</u>, McGucken, ref. 2, p.23.
- G.R. Kirchhoff, <u>Phil. Mag. 21</u> (Ser. 4), p.241; cf. A.J. Meadows, <u>Early Solar Physics</u> (Pergamon, 1970), p.114.
- 9. R.H. Curtiss, "Classification and Description of Stellar Spectra" <u>Handbuch der Astrophysik V</u> pt. 1, (1931).
- 10. G.B. Donati, <u>Monthly Notices</u> 23 (1863), p.100.
- 11. Agnes Clerke, <u>History of Astronomy 2nd Ed</u>. (Black, 1887), p.420.
- 12. Axel Nielsen has commented <u>/</u>"History of HR Diagram", <u>Centaurus</u> 1963, p.21<u>9</u>7 that Buffon allegedly supposed the colour of a star was a measure of its age, the white stars cooling to red in old age. Arago (<u>Popular Astronomy I</u> (1855), p.296), in an extended discussion of the colours of double stars and of temporary stars, speculatively considered blue stars as being stars in the process of decay, and asked: "... if the different shades of those stars do not indicate a process of combustion in different stages; if the tinge with an excess of the more refrangible rays, which the smaller star frequently

exhibits, does not arise from the absorbing power of an atmosphere which might be developed by the action of the usually more brilliant star which it accompanies ..."

- d'Arrest, <u>Astronomische Nachrichten 84</u>, p.360; <u>85</u>, p.249.
   Cf. A. Nielsen, <u>Op. cit.</u>, ref. 12, pp. 219-220.
- L.M. Rutherfurd, <u>American Journal of Science and Arts 35</u> (1862), p.71; <u>35</u> (1863), p.407.
- 15. J. Carpenter, <u>Monthly Notices 23</u> (1863), p.190.
- 16. Airy, <u>Monthly Notices</u> 23 (1863), p.188.
- 17. Rutherfurd, <u>Op. cit.</u>, ref. 14, p.77.
- M.J. McCarthy, S.J. "Fr. Secchi and Stellar Spectra" <u>Popular</u> <u>Astronomy 58</u> (April 1950), p.153.
- 19. Curtiss, <u>Op. cit.</u>, ref. 9, p.5.
- 20. A. Secchi, <u>Le Soleil</u> (1870).
- 21. Secchi, <u>Comptes Rendus 57</u> (1863), p.71; translation from: <u>Op. cit.</u>, ref. 18, p.161.
- 22. Ibid. See also Curtiss, Op. cit., ref. 9, p.14.
- 23. Letter, Secchi to De la Rue (4 March, 1865) reprinted in Monthly Notices 25 (March 10, 1865), pp. 154-155.
- 24. Ibid.
- 25. William Huggins and W.A. Miller, Phil. Trans. 154 (1864), p.413; p.437.
- 26. <u>Monthly Notices</u> 25 (March 25, 1865), p.155.
- 27. Ibid.
  - 28. <u>Ibid.</u>, p.157.
  - 29. Secchi, Comptes Rendus 62 (1866), p.591.
  - 30. Secchi, Comptes Rendus 63 (1866), p.366.
  - 31. Secchi, <u>Comptes Rendus 63</u> (1866), p.621; <u>64</u> (1867), p.345; Report of the British Association (1868), p.166.
  - 32. Secchi, Memorie della Societe Italiana della Scienze I (1867),

p.105.

- 33. Curtiss, <u>Op. cit.</u>, ref. 9, p.9.
- 34. McCarthy, <u>Op. cit.</u>, ref. 18, p.163.
- 35. D. Hoffleit, "The Discovery and Exploitation of Spectroscopic Parallaxes" <u>Popular Astronomy 58</u> (November, 1950), Harvard Reprint 342, pp. 4-5.
- 36. Secchi, <u>Comptes Rendus 69</u> (1869), p.39; translation from McCarthy, <u>Op. cit.</u>, ref. 18, p.163.
- 37. E.C. Pickering, Astronomische Nachrichten 127 (1891), p.1.
- 38. <u>Op. cit</u>., ref. 18, p.164.
- 39. <u>Ibid.</u>, p.166.
- 40. Secchi, Comptes Rendus 64 (1867), p.778. See Ibid.
- 41. Secchi, Comptes Rendus 69 (1869), p.1076. See Ibid.
- 42. William Huggins, Monthly Notices 25 (1865), p.112.
- 43. William Huggins, <u>Report of the British Association</u> (1866), p.140; reprinted in Holden, <u>Essays in Astronomy</u> (New York, 1900), p.375.
- 44. <u>Ibid.</u>, p.382.
- 45. <u>Ibid.</u>, p.387.
- 46. <u>Ibid.</u>, p.388.
- 47. <u>Ibid.</u>, p.390.
- 48. <u>Ibid.</u>, p.377.
- 49. G. Johnstone Stoney, Proc. R.S. <u>16</u> (1867), p.31; <u>17</u> (1867),
   p.48.
- 50. <u>Ibid.</u>, p.31.
- A. Clerke, <u>Op. cit.</u>, ref. 11, 3rd Edition (1893), p.453;
  Zöllner's arguments were put forward in his <u>Photometrische</u> <u>Untersuchungen mit besonderer Rucksicht auf die physische</u> <u>Beschaffenheit der Himmelskorper</u>, (in <u>Wiss. Abhandlungen Bd</u>. <u>1-4</u>, Leipzig, 1865-1881).

- 52. Clerke, Ibid.
- 53. N. Lockyer, Phil. Trans. 164 (1874), p.492.
- 54. <u>Ibid</u>.
- 55. <u>Ibid</u>., p.493.
- 56. H.C. Vogel, <u>Astronomische Nachrichten 84</u> (1874), p.113; translated in Frost, E. (tr.) <u>Astronomical Spectroscopy</u> (Ginn, 1898), p.236. This was a translation of J. Scheiner, <u>Die Spectralanalyse Der Gestirne</u> (Potsdam, 1890). This work will hereinafter be referred to as: Frost, 1898.
- 57. <u>Ibid.</u>, Frost, p.237.
- 58. See: Clerke, <u>Op. cit.</u>, ref. 11, p.423; Curtiss, <u>Op. cit.</u>, ref. 9, p.15.
- 59. Letter, Henry Draper to C.A. Young (23 August, 1880), Dartmouth.
- 60. Letter, E.C. Pickering to E.S. Holden (29 July, 1881), Lick.
- 61. William Huggins, Phil. Trans. 171 (1880), p.678.
- 62. Correspondence between Huggins and E.C. Pickering three days after the former had made his first successful photographic observation of the Orion Nebula showed that no immediate identification of the fifth line was made:

... Besides the 4 lines which I found (one) year ago ... there is a line in the ultraviolet  $\lambda$  3730.

(Huggins to ECP 10 March, 1882,Harvard) This line had, of course, been seen in spectra of the white stars. Early in April, Huggins also wrote on this observation to E.S. Holden, but in a more open manner, where he expressed great relief in finally producing a spectrum of the nebula. He regarded the 3730 line as a "new" line, and again offered no indication of its origin. His long letter spent considerable time attempting to right an incorrect statement in Newcomb's book <u>Popular Astronomy</u>, which Holden had, in an early edition, co-authored. The error placed Secchi as the discoverer of the gaseous nature of nebulae. Huggins was very anxious that this be corrected, and expressed considerable disgust for Secchi's failure to provide credit to Huggins when he published his own observations. Huggins remarked:

> My own observation was made in August, 1864 ... Secchi's observations were long afterward and I had an account <u>from Struve himself</u> how they came about. Struve visited Secchi & told him of my discovery. Secchi at first would not believe it ... Secchi, in Struve's presence pointed to a nebula & saw one of (those) lines. Almost immediately Secchi sent out a paper to Academie Du Science, Paris, but without due acknowledgement ... (Huggins to Holden, 10 April, 1882),Lick Obs.)

Suffice it to say for the present that this was not the only instance where Huggins was to become agitated over

proprieties.

- 63. Huggins, Op. cit., ref. 61, p.689.
- 64. <u>Ibid.</u>, p.690.
- 65. Norman Lockyer, Proc. R.S. <u>44</u> (1888), p.2.
- 66. A.J. Meadows, <u>Science and Controversy</u> (MIT, 1972), p.206. See also: R. Hirsh, "The Riddle of the Nebulae" /in prep.7.

Conclusion also based upon work in progress.

- 67. Letter, N. Lockyer to E.C. Pickering (7 December, 1887) Harvard.
- 68. Norman Lockyer, Proc. R.S. <u>43</u> (1887), p.117 ff.
- 69. Letter, E.C. Pickering to N. Lockyer (12 January, 1888) Harvard.
- 70. Lockyer, Op. cit., ref. 65, p.40.
- 71. Lockyer, Op. cit., ref. 65, p.27.
- 72. "Council Note" Monthly Notices 49 (1889), pp. 222-223.
- 73. Norman Lockyer, The Meteoritic Hypothesis (MacMillan, 1890),

74. Meadows, <u>Op. cit.</u>, ref. 66, p.192 ff.

75. Lockyer, <u>Op. cit.</u>, ref. 73, p.448.

- 76. B.Z. Jones and L.G. Boyd, <u>The Harvard College Observatory</u> (Harvard, 1971), Chapters 4 and 5; B. Gee, <u>The Harvard Studies</u> <u>in Stellar Astronomy 1840-1890</u> (Univ. of London thesis, 1968);
  S.I. Bailey, <u>The History and Work of Harvard College Observatory</u> (McGraw Hill, 1931).
- 77. "The Draper Catalogue of Stellar Spectra" <u>Harvard Annals 27</u> (1890). It should be noted that the <u>Annals</u> were arranged topically, so that parts of volumes appeared at different times. Thus the numerical order of the volumes was not chronological.
- 78. E.C. Pickering, <u>Harvard Annals 26 pt. 1 (1891)</u>. See: Discussion.
  79. <u>Op. cit.</u>, ref. 77, pp. 2-3.
- 80. <u>Nature 35</u> (1886), p.37; in the same year, Pickering received a letter from Lewis Rutherfurd, who also commented upon the striking similarity of the spectra of the stars in this cluster.
- Letter, C.A. Young to E.C. Pickering (23 March, 1885), Harvard.
  Letter, E.C. Pickering to E.S. Holden (29 July, 1881), Harvard. In this letter, Pickering indicated an interest in the association of red stars with nebulae: <u>Op. cit.</u>, ref. 62.

83. Op. cit., ref. 77, p.4.

- 84. Curtiss, <u>Op. cit.</u>, ref. 9, p.28; Jones and Boyd, <u>Op. cit.</u>, ref. 76, p.235.
- 85. Letter, George Ellery Hale to E.C. Pickering (9 September, 1892), Harvard.
- 86. E.C. Pickering, Astronomy and Astrophysics 12 (1893), p.718.

Lockyer, The Sun's Place in Nature (MacMillan, 1897), p.270. 87. Pickering (Op. cit., ref. 86, p.722), quoted in Lockyer, Ibid. 88. 89. E.W. Maunder, "Stars of the First and Second Types of Spectrum", J. Brit. Astr. Assn. 2 (1891), p.35 ff, p.39. 90. Ibid., p.35. 91. Ibid. Ibid., p.37. 92. 93. D.H. DeVorkin, J. Hist. Astron. 6 (1975), pp. 5-6. 94. Maunder, Op. cit., ref. 89, p.39. 95. Ibid. 96. Frost (1898), p. ix. A. Fowler, "The Draper Catalogue of Stellar Spectra", Nature 45 97. (1892), p.428. 98. Ibid. 99. E.C. Pickering, Harvard Annals 26 (1892), p.75. 100. "Report of the Council" Monthly Notices 53 (1893), pp. 288-289. W.P. Fleming, Harvard Annals 26 pt. II (1897), Ch. IV, p.260. 101. An earlier suggested change, however, appeared in Pickering's Congress paper (Op. cit., ref. 86). In the paper he prepared for the Congress, Pickering reversed the order of the A and B stars without comment, save for the fading of "Orion lines" (a designation based upon lines seen prominent in stars within the constellation of Orion) as one passed from the B type stars to the A types. 102. Antonia C. Maury, "Spectra of Bright Stars", Harvard Annals 28 pt. I (1897).

103. Ibid., "Preface" by E.C. Pickering.

104. Jones and Boyd, <u>Op. cit.</u>, ref. 76, pp. 236-238.

105. Maury, <u>Op. cit.</u>, ref. 102, p.2.

201.

- 106. <u>Ibid.</u>, p.4.
- 107. Ibid.
- 108. <u>Ibid.</u>, p.5.
- 109. <u>Ibid.</u>, p.6.
- 110. <u>Ibid.</u>, p.7.
- 111. <u>Ibid.</u>, p.11.
- 112. Jones and Boyd, <u>Op. cit.</u>, ref. 76, p.238; R.H. Curtiss, <u>Op. cit.</u>, ref. 9, pp. 29-30.

113. Certainly Pickering was aware of the possible role of

temperature. Private correspondence with Lockyer during this period shows Pickering's interest. Lockyer wrote to Pickering in January, 1899, on his continued laboratory work with a new large induction coil:

> I have recently been continuing the enhanced line work with the large coil ... & the result is so interesting & important that I send you a photo ... You see that we have caught all the lines of  $\alpha$  Cygni by the enhanced spark lines so that this series is as important for stars of intermediate temperature as the lines of Helium & are for those of the highest, and classification will be greatly licked thereby. (Letter, Lockyer to E.C. Pickering (January 21,1899),Harvard)

Lockyer also commented upon the use of the term 'metallic' in Maury's discussion of lines seen in c stars which were not due to calcium, hydrogen, or the Orion lines, which Lockyer felt should have been referred to as "proto metallic". This was again in reference to his theory of dissociation, which from a similar letter on the same day to C.A. Young, was believed through these same observations to be finally settled. From his observation of the enhanced line spectrum, Lockyer commented to Young: ... As this settles dissociation I am now turning my attention to evolution ... (Letter, Lockyer to Young (January 21, 1899), Dartmouth)

Several days later, Pickering responded enthusiastically

to Lockyer's letter:

... The comparison of the enhanced spark-lines with those of α Cygni is, indeed, very interesting, and the similarity between the two spectra is striking in the extreme ... (Pickering to Lockyer (January 23, 1899), Harvard)

- 114. Maury, <u>Op. cit.</u>, ref. 102, p.12.
- 115. E.C. Pickering, "Introductory Note" <u>Harvard Annals</u> 28 pt. 2 (1901).
- 116. A.J. Cannon, <u>Harvard Annals 28 pt 2 (1901)</u>, p.138.
- 117. Miss Cannon provided a detailed step-by-step discussion of each class and sub-class. In discussing the many spectral peculiarities that could not be accounted for by instrumental error, she returned several times to the character of line structure, and followed Maury's work closely, allowing for her divisions to be deduced from remarks placed within the Catalogue itself, even though they didn't appear explicitly in the classification.
- 118. Cannon, <u>Op. cit.</u>, ref. 116, p.140.
- 119. <u>Ibid.</u>, p.141.
- 120. F.J.M. Stratton, <u>Astronomical Physics</u> (Dutton, N,Y., 1925), p.104; Curtiss, <u>Op. cit.</u>, ref. 9, p.36.
- 121. Cannon, Op. cit., ref. 116, p.142.
- 122. Norman Lockyer, Phil. Trans. <u>184 Ser. A</u> (1893), p.677.
- 123. <u>Ibid.</u>, p.696.
- 124. <u>Ibid.</u>, p.707.
- 125. Letter, Huggins to Hale (10 December, 1892) Hale Papers. Huggins'

- 126. James Keeler, <u>Astronomy and Astrophysics</u> <u>13</u> (1894), p.59. These included, in addition to Lockyer's discussion of evolution and classification, various claims Lockyer had made about the inefficiency of the Lick 36-inch refractor for observing nebular spectra - extended objects rendered faint by the long focal ratio of the refractor.
- 127. <u>Ibid.</u>, p.62.
- 128. Frost (1898), p.318.
- 129. Letter, W.H. Christie to Editor, Phil. Trans. /Royal Society Library <u>RR 11</u>, 181 (3 January, 1893)\_7.
- 130. Letter, G. Darwin to Editor, Phil. Trans. /Royal Society
   Library <u>RR 11</u>, 182 (13 January, 1893)\_7.
- 131. Ibid.
- 132. Letter, Lockyer to Editor, Phil. Trans. (18 July, 1893), Royal Society Library <u>RR 11</u>, 183.
- 133. Lockyer, <u>Op. cit.</u>, ref. 122, p.677.
- 134. Meadows, Op. cit., ref. 66, pp. 194-196.
- 135. Lockyer, Proc. R.S. <u>61</u> (1897), p.150.
- 136. <u>Ibid.</u>, p.187.
- 137. <u>Ibid.</u>, p.198.
- 138. Ibid., p.198. See: Chemistry of the Sun, p.377.
- 139. Ibid.
- 140. Meadows, <u>Op. cit.</u>, ref. 66, pp. 168-169; W.H. Brock, "Lockyer and the Chemists: the First Dissociation Hypothesis" <u>Ambix 16</u> (July, 1969), p.83.
- 141. Lockyer, Op. cit., ref. 135, p.205.

- 142. Meadows, <u>Op. cit.</u>, ref. 66, p.174.
- 143. <u>Op. cit.</u>, ref. 113.
- 144. A. Schuster, Proc. R.S. <u>61</u> (1897), p.209.

145. Brock, <u>Op. cit.</u>, ref. 140, p.98.

- 146. Schuster, <u>Op. cit.</u>, ref. 144, p.210.
- 147. <u>Ibid.</u>, p.211.
- 148. <u>Ibid.</u>, p.212.
- 149. Ibid.
- 150. <u>Ibid.</u>, p.213.
- 151. Lockyer, Proc. R.S. 61 (1897), p.213.
- 152. Letter, Huggins to Hale (21 April, 1897), Hale Papers.
- 153. <u>Ibid</u>.

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154. We will discuss the bulk of Huggins' commentary when we examine his work during this period. But for now, we will mention only a comment to Young in January, 1897:

> I hope now that Astronomical writers will not fill valuable space and muddle their readers with long extracts from the cryings among the South Kensington tombs. When you have to revise the proofs for the new edition of your General Astronomy you will find no little use for the scissors in this connection. I hear that he has now a paper to show the decomposition of iron and calcium in the solar atmosphere. Though I do not see any a-priori impossibility in this view, I do not think that there are any known facts which may not be more simply explained. I do not go with Hale in regarding calcium as really decomposed. The progress of chemical thought is increasingly in the direction of very great molecular mobility, but without true dissociation.

> > (Huggins to Young, (14 January, 1897), Dartmouth)

In both letters by Huggins, to Hale and to Young, we see what has been mentioned before, that Huggins did not regard dissociation as impossible. His negative approach was based upon personal animosity for Lockyer.

### In March, Huggins wrote again, this time at greater length:

For the dissassociation of calcium there are two chief points:- (1) Great height of H and K lines; (2) The smaller shift of these lines under pressure. A day or so ago I met Stokes and asked his opinion. Put shortly, his reply was: - (1) The commotion at and near the sun's surface from convection currents &c. is so great that differences of vapour density tell very little. It may be that other gases also are carried up high, but are not so easily rendered luminous as the calcium molecule. That we have to do with the internal energy of the molecule as well as the external energy in motions of translation and collisions. The greater energy due to the lower regions may be carried up in the molecule, and though this will not be maintained by sufficient collisions, it will exist for a time, and this internal energy might most easily set up motions corresponding to H and K. (2) Stokes thought it more probable than otherwise that any effect of crowding of molecules through pressure, would not be the same for radiations of all lengths of waves. He would expect some lines, or series of lines, to be more shifted than others. His conclusion from all the phenomena: that he would be very slow to see in them any indications of a true disassociation of the calcium molecule. (Letter, Huggins to Young (7 March, 1897), Dartmouth)

From this commentary, Huggins then wondered if, from Stokes' first point, "... there may be some body, not yet separated from calcium, with which terrestrially it is always associated, which may give the H and K lines ..." This body, as we know today, is the electron, but Huggins carried the idea no further, save to search for an alternative explanation of the appearance of H and K in prominences as a result of electric discharge. Huggins asked Young to consider his comment as speculation for the time.

155. Lockyer, <u>Nature 51</u> (1895), p.374.

156. Norman Lockyer, Op. cit., ref. 87, p.261.

157. Ibid., pp. 261-262.

- 159. Ibid., p.400.
- 160. Lockyer, Proc. R.S. 65 (1899), p.186.
- 161. Meadows, <u>Op. cit.</u>, ref. 66, p.199.
- 162. Lockyer, <u>Op. cit.</u>, ref. 160, p.191.

163. A comparison of Lockyer's spectral features identifying various temperature regions has been made with modern standard references (H. Abt, et al., An Atlas of Low Dispersion Grating Stellar Spectra, Univ. of Chicago, 1968; Keenan, Stars and Stellar Systems III (Univ. of Chicago, 1963) Chapter 8). Many of those features are still in use today, and certainly do represent temperature sensitive criteria. A substantial fraction do not, however.

Since Lockyer's time, reviews of his work by astronomers have varied somewhat. D. Hoffleit in 1950 (Op. cit., ref. 35, p.6 ff) noted that Lockyer's use of the relative widths of the hydrogen lines and the enhanced metals constituted "... precisely the criteria used later at Mount Wilson for separating giant from dwarf stars ... " R.H. Curtiss in 1930 (Op. cit., ref. 9, p.25) was quite critical of Lockyer's criteria, noting that his "... attempt to allocate stars to this curve on grounds of spectra and theory was too difficult to permit of complete success ...", and further that it was Lockyer's "... preference for conspicuous type stars and the weakness of his criteria at some points /which/ undoubtedly influenced his choice ... " of archetypal stars. Curtiss did conclude, however, that Lockyer had been judged too harshly, and that "... Lacking knowledge of densities and masses Lockyer's selections are nothing short of remarkable ... "

- 164. W.H.S. Monck, Publ. Astr. Soc. Pacific <u>4</u> (1892), p.98; p.104.
- 165. Lockyer, <u>Inorganic Evolution</u> (MacMillan, 1900), p.142.
- 166. <u>Ibid</u>.
- 167. <u>Ibid.</u>, p.143.
- 168. <u>Ibid.</u>, p.150.
- 169. <u>Ibid</u>.
- 170. Ibid.
- 171. Cf. Chapter 1, John Perry, <u>Nature 55</u> (1899), p.247.
- 172. Lockyer, <u>Op. cit.</u>, ref. 165, p.152.
- 173. <u>Ibid.</u>, p.157.
- 174. <u>Ibid.</u>, p.160.
- 175. <u>Ibid.</u>, p.190.
- 176. Huggins, <u>Report of the British Assn.</u> <u>61</u> (1891), pp. 3-37; reprinted in <u>Smithsonian Report for 1891</u> (Washington, 1893), pp. 69-102.
- 177. McGucken, <u>Op. cit.</u>, ref. 2, p.132.
- 178, Huggins, <u>Op. cit.</u>, ref. 176, p.72.
- 179. <u>Ibid.</u>, p.80.
- 180. Ibid.
- 181. <u>Ibid.</u>, p.84.
- 182. <u>Ibid.</u>, p.81.
- 183. <u>Ibid.</u>, p.82.
- 184. <u>Ibid</u>.
- 185. <u>Ibid.</u>, p.85.
- 186. <u>Ibid.</u>, p.101.
- 187. William and Lady Huggins, <u>An Atlas of Representative Stellar</u> Spectra (Wesley, London, 1899), p.10.

- 188. Ibid., p.38.
- 189. Ibid., p.67.
191. Huggins, <u>Op. cit.</u>, ref. 187, p.68.

192. <u>Ibid.</u>

193. <u>Ibid.</u>, p.69.

194. Letter, Huggins to Hale (30 April, 1900) Hale Papers. At the time, Hale had not yet seen the book, as it was still in press. Huggins' letters therefore anticipated his reading of it. In February, 1900, Huggins wrote announcing that the book was ready, and that somehow copies would be sent to the Smithsonian for distribution to Yerkes and other places. This was to save the cost of duty. Again, as an advertisement, Huggins noted:

> I have gone into a theory of the order of stellar evolution which I think has much in its favour, and which I trust will meet with the general acceptance of yourself, and Professor Frost, who I believe is now in the Yerkes Observatory ... We have not gone into stars of class IIIb, in which you have been working, as none of these are included in our photographs. (Huggins to Hale (3 February, 1900), Hale Papers)

Huggins continued by indicating that his photographic spectra had finally allowed him to examine the ultra-violet continua of IIIa stars, "... which leaves no doubt of their evolutional place ..."

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In conclusion to his stellar structure discussion in his <u>Atlas</u>, Huggins mentioned that "... It is hardly necessary to point out that the rate of evolutional progress in any star, as well as the state of the reversing region, will be largely determined by its mass. We should expect stars of relatively small mass to pass through their successive stages on to an advanced condition of condensation in a period less prolonged than stars of greater mass ..." (<u>Atlas</u>, p.72). As simple as this statement appears, even though we know it today to be incorrect, it caused difficulties at the time as binary data began to accumulate and show that not all stars within coeval systems behaved in this way. Examples appeared where the more advanced body was also the more massive (cf. Clerke, <u>Problems</u> <u>in Astrophysics</u> (1903), p.276). In fairness at this point we should comment that the role of mass exchange in close binary systems has made any study of the relative states of evolutionary progress for binary members extremely complicated, and certainly not representative of 'normal' single star evolution.

In 1892, T.J.J. See was just finishing his thesis in Germany, and was corresponding regularly with Americans back home in the hope of obtaining a suitable position. At the time, this man had not obtained the rather unfortunate reputation normally associated with him after the turn of the century. His early theoretical work on the dynamical behaviour of double star systems experiencing tidal disruption was believed to have great merit, though he was not reticent in promoting it personally. In a letter to C.A. Young in 1892, See discussed how some of his work was being received by astronomers and others in London. In his discussions with Huggins, as See related them, we find evidence of a change in Huggins' idea as to the possible causes for the rate of evolution with an emphasis on mass dependence. In discussing Darwin's return to his 'ring theory' of planetary formation from meteoritic swarms, which was a view See felt untenable, he commented:

> I hesitate to adopt in my paper a view that is in conflict with such eminent authorities, and I am therefore glad Dr. Huggins called my attention to a possible method by which the two views may be reconciled. This consists in supposing the smaller star to have lighter elements than the larger, and hence it might

actually condense more slowly, and at any time appear younger. The detached mass would undoubtedly have more than its share of the lighter gases, and hence the idea is of some value ... (Letter, See to Young (6 April, 1892), Dartmouth)

See's theory involved the origin of binary systems by fission, and their subsequent evolution by tidal distortion, which included a consideration of the effect upon the eccentricities of the resulting orbits by tidal forces. Huggins' idea here, if accurately reported by See, was never seen in published form. It could very well account for anomalous binary systems as mentioned by Clerke, though it also brought up the question of differing of primordial composition, which, as we shall soon see, was dismissed as a possibility.

Even though See felt that his work on double star systems had been met with approval by many (he mentioned Turner, Maunder, Clerke, Huggins, "... and several other English astronomers ...") he added:

> Dr. Huggins finds it difficult to see how the <u>red</u> stars in double star systems can be younger (in their stage of development) than their blue companions. He and Prof. Sir G.G. Stokes quite agree that the blue stars must be the younger ... (Ibid)

See had been very keen on the alleged former redness of Sirius, as interpreted from tables of stars in antiquity. This controversial idea had helped to convince him of his views.

One final note of interest from See was that, within their general correspondence, See noted to Young that only spectroscopists in England seemed tied to the "Blue to Red" progression of evolution, while most others were, at worst, ambivalent. It is quite clear from Huggins' remarks to Hale about this time that he did not take See seriously (cf. Huggins to Hale (27 May, 1892) Hale Papers). Huggins vastly underrated See's almost mystical zeal.

196. Huggins, <u>Op. cit</u>., ref. 187, p.76.

197.

At this point in our discussion, we might pause for speculation. Huggins was of the belief that his general sequence from blue to red stars was actually a heating process. But why did Huggins go to the trouble of attempting to reconcile the apparent disparity of colour and temperature in stars when he could have easily allowed his sequence to behave in the more conventional cooling fashion? The nebulae were no problem, since they could be hot or cool. In fact, they could still have been cool, and could very easily have represented the perfect gas condition, which could have continued until the white star stage, or even until the solar stage. Huggins decided eventually that the solar stars were at a maximum temperature, after which cooling would actually set in. But the question remains, could he not have discarded his rather cumbersome rationale for the heating sequence (and for masking effects) by allowing the maximum temperature to occur at the white star stage, as Ritter had done, and as Russell was to do? This question must remain open for the time being. To answer it, or at least to attempt an answer, we must consider Huggins' great fear of supporting Lockyer in any way, and combine that with his fascination with the application of physical theory to his evolutionary schemes.

C.A. Young, <u>General Astronomy</u> (Ginn, 1898), p.245.
 C.A. Young, <u>General Astronomy</u> (Rev), (Ginn, 1904), p.516.

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- 200. <u>Op. cit.</u>, ref. 195. See related this as Huggins' idea in his letter to Young (6 April, 1892).
- 201. William and Lady Huggins, Astrophysical Journal 6 (1897), p.326.

202. Letter, Huggins to Young (20 August, 1897), Dartmouth.

- 203. Hale, Astrophysical Journal 11 (1900), p.291.
- 204. <u>Ibid.</u>, p.295.
- 205. <u>Ibid.</u>, p.296.
- 206. <u>Ibid.</u>, p.297.
- 207. <u>Ibid.</u>, p.294.
- 208. Letter, Huggins to Hale (17 December, 1900), Hale Papers.
- 209. W. Wien, Phil. Mag. S.5 43 (March, 1897), p.216.
- 210. A. Clerke, Problems in Astrophysics (Black, 1903), p.66.
- 211. <u>Ibid.</u>, p.67.
- H.C. Vogel and J. Wilsing, "Undersuchungen über die spectra von
  528 Sternen", <u>Pub. des Astrophys. Obs. zu Potsdam</u>. ≠ 39 <u>12</u>
  (Potsdam, 1899).
- 213. H.C. Vogel, Astrophysical Journal 2 (1895), p.333.
- 214. <u>Ibid.</u>, p.340.
- 215. <u>Ibid.</u>, pp. 340-341.
- 216. Ibid.
- 217. Ibid., p.343.
- 218. <u>Ibid.</u>, p.346.
- 219. Curtiss, Op. cit., ref. 9, p.18.
- 220. Frost (1898), p.318.
- 221. Potsdam Publications # 26.
- 222. N.C. Duner, <u>Vierteljahrschrift der Astronomischen Ges</u>. <u>32</u> p.165 (1897).
- 223. Duner, <u>Popular Astronomy 6</u> (1898), p.85 ff, translated by J.A. Parkhurst.

- 224. <u>Ibid.</u>, p.85.
- 225. Ibid., p.86.
- 226. <u>Ibid</u>.
- 227. <u>Ibid</u>.
- 228. <u>Ibid.</u>, p.87.
- 229. <u>Ibid.</u>, p.88.
- 230. Frost's commentary read:

"(This system of classification /Vogel's7 has necessarily been followed by the translator, but it is proper to state that many of the leading spectroscopists are of the opinion that the time has not yet come for an attempt at a classification along the lines of stellar development, and that any classification must for the present be regarded simply as provisional)" (Frost, p.238 of <u>Astronomical Spectroscopy</u>, 1894), quoted in Keeler, <u>Astronomy & Astrophysics</u> 13 (1894), p.689).

- 231. Keeler, Astronomy & Astrophysics 13 (1894), p.689.
- 232. Keeler, Ibid.
- 233. George Ellery Hale, Astrophysical Journal 10 (1899), p.88.
- 234. G.E. Hale and F. Ellerman, <u>Astrophysical Journal 8</u> (1898), p.237.
- 235. <u>Ibid</u>.
- 236. Letter, Huggins to Hale (22 June, 1898), Hale Papers.
- 237. Letter, Huggins to Hale (30 October, 1898), Hale Papers.
- 238. Hale, Ap.J. 9 (1899), p.271 (Ap.J. = Astrophysical Journal).
- 239. Hale, <u>Ap.J.</u> 9 (1899), p.273.
- 240. Ibid., p.274.
- 241. Frost (1898), p.315.
- 242. Cf. Hale, <u>Publications of the Yerkes Observatory II</u> (1903), "Preface".
- 243. G.E. Hale, F. Ellerman, J.A. Parkhurst, "The Spectra of Secchi's

Fourth Type", <u>Publications of the Yerkes Observatory II</u> (1904), p.253.

- 244. Ibid.
- 245. <u>Ibid.</u>, p.265.
- 246. Ibid. Lockyer's paper appeared in Proc. R.S. 66 (1900), p.137.
- 247. Ibid., Lockyer.
- 248. Hale, <u>Op. cit.</u>, ref. 243, p.265.
- 249. A. Fowler, Proc. R.S. 73 (1904), p.219.
- 250. W.W. Campbell, Astronomy and Astrophysics 13 (1894), p.448.
- 251. Hale, <u>Op. cit.</u>, ref. 243, p.384.
- 252. Hale, <u>Op. cit.</u>, ref. 243, p.379.
- 253. <u>Ibid.</u>, p.380.
- 254. Lockyer, <u>Proc. R.S. 74</u> (1905), p.53. In this paper, Lockyer's position was that sunspots were cooler than the surrounding solar surface. This most certainly was a change of opinion from his earlier view.
- 255. W.S. Adams, <u>Ap.J.</u> <u>24</u> (1906), p.70.
- 256. Ibid., pp. 76-77.
- 257. Letter, F. Baxandall to Hale (31 March, 1907), Hale Papers.
- 258. Hale, W.S. Adams, H. Gale, Ap.J. 24 (1906), p.205.
- 259. Hale, Op. cit., ref. 243, p.378.
- 260. Ibid., p.379, footnote 53.
- 261. Letter, Hale to Huggins (15 June, 1903), Hale Papers.
- 262. At the time, Hale's spectroheliograph studies were convincing him of dissociation of calcium in the Sun. Huggins, of course, was aghast. Hale had attempted to make the announcement as quietly as possible to Huggins:

... I must confess that the results of the recent work with the spectroheliograph seem to me to point toward the view that calcium is dissociated in the Sun. You of course
know that almost every other notion of
Lockyer's is contrary to my views, and even
in this case I cannot say that I think that
there is considerable evidence which favours
the view that the elements under certain
conditions can be dissociated.
 (Letter, Hale to Huggins (29 October, 1903) Hale Papers)

Hale added that continued work on the solar disk, comparing H and K and 4227 would have some bearing on the problem. Huggins responded quickly in two long letters in November, 1903, driving a wedge into the crack left open by Hale:

Without in any way wishing to dictate to you, the conclusion to which you seem disposed to come - the dissociation of Ca - is so serious as one that I should like to ask you to consider a few suggestions which I will put down in the rough, just as they occur to me. (Huggins to Hale (9 November, 1903) Hale Papers)

Huggins expressed a positivist's attitude feeling that "... What you observe are not the substances themselves directly but their spectral lines ..." He went on to rediscuss his conclusions that density was the chief variable (see: Proc. R.S. 61 (1897), p.433), and then added a warning:

> I will not say more now. If you once accept dissociation, in any true sense, as apart from the well known different arrangement of the parts of the molecule, you would find it very difficult afterwards to withdraw ...

If Huggins had made such a suggestion to anyone other than Hale, say to a personality like Campbell, the last sentence would have been considered as a thinly veiled threat, but Hale took it quite easily, and, in fact, indicated that his mind was still very much open on the subject, since some of his earlier laboratory work had not been reproduced.

In following years, however, Hale directed the work of A.S. King to the further examination of the relative behaviour

of the H and K lines and the 4227 line in his electric furnace studies (see: Contr. Mt. W. Solar Obs. # 32, # 35, # 38). Ironically, King was eventually to conclude, along with W.S. Adams and H.G. Gale, that sunspot spectra indicated both a reduced temperature and an increased density (see: Struve and Zebergs, Astronomy of the 20th Century, p.126). Closely associated with this result was Fowler's observation in 1906 of Titanium or TiO in sunspot spectra, an identification made almost simultaneously with Hale's staff. We have already mentioned the importance of Fowler's 1904 observation of Ti0 in Antarian stars. The low temperature identification made also by him for sunspot spectra in 1906 was quite possibly the final blow to Lockyer's original concept of dissociation in spots during sunspot maxima. This contended that many of the 'most widened lines' of known identification during sunspot minima changed to unknowns during maxima. Lockyer originally interpreted this as arising from a higher temperature in sunspots during maxima, and it formed a part of his view of general atmospheric circulation in the Sun, which included spots as regions of infalling, condensing material.

263. H. Kayser, Ap.J. 14 (1901), p.313.

264. Ibid., pp. 315-316.

265. <u>Ibid.</u> The reference to Campbell capped a difference of opinion that the two had held for several years concerning nebular reversals and mixed spectra.

266. D. Menzel, <u>Selected Papers on the Transfer of Radiation</u> (Dover, 1966), p.iii.

267. Letter, A. Schuster to Hale (14 August, 1903), Hale Papers.

268. A. Schuster, <u>Ap.J.</u> 21 (1905), p.17.

269.	<u>Ibid.</u> , p.19.
270.	<u>Ibid.</u> , p.20.
271.	Ibid.
272.	An earlier discussion has been noted by R.A. Sampson, Mem.
	<u>R.A.S. 51</u> (1894), p.123.
273.	A. Schuster, "The Evolution of Solar Stars", Ap.J. 17 (1903),
	p.166.
274.	<u>Ibid.</u> , p.169.
275.	<u>Ibid.</u> , pp. 180-181.
276.	Ibid.
277.	Ibid.
278.	Ibid., p.187.
279.	<u>Ibid.</u> , p.188.
280.	<u>Ibid.</u> , p.189.
281.	<u>Ibid.</u> , p.197.
282.	Ibid., p.199.
283.	Ibid.
284.	Ibid., p.200.
285.	<u>Ibid.</u> , pp. 199-200.
<b>2</b> 86.	Hale, <u>Op. cit.</u> , ref. 243, p.383.
287.	<u>Ibid.</u> , p.384.
288.	<u>Ibid.</u> , p.385.
289.	In 1906, Fowler and Hale corresponded frequently, primarily on
	matters involving the Committee on sunspot spectra. By this
	time, Lockyer was more or less politically boycotted by the
	committee (see: letters Hale to Lockyer 2 August, 1904

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(Leicester); Schuster to C.A. Young 6 October, 1905 (Dartmouth).) This situation, plus the removal by Lockyer of his equipment from South Kensington (a decision based upon

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unreasonable land-use demands by the Royal College) caused Fowler to lose access to observing equipment, which was a serious blow to his work. (Cf. Meadows, <u>Op. cit.</u>, ref. 66, p.282; p.291).

Hale's and Fowler's correspondence during this period reflects both their shared excitement over their mutual discoveries, and, from this, considerable mutual respect. Hale, in various ways, attempted to lift Fowler's spirits (intentionally or unintentionally) by noting that experimental work - about the only course left open to Fowler - was of extreme and immediate value. By October, 1906, Fowler had told Hale that the Royal College of Science had provided him with new laboratory facilities in a new science building. (Letter, Fowler to Hale 19 October, 1906, Hale Papers). In this letter, Fowler acknowledged a preprint of Hale's paper on sunspots, and commented:

> It is very satisfactory to find that our respective efforts to find an explanation of the characteristics of spot spectra point so decidedly in the direction of reduced temperature ... (Ibid)

Fowler indicated that his own paper had been delayed, but that Hale's more detailed spectra "... has already enabled you to go into far greater detail than I would attempt ..." Fowler still wished to publish his own work, as a confirmation of Hale's, since he was able to "... obtain some of the experimental results in another way ..."

At this time, Fowler was also re-examining Lockyer's stratified solar atmosphere, which ran counter to the generally accepted reversing layer. He described his work to Hale, and, apparently, was in full accord with the reversing layer concept: if other assistants are to be believed, Fowler had never really agreed with Lockyer (see: Letters, Shackelton to Young, 20 December, 1896; 30 November, 1896; 5/6 November, 1896 - Dartmouth; and William E. Rolston to Hale, 30 January, 14 March, 1908 - Hale Papers).

Both Hale and Fowler agreed, however, that more work was needed on the study of the structure of the reversing layer, and Hale indicated that such a project was being planned at Mount Wilson, though he noted: "Spectroscopic investigations, however, <u>/in contrast to his own use of the spectroheliograph</u> are more important from the present point of view, and your work on these lines will be specially interesting and valuable ..." (Hale to Fowler, 12 December, 1906, Hale Papers).

During 1907 and 1908, Fowler worked along lines very close to those of A.S. King, on Hale's staff. In 1907, Hale sent Fowler his new map of sunspot spectra, to be used as a world-wide standard for comparison, and Fowler expressed considerable delight with it, as it clearly verified many of his identifications. He noted to Hale that the map might cause lockyer to "... reconsider his position ..." regarding the line identifications (Letter, Fowler to Hale, 17 April, 1907, Hale Papers). Later in the year, Fowler had made such rapid progress on the molecular spectrum of MgH<sub>2</sub> that Hale told his own staff to stop work on it.

Fowler was also one of the first to hear from Hale in July, 1908, when the latter had recognised the Zeeman pattern in sunspot spectra - the splitting previously believed to be due to line reversals (Letter Hale to Fowler, 11 July, 1908, Hale Papers). This turn away from line reversals was not discussed in print possibly because it led Fowler, among others, to rethink the evidence thus far gained promoting reduced temperatures in sunspots:

> ... You have certainly made a tremendous advance in recognizing these vortices, and perhaps especially in tracing Mitchell's reversals to Zeeman effects. It seems very obvious now, but I confess that the 'reversals' of selected lines had previously puzzled me a great deal. Widening & 'reversal' you seem now to have accounted for, and I wonder if you still think reduction of temperature to account for selective strengthening of lines. (Letter, Fowler to Hale (4 August, 1908) Hale Papers)

During 1908 and 1909 little correspondence appears to exist continuing this conversation. During this period, Fowler corresponded chiefly with Adams on the structure of the chromosphere.

Hale's own opinion, expressed in correspondence to Frost after Fowler's query, was that, at last, the widening and reversals had been explained:

> Speaking generally, I think it may be said that the widening and doubling of lines in spot spectra is due mainly, if not exclusively, to the Zeeman effect. (Letter, Hale to Frost (29 August, 1908) Yerkes)

In 1908, the question of relative temperatures from sunspot studies might have been momentarily confused by Hale's discovery, but the chief criteria from the metal lines, and bands were retained to support the lowered temperature conditions. In the latter part of the year, however, an argument arose at a meeting of the British Association in Dublin about the cause of the changes seen in sunspot spectra. E.T. Whittaker argued that increased pressure could just as well play a part in retaining undissociated molecules as could reduced temperature, and cited a number of arguments from Huggins' work:

The conclusion to which the whole evidence seems to point is that the stratum which gives rise to absorption, selective and general, of the spotspectrum has a greater density and pressure than the stratum which gives rise to the ordinary Fraunhofer spectrum ... (Observatory 31, October, 1908, p.374)

In answer to these remarks by Whittaker, which were made in September, J. Evershed from the Solar Physics Observatory in Kodaikanal, India, argued that Hale's studies confirmed the temperature agent, and that pressure studies by Humphreys had failed to indicate significant pressure differences between the spots and the general field.

The primacy of temperature was accepted by most workers at the time. It is important to note, however, that continued work at Mount Wilson, mainly by A.S. King in the laboratory and W.S. Adams at the telescope, began to show by 1910 that significant pressure differences were present in the spectra of Sirius, Procyon, and Arcturus. /Ap.J. 33 (1911), pp. 7-87

Adams' detection of pressure effects, possible only with the very high dispersions available at Mt. Wilson (2.4  $\stackrel{o}{A/mm}$  at 5000), form an interesting prelude to his later work with Kapteyn and Kohlschutter on spectroscopic parallaxes, where both luminosity and pressure effects were interpreted via line spectra.

290. Letter, Hale to Huggins (9 May, 1908), Hale Papers.

291. Observatory <u>31</u> (May, 1908), p.221.

292. Hale, The Study of Stellar Evolution (Chicago, 1908), p.ix.

293. Letter, Kapteyn to Frost (31 October, 1908), Yerkes. Written from Mount Wilson.

294.	Hale, <u>Op. cit.</u> , ref. 292, pp. 3-4.
295.	<u>Ibid.</u> , pp. 121-122.
296.	<u>Ibid.</u> , p.164.
297.	<u>Ibid.</u> , p.174.
298.	<u>Ibid.</u> , p.182.
<b>29</b> 9.	<u>Ibid.</u> , pp. 175-176.
300.	<u>Ibid.</u> , p.202.
301.	Hale, <u>Op. cit.</u> , ref. 292, p.208.

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### CHAPTER 3

# Studies of the Spatial Distribution of Spectra

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#### CHAPTER 3

## Studies of the Spatial Distribution of Spectra

#### Introduction

In Chapters 1 and 2, we examined the development of ideas about the direction of evolution, and the spectral classification schemes which arose more or less in dependence on evolutionary thinking. By the 1890s, as spectra became plentiful, and as small sets of proper motion and parallax data began to appear, attempts at studying the spatial distribution of spectra were made.

We mentioned briefly in our sections on Lockyer and Hale that distribution studies were quickly linked to evolution. It was not rare to see in them indications of the evolution of the entire sidereal system, an idea still strong from the days of William Herschel.

In this chapter, we will examine the major statistical studies during the 1890s and the first decade of the 20th century that entered into discussions of classification and evolution.

#### The Spectroscopic Studies of Frank McClean

Frank McClean turned to stellar spectroscopy late in life in the 1890s with the construction, at his own expense, of an objective prism astrograph by Grubb, which was completed in May, 1895,<sup>1</sup> and set up in Kent. Within one year, he was able to report on an initial project: the spectroscopic examination of northern stars to the third magnitude. McClean indicated that, of the spectra already examined, evidence existed that would allow him to subdivide Secchi's types into several transition stages, though no elaboration was made.

By April, 1897, McClean had completed his northern survey, which included 160 stars, and was able to discuss his modifications to Secchi's types, and to give their spatial distribution. His overall plan was to photograph the spectra of all stars brighter than magnitude  $3\frac{1}{2}$ . His northern survey accounted for five-eighths of this task.<sup>2</sup> McClean found that stars of Secchi's Type I could be divided into three divisions, while Types II, III and IV remained, though renamed Divisions IV, V and VI.

McClean considered it important to discuss the order of stellar development first, for this discussion precedes his outline of the classification, itself. Three arguments for the helium stars, as the first stages of development, were given: (i) Correspondence of dark-line spectra with bright-line spectra of nebulae, as shown by Campbell and others; (ii) Helium-type spectra found common in stars associated with nebulosity; (iii) Helium-type stars have the same spatial distribution as gaseous nebulae. These three criteria were not unique to McClean, of course. He depended upon others for support for the first two, but his own research aided the third.

McClean's Division I, which included Orion stars with helium spectra, was broken into two sub-divisions: the first, for spectra with unknown additional lines, and the second, with known lines seen in addition to helium and hydrogen. This second subdivision then led to his Division II: and, from there, the progression through Division VI closely followed Secchi.

This last division, though defined by McClean, had not been observed on his project, since they were all fainter than his magnitude limit. After this presentation of his scheme, and a review of the distribution of his Divisions within various regions as compared with the distributions of planetary and extended nebulae, he said:

> We gather from this table that as the stellar types of spectra become more advanced they are found to be more evenly distributed

in space. The idea is suggested that stars of the solar type - Division IV - started their career as helium stars of Division I, before the condensation of the galaxy.<sup>3</sup>

This statement is remarkable, but, at present, must be considered as pure speculation with a clear Herschelian flavour. Today, we certainly do recognise stellar populations and their distribution in terms of the evolution of galactic structure: but, at the time, no solid data existed that would have allowed McClean to support his concept, though his evolutionary order for stars was certainly not unique.

Division III stars (solar type) were of interest to McClean, for he detected an asymmetric distribution with respect to the galactic plane. Noting that relatively few solar-type stars were found adjacent to the South Galactic Pole McClean mused, "... the sun itself is situated near the lower boundary of the galaxy ...."<sup>4</sup>

Division II (Sirian) did not behave in any recognisably distributive way, and McClean left them for further study. His first Division, however, was almost completely confined to the galactic plane.

As to the question of influence, McClean left little doubt:

It has been throughout assumed that the successive types or divisions are merely the manifestations of the successive physical states, through which every star naturally passes in the course of its career.<sup>5</sup>

Some of Lockyer's ideas can be seen in McClean's discussion and subdivisions of Secchi's Type I stars. Frank McClean and Lockyer were friendly,<sup>6</sup> and, after McClean's death, his sons became close companions of Lockyer and Lockyer's son, W.J.S. Lockyer. One of McClean's sons, William, became involved with the development of Lockyer's Hill Observatory.

Frank McClean's work was reprinted with a fuller discussion<sup>7</sup>

in the following year. Though no great change occurred, his discussion of specific spectra and other remarks bear mention. First, he used both physical and "chemical" (spectroscopic) arguments to support the association of his Ia Division with nebulae. Campbell's work was mentioned in the spectroscopic realm, without any comment on Keeler's, Huggins' or Lockyer's earlier work.

For physical evidence of association, McClean drew upon Isaac Roberts' photographs of the Pleiades and the Orion Nebula, E.E. Barnard's study of a large nebulosity near Antares (within which various Division Ia stars were found by McClean), and Barnard's study of an extended nebula around the Ia star  $\xi$  Persei. This last star also showed the same "hydrogen" series identified by Pickering in Zeta Puppis.

McClean felt that Secchi's original scheme had evolutionary overtones. In discussing his modifications to Secchi's types, he added:

> It must also be remembered that SECCHI fully recognised the special character of the spectra of the Orion stars.<sup>8</sup>

This apparently agrees with what we discussed in the early part of Chapter 2.

At the end of McClean's memoir were several tables comparing his classifications with those of Lockyer, Pickering, Secchi and Vogel. McClean separated his comparative list into 5 tables by galactic position; we here provide a synthesis (see Table 1, Chapter 2).

This comparison demonstrates that McClean's system best fit Pickering's 1890 scheme, and, interestingly, included only those Draper classifications that persisted, with but few exceptions. Comparison with Secchi's work was too crude to be significant; Vogel's was a bit better, owing to the greater resolution of his scheme. Lockyer's was heavily mixed, though his temperature criteria obviously fit better than ascending or descending, since McClean did not consider the latter. Maunder considered McClean's classification system to be one of the clearest and most reasonable modifications of Secchi's outdated and inadequate scheme, but lamented:

> It has, of course, long been felt that Secchi's classification was inadequate for the material at present at our disposal. The pity is that so many and such different methods have been adopted for extending it, so that a real danger lies before us that with so many different schemes of classification we may be landed in utter confusion.<sup>9</sup>

Maunder, as we have seen, had argued persuasively in 1890-1891 that spectral differences arose from composition differences. By this later date, however, he apparently changed his opinion somewhat, for he conceded that helium stars represented the earliest stellar stages of development from gaseous nebulae, based upon McClean's arguments. Maunder considered McClean's third argument for this view, based upon the spatial distribution of spectra,<sup>10</sup> to be extremely persuasive.

Frost felt that McClean's discussion of spatial distribution was necessary, but that it would have been more convenient if the arrangement of the spectra in McClean's published lists were on the basis of his system, rather than determined by location in space. No mention was made of McClean's discussion of the evolutionary significance of his work, nor of the interpretations he derived from the spatial distributions of stellar types. Frost felt that McClean's spectra were "... pictorial and qualitative rather than metrical and quantitative ...."<sup>11</sup> since the collection lacked exposure information. He did regard the collection as valuable, "... being the only memoir to which one may turn for a photograph of the spectrum of each of the brighter stars ..."

McClean's statistical studies came at a time when barely enough spectroscopic material had been collected to begin such work. We will now review other statistical discussions that were attempted during this time - studies not only of the distribution of stars in space, but comparisons of spectra with position, proper motions, and magnitude.

#### W.H.S. Monck

W.H.S. Monck, known only as residing in Dublin, a friend of R.S. Ball and J.E. Gore, and a frequent critic and contributor to astronomical literature during the eighties and nineties, provided mainly speculative, but well reasoned, opinions on many subjects. His most important work was on the correlation of proper motion with spectral class, begun in the early nineties, with the availability of the Draper Catalogue.

Monck's first statistical discussion appeared in 1889,<sup>12</sup> but it was not until 1892 that his first comparison, between the Draper Catalogue (1890) and M. Bossert's proper motion catalogue, produced any conclusions. His 1892 work yielded a mere 109 stars with both types of data. Thus Monck concluded:

> It is perhaps too soon to draw results from a table so incomplete as the foregoing. 13 It, however, suggests the following conclusions ...

He concluded that the Sun must be a member of a cluster of nearby stars of similar spectra. This cluster contains binaries, and the cluster drifts with respect to stars not in the cluster, thus affecting the value of the solar motion. He later added a postscript to his paper which he believed to be of interest to workers at Lick:

> The second or solar class of stars are divided in the Draper catalogue and elsewhere into several subdivisions. There are two of these which I would propose to distinguish as Capellan and Arcturian from the principal stars belonging to them. The Capellan include E and F of the Draper catalogue, the Arcturian H,I,K,L, with perhaps G, but I am not sure of its

position. My conclusion is that the Capellan is the prevalent type among the nearer stars and that it is so in a very marked degree.<sup>14</sup>

Monck's work was noted by J.C. Kapteyn in a letter to E.S. Holden.<sup>15</sup> Kapteyn had already come to similar conclusions, having contributed a paper to the Amsterdam Academy of Sciences on 29 April, 1892,<sup>16</sup> and was about to send another paper on the same subject to the same institution. Kapteyn felt, however, that readers of the <u>PASP</u> should also be informed of his work, even though "... Mr. Monck seems to have noticed the connection between Spectrum and P.M., nearly at the same time with myself ....<sup>17</sup>

Kapteyn's work, also with Draper spectra, and positions from the Cape Durchmusterung (along with proper motions from the work of Auwers, Herz and Strobel), reduced at Groningen, yielded almost 2400 stars for the study, and, hence, was far more significant than Monck's work, though they both came to the same conclusion. We will discuss Kapteyn's work after Monck's, and that of other preliminary workers in the nineties.

Later in 1892, Monck again discussed his statistical investigation, now attempting to place the discordant proper motion systems of the various catalogues he used on one standard system. Thus he was able to increase his data base to 600 stars, with spectra, magnitudes, positions, and proper motions. After tabulating his results, and equating the Draper spectra A,B,F,G,H,I,K, and M to various ranges of standardised proper motion, he again found that stars with spectra between F and K were "... far in excess of those of the Sirians (spectra A and B) ....<sup>18</sup> This reconfirmation allowed him to state:

> The nearness of the Capellan stars which seems to be thus established is a nearness relatively to stars of the same magnitude but with different types of spectrum. Researches on binary stars seem to establish that this is not due to smaller average mass, and it would

therefore appear that these stars are of the dullest or least light-giving class - more so not only than the Arcturian stars but than those of the type of Antares or Betelgeux. The Sun as a Capellan star may therefore be expected to give a small amount of light relatively to its mass when compared with most of the fixed stars. The comparisons hitherto made point in this direction.<sup>19</sup>

To Monck, there were red stars far brighter than solar types; and stars of solar mass gave out comparatively little light, when compared to the majority of the brighter stars. Both these musings were prophetic: the first, an inkling of the existence of red giants, and the second, the nature of the relation between mass and luminosity.

Shortly after this second work on systematic differences, Monck increased his data base by carefully calibrating proper motions from the Pulkowa Catalogue with the <u>Fundamental Catalogue</u> of Auwers that he had been using. He came to very much the same conclusions regarding relative luminosities, and went so far as to suggest a tentative order for the Draper classes, in terms of luminosity B,A,M,K,I,H,G,E, and  $F^{20}$  which was a most interesting order, especially in the reversal of the A and B types - a characteristic he was to carry through the decade.

It should be noted in passing that Monck's friendship with J.E. Gore could have had an effect on their mutual interests. In 1894, Gore discussed the relative sizes and brightnesses of stars, based upon parallax data from Elkin at Yale. Progressing through the stellar types, Gore found that for the assumed masses of Sirian stars, they had extreme brilliancy. For the brighter stars in the second or solar class (Capella, Arcturus, Aldebaran, Pollux, Deneb ...), Capella was found to be about 250 times the Sum's brightness, and, with the same surface brightness, to be about 16 times the Sum's diameter.<sup>21</sup> For Arcturus, with similar arguments, the values come out 6000 times greater in brightness, and 79 times the Sum's diameter, again assuming similar surface

brightness and density. Hence, Gore's mass for Arcturus came out to be 500,000 times the mass of the Sun. He rightly added that these were "... figures well calculated to 'stagger the imagination'".<sup>22</sup> Similar, though less drastic values, were reached for the other stars of the solar class, again singled out for their brightness. Very few in number were comparable with the general population of solar stars of lesser magnitude. When solar class stars at a smaller distance from the Sun were studied, Gore added that "... the Sun will contrast very favourably in size, or at least in brightness ...", and that "... We may therefore conclude that while some of the brighter stars are probably vastly larger than our Sun, others are certainly much smaller ..."<sup>23</sup>

Without realising it, Gore had found the rudimentary difference between giants and dwarfs of the solar (crudely defined - actually late solar) class; for the examples he discussed were mainly of the same general class.

Gore must have had some influence on Monck's ideas at the time, for in late 1894, Monck made the following suggestion in "The Spectra and Colours of Stars":

> I suspect, moreover, that two distinct classes of stars are at present ranked as Capellan, one being dull and near us and the other bright and remote like the Sirians. Capella itself, perhaps, occupies an intermediate position,  $\alpha$  Centauri and Procyon may stand as types of the near and dull Capellan, with large proper motion, while Canopus is a remarkable instance of a bright and distant one, with small proper motion, assuming that there is no doubt as to its spectrum. /Today it is classed as an FO Ib, or an early type super-giant7. Peculiar spectra (designated Q) seen to occur occasionally with stars of all colours. They are very probably made up of two spectra superimposed on each other;  $\alpha$  Persei may perhaps be classed with Canopus.<sup>24</sup>

Most of the stars mentioned by Monck were also mentioned by Gore, including Canopus. While Nielsen<sup>25</sup> has pointed to this statement

as one which "... nearly took the step, that ten years later was taken by Hertzsprung in his discovery of dwarfs and giants ..." it should be noted that nowhere else in Monck's writings has such an explicit statement been found. It quite possibly was a passing comment, based upon discussions with Gore, which was later considered to be too weakly determined, and was therefore dropped.

By 1897, Monck was able to expand upon his earlier work. With better data supplied by Pickering, and additional proper motion catalogues (Rambaut's catalogue from Dunsink), he was able to come to the following conclusion: that even though Capellan, Arcturian, and Sirian stars all seemed to be of the same magnitudes:

> The fact appears to be - however it may be explained that Capellan stars have, on the average, larger proper-motion than Arcturians of the same magnitude, and that Arcturians have, on the average, much larger proper motion than Sirians of the same magnitude.<sup>26</sup>

Monck wondered how this could come about. He rejected the possibility that Capellan stars had greater velocities than the others. Two tests were mentioned: parallaxes, to determine distances for these stars, and, hence, absolute motion; and radial velocities, to arrive at an independent measure of relative average motions. Parallaxes were scarce at the time and, in Monck's opinion, not to be relied upon, though they did seem to show that solar stars had larger parallaxes than Sirians of the same magnitude. Monck felt that Vogel's radial velocity work, though it included few Capellans, showed that all three classes had about the same average radial velocities. From this, and from the fact that the solar motion was sensibly the same as derived from groups of small proper motion stars as opposed to groups of large proper motions, he concluded that the Capellan stars are small or dull stars.<sup>27</sup> Monck was able to provide further support from binary studies, which indicated that for systems of Capellan and Sirian types with known

orbits, the latter came out about five times more luminous, on the average. But the Arcturian binaries gave different results: "... They appeared to occupy both extremes in the scale of luminosity ...,"28 This highly provocative idea was left, however, for Monck felt that the binary data for this class were far too meagre at the time. Further discussion brought Monck to the conclusion that Capellans were probably not the least massive stars, but were probably the least luminous, with respect to density "... for there are some reasons for thinking that a Sirian star, instead of presenting a much brighter surface than a Capellan of the same mass, presents a much greater extent of surface. The Capellan is rather denser than duller; but, mass for mass, it gives much less light ... "<sup>29</sup> This could have been taken from Gore's discussion we reviewed before, by reversing some of his logic. Monck's dependence upon equality of surface brightness was unfortunate, but was in common with most discussions at the time. Most certainly, his preoccupation with the less dense status of Sirians had evolutionary overtones, as he was quick to point out -

These results may not be inconsistent with a theory of stellar development, but if so, it must assume a different form from that which would naturally occur to us.<sup>30</sup>

In Monck's opinion, if Arcturian, Sirian, and Capellan stars represented different evolutionary stages, then the Capellans would be last, not second, if the course of evolution was one of condensation and cooling. This included the possibility that stars would also pass through class M, as very few M stars had been found at the time to have large proper motions. This was a serious limitation, which would not be circumvented until magnitude limitations on proper motion surveys had been relaxed. Thus, in Monck's system, stars would in some way pass through all the classes before reaching the solar, or Capellan class. Regarding the red stars of measured proper motions, Monck

noted that, of about 50 available from the Pulkova Catalogue, only four had proper motions in excess of 0".1 per year. This was an even smaller percentage than for the Arcturians. Monck concluded:

> Unless they are, on the average, larger stars than the Capellans - and we would not be likely to find the largest stars in the last stage of cooling - they are evidently more luminous, relative to their density, and must, therefore, represent an earlier stage of evolution.<sup>31</sup>

Continuing along these lines, Monck felt that the small proper motions of stars of the Orion type (B in the Harvard system), smaller than those for Sirians, rendered them even more luminous, and "... On the development theory, they represent the earliest stage, while the Capellans represent the latest ..."<sup>32</sup> Monck was not able to place either the red stars or the Arcturians, but he was able to support his general progression from binary stars. First, every spectroscopic binary of the Algol type was Sirian. These Algol systems with tidal coupling and very short period were believed to be at an early evolutional stage. When longer period binary systems were examined, their spectra indicated stars of Capellan type. Further, from the light curves of the Algols, the stars were found to be large compared with their mutual orbits, and, hence, were of low density. This argument foreshadowed Campbell's in 1910 and Russell's in 1899.

Monck's concluding remarks to his paper concerned the effect of greater luminosity upon the relative numbers of stars of each spectral class in the Draper lists. Basically, the relative numbers for each class could not be an indication of their true frequency distribution. He supported this by supposing that the Sirian and Capellans were equally numerous, but that the Sirians were visible over twice the distance (magnitude for magnitude). If this were true, Monck reasoned that we would expect to see eight times as many Sirians as

Capellans. But the actual proportion in the Draper Catalogue was only  $2\frac{1}{2}$ : 1. Thus the Capellans might be more numerous than Sirians. Monck's reasoning was correct, by today's standards, though the distribution was only a crude approximation. His conclusions from it, however, still hold true, as does his reasoning.

Frost, in Scheiner's book,<sup>33</sup> followed Monck's reasoning for the effect of luminosity differences upon the relative counts of stars of different classes. Scheiner, however, held to a different opinion; that the period of star creation was a continual one, progressing today, and that the rate of development was a direct function of the degree of condensation.

Monck's commentary on evolution, based upon relative luminosities and densities, was not repeated or elaborated upon by him in the future. It was certainly unique, though quite tentative.

In a supplementary note to his paper, Monck found that as he progressed from his early to late types, the percentage of stars with proper motions greater than 0".1 increased, from zero for B to 40.7% for G types, in the progression B,A,H,M,I,K,F,E,G.<sup>34</sup> Again, Monck had to reverse the order of A and B. His continued use of many of the outdated Harvard letters implies that he did not quickly gain access to the <u>Harvard Annals</u>: this paper was written just before he became aware of Pickering's and Fleming's modifications. Since Fleming's 1897 discussion did not reverse the A and B classes, one wonders if Monck's reversal was ever noticed.

In the next year, Monck again discussed his distribution studies, and also wrote a popular text. In his paper,<sup>35</sup> he restated many of his conclusions and offered several modifications, based upon the availability of more observational material gathered in the five years since he began his work. Pickering's alteration of the Draper system into fewer letter designations was considered, along with his tentative inversion of the A and B types. For kinematic and evolutionary reasons, the association of B type stars with nebulae and the plane of the Milky Way was reason enough for Fleming, Maury, and Cannon to place these stars before the A types. Even if Monck's prior reversal of their order was an influence upon Pickering, Monck himself also felt that the B types were closer to the nebular stage. He seems to have made this observation, in print at least, well before he provided his listing of the Draper types in order of brilliancy.

Pickering's contraction of the Draper system in 1897 caused Monck to rethink the position of the G types stars, which he had originally left out of his Sirian, Capellan, and Arcturian classes, since it was not clear to which the G types should belong. He now felt that the G types were most definitely of the Capellan class, since the Draper type E had become G, upon closer examination.

Monck continued to believe in the relative number densities for the Capellans and Sirians that he had arrived at in the previous year. In addition, he now felt that the Arcturians were the most numerous, the Capellans next, and the Sirians the least. From Bossert's proper motions he was able to examine 225 Sirians, 461 Capellans, and 366 Arcturians, and concluded:

> The Capellan stars with large proper motion thus exceed the Arcturian in the proportion of fully five to four, while they are probably less numerous on the whole. They exceed the Sirians in the proportion of more than two to one, while they are less than half as numerous on the whole.<sup>36</sup>

Monck concluded correctly as to relative populations, but failed to see the reason for the fewer large proper motion Arcturians observed - the mixing of giants of high luminosity and the extremely low luminosities of the red dwarfs. These latter were below the 8th

magnitude limit of the Draper catalogue, and certainly beyond the effective reconnaissance of any proper motion surveys done at the time, before the introduction of the 'blink' comparator in 1902.<sup>37</sup>

Monck's new arrangement "... in order of brilliancy (and consequently of average distance for stars of the same magnitude) ..."<sup>38</sup> was modified now to read: B,A,K,M,F,G, which lowered the relative luminosity of the M stars. The evolutionary significance of this new progression was then considered:

> If this order is correct, it seems clear that no continuous gradation of spectra can be traced through it. Arcturian stars are not Capellans which have cooled down to a lower state. If they were so, then proper motions would, on the average, be greater, not less than those of uncooled Capellans. Cooling would reduce the light of the star without affecting its proper motion. Consequently the cooled star would, on the average, have the greater proper motion, the magnitudes (i.e. quantity of light) being supposed equal. Are the Capellans cooled-down Sirians? The difference in the amount of their average proper motions is startling.<sup>39</sup>

Monck, however, felt that the difference was real, since he had found that intermediate types had intermediate proper motions. Though Monck still expressed some doubt as to his conclusions, he felt that binary star studies supported "... my conclusion that the Arcturian stars are not cooled-down Capellans and that on the supposition that these two types represent different stages of star-life, the Capellan, not the Arcturian stars must represent the later stage ...."<sup>40</sup>

Monck allowed himself greater space for interpretation of his work in his book <u>Stellar Astronomy</u> published in 1898. This popular work ranged over all aspects of stars, including the structure of the sidereal system and stellar evolution. In comparing his system with the Draper system, we find even greater resolution than before: Orion type-B; Sirians-A; Capellan-F; Arcturian-K and Antarian-M.<sup>41</sup> The text of his book offered much insight into Monck's ability to sense the direction of astronomical work. He discussed the possibility of two star streams and the existence of absorption in space as a possible systematic error in his work,<sup>42</sup> in a manner to be later studied in a more thorough way by Kapteyn. He seems to have been open at various points to the possibility that cooling and contraction were not necessarily the course of evolution, and that there might not be a single evolutionary line.

Alternatives for the former included differences in chemical composition, <sup>43</sup> and for the latter - the possibility that the two greatly different types of nebulae in space (Orion and Andromeda as examples) produced two different primary classes of stars - Orionic and Sirian, on the one hand, and Arcturian and Antarian, on the other.

#### Scheiner's Remarks on the Statistics of Stellar Spectra

Even though a far larger sample of spectral types was available from Harvard, Scheiner still preferred Vogel's work and apparently declined "... to accept as conclusive any inferences which may be based upon ..." the American work.<sup>44</sup> Vogel's sample of 3702 stars yielded 58.4% for Ia; 33.5% for IIa; 7.8% for IIIa; 0.3% for IIIb - not significantly different than that found from the larger Harvard sample.<sup>45</sup> The wide variation in the relative frequencies of the various types caused Scheiner to look for explanations in terms of evolutionary effects:

> Let us now, even at the risk of briefly indulging in speculation, examine the reasons for the great difference in the numbers of the spectra of the different types.

Assuming the order of stellar development asserted by Vogel's classification to be correct, we may more exactly state the question in this way: "Why does the number of stars (in a given stage) constantly decrease as their condensation and cooling progresses?"<sup>46</sup>

Scheiner first considered the possibility that it might be an evolutionary effect. Assuming that all stars began their development at the same time, the rate of cooling of each star would be a function of mass. While he did not state what type of function was to be expected, he felt that the range of mass found amongst stars should be "... distributed according to chance ..." causing stars of intermediate mass to predominate. These were believed to be solar type stars; and this did not fit the observed distribution, with a preponderance of white stars. Thus Scheiner rejected the condition of a common starting date for all stars, and considered the alternative - that stellar birth is a continuing event, occurring both in the past, and in the present epoch. Furthermore, the period of genesis was considered to be comparable with the period of stellar development. "... During this period - and there is no good reason to show that we may not be still in it - all intermediate stages between birth and decay will occur, and the relative age of the stars will be fortuitously distributed. A11 the spectral types would then occur with equal frequency, provided that the time spent in passing through each spectra class was equal .... "48

The provision of equal time spent in each spectral class was immediately rejected, for Scheiner felt that the duration of that condition when the star was most capable of condensation (i.e., when it closely resembled a perfect gas) would be a period when the greatest amount of heat could be generated through condensation (closest to the amount of heat lost by radiation) and thus prolong the heated condition of the star. This stage was believed to be the initial state, when the stars were least condensed, and were in Class I. The rate of cooling would increase with condensation. Hence, Class II would be at an intermediate rate, and Class III at the greatest rate:

On this hypothesis we can thus easily explain the disproportion of the different spectral classes, and possibly we might conversely reach some conclusion as to the relative length of time passed by the star in the different classes.

One would expect from Scheiner's reasoning that a star would spend longer intervals in Class I, intermediate intervals in Class II, and the least amount of time in Class III. This apparently is not what he derives: "... On this basis it would appear that a star remains something like five times as long in the condition of Class III as in Class II, and twice as long in Class I as in Class II ..."<sup>49</sup> This does not seem to follow, either from Scheiner's reasoning, or from the observed distribution of spectra, and requires further study. It might be a typographical error.

#### The Statistical Work of Kapteyn and Campbell

In the early 1890s, Kapteyn's primary interest was to see through the measurements and reductions of the great southern photographic survey by Gill. Their association began in 1885, as Kapteyn suggested collaborative work in the compilation of positional data for stars in the southern hemisphere.<sup>50</sup> Ten years later, Kapteyn's staff at Groningen had finally finished the tremendous task, and this allowed Kapteyn to prepare his own introduction to the catalogue, which appeared just after Gill's. Within his introduction, little statistical material appeared, though it was mentioned that such discussions would soon be forthcoming. The only specific observation made by Kapteyn at the time was that stars within the Milky Way plane tended to be bluer than those outside of the plane. At the time, Pickering, McClean and Monck suggested that blue stars were concentrated in the galactic plane, but Kapteyn wished to consider the existence of interstellar absorption. This quest was to last for several decades, and produced many important discoveries about the structure of the galaxy - as in 1904 with his identification of two star streams which we today know as due to the rotation of the galaxy, and in the period 1910 to 1914 when, in suggesting work to W.S. Adams, the technique of spectroscopic parallaxes was discovered.

Kapteyn had apparently found the same relationships for proper motion and spectral type as Monck did in 1892-93, as we have seen from correspondence between Kapteyn and Holden. Unfortunately, these early papers by Kapteyn have not as yet been located, and the earliest discussion of such material available is from 1898, where Kapteyn confirmed Monck's earlier study with better data. <sup>52</sup> By 1901, Kapteyn had been able to derive the solar motion from proper motion data, and had created a statistical method for measuring the distances to groups of stars by means of their parallactic components of proper motion due to solar motion - the technique of secular parallaxes. In three more years, he announced his theory of star streaming. In an early discussion of his two streams, he acknowledged that Kobold had anticipated streaming in the 1890s, but it was not until Kapteyn separated out the effect of solar motion that a clear picture emerged. In 1905, Kapteyn did not apply the existence of streams to evolution, or evolution to the interpretation of his streams. But by 1910, this was to develop into a major theme. 54

During 1908, Kapteyn worked as a guest investigator at Mount Wilson, by courtesy of George Ellery Hale. This contact inevitably exposed him to Hale's preoccupation with stellar evolution.<sup>55</sup> An early letter (in 1907, prior to Kapteyn's extended visits) from Kapteyn to Hale, which dealt mainly with various aspects of his Plan of Selected Areas, and how Mount Wilson and other observatories might cooperate, indirectly discussed evolution in terms of cluster motion and stream

... the radial velocities may confirm the idea which seems natural enough, that the parallel motion of the members of the group is nothing else than a common motion formerly possessed by one of the star streams, the existence of which I have recently tried to demonstrate. Starting from this hypothesis I am able to predict both the absolute parallax of the group and the linear radial velocity. If direct determination of the radial velocity confirms this prediction, the hypothesis will have gained very much in probability and with it the predicted absolute parallax.

However this be, direct parallax determination will soon give us a fair idea of the distance and with that, of the luminosity of its members. The amount of the observed relative motion will, I believe, give us some notion about the order of the age of the group.

In short if we know the radial velocity of this group we will soon know more of these stars than of any other stars in the sky. At the same time we will have every reason to think that they have a common origin.<sup>56</sup>

Kapteyn's idea was, of course, quite similar to Lewis Boss' employment of proper motions of selected Hyades stars to derive the distance to the cluster by the "moving cluster", or convergent point method. This method was first mentioned in print by Boss in the following year, and was, in fact, applied to the Hyades, or "Taurus Cluster".<sup>57</sup>

The idea that the initial motions of these stars were originally uniform, and that their internal motions were a measure of age, was a persistent one in Kapteyn's thinking. Hale, of course, expressed considerable excitement over Kapteyn's comments and suggestions:

> Many of the data you require can be easily furnished by us and, on the other hand, many of the results obtained in the course of your investigations will be indispensible, if we are to draw any joint conclusions regarding stellar development. The importance from the standpoint of stellar evolution, of the common origin of this group is very great indeed, and we cannot learn too much about it. I may
therefore say at once that the radial velocities of all the stars you mention in this group will be measured here. Moreover, I think it highly desirable to take up other similar groups and study them as completely as possible in cooperation with you. 58

After his extended stay at Mount Wilson, Kapteyn returned to Groningen, and, in February 1909, wrote back to Hale about an idea that had developed en route home:

On board ship I have been thinking about a certain point which seemed to promise another line of attack on the evolution problem. I have been collecting what little materials are available for a very first test.<sup>59</sup>

Kapteyn began with generally known and accepted ideas that the helium stars were a very early stage in stellar life, and that their linear velocities were low compared with other classes of stars. Kapteyn had been in close contact with Frost<sup>60</sup> and, therefore, was aware of Frost's and Adams' work on the radial velocities of Orion type stars, which were shown by them in 1903 to have very small radial velocities.<sup>61</sup> Kapteyn wrote to Frost often, trying to convince him of the reality of interstellar absorption. Frost was sceptical; partly because the problem was confused with the question of optical dispersion in space, which Frost felt was a foolish idea.<sup>62</sup> Kapteyn reasoned that, if stars originated from nebulae, "... then we are led to think that these nebulae too must have very small linear velocities probably even smaller than those of the helium stars ...,"<sup>63</sup>

From Kapteyn's belief in the existence of an all-pervading interstellar medium, he could easily imagine that the large crosssectional area and low density of luminous nebulae would cause them to be most easily 'retarded' by the resisting interstellar medium. Thus, the helium stars, condensing out of luminous mebulae, "... being far denser, will be accelerated under the influence of the attraction of the system / the rest of the assumed locally asymmetric distribution of mass and for a long time (surely many millions of years) the velocity will increase".

Kapteyn then provided a table of velocities for ranges in spectral type, where the average velocity was the residual "... freed from the Sun's motion in space ..." This table was nearly identical to one published one year later by him.<sup>65</sup>

Kapteyn found a clear increase in velocity from the B range through Ma: the N's were equivalent to A type velocities, and the L stars had even lower velocities than those seen in the B range (though only two stars were found in that outdated classification). The planetary nebulae exhibited the highest velocities; while the Orion nebula was the lowest of the group. From this list, Kapteyn concluded:

> I feel strongly inclined to conclude, from this list alone, that planetary nebulae do <u>not</u> produce stars - Nothing however hinders us in admitting that such a nebula as the Orion nebula <u>may</u> produce stars ... The line of inquiry suggested is evident: In order to get a first answer to such questions as:

Do nebulous stars Do spiral nebulae	) )	represent very
Do white nebulae	)	of development?
Do irregular nebulae	)	

... We have to measure a certain number of radial velocities. Even a moderate number of measurements may be sufficient to answer the question in the negative. I for my part think that for the planetary nebulae the question is already pretty well answerable in this sense by the data we have - They probably stand rather at the end of the development, and this seems to be in good keeping with what we know of the change of the spectra of new stars into that of planetary nebulae.<sup>66</sup>

This observation by Kapteyn, noted independently in 1910 by Campbell, was an important element in Eddington's thinking concerning evolutionary order. Writing in 1914 in his study <u>Stellar Movements and</u> the Structure of the Universe, Eddington closely followed Kapteyn's

reasoning:

If we have entire confidence in the law that the speed increases with the stage of development, it follows that a planetary nebula must be regarded as a final stage - certainly not as the origin of a star. There is some justice in a remark of R.T.A. Innes (<u>The Observatory 36</u>, p.270): 'The fact that we have seen a star change into a nebula ought to outweigh every contrary speculation that stars originate from nebulae'.<sup>67</sup>

Eddington felt that Innes' exclusion of all nebulae was a

bit sweeping, and concluded:

It is necessary to proceed cautiously in such an application; but we seem to have within our grasp a new method of deciding doubtful questions as to the order of development of the different stages in a star's history.

The problem at the time, however, as Kapteyn pointed out in his letter in 1909, was that only one irregular nebula, Orion, had its radial velocity determined.

Campbell had studied the different velocities between Sirian (Type I) and Solar (Type II) stars in 1901,<sup>68</sup> and had found that Type II actually seemed to have a slightly lower average peculiar velocity, in contrast to Kapteyn's results. Kapteyn referred to Campbell's study in his letter to Hale, but felt it had little weight, since the types were determined only by a crude colour index (visual magnitude - photographic magnitude). But Kapteyn did admit that even in his own radial velocity study, based in part on Frost's and Adams' work, little difference between the two types was found. Actually, to f ind a significant difference of velocity with advancing spectral type, Kapteyn had to combine all classes from F to K5 to show a significant change. He had, however, as early as 1898,<sup>69</sup> derived proper motions for the two classes, and had found that the Sirians had slightly less values than solar type stars. He admitted, however, that his spectral samples were not pure, and that some helium stars, no doubt, were included in the Sirian class.

In his long letter to Hale in February, 1909, Kapteyn was able to associate classifications with velocity ranges from proper motions and radial velocities. He found that Type II stars had velocities "at least" 1.3 times that of Type I stars. Average radial velocities on the Harvard classification ranged from 7.0 Km/sec for B stars to 14.5 for F, G and K, and to 19.3 for Type III red stars, though the last sample included only five stars, which was "... too small for drawing a conclusion". Kapteyn also examined the velocity distribution by determining the percentage of each class that had velocities above a limit of 20 Km/sec. B stars had 0 per cent; whilst the value increased to forty per cent for G and K. M alone, from a sample of six stars, was 41 per cent, and planetary nebulae had 54 per cent. Type N stars had only 25 per cent. Kapteyn concluded that "This table seems to show that up to G, there is a gradual increase ... " clearly indicating that his sample for M stars was not significant. Concerning the anomalous N stars, he wondered:

> Might it not be possible, if we had a greater number of stars of the 4th type (N), to settle the question whether or not they represent a later stage than the 3rd or whether they belong to an earlier stage?

He also wondered about the Wolf-Rayet stars:

And in the same way might we not find whether the Wolf-Rayet stars represent an early or a late stage. Do they stand at all near the planetary nebulae, as Pickering thinks, or nearer the Helium stars.

And, in general:

... are the novae in their last stage really to be considered as ordinary planetary nebulae? What is the place of the (bright) line Helium stars etc.<sup>70</sup>

Kapteyn considered whether his work might be able to indicate something

## about the relative rates of evolution:

There also seems some possibility of getting hold on the question whether A, the very luminous (big) stars go through their different stages of evolution, quicker or slower than the little luminous stars (less massive bodies)? To me there seems in my numbers indication in the sense A, but this letter is already so long, that I will not trouble you now with this.

Kapteyn closed by saying that he was delaying his further work on the scattering of light in space for a while until Boss' new catalogue became available: "It has not yet arrived and I have a violent desire of getting to some, be it provisional result ..." This is certainly a most frustrating situation for someone who has to wait for the maturation of proper motion data!

In March, Hale replied to Kapteyn's long letter, feeling that the "... hypothesis proposed ... is extremely promising and I am delighted with it ... We must make serious attack on this hypothesis as soon as possible, and arrange our programme with reference to it".<sup>71</sup>

In the Spring of 1909, Hale travelled to Europe for a round of meetings and lectures, and met Kapteyn. There is therefore no discussion of how their mutual interests developed. In late September, 1909, V.M. Slipher of Lowell Observatory began to find evidence for interstellar absorption, observing a stationary K line in the spectrum of the binary Sigma Scorpii. He had written about it to Frost<sup>72</sup> and prepared his discussion for the <u>Astrophysical Journal</u>. Hale of course heard about this, and, in turn, Kapteyn, too. Thus in late 1909 Kapteyn rushed two papers to Hale and Frost on absorption in space. Hale was cooperative and directed Frost to expedite the matter, and to have preprints published in the event the papers did not meet publication deadlines (the second one did not). Frost, too, was cooperative, though he was less than enthusiastic for absorption in space. This close contact between Hale, Kapteyn, and Frost helps to illuminate a situation that arose in 1910. Kapteyn had been working along the evolutionary lines he had suggested to Hale in early 1909. During this period (possibly through communication during the course of calibration studies and comparative testing programmes for telescopes at Lick, Wilson, Yerkes and elsewhere, preparatory to the implementation of Kapteyn's "Plan of Selected Areas") Campbell learned of Kapteyn's detection of the increase of radial velocity with spectral type, and word got back to Kapteyn that apparently Campbell was working along the same lines. Kepteyn's reaction was less than favourable, since during the course of comparative tests of telescopes between various observatories some difficulties arose with the Crossley at Lick, and Campbell's response created some ill will. While Kapteyn wished to keep Campbell's favour concerning participation in his "Plan", he was less diplomatic concerning his radial velocity studies:

> ... I contemplate the publication of a note in which I will give the best results I can get for the radial velocities of the different classes of spectrum and the planetary nebulae and will point out how they can contribute to settle difficult points in the order of the evolution of stars and nebulae. My reason for publishing it now, without waiting for more materials is, that after what happened, I feel little inclined to run the risk of letting Campbell have the priority. It is not a very high motive but I cannot help feeling that way. I wish to make it a contribution of Mount Wilson, for the idea undoubtedly came to me by work connected with your observatory. I hope that you have no objection.<sup>73</sup>

Hale agreed to have the work published quickly, and apparently had no objections.

Kapteyn's paper, along the lines of his letter to Hale<sup>74</sup> was finally ready in draft form in January, 1910, but required a number of revisions, mainly to some of his later speculative discussions. Frost received Kapteyn's draft from Hale at Mount Wilson, and, in late February, wrote to Kapteyn:

... while I shake my head somewhat, as you knew I would, still I think there are some very clever inferences in the paper.<sup>75</sup>

Kapteyn immediately reviewed the evidence - that the radial velocities increased with spectral type and, hence, age, and that the planetaries had the greatest measured velocities of all. In so doing, he emphasised his main question: the reality of the origin of stars from nebulae. Unlike Innes (as reviewed by Eddington), Kapteyn was willing to suppose that Orion type irregular nebulae were separate from nebulae of the planetary variety, in keeping with Monck.

The many questions posed by Kapteyn in his letter to Hale, which necessarily came to mind as a result of his work (the placement of the Wolf-Rayet stars and the N stars; where the novae fit in, etc.) all survived in his paper. But, in addition, Kapteyn also wondered in the paper if, from his work, one could still consider Lockyer's scheme -

### O, M, (G), A, (G), N

as a possibility. Kapteyn did not think so, since this would destroy the continuity of the velocity relationship. But, explaining the existence of the relationship itself was a problem, too - why should it exist at all?

To attempt various speculative answers to this, he began with his consideration of the effect of an interstellar medium as a resisting force on primordial matter. Here, though, he was still unable to provide an unambiguous explanation of why the velocity should increase with contraction. Certainly, the resisting medium would be less effective upon a denser object; but why, unless the mass distribution in the locale was actually highly asymmetric, would the body under condensation begin to move at all? To Kapteyn, the interstellar medium defined what we would today call the "Local Standard of Rest". It is not unreasonable to assume that some slight perturbation of the condensing star would cause it to begin to move with respect to the <u>LSR</u>. Possibly, Kapteyn's next observation, which was not in his letter, could help us to see what was in his mind. He turned to the space motions of well defined star clusters, like the Hyades and the Pleiades.

Specifically, Kapteyn wondered why these groups still existed with similar space motions, if his idea was correct. "It is certain that, under the influence of the mutual attraction of the members of the groups, and in part also under the influence of the attraction of other stars, this parallelism and equality cannot continue to exist indefinitely. The time must inevitably come when they will be so thoroughly destroyed that no appreciable trace of a community of motion will be left ....<sup>76</sup>

Obviously, the common origin of such groups meant that originally they had been formed out of the same primordial matter, existing at rest with respect to the <u>LSR</u>. Determining the age then of these groups was a goal of high priority set by Kapteyn. As he had noted in his correspondence, ages could be inferred from examining spectroscopic binary systems, using his own test for component rotation and Darwin's models.<sup>77</sup> Kapteyn did not explicitly mention this in his paper, but, as the Hyades was known to have several spectroscopic binaries in attendance, as determined by Frost,<sup>78</sup> Kapteyn indicated that "... we shall be able to determine roughly the time necessary to produce internal motions in the group of an amount equal to that which the observations allow us to assume as possibly now existing. This interval will be the maximum interval during which the system can have existed abandoned to the normal and unchecked action of mutual gravitation ..."<sup>79</sup> Kapteyn added in a footnote to this that he was in a position to get better data soon, presumably referring to his ideas about spectroscopic binaries, but added: "... I prefer to suppress my provisional results ..."

Thus, most definitely, Kapteyn saw mutual gravitational effects as the origin for the attainment of a finite velocity with initial condensation. Also, with increasing density, and the resulting diminution of the effect of the resisting interstellar medium, the progression of velocity with advancing type was explained.

Kapteyn carried his discussion to stellar systems and to star streaming. He felt that the regularity of motion exhibited by the two streams must become less with time due to the mutual attraction of members of each stream. "That the stream motion is still recognizable at the present time must be due to the fact that the perturbing forces have not effectually worked for an indefinite time".<sup>80</sup> Thus, the existence of the streams was another indication of the finite age of the system, dating from a time "... when gravitation apparently or really had no effect".<sup>81</sup> Kapteyn even considered the persistence of the Milky Way. But, at this point, he felt it wise to return to the primary discussion: finding relative ages of the spectral classes, from their other observed properties.

Kapteyn first considered luminosities, or average absolute magnitudes, which he left undefined:

It seems probable that the average <u>absolute</u> magnitudes are different for stars of different spectral classes, and that they will fall into a smooth curve when the stars are arranged in the proper order.<sup>82</sup>

It is quite possible that Kapteyn had actually seen, or heard of, Russell's work at this time. He surely knew of Hertzsprung's work, at least through Schwarzschild. We will examine these possibilities

later; but, for now, it remains a fascinating statement, which must be examined in the present context. Kapteyn indicated that various attempts had been made to determine average absolute magnitudes, but that they were fraught with error. The averages were also illusory, since none of the classes of stars had been observed completely, and most certainly had members whose luminosity ranges were far below observability.

Kapteyn drew additional interpretive material from the statistics of binary orbits. The percentage of known B type stars that were spectroscopic binaries was higher than for any other type. Even though this might be due to some sort of selection effect, Kapteyn pursued the idea, and compiled a table of average orbital periods of all types of binaries as a function of spectral type. Systems with advanced spectra had longer periods than systems with early-type spectra. This could be due to selection, but, to Kapteyn, this distribution could also be explained by George Darwin's theory of binary evolution, which showed that the period of a binary system would lengthen under mutual tidal effects. This idea persisted for quite some time, and was used also by Campbell and Russell, though Russell had a different interpretation for the evolutionary progression as we shall see.

Returning to his star streams, but this time examining them as a function of spectral type, Kapteyn surmised that stars of earlier type should show more well-defined stream motion, since less time had elapsed for their dispersal by mutual gravitational effects. He felt that observational evidence was already available, from F.W. Dyson's work,<sup>83</sup> showing that stars of Type I held to better defined stream motion than did other types. Dyson's work was limited to Type I and II stars, and was later confirmed by Eddington. Eddington felt that these early identifications by himself and by Dyson were simply precursors to the actual relation between velocity and spectral type discovered by

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# Kapteyn and Campbell.<sup>84</sup>

Dyson's observations were also used to demonstrate the fact that "... there cannot be the slightest doubt but that the cloud motion and the internal motion for the Orion stars7 must be very small as compared with the corresponding quantities for the rest of the stars".<sup>85</sup> Thus Kapteyn wondered if star streams began literally at rest, and only achieved stream motion when the stars themselves passed through the Orion stage. There was then no need to assume initial motions, or some primordial mover.

In the following years, largely due to Campbell's organised efforts, the number of radial velocities available was to increase enormously.<sup>86</sup> In 1914, however, Eddington still felt that, for the later spectral types, the relationship, while in the right order, was not significant; though the general behaviour of the progression from the Orion stars through the planetary nebulae was most definite, and of considerable importance in evolution. Eddington basically followed Kapteyn's reasoning for the acquisition of greater velocities with advancing condensation,<sup>87</sup> but also mentioned the work of J. Halm, who had greatly excited both Kapteyn and Gill with his interpretation of the spiral patterns of stream motion.<sup>88</sup> Halm suggested<sup>89</sup> that the increase in velocity could be due to the principle of equipartition of energy - the B stars, being the most massive, would move at the slowest rates.

At this point, Kapteyn's conclusions have been illustrated to a sufficient degree to allow us to turn to W.W. Campbell's remarks, as he developed arguments that produced very much the same evolutionary picture.

In a chapter entitled "Radial Velocity and Spectral Type" in his book <u>Stellar Motions</u>, <sup>90</sup> which was an expansion of a series of lectures

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given at Yale early in 1910 and was completed by June 1, 1912 (from the date of the author's Preface), Campbell discussed at length the history of his discovery. First, he stated that "There can be little doubt that the Class B stars of the Orion region are or have been intimately associated with the great nebulous structures we know to exist there ...." He then provided observed radial velocities for the densest portions of the Orion nebula, and for the B stars within the Orion region. Inexplicably, the values were +17.4 km/sec and +22<sup>1</sup>/<sub>2</sub> km/sec, which caused him to comment: "Here again we have an indication, more or less weighty, that the observed radial velocities of Class B stars are for some unknown reason about 5 km. too great ... " Campbell considered the discrepancy as possibly due to an atmospheric pressure effect, which remained a persistent explanation. It has since come to be known as the K term, representing possible systematic errors in the wavelengths for early type stars, or actual atmospheric effects.92

The important point to make here is that, even in the face of the K term, Campbell considered the <u>a priori</u> association of B type stars with the Orion nebula to be so certain that he was willing to consider the residual as either a spectral peculiarity, or as an error arising from systematic problems.

When discussing average residual radial velocities, we find Campbell close to Kapteyn. First he reviewed the spread of radial velocities, with respect to spectral types I (B to F4) and II (F5 to M). For II, a far greater dispersion was found. Within type II, Campbell included the few stars that really were of Secchi's third type. From his examination of the two types, he concluded that the type II stars had residual radial velocities "... nearly 50 per cent greater than those of the Type I stars ...."<sup>93</sup>

He then looked at the distribution in greater detail, recalling his own work in 1901 on the radial velocities of 280 stars of G, K and M types, and using Frost's and Adams' B star study in 1904. He concluded:

> I am led to the remarkable conclusion that the velocities of the stars must be functions of their spectral types; that is, of their effective ages ... The progression of average velocity with advancing spectral type is clear and unmistakable.<sup>94</sup>

At this point, Campbell wanted to set the record clear as to the priority of the discovery. He included a long statement that fell into three parts.

He began: "... As the question of priority in making this discovery is of interest to some writers, I make the following statement ..." First, before the Silliman Lectures at Yale in January, 1910, he had discussed his discoveries with "... high officials of the University of California ..." meeting in San Francisco. Also, by February, he had already compiled a list showing the increase of radial velocity with spectral type.

Thus, by his long footnote, Campbell was able to show that he derived the relation before Kapteyn's publication appeared. But it is evident from material thus far found that Kapteyn actually preceded Campbell's work by about one year. Even though Campbell certainly could not have known of Kapteyn's paper while he was delivering the Silliman Lectures (unless Hale had mentioned it to him, which is possible), he most definitely was aware of it by the time he prepared <u>Stellar Motions</u> for publication. It is strange to find no reference to Kapteyn's 1910 paper in <u>Stellar Motions</u> within this particular section.<sup>95</sup>

Campbell's tabulated listing of radial velocities showed that planetary nebulae had very high values, which surprised him, though he thought that since only 13 objects were in the sample as observed by Keeler, the result might be spurious. He noted, however, that in addition to the 13 planetaries, he had also included the Orion Nebula. When it was taken away from the sample, the average velocity increased, which indicated a very different order of evolution for the Orion Nebula:

> Here we may have evidence of great strength and importance, in support of a hypothesis that the planetary nebulae have been formed from stars through processes arising from collisions with or close approaches to other massive bodies.<sup>96</sup>

Campbell's interpretation is consistent with his work in the nineties on the spectra of novae. He had argued for the production of planetary nebulae from novae events, as a result of collisions. This idea remained quite controversial, but seemed to be decided for the moment in favour of Campbell, after Ritchey's photograph of a nebular aureole around Nova Persei in 1901.<sup>97</sup>

In April, 1911, Campbell addressed the American Philosophical Society in Philadelphia about the results of his work.<sup>98</sup> His review was very similar to Kapteyn's, and agreed with the general interpretation of the cause as an age effect, and due to real space motions. But Campbell also wondered about the K term.

He noted that it was essentially zero for classes F and G, and increased both for earlier and later classes. This he felt was due more to the fact that he had calibrated his wavelength system with solar spectra of type G, but still he wondered as to the origin of the deviations:

> Are the wavelengths increasingly greater than we have assumed them as we pass to stars of younger effective ages and to stars of older effective ages? Have the presumably deep atmospheres of early-type stars and the presumably thin and dense atmospheres of latetype stars increased the wavelengths beyond the 99 values which we obtain from the solar spectrum?

His explanation for the K term was evolutionary, based upon the prevalent view of the migration of the photosphere (or, as in the early type stars - the photospheric-type layer) from deep in the stellar interior to a region overlaid by only a narrow but dense stratum.

Campbell provided, in a summary, an extensive discussion of the possible causes of the increased radial velocity with advancing spectral type. He wondered, as did Kapteyn, how irregular nebulae and the earliest stars could not be affected by gravitation, and if this indicated that stellar matter in the "... ante-stellar state ..."<sup>100</sup> might not be subject to gravitation: "... Do these materials exist in forms so finely divided that repulsion under radiation pressure more or less closely balances gravitational attraction"? With subsequent condensation, as Kapteyn suggested, gravitational forces might begin to dominate. The great velocities seen for the planetaries might, in addition, be a result of the explosive nova event that produced them.

Campbell continued to speculate on this matter, and listed various phenomena associated with the increase of stellar velocity "... with increasing effective stellar ages ..." He provided nothing new, however. He was quite interested in the rarity of B stars, based upon Pickering's work, and emphasised their close relationship to the Milky Way; their association with irregular nebulae (through similar velocities); and their apparent clustering tendency - all of which suggested stars of young age and recent formation.

A complete picture of the rift between Campbell and Kapteyn has not emerged as yet,<sup>101</sup> but it is clear that, concerning evolution, they were very much in agreement.

We should not leave our discussion of Campbell without some mention of his work on spectroscopic binaries, which resulted in two large catalogues, both published in 1910. As this source was being compiled at Lick, Campbell noticed a certain relationship between spectral type and period for 65 systems with reasonably determined periods, as he recollected in <u>Stellar Motions</u>:

> My colleagues and I have been noticing, for several years, that for the binaries of early spectral classes there is a tendency toward short periods and orbits nearly circular; and for binaries of the older spectral classes, a tendency toward periods relatively long and orbits of considerable eccentricity.<sup>102</sup>

A period-spectrum relationship was clearly in evidence, 103 but Campbell had to do some talking to convince himself that there was a variation in actual component <u>separation</u>, as a function of spectral type, which would be the clinching argument in any evolutionary scheme based upon Darwin. At the time, even with assumptions about the average inclination for a spectroscopic system, Campbell felt that mass data were too meagre to allow for any discussion of actual orbital dimensions. But he had another argument - Aitken's compilations of the statistics for visual binaries. From Aitken's work (Aitken was assistant director at Lick, under Campbell's direction at the time), it was clear that the observed number of visual pairs increased with advancing spectral type. <sup>104</sup> Further, Campbell noted that for visual doubles of comparatively short period the populations in each spectral range were similar, except that they dropped for the later spectral classes (G - K). Thus: "Visual double stars clearly abhor the Classes O and B, and visual double stars of relatively short periods clearly abhor Classes M and N ... " indicating thereby that visual pairs of the late types exhibited only long periods. Campbell brought together periods and eccentricities for spectroscopic and visual systems (recalling that the eccentricities clearly increased with increasing period) and asked: "What is the significance of these facts"?

The significance was general agreement with models for binary

evolution developed by Darwin, Poincare, and See. Campbell reviewed

their general scheme:

... they came to the conclusion that a condensing nebulous mass, rotating about an axis, constantly faster and faster, to keep pace with loss of heat through radiation, should eventually separate into two nebulous masses revolving around their mutual center of mass. These two masses would in the beginning be revolving in contact, in orbits essentially circular. With advancing time, tidal disturbances within the more or less viscous bodies would cause them to draw apart, rapidly at first and less rapidly later. Jeans (Phil Trans 199 A,1, 1902) and others have called attention to certain limitations in these investigations, which their authors recognize. Darwin has, in fact, stated (Darwin and Modern Science pp. 548-9, Cambridge, 1909) that the assumed conditions in the parent mass are necessarily not in strict accord with probable distribution of density and other circumstances. However, confidence prevails that the deductions are substantially correct.<sup>106</sup>

In addition, See's Ph.D. thesis in 1892 provided theoretical evidence that eccentricities and periods should increase with age as a result of tidal friction.

Campbell apparently did not want to suggest in his discussion that visual binaries evolved out of spectroscopic ones, though Russell and others later felt that Campbell did hold to this opinion. In his book, at least, he felt that the absence of early type visuals and late type spectroscopic systems was a selection effect. In this manner, though he still felt that the preponderance of short period systems amongst early type stars was of evolutionary significance, he did not express any opinion as to the degree of orbital enlargement that might be expected as a system aged. At least nowhere did he state explicitly that with increasing age, an early type spectroscopic system could be expected to become a late type visual system.

These two elements in Campbell's main life-work - the radial velocities of stars and the analysis of spectroscopic binary systems -

yielded evidence of evolutionary progress that was of the classic, linear view of condensation of blue stars from nebulae and their subsequent contraction and cooling to the red stages. He was to rely heavily upon his own statistical work in a series of lectures on stellar evolution given in 1914 before the National Academy of Sciences. In the intervening years, Campbell and Russell corresponded on matters pertinent to this discussion. Russell was evidently trying to make Campbell see his own views, but Campbell remained unsympathetic, and barely mentioned Russell's new ideas in his address.<sup>107</sup>

#### Eddington

Eddington's book <u>Stellar Movements and the Structure of the</u> <u>Universe</u> has been considered by many to be the best statement of the degree of solution of the various problems under discussion here as they stood in 1914. From my own work, I can attest to the fact that most certainly he was less biased than Campbell in discussing the relative merits of the classic evolutionary view and that of Russell. We have already mentioned that, by that time, Eddington felt that the increase of velocity with advancing spectral type was significant for the earlier classes, but that the data at hand for the later classes were not conclusive. Eddington did, however, believe in the fundamental reality of the discovery, and interpreted it along evolutionary lines.

In another discussion of statistical importance, actually a continuation of some of Monck's original ideas, Eddington reviewed determinations of the mean distances of the various spectral types from the work of Boss, Kapteyn, Campbell, H.S. Jones, and Schwarzschild. All seemed to show that, from parallactic motion (mean parallaxes), the F, G and K types were closest, and the O, B and A's <u>and</u> M's were the most distant. "The order of distance is thus altogether different from the

standard order BAFGKM. In particular it appears that the stars of Type M are more distant than any other type except B ...<sup>108</sup> Of course, by this time Eddington knew of the great variation in luminosity of the M stars, from Russell's work, but at this point in his book he was not yet ready to discuss it, and so wondered:

> How then is it that the M stars show practically no galactic concentration, whereas the A stars are strongly condensed? Our previous explanation fails, because the assumption that Type M is much less remote than Type A is now shown to be false. It seems necessary to conclude that the apparent differences in galactic distribution are real; that the system of the A stars is very oblate, and the system of Type M is almost globular.<sup>109</sup>

His globular distribution was illusory, because of his mixed sample. (Today, we know that M subdwarfs indeed have a globular galactic distribution, which is an important element in the evolutionary concept of stellar populations.) Eddington's conclusions, however, are very close to the mark:

> The stars are formed mainly in the galactic plane. Type B, on account of the low individual speeds and the short time elapsed since birth, remains strongly condensed in the plane. In succeeding stages the stars have had time to stray farther from the galactic plane ... In the latest type, M, 110 the stars have become almost uniformly scattered.

While this was Eddington's conclusion, he cautioned that it should not be taken as truth until Russell's observations of the high luminosity M stars became better understood. Understandably, the statistical evidence supported the highly luminous view for M stars, since very few M dwarfs were bright enough to have been identified for measurement and discussion. That this was the case makes the work of Hertzsprung, some ten years prior, even of greater interest, as it emphasises his unique ability to detect, through statistical analysis, quite subtle effects caused by the mixing of giants and dwarfs.

### Hertzsprung

Possibly due to the originally obscure publication of Hertzsprung's work, and in response to the almost decade lag in the recognition of it, many reviews and appreciations of his work have appeared. Chief among these are Lundmark,<sup>111</sup> Hoffleit,<sup>112</sup> Nielsen<sup>113</sup> and Herrmann.<sup>114</sup> The last cited review reprints (in German) Hertzsprung's three primary papers published before 1910, together with an introductory preface outlining Hertzsprung's professional career. Nielsen prepared his review just a few years before Hertzsprung's death, and enjoyed the privilege of discussing the historical review directly with Hertzsprung, and of having direct access to his letters. Hoffleit has provided what is, in parts, a closely paraphrased discussion of Hertzsprung's original papers, and from a comparison of her text with the reprinted articles in Herrmann's work, has followed accurately the text of his remarks.

We will be interested in the development of Hertzsprung's realisation of his 'collateral' series, which allowed him to propose in 1905, though only very tentatively, the possible existence of parallel evolutionary paths for giants and dwarfs, or his "... Walen unter den Fischen ..." identified in 1909.<sup>115</sup>

In addition to the above three papers reprinted by Herrmann, a fourth, in 1906, which provided an explicit estimate for the diameter of Arcturus in terms of its apparent angular diameter (based upon blackbody laws) will be discussed. I am indebted to Dr. A.J. Wesselink, a student of Hertzsprung, for the knowledge of this important fourth Hertzsprung paper.<sup>116</sup> Its discussion, however, will be limited to the fact that an explicit diameter estimate was provided.

All the reviews cited above, and Hertzsprung's own statements, attest to the fact that the work of Antonia C. Maury, of Harvard College Observatory, was most influential in the development of his own work. Her identification of the a, b, c and ac subclassifications, and her statements about them were examined by Hertzsprung, who compared stars of similar spectra, but with different subclassifications, in order to find out why the differences in subclassification existed.

In his first paper, entitled "Zur Strahlung der Sterne I",<sup>117</sup> published in 1905, Hertzsprung discussed characteristic spectra separated into c and non-c lists. For these two lists of stars, chosen both from Maury's lists and A.J. Cannon's lists, he calculated reduced proper motions corrected for distance to m = 0 (m is apparent magnitude). Conversely, he calculated and listed the apparent magnitudes the non-c stars would have if their distances were adjusted so that their proper motions would be 0".01 per century.

His primary objective was to compare the "reduced proper motions" for the c and non-c stars. For the c stars, Hertzsprung found only small values for the reduced proper motions. It was quite easy to see that the c stars had smaller values than the a and b subclasses. One of his first observations was that the reduced proper motions for many of his c stars were of the same order as those for Orion stars. These reduced proper motions were all exceedingly small, well under onetenth second of arc, which indicated statistically great distances. The c stars that satisfied this criterion were all brighter than fifth magnitude (apparent), which, when added to their great distance, indicated great intrinsic luminosity. Significantly, the Orion stars mentioned were also above this limit. Thus, for all stars above the visual magnitude 5, those with smallest reduced proper motions, and hence with the greatest intrinsic luminosities, were the Orions and the c type stars. For stars fainter than 5, however, Hertzsprung found that the largest reduced proper motions were for stars in the F, G and K ranges, and not amongst the red M stars. Following Maury's suggestion that her c characteristic stars indicated "... that a single series was inadequate

to represent the peculiarities which presented themselves in certain cases, and that it would be more satisfactory to assume the existence of collateral series ... <u>which</u>? ... pursue parallel courses of development ....<sup>118</sup> Hertzsprung concluded:

> The collateral series hypothesis is suitable since it can explain the chief appearance that for stars which appear to us brighter than m = 5, the c and Orion type stars are the brightest, and for all other stars fainter than 5, not the red but the yellow are the weakest. The series should become the following:<sup>119</sup>

	NEDEL		Nebel
S	Monocerotis	$x_2$	Orionis
E	Orionis	β	Orionis
Υ	Orionis	α	Cygni
α	Leonis	δ	Canis Majoris
α	Canis Majoris	α	Bootis
α	Aquilae	α	Orionis
α	Canis Minoris		Vogels Type IV /sic7
	Sonne		
70	Ophiuchi		
	· ·		

61 Cygni

0 Eridani B.C.

.....

"schwarze" Sterne

Hertzsprung's reference to "Vogels Type IV" above no doubt includes the carbon stars of type N, which were Vogel's IIIb or Secchi's Type IV. Today, the left-hand column contains stars representative of the main sequence, while those of the right are giant and supergiant stars.

It should be noted that Monck, about one decade prior, also came to the conclusion that the yellow stars were the faintest, since they had the largest proper motions. But unlike Monck, Hertzsprung was able to employ Maury's c characteristic, and find that, for the red stars, the mean value of the reduced proper motion was diminished by the inclusion of stars present on both collateral series. In order to be able to explain this effect of mixing, Hertzsprung had to find out why it occurred so abruptly between the late G - K range and the M range, or Maury's classes XIII - XIV. Hertzsprung was able to provide an explanation in 1907 in his second paper, which utilised better criteria for the selection of data. Within this paper, he examined stars with measured parallaxes greater than 0".1 of arc, and computed their magnitudes assuming a common parallax of 1".0 second. This, of course, is similar to our present definition of "absolute magnitude", except that today we use 0".1, or ten parsecs, as the common distance.

With parallaxes for distance measurement, <u>/</u>obviously better than the statistical use of small samples of proper motions7 Hertzsprung was able to determine better luminosity functions for stars brighter than 5th, and for those which represented the general population of Maury's a and b subclasses. 120 Those brighter than 5th were predominantly c stars, but, these were very few in the list of stars with parallaxes greater than 0".1. Thus, the very luminous c stars were also quite rare in space. From a list of 95 parallax stars, he found that 78 were intrinsically fainter than the Sun; and that, of these 78 stars, about two-thirds were fainter than fifth magnitude. Thus, very few of these would be included amongst the c star lists. They would therefore not contribute significantly when the sample was based not on parallaxes, but upon reduced proper motions, which allowed for a much larger volume of space to be examined. Thus, Hertzsprung's first listing in 1905 contained predominantly highly luminous red stars, and excluded the This did not happen as markedly for the yellow stars of faint ones. types XIII and prior, since they had a larger number of dwarfs represented and hence, their reduced proper motions were larger on the average.

The differential effect of mixing, which caused the observed 'jump' from Maury's class XIII to XIV was supported by the fact that the c characteristic became less pronounced as one progressed to the later spectral classes. Hertzsprung noted:

> If the c- and ac- stars are considered in summary, one sees, that with increasing class, the c- characteristic diminishes and ceases just when the bright K stars begin.<sup>122</sup>

This supports the idea that Hertzsprung's first table in 1905 contained both a, b, c and ac stars in the groups later than XIII, since at that point the spectra became so complicated that, at the time at least, it was impossible to differentiate the a, b, ac or c criteria.<sup>123</sup> This also explains the fact that Hertzsprung's statistical listing of c and ac stars<sup>124</sup> ends with groups XIII and XIV.

By using selective parallax data to test the influence of selection based upon reduced proper motions, Hertzsprung strengthened his conclusions regarding the high intrinsic luminosities of the c stars. Lundmark has pointed out that along with this discussion of the differences between the XIII and XIV classes (of the a and b subclasses, as opposed to the c subclass), Hertzsprung noted that there was a distinct spectral change for the line at 4077 with a change in reduced proper motion. Lundmark expressed the belief<sup>125</sup> that this was the first observation of a luminosity dependent spectral change; but, as we have seen, Lockyer, Pickering and others had most certainly identified luminosity dependent criteria, too.

Hertzsprung completed his first paper in December, 1905, and his second, under the same title, in December, 1906. In March, 1906, as he developed his parallax material, and as the cause for the jump between Maury's groups XIII and XIV came to be seen as a selection effect, he wrote to E.C. Pickering to announce his findings:

Referring to my note "Zur Strahlung der Sterne", I should like to point out, that according to my present view of the collateral series, the faint stars with great parallaxes generally should have spectra of the "red" classes G, K, M. This assumption I find confirmed by the few spectra given in the table of known parallaxes in Kobold: Bau des Fixsternsystems, but the number of stars is still small.

If the maximum of reduced proper motion near the class G is caused by the fact, that in the following classes (K, M) the absolut /sic/ bright stars not belonging to the relative dark solar series are predominant in number, it is probable, that in some group near G the hypothetical two series (solar-ser. and brightser.) will be approximately equally represented, so that the proper motions, reduced to the same brightness, of stars ranked in this group will be considerably more different inter se as is the case in other groups. In the following two tables the reduced proper motions of the stars belonging to the groups XIIIa and XIVa of Antonia C. Maury are indicated.

It will be seen, that the reduced proper motions in group XIIIa are so, that all the stars indicated may belong to the solar series, while in group XIVa there is an extraordinary great number of stars with small reduced proper motions ...

... In the groups K and M the difference between absolute brightness of the stars is perhaps still greater and whatever may be the cause of such difference, it is a priori probable that there will be some marked distinction between the spectra of such stars, f. ex:

	Magn.	parall.		diff. in abs. brightness magn.	_o.c <u>.</u> 7
α Auriage	.21	".09	)	4.5	<b>∠</b> `G_7
α Centauri	.06	".76	)		
α Tauri	1.06	".12	)	5 9	/ <b>x</b> 7
61 Cygni	4.96	".30	)	0.0	6-2
a Bootis	•24	".03	)	7.6	
70 Ophiuchi	4.07	".17	)		
a Orionis	1.00	".03	)	19.1	/TM 7
Lal. 21258	8.50	".25	)	- <i>L</i> • -	<b>6</b>

Regarding the small reduced proper motions of the stars belonging to the divisions c and ac, I should like to mention, that  $\nu$  Ursa Majoris is the only ac- star for which a great reduced proper motion is found and this star also differs from the most of the same division in lying far from the milky way.<sup>126</sup>

This was a covering letter to Hertzsprung's first paper in 1905. From Hertzsprung's first paragraph, we see that he was well aware from parallax lists that the red stars should have the greatest parallaxes, and hence be the faintest, and that the deviation from this was caused by the inclusion of "absolute bright stars" of classes K and M.

The table included in the letter appeared in Hertzsprung's second paper (1907) with but slight changes (the star Lal. 21258 was replaced by Lac. 9352; 61 Cygni by 61 Cygni A), giving slightly different values for the magnitude differences and the addition of A.J. Cannon's classifications. From this table, it was clear that the magnitude difference between the normal a and b stars, and the c stars increased with the progression from G to M.

From his listings for stars with parallaxes greater than 0".1 in his second paper (1907), Hertzsprung pointed out that, if the stars were listed in order of his 'absolute magnitude', several stars were found to be quite displaced from their normal place as determined by spectrum.<sup>127</sup> The most notable displacement was for  $\alpha$  Tauri of spectrum XVIa (or of Draper Class K): the difference between  $\alpha$  Tauri (Aldebaran) and the 'normal' stars of this class was about  $7\frac{1}{2}$  magnitudes (i.e. a brightness difference of about one thousand). Without mentioning giants and dwarfs, of course, Hertzsprung concluded from this observation that his original table of Maury's normal a and b stars in fact contained many stars of high luminosity. This observation also allowed him to comment on evolution:

We are now able to accept, that the bright red stars ... are rare (or seldom seen ...) within the volume of space the normal solar series is seen in the majority. The bright red stages are, therefore, likely to be run quickly through, or the stars, which are found here, belong to a collateral series. Whether an eventual connection between the collateral series and the solar series exists, or a gap exists between the two, remains to be seen.<sup>128</sup>

Hertzsprung continued to favour the collateral evolution picture, but did not attempt any further discussion of it. Of great interest is his recognition of the possibility that the bright red stars are in a stage of evolution that passes quickly relative to the rate of evolution of the solar series (or the Main Sequence), an idea in vogue today.

Most of the reviews of Hertzsprung's work mention that his observations remained generally unknown because he published in a nonastronomical journal,<sup>129</sup> but Hoffleit has added that it was common at the time to have the work of an unknown newcomer unheeded.<sup>130</sup> Neilsen has recorded that Eddington, about twenty years later, wrote to Hertzsprung: "... One of the sins of your youth was to publish important papers in inaccessible places ..."<sup>131</sup>

Another factor causing the lack of recognition, as brought out in conversations with A.J. Wesselink,<sup>132</sup> was that Hertzsprung's methods of analysis were subtle and sophisticated, and that he had a way of understating his case. While this may be true, his letters to Pickering, as we shall see, were forceful. One final question, which has not as yet been answered at this time, deals with Hertzsprung's relationship to Kapteyn. Hertzsprung made great use of Kapteyn's work, especially his secular parallaxes. But Hertzsprung's detection of luminosity criteria in Maury's classification ran counter to Kapteyn's to interstellar absorption. How Hertzsprung's work was received by Kapteyn prior to Russell's work is not known at present. Needless to say, the two most certainly had some contact, since Hertzsprung married Kapteyn's daughter!

Kapteyn, in discussing his observations of the increase of radial velocity with spectral type in 1910.<sup>133</sup> believed that most stars, when arranged by spectral class and absolute magnitude, fell onto a smooth curve. This could have come from a reading of Hertzsprung's 1907 paper, in his discussion of the Pleiades, where he provided a table comparing luminosities and spectral types for stars in the cluster. Hertzsprung realised that the physical members of the Pleiades (or any finite cluster) must all be at practically the same distance, and therefore their relative apparent brightnesses must accurately betray their relative intrinsic brightnesses. Accordingly, he drew up a table which showed that, indeed, the fainter stars were redder than the brighter ones. 134 From this table, Hertzsprung tentatively recognised the relationship expressed by the Hertzsprung-Russell Diagram - the decrease of brightness with spectral class for main sequence stars. From such a small sample, the relation was at best qualitative.

While still at Göttingen, according to Nielsen, Hertzsprung attempted to strengthen his observation. He directed an assistant named Rosenberg, provided by Schwarzschild, to examine Copenhagen plates of the Pleiades and Hyades. This ended in a 1911 publication of colourmagnitude diagrams for the two clusters.<sup>135</sup> Hertzsprung's own graphical representations, in the <u>Potsdam Publications</u> for the same year, did not appear in the published volume until its release in 1913. Thus Rosenberg's were the first to see print.

Nielsen has indicated that Hertzsprung had created such a diagram for the Pleiades as early as 1908, but did not publish it

since it was known to be distorted by the colour-curve of the objective lens employed.

It should be noted that Hertzsprung's colour-magnitude diagrams plotted apparent magnitude against an effective wavelength value, which was a measure of colour. His designation of this quantity, clearly an important advance over the association of spectral class with brightness, allowed for the direct examination of what is called 'interval data', with a correlation possible between numerical coordinates. Hertzsprung's use of effective wavelengths was derived from his employment of Planck's radiation laws as early as 1906 in another, even obscurer paper not mentioned by other reviewers, but which was kindly made available to me by A.J. Wesselink, a student of Hertzsprung's. In this paper,<sup>137</sup> Hertzsprung determined the Planck functions for black bodies at different temperatures, corrected for observational error (instrument and eye); discussed the maximum intensity relations of Paschen and Wien; and even provided an explicit estimate for the apparent angular diameter of Arcturus.

Hertzsprung's work in 1905 and 1906, and his letter to Pickering in March, 1906, remained unheeded. Pickering's response has unfortunately not been found; but, in the next year, correspondence between Hertzsprung and Pickering indicates that an additional element in the obscurity of Hertzsprung's work was of importance. In 1907, Hertzsprung wrote to Pickering asking for copies of the Draper Catalogue, and, by April, Pickering responded by sending volumes <u>26</u> and <u>27</u> of the <u>Harvard Annals</u>, which included Fleming's early classifications.

In 1908, after <u>Volume 50</u> had been issued and Hertzsprung had received a copy, he realised that A.J. Cannon, who had assumed the main duty at Harvard in the continuance of classification work, had failed to reinstate Maury's horizontal subclassifications (which had been

excluded from <u>Volume 28 pt. II</u> in 1901). In Cannon's 1901 system, much of her criteria and designations could be imagined to carry over from Maury's ideas - with her numerical subdivisions, she accounted for 28 separate designations, the same number used by Maury<sup>138</sup> - but in subsequent issues of the Draper Catalogue, her criteria and subclasses became progressively simpler.

Hertzsprung was surprised at this apparent neglect of Maury's system, within which he saw so much significance. No doubt he felt that his original letter to Pickering in March, 1906, with his enclosed article, should have been appreciated, as they were by Schwarzschild. <u>Volume 50</u>, which was the publication of the <u>Revised Harvard Photometry</u>, was completed by Fleming and other staff assistants, and did not retain Maury's subclasses, preferring to use the simpler Cannon system. The important problem was that, while A.J. Cannon continued to provide explanatory footnotes for those stars that exhibited the c spectral peculiarity, the <u>Revised Harvard Photometry</u> did not provide any indication of peculiarity. This was of concern, as Hertzsprung pointed out to Pickering:

With great interest I have seen the new catalogue of stellar spectra contained in Vol. L of the Harvard Annals.

But in one respect I have been disappointed and I allow me directly to say a few words on that point.

On my opinion the separation by Antonia C. Maury of the c- and ac- stars is the most important advancement in stellar classification since the trials by Vogel and Secchi. But in the new catalogue the spectra of some of them as  $\alpha$  Cygni and  $\delta$  Cephei are not even mentioned as peculiar.

It is hardly exaggerated to say that the spectral classification now adopted is of similar value as a botany, which divide the flowers according to their size and color. To neglect the c- properties in classifying stellar

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spectra, I think, is nearly the same thing as if the zoologist, who has detected the deciding differences between a whale and a fish, would continue in classifying them together.

I hope that later you will also publish a catalogue of stars having spectra of the c- and ac- kind (not "subdivision") corresponding to the "Catalogue of stars having spectra of class B".139

Pickering's reply came in early August:

... I am glad to know your views regarding stellar spectra. Our objective prism is not well adapted to determining the breadth of the lines in stellar spectra. A slight change in focus, unsteadiness of the air or other causes wash the lines. It is difficult to decide whether this effect is due to these causes unless the spectra of other stars appear on the plate. When the dispersion is small many other spectra appear and in some cases this effect is well shown. See H.A. ? . . . extending Miss Maury's work to the southern stars. After consultation, we concluded that the causes <u>lof7</u> the differences in width were too uncertain and could be determined better with a slit spectroscope ... 140

Pickering, quite probably, was referring to <u>Harvard Annals 28</u>, pt. II "Spectra of Bright Southern Stars" by A.J. Cannon and himself, published in 1901. In their introduction, it was recognised that "... There are doubtless great differences in the width and sharpness of the spectral lines ...", but that "... Partly from the fact that so small a proportion of the total number of stars classified has been photographed with more than one prism, it was found inexpedient to make the divisions "a", "b" and "c", as given in Part I of this volume ....<sup>141</sup> Besides the scarcity of high dispersion spectra, the problems inherent in the objective prism technique - such as proper focus and alignment of the plate - were added to make the entire observation of the a, b and c designations suspect.

Pickering's caution has been interpreted by Jones and Boyd as due to his training as a physicist. Pickering's forte, whilst at MIT prior to his advancement to the directorship at

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Harvard, was laboratory physics. As has been pointed out elsewhere, 142 Pickering's early attempts in 1880-81 to deduce stellar characteristics from photometric and spectroscopic data, while theoretically possible, were impractical due to the poor consistency of astronomical data. This early experience, possibly an important element in Pickering's decision to embark on the vast photometric and spectroscopic projects at Harvard, no doubt left him sceptical of marginal data. It must be considered a positive attribute, save for the possibility that he was too conservative regarding Hertzsprung's work. It is also important that, at the time, Pickering was aware that Henry Norris Russell was coming to the same conclusions that Hertzsprung had come to (in great part aided by Pickering and Miss Cannon). As we will point out, by 1910 Pickering was to write to Frost and to Campbell asking for slit spectra, quite possibly in response to Hertzsprung's persistence, but it seems premature to discuss Pickering's reticence in terms of the question of his support for Russell's independent work.

An additional stimulus for conservatism on Pickering's part was that his classification system, which had been in existence for some seventeen years, was far from being accepted universally by astronomers. Even Hale continued to employ Secchi's classification. Clearly, if Pickering was ever to enjoy the acceptance and canonization of his Draper system, he had to be sure that it did not overstep carefully selected criteria, which included at that time the concept of a linear evolutionary sequence.

Hertzsprung was persistent, however, in his push for the recognition of Maury's subclassifications. He wrote back to Pickering on 17 August, 1908, arguing that there were other points to consider:

The fact that none of the stars called c by Antonia C. Maury has any certain trace of proper motion is, I think, sufficient to show that these stars are physically very different from those of divisions a and b.

I therefore am of opinion that we must lay the greatest stress on the c- peculiarities of stellar spectra nothwithstanding the difficulty in determining these peculiarities (which not only consists in the sharpness but also in the differing intensities of the lines).

I intend to write a note on the c- stars in the A.N. and shall allow me to send the manuscript in advance to you.<sup>143</sup>

To emphasise his beliefs, Hertzsprung included a list of eleven stars with Maury's c- characteristic from the proper motion lists available to him at the time. Hertzsprung's reference to his intention to write his third paper on the subject, this time in the <u>Astronomische Nachrichten</u> was a clear implication that, while he wanted Pickering's blessing, he intended to go on record in an astronomical journal with his interpretations of Maury's spectra. Thus, we might see his 1909 paper as a reaction to Pickering's reserve.

Hertzsprung was as forceful as possible with Pickering, and vented his annoyance for Pickering's reticence in letters to his friend and mentor, Karl Schwarzschild. Eleven days after his last letter to Pickering, Hertzsprung wrote to Schwarzschild:

> I have written a few words to Pickering about the removal of the c- characteristic in the /Harvard Annals/. I enclose his answer for your inspection ...

Due to the absence of the c- characteristic, I am annoyed with the Harvard spectral classification since one cannot determine how the spectral identification is to be taken. Thus the entire classification is inhomogeneous.<sup>144</sup>

Hertzsprung discussed various examples from the <u>Harvard Revised</u> catalogue, showing how very different stars were grouped together without explanation. At the end of his letter, he noted that he would be very interested to see Schwarzschild's recent work on stars of similar spectra whose colour-indeces (Farbentonung) were quite different.

Two months later (October, 1908), Hertzsprung had finished his manuscript, and sent Pickering a copy for suggestions:

I allow me to send you herewith a copy (not to be remitted) of the manuscript announced in my last letter.

I only wish to have stated my view so that it will not be wholly neglected.

Out of regard to your eventual objections the manuscript itself has not yet been sent to the A.N.<sup>145</sup>

Hertzsprung's letter continued with a detailed discussion of his use of the magnitudes published by Pickering in <u>Harvard Revised</u> <u>Photometry</u>, and how he altered them to his "color equivalents" between photographic and visual magnitudes, a most important ingredient in Hertzsprung's statistical work on clusters.

Unfortunately, no reply to Hertzsprung's letter or to his manuscript has been recovered from the Pickering collection. Within the next year, Pickering became involved with aspects of Kapteyn's Plan of Selected Areas, mostly in the production of systematic photometric data. Therefore, in some cases, it is difficult to assertain how Pickering eventually regarded Hertzsprung's work. There are some indications that work at Harvard, especially on the distribution of stars in space, ignored Hertzsprung. On this, his staff published a statement as a Harvard Circular in January, 1909.<sup>146</sup>

During 1909 and 1910, as plans developed for a meeting of the International Solar Union in Pasadena, contact amongst international groups increased. In June, 1910, Kapteyn arrived in the United States and progressed westward, stopping at many observatories.<sup>147</sup> Kapteyn's interests in distribution studies were at a peak, and no doubt he discussed them with everyone, and especially with Pickering and Frost. Doubtless, too, Hertzsprung's name emerged in conversation. In any event, by early July Pickering sent two letters to Frost and Campbell asking for slit spectra of stars that showed slight differences in line intensities:

> You have at your observatory a large number of photographs of stellar spectra which have already been measured. I understand that many of these show small differences in the relative intensity of the lines, so that the stars which are classified here as K, for example, with the better definition of the slit spectroscope show real differences in the composition of the stars. It has occurred to me that very valuable work could be done by Miss Cannon, who has had long experience in detailed classification ... and thus make a classification which would show differences, which I presume you are not likely to study ...<sup>148</sup>

Frost answered on July 6 and Campbell on July 10. Campbell was especially careful to have Paul Merrill select 50 spectra taken with the three-prism Mills spectrograph to represent adequately all spectral types (10 according to Campbell) with at least five separate spectra of each type. In late July, Pickering acknowledged receipt.

At present, it is not known definitely for what purpose these spectra were requested but, considering the developments of the previous two years, and his letter to Hertzsprung about the need for slit spectra to settle the issue, it is quite possible that Pickering finally came around to a serious test of Hertzsprung's views.<sup>149</sup>

By late August, Schwarzschild, Struve, and Dyson had all passed through Boston and Albany, N.Y., on their way to the meetings in Pasadena. No doubt the work of both Russell and Hertzsprung came up during this time and became generally known, as Lewis Boss, of the Dudley Observatory in Albany, wrote to Russell soon after their departure for the west and discussed Russell's recent publication of his parallax work.<sup>150</sup>

According to Nielsen, Russell and Schwarzschild met in Boston

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in August 1910 at a meeting of the Astronomical and Astrophysical Society of America held at Harvard College Observatory.<sup>151</sup> Russell presented a short paper entitled "Some Hints on the Order of Stellar Evolution",<sup>152</sup> and, as a result, Schwarzschild discussed Hertzsprung's work with him. Actually, by this time, Russell's ideas had already seen publication as a short note.<sup>153</sup>

Thus, in many ways, through the International Solar Union Conference and associated meetings and letters, Russell finally became aware of Hertzsprung's work. Hertzsprung, on the other hand, was sent a postcard by Schwarzschild<sup>154</sup> from Pasadena instructing him to send reprints of his papers to Russell as soon as possible. Hertzsprung did so, and on September 27, Russell (recently returned from Pasadena) replied to Hertzsprung upon receipt of the papers:

> Please accept my very hearty thanks for the reprints of your extremely interesting and important papers, which I have received this morning.

As you may hear from Professor Schwarzschild, I have noticed independently, - though some time later than you did - that the red stars fall naturally into two groups of very different absolute brightness.

I believe that they can all be fitted into one series of evolution by assuming that a star at first grows hotter as it contracts, then reaches a maximum temperature (corresponding to spectrum B or A) and later cools down.

The red stars of great luminosity (and great diameter, with small density) could then be young in evolution, and growing hotter. The red stars of small luminosity, as you suggest, could be late in evolution, and growing colder.

I read a paper on this theory at the Harvard meeting of the Astronomical Society of America, which I hope to publish soon in the <u>Astrophysical Journal /It did not appear</u>?. It will give me much pleasure to refer to your work, and to see, so far as I can, that you receive the credit which it richly deserves.

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Will you please convey my best wishes to Professor Schwarzschild? It was a great pleasure to make his acquaintance.<sup>155</sup>

On October 11, Hertzsprung replied from Potsdam:

The idea to place the bright red stars at the head of a series, was one of the first, which came to me. But I fail to find any evidence of its being correct and therefore have not mentioned this view in my papers. I should be very glad, if you find, where the bright red stars are to be placed - if in the series of development, to which our sun belongs or to a collateral series. (In this respect the variables of the Mira-Type are of special interest as they are relative /sic/ numerous pro /sic/ unit volume of the universe).

On my opinion one of the most important problems of present spectroscopy is to find the spectral equivalent of the great physical difference between bright and dark red stars. The object is to indicate the absolute brightness of a star only from the quality of its spectrum.<sup>156</sup>

With his letter, Hertzsprung included a small table of stars with high quality parallaxes which, in his opinion, showed "... The advance of spectrum with the decrease of absolute brightness and the relative high brightness of our Sun ..." This list included Main Sequence stars only. In another addendum, Hertzsprung reviewed his work from 1906: "If  $\alpha$  is radiating quantitative and qualitative as a black body with  $C_2/T = 5$ , its diameter should be about 0".1, a quantity which it will perhaps be possible to show". It is interesting that this star, Arcturus, was the same example used by Hertzsprung in a footnote to his 1906 paper on radiation,<sup>157</sup> though his value for the apparent angular diameter was twice as large as in 1906.

During the Harvard meetings in August, Schwarzschild also talked to E.C. Pickering, and evidently, from subsequent correspondence, asked him for some spectral class observations. By November, 1910, Pickering wrote back to Schwarzschild, who had returned to Europe, announcing that Mrs. Fleming had completed her careful examination, and added: "Under these circumstances the estimates seem to me remarkably good. As suggested by Hertzsprung, the figure is omitted only when the class of spectrum is a little uncertain ...."<sup>158</sup>

In this letter, we see Pickering at last acknowledging Hertzsprung's considerations (though the matter will take more searching, since it is not definite what Pickering meant by his word "figure").

It is safe to say, however, that by this time, with the Solar Union Conference completed, Pickering felt that his classification system was in a strong position. Prior to the meetings, he had fears that a planned session on spectral classification would not decide in his favour as to the most proper course to follow in classification in future work. These feelings were recorded by Pickering in a diary kept of his travels to the Solar Union.<sup>159</sup> Actually, as the diary records, Pickering did not wish to see the Solar Union extend its interests to stellar classification, though since the first meeting in 1904 the trend was inevitable. As he admitted to himself, he was reluctant to see a possible re-valuation of the Draper System. His fears were put to rest by acclamation by the conference attendees, so he might have felt more able to accept Hertzsprung's ideas, even though they suggested that the spectral sequence was non-linear.

Maury's designation of the c- characteristic was not accepted, however, for twelve more years. In 1922, it was finally reinstated in the Draper Classification by Commission 29 of the I.A.U. at its first general meeting in Rome. This was the first revision of the Draper System in more than a decade.<sup>160</sup>

We will leave our discussion of Hertzsprung's work at the point in late 1910 when it can be said that his early papers became

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finally recognised and appreciated. Beyond this point, Hertzsprung continued intensive work on clusters and binaries, but did not enter into discussions bearing directly upon evolution. At this point then, Russell becomes our central figure, and so, in order to provide a complete picture of his work, we return to the late 1890s.

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### References

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2.	F. McClean, Proc. R.S. <u>61</u> (1897), p.213.
3.	<u>Ibid.</u> , p.216.
4.	Ibid.
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6.	See: A.J. Meadows, Science and Controversy (MIT, 1972), p.282.
7.	McClean, Phil. Trans. Ser. A 191 (1898), p.129 ff.
8.	<u>Ibid</u> ., p.130.
9.	Maunder, <u>Observatory 21</u> (1898), p.163.
10.	<u>Ibid.</u> , p.164.
11.	Frost, <u>Ap. J. 10</u> (1899), p.367.
12.	W.H.S. Monck, Sidereal Messenger (Feb. 1889), p.62.
13.	W.H.S. Monck, Publ. Astr. Soc. Pacific 4 (1892), p.98; p.103.
14.	<u>Ibid.</u> , p.104.
15.	Letter, Kapteyn to Holden (28 August, 1892), Lick.
16.	See: A. Clerke, "The Distribution of the Stars", Astr. and
	<u>Astrophy. 12</u> (1893), p.517.
17.	<u>Op. cit</u> ., ref. 15.
18.	W.H.S. Monck, Astr. and Astrophy. 11 (1892), p.876.
19.	Ibid., p.878.
20.	W.H.S. Monck, Astr. and Astrophy. 12 (1893), p.11.
21.	J.E. Gore, The Worlds of Space (A.D. Innes, 1894), p.74.
22.	Ibid., p.75.
23.	<u>Ibid</u> ., pp. 78-79.
24.	W.H.S. Monck, J. Brit. Ast. Assn. 5 (1894-95), pp. 418-19.
25.	A. Nielsen, <u>Centaurus</u> 9 (1963), p.227.
26.	W.H.S. Monck, Pub. Astr. Soc. Pacific 9 (1897), p.124.
27.	<u>Ibid.</u> , p.125.

- 28. <u>Ibid.</u>, pp. 125-26.
- 29. <u>Ibid.</u>, p.126.
- 30. <u>Ibid</u>.
- 31. <u>Ibid.</u>, pp. 126-27.
- 32. <u>Ibid.</u>, p.127.
- 33. Frost (1898), p.320.
- 34. Monck, Op. cit., ref. 26, p.128.
- 35. W.H.S. Monck, <u>Ap. J.</u> 8 (1898), p.28.
- 36. <u>Ibid.</u>, p.30.
- 37. See: Basic Astronomical Data (Chicago, 1963), p.46.
- 38. Monck, <u>Op. cit.</u>, ref. 35, p.30.
- 39. <u>Ibid.</u>, pp. 30-31.
- 40. <u>Ibid.</u>, p.31.
- 41. W.H.S. Monck, <u>Stellar Astronomy</u> (1898), pp. 124-25.
- 42. <u>Ibid.</u>, pp. 79-80.
- 43. <u>Ibid.</u>, p.95 ff; p.157.
- 44. Frost (1898), p.ix.
- 45. Ibid., pp. 321-24.
- 46. <u>Ibid.</u>, p.321.
- 47. <u>Ibid.</u>, p.322.
- 48. <u>Ibid</u>.
- 49. <u>Ibid</u>.
- 50. See: Letters to/from Kapteyn and Gill in "Introduction to the Cape Photographic Durchmusterung", <u>Annals of the Cape</u> <u>Observatory III</u> (1896).
- 51. E.S. Holden did translate an abstract provided by Kapteyn in 1892. See: <u>Publ. Astr. Soc. Pacific 4</u> (1892), p.259.
- 52. Kapteyn, Astronomische Nachrichten # 3487 (1898).
- 53. Kapteyn, <u>Rep. Brit. Assn. Adv. of Sci.</u> (1905), p.257 ff;

repr. in H. Shapley, <u>Source Book in Astronomy 1900-1950</u> (Harvard, 1960), p.105.

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As a result of Kapteyn's early work on absorption of light in space, he and others came to consider other causes for the observed drop-off in number density of stars with increased distance from the Sun. From Kapteyn's observations one would have to conclude that the stellar density in the vicinity of the Sun was a maximum, which would place the Sun in a central position. It was more reasonable to search for some of this effect at least, in the form of interstellar absorption. In 1904, the available data was too poor for comment (Kapteyn, A. J. 24, 115, 1904), but later, in 1909, Kapteyn produced two papers (Ap. J. 29 (1909), pp. 46-54; 30 (1909), p.284) on selective absorption in space, wherein he not only confirmed his earlier view of the existence of general absorption, but furthered the idea in terms of the apparent redness of stars with smaller mean parallaxes. (For a full discussion of this work, see D. Seeley and R. Berendzen, JHA 3 (June, 1972), p.75). It might be noted here that after these initial papers, Kapteyn directed Adams to examine the spectra of stars of the same spectral class, but exhibiting different proper motions by photographing their spectra on the same plate under as identical conditions as possible. This technique eventually led Adams to find those spectroscopic criteria basic to his technique of spectroscopic parallaxes. Letter, Kapteyn to Frost (31 October, 1908), Yerkes. Letter, Kapteyn to Hale (17 February, 1907), Hale Papers. Lewis Boss, Astronomical Journal 26 (1908), p.31. Letter, Hale to Kapteyn (14 March, 1907), Hale Papers.

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- 59. Letter, Kapteyn to Hale (18 February, 1909), Hale Papers.
- 60. Letter, <u>Op. cit.</u>, ref. 55.
- 61. E. Frost and W.S. Adams, <u>Publ. Yerkes Obs.</u> (<u>Dicentennial</u> <u>Publ. II</u>) (1903), p.143.
- 62. Letter, Frost to Hale (18 December, 1908), Yerkes.
- 63. Letter, <u>Op. cit.</u>, ref. 59.
- 64. Ibid.
- 65. Kapteyn, <u>Ap. J. 31</u> (1910), pp. 258-69; p.260.
- 66. Letter, <u>Op. cit.</u>, ref. 59.
- 67. A.S. Eddington, <u>Stellar Movements and the Structure of the</u> <u>Universe</u> (Macmillan, 1914), p.156.
- 68. W.W. Campbell, <u>Ap. J.</u> <u>13</u> (1901), p.84.
- 69. Kapteyn, <u>Op. cit.</u>, ref. 52.
- 70. <u>Op. cit.</u>, ref. 59.
- 71. Letter, Hale to Kapteyn (23 March, 1909), Hale Papers.
- 72. Letter, V.M. Slipher to Frost (25 September, 1909), Yerkes.
- 73. Letter, Kapteyn to Hale (17 November, 1909), Hale Papers.

Hale had no objection. Within this same Kapteyn letter, a meeting with Gill is mentioned which was of extreme interest, not so much for evolution as for the interpretation of Kapteyn's two star streams:

> In London I was half a day with Gill and had great talks with him. He showed me a letter of Halm's in which H derives from his and Campbell's radial velocities, the conclusion that my star streams would not be rectilinear but somewhat curved. He thinks thus to have obtained evidence that the stars we see must have a spiral arrangement ...

Kapteyn asked to keep this confidential, but further mentioned that Gill would very much like to see photographs of the Andromeda nebulae and NGC 7078 (?), perhaps for comparison

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purposes. Kapteyn himself possibly did not understand the point at the time, since in a following letter, early in 1910, he indicated great interest in the Chamberlin/Moulton theory of planetesimal origin of the solar system. Kapteyn became interested in Darwin's theories of satellite development as a result. Little more than interest was expressed regarding the planetesimal theory; but with regard to Darwin's ideas, Kapteyn felt that Adams could make a definitive spectroscopic test:

It has lately struck me, that we may obtain considerable observational data in confirmation or refutation of Darwin's theory about the generation of satellites. If this theory is correct then as long as the satellite is nearly in contact with the mother-body, the rotation period of the latter must be nearly equal to the period of the satellite in its orbit, which orbit moreover must coincide very nearly with the plane of the equator - the distances increasing, the rotation period of the mother-body must even probably exceed the period of the satellite.

(Letter, Kapteyn to Hale (13 January, 1910) Hale Papers)

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Kapteyn felt that, for the Algol systems with known periods, the radial velocities of the components would give the dimensions of the system and, with the light-curve, the dimensions of the stellar components. From these data, one should be able to determine what the rotational velocity of each component must be, which could possibly be observed, Kapteyn thought, at the minimum light point during eclipse, when "... the light of the main body not cut off is mainly that of the right hand limb; a little time later it will be mainly the light of the left hand limb which we observe ..." Kapteyn then derived an expression relating the ratios of the diameters of the two stellar components to what would be expected to be the observed limb velocity due to rotation, after various orbital parameters had been identified. "As there are already known, I think, some 60 Algol variables, good part of which are bright enough at minimum for your instruments, there might be a pretty field of investigation here, at least if I do not overlook something ..." (Ibid.) This is an interesting discussion of the need to acquire an observational quantity that, even as long ago as 1898, was considered important by Monck, who, as we mentioned briefly before, considered that the rotational rates of Algol variables could be determined since line widths should change during eclipse, on the same Darwinian model. (Obs. 23  $\neq$  293 (June, 1900), p.255).

Hale, as usual, was quite supportive of this idea, and was glad to hear that Kapteyn was going to write directly to Darwin for suggestions. (Letter, Hale to Kapteyn (March 14, 1910), Hale Papers).

74. Letter, <u>Op. cit.</u>, ref. 59.

75. Letter, Frost to Kapteyn (24 February, 1910), Yerkes.

76. Kapteyn, <u>Op. cit.</u>, ref. 65, p.263.

77. <u>Op. cit.</u>, ref. 73.

78. Kapteyn was not sure of the actual number of spectroscopic binaries known to be members of the Hyades, and so sent off a hurried query to Frost in April, 1910. This was far too late to include in his paper, which appeared the same month. (Letter, Kapteyn to Frost (2 April, 1910), Yerkes).

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79. Kapteyn, <u>Op. cit.</u>, ref. 65, p.263.

80. Ibid.

81. Ibid., p.264.

82. Ibid.

- 83. F.W. Dyson, Proc. R.S. Edin. 29 pt. IV, p.390.
- 84. Eddington, Op. cit., ref. 67, pp. 155-56.
- 85. Kapteyn, <u>Op. cit.</u>, ref. 65, p.268.
- 86. In 1911 radial velocities for over 200 B stars; 1912 -212 A stars; 1913 - 915 stars of type F, G, K and M. (Cf. Malmquist "The Radial Velocities of the Stars" <u>Handbuch der</u> <u>Astrophysik VI</u> pt. 2 (1928), p.6).
- 87. Eddington, Op. cit., ref. 67, p.159.
- 88. <u>Op. cit.</u>, ref. 73.
- 89. J. Halm, Monthly Notices 71 (1909), p.634.
- 90. W.W. Campbell, <u>Stellar Motions</u> (Yale Univ. Press, 1913). Delivered as the Silliman Lectures at Yale, January 24 through February 4, 1910.

91. <u>Ibid.</u>, p.205.

92. Campbell was the first to identify this term, as a constant inserted into the equations of condition that were formulated to represent the observed radial velocity of a star, in terms of its own peculiar velocity, its position in space, and the spatial components of the solar motion. (R. Trumpler and H. Weaver, <u>Statistical Astronomy</u> (Dover, 1962), p.291 ff). More recently, this anomalous term has been interpreted by O.J. Eggen as a possible effect due to the "expansion" of a general system of B stars away from the galactic centre. (A. Underhill, <u>The Early</u> <u>Type Stars</u> (Reidel, 1966), pp. 123-24). Campbell also considered this in his book (p.203). It should be noted that the radial velocity values quoted above by Campbell represented <u>observed</u> values not corrected for the solar motion. When appropriate modern corrections are made (C.W. Allen, <u>Astrophysical</u> <u>Quantities</u>, (Univ. London, 1963), p.242) the B star <u>residual</u> velocity comes out as practically zero. While Campbell later discussed these types of velocities as "average residual velocities" his text was confusing in that both types were used without clear identification.

93. Campbell, <u>Op. cit.</u>, ref. 90, p.205.

94. <u>Ibid.</u>, p.206.

Campbell did mention Kapteyn's Ap. J. paper in his earlier Lick 95. Bulletin "Some Peculiarities in the Motions of the Stars" delivered before the American Philosophical Society in Philadelphia on April 21, 1911 (Lick Obs. Bull. # 196, 1911) though he did not provide a formal reference. Within this paper, too, he was most careful to review the fact that he had given his Silliman Lecture in late January that yielded the same discovery that Kapteyn's April paper professed to have found. Campbell claimed that within a fortnight of his Silliman lectures, he had the "... pleasure of showing these results, and of stating the general conclusion, to the chief investigators of stellar proper motions and radial velocities in this country ... " (Ibid., p.126). Doubtless one of these was Benjamin Boss and the other was his father, Lewis Boss, who had, by 1911, brought out the same conclusions based upon proper motions from their Preliminary General Catalogue, which were incorporated into Campbell's 1911 paper. 96. Campbell, Op. cit., ref. 90, pp. 207-208.

Campbell, <u>Astr. and Astrophysics 12</u> (1893), p.727; Ritchey, <u>Ap. J. 15</u> (1902), p.129.

98. Campbell, <u>Op. cit.</u>, ref. 95.

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99. <u>Ibid.</u>, p.129.

100. <u>Ibid.</u>, p.132.

Kapteyn's November, 1909, letter to Hale announcing that he 101. was going to rush his paper into publication, to keep Campbell from publishing first, indicates that Kapteyn knew that Campbell was working on the same idea prior to 1910. This was not indicated by Campbell in Stellar Motions, as we have seen. Further, regarding the Crossley evaluation, letters between Hale and Campbell during this period (as examined in the Lick Archives) show that Hale was anxious for Campbell to delay further tests of the Crossley until the 60-inch was put into better shape, and that Kapteyn's analysis of the limiting magnitude of a one hour exposure with the Crossley was incorrect. (Letters, Hale to Campbell (October-November, 1909), Lick). In correspondence thus far examined, no mention of Kapteyn's 1909 work on radial velocities and spectral types was made by Hale or Frost to Campbell. The Hale Microfilm collection has not yet been examined fully on this point, however. The significance of this issue goes beyond simple comparative tests, for during this period, Lowell was using generally ill-defined criteria to show that his Flagstaff site was most suitable for efficient telescope work (both direct and spectroscopic) and that, in effect, his 24-inch could see more than Lick's 36-inch or Yerkes' 40-inch refractors. Thus, we can well understand why so many comparative tests were being made at the time, not only for Kapteyn's selected area plans, but in order to obtain definitive data which could answer Lowell.

102. Campbell, <u>Op. cit.</u>, ref. 90, p.255.

Ibid., pp. 256-60. Campbell tabulated double entry tables which showed this observation to be true. These tables illustrated that the frequency of short period binaries went to zero with advancing spectral type, and conversely, the frequency of long period binaries tended to rise with advancing spectral type. Unfortunately, the analysis of a spectroscopic binary system does not allow for the unambiguous determination of stellar mass, since the orbital inclination is not generally known, unless the system is also an eclipsing one. To separate out the mass effect upon period, Campbell discussed statistical methods for assuming various "mean" orbital inclinations, and how deviations from assumed inclination values might affect mass results and observed radial velocity values. Even though he went through this preliminary work, which would be expected to result in some statement about the effect of average masses upon observed periods, little was concluded. He did provide the needed information in various tables however, which gave values for asini which is the projected semi-major axis of the relative orbit of the system. Each table isolated spectroscopic binaries into spectral ranges, arranged in each table by length

of period.

104. <u>Ibid.</u>, p.267.

105. Ibid., p.269.

106. Ibid.

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107. W.W. Campbell, "The Evolution of the Stars and the Formation of the Earth" <u>Scientific Monthly</u> (October, November, December, 1915); <u>Popular Science Monthly</u> (September, 1915). Lectures given December 7 and 8, 1914.

108. Eddington, <u>Op. cit.</u>, ref. 67, p.168.

- 109. Ibid., pp. 168-69.
- 110. <u>Ibid.</u>, p.169.
- 111. K. Lundmark, <u>Handbuch der Astrophysik V</u> pt. 1 (Berlin, 1933), pp. 437-41.
- 112. D. Hoffleit, Popular Astronomy 58 (November, December, 1950), # 9; # 10; 59 (January, 1951), # 1; Harvard Reprint, # 342.
- 113. A Nielsen, <u>Centaurus 9</u> (1963), pp. 219 ff.
- 114. D.B. Herrmann, Ostwalds Klassiker # 255 (Leipzig, 1976).
- 115. <u>Ibid.</u>, p.83.
- 116. E. Hertzsprung, Zeit. für wiss. Photographie iv (1906), p.43, footnote p.53.
- 117. E. Hertzsprung, Zeit. für wiss. Photographie iii (1905), p.449.
- 118. A. Maury, <u>Harvard Annals 28 pt. 1 (1897)</u>, p.4.
- 119. Cf. Herrmann, Op. cit., ref. 114, p.45. The original read:

Die Hypothese der kollateralen Serien ist geeignet, die Haupterscheinung zu erklären, dass unter den Sternen, welche uns heller als  $m_H = 5$  erscheinen, die cund Orion-Sterne am hellsten leuchten und unter den übrigen nicht die roten, sondern die gelben am schwächsten. Die Serien sollten etwa durch die.

- 120. Hoffleit, Op. cit., ref. 112, p.17 (Harvard Reprint).
- 121. Herrmann, <u>Op. cit.</u>, ref. 114, pp. 48-49; See: Hertzsprung (1907), "Tabelle 6a".
- 122. Ibid., p.37; See: Hertzsprung (1905):

Wenn die c- und ac- Sterne summarisch betrachtet werden, sicht man, dass mit wachsender Klassennummer der c- Charakter abnimmt und dass diese Sterne gerade da aufhören, wo die hellen K-Sterne anfangen.

123. I am indebted to Dr. Hoffleit for pointing this out to me in discussion, December 4, 1976.

124. Cf. Herrmann, Op. cit., ref. 114, p.36.

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- 125. Lundmark, Op. cit., ref. 111, p.440.
- 126. Letter, Hertzsprung to E.C. Pickering (March 15, 1906), Harvard.
- 127. Herrmann, <u>Op. cit.</u>, ref. 114, pp. 47-49; Hertzsprung (1907),

Table 6a.

128. <u>Ibid.</u>, p.52; Hertzsprung (1907):

Wir könnon jetzt annehman, dass die hellen roten Sterne ( $\alpha$  Bootis,  $\alpha$  Tauri,  $\alpha$  Orionis usw.) pro Volumeneinheit des Weltraums selten sind und die, welche der normalen Sonnenserie angehören, bei weitem die Mehrzahl bilden. Das helle rote Stadium wird deshalb wahrscheinlich entweder relativ schnell durchlaufen, oder die Sterne, welche sich darin befinden, gehören einer Kollateralserie an. Ob zwischen einer eventuellen Kollateralserie und der Sonnenserie Zwischenreihen oder eine Kluft besteht, bleibt noch zu entscheiden.

129. Nielsen, <u>Op. cit.</u>, ref. 113, p.235; Lundmark, <u>Op. cit.</u>, ref. 111, p.437.

130. Hoffleit, <u>Op. cit.</u>, ref. 112, p.18.

131.

Nielsen, <u>Op. cit</u>., ref. 113, p.236. According to Nielsen, Karl Schwarzschild, at the time director of the Observatory at Göttingen, was keenly aware of Hertzsprung's work, and appreciated its importance. In conversation with Hertzsprung, Nielsen found that Hertzsprung and Schwarzschild began an extensive correspondence at the time, which ended in Hertzsprung's being offered a post at Göttingen in April, 1909. Within the year, however, Schwarzschild and Hertzsprung left for Potsdam. Thus, in less than seven months, as Nielsen observed, "... Hertzsprung had a move-up from being a private astronomer in Copenhagen to Senior Astronomer at one of the largest observatories in Europe ..."

132. Personal communication, February, 1977.

133. Kapteyn, <u>Op. cit.</u>, ref. 65.

- 134. Herrmann, <u>Op. cit.</u>, ref. 114, p.67.
- 135. <u>Astronomische Nachrichten 186</u> (1911), p.71; Hertzsprung, <u>Pots. Publ. 22</u> (1911), p.1; Cf. Neilsen, <u>Op. cit.</u>, ref. 113, pp. 237-38.
- 136. Nielsen, <u>Op. cit.</u>, ref. 113, p.241.
- 137. Hertzsprung, <u>Op. cit.</u>, ref. 116. See: D.H. DeVorkin, <u>J. Hist.</u> <u>Astr. VI</u> (1975), p.5.
- 138. R.H. Curtiss, <u>Handbuch der Astrophysik V</u> (Berlin, 1931), p.35.
- Letter, Hertzsprung to Pickering (22 July, 1908), Harvard;
   partially reprinted in Jones and Boyd, <u>Harvard College</u>
   <u>Observatory</u> (Harvard, 1971), p.240.
- 140. Letter, E.C. Pickering to Hertzsprung (4 August, 1908), Harvard.
- 141. <u>Harvard Annals 28 pt. II (1901)</u>, p.138.
- 142. DeVorkin, Op. cit., ref. 137, pp. 5-6.
- 143. Letter, Hertzsprung to Pickering (17 August, 1908), Harvard.Letter written from Copenhagen.
- 144. Letter, Hertzsprung to K. Schwarzschild (26 August, 1908), Schwarzschild Microfilm Collection, American Institute of Physics. The original read:

Uber die Wegwerfung des c- Eigentumlichkeiten in H.R. habe ich Pickering ein Paar Worte geschrieben. Seine Antwort lege ich zür Dürchsicht anbei...

Nächst das Fehlen des c- Eigentümlichkeiten ärger ich mich bei des H.R. - Spektralklassifikation darüber, dass es nicht zü sehen ist, wenn die Spektralahgabe dem Draper Catalogue entnommen. Dadürch wird die ganze Klassifikation inhomogen ...

145. Letter, Hertzsprung to Pickering (29 October, 1908), Harvard.
146. Pickering, <u>Harvard Circular 146</u> (1909), p.3. This circular,
"The Distribution of the Stars", was based upon photometric data from the Revised Harvard Photometry, <u>H.A. 50</u> - the same

volume referred to by Hertzsprung. The circular discussed the fact that stars of spectral classes A and F seemed to have a different distribution than did stars within the range G, K and M. In fact, while the former group tended to follow the theoretical distribution (with distance) expected for uniform space without absorption, the latter behaved as if absorption existed. After discussing possible instrumental errors, which were believed to be small with respect to the differences found, they reached the conclusion that "It is difficult to understand how any absorption could affect unequally the number of stars whose spectra are unlike ... " It was assumed that a selectively absorbing medium would absorb as a function of distance, and rays of the same effective wavelength would be similarly affected in stars of different spectral classes. "It would not have any effect on the relative number of stars having different spectra if, as in the Draper Catalogue, the measures of brightness were all made of rays of the same wavelength ..."

While this was a report in progress, as was intended for the Harvard Circular series, reaction to it by H.H. Turner pointed to the possibility that it was a luminosity effect:

> I was immensely interested in your Circular 147 ... Certainly we cannot have the light of stars of one type of spectrum being absorbed or scattered while that of another type is not. Of course we could have one type <u>more</u> influenced than another if it is on the whole further away; i.e. if it is intrinsically brighter. (Letter, H.H.Turner to E.C.Pickering (17 February,

(Letter, H.H.Turner to E.C.Pickering (17 February, 1909), Harvard).

Turner, at the University Observatory, Oxford, had been interested in similar distribution problems, along with F. Dyson, and had recently published a discussion on the visual loss (in magnitudes) as a function of distance based upon various possible absorbing and scattering media (<u>Monthly</u> <u>Notices 69</u> (1908), p.63). Turner concluded from Pickering's work that, from his expected values of visual loss as a function of magnitude range and distance, the A and F stars had to be closer than the G, K and M types "... about 6 times as close as the others & therefore about 36 times intrinsically fainter. This seems unlikely, but is not impossible". We shall see in the next chapter that Pickering too was quite aware of this possible effect at the time, prior to Turner's letter.

Turner also felt that results for B stars were "remarkable". He had studied their distribution, which was known to be limited to the plane of the Galaxy, but from this most recent work, he found the distribution to be "... really in one dimension: i.e. <u>a long stream of stars</u> ..." This is quite interesting today, based upon our present picture that stars of class B are highly confined to spiral arms. As Turner was referring to brighter stars, he might have been examining the stars in "Gould's Belt" identified in 1879 by Benjamin A.Gould. If fainter stars were included, then the distribution would have been confined to the Galactic Plane, and not inclined by some 16 degrees, as was Gould's belt. (Cf. McCuskey, "Galactic Structure" <u>Stars and Stellar</u> Systems V (Chicago, 1965), pp. 4-5).

An important element in Turner's work was the observation that, with increased photographic exposure time, the expected increase in the number of stars recorded was not met, which was a well known result, and the basis of speculation on the existence of absorption or the finiteness of the universe. In a parenthetic statement within the text of his paper, however, he indicated that since the writing of the paper he had become aware of the work of C.E. Kenneth Mees and S.E. Sheppard, which showed "... that the law of equivalence and intensity begins to break down when a certain exposure is reached ..." (Turner, "Diminution of light in its Passage through Interstellar Space" <u>NN 69</u> (November, 1908), p.65). This we know today as reciprocity failure. Its true significance was not totally appreciated by Turner, who noted that from Mees' work, the law of equivalence broke down at exposure times of about 15 minutes duration, which he thought meant that his 40-minute and greater exposure times were therefore not seriously affected.

In final sum regarding Pickering's role here, if he had actually followed some of Hertzsprung's arguments in his 1905 and 1907 papers, he would have seen that the same mixing was occurring for the later type stars. There is no evidence that he did, since all distribution studies at the time were following Kapteyn's lead in his search for interstellar absorption.

- 147. Letter, Frost to E.C. Pickering (21 June, 1910); Letter, Kapteyn to Frost (28 May, 1910), Yerkes.
- 148. Letters, Pickering to Frost (2 July, 1910), Yerkes; to W.W. Campbell (2 July, 1910), Harvard.
- 149. We will further examine this point in our ultimate chapter, on the 1910 International Solar Union Conference meeting.
  (Chapter 5, p. 393).

- 150. Letter, Lewis Boss to H.N. Russell (23 August, 1910), Princeton.
- 151. Nielsen, <u>Op. cit.</u>, ref. 113, p.241.
- 152. H.N. Russell, "Some Hints on the Order of Stellar Evolution", Science 32 (December 16, 1910), pp. 883-84.
- 153. H.N. Russell, "On the Distances of Red Stars", Abstr. Science 31 (June 3, 1910), p.878.
- 154. Nielsen, <u>Op. cit.</u>, ref. 113, p.241.
- Letter, Russell to Hertzsprung (27 September, 1910); Cf.
   Nielsen, <u>Op. cit.</u>, ref. 113, p.244.
- 156. Letter, Hertzsprung to Russell (11 October, 1910), Princeton.
- 157. <u>Op. cit.</u>, ref. 116.
- 158. Letter, E.C. Pickering to K. Schwarzschild (18 November, 1910), Harvard.
- 159. Diary, E.C. Pickering, 1911, Harvard. Item HUG 1690-12.
- 160. Jones and Boyd, <u>Op. cit.</u>, ref. 139, p.242.

### CHAPTER 4

## The Early Work of Henry Norris Russell

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#### CHAPTER 4

### The Early Work of Henry Norris Russell

#### Introduction

Russell's work during the period 1899 to 1910 reflected his interests while a student at Princeton, where he graduated with highest honours in 1897, and received his doctorate in 1900 with a thesis on the perturbations by Mars upon the orbit of the minor planet Eros. His interest in <u>Eros</u>, and, indeed, his main early interest, lay in gravitational theory, applied to the orbits of binary stars. His first scientific paper, "A New Graphical Method of Determining the Elements of a Double-Star Orbit",<sup>1</sup> was completed in March, 1898, and was an independent exposition of a method derived at the same time by H.J. Zwiers of Leiden. Russell acknowledged Zwier's anticipation of his technique, and made no claims to priority.<sup>2</sup>

'Russell's most significant work prior to 1900 was a short study of the densities of Algol variables.<sup>3</sup> He apparently conceived of the problem, and worked out its major details while on summer vacation in 1899. By September, he had written to his teacher, C.A. Young, about his ideas, and Young replied with interest and aid, couching his discussion in the professorial manner:

> As to the Algol density matter, the curious way in which <u>linear</u> dimensions drop out in many cases of motion of spherical bodies moving around each other, leaving <u>density</u> & <u>time</u> alone concerned was pointed out by Maxwell long ago. If I remember rightly he proposed to derive the unit of <u>time</u> from the period of a particle revolving around a sphere of <u>density</u> of water close to its surface - the period being the same (if I remember rightly) for spheres of any diameter whatever. You will find also in my <u>General Astronomy</u>, note to Art. 279, p.188 an expression for the <u>Sun's</u> density (independent of the solar parallax) which is closely analogous to the formula

# $\rho = \frac{4K}{t^2 \sin^3 \pi} \left( \frac{d}{t} \right)$

by putting K = (in the note) =  $\pi^2 r/g$ , r being the radius of the earth; t, of course being reckoned in <u>seconds</u>, since g is a velocity per second. But your application of the principle is new and well worth printing.<sup>4</sup>

Within this letter, Young also mentioned that Russell's proposed thesis on <u>Eros</u> would be a difficult one, but one well worth the effort. At the time, of course, astronomers all over the world were organising to use Eros to determine the solar parallax. In the following years, Russell would be closely involved in the project, with A.R. Hinks at Cambridge.

Russell's Algol paper was dated October 9, eight days after he returned to Princeton for the Fall semester. The text of his work shows that Russell derived a relation for the upper limit of density based upon geometrical conditions of the orbit, the sum of the radii of the two stars, and the observed light curve, that closely resembled the equation provided by Young. Without recovering Russell's original letter to Young, however, we can only assume that Russell had already worked out the relationship and merely reported it to Young.<sup>5</sup>

Russell examined seventeen systems of the Algol type and found that the range in density of the stars was between .7 and 0.035 grams/ cubic centimetre, with a mean of about one-fourth the solar value of 1.41. Russell concluded:

> Notwithstanding the causes of uncertainty, it is evident that the Algol-variables as a class are much less dense than the Sun, probably less than one fourth as dense. If these stars consist of a nucleus and an extensive luminous atmosphere, the nuclei may, of course, be much denser.<sup>6</sup>

As with his first paper on visual binaries, Russell's derivation was anticipated, though by only a few months, by Alexander Roberts from South Africa.<sup>7</sup> By 1900, Russell had completed his thesis, but in that year became ill and did not return to Princeton until late in 1901.<sup>8</sup>

In early 1902, his attention again turned to binaries and, by May, he published "An Improved Method of calculating the orbit of a spectroscopic binary".<sup>9</sup>

His stated intention in this work was to provide an extended analytical method by which orbits of both small and large eccentricity might be determined.<sup>10</sup>

His analytical procedure allowed for greater flexibility in the analysis of various orbital characteristics. Of great interest to Russell was the effect of tides, as suggested by Darwin's work. In May, 1902, Russell wrote to Campbell about the orbital characteristics of  $\zeta$ Geminorum, which by Russell's theory should turn out to be a stable tidal system. Russell hoped that Campbell would take a refined set of spectroscopic observations to help settle the question and test Russell's method of orbit determination. This system was of great interest because it also underwent light variations, which Russell commented upon:

> As for the light variation, I think it can be accounted for on a hypothesis like that proposed by Dr. Johnstone Stoney for the cluster variables: assuming that the bright star has a period of free vibration nearly equal to that of the forced tidal oscillations. This will explain the greater magnitude of the variation, in proportion to the eccentricity, and also the phase reversal in this star, as compared with  $\eta$  Aquilae and  $\delta$  Cephei.<sup>11</sup>

Russell's intuition was correct here, since today the brighter component of the system (ADS 5742 A) is classed as a Cepheid variable. At the time, of course, the pulsation mechanism was not generally considered as the cause for Cepheid light-variation.

Russell's letter to Campbell continued to discuss tides and tidal effects in binary systems. He was most concerned with a solution to the  $\zeta$  Gem system, which he felt was "... an actual example of the periodic solutions of the problem of three bodies which have been discussed by Darwin and Poincare ... "Russell then concluded:

I hope to spend next winter in Cambridge under Professor Darwin, and, if the problem is too much for me with my present methods, I may attack it again then.

By the autumn term, Russell was in Cambridge and did attend Darwin's lectures, as a postdoctoral student at King's College. In the summer prior to Russell's departure for England, only one scrap of correspondence between Darwin and Young has been uncovered, and nothing was said of Russell. Darwin only mentioned in July that he was unable to provide Young with a simple, succinct statement (for popular publication) describing his theory of tides.

### Russell's Association with A.R. Hinks

In November, 1902, A.R. Hinks, who had gained a focal point position in the Eros campaign as the person responsible for overseeing the position reductions of Eros, found himself in a minor battle with Jacoby of Columbia University over the proper mode of reduction. Briefly, Hinks' reseau method (the photographic placement of a grid on the plate prior to exposure to the astronomical field) was contested by Jacoby, who refused to cooperate.<sup>12</sup>

The apparent problem was that Hinks was unable to convince others of the stability of his method. By February, 1903, he found some help in the form of Russell, who had attended his lectures the previous Fall. At first, though, Hinks wanted Russell to take over his other interests, which had been neglected since the Eros project had been taken on:

> ... we have here just now an exciting cause in the shape of H.N. Russell of Princeton, who wants to spend some time here on the photographic work, and

is as you know very fond of binaries. I don't think there can be any harm in telling you that we have applied to the Carnegie Institution for a research assistantship for Russell: and if we get it he is going to put in his time at stellar parallaxes. He is a first rate man, and we have here a first rate equipment laying half idle because my hands are full with Solar Parallaxes. So, we are earnestly praying that the Carnegie Trustees may think we are poor but deserving.<sup>13</sup>

This letter, written by Hinks to Hale in February, 1903, was also sent, in paraphrased form, to Campbell. Hinks was well aware that Hale had received support for parallax work (to be done by Frank Schlesinger), and so proposed cooperation. To Campbell, Hinks emphasised Russell's interests in binary stars, and indicated that a representative number would be added to the parallax lists. By April, Russell had been awarded the Carnegie postdoctoral assistantship, and Hinks had received favourable comments about his reseau technique from Hale. To Hale, Hinks replied:

> ... I might venture to prophecy that if Schlesinger used the two methods side by side for a month ... he would see the ease of use of the reseau.<sup>14</sup>

Schlesinger's technique involved the use of field stars for the establishment of position.<sup>15</sup> Today, the reseau has been replaced, due to its own intrinsic error, by direct measurement techniques with long screws. The accuracy attainable simply became far greater than the limiting accuracy of the reseau.<sup>16</sup>

Russell's contributions to Hinks' work came quickly. In the April letter to Hale, Hinks added:

Russell with his superabundant energy has been investigating the theoretical basis of the <u>linear reduction</u> formulae in photography and has got out some pretty results which will very much simplify the procedure.<sup>17</sup>

Hinks wrote the same lines simultaneously to Campbell. To both, it was evident that Russell's work was bolstering Hinks' argument for the reseau. By June, Hinks wrote to Hale and Schlesinger announcing that he and Russell were about to publish their "... manifesto on the subject of stellar parallaxes by photography ...,"<sup>18</sup> in the <u>Astronomical</u> <u>Journal</u> and asked for criticisms and suggestions.

This proposed paper was delayed, and finally published in 1905.

Later in 1904, in August, Russell gave a short paper "On the Masses of the Stars" at a subsection meeting of the BAAS at Cambridge. B. Cogen has noted that it was here that Russell and Lockyer met "... when they both presented papers at the same session of the meeting ....<sup>19</sup>

While they certainly did give papers in the same subsection, in fact Lockyer's was on Friday, August 19 ("The Temperature of the Stars" Title only paper  $\neq$  2) and Russell's on the following Monday, August 22. Thus, while it remains tantalisingly probable that they met, the question is still open. It is interesting to note that in Lockyer's session, A.L. Cortie reviewed Fowler's recent TiO identifications in Antarian stars, and noted that Lockyer believed these lines to also exist as intensified features in the spectrum of Arcturus. It is unfortunate that Lockyer's paper, and Russell's paper, were not printed.

In the month following, Russell became ill, experiencing a breakdown from typhoid contracted while on a visit home. Hinks therefore carried on with the observations, while Russell tended to the measurements and reductions. In June, 1905, they had finished their introductory paper: "Determinations of Stellar Parallax from Photographs made at the Cambridge Observatory".

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This first paper did not deal with results; it discussed methodology and technique. We will review the paper in outline.

In the project, two objects were kept in mind, which indicated the nature of the study:

- 1. To achieve the most efficient balance between expenditure of energy and accuracy of results.
- 2.
- To eliminate "at any cost" systematic error.

To achieve these goals, the following rules were adopted:

1. To take separate plates at each epoch and develop them at once.

This was in contrast to Kapteyn's plan of allowing all epoch exposures to be placed upon the same photographic plate before it was processed. Each plate of the Cambridge project was to be impressed with a reseau.

## 2. All stars brighter than 6<sup>m</sup>.0 are to be photographed with a colour screen.

This screen, actually a thin filter of small size, was to be placed over the position of the bright star to reduce its magnitude to within the range of comparison stars. In this manner, many of the "... most interesting ..." stars could still be included on the programme. We will be examining what criteria were used to select stars which were "interesting".

## 3. All photographs must be taken within half an hour of the meridian.

This rule has since become standard operating procedure, even though it greatly limits observing time scheduling, and must carefully be compared to the importance of obtaining maximum parallax factors. The need for the meridian restriction was to minimise differential flexure of the instrument (primarily the Sheepshanks polar mirror) and errors due to atmospheric refraction. It was found that accidental errors in measurement were very few and small, "... so that it does not pay to make many settings on a single image ..."<sup>21</sup> Two, or more when differences appeared, were considered sufficient. Furthermore:

4. Measure only two of the four exposures on a plate in the direct orientation, and the other two in the reversed.

This decision was later to draw some criticism from various quarters.

Another cut was to limit greatly the number of measurements in the 'y' direction, since, as the plates were all taken close to the meridian, this would generally be perpendicular to the parallactic motion of the stars. This was stated as Rule 5.

Their final rule was for reduction procedures, and was another substantial cut in effort:

> 6. Choose any plate, or the mean of any number of plates, as a standard, and reduce the others to this.

This rule introduced the concept of relative parallax, as opposed to the absolute parallax of a star, determined against an absolute frame of reference produced by meridian circle work. Of course, the authors did not wish to leave the relative parallaxes as such in final form. Russell was to examine later the theoretical common proper motions of his reference stars, based upon Kapteyn's statistical studies of proper motion as a function of brightness and spectral type, and derive a correction to relative parallaxes to the 'absolute frame'.

An important element in the reduction procedure was to be sure that comparison stars were equally distributed across the field, and that the parallax star was at the centre of gravity of the comparison field. If these two conditions were met, then a simplified reduction procedure could be used, not requiring least squares fitting of equations of condition. The authors noted that this simplified approximation was first proposed by Dyson, but the history of the technique has not been followed up as yet. Suffice it to say that, in spirit, these

approximations found their ultimate simplification in Schlesinger's three-comparison star method of "dependences", which he evolved between 1910 and 1926.

Finally, the authors discussed the criteria that led to the compilation of their working list. First, they noted that a general list, selected at random, was likely to provide spurious results, which, since this study had the nature of a pilot programme, was to be avoided. Thus, two selection criteria were used:

A. Stars with known parallaxes; visual binaries where the parallax would yield masses; variable stars; common proper motion stars; star clusters; nebulae.

Β.

Classes of stars likely to yield large parallaxes - bright stars and especially large proper motion stars.

Criterion A obviously contained objects of particular interest. One interesting outgrowth of the attention to visual binaries, in addition to mass and luminosity ranges, was the fact that these orbital pairs must yield similar parallaxes, and, hence, make a good test for spurious determinations. Part B was apparently the more objective of the two (especially the latter part). The authors noted that stars of spectral types I and III were of especial concern. It is interesting to note that they did not emphasise stars of type II (of the solar class) which were believed to be the closest - as a statistical group by Monck and to some extent by Kapteyn and others. Lockyer, however, did not hold to this distribution concept. The inclusion of bright stars also bears notice.

It is natural to wonder how this list actually came into being. Even though no thorough study of this has yet been made, a comparison of their parallax list<sup>22</sup> with Lockyer's "Catalogue of Four Hundred and Seventy of the Brighter Stars"<sup>23</sup> shows that of the 55 stars on the

parallax programme, 21 were common to Lockyer's catalogue. A breakdown follows:

### Argonian - 0

### Alnitamian - 0

Crucian	- 0	Achernian	- 0
Taurian	- 1	Algolian	- 1
Rigelian	- 0	Markabian	- 0
Cygnian	- 0	80) an 81) th 61) an 61) an	
التي الله التي التي التي التي التي		Sirian	- 4
Polarian	- 2	Procyonian	- 3
Aldebarian	- 0	Arcturian	- 5
Antarian	- 5	Piscian	~ 0

From this compilation of the 21 stars found on Lockyer's lists which were studied by Hinks and Russell for parallaxes, we see clearly an even distribution amongst those stars most likely to show luminosity differences that would place them on the rising or falling temperature branches. This certainly does not prove anything, but demonstrates that Russell was capable of testing Lockyer's hypothesis, if he chose to do so.

I have not examined how many of Lockyer's stars <u>could</u> have been included, from their criteria, and do not feel that it would provide more than circumstantial evidence at this time, unless further archival study allows a further insight into the criteria for inclusion.

At the appropriate chronological point I will examine how these 21 stars fared for parallax and luminosity.

Immediately succeeding the paper by Hinks and Russell was a detailed analysis of two stars on the programme by Russell.<sup>24</sup> The first, Lalande 21185, was a star already studied by Winnecke, Kapteyn, and Flint, but with large discrepancies between the three. The second,

 $\gamma$  Virginis, is a well known visual binary of Draper Class F, and is found on Lockyer's lists. He found for  $\gamma$  Virginis that the parallaxes for both components agreed to within their mutual probable errors (which were between 30% and 50% of the parallax values themselves). A mean taken, assuming equal weights for both, gave a parallax fully three times its own probable error.

Using Kapteyn's definition of absolute magnitude (magnitude of stars at a distance of 10 parsecs or 0".1 of arc - the modern definition), Russell found values for both stars studied on his programme. For Lalande 21185, the absolute magnitude came out to be +10.0, or about 1/100 that of the Sun. As no spectrum was provided at the time, no further discussion was made.

 $\gamma$  Virginis, however, was quite another matter. Here, from its parallax, the absolute magnitude of the two stars together was +2.4, and, assuming that the two components were equal in brightness (which is a good approximation), their individual absolute magnitudes came out to be 3.2, or about nine times that of the Sun. From elements for the binary orbit derived by T.J.J. See, the mass of the system, with the parallax, came out to be 3.3 times the Sun's mass, and, again assuming equality among the components, they come out to be about 1.6 the mass of the Sun. Their slightly greater masses, but much greater luminosities, caused Russell to state:

> These stars must therefore be either less dense than the Sun or have a greater surface brightness, which accords well with the fact that their spectra are of the first type.<sup>25</sup>

This represents most certainly an awareness of evolution, and a concern for interpretation in terms of evolutionary direction. This star system was classed as Sirian by Lockyer, XI ab by Maury, F in the DC, Ia2 and Ia3 by Vogel, and III by McClean. Unfortunately, in this case, lower density does not distinguish between Lockyer's and the classical view of evolution. It must be noted from the above quotation, that Russell did not state whether surface brightness or density were dominant, since Type I stars were both bluer (higher surface brightness) and supposedly less dense.

After Russell's illness, which began in September, 1904, Hinks carried on the observations, and some time in 1905, presumably after they had finished the drafts of their papers for the <u>MN</u> in June, Russell returned to the United States and Princeton.

### Return to Princeton

Once back at Princeton, Russell accepted a post on the faculty as an instructor and began the long task of reducing the data on the 55 stars of the parallax programme. From 1906 to 1909, correspondence between Hinks and Russell examined at Princeton yields little save for conversations on the limitations of the parallactic technique, and Hinks' interest in proper motions for use in studies of the structure of the nearby stellar realm.

During 1906, Russell's parallax reductions included Mira (o Ceti), two double star systems ( $\eta$  Cassiopeiae, Groombridge 34), and suspected high-velocity stars. In late 1906, these studies saw publication in the <u>MN</u>, without detailed elaboration,<sup>26</sup> but with explicit attention not only to the determination of absolute magnitudes (on Kapteyn's scale), but to intrinsic "light" output, in terms of the Sun. His listing showed stellar luminosities ranging between 23 and .0015 times the solar output. For two bright stars,  $\beta$  and  $\eta$  Cas, which are of similar spectral class (F5 and F8), Russell found that their brightnesses were 23 and 1.5 times that of the Sun, respectively. These spectral classes were not noted by Russell, neither did he comment

upon the great differences in brightness. Modern (Yale Bright Star Catalogue) spectral classifications are F2IV and GOV, respectively, which shows a greater class separation, and, more significantly, a separation into separate MK luminosity classes.

Russell did not discuss any of this, of course, and was most interested in the motion of Mira, and the mass values for the binaries on the list. He supplemented some of the photographic work at Cambridge with additional observations at the Halstead Observatory at Princeton with their 23-inch refractor. (Unfortunately, this telescope is now dismantled and in storage at the Naval Observatory at Flagstaff, Arizona.)

Another developing interest at the time was the use of stellar spectra in his reductions. Russell had long been familiar with classification systems. Young saw to it that his mathematically oriented student was given some practical experience while an undergraduate. He gave him, for a senior project in 1897, the task of observing the visual spectra of some 30 stars, and classifying them according to any desired system. The system chosen by Russell had four major classes: Sirian, Solar, Banded, Bright-line, labelled I, II, III and IV.

By 1907, as a new instructor at Princeton teaching the senior course in practical astronomy that Young had created, Russell generated a series of lecture notes that happily have been preserved. His notes for March 14, 1907, on types of stellar spectra, followed Secchi's classification closely, comparing it with the Draper System. After reviewing the characteristics of spectra, and their distribution in space (providing nothing more than a discussion of relative degrees of confinement to the Milky Way), he ordered them according to the standard Draper order, and then commented:

It is therefore extremely probable that these types represent different stages in the history of a star probably intimately related to its temperature but also to other factors mass, density, surface gravity.<sup>27</sup>

Russell added that "... It is not certain which stars are the youngest: A cool star may be getting hotter ('young') or colder ('old') ... " He unfortunately left his cryptic remark stand, and then progressed to a discussion of radiation laws and how one might derive stellar temperatures from Stefan's law or Planck's law. He cautioned that atmospheric masking might very well lead one astray, causing a hot star to appear red, but then indicated that "... we have a second line of attack ... " from the direction of an analysis of the change in laboratory line spectra in sources at different known temperatures, along the lines of the Mount Wilson sunspot work of Hale, Adams and Gale, and the work of Lockyer, "... who was the pioneer in this subject ..."

Immediately following his discussion of temperature determination, he reviewed stellar evolution, and began:

Because one star is hotter or cooler than another it is not certain that it is older or younger. The course of temperature may be something like this



or like this



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We cannot be sure at present though some things look as if the first hypothesis is correct.

Not only was Russell favourable to Lockyer's scheme, as we see from his 1907 lecture notes, but also we see in the second relationship, which presumably represented Secchi's classification, a line of decreasing temperature vaguely like one might expect to see when associating brightness with colour - the basic ingredient in the HR Diagram.

Russell did not elaborate upon this, but concluded his section on evolution by remarking that spectra revealed only surface temperatures, and not internal values, and that -

> The great conclusion to be drawn from the whole subject ... is that the whole visible universe seems to be made up of the same materials under the same laws and undergoing the same processes of development.

No other proof of the uniformity of nature is anything like as impressive as this ...

Russell did not explain his bias towards a temperature scheme like Lockyer's, but evidently it came from secondary sources, possibly Lockyer's books, or Young's review of the state of the Lane/Ritter theory in 1899-1900. The latter possibility stems from the fact that in 1901, Russell wrote a manuscript entitled "The cooling of a perfectly gaseous star" which was left unfinished, since, during the course of the derivation, he decided that he had been "anticipated" by others.<sup>28</sup>

In order to reduce his parallax stars from relative to absolute values, Russell needed the brightnesses and spectra of the comparison stars. He turned to the best source available.
## Pickering's Aid

During early 1908, Russell met E.C. Pickering at a meeting of the American Astronomical Society. On April 4, Russell wrote to Pickering, recounting their meeting, and expressed a wish to visit the Harvard College Observatory.<sup>29</sup> It may have been this meeting, which occurred in the following week, that is referred to by Jones and Boyd:

> Henry Norris Russell, too, bore witness to Pickering's gracious welcome to the beginner and to many succeeding acts of generosity. Shortly after his first interview with Pickering, in which he mentioned his initial astronomical work on stellar distances, he received a letter suggesting the usefulness of determining the magnitudes and spectral types of his stars, and offering to have the work done at Harvard.<sup>30</sup>

After a general search for such correspondence at both Princeton and Harvard, these letters did not appear. As Jones and Boyd referred immediately to Russell's obituary of Pickering, after the quoted discussion above, it is possible that the entire source derived from Russell's 1919 recollection:

> If a more personal allusion may be excused, it may be recorded that, shortly after the writer's first interview with Professor Pickering (during which he had described his first serious astronomical work, on stellar parallax) a letter arrived from Harvard, saying in substance "I think that it would be useful to determine the magnitudes and spectra of all your stars. If you will send me a list of them, we will have them observed, and send you the results." This involved the photometric and spectroscopic observation of some three hundred stars (the photometric settings being made by Professor Pickering himself) and was offered as an unsolicited contribution to the work of a young and unknown instructor!<sup>31</sup>

The fact that it was an unsolicited offer is of extreme interest.

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Most certainly Pickering was being quite generous, but why the aid to Russell at this time? Most probably it was coincidental, but we must remember that by this time, Pickering had received Hertzsprung's early papers and arguments, which were leading to conclusions, if correct, of great significance, for correlating luminosities and spectral types. It should also be realised that Lockyer's temperature arch most certainly was known to Pickering, which, if literally examined on physical grounds alone (spectrum/luminosity) was, in fact, a crude HR Diagram, with ordinate and abscissa reversed, and the direction of spectrum (or luminosity) also reversed.

It must also be remembered, however, that Pickering was quite interested in the spatial distributions of stars - many of the <u>Harvard</u> <u>Circulars</u>, and longer papers in the <u>Annals</u> had been long concerned with this study. Most probably, Pickering saw Russell's parallax studies as an important step in this study, specifically as Russell was an early proponent of Kapteyn's concept of "Absolute Magnitude".

In the week following Russell's April 4 letter, Russell evidently met with Pickering, and by April 10, wrote back to Harvard, sending parallaxes for some of his stars, and commenting: "... before long ... /Russell will get more parallaxes to Pickering7 ... and I will be very greatly obliged if you will give me the information you offered concerning their spectra, when they have been observed ...."<sup>32</sup> Russell also thanked Pickering for his hospitality during his visit. By the 15th, Pickering responded thanking Russell for the "statement regarding the parallaxes of the bright stars. They are interesting and instructive and show a better accordance than I had anticipated ... Come and see us again when you are next in Cambridge ...."<sup>33</sup>

At this point, I would like to insert parenthetically the interesting fact that back in February, 1900, Lewis Boss, the Director of

the Dudley Observatory, wrote to Pickering suggesting that his star places and proper motions be included in the various spectroscopic and photometric catalogues compiled at Harvard: "... it occurred to me that a mere reference to the fact of p.m. and its approximate amount might be a desirable feature ..."<sup>34</sup> Pickering's reply has not been found. At the time, it was certainly a helpful remark, and would have been of great aid, if heeded.

In the eight years interval, Pickering's interest in astrometric data obviously increased, in relation to support for Russell's work. On April 22, 1908, he replied again to Russell, evidently after reading copies of his 1905 papers with Hinks:

> An examination of your papers satisfies me that the determination of the spectra of your comparison stars will be a work of much value. Mrs. Fleming will accordingly do this, and enclosed are the results for two of the regions. Each region is examined independently on two plates. The differences are, as you see, insensible although it is necessary for such faint stars to use spectra only about 2 mm long. I understand that you obtain the relative parallax of each comparison star with relation to the mean of all. The material would perhaps be sufficient to determine which were the most distant, stars of Class A or Class K.<sup>35</sup>

It should be recalled that Hertzsprung's first letter to Pickering in March, 1906, announced that the 'red' class G, K and M should have the largest parallaxes, as opposed to the earlier classes. At the time, as he stated, his maximum at G was possibly due to mixing of bright and faint stars in the later classes, not yet recognised by Maury's subclassifications (or subdivisions a and c).

Pickering's interest, therefore, in the relative distances, as a class, of A and K stars, is curious, though it is by no means clear that it was influenced by Hertzsprung's work alone, as Kapteyn, Pickering (see ref. #146, Chapter 3), and many others, were working in

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the same general field. No-one apparently had seen the mixing effect (Maury's c stars mixed in with a and b for the later classes) that Hertzsprung had noticed.

At no time during these discussions between Pickering and Russell did Hertzsprung's work come up - at least in the correspondence thus far examined. This might well mean that Pickering did not yet consider Russell as a worker on an equal footing with himself, and as such, not privy to matters discussed in correspondence with others. It would have been simple for Pickering to refer Russell to Hertzsprung's papers though, except that they had not yet appeared in anything save for the <u>Zeit. f. wiss. Photog</u>., (Hertzsprung's "whales and fishes" letter came in August, 1908).

By the end of April, 1908, additional letters between Pickering and Russell indicate that, as Russell rushed through his parallax reductions, he sent them to Pickering, who had them placed on his programmes of spectroscopic study. By April 29, Pickering commented: "The results for parallax are most interesting ...."<sup>36</sup>

Just what these results were is not yet known: Russell may have been compiling early lists for distribution and luminosity studies or he may simply have sent Pickering a long listing of his parallax stars, and those stars used for comparison. This would, of course, explain Russell's statement that over 300 spectra and magnitudes were obtained for him at Harvard.<sup>37</sup>

By May, 1908, a long gestation period set in while Pickering's staff, directed by Fleming, made the observations and reductions. In any event, no additional correspondence between Pickering and Russell appeared until August, 1909, when Pickering sent a list of spectra and magnitudes back to Russell. In September, Russell replied, after returning from vacation, and made some comments on the data:

On my return from my vacation, I found your splendid list of magnitudes and spectra of the stars which I observed for parallax.

I cannot express too strongly my sense of their great value, or my very hearty thanks for them.

I am beginning to discuss them, and I find interesting things at the very start.

My comparison stars were chosen with regard to their position and photographic brightness, and ought to be a fair sample of the stars of about the 9th (photographic) magnitude. There are enough of them to be divided into two groups, one near the Milky Way, the others more remote.

If from my 'parallax stars' I exclude the naked-eye stars, and one or two others chosen for study on account of their peculiar spectra there remains a group of stars comparable in magnitude with the comparison stars, and differing from them only in their large proper motions. Finally, I may pick out of these the stars with large parallaxes (over 0".10) - usually confirmed by two or three observers.

This gives four groups of stars which are distributed according to spectral type, as follows: (where everything from  $F_6$  to  $G_5$  is reckoned as G, etc.)

. *		Α	F	G	K	М
I.	Galactic Stars	35	24	21	25	0
11.	Non-galactic	8	29	46	31	3
111.	Proper motion	2	0	11	13	3
IV.	Large parallax	1	0	1	6	3

The mean types of spectrum (reckoning the intervals of the different types as equal) are  $F_4$ ,  $F_9$ ,  $G_5$  and  $G_9$ , respectively.

Now these groups - or at least the last three are in order of decreasing distance, and hence of decreasing real brightness (since they all look about equally bright on the average).

This is very strong evidence that the fainter stars average <u>redder</u> than the brighter ones. I do not know of any previous direct evidence on this question.

I would not now risk reversing the proposition and saying that the red stars average intrinsically fainter - some of them certainly do: but Antares and  $\alpha$  Orionis are of enormous brightness, and the average may be pretty high.<sup>38</sup>

Thus, as initial observations, we see that Russell saw that the red stars were fainter than the blue stars, when the brightest naked-eye stars were excluded, but they they were included, the reds were amongst the brightest in the sky. These were conclusions strikingly similar to Hertzsprung's from 1907 and in his letters to Pickering. When Russell indicated that he knew of no previous work on these lines, one would have expected Pickering to respond with information about Hertzsprung, even to mention the fact that he personally did not believe that Hertzsprung's use of Maury's c- and non-c characteristics was valid, but that it led to the same conclusions Russell had reached. Of course, Pickering might have wanted the two to be as independent as possible, so as to be sure of the extraordinary results. There are many possibilities, but still the situation remains unusual, for in Pickering's reply (as determined by an acknowledged date written on the face of Russell's letter), he simply mentioned that the results looked good, and he would supply any additional information needed.<sup>39</sup>

Russell's letter in September continued to discuss his intentions to complete his study of the mean parallaxes for each spectral type, with an expected probable error not in excess of +/- 0".005, which he hoped would "settle the question ..." He was careful to point out, however, that "... the difference of parallax lies just near the limit of accuracy of the measures ..." which presumably referred to his comparison stars, and to his observation of the variation in spectral class and magnitude (absolute).

Russell proposed further work. Firstly, to examine the spectra of all stars with proper motions greater than one second of arc per year, of which there are at least 350, and secondly, to examine the spectra of all stars with parallaxes greater than 0".10, of which there

are perhaps thirty. With this larger sample, Russell felt that confirmation or refutation of the results from his small sample would be possible. He then commented:

> The small proportion of A stars in the nongalactic plane surprises me. I should think that my method of choice, depending as it does on photographic brightness would <u>favor</u> them. Perhaps there is some systematic influence at work: but otherwise it would look as if the relative increase of stars of Type I, compared with Type II, for decreasing magnitudes from 4 to  $6\frac{1}{2}$  (described in Harvard Circular 147) was reversed for the fainter stars. However, my sample is far too small to build much on in that respect.<sup>40</sup>

It is significant that Russell considered the possibility that some systematic effect was at work on the fainter stars - a systematic luminosity effect possibly - which would, of course, lead him in Hertzsprung's direction. Is it unreasonable to suggest that Pickering may have realised this, especially from Hertzsprung's conclusion that intrinsically bright red stars were always excluded, since they had no measurable parallax?

By this time, Russell began to pay closer attention to binaries on the programme, and noted several interesting systems. For one (B.D. +59° 1915) it was found, from a paper in the <u>Astronomische</u> <u>Nachrichten</u> by Bohlin,<sup>41</sup> that the fainter companion was blue. Russell felt this was very interesting and wanted to know its spectrum. Russell's interest in this star, and his awareness of Bohlin's work attested to the fact that Russell read the <u>Astronomische Nachrichten</u>, or at least he read an article in it which appeared in the same year as Hertzsprung's third paper. Presumably, Russell missed the Hertzsprung paper. Another system on his lists, Groombridge 34, was also of interest. This star had two companions - one which Russell felt was 'optical', but another which shared a common proper motion with the Groombridge star. Russell commented: It must be about 1/1000 as bright, intrinsically, as the Sun, and it would be of interest to know its accurate magnitude.<sup>42</sup>

Within this letter, Russell enclosed tabulated results for his work on mean parallaxes of comparison stars with reference to magnitude and spectrum.

In this latter work, Russell compared his results with what would be expected from mean parallaxes derived from Kapteyn's methods. Since the differences were so small, Russell felt that the systematic errors "... of photographic determinations of parallax under suitable precautions, such as I have taken, are quite too small to detect. This is of great practical importance ..."<sup>43</sup> The practical aspect was, simply, that he had successfully defined his reference frame for the reduction of relative to absolute parallaxes for his programme stars. But, on another line, he also saw the value in such studies for the determination of the average distances of stars within each classification. Thus, he announced to Pickering that he would carry on this second task, supplemented with proper motion data, and hoped soon to come to "... positive results as to the relative distances of the different groups ..."

## On the Origin of Binary Stars

While Russell continued to analyse his parallax data, he also returned to his first love - binary stars. In January, 1910, he prepared an extensive paper "On the Origin of Binary Stars", which was an expansion of a paper he delivered at a local meeting of the National Academy of Sciences: presumably the one held at Princeton in November (8 - 10), 1909. Russell's paper began:

> One of the fundamental problems of cosmogony may be stated as follows: Will a rotating mass of fluid, in equilibrium under its own gravitation, and free from sensible external disturbance, but

subject to loss of heat and consequent contraction, eventually break up into separate parts; and if so, how will the separation take place?<sup>44</sup>

Russell reviewed Darwin's solutions and criteria for stability and instability in fission. Briefly, Darwin's work had shown that a mass subject to the conditions expressed by Russell would flatten, tend to lose stability, but then recover it "... by changing into an ellipsoid of three unequal axes, which similarly goes over into an elongated "pear-shaped" figure with one end larger than the other ..." If the mass were to undergo fission, however, a period of instability or turbulence, was expected.

As this study related to homogeneous incompressible fluids, Russell noted that the actual objects of astronomical study "... are gaseous - highly compressible, and much condensed toward their centers ..."<sup>45</sup> Thus the situation was considered to be very uncertain regarding the process of formation of binary systems. Russell quoted a recent Darwin statement on this:

> Originally the star must have been single, it must have been widely diffused, and must have been endowed with a slow rotation. In this condition the strata of equal density must have been of the planetary form. As it cooled and contracted the symmetry around the axis of rotation must have become unstable, through the effects of gravitation, assisted perhaps by the increasing speed of rotation. The strata of equal density must then become somewhat pearshaped, and afterwards like an hour-glass, with the constriction more pronounced in the internal than in the external strata. The constrictions of the successive strata then begin to rupture from the inside progressively outwards, and when at length all are ruptured we have the twin stars portrayed by Roberts and others.<sup>46</sup>

Russell immediately turned from Darwin to the opposing ideas of Chamberlin and Moulton, which viewed double star systems as ones evolved not by fission, but by the contraction of well-defined nuclei within the primordial nebula. Their conclusions as to the effects of fission, as Russell related it, were that if it did occur, it would not produce separate bodies of similar mass. During the process, many small masses would be successively shed, with danger of the entire mass breaking into many small bodies. We may assume that Moulton concurred with this description of his ideas, since Russell had sent him the manuscript for comments before publication. Russell then started his own analysis. He recognised the fact that binary systems with only two components would be inappropriate to study since they could be explained equally well on both theories. "When it comes to the triple and multiple systems, the situation is different".<sup>47</sup> He continued:

> There seems to be no a priori reason why systems originating from independent nuclei should show any definite relations of mass or relative distance.

Russell suggested that, if multiple systems were formed from independent nuclei, then their orbital distributions should be random, and well defined pairs within the larger system would be rare. "On the contrary such a grouping is a necessary consequence of the fission theory ... It is the purpose of the present discussion to develop these consequences, and to see how far the results agree with observation".

In developing his formalism, Russell considered various possible multiple systems forming with a range of relative separations between pairs; the results of primary and secondary fission; and the ratio of the orbital sizes of the primary and secondary systems within the multiple group. One of his first dynamical results was that:

> If a mass divides by fission into equal parts, and one of these divides again in the same fashion, owing to its rotation alone, the initial distance of the secondary pair cannot be greater than about 1/18of that of the primary pair; and hence the mean density of the mass at the time of the second separation must be at least 2500 times as great as at the time of the first.<sup>48</sup>

While his analysis was going to show eventually that well defined multiple systems are best understood in terms of fission origins, our interest here is to show the development of his ideas regarding the role of binary systems in the general discussion of evolution. Here we see Russell's belief in the relationship of binary separation and component density. Most certainly, in his view, which was Darwin's, a fission event occurring early in the developmental process occurred when the original mass was still relatively uncondensed and, hence, quite large. Thus, one would expect systems with large separations to have undergone fission early, and to exhibit components of very low density at the time of fission. From this, one would have to conclude that binaries of wide separation were older systems, and would have to exhibit spectral characteristics appropriate for their age. Russell did not develop this theme fully in the paper under discussion, though it certainly was in his mind, and was to appear within the year.

Russell reached a number of conclusions from his analysis:

Given a gaseous mass, which divides by fission, without external disturbance, into two parts:

1) The distance of centers at the time of separation is greater, and the density less, the more unequal these parts are.

2) The ratio in which the initial distance can be increased by tidal action increases as the masses become more unequal.

3) The smaller mass has the greater density just after separation.

4) The ratio of contraction necessary to bring about a second fission (other things being equal, and tidal friction absent) is less for the greater mass.

5) The ratio of the dimensions of the separating masses at the time of the second fission to that of the first ... is always small.

6) The same is true of the final orbits resulting from the successive fissions.<sup>49</sup>

If tidal action was present, before the second fission, it would increase the primary separation, and decrease the secondary separation,

1

at the time of secondary fission. If tidal action is strong enough, the smaller mass will be the only secondary fission body, the larger one reacting less, and, hence, triple systems instead of symmetrical quadrupal ones result.

Russell turned to observational data for verification:

For such a distribution of masses as is found among binary stars, the results of the fission theory are quite definite. Multiple systems arising in this way must be pairs, one or both of whose components are themselves double, with a distance less than about one-fifth of that of the wide pair - usually much less.<sup>50</sup>

But he was quick to caution that evidence for the actual physical connection of observed multiple systems must be forthcoming. Russell felt that the least one could do was to establish common proper motion, since "Among the many systems of this sort there is not one in which we can yet determine the orbital elements of the wide pair; and in most cases this is still impossible even for the close pairs".

Without actual orbital data, the observed separations of the components in these systems were subject to many errors; inclination to the line of sight and orbital eccentricity being the most important amongst them. Russell felt, however, that he could examine a large body of data on orbital separations, group them in terms of degree of space motion (or simply proper motion, in most cases), and evaluate them statistically. He tabulated systems taken from S.W. Burnham's "General Catalogue of Double Stars" separated into four groups based upon their primary separations (expressed in terms of 100, 300, 1000, and greater than 1000 years common proper motion - presumably a criterion for the evaluation of the reality of the systems). Within each group, the ratio of secondary to primary separation was the only tabulated quantity.

In a second table, Russell compared the distributions of the ratios of orbital sizes to proper motion group in two categories:

separations less than 1000 years proper motion, and separations greater than 1000 years proper motion. The smaller separations yielded a distribution close to the theoretical results expected from fission. But the larger separation group did not agree, which suggested Moulton's mode of origin, as condensates from separate nucleii.

> These wide pairs, however, with a separation of many thousand astronomical units, showing as yet no sign of relative motion, whose periods, if they are really in orbital motion, must be counted by hundreds of thousands of years, are very far from what are usually called binary stars. It is probable that if we could extend our survey to systems of still greater linear extent we would find them grading into the irregular star clusters, like the <u>Pleiades</u>, whose members have a common proper motion.<sup>51</sup>

By association then, Russell inferred that clusters like the Pleiades were co-evolutionary, being formed out of separate nuclei in a common nebula. But for the systems within his proper motion limit (which should be seen as a criterion for association as well as dimension) "... we find everything in harmony with the fission theory, up to a distance exceeding more than tenfold that of the widest pairs which so far show signs of relative motion ..."<sup>52</sup>

Russell admitted, however, that the present studies of incompressible fluids undergoing fission did not yet yield stable solutions, as Chamberlin argued. But Russell felt that "The facts already detailed /which included the existence of contact binaries like Beta Lyrae7 establish a presumption that when the subject is mathematically explored, some series of figures of equilibrium of a <u>compressible</u> gas, ending in fission into two comparable masses, will be found to be stable".<sup>53</sup>

In conclusion then, Russell felt that in very widely separated random groupings, like the Pleiades or the Trapezium in Orion, origins through separate condensation nuclei was most reasonable. "But the arrangement in close and wide pairs, so characteristic of the large majority of multiple stars, is on this theory a positive difficulty".<sup>54</sup> They could best be accounted for by fission, "... and this theory may well be adopted as a working hypothesis until some evidence, either observational or theoretical, is produced to oppose it".<sup>55</sup>

In this extensive discussion Russell did not provide any explicit spectral criteria for observed binary separation correlated with system age, as he was later to do. He merely provided the all important footing for his evolutionary scheme, which, if spectroscopic and eclipsing binaries were to be included in it, had to account for observed characteristics in terms of modes of origin.

## Final Reductions of Russell's Parallax Data

Returning now to his parallax work, in the months after January, 1910, we find Russell attempting to secure better photometric and spectroscopic data on binaries from Pickering, and thanking the Harvard Director profusely for his aid. By early April, only a very few stars still did not fit the relation he had earlier found - that as one goes to the redder stars, proper motions and parallaxes increase. On April 7, Russell wrote to Pickering with two stars classed as A, which had large proper motion, and asked that their spectra be checked, since all other large proper motion stars were of later spectral class, and all the other A types had very small motions. He noted that their photometric properties suggested a later type too:

> The two stars first mentioned are the only apparent exceptions to the general rule that increasing redness goes with increasing P.M. or parallax. If their spectral types are in accordance with their photographic faintness, they will fall in with the others.<sup>56</sup>

Russell mentioned that he expected to read a note on this at the next meeting of the American Philosophical Society on April 23, and

noted that he understood Pickering to be presiding.

Russell's talk before the American Philosophical Society was the first published statement of his belief in the existence of two classes of red stars, which first appeared in correspondence in September and October, 1909. An abstract of his paper appeared in June, 1910, in the journal <u>Science</u>. The paper "On the Distances of Red Stars" began by stating that a "marked correlation between spectral type and parallax" had been found from his work in association with Hinks, and from Pickering's contribution of spectra. The remainder of the short text is here reproduced in full:

> The proportion of orange and red stars (types K and M) among those of large proper motion, and especially among those shown by direct measurement to be our near neighbors, is very much greater than among the general run of stars of the same apparent brightness. Conversely, stars of the same apparent brightness and proper motion average nearer to us the redder they are.

> It follows that these stars are intrinsically fainter the redder they are, the reddest ones averaging only one fiftieth as bright as the sun. On the other hand, many bright red stars (such as Arcturus) are at great distances, and are actually at least one hundred times as bright as the sun.

All this can be explained on the hypothesis (now well established on other grounds) that the reddest stars are the lowest in temperature. With the latest determinations of temperature and surface brightness, it appears that the fainter red stars are somewhat smaller, and presumably denser, than the sun, while the brighter ones are very much larger than the sun, and presumably of very small density. The latter class probably represent an early stage of evolution, and the former the latest stage that can be observed.<sup>57</sup>

Though in abstract only this represents Russell's first statement in print of his theory of evolution.

No published reaction to this paper has been found. Actually, the brief report seems to have been buried amongst Russell's many papers at the time. It is not even mentioned in Shapley's bibliography,<sup>58</sup> nor in a bibliography received from the Princeton University Library. Of course, this was only a brief statement, which was to be expanded upon more than once in the same year, and to a vastly greater extent in the following years. But still, it seems to have been ignored, and in this sense shared the same fate as Hertzsprung's early papers.

During 1910, especially after his April address, Russell prepared a more extensive discussion of his parallax work. Actually, two publications of very similar nature were in progress: one, of intermediate length for the <u>Astronomical Journal</u>, which was first received by the editor (Boss) on July 30, 1910, and a very long memoir for the Carnegie Institution <u>/his benefactor at Cambridge</u>, which appeared in 1911.

One person did react quite favourably to Russell's original abstract in <u>Science</u> for June, 1910. In October, 1910, Norman Lockyer wrote:

> I have been very much struck by your paper in the Proceedings of the A.P.S. & wish to ask you if you can kindly send me a copy of the complete paper when it is published.

I have had to wait some years for such a clear cut support of my views & am delighted that it is afforded by researches of a different order from my own.<sup>59</sup>

With this letter, Lockyer sent several of his papers, although he commented that he thought they should be available in the Princeton Library, since Lockyer and Young had corresponded frequently in the past. A rough draft of a reply to Lockyer has been found in the Princeton collection, dated January 14, 1911. This rough draft has many erasures, crossed out passages, reworded sentences, and incomplete comments. Without knowing the final nature of the letter actually sent to Lockyer, we can only use the following transcript as a rough guide to Russell's thoughts: I must apologize for my slowness in responding to your letter of October 5, which has been due to delay in obtaining reprints of the papers dealing with stellar evolution which I have so far published. It gives me great pleasure to enclose them herewith. The single sheet, reprinted from "Science" contains a summary of my views (the theory). Other evidence bearing on the subject can be found on pp. 151-154 of the reprint from the Astrophysical Journal and the paper "on the origin of Binary Stars" deals with a closely allied question.<sup>60</sup>

The reference to the <u>Ap.J.</u> is apparently an error by Russell, for the page numbers referred to are to his paper "Determinations of Stellar Parallax"<sup>61</sup> in the <u>Astronomical Journal</u>, which also discussed the bearing of his work on stellar evolution. The draft continued:

> Please accept my thanks for the copy of your Catalogue of 470 stars (... I have been familiar with this for some years...) which I should value very highly, especially as a gift from its author.<sup>62</sup>

The phrases and words in parentheses refer to parts crossed out in the draft. This is of extreme importance, as it confirms that Russell had long been privy to Lockyer's schemes, and, as we have shown, he might very well have consulted the Catalogue in setting up the parallax observing lists:

> (It may well have been from this paper that I first became acquainted with the idea of the classification of stars according to rising & falling temperature - I cannot now remember)

I am at work now on a detailed discussion of this subject, which I hope will put the theory that the course of stellar evolution consists of a rise in temperature ---

I wish to express my appreciation of your courteous reference (in the Proc. of the Roy. Soc. for December) to my preliminary work ... and incidentally to remark that I am not ... Director here (the post being vacant) but assistant professor.

Russell later amended the last quoted paragraph to read:

I am at work now on a detailed discussion of the evidence (all available parallaxes, proper motions, binary and variable stars, etc.) bearing on the order of stellar evolution which I will send you as soon as it appears.

This probably referred to his long discussion of parallax material which appeared later in 1911 as a <u>Carnegie Institution</u> <u>Publication</u>, though an earlier letter from Russell to Frost<sup>63</sup> suggests that he finished his parallax discussions in late 1910. It is possible that Russell, at the time, planned to come out with a paper similar to his famous 1914 discussion, but eventually decided to delay its publication until sufficient binary star data was available.

The Russell draft continued:

I hope this will at least establish a strong presumption in favor of the view (which you have so long advocated) - that stars at first rise in temperature & become whiter, then cool off and get redder again - as you so long have maintained.

I should however differ with you very widely as regards the assignment of individual stars to the classes of rising and falling temperature -

It looks to me as if the whole series of spectral types was run through on both branches of the temperature curve - the order in the Harvard relation being

## $M \rightarrow K \rightarrow G \rightarrow F \rightarrow A \rightarrow B \rightarrow A \rightarrow F \rightarrow G \rightarrow K \rightarrow M$

though probably only the (brightest) most massive stars get up to B - and others of small mass may not go beyond F. $^{64}$ 

In this section, we see the all-important distinction between Russell's and Lockyer's work - the concept that not all stars followed the same complete spectral path. This placed Russell's ideas more in line with Ritter. In addition, we see here Russell's statement, which was already in print at the time, that even though their results were very much the same, they differed in the assignment of certain stars to one or the other branch. Russell continued to discuss this:

333.

In my opinion the real test of place in the evolutionary series is <u>density</u> (which must ... increase) we cannot get at this directly, but low density means extended surface and ... <u>/illeg.7</u>

Hence I am inclined to regard the actual luminosity of a \_\_\_\_\_\_ or a red star (i.e. its total light radiation) as the best evidence of its place in the series (early or late part of the series). The spectroscopic peculiarities which are the basis of the separation (as regards) in your catalogue appear to be common to stars of very different intrinsic brightness.

At first, binary star data yielding densities were meagre, so, as we have seen, Russell depended upon magnitudes and distances for luminosities. When coupled with colour, these yielded density estimates based upon the laws of black body radiation. At the time, however, Russell was not prepared to depend solely upon this line of evidence: the weakness presumably was felt to be the applicability of the radiation laws. Possibly he felt that emphasis upon them alone, without verification, would not produce a convincing argument. Thus he turned to binary stars for 'direct' density estimates. He was not able to use Lockyer's spectral criteria for the important reason quoted above, which, if actually written to Lockyer must have caused disappointment. In the same manner, Russell probably felt it wise not to rely too heavily on Hertzsprung's recognition of the Maury c-characteristics, of which he certainly was aware by the time of the writing of the rough draft to Lockyer. Quite possibly, once Russell had been told of Hertzsprung's work by Schwarzschild at the August, 1910, meetings of the Astronomical and Astrophysical Society in Cambridge, he discussed the matter with Pickering, and learned of Pickering's scepticism concerning the reality of these spectral differences. In this light, it is understandable that Russell would wish to keep his own discussion independent of spectral criteria. Later in 1911, in a letter to Pickering discussing a planned

talk (at a meeting of the Astronomical and Astrophysical Society of America in Ottawa, August 1911), Russell indicated that he would like to talk on "this matter of giant and dwarf stars ... it is new, has important consequences and is not controversial if the bearing on stellar evolution is not emphasised unduly ..."<sup>65</sup> Clearly then, Russell recognised the controversial aspects of Lockyer's scheme, and wished to provide evidence in the clearest, strongest, and most independent light possible.

Russell's long draft to Lockyer in January, 1911, continued with examples of various stars found in Lockyer's <u>Catalogue</u>, and how they fitted into his own scheme. He identified two branches, and provided the following list:<sup>66</sup>

... for example I find

Arcturian Stars

2	-		1
Aurigae		τ	Ceti
Bootis		ζ	Ursa Maj.
Cygni		α	Centauri
Leonis		ζ	Herc
		70	Ophiuchi
	Procyonian Stars		
Hydrus		[?]	Cass

α Can

Aldebarian Stars

 $\beta$  Hydri

 $\gamma$  Androm.

α Tauri

α

α

β

Υ

α

The measures of parallax (given by Kapteyn in Groningen Pub.  $\neq$  24) show that there is an enormous difference in the real brightness of the stars in these two columns - those in ... (1) ... being comparable to the Sun, and those in ... (2) ... 100 or more times brighter.

I am therefore inclined to think that the relative thickness of the hydrogen and 'enhanced' lines must depend on some quite independent set of physical conditions. It is not improbable that some actual spectroscopic distinction may correspond to the great differences in density between these two classes of stars, but I believe that we must first classify as many stars as we can by other means and then by comparison find what the spectroscopic differences are.

From the above list, it is obvious that stars classed as Arcturian, Procyonian and Aldebarian by Lockyer were found equally mixed in luminosity by Russell.<sup>67</sup> Thus, Russell rejected Lockyer's interpretation of spectral differences as indications of rising and falling temperature, but left the possibility open that some distinction would eventually be found empirically. Possibly the most injurious piece of evidence referred to Lockyer's Piscian stars, which he had placed at the base of the descending branch. Russell commented on these stars:

> Kapteyn's recent paper showing that stars of the Piscian type are very remote, and subsequently very bright, inclines me to place them at the very beginning of the evolutionary series. At the end I should place such stars as \_\_\_\_\_\_2164 ( $\sum 2398$ ) and Kruger 60 which though of half the Sun's mass are about 1/200 as bright. (These must be almost extinct) ... (Professor Pickering writes me that ...) Their spectra are of the K or M types on the Harvard 68 Classification as are all the similar stars ...

Russell eventually rewrote this last section and included a listing similar to the one preceding, with more examples of Arcturian stars. Throughout his draft, he never faltered from the interpretation that the bright stars were rising in temperature and the faint ones falling. In the conclusion to his draft, he reworded his statement about Lockyer's spectral criteria:

> Those of similar class convince me that the (spectroscopic diff) relative thickness of the hydrogen and proto-metals - ('enhanced') lines depend on some other set ... physical conditions than those which accompany rising & falling temperature. I think it not improbable that some spectroscopic distinction

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between the two can be detected but we must first separate the two classes by the more effective criterion of luminosity.

This ends Russell's draft. It is hoped that the version actually sent, if sent at all, will someday be recovered, and Lockyer's reaction also found. For the present, we see in this draft the basic differences between Lockyer's and Russell's treatments, the possible early influence of Lockyer's Catalogue upon Russell's ideas, and the early identification by Russell of the fact that not all stars proceed through identical evolutionary paths. When, in the following five to six years, vastly increased data on stellar masses became available, and the main sequence became identified as an evolutionary track (Lockyer's falling temperature branch), Russell was to have recourse to this idea to explain why stars along the sequence should not all be of the same mass. It was difficult to see how a high mass B star could evolve through cooling and contraction to a low mass M star, unless, in a statistical sense, as Russell argued, the lower masses found at the red end of the main sequence were a result of the mixing of high mass, but cooled B stars (which were assumed to be rare), and the low mass red stars, which themselves never rose in temperature to the B range.

We return now to the summer of 1910, the critical period in our discussion, when Russell first learned of Hertzsprung's work.

It is not unreasonable to suggest that Pickering was very much aware of the similarity of Russell's and Lockyer's views, by the April 23 meeting of the Philosophical Society. Further, it is conceivable that, in strictest confidence, Pickering did discuss Hertzsprung's interpretation of the c-characteristic with Russell. This would help to explain Pickering's simultaneous letters to Frost and Campbell in July, 1910, asking for high quality slit spectra to examine for possible composition differences as indicated by differences in line structure, and Russell's emphasis that the spectral differences used by Lockyer led to illusory results.

By late July, 1910, Russell had completed his paper, "Determinations of Stellar Parallax", for the <u>Astronomical Journal</u>. It was submitted by July 30 and printed in the issue of 28 October. Russell's introductory remarks and general discussion follow material we have already covered, except for the important inclusion of work done subsequently at Princeton and his employment of proper motion data from Boss' <u>Preliminary General Catalogue</u>. In this paper, also, we find the first completed listing of his parallax stars from his Cambridge observations, and a detailed discussion of his use of Kapteyn's formula for the mean parallaxes of stars of a given magnitude, which allowed him to reduce his relative parallaxes to 'absolute' values.

In this paper Russell also began to discuss his 'hypothetical' parallax techniques (today called dynamical parallaxes) to determine masses and distances of binary systems with poorly determined orbits. At the time he did not carry this through, but treated systems on his list with poorly known orbits in a statistical manner to derive approximate masses. In this way, he was able to treat enough systems of very long period to find that their components had masses far less than the Sun. These low mass systems were also quite faint, and, even assuming high densities of the order of 8 times the Sun's value, their surface areas came out to be of the order of 1/10 that of the Sun. "The actual surface brightness of the ... stars must therefore be very small, which is direct evidence of their low temperature. The spectral types of the brighter components are Ma and  $K_5$ . According to Scheiner and Wilsing, this indicates a surface brightness of about 1/40 that of the Sun. Unless the actual brightness is considerably lower than this, their density must be high".<sup>69</sup> Russell's reference to the work of

Scheiner and Wilsing deserves attention. Cogan has noted that "Russell had a distinct advantage over his predecessors when it came to determining surface temperatures of the stars. There seems never to have been any question in his mind that the white stars were the hottest ones and the red ones the coolest". Quite possibly this is true, except for his 1907 commentary in his lecture notes. Russell was not as involved in spectroscopic interpretations of the atmospheric conditions found in stars as were Hale, Huggins, Lockyer and others. These latter were more familiar with the long struggle through the latter part of the nineteenth century over the interpretation of the effect of a masking atmosphere upon the colour temperature of a stellar body. He apparently had no reason to worry over the possible illusory effects: a worry that lingered in the minds of the older astronomers.<sup>71</sup> Furthermore, by 1909, the Planck radiation laws, which embodied Wien's and Stefan's relationships between colour, temperature, and intrinsic brightness (surface brightness and extended area of radiation), had become more widely known and applied. The specific work along these lines used by Russell was by Wilsing and Scheiner on the temperature determination of about 100 of the brighter stars by the technique of spectrophotometry.<sup>72</sup> As Russell noted:

> Recent investigations make it probable that a star's spectral type is very closely related to its surface temperature, and presumably to its surface brightness. In fact, the recent temperature determinations of Scheiner and Wilsing indicate that a star of type G should be about two magnitudes brighter (visually) than one of equal diameter but of type K.<sup>73</sup>

Wilsing and Scheiner were able to show, from studying isolated parts of the continuum radiation from stars, that the relative intensities in these parts agreed reasonably well with the relative intensities predicted by the Planck radiation law for black bodies radiating with

effective temperatures close to those predicted from an analysis of their colour. Of course, Hertzsprung had long employed this line of work in his use of the 'effective wavelength' of a star as an indication of its colour. Russell was to support strongly continued calibration work in this area in the following years, encouraging workers such as J.A. Parkhurst at Yerkes to develop and refine the use of the colour index.<sup>74</sup>

Russell was thus able to establish that the Harvard Sequence varied with mass (decreasing), density (increasing), and temperature (decreasing) with advancing spectral type for the majority of stars studied for parallaxes, which, of course, excluded the bright red stars. He then turned to an examination of the distribution of the different spectral types in space, using not only his parallax stars, but his comparison stars, too, for which spectra had been obtained. The table that summarised his findings was an expanded version of the one he provided for Pickering's examination in September, 1909, which was his first evidence that with increasing redness, the average intrinsic brightness decreased, or, that the fainter stars average redder than the brighter ones. Russell reasoned that stars of large parallax, but invisible to the naked eye, must be intrinsically very faint, and therefore red. For large proper motion stars, the distance criterion is relaxed. Therefore, more yellow stars become included in the sample. Similarly:

> The absence of faint stars of large propermotion and spectra of B, A and F is explainable on the hypothesis that these stars are of greater luminosity than those of type G, so that, to be invisible to the naked eye, they must be so remote that even the greatest velocities which actually occur among the stars do not give rise to an apparent proper-motion large enough to be included in our lists.<sup>75</sup>

Bright red stars with no sensible parallaxes were reserved for

his next discussion, where he examined his results and their "Bearing on Stellar Evolution". This section began with Ritter and Lane, and provides nothing unusual. Russell interpreted Lane's and Ritter's theoretical treatments in terms of the physical parameters he was interested in. "Before the maximum temperature is reached", he believed, "the surface-brightness increases as the diameter diminishes, and it is not obvious whether the total light-emission rises or falls; but after the star begins to cool, diameter and temperature diminish together, and the decrease in luminosity must be rapid".<sup>76</sup> This was later to be seen in his "reversed 7" colour-magnitude diagram.

The above, of course, can be understood easily since the lack of definitive parallax data for the bright red and yellow stars precluded assignment of accurate intrinsic brightnesses. There was little doubt in Russell's mind that all the stars he studied were in the cooling stage. "The very rapid decrease of light with increasing redness, and the extremely small luminosity of the reddest stars, in spite of the fairly considerable masses of those which can be investigated, are distinctive marks of stars in a late stage of evolution, past their prime, and in some cases verging toward extinction".<sup>77</sup>

It should be noted here that Russell did not discuss evolution in terms of binary origin, as he was soon to do, and did not attempt to discuss what he revealed in private later to Lockyer - that he believed that not all stars followed the same complete evolutionary path described by the Harvard Sequence.<sup>78</sup>

During August, Russell prepared the two papers he was to deliver later that month in Cambridge at the Harvard meetings of the Astronomical and Astrophysical Society. These were arranged, of course, by Pickering - the guiding force of the Society. This eleventh meeting of the Society was arranged to accommodate the attendance of foreign astronomers, who were travelling to America to attend the meeting of the International Union for Co-operation in Solar Research (to be held in September, 1910, at Mount Wilson). The Harvard meetings were held on August 17, 18 and 19 - two weeks before the Mount Wilson meetings.

"This proved an excellent meeting".<sup>79</sup> Indeed, as the editorial commentary suggested, the plan allowed the following Europeans to attend, among others:

H.F. Newall; H.H. Turner; J.S. Plaskett;A. Belopolsky; F.W. Dyson; K. Schwarzschild;J. Larmor; A. Fowler; J.R. Rydberg; Ch. Fabry;

... which evidently was not the entire contingent. Among the Americans, Pickering, of course; Russell, Stebbins, the general Harvard staff including Miss Cannon, Miss Harwood, and Miss Leavitt; totalling some one hundred in attendance. Five sessions for papers were planned, with a total of forty-four papers read. Russell delivered two papers: "On the Determination of the Elements of Algol Variables" and "Some Hints on the Order of Stellar Evolution".

His first paper discussed how the actual brightnesses of components in Algol systems could be approximately determined from an analysis of the orbit, assuming spherical stars with uniform surface brightness in a circular orbit and total eclipses (constant light at minimum). One of the elements required was the ratio of the diameter of the eclipsing star to its orbit. From his analysis, distances were derivable, since they were directly associated with differences between absolute and apparent brightness. With distance, stellar dimensions and, hence, densities naturally followed. Russell commented that it was possible to derive the orbital parameters by his method (by the graphical generation of a predictive light-curve which could quickly be matched to the observed curve) in "less than an hour ..."<sup>80</sup> He also discussed

iterative methods for examining partial eclipses. This short paper was to be greatly expanded upon in the near future,<sup>81</sup> and was to form the basis for the reductions of some eighty systems by his student, Harlow Shapley. Not mentioned in this short note was the application to the densities of the stellar components. In the longer papers quoted above, densities were discussed, referring to Russell's and Roberts' 1899 treatments.

His second paper, clearly the more spectacular of the two, began with a re-statement of the laws of Ritter and Lane, and their application to evolution. At the outset, the question of the assignment of the critical density point (below which the gas was 'perfect', and above which it no longer held to the gas laws) was emphasised and placed "... probably between those of air and water, and nearer the latter ..."<sup>82</sup>

Russell did note here that "The most massive stars will reach the highest temperatures at maximum". This was the germ of the idea that not all stars went through exactly the same evolutionary track, but at this reading, nothing else was mentioned, though he later mentioned that "... Those stars that are hottest at any given time will, therefore, be more massive than the average ..." which was directly applicable to the interpretation of diminution of mass from the blue to the red along the dwarf sequence. Russell went on to discuss the distinction of the two classes of red stars, and his employment of binary statistics in support of his theory, which first appeared here. Starting with initial nebular conditions ...

> ... As contraction proceeds, the stars, whose angular momentum is large, will break into pairs, those formed earliest having the longest periods. The farther evolution proceeds, the greater will be the proportion of such pairs among the whole number of stars. Periods less than a day or two can not arise unless the density is already near

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or beyond the critical value defined above work on spectroscopic binaries has shown that the proportion of these is greatest for type B and least for types K and M; that short periods, especially those less than two days, are practically confined to types B and A; that the systems which give evidence of unusually great mass are almost all of type B; that the relation between period and eclipse-duration among the Algol variables (which are almost all of types B and A) shows that their densities are of the 'critical' order of magnitude; and that the distribution of proper motions among the stars of given apparent brightness and spectral type shows (as Hertzsprung has pointed out) that the redder stars from type G onward, fall into two groups: one remote, of small proper motion and great luminosity, the other near us, of large proper motion and small luminosity.<sup>83</sup>

Russell's employment of binary statistics is different from Kapteyn's. We recall this was that, from considerations of tidal evolution, a binary pair that formed at an early stellar stage would evolve through tidal friction into a widely separated pair, with advancing evolution. 84 Thus, early type stars in binary systems had short periods, and late type stars had longer periods. But Russell's idea did not directly imply that the separation for any one system would increase greatly during the life of its components. He merely reasoned that widely separated pairs had to fission at an early evolutionary stage. Later, in 1913-1915, he was to explain this further, in the face of criticism which interpreted his discussion as that of Kapteyn's, by saying that he did not feel that an early-type spectroscopic or eclipsing system would, during its life-time, evolve into a visual pair of late type stars. The degree of separation increase by tidal action was not that great, in his mind, though his critics felt, in their misinterpretation, that it had to be great, and way beyond reasonable theoretical bounds.

Russell's parenthetic acknowledgment of Hertzsprung's work

appeared also in an earlier abstract of Russell's paper<sup>85</sup> in December, 1910. It is not known why the reference appeared in parentheses, since others referenced either directly or indirectly (Ritter, Lockyer) appeared within the text. Nielsen<sup>86</sup> has suggested that Hertzsprung's name was added after Russell's talk, when, in conversation with Karl Schwarzschild, he found out about Hertzsprung's similar work.

In conclusion to his short paper Russell mentioned Lockyer.

The scheme of evolution here suggested is presented tentatively, as a working hypothesis. Its fundamental conception is similar to that underlying Lockyer's classification - from which, however, it differs radically as regards the criteria for distinguishing rising and falling temperatures.<sup>87</sup>

The holding of the International Solar Union Conference in September, 1910, at Mount Wilson, and the Harvard meetings just described, afforded a certain amount of time during which astronomers from the United States and Europe were in close contact while en route from Boston to Pasadena, California. From accounts of the trip, it is evident that as the train proceeded westward, more and more American astronomers joined the party. As the commentary on the eleventh meeting of the American Astronomical and Astrophysical Society suggested:

> Never before had so many representative European and American astronomers been together for so long a period with such opportunity for the exchange of views on topics of mutual interest. During the three weeks from the assembling of the Astronomical Society in Cambridge to the close of the Solar Union meeting in Pasadena, astronomical conferences were nearly continuous.<sup>88</sup>

Russell was exhausted after the trip. He wrote to Frost on October 14 that he had nothing for the <u>Astrophysical Journal</u>, though he had just finished his parallax work "... which has felt like a millstone round my neck ...."<sup>89</sup> Evidently, within one month, he did decide to send something to the journal, for a study dated November 12, 1910, on the mass-ratios of two binary star systems - Krüger 60 and Castor - appeared soon after.<sup>90</sup>

Quite possibly the appearance of W.W. Campbell's spectroscopic binary catalogue stimulated Russell to discuss these systems. While preparing the paper, he wrote to Campbell thanking him for the catalogue:

> It is a veritable mine of information, and of the very first importance in such work on stellar evolution as I am trying to do.<sup>91</sup>

Russell, however, was perplexed by some aspects of Campbell's discussion. Principally, the distribution of values of what Russell had identified in his work on Castor as the system's "apparent mass".<sup>92</sup>

This link between binary studies and evolution was uppermost in his mind at the time. On November 14, he wrote to Frost, after submitting his mass-ratio paper, and commented that with this submission, he was now ready to take up the question of stellar evolution.<sup>93</sup>

During this period, Russell also corresponded continuously with Pickering - asking for more data on doubtful cases that did not fit his relation between brightness and redness. By November 26, 1910, the "last apparent exceptions to the rule ..."<sup>94</sup> were removed, "... and, I think, will help to establish the theory that these stars are in a very late stage of evolution".

He indicated, though, that he was about to look into a new set of binary systems, for which he would need spectroscopic classifications, and would be contacting him soon about it. By December 9, Russell indeed began to ask for spectra on binaries, starting with the faint, but large, companions of Algol variables in systems like U Cephei, Delta Cancri, and U Sag. Most of the stars on his list were taken from Campbell's and Aitken's listing of spectroscopic binaries with well determined orbits.<sup>95</sup> Russell noted:

The three faint companions of bright stars are of exceptional interest - the last two being very much the faintest stars, in proportion to their mass, whose orbits we know with any precision. The companion of  $o_2$  Eridani is of special importance, as its parallax is well determined, and, in my opinion it would be worth taking special plates for.<sup>96</sup>

Russell also asked Pickering for the spectra of the faint large companions of various Algol variables, which he noted would "... be of very high theoretical importance ..." especially, one would assume, if they came out to be red. He noted that some had shown spectra ranging roughly between II and III, but did not specify the systems he referred to.

While in Pasadena at the Solar Union, Russell became deeply impressed by the fine slit spectra they were producing, and so wrote to Adams asking for spectroscopic identifications, and more information on some spectra that were classified differently at Mount Wilson. A star classified at Harvard as K5 was believed to be F at Mount Wilson. Mrs. Fleming, with Russell at Pasadena, verified that from its spectrum there, it was, indeed, F. But upon further examination, they found that the Mount Wilson spectrum was of another star! Russell explained to Adams how he was led to this particular problem:

> What made me doubt that the F type was correct was the star's extreme intrinsic faintness. It is but 1/80 as bright as the Sun, and all such faint stars appear to be yellow or red.<sup>97</sup>

Russell expressed no doubts that faint stars such as these were at the end of the evolutionary series. Concerning his arguments from the statistical correlation of "age" with number of observed binaries and their periods, Russell sought further information:

> Have you any evidence yet whether there are many spectroscopic binaries among them? /The faintest stars/ The proportion is of considerable theoretical importance. I would expect it to be large with a good proportion of short periods.

By December, Russell began receiving some reactions to his work, in addition to the letter from Lockyer already mentioned. Lewis Boss wrote from Dudley Observatory:

> I shall be interested in your article on Cosmogony, but it will be very difficult to persuade me that Helium stars are not very near the beginning of the scale. I have been paying much attention to this subject during the past three months, and I think I can add some pretty exact information from a rather novel point of view.<sup>98</sup>

What Boss had in mind has not been followed up, but the three month interval most certainly refers to the time since the Harvard meetings.

After Russell received Pickering's 20 December letter,<sup>99</sup> wherein o<sub>2</sub> Eridani was classed as an A star (which gave both Russell and Pickering something of a headache, as we shall see), Russell responded discussing his use of all the spectroscopic information he had at hand for visual binaries. Briefly, "... most of the differences in the masses completed for various systems arise from accidental errors in the parallax determinations ..."<sup>100</sup> He had found that in 21 out of 26 cases studied by Aitken directly, he (Russell) could predict parallaxes, assuming combined masses of 2.4 that of the Sun, to "... within the limits assigned by the observed probable error ..." of the measured parallaxes themselves. He thus concluded that "Double stars are evidently far more alike in mass than in any other characteristic ..." and, hence, could yield distance and absolute luminosity information as well as parallaxes could.

Thus Russell became more and more convinced of the great value of 'hypothetical parallaxes', which he was soon to discuss in print. The simple fact that he could predict parallaxes to within observed probable errors for some systems was exciting, as was the

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realisation that now his observed relation of faintness with redness could be extended to binaries. Most of his stars behaved quite well, as he told Pickering many times. But for  $o_2$  Eridani and another system,  $\mu$  Herculis:

> They are not more than 1/1000 as bright, in proportion to their mass, as the least brilliant of the other binaries of spectra A or A2 and so I rather hope that it will turn out that there has been some confusion in the case I spoke of for otherwise  $o_2$  Eridani will be a very tough nut to crack theoretically.

Russell was quite right on this. It was to be some time before white dwarfs were to be understood. Russell and most others, as a result, ignored their presence, and hoped they would go away, one way or another.<sup>101</sup>

In the conclusion to Russell's long letter to Pickering, we see an early inkling of the all important mass-luminosity law that became observationally detected in subsequent years, but was not to be explained theoretically until Eddington's treatment in the twenties:

> There are a lot of slow binaries of the 61 Cygni type of this order of intrinsic brightness  $\sqrt{o_2}$  Eridani/ and with big parallaxes. It is possible to get at the average mass, though not the individual values, in this case, and I find that it is about 0.8 times that of the Sun. Campbell's spectroscopic binaries (mostly of spectra A and B) have an average mass about 9 times that of the Sun, and their brightness must be three or four hundred times the Sun's. So there appears to be a regular progression of mass with brightness, but at a relatively very slow rate.<sup>102</sup>

By the new year, Russell began corresponding with Schlesinger, A.S. Flint at Washburn, Campbell, Kapteyn, Lockyer, and others about his ideas. Kapteyn wrote in March, 1911, that he set a high value on Russell's parallax work and on relations between parallax and spectrum.<sup>103</sup> Shapley was to note later, however, that Kapteyn did not care much for Russell's evolutionary interpretations.<sup>104</sup> In this note, Shapley

mentioned that he had been discussing evolution with Hale, who was very much interested. Campbell, in June, noted "... Your publications have been fine ...."<sup>105</sup>

During 1911, Miss Cannon took over Mrs. Fleming's work and continued to provide Russell with the data he requested. In early January, 1911, Russell informed Pickering that "The work on the masses and brightness of double stars is leading to some very interesting results ...."<sup>106</sup> He was now ready to employ hypothetical parallaxes:

> The relation between a star's spectrum and its 'hypothetical magnitude' (i.e. its true brightness if its mass has the average value) seems to be sufficiently uniform to make approximate prediction possible.

Russell noted that usually he was able to predict the spectrum of a star to within one spectral class. With this new technique, he felt that he was able to double "... the available information about stars of class K, and make it possible to give a good value for the relation of mass and brightness for this class as well as A, F and G". He also indicated that the relation held for stars of the Orion type, too, but declined to give examples. He then turned to densities and surface brightnesses for Algols, concluding that

> ... the surface brightness must be of the order of 10 times that of the Sun ... I think that this is the first direct evidence bearing on this important question. It will probably be possible, with the aid of the Algol variables, to get approximate values for the surface brightness of the A and F stars, too.

By June, 1911, Russell had sent what he felt was his final request for spectral classifications of binary systems. He noted that on another enclosed sheet, he included what he believed their spectra should turn out to be, by his predictive method "... based upon the relation between mass and brightness which holds good among the binaries so far investigated - except o<sub>2</sub> Eridani. It will be interesting to see how well the predictions turn out. They are not to be expected to be good within one class".<sup>107</sup> Within this letter is apparently his earliest use of the famous terms "giant" and "dwarf":

> I hope to send you soon a list of "giant" and "dwarf" stars - i.e. stars of the same spectral type but very different luminosity per unit of mass. - which may be of interest to Miss Cannon.

Russell then suggested that he talk on the subject of giants and dwarfs at a meeting of the Astronomical Society to be held in August, 1911, in Ottawa. He noted:

> I think that the matter of giant and dwarf stars would be the best thing I could present. It is new, has important consequences, and is not controversial if the bearing on stellar evolution is not emphasized unduly - and I think it is interesting. I have got the evidence in pretty conclusive shape, and it can be presented in reasonable time.

As a final note in connection with this line of work, Russell noted Hertzsprung's work ("... in the last Potsdam Publications ..."). which showed that "... the fainter stars of the Pleiades, Hyades etc. are progressively redder ... ", and felt that the work was "... of much importance ... " He added, predictably: "I should like to see their spectra investigated some time, but I would not ask for so much now when I know you are very busy and short-handed". There is little question that Russell was referring here to Hertzsprung's first published diagrams. As Hertzsprung apparently did not discuss "giants" or "dwarfs" in this paper (dwarf - Zwerg; giant - Riese), one still wonders where Russell got the ideas for the names, or whether, in fact, he himself created them. This might be supported by the fact that Russell felt compelled to place the terms in quotation marks and provide explanatory remarks for Pickering's use, but then did not continue the practice the second time he mentioned them. We shall see that by August, 1911, the date of the Ottawa meeting, Russell was to attribute
the term "giant" to Hertzsprung (presumably from his use of the term "whale" in 1909), reserving the term "dwarf" to either an anonymous origin, or an origin within his paper.

As Russell continued to prepare his binary work for discussion at the Ottawa meeting, he maintained his usual non-stop correspondence with Pickering. By late June, he had received all the spectra from Cannon and Pickering, and was quite happy to see that his "... somewhat bold attempt to predict the spectra from the relation between mass and brightness has met with success".<sup>109</sup> By this time, he had modified his main theme for the Ottawa meeting to concentrate on his double-star work, though giants and dwarfs would still play an important role. Russell expressed some concern that his presentation was long for an ordinary paper, and hoped that Pickering would be able to manage more time, and a good position on the programme for him. In this letter, Russell indicated that he was looking forward to a visit from Kapteyn, who was scheduled to arrive later in the day.

Through correspondence, Russell learned that Frost was not planning to attend the Ottawa meeting. Russell was disappointed, possibly since Frost by this time was the primary editor of the <u>Astrophysical Journal</u>, and so wrote him a long letter reviewing his paper.<sup>110</sup> Russell expressed the distinct hope that his talk would be published in the journal, but, in fact, it appeared in <u>Science</u>.

His paper "A Study of Visual Binary Stars" centred upon his predictive distance method, now directly referred to as "hypothetical parallaxes", though there was much on his studies of the relationship between density and surface brightness for Algols. His first conclusions were emphasised:

> It is evident that the binary stars are much alike in mass, and that the assumption of equal masses gives very good approximations to true distances ...<sup>111</sup>

351.

Turning to surface brightnesses and densities, Russell derived his method for determining their relationship without recourse to the distance of the system. The relation showed a definite massluminosity relation, but Russell wished to emphasise it as an indicator of giant or dwarf conditions.

Within each spectral class, the variation in brightness and mass was small, as derived from his tests for systems with independently derived orbits. Turning his analysis around, he was then able to predict masses for large statistical samples of "... all available physical systems /common proper motion pairs/ brighter than the sixth magnitude ... raising the whole number of stars discussed to 349 ....<sup>112</sup>

> ... for about half these stars, the relations between mass and surface brightness are very similar to those already found among stars for which orbits have been computed ...

The other half, which included all B stars in a general distribution of all classes, were

very much brighter in proportion to their mass than those previously studied ...

Russell identified this latter sample as belonging to the giant class, and the former sample to the dwarf class:

These stars are probably similar to the stars of great luminosity to which Hertzsprung has called attention under the name of "giant stars". The others may be called "dwarf stars". In type A the two kinds run together, but among the redder stars they are more and more widely separated, though a few intermediate cases exist.

Russell had grouped his stars according to similar spectrum, and had found that mass did not vary widely. He thus established the fact that the distinction between giants and dwarfs was not one of mass; though, at the time, he was not absolutely sure of this. He still had to separate mass from surface brightness. In attempting to do this, he noted that for giants, the "relation of mass and brightness is much the

same for all spectral types ... ", but, among the dwarfs, the brightness fell off rapidly and the mass somewhat more slowly, with increasing redness. This meant that the product of density and surface brightness rose rapidly with advancing spectral type, which he found to be the case from his binary work. Since with advancing redness, surface brightness diminishes, densities therefore had to increase. Russell already knew that giants had small densities, and that mean densities increased with redness down the dwarf sequence. These considerations helped him to find a way of separating out mass and surface brightness, though at the time he preferred to rely upon binary systems with known parallaxes for masses. He found that, as a group, the giants were about ten times more massive than the Sun (taking the combined system mass) and the dwarf systems ranged from three times the solar mass for type F, to less than the Sun's mass for a K type system. He next examined, as separate groups, the luminosities of giants and dwarf systems, and concluded that "... It appears therefore that the more massive stars are by far the brightest ...."113

Finally, to separate densities from surface brightnesses, he turned to the Algol systems already analysed. From them, densities were directly determined, which allowed him to compare the surface brightnesses of giants and dwarfs of the same spectral class. For the A stars, he found the values quite similar, though the data were meagre, and obviously had to be supplemented with more observations of eclipsing systems. The values of surface brightness he obtained, however, agreed "... closely with those derived from the work of Wilsing and Scheiner ...", and allowed for an "... independent confirmation of the hypothesis that the effective surface temperature of a star is the principal factor which determines its spectral type".

With this confirmation of Wilsing and Scheiner's spectro-

photometric work, Russell then reversed his method, using their values, to go back and determine densities for his systems directly from spectra, "Assuming that the surface brightness of giant and dwarf stars of the same spectral type is the same ..." He found that "... the mean density of the giant stars increases steadily with <u>decreasing</u> redness from less than 1/10,000 that of the sun for type M, and 1/1,000 for type K, to 1/8 for type B. That of the dwarf stars increases with <u>increasing</u> redness ...."<sup>114</sup> This was indeed a beautiful result, for not only was he able to show great differences in density between the giants and dwarfs, but was also able to show that his line of evolution was a continuously increasing line of density. Thus he concluded:

> It may be noted that all these facts (except the existence of one very faint star of type A) are in harmony with the scheme of stellar evolution sketched by the writer last year.<sup>115</sup>

More work on eclipsing systems was necessary to verify the density relationship, since only the A stars behaved well, the others being too insufficiently examined to allow for definite values to be assigned to each of them. Thus, the priority was for work on eclipsing binaries.

In the beginning of the Fall semester at Princeton, Russell gained a new student - Harlow Shapley. Within the following several years, Shapley was to observe over eighty eclipsing systems at Princeton,

and provide confirmatory data on stellar densities, as part of his doctoral research with Russell.

During the latter part of 1911, Russell corresponded with Pickering as new systems were studied. Pickering had followed up Russell's earlier request to acquire the spectra of stars in the Pleiades and Hyades - stimulated by Hertzsprung's papers in 1911 - and, by March, 1912, had completed the task. This time, Pickering evaluated the data as he presented them to Russell:

Miss Cannon has at long length classified the spectra in the Pleiades and the Hyades which you wished. The relation of spectra to magnitude is extraordinary.<sup>116</sup>

During this period, the continuation of Shapley's binary work was Russell's main line of evidence, especially since, by early 1913, Russell reported to Pickering that Shapley's work: "... seems to be the most conclusive evidence in favor of my notions about stellar evolution that I have got yet ...."117 But from Russell's interest in confirming Hertzsprung's diagrams for the Pleiades and Hyades, which most certainly followed what he had found about spectrum and brightness, we see that Russell maintained projects along many different lines. As an outgrowth of this confirmatory work on Hertzsprung's clusters, Russell became keenly interested in calibrating colour indices, spectral classes, and surface brightnesses in terms of black body relations. When in late 1912, J.A. Parkhurst published his "Yerkes Actinometry" in the Astrophysical Journal, Russell wrote to him plying him with questions on colour indices. Russell had noted differences between the photovisual and photographic magnitudes for components in his binary systems, and felt that information like this could be used to:

> ... determine from observation the relation between color index and absolute surface brightness, and then to estimate the actual diameters of all the stars whose parallax and spectrum is known. /It/will also be able to apply a further test to the question how closely the radiation of the stars approaches that of a black body. In this connection it is of vital importance to know just what the published color indices really mean; and this is my excuse for writing you such a long letter.<sup>118</sup>

Parkhurst was to provide explicit discussions on how he derived his indices, which then allowed for their calibration. What is fascinating here is that Russell immediately saw the value of such calibrations in the wholesale study of stellar dimensions. He had unwittingly rediscovered Hertzsprung's 1906 method, and Pickering's 1880 method (which he was later to acknowledge) for predicting the angular diameters of stars.<sup>119</sup>

By this time, Hertzsprung had spent considerable time at Mount Wilson, coming there initially with Kapteyn, and expressed his enjoyment in discussing Russell's work with Shapley. Hertzsprung noted that, while their treatments were similar, their results for absolute magnitudes seemed to differ - by almost five magnitudes, according to his calculations. In this regard, Hertzsprung noted that, from H. Leavitt's work and his own, he suspected that the absolute magnitudes for Cepheids and RR Lyraes were quite different, and that the latter had values similar to the Sun. Dropping this tentative line for the moment, Hertzsprung turned to another topic he and Shapley discussed:

> Mr. Shapley also told me a little about your investigations on stellar evolution. The idea, that the life of a star embrassed <u>/sic</u>? twice the spectral series, from red to white as a giant and from white to red as a dwarf, was one of the first, which came to me, but I have not printed anything about it, because I failed as I do still now, to find any evidence of its being correct. I shall be very glad to know, which evidence you might have found, as there may be some serious objections to that view. For instance:

1. How do you explain, that in some clusters (Hyades and Praesepe - double stars like  $\gamma$  Andromedae and  $\epsilon$  Bootis show the same phenomenon at a smaller scale) we have two kinds of stars supposed to be of the same age, first a few bright yellow ones ... secondly the rest forming the usual solar series (commencing in the cases of the Hyades and Praesepe with stars of A2 about) and nothing between! Where are all the connecting stages?

2. The bright yellow stars have just as the faint yellow stars great peculiar motions, and the Helium stars small ones. How to explain, that the peculiar motion is first great, then small and then great again in the course of star life? How this may be, there is one consideration, I should like to see tried, namely that there may be stars, which at their highest are like Arcturus or Aldebaran, and never reach the white stage. With other words, that there are rather different series of development for a star, perhaps well separated without intermediate series, so that a star may fall into one or another.<sup>120</sup>

Hertzsprung closed his letter hoping to see Russell in Bonn in the near future, so that they could continue this discussion. Hertzsprung was writing from Potsdam, and this letter was a duplicate of one he had previously sent; but for some reason, Hertzsprung sent the second one, too, for he feared that the original copy, written from Mount Wilson, was lost.

En route to the Bonn meetings, Russell stopped, along with the American contingent, for a short stay in England, and delivered a paper before the Royal Astronomical Society on his work. In print, it remained obscure, but during his stay he was able to meet with Lockyer, who wrote to him at his hotel in London expressing delight that they were to meet the following day, the 18th of June. Clipped to this letter from Lockyer<sup>121</sup> was a set of graphs and diagrams which bear striking resemblance to what would be expected for early forms of Russell's 'Diagram'. They have not been evaluated, as yet, but offer the fascinating possibility of having been amongst Russell's papers which he took to Lockyer on that day.

Pickering had just seen Lockyer on a previous day and had looked for Russell, hoping that he would accompany him for the visit. Afraid that Russell was not going to be able to see Lockyer, Pickering wrote to him noting that Lockyer was anxious to see him and discuss stellar spectra. This was on the 15th, two days after Russell had delivered his address "Giant and Dwarf Stars" before the RAS.<sup>122</sup>

This paper was, of course, a summary of all aspects of

Russell's work. The original talk evidently included a number of diagrams, but from the description given in the text of Russell's address, it doesn't seem as if the graphs and tables found in Russell's papers were among them. Significantly, in describing the first graph he had shown on the screen, Russell noted:

> The vertical coordinates give the spectra, and the horizontal the absolute magnitude according to Kapteyn's definition.<sup>123</sup>

This was Hertzsprung's format (even though Rosenberg's had the conventional orientation), and most certainly could have described the format implied by Lockyer's temperature arch. Russell gave full tribute to Hertzsprung: "These series were first noticed by Dr. Hertzsprung, of Potsdam, and called by him "giant" and "dwarf" stars. All I have done in this diagram is to use more extensive observational material".<sup>124</sup> Russell also showed slides of diagrams of clusters. On one, it was evident that he combined four moving clusters whose distances had been determined: Hyades, Ursa Major, 61 Cygni and the controversial Scorpio-Centaurus group, independently discovered by Kapteyn, Boss and Eddington.

Apart from the clusters and diagrams, most of the rest of the paper was a review of material we have discussed previously, though its impact was considerable for it brought all the information at hand together. His discussion of evolution was not too extensive, and began in this interesting fashion:

> As almost everybody will agree that a star contracts as it grows older, this leads us to suppose that the giant stars of Class M represent a very early stage of evolution ...

The phrase "As almost everybody will agree ..." bears much significance, for indeed it was the only known course of evolution, based upon gravitational energy alone. Russell's discussion of the theoretical side yielded nothing new save for an indirect reference to the assignment of a value for the critical density/maximum temperature point in a star's life:

Lord Kelvin some years ago estimated that the maximum temperature would be reached when the central density was about one-tenth that of water.<sup>126</sup>

Russell also noted that "It is less well known, but equally easy to prove, that the more massive the body of gas is, the higher will be its maximum temperature". He observed that "... a crucial test ..." of the theory had been that the densities of stars of classes A and B were about what Lord Kelvin had assigned for the maximum temperature condition. Russell then employed Hertzsprung's recent work as a possible solution to objections that might be raised. It contained, indeed, what Russell had considered to be an important ingredient in his theory for several years:

> Only bodies of unusually large mass should reach the very highest temperatures, so it is not surprising to find the hottest stars (Class B) are actually unusually massive. Again, a body of very small mass would be a very poor "selfheating" affair (as one of my students once put it); and this gives a reason for the rarity (indeed the apparent absence) of stars of very small mass - such bodies never get hot enough to shine of any account.

The fact that Jupiter and Saturn, though comparable in density with a number of the stars, are dark bodies confirms this explanation.<sup>127</sup>

In concluding his highly compact address, Russell hoped that "... anyone who has any criticisms to make will do me the favour of telling me them ..."

At this point in our discussion, we might ask: What role in Russell's work did the actual construction of a diagram take? As we have seen, Russell's first diagrams, as presented to the RAS, were similar in format to Hertzsprung's. Amongst these was one Eddington wished to publish in his book <u>Stellar Movements and the Structure of</u> <u>the Universe</u>. When it appeared, however, it had been transformed to the usual format with luminosity vertical and spectral class horizontal, as were the graphs and tables clipped to the letter from Lockyer to Russell in 1913. The diagrams that appeared in Russell's most extensive discussion were also in the modern format. How, or why, they changed in the interim is not known.<sup>128</sup>

It is clear from his discussions in 1913 that the diagram was an aid in distinguishing giants from dwarfs. Hertzsprung, too, from his letters to Russell, seemed to have this in mind, since one of his criticisms was that gaps existed between the two series, and it was hard to find transition types. These problems also appeared in Hertzsprung's 1909 paper.

## Criticisms of Russell's Interpretation of the Diagram

In addition to Hertzsprung's remarks, Eddington reacted critically in a letter fifteen days after Russell's address to the RAS. It was sent presumably to Russell while in Bonn attending the Solar Union meetings, and Eddington reported that Russell's paper would be delayed until a later edition of the <u>Observatory</u>. Eddington's arguments included considerations from peculiar velocity distribution studies, and, hence, were similar to Hertzsprung's comments.

Eddington's first public criticism of Russell's theory came in December, 1913. While it was quite attractive in terms of the Lane-Ritter theory, and, indeed, had many merits, Eddington felt that "... it cannot be accepted without overthrowing a great deal that has been considered firmly established".<sup>129</sup> Noting that only giant M stars had entered into the statistical discussions in previous works by himself, Kapteyn and others, the order of evolution would have to be

reversed. He then listed several objections that would arise from this:

 ... the steady increase in the periods of spectroscopic binary systems with advancing type is a strong argument for the usual order ...
 Instead of the stars being formed in the

galactic plane and spreading away from it with advancing age, the <u>earliest</u> stars would be nearly uniformly distributed, and in the later stages they must gradually concentrate into the plane ...

Instead of starting with very little motion and gradually increasing in speed, the stars must start with large velocities and gradually become reduced nearly to rest ...
the Dwarf M stars i.e. the latest type of all, are found to have very large velocities, exceeding the Giants on the average; thus the decrease of speed proceeds only up to a certain point and afterwards a great increase takes place ...

Expanding upon the last objection, Eddington felt that since both the dwarf and giant M stars had large velocities, he could argue for their essential unity. This was the main objection drawn by Hertzsprung in his letter to Russell in June, 1913.

Eddington's final statement read:

There are other problems arising from the relation of spectral type to stellar motion - in particular, those regarding the phenomenon of star-streaming. But the above discussion will show that, without entering into more complex problems, the comparatively elementary facts are full of difficulty.

It seems as though those who had the greatest objections were those chiefly involved in stellar motions. In a letter from Shapley to Russell, we find that Kapteyn was "skeptical"<sup>130</sup> of Russell's ideas. Lewis Boss, too, was worried about the effect on the two-streams hypothesis, as were Campbell and Eddington. Boss worried about the technique of hypothetical parallaxes, and how this might be affected by star streaming, since it would cause common proper motion pairs to occur with frequencies greater than would be expected from random motions, and would be a function of age, if treated from Kapteyn's view. In general, Boss was confused: "As to the two kinds of red stars I cannot really understand how you can get them; nor how you could recognize them".<sup>131</sup> In reply, Russell dealt more with the question of the reality of the two streams than with anything else, noting that the implication for evolution of the two streams was dependent upon their reality, and that recent work by Eddington, Halm, and Schwarzschild (two stream, three stream, and ellipsoidal hypotheses respectively) seemed to suggest that the origin of the stellar components of the universe was singular:

> There seems to me therefore to be not the slightest reason for treating the stellar universe as consisting of two parts of different origin. Your work on the Orion stars is of course the most conclusive evidence for the existence of one and not two nuclei: but I don't see that there is any evidence at all on the other side.<sup>132</sup>

This produced a reaction from Benjamin Boss, Lewis' son, who had long been advocating the similarity between the three streams suggested by Halm and the ellipsoidal hypothesis, and who was happy to see that Russell was coming round to the idea, too.

Russell answered Eddington's objections in the April issue of <u>Observatory</u> in a paper entitled "On the Probable Order of Stellar Evolution".<sup>133</sup> Eddington, at the outset of his objections, asserted that the reality of the separation of the red stars into two classes was doubtful, since it could easily be due to some selection effect. Russell took this up first, stating that his parallax work, and much of the work on proper motions, was done without any recourse to spectra or colour. He then proceeded to show that his analysis indicated real differences, though a sharp distinction existed only for Classes K5 and M.<sup>134</sup> (The possible selection effect caused by Russell's interest in Lockyer's stars has not been examined.) Russell preferred to emphasise his own objections to the classical view of the course of evolution rather than answer Eddington's criticisms as outlined above directly. In this regard, he turned upon the conventional view, supposing that the cooling process would cause a rapid diminution in brightness. This was observed for the dwarf sequence, but not for the giant sequence. Quite obviously, from a neutral standpoint, this objection would not have arisen, since it required the acceptance of the existence of giants, which Eddington apparently was not ready to do.

As his second objection, Russell commented:

Again, no one supposes that the stars of Class B were <u>created</u> hot. They have doubtless originated in some way or other from more widely diffused primordial matter. Such primordial matter, if any still exists, must be relatively cold, for it is not luminous. Between it and spectrum B there must be intermediate stages in which the mass is growing hotter, while its light is yellow or red, and, on account of its greater size, is much brighter than in the later stage of its history when it reaches the same temperature while cooling off.<sup>135</sup>

Of course many in the past, especially Vogel, had simply assumed that the primitive stages of contraction were invisible. To Russell, however, there could be only three possible explanations:

1. Star formation had ceased in recent cosmological time, and these early stages no longer exist.

2. The rate of evolution through these stages is very rapid thus causing the number observed at any one instant to be extremely small.

3. Stars in such stages exist, and we see them. If this is true, the giant stars have just the characteristics which they might be expected to show.<sup>136</sup>

Predictably, the third explanation was considered by Russell to be far more probable than the first two.

Russell's third objection was based upon the densities he had

derived for stars of the B and A classes, which were "... far removed from a primitive state ..." He referenced Shapley's results for eighty eclipsing binary systems<sup>137</sup> which gave densities for stars of known spectral type. Russell indicated that the majority of the B, A and F stars "... are denser than the Sun would be if expanded to only four times its present diameter ..." Over half of the systems studied with spectra between F8 and K had densities 1/1000 that of the Sun. "The periods of these stars are relatively long, but the light curves are perfectly normal in character, and there is not the slightest reason to doubt their interpretation in terms of the eclipse theory. It is evidently to such stars, and not to those of Classes B and A, that this important evidence points as being in the early stages of evolution ...."<sup>138</sup>

His fourth objection dealt with spectroscopic binary statistics, and was a direct answer to Eddington's first criticism, as indicated above:

> The steady increase in the proportion of spectroscopic binaries, from the redder to the whiter stars, and the prevalence of short periods among Classes B and A, and of long periods in Classes G, K, and M, is in reality strong evidence against the conventional view of their relative ages. It is often stated that the longperiod systems of the 'later' types represent the results of the tidal evolution of short-period systems of 'early' type. But it is very easy to prove that, while a great increase of period may be produced in this way in systems whose components are very unequal in mass (as in the case of the Earth and Moon), the period cannot be greatly increased by tidal action if the two components have comparable masses.139

This extremely important, but much misunderstood, element in binary evolution had been discussed before by Russell in private correspondence. The persistence of confusion on this point was later to be seen in Campbell's interpretations, and in the work of others. To bolster his position, Russell referred to his own work and noted: "This proposition is implicitly contained in Darwin's original equations, and attention has been specifically called to it by Moulton ... and by the writer". After pointing out what type of statistics would be expected on the former interpretation, and showing that they did not exist, Russell concluded this objection with his views:

> Out of a large number of contracting masses, possessed of different amounts of angular momentum, more and more will break up into binary systems as the process of contraction goes on, and those formed later, when the density is higher, will be closer and will have shorter periods. The prevalence of large numbers of such systems among the stars of Classes B and A is therefore a cogent reason for supposing them to represent a relatively late stage.

Russell had sent his article to Eddington for examination, but since it was sent so close to the publication deadline for the next issue of the <u>Observatory</u>, Eddington had only a short time to glance at it. Russell's arguments concerning spectroscopic systems had an effect, as Eddington replied:

> I see your point about spectroscopic binaries. I suppose the angular momentum is a difficulty. Yet I cannot help a sort of instinctive feeling (in vulgar phrase - prejudice) that (a) double stars come about by fission (b) increasing distance means increasing evolutionary stage whatever the cause. Perhaps tidal friction must be definitely excluded ...

Eddington also noted that he was glad that Russell had replied to his remarks in the <u>Observatory</u>, and that it was a "... very interesting problem ..." Eddington continued:

Really, I can't make up my mind about it; except that there is something more to be found out. I do not think I shall need to alter what I had written in the book, it is perhaps a little more impartial than my brief remarks in the Observatory.

Russell's fifth and last argument was his strongest - that stars of class B were most certainly more massive, by at least a factor of three on the average, than stars of other types. "Unless we are prepared to admit that a star loses two-thirds of its mass as it grows

older, we must regard the stars of Class B as a selected group, not typical of the stars in general".<sup>142</sup> As before, he then reviewed the Lane-Ritter theory, emphasising that only the greatest masses would heat to the B star stage. In conclusion, to these objections to the conventional theory, Russell observed:

> The combined force of these four arguments makes the conventional view of the evolutionary order of the various spectral classes appear to the writer to be simply untenable, - quite apart from the question whether a better substitute is now available.

Russell then turned his attention to peculiar velocities, noting immediately that, even on the conventional theory, "... the variations in the mean peculiar velocity from class to class are very puzzling, and seem to point to some accelerating force of unknown nature, which transfers enormous amounts of translational energy to the stars as they grow older".

Russell's alternative was to refer to his fifth objection, which pointed out the possibility that the B and A stars had to be treated as special cases, reached only by massive stars. In this way, one could easily see that a correlation between mass and velocity existed. "Only the most massive stars will attain the B - type of spectrum, and the average velocity of these will be small".<sup>143</sup> Russell ingeniously discussed the effect of this selection by mass, noting that, for the B and A class, both giants and dwarfs are mixed. Thus, while the B stars are the most massive and the slowest, the A stars will be a combination of massive stars on their way to becoming B's, and less massive stars experiencing their maximum temperatures at A. This mixing of less massive stars would cause the mean peculiar velocity of the A's to be greater than for the B's, as observed by Russell, through the continued efforts of Campbell at Lick. Russell explained: The increase of mean velocity should be considerable from class to class, so long as each new class includes a considerable proportion of stars near their maximum temperature, and therefore intermediate between the typical 'giants' ... and 'dwarfs' ... If the number of such intermediate stars is insignificant, i.e., if the giants and dwarfs are well separated, the mean mass and mean velocity should nearly be the same as the preceding class, 144 and about the same for the giant and dwarf stars.

In effect, with advancing spectral class, a smaller proportion of stars was expected to be at their maximum temperature; thus, the effect of mixing would be diminished. Using Campbell's radial velocity data, Russell showed that this was the case: the increase in mean velocity from class to class becoming less and less with each advancing type. Eddington must have taken a very long look at this argument, for he was on record in his book as saying that the increase in velocity from B to A and possibly to F was significant; but that, even though "... The velocities for F, G, and K come in the right order ... it would be straining the figures too far to attach much importance to this ..."<sup>145</sup> Russell was able to show, by his analysis, why this small increase among the later types was significant:

> So far as the writer knows, no previous attempt has been made to explain this singular progression in the increments of mean velocity from type to type.<sup>146</sup>

He did not attempt to discuss why the velocity of less massive stars should be greater, though he was undoubtedly aware of J. Halm's arguments from the standpoint of equipartition, for he was quick to argue:

> It may, however, be remarked that a correlation between mass and velocity, especially in the sense here assumed, seems more probable than one between temperature and velocity, or velocity and age.

We recall that Kapteyn's argument was for an age effect (based upon diminishing resistances with decreasing radius), though it was presented in a highly speculative manner. Russell noted briefly at the end of his argument that, contrary to Eddington's (and Hertzsprung's) observation that M giants, too, had high peculiar velocities, recent studies (by Boss and Hertzsprung) were yielding examples of giants with low velocities.

Hertzsprung's and Kapteyn's study of the mean parallaxes for 45 of these stars yielded an extremely small value of 0".002, which, since their mean apparent magnitude was 4.5, indicated stars of great luminosity. Russell found 24 of these stars in the spectral classification lists of Harvard to have spectra of F and G; and from the radial velocities determined by Campbell, and the proper motions by Boss, he concluded that their peculiar velocities were also very small. In addition, concerning spatial concentration and distribution, he answered one of Eddington's criticisms

> Both the "c stars" and the Cepheid variables are strongly condensed towards the Milky Way, thus resembling the stars of Class B in yet another of their most striking characteristics.<sup>147</sup>

From the arguments thus presented, Russell felt that while his own remarks "... should not be regarded as in any sense a logical or complete presentation of the provisional theory of stellar evolution here described ..." they should, at least, make it quite clear that the conventional theory was no approximation at all to the truth.

As we have said, Eddington and many others were to be swayed eventually by the power of Russell's arguments, and most became wholly converted. Yet, from the study thus far completed, it appears that some simply refused to consider seriously Russell's alternatives. We use W.W. Campbell as an example, because of his influence and his character. During this period, Russell and Campbell corresponded on the interpretations of binary data, and what would be expected from various theories of binary evolution. Campbell could not believe the low densities Russell and Shapley were finding and admitted in correspondence:

> I find myself resisting the view that G and K stars can be so lacking in density, and inclined to the belief that we shall find some other factor which will relieve us of that necessity. Just what that unknown factor may be, I cannot predict. Of course, your results for the densities quoted may be quite right. My point of view is a feeling which is not supported by direct evidence, and hence is not entitled to much weight. Pretty definite ideas as to the course of stellar evolution are at the bottom of my trouble.<sup>148</sup>

Campbell's attitude is significant, in that he was soon to be chosen by Hale to represent the astronomical community in a series of lectures by prominent scientists on aspects of evolution, sponsored by the National Academy of Sciences. Rutherford and T.C. Chamberlin were also scheduled to participate. Hale's diplomacy was quite evident in his planning of the series, which, in his mind, was to highlight the great advances recently seen in astronomical and atomic physics.<sup>149</sup>

Campbell had been secured by March, 1914, well after Hale had become familiar with Russell's new ideas. It is clear then that Hale wished to take the established course for review.

When Campbell was asked to give the lecture, he contacted Chamberlin in Chicago to know his latest thoughts on the Chamberlin/ Moulton Planetesimal hypothesis, which Campbell wanted to represent in an up-to-date and fair manner. He apparently did not ask the same courtesy of Russell, and even received a letter from the latter, asking gently that his theory be represented. In a letter in late May, 1914, Russell sent a collection of his papers to Campbell:

> They may perhaps be of interest to you since you are to lecture on stellar evolution before the National Academy next Fall, and these papers give for the first time enough details about the notions which I have been advocating to enable you to criticise them satisfactorily.

Of course I am not asking you to pay more attention to them than you think they deserve: but there is one phase of the evolution question that I would like to commend to your consideration before you write those lectures; - namely, the question of the relation between visual and spectroscopic binaries.<sup>150</sup>

Russell noted that "... There has been a great deal of discussion based on the view that spectroscopic binaries, as time goes on, may evolve, by tidal lengthening of period, into visual pairs, - or at least into periods as long as those of some visual binaries. I think that Moulton was the first to call attention to the grave mathematical difficulties of this assumption ..."

Russell then provided several pages of extensive arguments to show that tidal evolution cannot, except in extremely rare cases, change spectroscopic binaries into visual pairs. However, evolution within each type was possible.

Campbell gave his lectures before the National Academy of Sciences in December, 1914, and no record has been found of a personal reply to Russell.

Campbell presented the conventional view of evolution, which need not be repeated here. In paper III (November, 1915), he began to provide evidence in support of this conventional theory: the association of bright line stars with nebulae; the progression of velocities with spectral type, which he had played a large part in finding; statistical observations of double stars as a function of spectral class, showing that observed numbers increased to a maximum at classes A-F, which was interpreted as tidal evolution on a large scale; and, finally, Kapteyn's two star streams, which implied an age differential.

Campbell felt that "The eight lines of evidence <u>Abstracted</u> above outlined are in harmony to the effect that there is a sequence of development from nebulae to red stars". At this point, he mentioned

## the detractors from the classical view:

It should be said that a few astronomers doubt whether the order of evolution is so clearly defined as I have outlined it; in fact, whether we know even the main trend of the evolutionary process. We occasionally encounter the opinion that the subject is still so unsettled as not to let us say whether the helium stars are effectively young or the red stars are effectively old. Lockyer and Russell have proposed hypotheses in which the order of evolutionary sequence begins with comparatively cool red stars and proceeds through the yellow stars to the very hot blue stars, and thence back through the yellow stars to the cool red stars.

I think the essentially unanimous view of astronomers is to the effect that the great mass of accumulated evidence favors the order of evolution which I have described. We are all ready to admit that there are apparent exceptions to the simple course laid down, but that these exceptions are revolutionary in effect, and not hopeless of removal, has not yet, in my opinion, been established.<sup>152</sup>

It certainly can be questioned whether Campbell's statement that there was unanimous approval of the conventional theory by 1915 was correct, as we have shown that many became seriously interested in Russell's revision. But as we have seen, Campbell possessed deep convictions about the evolutionary process, as he admitted to Russell. His feelings about tidal evolution of binaries were equally strong. The only other point within his paper where he mentioned Russell was in a discussion of the effects of tidal action in stellar and planetary systems. Here, Campbell was more receptive to the possible existence of another, yet unknown, force to account for binary separation than to accept the arguments of Moulton (that tidal forces were not sufficient to produce observed separations) and Russell, which indicated that these separations were not primarily evolutional, but due to initial separations upon fission.

Campbell's orthodox views were not nearly as sympathetic to

Russell as others reviewing stellar evolution at the time. Fowler, Eddington, and Waterman (a graduate of Lick) were far more impressed with Russell's arguments. Other studies have shown<sup>153</sup> that Campbell was a man with strong opinions and a disciplined belief in the objective analysis of data. To this end, it is important to note that nowhere in his paper did Campbell mention the existence of giants and dwarfs, nor the diagram representing them.

Campbell might have sensed an uncritical growing interest in Russell's work. R.G. Aitken, Campbell's second-in-command at Lick, was very much in favour of Russell's ideas, and sided with him against Campbell in his discussion of binary evolution. Adams, at Mount Wilson, was most anxious to learn more about the two types of red stars; especially since, at the time (1912-1914), he was coming to the realisation, through working with Kapteyn, that a spectroscopic technique existed for the determination of relative luminosities, which could well be calibrated to produce wholesale absolute luminosities. R.T. Innes was not in complete agreement with Russell on the use of the Lane-Ritter laws, and wondered: "What evidence is there that stars <u>do</u> contract?"<sup>154</sup>

Along these lines, Ernest Rutherford, attending a lecture by A. Fowler on stellar evolution in 1915, wondered, after hearing general discussion by Fowler, Dyson (advocating Russell's views), Eddington, Cortie, and others:

> In listening to this discussion it seemed to me that astronomers may be proceeding too much on the assumption that the evolution only proceeds in one direction. That does not seem to be necessary. I see no reason why we should not have some stars condensing and others diverging. Consider the phenomena accompanying the appearance of a new star; there is an enormous generation of heat and rise of temperature resulting from impact, and the star must spread out into a more diffused state. I dare say that these views may sound perilously like those put forward by Prof. Bickerton; but I see no reason why the evolution should always proceed in the direction of condensation.<sup>155</sup>

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This most fascinating statement by Rutherford, whose idea was not to become realised until the 1950s, seems to have been ignored. In summarising the general discussion, Fowler commented:

> Our views as to the course of stellar evolution must necessarily depend largely upon our ideas as to the nature of nebulae ... I quite agree with the Astronomer Royal and Prof. Eddington that Russell's work demands careful consideration, but I am not yet prepared to acknowledge that the order of evolution suggested by the Draper classification has been overthrown.<sup>156</sup>

Quite an interesting conclusion for a former Lockyer assistant to reach.

In the following years, the technique of Spectroscopic Parallaxes greatly increased the number of identified giants and dwarfs. The eventual interferometric observation of an angular diameter at Mount Wilson in 1920 showed that the phenomenon of giants and dwarfs had to be accepted and reconciled with evolution. Even though this aided Russell's theory, which indeed became better accepted by 1919-1920, other elements persisted which detracted from this view: the persistent lack of association of red giants with nebulae, the mass-luminosity relationship, and Eddington's treatment of radiative transfer as the chief mode of energy transfer in stellar interiors. The third line of inquiry at first was thought to be reconcilable with the concept of the main sequence as an evolutionary track, but by the late twenties, too many difficulties arose with it, and so still other alternatives had to be found. Russell himself was still very much in the forefront of interpretation, and his exposition in his textbook in 1927 set the stage for future work.<sup>157</sup>

## References

- 1. H.N. Russell, Popular Astronomy 6 (1898), p.149.
- 2. This instance of simultaneous discovery or development in technique was to characterise much of Russell's career.
- 3. H.N. Russell, Ap.J. 10 (1899), p.315.
- 4. Letter, C.A. Young to Russell (6 September, 1899), Princeton. Young's reference to Maxwell was correct. In 1856 Maxwell demonstrated that the rings of Saturn could not be a continuous fluid or solid by deriving an upper limit to their density. Presumably, the technique he used to do this was the one Young references in his discussion.
- 5. Due to circumstances still unknown, no correspondence between Russell and Young exists in Young's collection at Dartmouth, and their letters are also quite sparse at Princeton.
  6. Russell, <u>Op. cit.</u>, ref. 3, p.318.
- 7. Roberts' paper, dated April, 1899, (Ap.J. 10 (1899), p.308), was a far more complex treatment of the problem which later drew some mild criticism from H. Seeliger (Ap.J. 11 (1900), p.247; Seeliger also mentions that it was not original). From his own observations of four Algol systems, Roberts found that their mean density was 0.13, or about one-eighth of the solar value. Roberts felt that while this result was "striking", it was not unexpected, and that the important point here was that it could act as an observational test of relations between stellar densities and temperatures. This was quite possibly an indirect reference to the controversy over the chief cause of differences in spectra amongst stars, a controversy which, as we well know, found Huggins on one side (density), and Lockyer on the other.

Russell's paper appeared right after Roberts' paper, but their treatments were quite different. Roberts attempted to derive densities for each component, while Russell preferred to assume that the two components were similar, or at least to derive his relation assuming that it was a mean density for the entire system. That their means differed from each other by a factor of two is not significant, since they used different data (except for one system common to the two from Roberts' observations) and treated far different data set sizes. It is important only to note that both showed that stars of the Algol type were far less dense than the Sun.

B. Cogan, "Henry Norris Russell" <u>Dictionary of Scientific</u> <u>Biography 12</u> (Scribners, 1975), p.17. Also from subsequent discussions with Mrs.F.K. Edmondson.

9. H.N. Russell, <u>Ap.J. 15</u> (1902), pp. 252-260.

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At the time, most methods, such as the classic one due to Lehmann-Files (<u>Astr. Nach. 136</u> (1894), p.17; Cf. Henroteau, <u>Handb. der Astrophysik VI</u> pt. 2 (Berlin, 1928), Chapter 4, p.366), were graphical procedures which depended heavily upon the exact determination of the maximum and minimum orbital velocities. Russell realised that the analytical representation of the entire velocity curve by the use of a trigonometric series would be less critical for the data and would provide a better representation.

Unfortunately, only one analytical method was available at the time, by Wilsing (<u>Astr. Nach 134</u> (1893), p.90), and it applied to small eccentricities only.

Letter, Russell to W.W. Campbell (26 May, 1902), Lick.
 Letters, A.R. Hinks to Campbell (6 November and 14 December,

1902), Cambridge Univ. Observatory.

13. Letter, Hinks to Hale (21 February, 1903), Cambridge Univ. Obs.
14. Letter, Hinks to Hale (16 April, 1903), Cambridge Univ. Obs.
15. He was later to establish, in 1926, his well known dependence method, a highly efficient graphical form of position determination.

The use of the reseau was initially intended, at the Astrographic Chart Conference of 1887 <u>See:</u> H.H. Turner, <u>The Great Star Map</u>, p.26 (London, 1912), to relieve the suspected inaccuracies of the glass backed photographic emulsion, which included distortion due to unequal development and processing (i.e. shrinkage in drying after processing).

16. Cf. Konig, Stars and Stellar Systems II (Chicago), p.463.

17. Letter, <u>Op. cit.</u>, ref. 14.

- Letter, Hinks to F. Schlesinger (17 June, 1903), Cambridge Univ. Obs.
- 19. B. Cogan, "The Origins of Henry Norris Russell's Theory of Stellar Evolution", unpublished MS, 1972, p.11.
- 20. A.R. Hinks and H.N. Russell, Monthly Notices 65 (1905), p.776.
- 21. <u>Ibid.</u>, p.779.
- 22. Ibid., pp. 785-786.
- 23. Sir Norman Lockyer, "Catalogue of Four Hundred and Seventy of the Brighter Stars" (London, 1902), Solar Physics Committee Report.
- 24. Russell, Monthly Notices 65 (1905), p.787.
- 25. Ibid., p.800.
- 26. Russell, <u>Monthly Notices</u> <u>67</u> (1906), p.132.
- H.N. Russell, "Lecture Notes for Senior Practical Astronomy",
   March, 1907, p.10. Princeton University Library.

"On the Cooling of a Perfectly Gaseous Sphere", 1901. 28. H.N. Russell Papers. Princeton University Library. 29. Letter, Russell to E.C. Pickering (4 April, 1908), Harvard. 30. B.Z. Jones and L.G. Boyd, The Harvard College Observatory (Harvard, 1971), pp. 430-431. H.N. Russell, "Edward Charles Pickering", Science 49 (1919), 31. p.154. 32. Letter, Russell to Pickering (10 April, 1908), Harvard. 33. Letter, Pickering to Russell (15 April, 1908), Harvard. 34. Letter, Lewis Boss to Pickering (12 February, 1900), Harvard. Letter, Pickering to Russell (22 April, 1908), Princeton 35. Univ. Library. 36. Letter, Pickering to Russell (29 April, 1908), Harvard. Russell, Op. cit., ref. 31. 37. Letter, Russell to Pickering (24 September, 1909), Harvard. 38. Letter, Pickering to Russell (14 October, 1909), Princeton 39. Univ. Library. Russell, Op. cit., ref. 38. 40. Bohlin, Astronomische Nachrichten 182 (1909), p.63. 41. Letter, Russell to Pickering (18 October, 1909), Harvard. 42. 43. Ibid. Russell, Ap.J. 31 (1910), p.185. 44. Ibid., p.186. 45. Ibid. See: G. Darwin, "The Genesis of Double Stars", Darwin 46. and Modern Science (Cambridge Univ. Press, 1909), p.563. 47. Ibid., p.187. Ibid., p.192. 48. Ibid., p.195. 49. Ibid., p.196. 50.

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- 51. <u>Ibid.</u>, p.201.
- 52. <u>Ibid.</u>, p.202.
- 53. <u>Ibid.</u>, p.203.
- 54. <u>Ibid.</u>, p.206.
- 55. <u>Ibid.</u>, p.207.
- 56. Letter, Russell to Pickering (7 April, 1910), Harvard.
- 57. Russell, "On the Distances of Red Stars" <u>Science 31</u> (June 3, 1910), p.878.
- 58. H. Shapley, "Henry Norris Russell", <u>National Academy of Sciences</u> <u>Biographical Memoirs 32</u> (1958).
- 59. Letter, Norman Lockyer to Russell (5 October, 1910), Princeton Univ. Library.
- 60. Rough Draft Letter, Russell to Lockyer (14 January, 1911), Princeton.
- 61. Russell, Astronomical Journal 26 (1910), p.147 ff.
- 62. Letter, <u>Op. cit.</u>, ref.60.
- 63. Letter, Russell to Frost (14 October, 1910), Yerkes.
- 64. Letter, <u>Op. cit.</u>, ref. 60.
- 65. Letter, Russell to Pickering (3 June, 1911), Harvard.
- 66. Letter, <u>Op. cit.</u>, ref. 60.
- 67. A simple check of the fifteen stars used as examples by Russell reveals the interesting fact that only <u>one</u> was to be found on Russell's own parallax lists. Further, on the branch which had stars corresponding to giants, the Maury classes went from XIVa to XVIa, all far too red to allow for any spectroscopic distinction into her subclasses, since, as we have seen, the c-characteristic ceased at about group XIII/XIV. We <u>do</u> find, however, that Russell's sample included five stars common to those used by Hertzsprung in 1906 to demonstrate the reality

of his two sequences in his March letter to Pickering. Further, four of these stars broke down into the same sets used by Hertzsprung to illustrate the luminosity differences ( $\alpha$  Aurigae vs.  $\alpha$  Centauri;  $\alpha$  Bootis vs. 70 Oph.). This is most certainly a curious coincidence, for Russell had no way to derive his two series, as illustrated, from his own work and apparently relied on Kapteyn's parallaxes. Since this was January, 1911, however, Russell had already acknowledged Hertzsprung's work. Almost all the stars used by Russell in the table could be found in Hertzsprung's papers.

68. Letter, <u>Op. cit.</u>, ref. 60.

- 69. Russell, <u>Op. cit.</u>, ref. 61, p.152.
- 70. Cogan, <u>Op. cit.</u>, ref. 19.
- 71. Russell's 1907 lecture notes (<u>Op. cit.</u>, ref. 27) revealed his acceptance of temperature criteria based upon the sunspot studies of Hale, Gale and Adams.
- 72. Potsdam Publications XIX pt. 1 (1909), p.67 ff.

73. Russell, <u>Op. cit.</u>, ref. 61, p.151.

74. At the time this was approximately photographic magnitude minus photovisual magnitude. A number of Potsdam workers used these indices to predict angular diameters of stars during this period (see: DeVorkin, <u>J. Hist. Astr. VI</u> (1975), pp. 1-18) though they were ignored by everyone else until Russell revived the predictive technique in 1919, just before an angular diameter was interferometrically measured at Mount Wilson by Michelson, Pease and Anderson.

75. Russell, <u>Op. cit.</u>, ref. 61, p.153.

76. Ibid.

77. <u>Ibid</u>.

- 78. Letter, <u>Op. cit.</u>, ref. 60.
- 79. Publ. Astr. and Astrophy. Soc. America II (1915), p.1.
- 80. Russell, <u>Ibid</u>., p.33.
- 81. Russell, <u>Ap.J. 35</u> (June, 1912), pp. 315-340; <u>36</u> (July, 1912), pp. 54-74.
- 82. Russell, <u>Op. cit.</u>, ref. 79, p.33.
- 83. <u>Ibid.</u>, pp. 33-34.
- 84. See: Kapteyn discussion, Ap.J. (1910), p.265.
- 85. Russell, <u>Science 32</u> (December 16, 1910), p.883.
- 86. A. Nielsen, <u>Centaurus 9</u> (1963), p.241.
- 87. Russell, <u>Op. cit</u>., ref. 85.
- 88. <u>Op. cit.</u>, ref. 79.
- 89. Letter, Russell to Frost (14 October, 1910), Yerkes.
- 90. Russell, <u>Ap.J. 32</u> (1910), p.363. Russell had measured the parallax of the first star, and used position angle and separation values from Barnard and Burnham for the determination of its visual orbit. For the second star, which had long been studied as a binary, but still lacked a definitive orbit, Russell chose to invert the process and derive possible parallax and mass values from various sets of orbital elements derived by others. The parallaxes were found to vary within a range not unreasonable for direct measurements by different observers, and the masses all came out quite close. This was, in essence, a confirmation of Russell's discussion earlier in the year.

91. Letter, Russell to Campbell (25 October, 1910), Lick Observatory.
92. This was defined as:

 $m_1^3 \sin^3 i/(m_1 + m)^2$  (Op.cit., ref. 90, p.363; p.370) and was believed by Campbell to be generally a similar quantity for all spectral types when the ratio was large. The ratio quoted above is a mass ratio between the two components of a spectroscopic binary system of masses m and  $m_1$ , where the brightness difference between them is large, rendering the system as a single line spectroscopic binary. This luminosity difference would imply a mass difference, too, and in the case of single line binaries would be large ( $m_1$  is the mass of the brighter star). If the inclination were known, of course, the ratio would approach the value  $m_1$  for i = 90 degrees and  $m_1$  large relative to m.

Campbell's belief that this quantity was the same for systems of all spectral classes implied that all the brighter stars had similar masses, or that their actual mass ratios were similar. Assuming the former, Russell begged to differ in his letter. Upon analysing Campbell's own data, and not caring whether he included single or double line binaries in his sample, Russell found the following (<u>Op. cit.</u>, ref. 91):

"Apparent mass"

Spectrum range

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	05-B3	B5-A5	F-M	A11
10 - 0.3	12	4	1	17
0.3 - 0.03	5	11	8	24
less than 0.03	5	7	9	21
Total	22	22	18	

Russell was careful to make each of his columns contain close to the same amount of systems, so that the relative numbers could easily be compared. He observed: "This looks to me as if there was a distinct preference for large values of this function among 'early' type spectra". Russell commented that this result had theoretical importance, and was surely referring to his earlier discussion "On the Origin of Binary Stars" where criteria for fission depended upon mass ratios.

- 93. Letter, Russell to Frost (14 November, 1910), Yerkes.
- 94. Letter, Russell to Pickering (26 November, 1910), Harvard.

95. Lick Observatory Bulletin # 181, p.40.

96. Op. cit., ref. 94. The o<sub>2</sub> Eridani system is triple and it is not known whether the companion referred to by Russell was component "B" or "C". Quite possibly at the time it was "B" at a visual magnitude of 9.6 ("C" being 11.05) which would certainly turn out a surprise for Russell, as "B" is a white dwarf! As a matter of fact, on 20 December, Pickering responded with spectral types for Russell's binaries and indeed, the spectrum of the o<sub>2</sub> companion was listed as "A to A2". (Letter, Pickering to Russell (20 December, 1910), Harvard).

Russell reacted predictably to Pickering's 20 December letter with the spectral class information:

> Most of them fall in very nicely with the data from the brighter stars, but A to A2 for the faint companion of  $o_2$  Eridani is indeed an exception. In such a case identification would seem almost certain, but I feel that  $o_1$ Eridani  $1\frac{1}{2}$  degrees away has also a double companion of the 9th magnitude /?/? (though at a much greater distance) and so I enclose a sheet with full particulars, and make bold to ask you to have it looked up. I wish I had noticed this before and sent a note about it with my original list.

(Letter, Russell to Pickering (23 December, 1910), Harvard).

97.	Letter, Russell to Adams (14 December, 1910), Hale Papers.
98.	Letter, Lewis Boss to Russell (20 December, 1910), Princeton
	Univ. Library.

99. Letter, Pickering to Russell, Op. cit., ref. 94.

100. Letter, Russell to Pickering (23 December, 1910), Harvard.

101. See: Struve and Zebergs, Astronomy of the 20th Century,

(Macmillan, 1962), pp. 245-246, for another version of this story. Russell, in later years, recalled that while visiting Pickering at Harvard, they asked Mrs. Fleming to make a check on the spectrum of this star. He recorded this on tape for James Cuffey, at Indiana University in the early fifties. (Copy of this tape in personal possession, and at the American Institute of Physics). His recollection was more dramatic than one would expect from correspondence at the time. It seems as if the realisation of the problem was made in correspondence, not in person.

- 102. Letter, <u>Op. cit.</u>, ref. 100.
- 103. Letter, Kapteyn to Russell (14 March, 1911) Princeton Univ. Library.
- 104. Letter, Shapley to Russell (20 May, 1914), Princeton Univ. Library.
- 105. Letter, Campbell to Russell (3 June, 1911), Princeton Univ. Library.
- 106. Letter, Russell to Pickering (4 January, 1911), Harvard.
- 107. Letter, Russell to Pickering (3 June, 1911), Harvard. No record has yet been recovered as to how well these predictions came out.
- 108. Hertzsprung, Potsdam Publications 22 (1913), issue 63 released in 1911.

109. Letter, Russell to Pickering (26 June, 1911), Harvard.

110. Letter, Russell to Frost (31 July, 1911), Yerkes:

I am finishing up my paper on visual binaries. Having got hold of a statistical method for getting the average mass, (or, at least, the relation between mass and parallax) in the case of physical double stars in slow motion, for which it may be thousands of years before we can compute orbits. I shall be able to give average masses for groups of stars, including about 325 in all, and also much

information about the relation of surface brightness and density. The existence of the two kinds of second-type stars, (of rising and falling temperature, as I believe) comes out very clearly. For example among the stars of spectrum G one type closely resembles the Sun in mass and brightness: the typical star of the other apparently has about four times the Sun's mass, but gives 100 times as much light, pointing to a density 1/250 that of the Sun. The whole series of data falls in beautifully with the ideas I expressed last I will send you the whole thing for year. the Astrophysical as soon as I can write it up.

- 111. Russell, <u>Science 34</u> (1911), p.523.
- 112. <u>Ibid.</u>, p.524.
- 113. Ibid.
- 114. Ibid., pp. 524-525.
- 115. <u>Ibid.</u>, p.525.
- 116. Letter, Pickering to Russell (29 March, 1912), Princeton Univ. Library.
- Letter, Russell to Pickering (5 February, 1913), Princeton
   Univ. Library.
- 118. Russell to J.A. Parkhurst (12 November, 1912), Princeton Univ. Library.
- 119. DeVorkin, <u>Op. cit.</u>, ref. 74. We have already seen a number of examples of this. To the list must be added the fact that Russell and Hertzsprung 're-discovered' the technique of 'hypothetical parallaxes' almost at the same time in 1911. In September, 1912, Hertzsprung wrote to Russell remarking that their two papers on visual binaries treated the same topic, but with a different approach. Russell's was statistical, while Hertzsprung's introduced "... the minimum hypothetical parallax for ..." each single star. (Letter, Hertzsprung to

Russell (16 September, 1912), Princeton). Later, in 1913,

Hertzsprung wrote again, at length, about another duplication:

It is a curious thing, that we seem to have for the second time made something similar, as I am just finishing a small paper on the absolute brightness and distribution in space of the  $\delta$  Cephei - variables, and I see from a meeting report, that you have read a paper on absolute magnitudes of variables stars ... (Hertzsprung to Russell (12 June, 1913), Princeton).

This, of course, had to do with the derivation of the famous

period-luminosity relationship for Cepheids.

120. Ibid., Hertzsprung's 1913 letter.

121. Letter, Lockyer to Russell (17 June, 1913), Princeton Univ. Library.

- 122. Russell, Observatory 36 (1913), p.324.
- 123. <u>Ibid.</u>, p.325.
- 124. <u>Ibid.</u>, p.326.
- 125. <u>Ibid.</u>, p.328.
- 126. <u>Ibid.</u>, p.329.
- 127. Ibid.
- 128. Russell, "Relationships between the Spectra and other characteristics of the Stars", <u>Popular Astronomy</u> 22 (1914), p.275 ff. We should recall, however, that in Russell's 1907 lecture notes, he presented what would be a vague outline of the diagram. It is not definite that he actually had the relationship in mind at the time though.
- 129. Eddington, Observatory 36 (1913), p.471.
- 130. Shapley letter, Op. cit., ref. 104.
- 131. Letter, L. Boss to Russell (17 January, 1912), Princeton Univ. Library.
- 132. Letter, Russell to Lewis Boss (22 January, 1912), Princeton Univ.

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	Library.
133.	Russell, Observatory 37 (April, 1914), p.165.
134.	<u>Ibid</u> ., p.171.
135.	Ibid.
136.	<u>Ibid.</u> , pp. 171-172.
137.	Shapley, <u>Ap.J. 38</u> (1913), pp. 162-168.
138.	Russell, <u>Op. cit</u> ., ref. 133, pp. 171-172.
139.	Ibid.
140.	<u>Ibid.</u> , p.173.
141.	Letter, Eddington to Russell (1 March, 1914), Princeton Univ.
	Library.
142.	Russell, <u>Op. cit.</u> , ref. 133, p.173.
143.	<u>Ibid.</u> , p 174.
144.	Ibid.
145.	Eddington, Stellar Novements and the Structure of the Universe
<ul> <li>C 1</li> </ul>	(Macmillan, 1914), p.156.
146.	Russell, Op. cit., ref. 133, p.174.
147.	Ibid.
148.	Letter, Campbell to Russell (28 May, 1913), Lick Obs.
149.	We can see Hale's mode of approach and philosophy of organisation
	of the series in his letters to Rutherford, to whom he explained:
	These divisions of the general subject are only provisional, but you will appreciate our purpose, which is to give a connected and, as nearly as may be, a homogeneous account of evolution in the light of recent research. (Hale to Rutherford (24 November, 1913), Hale Micr.).
	Hale was most familiar with the second topic - stellar evolution -
	so he used it as a model for what type of material was expected
	in the lectures, which were, on the whole, to be popular in
	nature:

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In the case of the second topic, for example, it would seem advantageous to begin with a brief description of the typical heavenly bodies, illustrated with the best photographs, and follow this with an account of our present knowledge of the structure of the universe and the grouping of stars and planets in systems. This might occupy one lecture. The second lecture, taking this material to start with, would discuss the modes and causes of the evolution of stars and their satellites, with special reference to the case of the sun and the earth.

Hale hoped that this series would be eventually put into the form of a book, intended for distribution in schools. Though each lecture was to be published eventually, no coherent text ever appeared.

150. Letter, Russell to Campbell (25 May, 1914), Lick Obs.

- 151. Campbell, "The Evolution of the Stars and the Formation of the Solar System" <u>Popular Science Monthly</u> (Sept., 1915); <u>The</u> <u>Scientific Monthly</u> (Oct., Nov., Dec., 1915), papers I, II, III, IV, respectively. Quotation from paper III, p.180.
- 152. Ibid.

153. DeVorkin, Q.J. Roy. Astr. Soc. 18 (March, 1977), p.37.

- 154. See: letters, Aitken to Russell (15 October, 1912); Adams to Russell (5 March, 1913); Innes to Russell (n.d.); Shapley to Russell (24 June, 1913), Princeton Univ. Library. In Shapley's letter he indicated that Charlier was much impressed with Russell's giants and dwarfs.
- 155. Commentary following Fowler's address: "Spectral Classification of Stars and the Order of Stellar Evolution", <u>Observatory 38</u> (1915), p.379; p.390. Discussion at a meeting at Manchester of the British Association, Sec. A, September 9, 1915.

156. Fowler, <u>Ibid.</u>, p.392.

157. Struve and Zebergs, Op. cit., ref. 101, pp. 250-252.

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# The International Solar Union Conference - 1910

By 1900, the proliferation of schemes of spectral classification had produced a considerable amount of confusion and frustration on the part of those who were constantly in need of comprehensive classification data. One of these was Edwin Frost, who took the occasion of the 1904 International Congress of Arts and Sciences section meeting on Astrophysics, held in St. Louis, to provide an editorial entitled "A Desideratum in Spectrology" which began:

> It is to be presumed that most astronomers will agree in regarding the knowledge of stellar evolution as one of the greatest ultimate problems of astrophysics. It can hardly be questioned that great simplification would be gained, and the coordination of observations be rendered more certain, if a comprehensive scheme of stellar classification according to spectra could be adopted by all workers in this field.<sup>1</sup>

Frost hoped that the International Congress would provide an appropriate stage for the beginning of cooperation in the development of such a scheme. As an illustration "... of the confusion and mnemonic difficulties ..."<sup>2</sup> of using the systems present at the time, Frost cited the various classifications of the star Procyon: "Secchi, II; Vogel (1875), Ia; Pickering (Draper Catalogue), F; Lockyer (1893),  $A(\beta)$ ; Vogel (1895), Ia3; Miss Maury, XIIa; McClean, Division III; Miss Cannon, F5G; Lockyer (1899), Procyonian". After this exposition, Frost challenged:

> I venture to say that in this company there are not many who could instantly localize a spectrum of Vogel's Class Ia2; or of the Draper Catalogue E; or Miss Maury's VIIc; or of Lockyer's Taurian or Achernian groups.

Frost also pointed to the great difficulties for the teacher of astronomy, as well as the researcher, in presenting the schemes of classification. He did not mention, of course, that most teachers and writers of texts had their preference for one system or another. Scheiner used Vogel's system; though Young, in his <u>General Astronomy</u>, seemed to have no preference for either Secchi's or Vogel's systems, presenting them in chronological order.

Frost indicated some bias in favour of the examples set by biologists and palaeontologists, feeling that "There is a great advantage in the use ... of such a term as "carnivora", which is available for all languages and is at once comprehensible". He considered these types of systems helpful, and suggested divisions corresponding to orders, families, and genera. But at this point he drew a line. He felt that the venture should be international and widely representative. Therefore, he did not propose any system, but merely pointed out the need for an organising effort. He did, however, provide a few important guidelines.

First, the systems based upon evolution, such as Vogel's and Lockyer's, were highly different in structure, as would be any system based upon different theories of development. Frost wondered: "Is it not, therefore, desirable that any new system of classification should be based, rather, purely upon observed data?"<sup>3</sup>

Next, Frost called for simplicity, and an objective form of nomenclature that would involve some recognisable aspect of the physical appearance of the spectrum itself, such as the designation of 'helium' stars, though he recognised this as potentially confusing, also.

As a third possible line he suggested:

In the light of our present knowledge - or, better, in the darkness of our present ignorance - the use of temperature as a basis of classification would seem very doubtful; but, without committal to any theory, the different electrical behavior of the different lines - occurrence of enhanced lines, so called - might properly serve as a criterion in arranging some subdivisions.

Frost felt that the new classification could have as many as one hundred basic divisions, but that it should be grouped into relatively few major areas. Further, both the photographic and the visual regions should be employed, "... and studies of the distribution of energy in the entire range of the spectrum should add important data".<sup>4</sup> He considered that the project might take at least five years, if done on an international scale. Realising that many of the pioneers in spectroscopy and classification were to be at hand only for a short while, Frost urged, in conclusion:

> Is it not time that a beginning be made by the organization of an international committee to consider the question of a new classification of stellar spectra, representative of the observable facts of the first decade of the twentieth century?

The meeting at which Frost delivered his editorial address was very much the inspiration of Hale, who was well on the way to organising the International Union for Cooperation in Solar Research. At the time, many cooperative projects were in progress or just commencing. The Eros Campaign was well underway, and the great Carte du Ciel project was in progress, more or less, amongst European observatories. By 1905, Campbell was proposing his catalogue of spectroscopic binary stars, which required input from many sources, together with standardisation.

Possibly the most important cooperative effort was proposed and organised by Kapteyn, with Hale's wholehearted support - the <u>Plan</u> <u>of Selected Areas</u>. In late 1906 through early 1907 Kapteyn's plan developed, largely in correspondence with Hale. Hardly one year after Hale established the Solar Observatory, he was most anxious to expand the scope of Mount Wilson to stellar astronomy, and saw the plan as one of their most important activities.<sup>5</sup> He felt that they might best contribute by the determination of radial velocities and general faint field work with their 60-inch, which was still under construction at the time. Kapteyn hoped that the Mount Wilson people would extend Pickering's bright star classification work to the fainter fields also, and wanted them to do the classification themselves. This could have been due to a feeling on the part of Kapteyn, shared by others closely involved with the use of spectral classification studies at the time, that Pickering's techniques left something to be desired. This was well expressed by Frost, in a letter to his old teacher, Young:

> ... Pickering's work with objective-prisms was done on too wholesale a scale to allow the correct exposure to be given for individual stars, and a knowledge of the subdivisions of the first type is very desirable for many stars.

But international cooperation suffered from personality differences, and in some cases, mild political boycotting. Lockyer was shunned as a possible candidate for chairman of the sunspot committee of the International Solar Union, which caused some difficulties.<sup>7</sup> In 1907, when Vogel died, differences arose concerning who was to be his replacement. Nost Americans favoured Schwarzschild, with the exception of Campbell, who preferred the next in line at Potsdam, Hartmann.<sup>8</sup> Schwarzschild was the successful candidate, and there is a little evidence that some Europeans felt the Americans were too influential in the choice.

Nevertheless, there was great need for international cooperation. As telescopes grew larger and observing projects became more extended (with the addition of fainter objects for photographic and spectroscopic study), Frost began to feel that an effort should be made to be sure that observing projects would not be duplicated, and that everyone might contribute to a general forum which could continually examine observing priorities.<sup>9</sup> Of course, some of this could have been

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Frost's new enthusiasm for being the primary editor of the <u>Ap.J.</u> - which he saw as the proper forum - but still, there was much to be said for cooperation.

Schwarzschild's succession to Vogel's position at Potsdam also placed him in the role of defender of the classification system. It actually needed little defence, since it had been in use for so long, and had proved to be adequate in most cases. Hale, as we have seen, began by using Secchi's system, but made continual reference to Vogel's. Frost used Vogel's system, too, though only for ease of identification. In this regard, it is evident that Frost, using Vogel's system, still felt hampered by the lack of any identification of line structure. As he noted to Campbell:

> Many stars of Vogel's types Ia1, Ia2 and Ib show the calcium lines H and K sharp and narrow, in marked contrast to the other lines in the spectrum, generally broad and hazy.<sup>10</sup>

Later in this letter, he noted that one would have to refer to notes appended at the end of Vogel's and Wilsing's catalogue to realise that fully one-fifth of the types mentioned showed this anomalous characteristic.

By 1910, the general situation since 1904 had not changed, except for the fact that the Harvard staff had been continually adding stars to their already vast collection of spectra and the need for international cooperation became more apparent. From the physical side, a number of advances had been made, as we have seen, in that the Harvard Sequence had now become established as a temperature sequence, partly through the efforts of Hale and Fowler.

In this atmosphere, Pickering was aware that at the 1910 Solar Union meetings in Pasadena, the question posed by Frost was to come up again. He was not completely confident about the survival of his own system, which had gained enormous momentum from the numbers of stars classified. As a result, he had apprehensions of extending the scope of the International Solar Union to include astrophysics because it would surely mean close scrutiny of his system which, while it might have lacked little, would therefore become vulnerable to external pressures. These feelings were expressed in a diary kept of his travels to Pasadena.<sup>11</sup>

Apprehensions aside, Pickering decided to prepare for the eventuality. While on the train west he held several 'informal' meetings with unspecified parties about classification problems, and began to feel a bit better about the position of his own scheme. After the Pasadena meetings, a similar series of sessions aboard the return train were held, which were mentioned later.<sup>12</sup>

After the regular sessions of the International Solar Union had been completed, actually as part of the last session on 2 September, the question was brought up as to "... whether the Union should not extend its activities to include general astrophysics ..."<sup>13</sup> The positive reaction was, as reported, unanimous, and the first action was the establishment of a committee on spectral classification whose members were:

> Adams, Campbell, Frost, Hale, Hamy, Hartmann, Kapteyn, Newall, Pickering (Chairman), Plaskett, Russell, Schlesinger (Secretary), Schwarzschild, Küstner (added later by the Committee).

Americans comprised about half the committee. Much later, after discussions with A. Schuster, Pickering added Fowler's name to the list. As to Lockyer, Schuster commented:

Lockyer talked to me the other day about the classification of stellar spectra. He takes his exclusion from the committee pretty well, but I think he feels it.<sup>14</sup>

Fowler's name later appeared as a correspondent of the committee,

and he was consulted in subsequent actions of the committee.

During the meeting in Pasadena, the members of the appointed committee met to discuss their personal preferences on classification, and what the scope of the committee might be. A summary of the opinions were then drafted into a circular letter, dated 7 November, 1910, and distributed to a number of astronomers for comment, with appended questions for explicit reactions. The latter was designed by Pickering, Schlesinger, Russell, and Father Cortie.<sup>15</sup>

We will first examine the reactions of the committee members, in abstract form based upon the summary provided by Schlesinger's circular letter:

- 1. Preference for Draper System, without modifications: <u>/Adams</u>, Küstner, Plaskett, Schlesinger7
- 2. Preference for Draper System, with modifications: /Hartmann, Russell7
- 3. Recommendation to modify or replace Draper System with a numerical system: /Schwarzschild (for colour-index work), Russell (only as a numerical modification of Draper System)
- 4. No opinion until general circular letter has been sent out and returned: /Frost7

We see Frost adhering to his original philosophy here, as expressed in 1904. Schwarzschild acted predictably, advocating a system which would have numerical significance, based upon colour indices (which had already been used extensively by Hertzsprung). Though Schlesinger advocated the Draper System in its present form, he recognised the need for greater resolution, especially in the early type stars. When it came round to Newall's turn, an interesting and crucially important discussion took place, which was repeated in Schlesinger's circular letter:

> Mr. Newall raised the question whether the committee should not consider the matter of stellar evolution. The members present seemed to be of the opinion that this was legitimately within the scope of the committee

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but that its immediate business would be the establishment of a uniform system for classification. Messrs. Russell, Hartmann, Kapteyn, and Schlesinger urged that no evolutionary basis for a classification be adopted at the present time; astrophysicists are not agreed as to the proper sequence from this point of view; if our ideas on this matter should be modified in the near future (as seems very possible), then it would be necessary to modify or to abandon altogether any system of classification based upon these ideas. For similar reasons Mr. Russell asked that the use of such terms as "early" and "late", now so frequently used in describing spectra, be discontinued in favor of "white" and "red".

Thus the committee sought to separate, once and for all, evolutionary considerations from spectral classification studies. It is evident that they were not too successful, though most who replied to the circular letter, and who discussed evolution, seemed to agree that it should be separated. The explicit questions asked in the circular letter, as drafted by Pickering, Russell and Schlesinger (with Cortie) on the returning eastbound train after the Pasadena meeting, read:

- (1) It will be noticed that, at the meeting reported above, there seemed to be a practically unanimous opinion that the Draper Classification is the most useful that has thus far been proposed. Do you concur in this opinion? If not, what system do you prefer?
- (2) In any case, what objections to the Draper Classification have come to your notice and what modifications do you suggest?
- (3) Do you think it would be wise for this committee to recommend at this time or in the near future any system of classification for universal adoption? If not, what additional observations or other work do you deem necessary before such recommendation should be made? Would you be willing to take part in this work?
- (4) Do you think it desirable to include in the classification some symbol that would indicate the width of the lines, as was done by Miss Maury in <u>Annals of the Harvard College Observatory</u>, <u>Vol. 28</u>?

(5) What other criteria for classification would you suggest?<sup>16</sup>

In conclusion to his circular letter, Schlesinger expressed the hope that replies would be forthcoming soon, especially for the third question. In addition, opinions from other astronomers not on the original mailing list were solicited. Schlesinger noted that replies "... were received from nearly all to whom they were addressed..."<sup>17</sup>

Of the twenty-eight respondents, fifteen were Americans (though Albrecht was in Argentina by that time, he was recently of Lick), two from England; seven from Germany; two from South Africa; one from France; one from Canada. Four were from Harvard alone, and, except for Slipher, the rest of the Americans were somehow involved with Lick, Yerkes, or Mount Wilson. (Though Schlesinger was closely tied to Yerkes, and had very close ties with Pickering, he, too, should be considered separately, as should Russell.) In a later compilation of this material, Fowler and Belopolsky (of Pulkowa) were added to the list of correspondents, though, strangely enough, only Belopolsky's reply was recorded.

Table 3 lists the nature of the responses gathered. All of the Americans, and essentially everyone else, answered the first question in the affirmative, though only about nine respondents were affirmative on Question 3, which was to establish the system as a universal one. There was no obvious national split on this question, since six Americans, including Pickering, dissented. The only person not to vote in the affirmative for the Draper system was, logically, Scheiner, though Belopolsky did not provide an explicit answer.

Most significantly, answers to Question 4 on the need to include indication of line character, following Maury, were sixteen for, and only four definitely against.

Answers to Question 5, on suggestions for other criteria, ranged widely. Campbell felt more observations were needed before definite

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		Table 3		
Questions:	(1)	(2)	(3)	(4)
ADAMS	Yes	more distinction	No	useful but No
ALBRECHT	Yes	11	No	NA
CAMPBELL	Yes	No	No	Yes
CANNON	Yes	Numerical subdivisions	Not at present	Yes with caution
CORTIE	Yes	" + Secchi	Yes	No
CURTIS	Yes	No (notation <b>a</b> rbitrary)	Yes	No
CURTISS	Yes	No	Yes	Yes
LUDENDORFF & EBERHARD	Yes	No	Yes	Yes
FLEMING	Yes	Subdivide later classes	No	Yes
FROST	Yes	Yes (various)	No	Yes
HAMY	Yes	No	Yes	No
HARTMANN	Yes	+ Secchi 100% empirical	No	Yes
HERTZSPRUNG	Yes	Yes (numerical)	NA	Yes
HOUGH	Yes	Yes (change nomenclature)	NA	NA
KÜSTNER	Yes	NA	NA	NA
LORD	Yes	Photos of spectra	Yes	-No (pix are better
LUNT	Yes	Uniform sub- division symb.	Not at present	Yes (Maury)
MAURY	Yes	Yes - Stellar evolution consideration	Yes	Yes
PARKHURST	Yes	Better sub- divisions	Yes (with more work)	NA
<b>PICKERING</b>	Yes	Yes - extend classification	No (until evol <sup>n</sup> is cleared up)	Yes (see text)
PLASKETT	Yes	Yes - too arbitrary	Yes (by 1913 Bonn meeting)	Yes
RUSSELL	Yes	No (need numerical subdivisions)	Yes (with refinements)	Yes
SCHLES INGER	Yes	No	Not yet (more C.I. work)	NA
SCHWARZSCHILD	Yes	Numerical order needed	No	NA
SIDGREAVES	Yes	Change to stellar names	No - need link to evolution	Үев
SLIPHER	Yes	Alphabetic	Yes	Yes
WILSING	Yes	Characteristic spectra	NA	NA
SCHEINER	No	Needs more work obj. prism & slit work needed	No	NA

answers might be forthcoming; Cannon and Fleming felt the need to identify peculiar spectra better; Curtis was adamant that evolution be separated from classification, and wanted to include notation to identify variable spectra; Hamy wanted to include considerations of Lockyer's work; Hartmann, Maury, Cortie and others wanted to include a provision for very general classification, following Secchi's four types. Schwarzschild, Russell, Hertzsprung and Hough and others were most concerned that numerical divisions or subdivisions be included for precise calibration; Lunt wanted to have a chemical classification; Russell wanted to classify in terms of lines and bands; Scheiner could see no advantage of the Draper system over Vogel's, though both were in need of revision. Many individual responses dealt with evolution and kindred topics. We now take them in turn.

### W.S. Adams

Adams felt that attention to line widths was a secondary interest for the present. He had not yet come to look for spectroscopic parallaxes, but he was quite concerned that studies of line spectra be continued.

### H.D. Curtis

Curtis felt that no new systems were needed, and that any system based upon evolution would "... be most unwise ..." and would lead to further confusion.<sup>18</sup>

# W.P. Fleming

Fleming left all statements to Pickering.

### E.B. Frost

Frost felt that everyone agreed that the stage of evolution of a star was somehow shown in its spectrum, and felt that evolution could eventually be reduced to a temperature sequence. He continued: It is desirable that both branches of the temperature-curve should be recognised, in so far as their validity can be established, in any new classification, particularly for the red stars. One cannot help feeling that Sir Norman Lockyer must be right, to some considerable extent, in his contentions on this point, even if the evidence he has thus far presented may not be entirely convincing.<sup>19</sup>

Frost felt that the recent work by Russell, as announced at the Harvard meetings, must be considered in this regard. "If it can be proven that of two red stars having very similar spectra one is on the rising branch and the other on the descending branch of a temperaturecurve, then the classification ought to show it". Finally, Frost wondered if it would be possible to establish a system which could provide for "collateral" lines of evolution, or non-evolution, "... where the sequence of spectra has a choice of collateral branches to follow before the next group is reached".

### E. Hertzsprung

Hertzsprung recommended the Draper System with the following modifications:

A subdivision or second co-ordinate of spectral classification may in the future be connected with the absolute brightness of the stars. At the present time this can be done only in a very imperfect way ... For the 'earlier' spectral types we have the valuable subdivisions <u>c</u> and <u>ac</u> of Miss Maury ... Using the D.C. classification the <u>c</u> and <u>ac</u> properties should at least be indicated by the letter <u>p</u> (peculiar), which has occasionally been omitted, for example from the very peculiar star  $\alpha$  Cygni.<sup>20</sup>

This recommendation was to be taken up much later in the designation of luminosity classes by Morgan, Keenan and Kellman.

J. Lunt

At the present time I think that an evolutionary basis of classification is premature, but the terms "early" and "late" are so convenient and so frequently used that I would continue to use them in preference to "white" and "red" (which have a more restricted meaning), even though they may ultimately have to be abandoned.<sup>21</sup>

# Antonia C. Maury

In answering the second question, on possible modification to the Draper Classification, she commented:

> However, it does seem to me important that in a final classification this sequence, which shows in so very marked and wonderful a way the gradual transformation of spectral type, and must in some manner express the law of stellar evolution, should be represented either by numerals in natural sequence or by letters in alphabetical order. If this be not done, the attempt to grasp in thought the evolutionary changes and fix in memory the places of individual stars becomes bewildering.<sup>22</sup>

She believed that Secchi's numerals might be retained to represent the gross characteristics of spectra, and:

Since ... the vast majority of stars evidently fall in this sequence, there would seem to be little doubt that it represents the main outlines of stellar evolution.<sup>23</sup>

She also felt that the transition from Pickering's "Type V" in Secchi's notation, which was from O to B in the Draper Classification, was shown to be reasonably smooth by Miss Cannon, but that "... the scarcity of stars of this kind raises a doubt whether they represent a universal phase of evolution or merely an occasional one ..." Finally, regarding the direction of evolution, she was quite frank:

> As to the question whether the order O,B,A,F,G,K,M,N should be reversed, the evidence seems to me almost conclusive against this. For, if we reverse the order, we should have to assume either that cooling stars change from red through yellow and white to blue, instead of pursuing the opposite course, or else that such stars, as, for example, the sun, are growing hotter instead of cooler, which seems unlikely. The high lightintensity in the ultra-violet of B and A stars and the gradual falling off of light in this region and later in the blue, with advance

toward Secchi's types III and IV, seem clear proofs of loss of energy by radiation.

Maury continued this discussion to assert that the extensive hydrogen and helium atmospheres surrounding early type stars was conclusive evidence for their early stage of evolution - as compared with the heavier, and evidently "more condensed", atmospheres surrounding solar and later types. To add to her conviction, she referred to the Algol variables as being less dense than the Sun. She evidently failed to appreciate the significance of Hertzsprung's investigations of her own work, for she concluded:

> ... were we to reverse the commonly accepted order, we should have to assume that the solar and even the deep-red stars with fluted spectra are growing not only hotter but also rarer, which seems out of the question.

Miss Maury did, of course, acknowledge Hertzsprung's interpretation of her <u>c</u> and <u>ac</u> stars, but felt somehow that the spectral peculiarities were due to "... important differences of constitution in these stars, which seem to pursue a line of evolution that, through a portion at least of its course, deviates from the normal".<sup>24</sup> Evidently, she was not ready to accept Russell's belief that they represented a stage of evolution prior to the blue star stage, and preferred to consider them abnormal stars - a not totally unreasonable conclusion since there were so few of them observed unambiguously at the time.

### E.C. Pickering

In response to the third question, Pickering answered in the negative - that no system should be adopted universally for the present, for the following reason:

> It would not be wise for this committee to recommend a system of classification until the laws of evolution of stars are fairly established.

What is fascinating here is the fact that this represents the

only explicit statement by Pickering yet found concerning evolution. It is evident that Russell's work was at the base of Pickering's remarks:

> In order that the class of spectra should be measured instead of estimated, as proposed by Professor Russell, it would perhaps only be necessary to measure accurately the intensity of different portions of the spectrum.

Pickering indicated that he was interested in taking up this line of work, but was not, as yet, sure of how to do it most accurately and efficiently. He then went on to conclude to this question:

> By the courtesy of two members of the committee, fifty excellent photographs of stellar spectra taken with the slit spectrographs of the Yerkes and Lick Observatories have been sent here for examination. A careful study is now being made of them.

We have already seen that Pickering had made these requests of Frost and Campbell in early July, 1910, soon after he had visits from Kapteyn and well after he had realised that Russell's work was leading to Hertzsprung's conclusions regarding the c stars. In examining his answer to Question 3, and his subsequent answer to Question 4, Pickering's requests to Frost and Campbell become more readily understandable:

> (4) Yes. With an objective-prism, increased width of lines is not easily distinguished from change in focus of the telescope. With a slit spectroscope, the comparison spectrum shows that a widening cannot be due to this cause. The width of two lines compared with their distance apart could in some cases be well estimated or measured.

We now see clearly that Pickering was sincerely sceptical of the c-characteristic, until, forced by Russell's work and Hertzsprung's backers, he re-examined stars of all Draper classes using higher quality slit spectra - something he had always mentioned had to be done. Thus if favouritism, or nationalistic pride, had anything to do with Pickering's actions, they most certainly were secondary.

# Annie J. Cannon

Miss Cannon also discussed this study of the Lick and Yerkes spectra. She was careful to avoid bias, so, in classifying the spectra, she purposely had their identities masked or coded (which means that there was a good chance that she would have recalled the spectrum of a star by its name only!) and then proceeded to classify them. She found that each spectrum was classified as they would have been with objective prism spectra, where the deviation was barely by one subdivision. As she was careful to note that some of the deviations were in the "... relative intensities of a few lines ..." we may very well see why, when it came to answer Question 4, she answered strongly in the affirmative - so long as slit spectra were employed.

### J.S. Plaskett

Plaskett felt that the Draper nomenclature was arbitrary, and should be combined with one of Secchi's designations, so that a numerical sequence could be established.

> There is, of course, the objection that one would have a tendency to associate the order of the numerals therein used with the order of stellar development, and this, considering the present state of our knowledge of stellar evolution, would be inadvisable.<sup>26</sup>

Plaskett also felt that the order of the Draper Classification, for each designation (i.e. A4F) biased evolutionary considerations by indicating direction from A to F, "... and not, as may be possible, from F to A ..."

## Henry Norris Russell

Russell was already well aware that Pickering had directed Annie Cannon to the study of high quality slit spectra from Lick and Yerkes. In his commentary, he referred to the study as amongst the most important that must be done. To this he appended a suggestion:

I would add the suggestion that a comparative study should be made of the spectra of stars of very different total luminosity but the same spectral class (Hertzsprung's "giant" and "dwarf" stars). If any definite and constant <u>spectroscopic</u> differences exist, they will be of value in classification.<sup>27</sup>

This suggestion, whether heeded or not, was taken up inadvertently by Kapteyn, who, within one year was to suggest to Hale that Hertzsprung be allowed to travel to Mount Wilson for an extended visit to use the 60-inch telescope. Within three years, he was to direct Adams to place stars of large and small proper motion, but of the same spectral class, side by side on the same spectroscopic plate to search for interstellar absorption.

We make the important digression here to note that this suggestion by Kapteyn led Adams and Kohlschutter to the technique of spectroscopic parallaxes, which, in turn, by providing a more powerful and efficient method of determining absolute magnitudes, greatly increased confidence in the existence of giants and dwarfs, as I have shown elsewhere.<sup>28</sup> The importance of this new technique can be seen in a letter Eddington wrote to Adams in 1916:

> It seems to me you have proved your point fully; and in particular I think the question of giant & dwarf divisions of the red stars must now be considered finally settled.<sup>29</sup>

Russell was also impressed with Adams' work, as was Hertzsprung, who had actually suggested to Adams in correspondence that he look for such effects in spectra, referring to his own work on the detection of giants from Miss Maury's classes.<sup>30</sup> Russell later wrote to Adams extensively on possible apparent angular diameters, hoping that one might be in the range of detectability. In sum, Eddington's comments in 1916 to Lockyer reveal the importance of the reality of giants and

dwarfs, as determined by Adams, to Russell's concept of evolution:

Although I find some difficulties in the theory of ascending and descending temperature stars, I believe that in the main it must be right. I find myself even in the last year advancing towards that point of view.

I am no spectroscopist myself, so that it is rather by Russell's arguments that I approach the question. I think it is clear that something more than the Harvard subdivision is required, both for evolutionary theory, and for our statistical work on proper motion, etc.; your discrimination of differences between stars which on the Harvard system would be classed together is a most useful line of work, and I feel sure that it will lead to an advance in our knowledge of the relations between spectrum and velocity and luminosity - the aspect in which I am specially interested.<sup>31</sup>

Returning now to Russell's response to the committee's questions, it is just these spectral criteria that Russell wished to emphasise. Russell felt that Maury's criteria, as Hertzsprung used them, "... show conclusively that the fineness of the spectral lines is of great astrophysical importance ..." As a general observation, Russell thought that the Draper Classification sequence of letters OBA ... being out of any recognisable order (numerically or alphabetically) was preferable because "This helps to keep the novice from thinking that it is based on some theory of evolution ..."

### Frank Schlesinger

He felt that careful study of both the visual and photographic portions of the spectrum should be made to develop more practical classification criteria and aid "... the more general and more important ... question of stellar evolution ..."<sup>32</sup>

### Karl Schwarzschild

While he generally agreed with the use of the Draper System, he

wanted to give some attention to Hertzsprung's physical interpretation of Maury's classifications into subdivisions, which he noted had evolutionary significance.

This then completes our discussion of those published responses to Schlesinger's circular letter that included mention of evolution, or matters pertinent to its study, at the time. We thus have a fairly clear picture of the state of affairs circa 1911. Evolution was most certainly in mind, but was in a somewhat confused state. Those who felt that evolution must be considered, held to the conventional view (i.e. Maury); whilst those who felt that the time was not right for such an influence upon classification were, partly at least, willing to re-examine Lockyer's controversial ideas, in the light of recent discussions by Russell.

Through 1911, as we have mentioned, additional responses came in to Schlesinger. Fowler and Belopolsky were added to the list of committee members and correspondents, and, by December, 1912, in preparation for the 1913 Bonn meetings of the Solar Union, Schlesinger again took up the organisational and editorial work associated with the committee. He prepared a synopsis of the published responses, with additional letters added too late for publication in 1911, and then after a meeting of the executive committee (he and Pickering), distributed a circular letter which repeated these abstracted responses, together with the proposals to the Solar Union based upon them.

The primary purpose of the circular letter this time was to gauge the reaction of the committee members to their (Pickering's and Schlesinger's) conclusions, drawn up as committee proposals: It appears from these replies that the preference for the Draper Classification is nearly unanimous, but that the general feeling among investigators is opposed to the adoption at the present time of any system as a permanent one  $\dots$ <sup>33</sup>

Two resolutions were drafted for submission at the Bonn meetings. The first was that the committee be asked to acquire (through cooperation) observational material necessary for the adoption of a universally accepted and permanent system; and, second, that while this was in progress, the Draper System be used, as defined in <u>Harvard Annals 56</u> (p.66), with only very minor modification.

Both Hale and Adams signed the circular in support of the resolution, but both indicated that they would not be able to travel to Bonn. By May, through the grapevine, Hale had learned that most members of the committee favoured the resolutions and felt that the matter had been dealt with completely and correctly. Nothing much seems to have transpired in the year following until the August, 1913, meetings; though just before the meetings, Schlesinger distributed a hasty questionnaire asking the committee members for comments on the most pressing problems in laboratory spectroscopy, in the aid of spectral classification.

The recommendations of the committee were passed unanimously and without comment at the Bonn meetings.

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### CONCLUSIONS

We have followed the study of stellar evolution and its relationship to developing themes of spectral classification from the inception of classification in the early 1860s to 1910, when both the Harvard Classification became established and the classical view of evolution as a linear cooling sequence became seriously questioned through Russell's interpretation of the HR Diagram. Since we have been able to show that, at most points during this period, evolution greatly influenced classification, it is a little ironic to see that, at the close of the period, with the establishment of a linear classification, it had also become obvious that evolution, itself, was highly nonlinear in terms of the progression of spectral type with age, and with other stellar properties. It is also clear that, by 1910, few astronomers still openly wished to be guided by evolution in pursuit of the best scheme of classification. For this reason, as we saw in Chapter 5, the Harvard Classification was accepted, rather than any possible scheme based on evolution. Still, the Harvard Classification was not accepted universally, and one of the causes for this was simply that the course of evolution was very much in question. This last conclusion points to the continued influence of evolution at a more subtle level.

By 1921, mainly through inertia, the Harvard Classification did become the universal standard, and has remained so since, except for additions to identify luminosity classes. Thus standardisation of classification occurred long before stellar evolution was clarified. In the twenties, Russell, Eddington and others became keenly aware that Russell's evolution model, which had become widely accepted, would not survive intact. Eddington's studies of radiative equilibrium in

stars before World War I, coupled with a better understanding of the nature of ionisation from Saha's studies of the solar atmosphere, and J. Eggert's discussion in 1919 of the effect of ionisation in the stellar interior, helped to convince Eddington that the main sequence could not be a cooling sequence under general contraction. Mainsequence dwarfs were still in the ideal gas state, from his calculations, and would therefore heat upon contraction as giants were thought to do. This criticism of Russell's theory came in 1924, when Eddington first discussed theoretically the mass-luminosity relation, which Russell had ingeniously tried to account for in the preceding decade. But, by 1924, Eddington had realised that the smooth curve representing the mass-luminosity relation for all stars could not be reconciled with Russell's theory. If dwarf stars were truly incompressible, their deviation from the perfect gas laws would cause a drop in luminosity for a given mass, which would create a discontinuity in the massluminosity relationship. Not finding the expected discontinuity led him to abandon Russell's theory in his book The Internal Constitution of the Stars:

> I do not think it is too blunt an expression to say that this <u>/theory</u> is now overthrown; at least it has been gutted, and it remains to be seen whether the empty shell is still standing.

Eddington's radiative equilibrium studies also caused the earlier theories of stellar constitution of Lane and Ritter (or as Eddington referred to them, "Lane's and Lockyer's earlier proposals ...."<sup>3</sup> to be by-passed. The role of convection in stellar structure became modified to include radiative equilibrium though stars still were believed to be highly mixed. By the thirties, models of stars with convective cores, radiative envelopes, and a high degree of mixing became standard. But, by the late thirties, unmixed models began to

appear in an attempt to explain giant stars. This important step, along with the final recognition of the source of stellar energy in the late thirties, ushered in the modern era of studies in stellar evolution. These became truly modern only in the fifties with the successful adaptation of Gamow's shell source concept to the structure of red giants, so as to explain them as expanded main sequence stars dependent upon fusion in a shell surrounding an exhausted core. Also by the mid-fifties, the application of high-speed electronic computers to the solution of stellar models made detailed studies practicable.

Throughout this entire period, however, the fundamental observations expressed by the Hertzsprung-Russell Diagram did not change, though, as we have seen, the interpretation of the diagram varied drastically. Eddington noted this in his same discussion:

> In speaking of the "overthrow" of the giant and dwarf theory, we do not intend to imply that there is to be any retrogression with regard to these results. They are mostly the ascertained facts ... In any reconstruction of theory these facts must be attended to.<sup>4</sup>

The dominant concept of contraction as the only possible direction of evolution has been documented in this thesis. It appears that contraction was never doubted as the only possible direction by workers during this early period (1860-1910). Russell, as we have seen, expressed this feeling directly:

> As almost everybody will agree that a star contracts as it grows older, this leads us to suppose that the giant stars of Class M represent a very early stage of evolution.<sup>5</sup>

In discussing reactions to Russell's work, we saw that the only major statement questioning contraction was by E. Rutherford in 1915. There is no evidence that it was seriously heeded for some time. Uppermost in any future consideration of this important historical question will be to see if expansion against gravity could have been seriously proposed before acceptable non-gravitational forces could be postulated to provide the impetus.

In addition to contraction, we have shown how astronomers in the 1890s and the first decades of the 20th century attempted to draw in studies of stellar kinematics as a function of spectral type to discussions of evolution. None of these led to much success, and provided more confusion than clarification. The historically important fact remains that these workers were aware that all aspects of stars their individual physical characteristics and their group characteristics had to be considered in any discussion of evolution. The statistical investigations of kinematics, themselves, were illusory until Hertzsprung recognised that there existed two very different ranges of luminosity for stars of the same spectral class. Even then, star streaming and the observations of high velocity objects persisted as interpretive problems in theories of evolution until the Galaxy, itself, became better understood in the twenties.

We have been able to identify here the relationship between Hertzsprung and Pickering, and Russell and Pickering, that led to the Hertzsprung-Russell Diagram. At the date of the writing of these conclusions, the writer is satisfied that Pickering's motives in keeping Hertzsprung's work from Russell's attention, and in his general scepticism for its validity based upon Maury's classification system, was sincere. It is apparent, however, that his negative feelings were slightly extreme, insofar as they kept him from even discussing Hertzsprung's work with Russell.

Russell's development of the diagram can now be seen as a partial vindication of Lockyer's earlier schemes. It is clear that Russell was stimulated by Lockyer's concept of stellar evolution, and chose a good fraction of his parallax stars to test the temperature

arch. There is also no doubt now that Russell came to the diagram from the direction of evolution.

The important question of why theories of evolution appeared simultaneously with schemes of classification has been touched on in this thesis. On the one hand, it is easy to argue that the appearance of the work of Kirchhoff and of Darwin in about the same year provided both the means and the stimulus. But we have looked further here to see that the concept of stellar constitution also radically changed in the sixties. Except for Helmholtz's ideas, all meteoritic theories in the fifties were non-evolutionary in character, trying simply to maintain solar heat. But, in the sixties, aided by knowledge that the Sun was gaseous, meteoritic processes were relegated to pre-history by Thomson, and cooling and contraction became evolutionary processes. Thus while prior to 1860, evolutionary phases existed in pre-stellar stages of the formation of a star out of a nebula (as in Herschel's concept), and, here and there, a few did consider the process of subsequent cooling, we do not see major efforts to discuss the lives of stars beyond the attainment of the stellar state until the sixties. At that time came the realisation, largely, but not wholly, as a result of Kirchhoff's work, that the Sun and stars were gaseous.

Meteoritic theories persisted, and led to that of Norman Lockyer's in the eighties. We have seen that Lockyer was not alone in considering a heating phase in the life histories of stars. But he was very much alone by 1900 in maintaining his Meteoritic Hypothesis.

When classification studies began in the sixties, it also became possible to discuss the evolution of a star not only in terms of a temperature progression, which became identified with a change in colour, but also by a progression (i) in the appearance of different substances in the stellar spectrum, as Lockyer and others thought;

(ii) in a spectral similarity to that found in nebulae; (iii) by a progression in atmospheric density, which was logically thought to change with continued contraction. None of these progressions, save for colour and the association of stars with nebulae in space (not in spectrum), were possible before spectrum analysis was applied to the stars. Thus, just as we have shown classification to be influenced deeply by theories of evolution, evolutionary theories, themselves, derived much from spectroscopic studies associated with classification efforts. The relationship between the two was, indeed, symbiotic.

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1.	J. Eggert, Phy. Z. 20 (1919), p.570.		
2.	Eddington, <u>Internal Constitution of the Stars</u> (Cambridge, 1926), p.163.		
3.	Ibid.		
4.	H.N. Russell, Observatory 36 (1913), p.328.		
5.	Eddington, Op. cit., ref. 2, p.163.		

Archival Sources

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\* \* Dartmouth College

Dartmouth College Library holds the Papers of Charles Augustus Young. The collection, chiefly correspondence with astronomers, drafts of articles and lectures, diaries and notebooks, is in 10 boxes totalling 12½ linear shelf feet. A detailed inventory is available from the library. Correspondence examined and/or used in this thesis comes from boxes 6 and 7. The majority of Young's professional career was as professor of astronomy at Dartmouth and Princeton, where he was Henry Norris Russell's teacher. This collection comprises Young's years both at Dartmouth and Princeton. A small collection of Young papers exists at Princeton. Address: Dartmouth College Library, Hancver, New Hampshire, 03755.

#### # \* Lick Observatory Archives

The Lick Observatory Archives is housed in the University Library on the Santa Cruz Campus of the University of California. Since the move of the Observatory (offices andstaff) from Mount Hamilton to Santa Cruz in the mid-sixties, Mrs. C.D. Shane has collected and organised the personal and institutional collections of the Lick staff dating from the Observatory's beginnings in the late 19th century. The chief collections examined and used in this thesis are the papers of E.S. Holden, James Keeler, and W.W. Campbell. The organisation of the general Lick collection is chronological, and alphabetical within each chronological range, with the exception of letterpress copybooks containing outgoing director's correspondence. While a published finding aid is not available, the volunteer staff, headed by Mrs. Shane, maintains a detailed file card crossindex of the collection. Address: Lick Observatory Archives, The University Library, University of California at Santa Cruz, Santa Cruz, California 95060.

#### \* \* George Ellery Hale Papers, Microfilm Edition

The papers of George Ellery Hale were organized and microfilmed in the mid-sixties and are available in various locations, including the Mugar Library at Boston University and the Niels Bohr Library of the American Institute of Physics in New York City. The collection is contained on 100 reels of 35mm microfilm. A published guide is available from the Hale Observatories, 813 Santa Barbara Street, Pasadena, California 91101. The author has used the collection at the above two named sites, in addition to several reels purchased in past years from the Microfilm Company of Los Angeles, 1977 South Los Angeles Street, Los Angeles, California 90011. Any future users should note that Hale's scientific correspondence is contained in two alphabetical ranges. The first range is "Correspondence with Individuals" reels 1 - 40. and the second is "Director's Files of the Mount Wilson Observatory" reels 81 - 100. In this thesis, I have not recorded which range or reel was used in citation. Therefore, both ranges must be examined to determine exact origin. In addition, in the summer of 1977 the author visited the offices of the Hale Observatories and found a great

deal of Hale correspondence contained in early (1895 - 1904) letterpressbooks not included in the Microfilm Edition. None of this latter material has been used in this thesis, however.

#### \* \* Harvard University

The University Archives of the Harvard University Library contains two collections of E.C. Pickering letters -- his private and director's correspondence -- and two similar collections of the letters of Harlow Shapley. Only the Pickering collections were used in this thesis. The Pickering letterpress copybooks forming his outgoing Director's correspondence are located by the call number range UAY 630 XX.X, the last three digits ranging from 14.0 through 17, and comprising some 62 volumes of material from 1877 to 1918. His personal correspondence is located by the call letters HUG 1690.XX and runs through the indicated decimal range. I have not retained the individual call numbers of the folders or books containing cited items. Address: University Archives, Harvard University Library, Cambridge, Massachusetts 02138.

#### \* \* Norman Lockyer Papers, A.J. Meadows File

Professor Meadows has collected a number of copies of Lockyer correspondence obtained from the Norman Lockyer Observatory (Lockyer the Hill Observatory) at Sidmouth. The present location of the originals is not certain. Part of the original collection is at Exeter University, part are at the Royal Astronomical Society (Hill Observatory Records), and part are at the University Library, University of Leicester (Hill Observatory Records). Only those letters found in Professor Meadows'

collection have been used in this thesis. Address: Department of Astronomy, University of Leicester.

#### \* \* Princeton University

The Henry Norris Russell Collection is held in the University Library Manuscript Division. The collection code is AM 15959, and it is contained in 49 transfer boxes. A preliminary list of the contents of the boxes exists, but the organisation is rough and in places unreliable. Generally, cited correspondence comes from boxes 26 through 31, and other material cited comes from boxes 8, 22, and 23. The author is presently preparing a grant proposal to organise the Russell Collection.

Princeton also holds a very small collection of C.A. Young papers which mostly deal with observatory matters during his tenure at Princeton. Address: Princeton University Library, Manuscripts Division, Princeton, New Jersey 08540.

#### \* \* National Archives

The personal and scientific papers of Jonathan Homer Lane are contained in the Industrial and Social Branch, Civil Archives Division, The National Archives and Record Service, Washington DC 20408. A preliminary finding aid is available for his papers, which cover the period 1859 - 1878, identified as Job No. III - NIR - 324, NBS Accession. The latter designation identifies the source of the collection as the National Bureau of Standards, and the collection is part of the records of that organisation, identified as Record Group 167. The cited Lane

material was identified from the preliminary finding aid by item number, as reproduced in the citations in this thesis. These item numbers seem subject to change, however, as the listing was clearly provisional and revisions had already been made several times.

#### \* \* Yale University

A few Lane letters to and from E.C. Herrick are to be found in the Herrick Papers, Manuscripts and Archives, Sterling Memorial Library, Yale University. Their location code is Bl7a, folders 6 - 7 and the date range is 1847 - 1859.

#### \* \* Yerkes Observatory, University of Chicago

The Yerkes Observatory Library contains about ten fourdrawer filing cabinets filled with correspondence of the directors and staff of the observatory since its beginnings in the mid 1890s through the 1930s. The chief collections might be identified with G.E. Hale, E. Frost, and O. Struve. Citations from the Hale and Frost period have been used in this thesis. At the times of inspection (Summer 1966, Winter 1971, Summer 1974) the collection was housed in the attic of the observatory, without protection from moisture, heat, or vermin. The organisation of the collection was by rough chronological periods (from four years to a decade) and alphabetical within each period. It is understood that the central library (Joseph Regenstein) at the University of Chicago is arranging, through partial support of the American Institute of Physics, for the organisation and transfer of this important collection. Actual present status unknown. Address: Yerkes Observatory, Williams Bay Wisconsin 53191.

#### \* \* Royal Society

Material examined in the Library, The Royal Society, for this thesis centred around correspondence and referee reports. The correspondence included letters from William Huggins and Norman Lockyer, correspondence and other communications concerning their submitted manuscripts, and the unpublished manuscripts themselves (Misc. Manuscripts MM 7 3 - 5; MM 16 55; Archives Papers AP <u>67</u> 3, 8; Schuster Lätters SC 44 - 402, not inclusive). Referee Reports, as submitted to the Secretary of the Royal Society, were examined for papers submitted by Lockyer. Reports by Christie and G.H. Darwin were cited (RR <u>11</u>: 63 - 66; 181 - 183). These codes included Lockyer's responses to the Secretary. Address: The Library, The Royal Society, 6 Carlton House Terrace, London, SWI Y5AG.

### \* \* Karl Schwarzschild Papers, Microfilm Edition

Copies of the Karl Schwarzschild Papers Microfilm Edition are at the Niels Bohr Library of the American Institute of Physics, and the Princeton University Observatory. The copy at the AIP was used to examine discussions between E. Hertzsprung and Schwarzschild during the period 1908 - 1910 (Reel 5, sections 2 and 3). Correspondence is arranged alphabetically by correspondent. A finding aid is available. Address: Niels Bohr Library, The American Institute of Physics, 335 e 45 Street, New York City 10017.

#### \* \* Cambridge University Observatory

Letterpress copybook collections of A.R. Hinks and R.S. Ball letters were examined, as well as Ball scrapbooks, for documentation concerning Henry Norris Russell's years at Cambridge, 1902 - 1905. Material cited is from the Hinks Collection, in the archive room at the Library, Cambridge University Observatory.

Select Bibliography of Books and Journal Articles

The material cited here has been, for the most part, cited within the text of this thesis. Several items have not been explicitly cited, and a few were read for background only.

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