Plausibility and the Theoreticians' Regress: Constructing the Evolutionary Fate of Stars

by

Alex I.Ipe, B.A./M.A.

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The undersigned recommend to

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Chair, Department of Sociology and Anthropology

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External Examiner

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Abstract

This project presents a case-study of a scientific controversy that occurred in theoretical astrophysics nearly seventy years ago following the conceptual discovery of a novel phenomenon relating to the evolution and structure of stellar matter, known as the limiting mass. The ensuing debate between the author of the finding, Subrahmanyan Chandrasekhar and his primary critic, Arthur Stanley Eddington, witnessed both scientists trying to convince one another, as well as the astrophysical community, that their respective positions on the issue was the correct one. Since there was no independent criterion – that is, no observational evidence – at the time of the dispute that could have been drawn upon to test the validity of the limiting mass concept, a logical, objective resolution to the controversy was not possible. In this respect, I argue that the dynamics of the Chandrasekhar-Eddington debate succinctly resonates with Kennefick's notion of the Theoreticians' Regress. However, whereas this model predicts that such a regress can be broken if both parties in a dispute come to agree on who was in error and collaborate on a calculation whose technical foundation can be agreed to, I argue that a more pragmatic path by which the Theoreticians' Regress is broken is when one side in a dispute is able to construct its argument as being more plausible than that of its opponent, and is so successful in doing so, that its opposition is subsequently forced to withdraw from the debate. In order to adequately deal with the construction of plausibility in the context of scientific controversies, I draw upon Harvey's Plausibility Model as well as Pickering's work on the role socio-cultural factors play in the resolution of intellectual disputes. It is believed that the ideas embedded in these social-relativist-constructivist perspectives provide the most parsimonious explanation as to the reasons for the genesis and ultimate closure of this particular scientific controversy.

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Introduction

Contemporary sociology of science is primarily concerned with the techniques and practices that scientists employ to generate a rational, conceptual image of the physical world. Indeed, one of the most fascinating – as well as highly controversial – conclusions of this field of study is that scientific knowledge is not simply the end product of intellectual, logical analyses of nature, but is in fact inexorably intertwined with factors that exist outside the narrow parameters of cognition, factors which ultimately reside in the larger socio-cultural environment in which scientific practice is undertaken. Indeed, those who study the social fabric of scientific research have taken this notion a bit further, arguing that the seemingly authoritative statements and conclusions generated by the scientific establishment are, in essence, by-products of social negotiation; this bold statement raises the distinct reality that scientific knowledge claims can never be absolute, but is highly susceptible to alternative conceptualizations and interpretations (e.g.Bloor,1976; Collins,1975,1981; Harvey,1981; Mendelsohn,1977; Pickering,1981,1984,1992; Pinch,1981; Wynne,1976).

While many physical scientists would view the above comments with no small amount of disdain, the reality remains that scientific practice is an endeavour constituted by human beings interacting with one another in a highly specialized socio-cultural environment. It is, in fact, an activity that is governed by rules, logics and techniques that have been wrought, chiselled and refined by human beings existing in social groups; consequently, the findings generated by science is as much influenced by the social as it is of cognitive deliberation and study (Kuhn,1970; Mendelsohn,1977).

Interestingly enough, many scientists – both natural and social – as well as a large majority of the public perceive science as a practice in which social forces play little or no role. In this particular conceptualization of science, one seems to gaze upon scientists almost as if they were androids, the knowledge they produce, carefully cultivated from the complex web of nature

through sheer intellectual effort and a thoroughly objective examination of data. In this respect, when one beholds mathematical statements such as Albert Einstein's very famous, $E = mc^2$, one is almost lulled into a state of mind that interprets such symbolic formulism as being crafted by the forces of nature as opposed to being constructed by human beings, and thus assumes that it must be tantamount to absolute truth. To emphasize this notion metaphorically, as Harry Collins (1975) did so elegantly over two decades ago, it is as if scientific knowledge is perceived as a ship in a bottle; as Collins states, "A ship *within* a bottle is a natural object in this world, and because there is no way to reverse the process, it is not easy to accept that the ship was ever just a bundle of sticks" (Collins,1975:205).

In this project, I am primarily interested in examining the processes by which sociological factors are intertwined with the production of scientific knowledge. It is, in essence, a study that is guided by a relativistic-constructivist approach that has come to dominate current research into the social nature of the scientific enterprise (e.g. Barnes,1982; Bloor,1976; Collins,1975,1981,1985,1999; Gieryn,1999; Gilbert & Mulkay,1984; Gooding,1992; Harvey,1980,1981; Hess,1997; Knorr-Cetina,1992; Latour & Woolgar,1986; Latour 1987, 1988a, 1993,1999; Pinch,1977,1981,1994; Pickering,1980,1981,1984,1992,1995; Pickering & Stephanides,1992; Rouse,1987; Shapin,1994; Simon,1999; Star,1983; Star & Gerson,1987; Woolgar,1988), an intellectual tradition that arose in the wake of landmark works by scholars such as Popper (1959), Kuhn (1957,1970,1977) and Lakatos & Musgrave (1970). It is hoped that this specific conceptual orientation will facilitate the examination of a controversy that occurred in astrophysics some seven decades ago over a theoretical discovery that strongly challenged the prevailing orthodoxy of the time with respect to the evolution and fate of stars.

While a relativistic perspective is employed for the purposes of this project, it should be noted that the examination of the nature and structure of scientific knowledge and scientific controversies is by no means an enterprise that is the exclusive domain of the constructivist camp. Indeed, over the years, anthropologists, historians, philosophers, scientists and sociologists – who

are not necessarily relativists – have contributed to academic comprehension of the complex phenomenon that is science (see for example Baltas,2000; Bateson,1987; Brush,1976,1983,1986,1988,1990; Cohen,1985; Dolby,1975; Duncan,1974; Frankel,1987; Franklin,1979,1983; Freudenthal,2000; Galison,1987; Giere,1987; Greene,1980; Heidelberger,1980; Hilgevoord,1994; Holton,1978; Hull, Tessner & Diamond,1978; Kitcher,2000; Laudan,1980; Laughlin & d'Aquili,1974; Mamiani,2000; Markle & Petersen,1987; McMullin,1987; Oreskes,1999; Rubinstein et. al,1984; Rudwick,1985; Ruse,2000; Salmon,2000; Stewart,1986; Traweek,1988, Wallace,2000).

With respect to the sociology of scientific knowledge, it is a curious aspect of the field that much of the research that has been published investigating the sociological nature of scientific activity has focused primarily on experimental science, with similar analyses of the theoretical nature of science not as prevalent in the literature, though there have been a few critical works in this area (e.g. Kennefick,2000; Latour,1988b; MacKenzie,1996,1999; Pinch,1977; Pickering,1980,1984,1992,1995). This reality is somewhat ironic given the great interest that scientific specialities such as theoretical physics has generated among social scientists; yet, despite this intense curiosity, it appears that a majority of the publications generated by sociologists of science in past years tend to concentrate upon the socio-cultural structure of experimental physics and its attempt to test certain predictions born of theoretical research (e.g. Collins,1975,1981,1985,1999; Harvey,1981, Pickering,1981; Pinch,1981,1994; Simon,1999; Woolgar,1988; Wynne,1976).

In itself, this is not terribly surprising; on the face of it, theoretical physics tends to have an image of being an intrinsically complex, conceptual endeavour with scientists engaged – seemingly in solitude – in wrestling with highly abstract logic and mathematical symbolism, entities that are perceived to be the by-products of purely rational thought and thus, appear to be more impervious to social analysis than experimental physics where the design and construction of experiments, as well as the interpretation of experimental data, tends to occur within the setting

of social groups thus providing a wealth of data for the curious sociologist about scientific practice and the construction of knowledge. At least, this is the popular perception.

Yet, recent social analysis of theoretical physics reveals this public view of the discipline to be grounded in a less than solid empirical foundation (e.g. Pickering,1984,1995; Pinch,1977; Kennefick,2000). Indeed, what these studies have shown is that the nature and legitimacy of theoretical knowledge is as much influenced by social factors, as is knowledge generated by experimental research. As will be demonstrated in this project, while the work of theorists is governed by explicit mathematical rules and the logic of physics, there is no guarantee that the fruits of such cognitive investigation will be accepted by the scientific community. Indeed, given the fact that much theoretical research rests upon technical matters and conceptual assumptions, both of which are susceptible to academic critique and judgment, the potential for scientific disputation is as great in theoretical physics as it is in its experimental counterpart, especially if novel, unorthodox conclusions are reached (Kennefick,2000: 5-6; Pickering,1984:404-407).

Perhaps, what is most intriguing about theoretical physics is that theoreticians attempt to comprehend physical phenomena that are often beyond the reach of convenient laboratory analysis (stellar objects such as the sun, or more exotic entities such as neutron stars and black holes). Thus, in order to unlock the secrets of nature, physicists must turn to the abstract tools of mathematics in seeking solutions to many academic questions concerning the reality in which they live; almost inevitably, these solutions take the form of paradigms – conceptual models – that tend to offer theoretical explanations for a given set of observations, explanations whose validity can be *potentially* tested against empirical observations; I say *'potentially'* as a means of acknowledging situations that have arisen in the history of science in which the full empirical soundness of a novel, theoretical framework remained in doubt for years due to the technical limitations inherent in the experimental or observational apparatus of the time in which the young paradigm emerged into the intellectual consciousness; indeed, perhaps the most famous example of this is Einstein's special theory and general theories of relativity which had to await the

passage of decades before they were considered beyond refute (Clark,1972; Douglas,1956; Hawking,1988; Kaufmann,1988).

The implication of the above statements is most readily obvious when situated within the context of scientific controversies; in such situations when an unorthodox paradigm appears upon the academic landscape, one that essentially offers a fresh, conceptual explanation regarding a given natural phenomenon that strongly challenges the traditional model, it becomes difficult to decide between the two theories solely on the basis of mathematical and physical logic. Given this reality, the respective supporters of both paradigms end up critiquing the technical details and conceptual assumptions inherent in the other's theory, giving rise to a virtually endless cycle that may perpetuate itself for countless years. This problematic issue essentially gives birth to an intellectual scenario that Kennefick has christened the 'Theoreticians' Regress', which is very much a conceptual extension of Collins' 'Experimenters' Regress'; in short, it describes the practical difficulty of choosing between rival theoretical models in the near complete absence of empirical support for either model, by simply critiquing a disputed calculation (Kennefick,2000:6).

Although the influence of cultural factors on controversies in theoretical physics has been addressed to some extent by the literature, the social processes by which such disputations are ultimately resolved has not received as much attention, partly due to the fact that, as stated previously, much of the pioneering research conducted in the sociology of scientific knowledge tends to concentrate on the experimental work of physicists as opposed to their purely theoretical investigations. The aim of this study represents an attempt to push the focus more toward the conceptual endeavours of the discipline in order to provide a more holistic comprehension of what is perhaps one of the most socially examined fields in the physical sciences.

1.1 Research Question/Issue

This project deals with a controversy – known as the Chandrasekhar-Edington controversy – that arose in astrophysics over seventy-years ago as a result of a purely theoretical discovery on

the topic of stellar evolution made by a young doctoral student named Subrahmanyan Chandrasekhar; briefly stated, Chandrasekhar's research strongly suggested that stars whose initial masses were greater than 1.44 solar masses would not retire and die as white dwarf stars, but would evolve into some other kind of entity whose specific qualities were unknown at the time. Suffice it to say, this result forcefully contradicted the conventional wisdom of the period that proclaimed that all stars, irrespective of their mass, would fade into oblivion as white dwarfs[1] (Evans,1998:143-146; Gupta,1995:200-201; Horgan,1994:500-501; Nityananda,1995:554; Parker,1995:106-108;Penrose,1997:62-63;Wali,1982:34-36;1991:76-77,117,119,123-124).

In many respects, this scientific dispute constitutes an intriguing case-study that has, thus far, not received any attention from sociologists or historians of science, though it has been discussed in detail by Chandrasekhar's biographer, Dr. K.C. Wali, and to some degree by Chandrasekhar himself, and a number of other scientists (see Chandrasekhar,1969,1972,1987; Douglas,1956; Evans,1998; Gupta,1995; Horgan,1994; Nityananda,1995; Penrose,1997; Wali,1982,1991,1997). One of its important lessons is that it is very difficult for a novel theoretical model to cast doubt upon the conceptual validity of an older, traditional framework by relying purely on the basis of its mathematical design, especially if the new paradigm makes predictions whose validity cannot be conclusively determined due to the lack of an independent measure of quality.

In many ways, this dilemma is very similar to that experienced by experimental physicists who claim to have discovered an elusive phenomenon, or have generated empirical evidence that throws the validity of a well-established theory into question; in many of these cases concerns were raised regarding the assumptions used in designing the experiment, the possibility of technical errors, the thoroughness of the analyses, the plausibility of the subsequent

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[1] White Dwarfs are tremendously compact stars that, essentially, have the mass of the sun but only possess approximately, 3.9×10^{-7} the volume of the sun; this physical structure results in a density that is over three million times the density of water (Gatewood & Gatewood,1978:191,195;Giancoli,985:185; Kaufmann,1988:359-360,609; Lasota,1999:42; Serway,1994:334).

interpretation, and the skill of the experimentalist (see Collins,1975,1985; Gieryn,1999; Pinch,1994; Simon,1999).

In his investigation of a controversy occurring in gravitational physics, Collins (1975,1985) describes the experiences of a scientist who purported to have built an instrument capable of detecting an elusive, physical phenomenon known as gravity waves, a phenomenon no one had ever empirically measured before, but believed existed due to theoretical considerations. Not surprisingly, the claim generated much debate, and soon, consensus began to form that the results were flawed. However, attacking the experiment alone did not conclusively resolve the matter as there was no objective criterion with which to measure the quality of the experiment, thus, giving rise to a circular pattern of logic. As Collins explains,

> ...The ... reader might feel that, given some time, he or she now knows how to build a gravity wave detector...
>
> [However] ... we will have no idea whether we can do it until we try to see if we obtain the correct outcome. *But what is the correct outcome?*
>
> What the correct outcome is depends upon whether there are gravity waves hitting the Earth in detectable fluxes. To find this out we must build a good gravity wave detector and have a look. But we won't know if we have built a good detector until we tried it and obtained the correct outcome! But we don't know what the correct outcome is until ... and so on *ad infinitum*.
>
> Where ... a clear criterion is not available, the experimenters' regress can only be avoided by finding some other means of ***defining the quality of an experiment***; a criterion must be found which is independent of the output of the experiment itself (Collins,1985:83-84; emphasis added).

In short, what Collins' analysis shows is that doing an experiment – setting up the apparatus, ensuring that all the instruments have been properly calibrated and functioning within acceptable parameters, that extraneous variables that could potential influence the experiment have been accounted for, and finally, executing the experiment – essentially constitutes a matter of competence and skill, and during the tenure of a controversy there are no clear guidelines of how that skill is to be independently assessed (Collins,1985:83-84; Pinch,1994:93).

In much the same fashion, these very issues tend to materialize during the tenure of disputations in the theoretical branch of physics where a problematic result cannot be assessed in

an objective fashion due to the absence of an agreed upon criterion for defining the quality and validity of a theoretical calculation.

However, the social character of these debates becomes all the more interesting when duelling conceptual models purport to explain a physical phenomenon for which empirical verification is extremely difficult at best. Under such circumstances, the probability that the unorthodox research programme can significantly cast doubt upon the credibility of the traditional framework is very low, especially if the orthodox model is perceived by a community of scientists as being more ***plausible*** than its rival on non-empirical grounds. In this fashion, an alternative vision of nature may not be taken seriously, or ignored altogether, at least for a given period of time, even though it cannot be conclusively proven to be completely erroneous in design (Collins,1975,1985; Harvey, 1981; Kennefick,2000; Pickering,1981; Pinch,1977).

In specific terms, the primary research questions I will focus upon in this study are: (1) When faced with two competing theoretical models, how do theoreticians evaluate which model is "correct" if experimental or observational verification of the rival paradigms is difficult at best? and (2) What are the processes through which this evaluation is achieved? In essence, my aim in this project is to demonstrate that the assessment of a novel, theoretical model is a direct function of the cultural and social context in which it emerged; in this respect, the legitimacy of a given body of conceptual knowledge is more a consequence of social agreement and action than it is of a rigorous, objective examination of an alternative cognitive image of the physical world.

In short, I argue that when scientists are faced with a disputed theoretical result that is not amenable to empirical analysis, a circular pattern of reasoning will emerge that resembles the "theoreticians' regress"; in addition, it will also be argued that this regress will only be broken when one of the conceptual viewpoints is perceived – due to various technical, cultural reasons and even metaphysical reasons – by scientists as being more plausible than its rival. While these two conceptual perspectives have been employed individually in the analysis of distinct cases involving scientific disputations (e.g. Harvey,1981; Kennefick,2000), they have never been

combined theoretically for the purposes of investigating a specific intellectual controversy, a reality this study will attempt to address (Harvey,1981:96; Kennefick,2000:6,31;Pinch,1994:93).

To this effect, I will draw heavily upon the theoretical tradition that has come to dominate the social studies of science, namely, the relativistic-constructivist approach which argues that neither the rules of logic, nor the character of empirical data itself can decisively determine whether or not a new knowledge claim will be ultimately accepted by the scientific community given the reality that the evaluation of such claims occurs in a social milieu, in a intellectual context wrought by human beings (Harvey,1981:95-96; Jones,2000:317-318; Mendelsohn,1977:3-4). As such, research undertaken in the sociology of scientific knowledge (SSK) not only attempts to challenge the popular notion that scientific knowledge is essentially the by-product of a purely logical and objective analysis of reality, but also to demonstrate that conceptualizations of the natural world are negotiated, constituted, constrained and ultimately constructed as much by socio-cultural factors as technical ones (Jones,2000:318; Woolgar,1988:53-66).

In effect, it is hoped that this theoretical approach will permit an effective analysis of the Chandrasekhar-Eddington controversy, an approach that not only permits the reader to view the controversy as a by-product of a number of socio-political factors, but to also appreciate, in a more aggregate sense, how the social strongly influences the way scientists themselves perceive and construct the very reality they study.

1.2 The Map of the Thesis

This research project commences with a detailed discussion of the theoretical and conceptual background for the dissertation. The specific nature of the analytic perspectives that have been selected to guide the conceptual structure of this dissertation – specifically, the Theoreticians' Regress and the Plausibility Model – is fully presented in Chapter 2. In addition, an expansion of the criteria for the genesis of the theoreticians' regress is proposed. Furthermore, an argument is made that these two models can be linked conceptually in order to comprehend the birth and resolution of controversies in the theoretical sciences. Based upon this discussion, Chapter 3

outlines the methodological strategy that was adopted in order to effectively comprehend the diverse socio-cultural and technical background of the Chandrasekhar-Eddington controversy. Since this particular scientific dispute occurred nearly seventy-years ago, I had to rely on a large reservoir of primarily documented resources in order to adequately understand the reasons this debate first arose in the astrophysical community, resources that are archival, primary and secondary in nature. I believe the range of documentation studied during the course of this project to be relatively exhaustive, and is presented in sufficient detail to allow the reader to sufficiently replicate my findings.

Chapters 4 to 5 form the historical and analytical heart of the project. In specific terms, Chapter 4 provides a very detailed and balanced historical overview of the Chandrasekhar-Eddington controversy. The goal of this presentation is to tell the story of this little known dispute in a fashion that would be perceived as symmetrical as possible by the audience; that is, to say, a presentation in which the author does not take sides in the controversy, but attempts to communicate the logic and rationale of both parties involved in the dispute in a non-prejudicial and equivalent manner (Pinch,1994:89). Chapters 5 and 6 focus upon analyzing the dynamics of this controversy from the theoretical perspectives provided by the theoreticians' regress and the plausibility framework, arguing that these two models provide the most parsimonious explanation for the origin, evolution and eventual closure of the dispute. The concluding chapter presents a concise synopsis of the results and discusses a potential conceptual avenue for sociologists of science to explore in future investigations of not only this specific case study, but other intellectual controversies in the physical sciences.

2.0 The Theoretical and Conceptual Background

For much of the recent history in the social sciences, sociologists have been content to examine the social structure of science and its influence on society, and in this respect, works adhering to this academic tradition can be traced back a number of decades to researchers such as Barber (1953,1962), Cole (1970), Cole & Cole (1973), Crane (1965), Holland (1957), Merton ([1935] 1962,1968) and Zuckerman (1967a,1967b,1970, 1977) just to name some of the more prominent sociologists of the past who conducted a number of pioneering studies into the socio-cultural nature of the scientific enterprise.

Indeed, it is only relatively recently that social researchers have turned their focus toward conducting a thorough examination of the conceptual content of science. In itself, this early lack of attention paid to the cognitive composition of science is not wholly surprising. From its earliest days, sociologists like Karl Mannheim ([1929] 1954), believed that the essence of scientific practice – the testing of theories by experimentation – was beyond the influence of social variables; truth was an entity that was looked upon as an entity that did in fact exist, and need only be revealed by diligent and patient work; in this respect, discovering truth was analogous to discovering a new and exotic creature hidden in the depths of an unexplored forest, simply waiting to be revealed by an adventurous explorer (Cole,1992:3-4; Jones,2000:318; Oudshoorn,1996:122). Perhaps, Stephen Cole summarizes this perspective best when he writes:

> In natural sciences, at least, a "truth" existed, and any scientific theory which did not express that truth would eventually be found inconsistent with empirical data and ultimately discarded. In this sense, ***natural scientists were trying to discover the next page of a book that had already been written***, whose conclusion, though currently unknown, was predetermined or inevitable. Nature, rather than sociological processes, determined the way in which scientific knowledge developed (Cole,1992:3; emphasis added).

Although contemporary sociologists of science have long since rejected the above cited image of science, many philosophers, historians of science and scientists themselves still cling strongly to this idealistic vision of scientific practice, and have forcefully critiqued the social constructivist

view (see Franklin,1994; McMullin,1987; Roth & Barrett,1990a,1990b; Weinberg,1994). Yet, despite this opposition to their work, sociologists of science – or more accurately, sociologists of scientific knowledge – have forged ahead, generating a large number of exciting works that have attempted to demonstrate that knowledge is an outcome of a number of heterogeneous forces, and is not simply wrought by the hands of pure logic and objectivity. In this respect, the apparent validity or "truth" of a given theoretical or empirical finding does not reside solely in the finding itself, but is acquired through a complex set of social processes; if this were not the case, then scientific disputations would be a rare sight indeed in the halls of science for under such a reality, one need only look to a mathematical derivation, or an experimental result in order to assure oneself that a given piece of knowledge is in fact legitimate (Bijker,1995; Jones,2000:318; Woolgar,1988:53-66).

As the above discussion indicates, the analysis of the nature of scientific activity must, inevitably, take into consideration socio-political and cultural variables if one is to gain a holistic understanding of the processes by which knowledge is evaluated. In this respect, two conceptual perspectives developed over the years in the sociology of science – specifically, the 'theoreticians' regress', and the 'plausibility model' – will be employed to gain a tentative comprehension of these processes as they relate to the Chandrasekhar-Eddington controversy; consequently, the theoretical foundations of both concepts are presented below.

2.1 The Theoreticians' Regress

One of the most interesting sociological findings to emerge from the domain of science studies is that experiments and theoretical models do not have the capacity, in and of themselves, to resolve an intellectual controversy.

For example, when a scientist announces that a new phenomenon has been detected within the strict physical confines of a laboratory experiment, a phenomenon that has never been witnessed before, other scientists will inevitably attempt to replicate the experiment, which first produced this exotic result; this is exactly what transpired during the cold fusion controversy, when

laboratories around the world attempted to duplicate the seemingly astonishing claim by two chemists, Pons and Fleischmann, that they had reproduced the energy generating capabilities of the sun within the humble boundaries of a test-tube. Initially, a few research centres declared that they too had successfully discovered cold-fusion, but these announcements were soon retracted as errors were found with the laboratory apparatus and suspicion began to grow that Pons and Fleischmann had really discovered nothing at all. However, a few independent researchers did find positive results, but their claims were soon overshadowed by a growing consensus that cold fusion was a theoretical impossibility (Gieryn,1999; Pinch,1994; Simon,1999).

Yet, in the wake of the controversy what became very clear was that Pons and Fleischmann did not help their cause by providing colleagues with insufficient details with respect to the methodology they had used to generate their fantastic result; as a result, many of the "replications" undertaken by other scientists in the field were less than perfect carbon copies of the original experiment. This reality gave rise to the classic problem of replication often seen in scientific controversies: for supporters of cold fusion, negative results were due to significant discrepancies between the original experiment and the subsequent imitations; for opponents of cold fusion, negative results simply confirmed that the phenomenon did not exist and was simply a spurious result. This circular pattern of claims and counter-claims is what Harry Collins christened the "experimenter's regress" (Collins,1975,1985;Pinch,1994). As Pinch succinctly summarizes, "The problem is that doing an experiment is an exercise in skill and it is not clear during a controversy how the necessary skill is to be judged. For the believers, a competently performed experiment is one that will produce the phenomenon. For the critics a competently performed experiment is one that is negative ... The importance of this is that it shows that experiments are never alone capable of settling a controversy, ***other factors always have to be brought in to try to break the regress***"(Pinch,1994:93;emphasis added). Indeed, in practice, the experimenter's regress can only be broken if scientists can come to some sort of agreement as to what test should be used to establish a standardized criterion by which the quality of a given

experiment can be assessed; as Collins states, "... a criterion must be found which is independent of the output of the experiment itself"(Collins,1985:84).

While Collins' notion of the "experimenter's regress" was born out of observations gleaned from studying a controversy in the realm of gravitational physics in which a surprising empirical observation had been made, the circular pattern of reasoning that emerges during the tenure of scientific controversies is not solely restricted to disputes over experimental results, but does, in fact, manifest itself within debates concerning purely theoretical discoveries.

Indeed, Daniel Kennefick has extended Collins' concept of the regress to take into consideration controversies that occur in the domain of theoretical physics. In experimental work, a disputed result forces scientists to study the technical apparatus of the experiment (how was it accomplished?); similarly, when faced with a problematic theoretical finding, scientists will tend to focus their attention predominately on the technical 'apparatus' responsible for the discovery (was the calculation performed accurately?), as opposed to conceptual ones (are the assumptions underlying the analysis reasonable?).

As in experimental research, attempts will be made to replicate the discovery. However, replication in theoretical work is no easier than in its experimental counterpart; indeed, as already discussed, conducting an exact duplicate of a controversial experiment is difficult at best given the reality that the specific details of the experimental design, as well as the exact conditions under which the experiment was done is often unknown. The problem is further compounded by the fact that during such disputations, a universally agreed criterion of experimental quality is often absent, forcing both sides to proclaim specific experiments that reinforce their respective positions as the ones to be believed as they were properly executed; however, since there is no independent methodology to test the quality of an experimental design, researchers are thrown into a seemingly endless pattern of argument (Collins,1985:84; Kennefick,2000:6; Pinch,1994:93).

In a similar fashion, these very socio-cognitive issues tend to emerge in the context of theoretical work. In order to appreciate this statement one must first realize that many avenues of contemporary research in theoretical physics tend to employ what is known as 'numerical methods'; in practical terms this means that a scientist employs the aid of a computer to undertake complex, sophisticated modelling of a specific physical phenomenon; such models run on a computer program written by the theorist, and involve millions of lines of code that often possess a character and style that is unique to the person who has constructed the software. Indeed, given the reality that such programs take months to construct, and even longer to refine, the author of the algorithm, inevitably, develops a familiarity with the intricacies of the analysis that is difficult for colleagues to appreciate.

Thus, if the model makes predictions that contradict orthodox views, predictions that are not amenable to empirical investigation, physicists have to find alternative methods with which to judge the plausibility of the results. One such course of action would be to replicate the program that generated the unexpected result in the first place, run it, and see if the surprising outcome continues to appear. Of course, this is easier said then done. As mentioned earlier, since programs are written in a fashion that reflects the style of the creator, two models, wrought by two different scientists, purporting to investigate the same controversial phenomenon will, in all probability, generate two very divergent results (Kennefick,2000:24-28).

The problem is further complicated by the fact that the very style of a theorist influences how s/he approaches a given problem, the assumptions used and the logic employed, qualities that can make communication of a disputed result very problematic; this is especially true if the intellectual background and specialty of those presenting a controversial phenomenon is very different from their intended audience (Kennefick,2000:6,27-28). As Kennefick states, "If two groups of theorists find themselves working on a similar problem with very different methods, coming from different disciplinary backgrounds, the communication gap may prove almost

insurmountable. In such a situation, it may be impossible to resolve disagreement over the outcomes of their respective calculations" (Kennefick,2000:6).

In light of these factors, it is impractical for other theorists to gain an intricate familiarity with the problematic program responsible for the unorthodox theoretical prediction, and to truly see the model in the same fashion as its creator(s), thus making precise replication of the original code nearly impossible.

Given this cognitive scenario, how does a community of theorists, faced with a contentious knowledge claim, ascertain whether or not the novel discovery is in fact valid? Strong scepticism of the analysis is one way in which suspicion concerning the validity of a scientist's work can propagate throughout a given field, thus endowing it with very little credibility.

Another critical factor is how researchers themselves view the skills and talents of those who generated the unorthodox discovery; in specific terms, this includes an assessment of the scientist's approach to theoretical research, specific style, as well as intellectual reputation, factors that inevitably influence how a community of specialists perceives controversial results generated by a colleague. Indeed, if the surprising theoretical result was made by researchers whose theoretical techniques, conceptualization of a specific problem, and expertise differ even modestly from their critics who might dominate a particular field of inquiry, the credibility of these scientists will, inevitably, be thrown into question, and thus, the credibility of their findings (Collins,1975:214-215,219-220;Kennefick,2000:27). As Pinch states, "The struggle between proponents and critics in a scientific controversy is always a struggle for credibility. When scientists make claims that are literally "incredible,"... they face an uphill struggle" (Pinch,1994:96).

Thus, theorists face the same quandary as their experimental counterparts: like an experiment, a complex mathematical model is a product of a great amount of skill and talent, and under the shadow of an intellectual disputation, it is very difficult to ascertain how that skill and talent is supposed to be measured. For the supperters, a competently designed model is one that will

produce the unorthodox result; for the opponents, a competently designed model is one that will not produce the disputed phenomenon. In this respect, whether or not the surprising claim is real is a direct function of which model is deemed to be competently constructed, an assessment that is highly subjective in nature, at least to a certain degree (Kennefick,2000:24-27; Pinch,1994:93). As Kennefick states, the theoreticians' regress denotes "... the dilemma theorists face when the best or only check of a calculation is its own disputed result...."(Kennefick,2000:25).

At this point, I would also like to argue that the communication/perceptual difficulties between two competing camps that have been highlighted above is further hindered in some cases by what may be termed *metaphysical* considerations, a concept Kennefick does not address in his framework; indeed, at times proponents for and against an unorthodox discovery or conclusion may adhere to incompatible belief systems concerning the nature and structure of the physical world that are very difficult to overcome, or to easily set aside, thus preventing one side in a dispute from accepting a controversial finding, or perceiving it as being the outcome of a skilfully constructed apparatus. Indeed, one might point out that a metaphysical component was involved, at least to a certain degree, with the initial resistance by scientists to Einstein's special relativity theory, and even in Einstein's own adamant rejection of some of the principles of quantum mechanics. In addition, metaphysical reasons are cited as playing a not insignificant role in the scientific controversy that erupted between physicist C.G. Barkla and the orthodox establishment over the J phenomenon in theoretical physics in the early years of the twentieth century (Clark,1972; Hawking,1988:56; Wynne,1976).

In essence, from a practical stance, the only way for scientists to resolve the regress is for all camps involved in the controversy to somehow come together and find a common, conceptual – metaphysical – language that will allow both sides in the debate to comprehend the logic and perspective of the other, thus facilitating the creation of a model whose theoretical foundations can be supported by the respective parties. Of course, this is easier said then done. However, in the absence of such a shared viewpoint, history shows that the unconventional finding will come

to be seen as less and less feasible over time until, eventually, it becomes so marginalized that it simply fades from the academic consciousness (Collins,1975:219-220; Kennefick,2000:31; Pinch,1994:97-99).

In articulating the notion of the theoreticians' regress, Kennefick's primary goal is to demonstrate that when two rival mathematical models make incongruent predictions concerning a given phenomenon, predictions that are beyond the scope of empirical analysis, it is practically impossible to choose between the two frameworks by simply critiquing the calculation. Since there are countless avenues by which a complex calculation can generate an erroneous result, not to mention a number of extra-scientific factors involved in such a debate, this particular strategy is incapable of conclusively settling a conceptual disputation.

Yet, the theoreticians' regress need not only emerge in controversies that involve a sophisticated computer model ploughing through thousands if not millions of computations that cannot be precisely duplicated, and which, in the end, happens to produce an unorthodox result, but can manifest itself in disputes involving research born of pen and paper analysis. In this respect, the concept of the regress need not be limited solely to debate over mathematical modelling, or situations in which replication is not practical (Kennefick,2000; Pickering,1995;Pinch,1977).

Indeed, if anything, the essence of Kennefick's work demonstrates that disputes that give rise to the theoreticians' regress are the result of an inability to communicate, of theorists approaching a given problem in a unique fashion, employing a technical language, logic and possibly world view, that is not readily comprehensible to colleagues whose training, experience and beliefs in the area might be very different. Thus, even if the troubling model, or analyses is perfectly amenable to replication, the emergence of a regress is still highly probable (Kennefick,2000:6,33). As Kennefick states, "Even when two theorists are experts in the same topic, and therefore share a theoretical tradition, ***their experiences of a particular problem or***

calculation will differ sufficiently to make comparison of their [technical]***'apparatuses' very problematic***"(Kennefick,2000:6;emphasis added).

As such, one can argue that, whether one is a theorist who works with computer models, or is a scientist who confines him/herself to handwritten manipulations of algebraic symbols where each step can be traced and followed, if h/she discovers an unexpected result which contradicts accepted values, one whose validity cannot be assessed through observational or experimental means, or judged against a well established, independent criterion, the potential for the emergence of the theoreticians' regress becomes highly probable.

Ultimately, the community affected by such a debate must decide which proclamation to believe, and such a decision is inevitably influenced by whether or not individual scientists perceive a given analysis to have been skilfully and competently performed. Yet, in such situations, perceptions of competence also strongly depend on whether one has confidence in the legitimacy of the disputed phenomenon in the first place (Kennefick,2000:32-33). As Kennefick concisely summarizes, "Depending on one's frame of reference, one is generally free, in any given instance, to view an apparently non-confirming result as a refutation of the original result, or as a clue to its real nature and origin: but, obviously, ***this choice depends on whether or not you believe in its existence***"(Kennefick,2000:32; emphasis added).

2.2 The Plausibility Model

The above exposition presented the essential elements of a phenomenon known as the theoreticians' regress that tends to emerge during disputes over a controversial theoretical discovery, a discovery whose empirical reality cannot be subjected to experimental or observational tests. According to the theoreticians' regress, the only viable means of resolving the regress is for the various scientists involved in a scientific debate to make a serious attempt to agree on some fundamental technical and conceptual issues, and to adopt an intellectual language that will permit the other to understand the logic of his/her colleague (Kennefick,2000:21,31-34).

However, the development of such a linguistic bridge is extremely difficult from a practical standpoint, at least in the short term of an academic controversy. As such, I argue that under most conditions, the theoreticians' regress is broken when the orthodox idea comes to be seen, not so much as constituting the truth of the physical phenomenon under investigation, but as much more *plausible* than its troubling counterpart; in this respect, the "winning" party has not achieved victory because it comes to possess the ultimate proof of the validity of its claims, but because it has consolidated the notion of plausibility in its favour (Harvey,1981:96-97,124). The concept of scientific plausibility, and the various factors that compose and influence its structure will be discussed in detail below.

One of the most curious features of science is how, after an overwhelming level of academic consensus has been reached concerning the validity of a particular body of knowledge or a specific theoretical model, it comes to be perceived by scientists and non-scientists alike as constituting the truth; an excellent example of this would be Albert Einstein's special and general theories of relativity which have, over the intervening decades since their emergence into the public domain, achieved a near mythical status in the eyes of the scientific community and the public, alike, so much so that both theoretical frameworks appear to comprise the final word on the phenomena they attempt to elucidate, wholly impervious to criticism (Clark,1972; Hawking & Penrose,1996).

In many respects, one can argue that such intellectual works represent, at least in a metaphoric sense, a black box, knowledge that is looked upon as so obvious and commonsensical that its essential structure and composition is not questioned (Bijker & Law,1992:10; Jones,2000:319). As Jones writes, "Whether we consider the double-helix ... the gendered form of the bicycle ... or the intensity or colour of a fluorescent light bulb ... black boxes are the knowledge and technologies which we take for granted in everyday life. They are that which we assume to be common knowledge and whose meaning we no longer consider except when taken as a whole" (Jones,2000:319).

But why does a given black box – a given "truth" – continue to be accepted and perceived as such even after generations of existence, often facing little or no opposition? One can argue that this is because the knowledge which the black box represents is judged to be more "plausible" or "believable" than any prospective alternative; in this respect, a belief that a theory is true implies that it is perceived as plausible in the eyes of a supporter. What makes this conceptualization effective for the social theorist is that it can be pragmatically employed in investigating scientific controversies in which a dominant paradigm successfully resisted the challenges of a rival theoretical framework; such an effective campaign need not necessarily be the result of overwhelming empirical support, or even any real empirical evidence whatsoever, but because the orthodox model came to be perceived by scientists – due to a variety of factors – as more plausible than its potential replacement (Harvey,1981:96).

Since scientific activity is an inherently social phenomenon, it stands to reason that a critical determinate in comprehending why a given body of knowledge is seen as more plausible than another, in effect, lies with the behaviour of scientists themselves, in the intellectual paths they *chose* to travel and the judgments they *chose* to pass. In this fashion, the notion of plausibility comes to be intertwined with social action as much as with the proclamations of empirical data.

This is not to imply that scientists *arbitrarily* chose to perceive one theoretical framework, or body of data, as more believable than a potential alternative, but rather to suggest that the perceptions scientists have of a given experiment or paradigm, and the subsequent choices which flow from those perceptions are, in essence, dependent on having faith in some fundamental principles derived from the culture of their specialty; indeed, as anthropologists have demonstrated, a given culture not only moulds and shapes an individual's view of the world, but it also sets clear boundaries on the type of explanations that can be invoked to comprehend a given phenomenon. As such, faith in the certainty of supposedly well-established traditional notions is critical if scientists are to convince themselves of the validity of a specific body of beliefs. Thus, for a scientist to state that a given *experiment or theory is believable* means he or

she has *faith in believing that a given experiment or theory is believable*. Inevitably, this faith in the underlying concepts and principles of their discipline, strongly influence the distribution of plausibility in the context of scientific disputations (Bateson,1987:96-97; Collins,1975:206-208,217; Harvey,1981:97,124; Kottak,1991:40-41;Mendelsohn,1977:3-4).

At this point it is perhaps instinctive to ask how scientists come to have faith in the conceptual tools of their specialty. One answer is that these abstract tools have, in the past, proven highly effective in the elucidation of a given set of problems relating to a specific phenomenon, and as such, have come to constitute a socially accepted cultural resource for investigating a given physical problem. In time, these problems come to constitute what Thomas Kuhn referred as 'exemplars', meaning 'shared example'. As Kuhn wrote,

> ... By it [exemplar] I mean, initially, the concrete problem-solutions that students encounter from the start of their scientific education ... All physicists, for example, begin by learning the same exemplars: problems such as the inclined plane, the conical pendulum, and Keplerian orbits ... As their training develops, however, the symbolic generalizations they share are increasingly illustrated by different exemplars....(Kuhn,1970:187).

By articulating and demonstrating the conceptual utility and power of a theoretical framework through exemplars, concepts become tightly intertwined with the natural world and acquire a concrete reality, giving rise to, or perpetuating, a body of practice that guides the future research activities of a given group, or network. Through this process of elucidating the efficacy of a theory through exemplars, scientists acquire a certain degree of trust and faith in the validity of a conceptual model or perspective and its effectiveness in solving a specific field of related problems. Over time, as the theory in question amasses a body of exemplary solutions to a range of important, disciplinary puzzles, it transforms from being simply a cognitive tool devised by an individual scientist, or scientists, to a construct whose features denote a solid answer as to the structure of the physical realm. In this fashion, concepts and models become critical, cultural products that constrain scientists' perception of what constitutes a plausible solution to a specific

problem, or the legitimacy of a given experimental or theoretical discovery (Harvey,1981:106; Pickering,1980:108-110; 1981:73).

In this respect, one can argue that in the milieu of intellectual controversies, the tradition that proclaims victory does so, not because it has conclusively demonstrated that its particular model or experiment constitutes the final truth of the phenomenon in contention, but because it has monopolized the notion of plausibility. The mechanism by which this is achieved often involves both sides focusing communal attention upon technical, conceptual, empirical and methodological issues inherent in the work of the other.

In effect, the struggle to show the plausibility or implausibility of a disputed result is a struggle that sees both sides in a dispute trying to convince the other, as well as their peers, that their particular position is the correct one. In this respect, the two sides in a controversy engage in a vigorous process of negotiation, and while that negotiation concentrates on cognitive matters, its scope and scale is inevitably limited by the socio-cultural context in which it exists.

Indeed, scientists perception of the viability of a given finding is strongly influenced by the disciplinary culture[2] in which they were trained; this culture not only shapes a scientists' perceptions and expectations concerning the natural world, but it also places well defined conceptual boundaries on not only the kind of explanations that can be brought to bear in explaining a problematic finding, but also the type of results that are judged to be believable. As such, in the context of a intellectual controversy, which finding is seen as plausible is a function of whether that finding has qualities that resonate with – as Pickering would argue – the socially accepted conceptualizations of the physical world that guide a specific discipline (Collins,1975:214-218;1985:136; Harvey,1980:161; Harvey,1981:124-125; Pickering,1981:88-89; Pinch,1994:95-99).

Of course, one can expand the above comments to argue that every belief we cherish and support is a direct consequence of social negotiations that transpired during a particular period in

[2] This concept, as well as other key concepts used in the project, will be fully defined in section 2.3.

history and that have, consequently, come to circulate within the cultural boundaries of a specific community – with the approval of specific social institutions which constitute symbols of legitimacy and authority – for years if not generations, thus, allowing these beliefs to take on an air of invincibility, certainty and, inevitably, truth. In this respect, beliefs flow within the cultural topography of societies in the same fashion as currency: our beliefs circulate as long as nobody challenges them; in much the same fashion, bank notes circulate as long as nobody refuses them (Harvey,1981:125; Shapin,1994:5-7). As Harvey concludes, "... the plausibility of a belief, here and now, is a manifestation of the distribution of social control in our knowledge-related institutions at some time in the past"(Harvey,1981:126).

While the plausibility model appears, in some instances, to echo arguments uttered by Foucault many years ago, namely that truth is a function of power, existing and thriving under the domain of specific "regimes", it differs from the Foucauldian position in that its emphasis is on describing the plausibility of beliefs as opposed to their 'truth-content', and the mechanisms by which the notion of plausibility becomes interlinked with social processes in the context of scientific controversies. Indeed, one of the critical predictions of this framework is that, in the face of an intellectual dispute, the believability of a particular knowledge claim does not reside solely in the purview of strict logic and analysis, but is, to a large extent, shaped and influenced by *cultural* variables (Collins,1975:218;1981:34; Foucault,1980:131-133; Harvey,1981:126; Pickering,1981:64-65; Pinch,1994:95-96). As Harvey emphasizes, "Even when it is pointed out that the viewpoint of the 'loser' remains logically tenable, it is difficult for the reader to remain impartial in the face of the sheer weight of numbers in the 'winning' camp. The plausibility of the technical arguments cannot be treated as distinct from the social context in which these arguments operate"(Harvey,1981:126).

Yet, to state that one side eventually emerges 'victorious' from the shadow of a scientific controversy does not mean that the 'winners'' viewpoint will continue to dominate the intellectual landscape for eternity. If anything, the history of science has taught us that scientific

beliefs change over time, and ideas that seemed highly plausible in one generation, cease to be plausible in another. This is in itself hardly surprising given that science is a socially dynamic enterprise, where the legitimacy of a given knowledge claim rests upon criteria that are both technical as well as cultural in nature.

In truth, it is important to note that assessing the plausibility of a controversial discovery can be an inherently difficult phenomenon in practice. As past works in the sociology, history and philosophy of science has demonstrated (e.g. Galison,1987; Harvey,1980,1981; Pickering,1980,1981a,1981b; Pinch,1994; Rudwick,1985), the plausibility of a finding is a function of social, cultural, cognitive and experimental factors all of which interact in a complex fashion to yield a specific resolution to a particular scientific dispute. Not surprisingly, the sociologist tends to place greater emphasis on the socio-cultural context of intellectual debates than the other aforementioned variables, and while this may, at times, upset philosophers and historians of science, the accumulated weight of countless sociological investigations of scientific practice and disputes reveals that the social does play a not insignificant role in the examination of the physical world.

Indeed, since scientists do not have a direct and unfiltered access to nature, their conceptualizations of reality are a result of socialization into theoretical and experimental practices that are situated in a socio-cultural context; when faced with a surprising, controversial result, this context can influence scientists' perception of the plausibility of the discovery in question. While the cultural plays an important role in the allocation of plausibility, as has been argued above, the social also plays a vital function in this process. For instance, in most cases, the two sides in a dispute do not face one another on an equal footing; one side may be composed of prominent scientists, have access to superior equipment or financial resources, or represents an orthodox tradition whose ideas and beliefs have circulated for so long within a given discipline, or has become entrenched across a network of specialties – or a network of knowledge as Mary Hesse would argue – that it is no longer explicitly associated with its creator(s), but with the

natural world. Suffice it to say, under such conditions, demonstrating the plausibility of a novel results can prove an exceedingly difficult enterprise (Harvey,1980:161;1981:125; Hesse,1974; Mendelsohn,1977; Pickering,1980:130).

In essence, if one is to comprehend why a given body of knowledge is perceived as more plausible than a potential alternative, one must investigate the socio-cultural, as well as technical factors that went into that judgment, since the former cannot be entirely divorced from the latter. As Harvey writes, "... our knowledge about the world cannot be isolated from the social context in which that knowledge operates"(Harvey,1980:161).

2.3 Analytical Concepts

In the context of this project, three, critical concepts will be used quite frequently; as such, it is important that these concepts be defined as explicitly as possible; in no particular order these concepts are: community, culture, and metaphysics.

The term *community* is, perhaps, one of the most difficult sociological concepts to define precisely given its inherently abstract nature. Nevertheless, for the purposes of this study, **community** refers to a group of interacting scientists who shared training in the techniques and principles of astrophysics, possessed a doctoral degree in the discipline, and constituted members of the Royal Astronomical Society (RAS) from at least 1930 to 1940.

The above definition, of course, refers to the immediate community that Chandrasekhar was a part of during the early tenure of his controversy with Arthur Eddington and Edward Milne. But since Chandrasekhar's controversial research circulated beyond the confines of the RAS, one is forced to acknowledge the global community of astrophysicists who were confronted with the implications of Chandrasekhar's unorthodox findings at the height of the controversy, but also in the years after this particular dispute had found closure. In this respect, an alternative, more expansive definition of community is required. To this effect, the term **global community** will be defined as a group of interacting scientists, trained in the techniques of astrophysics, in possession

of a doctoral degree in the discipline, and published in, and read journals specializing in astrophysical issues (Allen, Qin & Lancaster,1994:281-282; Neuman,1997:7).

The term **culture** is defined as "... the field of resources that scientists draw upon in their work...."(Pickering,1992:3). In this respect, the notion of resources refers to the well-established body of knowledge, practices, and personal experience that scientists draw upon in their investigation of the physical world (Harvey,1981:96; Pickering,1992:3).

Lastly, the term **metaphysics**, in its traditional sense, refers to a preoccupation with comprehending the nature and structure of reality through the sheer power of rational thought. Yet, for the purposes of this project, the term will be modified to mean a strong, personal conviction as to the structure and nature of physical phenomena (Marshall,1998:411; Williams,1977:568).

In short, these three concepts form a critical component of the analysis of the Chandrasekhar-Eddington controversy that is to follow. The methodology used to comprehend the social and technical dynamics of this case study is described in the following section.

3.0 Methodology

It is an interesting fact that a number of important works in SSK investigating the social structure of scientific controversies, or scientific practice in general, have been anthropological in nature; that is to say, examinations of intellectual disputations in science conducted by figures such as Collins (1975,1981,1985,1999), Kennefick (2000), Latour & Woolgar,1986; Latour,1999; Pickering (1984), and Pinch (1981) have involved participant observational research, or in depth interviews of the principle actors implicated in a specific controversy that has yet to be resolved. This strategy has a number of benefits, not the least of which is enabling the researcher the luxury of witnessing the genesis, evolution, and perhaps even resolution of an academic debate over a contentious empirical or theoretical discovery.

An added strength is the priceless opportunity of discussing the controversial claim with scientists located on both sides of the debate, to comprehend in as much detail as possible the logic and arguments put forth by proponents and critics of the disputed result, and thus, achieve an explanation as to why an unorthodox discovery is believed by some and rejected by others. This methodological stance is known within SSK as 'symmetry' (Bloor, 1976; Pinch, 1994). As Pinch explains, "The application of symmetry has dramatic effects. It means, for instance ... that we cannot separate the wheat of genuine science from the chaff of error. ***Both sides in the dispute have to be treated equivalently. That is to say we cannot prejudge which side is right***"[3] (Pinch,1994:89;emphasis added).

Unfortunately, the above-cited strategy is not possible with respect to the Chandrasekhar-Eddington controversy as the dispute itself lasted for approximately four years (1935-1939)

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[3] Of course, when investigating a scientific controversy that has long since been resolved, the researcher knows from the pages of history which side was right and which side was in error. However, even when studying a scientific dispute that has been touched by resolution, the sociologist should try to remain as objective as possible and concentrate his/her efforts on offering a sociological explanation of the controversy that permits the reader to see the debate from the perspective of both protagonists. In this fashion, the social scientist of knowledge may convey to the reader the important notion that, "Beliefs that are taken to be true are shaped or constructed by the same social processes that shape false beliefs" (Pinch,1994:89).

before fading into the intellectual backwaters unresolved, only to be conclusively settled about seventeen years ago. Furthermore, the principal actors in the debate have all passed on, and thus no living individual who had been involved in this scientific controversy remains to discuss his/her perspectives on the matter (Horgan,1994:500-501;Penrose,1997:62-63;Wali,13-14,125-126,136-138).

In light of this reality, my introduction to this particular case study, and further detailed examination of the events involved in the Chandrasekhar-Eddington debate have relied upon a number of sources which range from the biographical and historical to the purely technical. This expansive approach, it is hoped, will enable me to draw a fair representation of the cognitive as well as cultural context in which this specific controversy transpired. In light of these comments, I now turn to the body of literature that first exposed me to the details of the Chandrasekhar-Eddington dispute, which are subsequently listed below:

Wali, Kameshwar C.
1982 **"Chandrasekhar vs. Eddington – An Unexpected Confrontation."** *Physics Today*, 33-40.

1991 ***Chandra: A Biography of S. Chandrasekhar***. Chicago: University of Chicago Press.

It should be noted that Dr. Wali was not only a good friend of Dr. Chandrasekhar's, but also his official biographer. Indeed, due to their many years of friendship, Wali was allowed fairly liberal access to Chandrasekhar's personal letters; these invaluable documents, written over a period of decades, and which include correspondence with family, friends and colleagues reveal Chandrasekhar's thoughts on life, work and his heated intellectual controversy with Dr. Arthur Stanley Eddington, thus forming a critical source for Wali's definitive biography and preliminary article on the topic (Evans,1998:141-143; Wali,1991:10-12,329-331).

Yet, despite the comprehensive nature of Wali's work, one must be very careful not to rely too heavily on any one source for information concerning a scientific dispute; this is especially important in the case of Wali's research. After all, the above-cited works were written by an

admirer and close friend of Chandrasekhar, and thus, one would expect the author to weave an account of the controversy that would present his subject in a most favourable fashion. As such, one is forced to look for additional, more independent references with respect to this scientific dispute. However, this is easier said then done. The fact remains that Chandrasekhar never achieved the celebrity status of Einstein; while he became quite famous within the astrophysical community for his pioneering research, and subsequent acquisition of the Nobel Prize, his work did not revolutionize physics in the same manner as Einstein's and thus his life never did come under the intense public scrutiny of his late academic colleague; as such, there are very few written documents to be found presenting even a general life-historical portrait of Chandrasekhar and the intellectual controversy he was embroiled in so many decades ago. Indeed, until the publication of Wali's preliminary article on the Chandrasekhar-Eddington controversy and the subsequent biography, few, even in the scientific community were aware of the details of Chandrasekhar's life and his dispute with a famous astrophysicist (Wali,1991:10-11,13-14).

While Wali's work remains to this day as the most comprehensive exposition of Chandrasekhar's life and his intellectual battle with Arthur Eddington, there are a number of alternative references I was able to consult in order to get a less biased view of the controversy this project will attempt to explore, though a majority of these references are quite limited in detail and scope; nevertheless, these sources are as follows:

Douglas, A. Vibert
1956 ***The Life of Arthur Stanley Eddington***. New York: Thomas Nelson and Sons, Ltd.

Evans, David S.
1998 ***The Eddington Enigma: A Personal Memoir***. Princeton: Xlibris Corporation.

Gupta, Alok K.
1995 **"Subrahmanyan Chandrasekhar."** *National Academy of Science Letters*, 18:199-202.

Horgan, John
1994 **"Profile: Subrahmanyan Chandrasekhar."** *Current Science*, 67:500-501.

Lankford, John
1997 ***History of Astronomy: An Encyclopedia***. United States of America.

Nityananda, R.
1995 **"Subrahmanyan Chandrasekhar."** *Current Science*, 69:554-556.

Parker, Eugene N.
1995 **"Obituaries: Subrahmanyan Chandrasekhar."** *Physics Today*: 106-108.

Penrose, Roger
1997 **"Chandrasekhar and the End of Time."** Pp. 61-69 in K.C. Wali (eds.), ***S. Chandrasekhar: The Man Behind The Legend.*** London: Imperial College Press.

The above documents, in aggregate, provided a fairly expansive overview of the Chandrasekhar-Eddington controversy. As indicated earlier, a large number of these references outside of Wali's work are very brief in nature, all of them – with the exception of Douglas, Evans and Horgan – having been written by admirers, colleagues, or friends in the wake of, or a year before, Chandrasekhar's death on August 21st,1995 (Nityananda,1995:554;Parker,1995:106).

In light of these words, the reader will no doubt wonder how much more "independent" these various accounts are relative to Wali's biography? The answer is, perhaps not a whole lot. However, five of the cited references were composed in a more impartial manner (e.g. Douglas,1956; Horgan,1994; Parker,1995; Evans,1998; Lankford,1997) – especially the monographs written by Douglas and Evans which constitute, primarily, two distinct biographies of Arthur Eddington, though Eddington's dispute with Chandrasekhar is, in fact, discussed in both texts, albeit very briefly in Douglas' book, and in greater depth in Evan's work. While it would have been instructive to have drawn upon a large body of neutral references which examined this specific controversy in depth, such a luxury was not possible; as suggested earlier, since Chandrasekhar's research did not attract much public attention, his academic work remained ambiguous except to astrophysicists and theoreticians, his life-history even more of a mystery. Consequently, there exists only a limited, and somewhat biased pool of data a researcher can consult in order to gain an overview of the Chandrasekhar-Eddington controversy. Nevertheless, when taken as a whole, the aforementioned documents do provide the investigator an empirical foundation upon which a reasonable assessment of this dispute can be made.

Of course, in any scientific debate, it is important for the sociologist to be sensitive to the views expressed by the respective parties involved, to comprehend the logic and beliefs of those who staunchly support an unorthodox discovery, as well as those who strongly reject its legitimacy without showing bias toward either side. In contemporary SSK research, this strategy is accomplished by in-depth interviews of proponents and detractors of a problematic finding (e.g. Collins,1975,1981,1985,1999; Kennefick,2000; Pinch,1981).

Yet, with respect to the case study that forms the basis of this project, such an option is not available. However, public documents do exist that a researcher can consult in order to gain an understanding of the intellectual context in which the Chandrasekhar-Eddington controversy was born and evolved, as well as the rationale and argumentative stance of both Chandrasekhar and Eddington with respect to their scientific disagreement over the evolution and fate of stars. Not surprisingly, some of these sources are the very ones listed earlier in the chapter; others, constitute the original papers written by Chandrasekhar that formed the basis of the debate – including articles published in scientific journals in later years in which he reflects on the controversy with his senior colleague – as well as critiques of these papers by Eddington either in printed form, or on the public record.

However, before we proceed with a discussion of these specific references, it may be instructive to point out that one cannot commence investigating a controversy without first coming to an understanding of the established academic views that preceded the emergence of the specific unorthodox finding whose nature is the focus of an intellectual dispute. Indeed, as Kuhn has emphasized, a particular knowledge claim can only be seen as controversial if it contradicts a fairly well accepted body of research; in this sense, what is considered controversial is a relative concept and does not exist in a cognitive or cultural vacuum (Kuhn,1970:10).

With respect to the Chandrasekhar-Eddington controversy, in order to gain an adequate understanding concerning the established views of stellar evolution that formed the conceptual background upon which the controversy unfolded, I examined the various aforementioned

sources listed previously in this discussion. In addition, I consulted the original research paper by Dr. Ralph Howard Fowler (1926) that essentially established the orthodox view at the time concerning the evolution of stellar matter; in specific terms, this view articulated the notion that all stars, regardless of their mass would end their life cycle as white dwarfs.

In addition, I studied the main textbook of the period that all students specializing in astrophysics had to read, a monograph that in great detail succinctly summarized astrophysical knowledge as it stood in the 1920s, and one that had been composed by Arthur Stanley Eddington himself (Russell,1928:88; 1945:134). As Kuhn remarked so many years ago, textbooks hold a special place in the socialization of future scientists, as they present the current state of knowledge of a given scientific field in bold, authoritative statements that students learn to absorb and regurgitate without question (Kuhn,1970:136-139). In Kuhn's words, "As the source of authority, I have in mind principally textbooks of science... They address themselves to an already articulated body of problems, data, and theory, most often to ***the particular set of paradigms to which the scientific community is committed at the time they are written***"(Kuhn,1970:136; emphasis added).

In more concrete detail, these two critical sources of information are as follows:

Eddington, Arthur Stanley
1926 ***The Internal Constitution of the Stars***. Cambridge: Cambridge University Press.

Fowler, Ralph Howard
1926 **"On Dense Matter."** *Monthly Notices of the Royal Astronomical Society, 87:112-122.*

After reviewing these works, and having gained a fairly solid comprehension of the state of knowledge as it existed at the time with respect to stellar evolution prior to the emergence of Chandrasekhar's problematic discovery, I turned next to an examination of four technical papers, written by three different scientists, that had also developed the notion that only stars of a certain mass could develop into white dwarf stars; three of these paper preceded Chandrasekhar's own surprising theoretical discovery of the limiting mass, with the remaining article being published a

year after the publication of Chandrasekhar's own initial research on the matter. In specific terms, these papers are as follows,

Anderson, Wilhelm
1929 **"Relativity Effects in White Dwarfs."** *Zeitschrift fur Astrophysik*, 56:851-856.

Landau, Lev.
1932 **"On the Theory of Stars."** *Physikalische Zeitschrift der Sowjetunion*, 1:285-288.

Stoner, Edmund C.
1929 **"Limiting Density of White Dwarf Stars."** *Philosophical Magazine*, 7: 63-70.

1930 **"The Equilibrium of Dense Stars."** *Philosophical Magazine*, 9: 944-963.

After a thorough examination of the above listed sources, I next turned my attention to the controversial articles written by Chandrasekhar[4] that formed the focus of the scientific dispute with Eddington, these being as follows:

Chandrasekhar, Subrahmanyan
1931 **"The Maximum Mass of Ideal White Dwarfs."** *Astrophysical Journal*, 74: 81-82.

1932 **"Some Remarks on the State of Matter in the Interior of Stars."** *Zeitschrift fur Astrophysik*, No. 5: 321-327.

1934 **"Stellar Configurations with Degenerate Cores."** *The Observatory*, 57, No. 727: 373-377.

1935a **"The Highly Collapsed Configurations of a Stellar Mass (Second Paper)."** *Monthly Notices of the Royal Astronomical Society*, 95, No.3: 207-225

1935b **"Stellar Configurations with Degenerate Cores."** *Monthly Notices of the Royal Astronomical Society*, 95, No.3: 226-260.

1939 **"The White Dwarfs and Their Importance for Theories of Stellar Evolution."** *Conferences du College de France, Colloque International d'Astrophysique*, 17-23 Juillet: 239-248 (Paris: Hermann, 1941).

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[4] It should be noted that nearly every scientific paper published by Chandrasekhar can be found in a special six volume series published by The University of Chicago Press, entitled ***Selected Papers of S. Chandrasekhar***. The specific volume that constituted the main source for my investigation of Chandrasekhar's research into stellar structure and white dwarf stars is volume 1 of the series entitled, ***Stellar Structure and Stellar Atmospheres***, 1989.

Eddington's opinions of his junior colleague's unorthodox theoretical finding is listed in five key documents: one is the transcript of the meeting of the Royal Astronomical Society in which he critiqued Chandrasekhar's work; the other, is a detailed article which formed the basis of his public comments against Chandrasekhar's conclusions with respect to stellar evolution. The third reference is a note responding to Chandrasekhar's defence of his model; the fourth reference is a reprint of Eddington's address given at the Harvard Tercentenary Conference of Arts and Sciences entitled on the "Constitution of Stars". Lastly, the fifth document constitutes Eddington's final public comments on Chandrasekhar's white dwarf theory. The full references to these papers are as follows:

Eddington, Arthur Stanley
1935a **"On Relativistic Degeneracy."** *Monthly Notices of the Royal Astronomical Society*, 87: 194-206.

1935b **"Note On "Relativistic Degeneracy."** *Monthly Notices of the Royal Astronomical Society*, 96, No.1: 20-21.

1937 Harvard Tercentenary Conference of Arts and Sciences **"Constitution of the Stars."** *Smithsonian Report*, 131-144.

Royal Astronomical Society
1935 **"Proceedings of the Meeting of the Royal Astronomical Society."** *The Observatory*, 58: 35-41.

1939 **"Report on the Theory of White Dwarfs."** *Conferences du College de France, Colloque International d'Astrophysique*, 17-23 Juillet: 269-289 (Paris: Hermann, 1941).

While the above sources give a detailed account of Eddington's views of Chandrasekhar's research, and the reasons why he objected so strongly to the younger scientist's work, a more in-depth understanding of Eddington's argumentative stance must take into consideration the way in which he perceived the physical world, and his beliefs – consolidated over many decades of intellectual pondering – concerning the structure of reality; this is important to consider as Eddington's personal philosophy played a not insignificant role in his dispute with Chandrasekhar

(Douglas,1956; Evans,1998; Wali,1991). Thus, in order to empathize with Eddington's worldview, the following monographs written by the late astrophysicist were reviewed:

Eddington, Arthur Stanley
1930 ***The Nature of the Physical World.*** Cambridge: Cambridge University Press.

1935 ***The Nature of Physical Science***. Cambridge: Cambridge University Press.

1936 ***Relativity Theory of Protons and Electrons.*** Cambridge: Cambridge University Press.

1967 ***The Philosophy of Physical Science***. USA: Michigan Press.

1953 ***Fundamental Theory.*** Cambridge: Cambridge University Press.

In essence, the above listed documents enabled me to come to a satisfactory comprehension of Eddington's intellectual as well as metaphysical stance with respect to Chandrasekhar's discovery. While it would have been invaluable to supplement the cited, written records with face-to face interviews with Eddington himself in order to flesh out the particulars of his objection to his colleague's findings, this option was not available, and thus, the above methodological strategy represented the most efficient means of viewing this particular controversy from Eddington's perspective.

In a similar fashion, in order to understand the intellectual stance of Chandrasekhar's other main critique in this controversy, namely, Edward Arthur Milne, I attempted to study his original publications in the area of stellar structure in order to ascertain his conceptual stance with respect to this particular research area. Yet, this was not an easy task; unlike in the case of Eddington, whose complete set of publications is listed by his biographer, Alice Vibert Douglas (1956), no similar comprehensive reference exists for Milne. Thus, my search for specific articles written by Milne on stellar structure I consulted bibliographical sources such as *Physics Abstracts and Science Citation Index*.

However, the use of these sources posed problems of their own. Firstly, it was difficult to find a complete collection of *Physics Abstracts* that stretched back to the 1920s, the time period in

which Milne began publishing his thoughts upon stellar structure (Evans,1998: 133-134; McCrea,1951:166-167); the various institutions I visited only possessed volumes going back to the 1940s or 1960s.

Secondly, with respect to the *Science Citation Index*, while this database was only started in 1955, it tracks all citations made by scientists in a given year (Garfield,1983:24-30). Since some researcher somewhere will often cite classic papers dating back many decades, this reference tool proved a bit superior to *Physics Abstracts*. Nevertheless, its one drawback is a lack of a full citation of a paper's title or topic area, thus, making it difficult for a researcher to know what specific research paper is being cited.

As a result of these limitations, I supplemented what information I could find with these sources with a number of secondary materials such as William McCrea's biography on Edward Milne for more detailed and specific references, as well as Evans (1998) and Chandrasekhar's aforementioned papers which posted full references to Milne's work on stellar structure and stellar mechanics. The fruits of this somewhat difficult search resulted in four critical pieces written by Milne which clearly and concisely stated his earliest and significant publications on the physical structure of stars[5]. These papers are as follows:

Milne, Edward Arthur

1929 **"The Masses, Luminosities and Effective Temperatures of the Stars."** *Monthly Notices of the Royal Astronomical Society*, 90: 17-54.

1930a **"The Masses, Luminosities and Effective Temperatures of the Stars (Second Paper)."** *Monthly Notices of the Royal Astronomical Society*, 90: 678-689.

1930b **"The Analysis of Stellar Structure."** *Monthly Notices of the Royal Astronomical Society*, 91: 4-55.

1931 **"Stellar Structure and the Origin of Stellar Energy."** *Nature*, January: 120-150.

[5] It should be noted that much of Milne's scholarly research into stellar structure was published from 1929 to 1931, though he continued to conduct research on stellar interiors until 1934 though in a limited sense (Chandrasekhar,1987: 83-85; Evans,1998:133-134; McCrea,1951:167-168).

While the above references gave a clear indication of Milne's conceptual stance with respect to the topic of stellar structure, his public reaction to Chadrasekhar's controversial research can be found in the following sources:

Royal Astronomical Society
1935 **"Proceedings of the Meeting of the Royal Astronomical Society."** *The Observatory*, 58: 35-41.

1935 **"Correspondence: The Configuration of Stellar Masses."** *The Observatory* : 52.

Milne's private reactions to his junior colleague's work have been published in Chandrasekhar (1969;1987) and Wali (1982; 1991).

Now, Chandrasekhar's personal reaction to Eddington and Milne's rebuttal of his surprising discovery is not present in the literature period. However, many decades after the dispute had transpired, Chandrasekhar did discuss the dispute to some extent in a number of articles as well as a monograph; these public documents, as well as the extensive comments he provided his biographer provided an ample pool of data with which to understand his perspective on the whole controversy. In specific terms, these sources are as follows:

1969 **"The Richtmyer Memorial Lecture."** *American Journal of Physics*, 577-584.

1972 **"The Increasing Role of General Relativity in Astronomy."** *The Observatory*, 92: No. 990: 160-174.

1987 ***Truth and Beauty: Aesthetics and Motivations in Science***. Chicago: Chicago University Press.

Lastly, it was important to assess what other members of the astrophysical community of the time felt about Chandrasekhar's results, in addition to Eddington's comments on the matter. Interestingly enough, papers published in the field of astrophysics in the wake of Eddington's critique of Chandrasekhar's surprising discovery make very little mention of the controversy; indeed, opinions concerning the young scientist's unorthodox conclusions, as well as the remarks of his elder, distinguished colleague are virtually absent. This comment is partly based on Wali's

exhaustive archival research, as well as an intensive review of the astrophysical literature immediately following the publication of Chandrasekhar's first and subsequent articles on his controversial discovery. However, a few scientists of the era did publish works that alluded to this particular intellectual dispute, or at least, discussed Chandrasekhar's theory of white dwarfs; the references for these documents are as follows:

Fowler, Ralph H.
1936 ***Statistical Mechanics: The Theory of the Properties of Matter in Equilibrium.*** Cambridge: Cambridge University Press.

Marshak, R.E.
1940 **"White Dwarf Stars."** *Astrophysical Journal*, 92: 321-353.

Moller, Christian and S. Chandrasekhar
1935 **"Relativistic Degeneracy."** *Monthly Notices of the Royal Astronomical Society*, 95:673-676.

Peierls, Rudolf F.
1936 **"Note on the Derivation of the Equation of State for a Degenerate Electron Gas."** *Monthly Notices of the Royal Astronomical Society*, 96: 780-784.

Rosenfeld, Leon and S. Chandrasekhar
1935 **"Production of Electron Pairs and the Theory of Stellar Structure."** *Nature*, vol. 135: 998-999.

Russell, Henry N.
1935 **"Impossible Planets."** *Scientific America*, July: 18-19.

In essence, all of the above cited documents provided vital, empirical data that shed light upon the technical, intellectual and social character of the Chandrasekhar-Eddington controversy, references which constituted the critical biographical, historical and academic papers and monographs that presented the essential details of this scientific dispute; it is hoped that these diverse works will permit the author to write a truly "symmetrical" analysis of an intellectual debate that has thus far, not received much attention from members of the scientific community or sociologists of scientific knowledge.

3.1 Interviews

As indicated in the earlier portions of this chapter, in any scientific controversy it is critical that a researcher gain a balanced perspective of the issues involved, to understand the logic and rationale of participants located on both sides of the debate. Often in SSK research, this is accomplished through in-depth interviews of the various actors engaged in the controversy in question. However, with respect to the Chandrasekhar-Eddington case study, this luxury is not possible for practical reasons already mentioned in the above exposition. Instead, I had to rely predominantly on published works in order to gain a sense of the complex intellectual, socio-political fabric of this controversy.

Yet, in order to flesh out my own comprehension of the technical and cultural elements intertwined within Chandrasekhar-Eddington debate, I found it necessary to search for scientists who have a detailed knowledge of the case, scientists who would be willing to lend their respective interpretations concerning the reasons as to the genesis of this fascinating academic disputation. Given the reality that very few astrophysicists really know the details of the Chandrasekhar-Eddington controversy, it was very difficult to locate knowledgeable informants. To compound the problem, when interested parties were, in fact, found, their willingness to participate in formal interviews quickly evaporated in light of a specific, administrative task.

As any social scientist knows, when working with human subjects in the course of academic research, most university departments require the researcher to submit a detailed report on the investigation to a Departmental Ethics Committee which subsequently reviews the nature of the work and determines whether or not it protects the rights of the participants. Part of this report contains an Ethics Protocol form[6], which the respondents have signed acknowledging that they have been fully informed of their rights, as well as the specific nature of the study they have agreed to formally join. The whole logic of this clerical exercise is to ensure the well-being of the subjects, to protect them from any potentially harmful actions undertaken consciously or

[6] This form can be seen in Appendix A.

unconsciously by the interviewer (McCracken,1988:27,69). Yet, despite the honest intentions of the Ethics Form, nearly all of the scientists contacted viewed the form with suspicion.

In truth, a total of seven physicists were approached who were deemed to have a strong knowledge of the Chandrasekhar-Eddington controversy. This list of qualified candidates was, in a small sense, constructed through the use of snowball sampling techniques starting with a scientist I had interviewed extensively in connection with my Master's thesis; the remaining scientists were selected after reading a number of articles they had written on Chandrasekhar, as well as inquiries at a number of universities in Canada and the United States.

Suffice it to say, of the seven physicists that were contacted, one did not reply; two indicated a strong interest in discussing the controversy, but refused any further response after I mentioned the Ethics form; one astrophysicist bluntly told me that he would only talk about the case in a very informal sense, and did not want to sign any form as he did not wish to become entangled in university bureaucracy; one did reply to a series of general, brief questions I had concerning the case via e-mail, but upon contacting him again in order to set a formal interview, no response was forth coming even after the passage of two months; the remaining two scientists did agree to a formal interview, and while they, too, had questions concerning the Ethics form, after I had answered their inquiries and assured them that signing the form would not require any commitment or undue impact on their time beyond the interviews, they readily agreed[7]. At this point, some brief comments of these willing participants will be discussed below.

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[7] It is unfortunate that the Ethics Protocol Form induced an unwarranted amount of suspicion with many of the physicists contacted in connection with this study; after all, the very nature of the form is to protect their rights and ensure them of the sincerity of my intentions with respect to the interviews! My experiences in this matter have forced me to re-evaluate the very relevance of this bureaucratic exercise. It is true that such documents are important in situations where the researcher has considerable power over his/her subjects, and where the particulars of the research may unduly impact the participant in a negative fashion. But in my case, the objective was to interview established – and in some cases, very prominent scientists – on a controversy that transpired over seven decades ago; one can argue that in such a scenario, the balance of power is weighted in favour of the physicists. Furthermore, given their troubled reaction to the ethics form, I believe that in certain circumstances, it is more productive for the researcher to verbally explain to their subjects their rights in as much detail as possible, and make certain the interviewee understands his/her rights before proceeding. This process seems to be much less intimidating and much more constructive than taking out a formal document and demanding a signature.

The first interview subject is Dr. Kameshwar C. Wali, Professor Emeritus and Professor of Theoretical Physics at Syracuse University, and Chandrasekhar's biographer. I had numerous e-mail correspondences with Dr. Wali commencing in 1998 and a lengthy telephone conversation in the summer of that year. In February 1999 a formal interview was conducted over the telephone. The main purpose of these talks was to enhance my own understanding of the controversy, to ensure that I understood the rationale of both sides in this debate as opposed to acquiring a large pool of quotable material. Indeed, since much of Dr. Wali's responses to my questions are already addressed in his biography, the quotes used tend to reflect comments on issues that were not explicitly discussed in the monograph.

The second scientist willing to partake in the interview process was Dr. M.K. Sundaresan, Adjunct Professor of Theoretical Physics at Carleton University who knew Chandrasekhar personally and possesses a detailed knowledge of the case. Over the course of two-and-a-half years I had a number of informal conversations with him concerning the Chandrasekhar-Eddington controversy; these conversations first commenced in November of 1996, and took place intermittently thereafter when our schedules permitted a window of opportunity to meet. A formal interview was conducted on September 25th 1997 at Carleton University that lasted approximately two-and-a-half hours.

In the cases of both participants, a transcript was prepared that included the quotes chosen to appear in the thesis and submitted to the respective parties for review and correction of any errors that might exist in the body of the document.

In sum, this project will examine the Chandrasekhar-Eddington debate from a multi-dimensional methodological and theoretical perspective, drawing on a variety of primary and secondary sources. It is hoped that this holistic approach will shed some light on the fascinating intellectual and socio-political forces that materialized during the tenure of what appears to have been one of the most interesting, yet under-cited, scientific disputes in the history of science.

4.0 The White Dwarf Controversy

4.1 The Historical Background

In the early decades of the twentieth century, theoretical astrophysics was predominately concerned with comprehending the intricacies of stellar matter. Indeed, at this point in time, scientists were just beginning to gain a tentative understanding of how stars formed, how they evolved, the complexities inherent in their physical composition, as well as how they generated their tremendous heat and energy. While much was unknown about these burning giants of the cosmos, astrophysicists had – thanks to the pioneering efforts of scientists like Arthur Eddington, Edward A. Milne, Ralph Fowler and Henry Norris Russell – developed a number of fairly sophisticated mathematical models of stars that appeared to satisfactorily explain a wide spectrum of observational data collected through the years on celestial entities such as the sun. However, despite this measure of success, one particular heavenly body continued to perplex physicists, this being the mysterious stellar creature known in the astrophysical literature as the white dwarf star. (DeVorkin,1989;Douglas,1956; Evans,1998; Lankford,1997).

The discovery of this very unusual object was, as is true of many scientific discoveries, a chance occurrence. As long ago as 1844, the German astronomer, Friedrich Bessel, was observing the star Sirius[8], the brightest object in the evening sky, from the Konigsberg Observatory located in Prussia or what is now Kaliningrad in the former Soviet Union. After some time he noticed that Sirius' motion across the sky was not linear but appeared to wobble back and forth slightly, as if its voyage through space was being influenced by an invisible, unseen companion. While empirical evidence and the laws of physics told Bessel that Sirius had to have a stellar neighbour, he could not see it try as he might. That Sirius had a companion was beyond dispute for Bessel, but it was an invisible entity hidden by the tremendous glare of Sirius.

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[8] Sirius is located 8.7 light-years, or approximately 51 trillion miles (82 trillion kilometres) from the Earth (Kaufmann,1988:608-609; Sullivan,1979:30).

Then, in 1863, an American researcher named Alan Clark, born to a famous telescope crafting family, finally located the cabalistic companion of Sirius while testing a new eighteen-inch telescope that was scheduled to be installed at the observatory located at Northwestern University. The fact that Clark was able to see Sirius' spectral partner was a testament to his observational skills and the resolving power of the new telescope since the companion star was incredibly faint; indeed, if Sirius' companion was brought to our solar system and positioned in the exact same location as the sun, it would be roughly one-four-hundredth as bright. Yet, despite its nearly invisible nature, Clark was able to determine that Sirius Comes, as it came to be called, glowed with a distinctly white light, though a much weaker light than emitted by Sirius' brilliant surface[9] (Kaufmann,1988:433; Sullivan,1979:30).

Further observational and theoretical examination revealed Sirius Comes possessed a surface temperature that rivalled, if not surpassed that of its luminous partner as well as the sun. This reality posed a curious dilemma for astrophysicists: if Sirius Comes was supposedly hotter than its companion why was it so difficult to see, while Sirius can easily be viewed with the unaided eye? The only logical explanation that scientists could think of was that Sirius Comes was a relatively small star. As Henry Norris Russell wrote of this predicament, "... a body hotter than the sun should emit more light per square mile, hence these stars must be small – no bigger, in fact, than several of the planets"(Russell,1935:18).

Another puzzling feature of Sirius Comes was for it to exert such a tremendous gravitational pull on its companion, one so strong that it affected the motion of Sirius' path through the cosmos, it must possess a mass equal to or just slightly less than that of Sirius itself. Since these two stars form a binary star system, calculation of their respective masses was fairly straightforward with the aid of elementary physics, algebra and many months of patient observation. In time, it was estimated that Sirius had a mass that was approximately 2.28 times

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[9] From a technical perspective, relatively "cool" stars appear red in colour, whereas very hot stars burn with a dazzling white hue (Kaufmann,1988:353,355).

that of the sun[10], and Sirius Comes, a mass that was 0.85 times that of the sun. Further analysis indicated that Sirius Comes was endowed with a radius of only 18,800 kilometres (11,675 miles)[11], or just under three percent of the radius of the sun. Suffice it to say, the implications of these findings were remarkable for it implied that Sirius Comes was an object whose mass nearly equalled the sun, but was only 1/51,000th the volume. The accumulation of all these data forced scientists to conclude that this particular star had an astonishing density of 61,000 g/cm^3, or 61,000 times the density of water.[12]

In 1925, a prominent astrophysicist, Walter Sydney Adams, used Albert Einstein's revolutionary theory of gravity as a means of checking the validity of Sirius Comes' phenomenal density measurement; theoretical predictions concerning the spectral displacement of the star given its enormous density, were compared with empirical observations of the star's spectra and the two were found to be in excellent agreement, forcefully confirming the tremendous density of this strange stellar entity (Eddington,1926:171-193; Evans,1998: 136-139; Giancoli,1985:185; Kaufmann,1988:97-98,359-360; Russell,1935:18; Serway,1994:334; Wali,1982:34). In response to this confirmation, the most prominent astrophysicist of the era, Arthur Eddington, made the following comment:

> "This observation is so important that I do not like to accept it too hastily until the spectroscopic experts have had full time to criticize or challenge it; but so far as I know it seems entirely dependable. If so, Professor Adams has killed two birds with one stone; he has carried out a new test of Einstein's general theory of relativity and has confirmed our suspicion that matter 2000 times denser than platinum is not only possible, but is actually present in the universe"(Eddington,1926:173).

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[10] The Sun has a mass of 1.99 x 10^{27} metric tons (Kaufmann,1988:609).

[11] Modern estimates place the mass of Sirius and its companion at 2.143 +/- 0.0056 and 1.053 +/- 0.028 solar masses; their respective diameters are (2.34 +/- 0.07) x 10^6 and (1.02 +/- 0.17) x 10^4 kilometers, respectively (Gatewood & Gatewood,1978:191,195; Kaufmann,1988:433; Lasota,1999:42).
As can be seen from these figures, astrophysicists of the time had over estimated the diameter of Sirius Comes by a fairly large degree. However, scientists of the period were aware that their calculations possessed a relatively large margin of error. As Russell wrote, "...The calculated sizes are more likely to be too large than too small ... the calculated diameter may be wrong by 25 percent...."(Russell,1935:18). In truth, the estimated diameter is nearly four times the currently accepted value (Lasota,1999:42).

[12] The latest evidence suggests that Sirius Comes' density is actually over three million grams per cubic centimetre (Gatewood & Gatewood,1978; Kaufmann,1988; Lasota,1999).

In time, Sirius Comes came to be known as a white dwarf star due to its brilliant white colour, relatively small size and ability to shine with an intensity that exceeded that of most stars. Soon, two more white dwarfs were discovered by astronomers, specifically, the companion of the star known as Omicron Eridani and van Maanen's star. Yet, despite these additional findings, the physical structure of the white dwarf continued to puzzle scientists. In order to understand the conceptual dilemma these objects posed for astrophysicists, a brief, technical discussion is required (Eddington,1926:172-173; Russell,1935:18; Wali,1982:34).

By the mid to late 1920s, scientists had an approximate idea about the physical dynamics involved in stellar mechanics. They knew that stars were great balls of superheated gas that were constantly being suppressed by gravity. They also knew this compression was counterbalanced by the extraordinary heat and temperature present in a star generated by some kind of unknown sub-atomic, nuclear process, thus, allowing a star to exist in a state of equilibrium (Eddington,1926:170-172,306-313; Hawking,1988:82-83; Russell,1935:18; Sullivan,1979:32-33; Wali,1982:33). This state of equilibrium is analogous to that found in "...a balloon – there is a balance between the pressure of the air inside, which is trying to make the balloon expand, and the tension in the rubber, which is trying to make the balloon smaller" (Hawking,1988:83).

Yet, scientists also realized that this state of stellar tranquility would not last forever; in time, a star will exhaust its supply of nuclear fuel and will no longer be able to withstand the crushing force of gravity; it will be forced to contract, much like a deflating balloon. As a star contracts into a smaller volume of space, the logical consequences is that the pressure of the gas comprising the star will increase, leading to an increase in both temperature and density[13]

[13] It should be noted that much of the early investigations into stellar structure was based on the assumption that stars behaved as if they were composed of a perfect gas. In short, a perfect gas represents a theoretical conceptualization of gas where the relationship between temperature, pressure and density are simple and straightforward. In this model, the pressure exerted by the gas is directly proportional to both the temperature and density of the gas. For all practical purposes, the gas that constitutes a star is assumed to behave like a perfect gas; if the star contracts, the gas is compressed, resulting in an increase in temperature and density; if the star expands, temperature and density decreases and the star cools down (Kaufmann,1988:417; Lankford,1997:75; Tver,1979:114).

(Eddington,1926:172; Evans,1998:137-139; Hawking,1988:83; Kaufmann,1988:417; Russell,1935:18).

Given this theoretical knowledge base, astrophysicists found the empirical characteristics of white dwarfs highly perplexing for the following reason: in a healthy star like the sun, the force of gravity is nicely counterbalanced by the incredible pressure and heat generated by thermonuclear processes. However, once a star's nuclear fuel is close to depletion, it can no longer adequately withstand its own gravitational pull and will commence to contract; as it contracts into a smaller volume of space, its density will increase. Yet, stars are continually radiating energy into space, and thus, as a consequence, one would expect their temperatures to decrease resulting in a subsequent reduction in pressure and density. But, observational data indicated that this was not occurring in white dwarfs; even if one were able to remove the tremendous pressures existing within these stars, their respective densities would remain unnaturally high. How was this possible?

The problem was further compounded by another puzzling feature of white dwarfs: these stellar objects had been born due to the fact that they had exhausted much of their nuclear fuel and were now at the mercy of gravity. However, gravity was only able to compress them to a certain finite state and no further. This implied that white dwarfs were able to successfully resist further compression, but since they had very little fuel reserves remaining, where did they find the necessary energy to counterbalance the monstrous force of gravity (Eddington,1926:172; Evans,1998:137-139; Hawking,1988:83; Kaufmann,1988:417; Russell,1935:18)? Arthur Eddington wrote about this dilemma, stating:

> "I do not see how a star which has once got into this compressed condition is ever going to get out of it. So far as we know, the close packing of matter is only possible so long as the temperature is great enough to ionize the material. When the star cools down and regains the normal density ordinarily associated with solids, it must expand and do work against gravity. *The star will need energy in order to cool* ... It would seem that the star will be in an awkward predicament when its supply of sub-atomic energy ultimately fails. Imagine a body continually losing heat but with insufficient energy to grow cold! It is a curious problem and one may

> make many fanciful suggestions as to what actually will happen (Eddington,1926:172).

The solution to this perplexing problem came in an article published in 1926, not too long after the release of Eddington's now classical textbook on stellar physics, by the eminent mathematician turned astrophysicist, Ralph H. Fowler. Fowler commences his paper by stating, "The accepted density of matter in stars such as the companion of Sirius is of the order 10^5 gm/c.c. This large density has already given rise to most interesting theoretical considerations, largely due to Eddington" (Fowler,1926:114).

Fowler then proceeds to briefly discuss Eddington's comments on the matter. Afterwards, he attempts to solve the puzzling problem of the white dwarf's physical structure by drawing upon the very recently developed statistical model known as the Fermi-Dirac statistics.[14] In his analysis, Fowler demonstrated that under the phenomenally extreme physical conditions found in white dwarfs, the electrons[15] were degenerate, that is, they were so closely packed together that any further compression would not be permitted by the Pauli exclusion principle. Due to this reality, the electrons would generate an incredibly powerful pressure that would resist further contraction, known as degenerate electron pressure. This pressure Fowler realized was more than

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[14] The Fermi-Dirac statistics, developed by physicists Enrico Fermi and Paul Dirac in 1926, is a mathematical model that describes the behaviour of electrons subjected to the Pauli Exclusion Principle. In all atoms, with the exception of the hydrogen atom, the nucleus is orbited by numerous electrons which are distributed in "orbital shells" that permit only a specific number of electrons. Research by the Austrian physicist, Wolfgang Pauli, demonstrated that no two electrons can exist in the same position, nor have the same velocity within the physical reality of the atom (Hawking,1988:67; Kaufmann,1988:417; Serway,1994:860-861;Sullivan,1979:34,38). In this sense, an "...atom can ... be likened to a rather strange tennis club that allows two players on the first court (the innermost orbital shell), 8 on the second court (the second shell), and so forth. While the players on these courts are in constant motion, each one on average plays a specific position and does so alone. No other member can play there at the same time" (Sullivan,1979:34). What Fermi and Dirac discovered was that this seemingly "snobbish" behaviour among electrons persisted even when they were not orbiting an atomic nucleus, but are roaming freely in a highly ionized gas (Sullivan,1979:34).

[15] It should be emphasized that scientists of the time realized that the great densities found in white dwarfs was only possible if the atoms composing the interior regions of the star had been completely stripped of their electrons. The problem was that this could only occur at extremely high temperatures, but white dwarfs were constantly losing heat. In order for the star to truly cool down, the electrons would have to return to their respective atoms, but, this would demand a tremendous increase in stellar volume requiring enormous amounts of energy to expand in the face of an incredibly powerful gravitational field, energy the star could not possibly have. This led Eddington to utter the seemingly paradoxical statement that the star did not have sufficient energy to grow cold (Eddington,1926:172; Evans,1998:139; Hawking,1988:83).

sufficient to allow a white dwarf to counterbalance the crushing force of gravity, just as in its youth, gravity had been balanced by nuclear processes. Furthermore, Fowler noted that this degenerate pressure did not follow the perfect gas law, and thus, could exist even when temperatures were relatively low; in this sense, degenerate pressure was independent of temperature (Evans,1998:139; Fowler,1926:114-115; Kaufmann,1988:417,433-434; Zellik & Smith,1987:293-294). As Fowler wrote, "Temperature then ceases to have any meaning, for the star is strictly analogous to one gigantic molecule in its lowest quantum state. We may call the temperature then zero" (Fowler,1926:115).

In short, Fowler's research implied that once a star, irrespective of its mass, had evolved into a white dwarf, it could exist in this state until it had depleted every ounce of its fuel and subsequently faded into oblivion. Alternatively, one could interpret Fowler's result – as Eddington did – as suggesting that at some point in the far future, the star would somehow find enough energy to expand once again and thus cool down. For his part, Fowler did not seem terribly concerned over which conceptual interpretation his colleagues would favour; on the contrary, he seemed very pleased with the elegance of his analysis, and the ease with which it settled the paradoxical structure of the white dwarf star (Evans,1998:139; Fowler,1926:115; Russell,1935:18-19; Sullivan,1979:35; Wali,1982:34-35). As he summarized, "Whether or no some such explanation may not be equally possible using other forms of statistical mechanics (perhaps the classical) I am not prepared to say. The new form used here seems for entirely independent reasons so satisfactory that its applicability need not be questioned. ***On application it clears up Eddington's question in a convincing manner, and I am content to leave the matter so***" (Fowler,1926:115;emphasis added).

The astrophysical community, too, appeared happy with Fowler's conclusions and matters concerning the evolution and structure of the white dwarf star seemed closed to any further commentary or debate (Chandrasekhar,1972:165; Evans,1998:139; Wali,1982:35).

4.2 The Surprising Discovery

In the summer of 1930, a young student, Subrahmanyan Chandrasekhar, left India for England in order to pursue a doctoral degree in theoretical astrophysics under the supervision of Ralph Fowler. During the three-week voyage, Chandrasekhar began to reflect upon a paper he had written a few days before his departure. In this article, the young scholar had extended the important theoretical work of Fowler concerning the perplexing physical structure of white dwarf stars. By integrating Fowler's analysis with Eddington's work on polytropic[16] gas spheres, Chandrasekhar arrived at a number of very critical results, these being as follows: 1) the radius of a white dwarf is inversely proportional to the cube root of its mass; 2) the density of a white dwarf is directly proportional to the square of its mass; 3) the central density of a white dwarf is six times its mean density (Hawking,1988:83-84; Horgan,1994:500-501; Parker,1995: 106; Penrose,1997:62-63; Wali,1982:34).

Chandrasekhar found his third and final conclusion fascinating, and it induced him to take the following line of reasoning: the density at the center of a white dwarf star was enormous by all accounts, approximately one ton per cubic inch. In such an environment, the electrons are so tightly packed together that their behaviour comes under the purview of the Pauli exclusion principle which forces them to literally flee from one another's presence at velocities which would approach a significant fraction of the speed of light. As such, would it not be necessary to incorporate Einstein's special theory of relativity into the analysis? And if this were done, what would be the theoretical consequences? Such were the questions that touched Chandrasekhar's thoughts.

But before pursuing this line of inquiry any further, Chandrasekhar decided to refresh his memory of the vital conceptual issues involved in stellar and atomic theory by re-examining the works of three physicists who had pioneered research in these two areas, namely, Compton,

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[16] Polytropes refer to a theoretical model of a sphere composed entirely of gaseous material, analogous to a star existing in a state of equilibrium with respect to its own gravity. Polytropes constitute an important conceptual framework in trying to comprehend stellar interiors (Tver,1979:181; Wali,1991:316).

Eddington, and Sommerfeld, whose classical monographs Chandrasekhar had brought along on his journey. After a period of study, Chandrasekhar commenced to systematically work out the relativistic consequences of Fowler's white dwarf model, all the while expecting to find results that subtly refined Fowler's analysis but nothing more. However, as he completed his preliminary work on the matter, a surprising theoretical picture began to emerge from the calculation. Indeed, what his analysis revealed was that there was a physical limit to the amount of sub-atomic resistance that the exclusion principle could generate against gravitational contraction; in essence, the finding appeared to suggest that only stars whose respective masses were below a specific critical mass could ever evolve into the white dwarf stage. This finding surprised the young graduate student for it seemed to strongly contradict Fowler's model. While Chandrasekhar found the results intriguing, he was also troubled by the implications of his analysis, the main point of concern being the fate of stars whose initial masses were greater than the critical mass. What became of these large celestial objects? What kind of stellar creature would they evolve into over time? These were significant questions that demanded a detailed response. However, he would need time to work out the calculation in a more thorough manner and set it on a firmer, theoretical foundation, one that would see him eliminate some of the assumptions he had made in order to facilitate his calculations. In the meanwhile, he would show the results to his supervisor immediately upon his arrival at Cambridge University (Chandrasekhar,1969:582; Gupta, 1995: 200-201;Hawking,1988:83-84; Horgan,1994:500-501; Penrose,1997: 62-63; Wali,1982:34; 1991: 76-77).

On October 2nd, 1930 Chandrasekhar met his doctoral advisor, Ralph H. Fowler, one of the pioneers of modern theoretical astrophysics and a distinguished scientist who had accrued numerous awards and honours for his research in mathematics and physics. Within a few moments of greeting this esteemed academic, Chandrasekhar showed him the results of his startling discovery. Upon looking over the finding, Fowler voiced his reservations concerning the notion that there was a specific stellar mass above which a star cannot evolve into a white dwarf.

Nevertheless, he told his student that he would forward the paper to a colleague, Edward Arthur Milne, for a second opinion. Milne was yet another pioneering figure in the discipline, having published a number of critical works on topics such as radiative transfer, atmospheric stellar physics, and stellar structure. Indeed, given the fact that stellar structure was one of Milne's specialties, Fowler believed it vital that he comment on the unorthodox nature of Chandrasekhar's finding (Chandrasekhar,1945:1-5; Debus,1968:594,1186; McCrea,1951:111,165,168; Parker,1995:106-107; Wali,1991: 81).

Chandrasekhar had to wait a little while before receiving Milne's assessment of his theoretical discovery, but in time the judgment was passed and it was not a positive one. Indeed, Milne thought Chandrasekhar's analysis was seriously flawed as it simply suggested that there was a finite limit to the mass that can exist in the degenerate centre of a star, but did not place any restrictions on the total mass of the star outside this degenerate core. Furthermore, Milne argued that in stars whose respective masses surpassed this limit, one of two things *must* occur: firstly, the core might become even more condensed in structure – what he termed a centrally condensed configuration – or secondly, the core might develop a new supporting surface within the degenerate centre of the star – Milne referred to this state as a collapsed configuration. In closing, he urged Chandrasekhar to pursue his analysis further to prove that all stars, irrespective of their mass, had incompressible cores, or domains of degeneracy (Chandrasekhar,1987:83-84; Evans,1998:144; Milne,1930b; Parker,195:106-107; Wali,1991:121).

In essence, Milne found Chandrasekhar's discovery highly problematic chiefly because it seemed to suggest that only stars of a certain mass were capable of having a degenerate core. In order to understand Milne's objection to this conclusion, a brief, technical detour is required at this point.

In the early 1910s and 1920s, Arthur Eddington was convinced that the surface layers of a star were only marginally relevant in estimating the central density and temperature of a stellar mass. As he wrote, "We can scarcely expect that in an actual star any simple physical law will hold

uniformly from a central temperature of $10{,}000{,}000^0$ [K] to an outside temperature of $10{,}000^0$ [K]..." (Eddington,1926:94-95).

Eddington developed a theoretical model of stellar structure – known as the standard model – that assumed, for the sake of conceptual and mathematical ease, that the surface temperature of a star to be zero, and based upon certain theoretical assumptions regarding the internal properties of stellar matter, his model made specific predictions concerning the surface features of a star. From Eddington's perspective, this arrangement made perfect sense; given the fact that the temperatures in the interior regions of a star were approximately one-thousand times greater than at the surface, it did not really matter whether one assumed the surface temperature to be a few thousand degrees or zero.

Yet, Milne strongly objected to this line of reasoning. He argued instead that, since the surface characteristics of a star are clearly observable and quantifiable, why not employ these indicators in theoretical work on stellar structure and see whether the internal properties of a star deduced based upon empirical observations of surface characteristics, correlate with existing assumptions concerning stellar interiors. As Milne wrote in an early paper, fleshing out his thoughts on this matter:

> In this paper I investigate the relations between the masses, luminosities, and effective temperatures of the stars from a ***standpoint which is philosophically different from that adopted by Professor Eddington*** in his well-known writings. The main conclusions are that it is not possible to infer from the observed masses, luminosities, and temperatures that the interiors of stars are necessarily composed of perfect gas; and that it is not possible to deduce the value of the absorption coefficient for the stellar interior. Instead, we are led to infer a single definite fact concerning the internal density –distribution (Milne,1929:17; emphasis added).

While this stance seems entirely reasonable, Eddington forcefully opposed such logic and ridiculed Milne's ideas in public, asking his colleague at one meeting whether he believed a dog wagged its tail, or if the tail wagged the dog? In light of such harsh criticism from the prominent, senior scientist, Milne began to adopt a conceptual position that demanded that every star have a degenerate core, and hence, his opposition to Chandrasekhar's discovery which implied a

different theoretical picture of stellar structure from his own (Eddington,1926; Evans,1998:133; Milne,1929;1930a;1930b,1931; McCrea,1951; Wali,1991:119-120).

For his part, Chandrasekhar thought Milne had incorrectly read his derivation of the limiting mass, believing that his results showed that not only was there a physical limit to how large a star can grow in aggregate if it is to evolve into a white dwarf, but that this reality also placed a finite limit on the mass of the degenerate core; consequently, this finding strongly implied that in sufficiently massive stars, the phenomenon known as degeneracy could not possibly manifest itself (Chandrasekhar,1987:84; Evans,1998:144-145; Gupta,1995:200-201; Horgan,1994:500-501; Nityananda,1995:554; Parker,1995:106-107).

Despite Milne's criticism, Chandrasekhar believed his results were significant enough to warrant publication in a well-respected astrophysical journal. While he would have liked to have seen his paper published in the *Monthly Notices of the Royal Astronomical Society* (MNRAS), one of astronomy's premiere periodicals published by Cambridge University, he realized that such an expectation was not viable as his article would need to be forwarded to the MNRAS by a Fellow of the Royal Astronomical Society (RAS) who attested to its quality and scholarly merits. Since both Fowler and Milne were troubled by the validity of the paper's conclusion, there was no chance that they would support its publication in the society's main journal.

In light of this reality, Chandrasekhar decided to send his to the American based *Astrophysical Journal*, which constituted another well-known astrophysical periodical of the time. Within a matter of weeks, Chandrasekhar was contacted by the paper's referee, Carl Eckart, who, too, expressed serious reservations over the derivation of the limiting mass. However, rather than rejecting the paper, Eckart allowed Chandrasekhar an opportunity to clarify the rationale behind his calculation. When he was satisfied with Chandrasekhar's explanation, the referee recommended the paper for publication (Wali,1991:121-122). This somewhat controversial paper, entitled, "***The Maximum Mass of Ideal White Dwarfs***" made the following, unorthodox statement concerning stellar evolution:

> The theory of polytropic gas sphere in conjunction with the equation of state of a relativistically degenerate electron-gas leads to a unique value for the mass of a star built on this model. The mass (=0.91O[17]) is interpreted as representing the upper limit to the mass of an ideal white dwarf (Chandrasekhar,1931:81)[18].

A few months after the publication of his first paper on white dwarf stars, Chandrasekhar completed a second paper in which he demonstrated quite explicitly that if the mass of a star was greater than the critical mass, then the phenomenon known as degeneracy could not manifest itself, leaving the gaseous composition of the star under the physical purview of the perfect gas law. He showed his analysis to a friend, a young theoretical physicist, Leon Rosenfeld, who happened to be a close associate of Nobel Laureate, Niels Bohr; Rosenfeld believed the analysis to be critical enough to warrant publication. Encouraged by the words of his colleague, Chandrasekhar began to search for a journal that would have an open mind to his findings. In the end he decided to submit his article to the German astrophysical periodical, *Zeitschrift fur Astrophysik*, whose editor, Erwin F. Freundlich, he had met a number of months earlier. And so, in the fall of 1932, Chandrasekhar mailed Freundlich his paper, perhaps hoping for a sympathetic ear to his unconventional research.

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[17] This circular symbol with a dot in it represents the sun; in short, this numerical value is read as 0.91 solar masses.

[18] In 1929 physicist, V. Wilhelm Anderson, published a paper in which he incorporated relativistic effects into Fowler's white dwarf model and ascertained the existence of a physical limit to the mass of a white dwarf amounting to 1.1 solar masses. A year later in 1930, Edmund C. Stoner published an article critiquing Anderson's paper, arguing that his derivation of the limiting mass was not based on a concise methodology, and attempts to devise a much more rigorous relativistic model of white dwarfs, though in the end, he too cites the upper limit to a white dwarf star as being about 1.1 solar masses (Anderson,1929;Evans,1998:139; Stoner,1929;1930).

In his paper, Chandrasekhar critiques Stoner's work as being flawed as it assumed "... a uniform distribution density in the star which is not quite justifiable" (Chandrasekhar,1931:81), and proceeds to correct this conceptual error.

Lastly, it should be noted that in 1932, the Russian physicist, Lev D. Landau independently duplicated Chandrasekhar's discovery. However, Landau became so distressed over the implication of his finding, which in theory suggested that a star whose mass was greater than the critical mass could collapse to a mathematical point, that he made the strange comment that sufficiently massive stars contained regions in which the laws of quantum mechanics were not adhered to, thus preventing them from enduring such a ridiculously implausible fate (Landau,1932:285-288; Hawking,1988:84; Lightman,1984:34-35).

However, as fate would have it, Milne happened to be visiting Germany at the time, and Freundlich asked Milne if he would referee Chandrasekhar's paper. Not surprisingly, Milne told the editors of the journal that he could not recommend acceptance of the paper as its conclusions concerning the fate of large stellar matter was technically flawed. Milne wrote Chandrasekhar of his decision, and further stated that he believed publication of the article would only hurt the young scholar's reputation. In response to Milne's rejection, Chandrasekhar wrote a detailed response outlining why he thought the paper had merit. Surprisingly, after reading this letter, Milne reversed his initial assessment, and recommended – albeit very reluctantly – acceptance of Chandrasekhar's work (Parker,1995:106-107; Wali,1991:122).

Within the pages of this article, Chandrasekhar makes a number of bold, controversial statements concerning the fate of stars whose masses exceeded the critical limit whose existence he had discovered over a year earlier. For instance after a series of calculations that reaffirmed the theoretical existence of the limiting mass, he writes the following,

> Here, we have the result that for *all centrally condensed stars of mass greater than M* [0.91 solar masses], *the perfect gas equation of state does not break down, however high the central density may become, and the matter does not become degenerate. An appeal to the Fermi-Dirac statistics to avoid the central singularity cannot be made*"(Chandrasekhar,1932:64).

Chandrasekhar concludes his paper with a statement that, undoubtedly, irked Milne's sensibilities when he wrote,

> *Conclusion:* We may conclude that great progress in the analysis of stellar structure is not possible before we can answer the following fundamental question: *Given an enclosure containing electrons and atomic nuclei, (total charge zero) what happens if we go on compressing the material indefinitely?* " (Chandrasekhar,1932:67).

In essence, Chandrasekhar was defending the results of his first paper, arguing that not only was the limiting mass a valid conceptual entity, but in addition, he explicitly emphasizes, through the use of italics, that *all stars* whose masses exceeded the critical limit could not be saved from total collapse by appealing to the phenomenon of electron degeneracy. Given this reality, what

happens to these stellar objects? Did they evolve into some novel, bizarre entity whose nature can only be imagined and speculated upon? And what about stars whose masses were so enormous that it was conceivable that their matter could be compressed indefinitely without end? While such questions posed exciting prospects, they forcefully contradicted academic wisdom of the time which had comfortably accepted the implications of Fowler's analysis that the ultimate, terminal stage of a star was as a white dwarf (Evans,1998:138-139; Gupta,1995; Horgan,1994; Nityananda,1995; Parker,1995; Penrose,1997; Wali,1991:122).

Not surprisingly, Chandrasekhar's unorthodox research soon attracted the attention of one of the most prominent astrophysicists of the time – Arthur Stanley Eddington – who had been quite pleased with Fowler's work, and the concise, neat image of stellar evolution it presented. Suffice it to say, Eddington was not at all pleased to learn that the validity of this conceptual view was under attack by a young scholar who had derived a number of bizarre consequences which the senior scientist found difficult to assimilate, let alone accept. As such, he took it upon himself to prove to the astrophysical community that Chandrasekhar's conclusions concerning stellar evolution were the result of a number of technical errors, and thus, should be completely dismissed (Horgan,1994; Parker,1995).

4.3 The Unorthodox White Dwarf Model

In the summer of 1933, Chandrasekhar received his doctorate, and soon afterwards became a Fellow of Trinity College – his alma mater – and then a Fellow of the Royal Astronomical Society. Ironically, his election to both fellowships had come about due to the strong support of Edward Arthur Milne, who, while strongly disagreeing with the nature of some aspects of Chandrasekhar's research, had great respect for the overall quality of his work and thus had given his young colleague a resounding appraisal.

As a full member of the RAS, Chandrasekhar began to attend the society's meetings on a regular basis; the meetings were held every second Friday of the month in Burlington House on Piccadilly, in the vicinity of the famous Piccadilly Circus. The meetings which had an average

audience of sixty, commenced promptly at 4:30 p.m. and lasted until 6:00 p.m. These sessions would be punctuated with three to four, fifteen-minute presentations that would be followed by lively and rigorous discussion and debate. Chandrasekhar presented papers a number of times before his peers, all these works relating to mainstream, uncontroversial, theoretical issues that were greeted warmly and with great enthusiasm by the membership. Indeed, Chandrasekhar's work attracted a great deal of attention from prominent scientists, as well as junior colleagues. One young scientist in particular, William McCrea, whose specialty was theoretical astrophysics and relativity, became a close friend of Chandrasekhar's, and the two would often attend the meetings together, sitting in the back rows which were reserved for up and coming scholars (Debus,1968:1141; Wali,1991:110,115).

A year after becoming a newly minted doctor, Chandrasekhar travelled to the Soviet Union in order to deliver a series of lectures at the Pulkovo Observatory in Leningrad. One of his lectures focused exclusively on his cutting edge research into stellar evolution, and the discovery of the limiting mass, a finding that had generated very little interest or sympathy at Cambridge, but which seemed to intrigue Russian astrophysicists. One specific scientist, Viktor A. Ambartsumian, a young theorist around the same age as Chandrasekhar, made a critical suggestion to the Trinity Fellow concerning his white dwarf work; he strongly advised Chandrasekhar to construct a complete, general theory of white dwarfs by minimizing, as much as possible, the assumptions and approximations he had made in his earlier papers in order to facilitate the mathematical computations. The enthusiasm and encouragement Ambartsumian showed in the research of his foreign colleague inspired Chandrasekhar to set about creating as exact a theory of white dwarfs as was possible within the conceptual parameters of stellar theory and quantum statistics (Turkovich,1969; Wali,1991:117-119).

And so, from August to December of 1934, Chandrasekhar spent much of his time tied to his desk at Trinity working out complex, algebraic equations and tedious numerical calculations. During these intellectually taxing months, Arthur Eddington, who had begun to take a keen

interest in the junior Fellow's unorthodox research, would visit Chandrasekhar in his quarters approximately two to three times a week to keep abreast of the young scholar's progress; while Eddington uttered no comments to Chandrasekhar during these sessions, he did study the details of the evolving model with great intensity.

By December 1934, Chandrasekhar had finally completed his exhaustive theoretical and computational analysis and had succeeded in developing what was – at least from his perspective – an elegant, general model of white dwarf stars which forcefully reinforced the conceptual validity of the limiting mass, proving to Chandrasekhar that this abstract entity was not simply a mathematical artifact but a direct consequence of integrating relativity with the Fermi-Dirac statistic. Based on the theoretical considerations of his paradigm, as well as the results of his computational work, Chandrasekhar produced a graphical representation of the exact correlation between the mass and radius of a white dwarf.[19] On the Y-axis of the graph, the radius of a star is listed as a ratio of the Earth's radius; the X-axis lists the mass of a star as a ratio of the Sun's mass. Now, the graph has a dashed line which flows down from the top, upper left hand corner of the graph to approximately the mid-point of the graph. In this particular region of the graph, the line begins to take a path that is almost parallel to the X-axis. It should be noted that this specific line represents the mass-radius relationship that results from Ralph Fowler's non-relativistic degeneracy model. It clearly shows that as the mass of a star increases, its radius decreases until it begins to stabilize at 1.44[20] solar masses. In short, Fowler's research concluded that stars, regardless of their mass, would retire comfortably as a white dwarf star.

Yet, Fowler's analysis had ignored the relativistic effects that would be induced in electrons moving close to the velocity of light due to the influence of the Pauli exclusion principle. By rectifying this oversight, Chandrasekhar's work resulted in a curvilinear line that flowed down

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[19] While it would have been extremely convenient for both the author and the reader to have had a copy of Chandrasekhar's graphical analysis reproduced in this section, practical limitations allow only a written description of this graph to be presented (See Wali,1982:37)

[20] The construction of the general, white dwarf model led Chandrasekhar to revise the value of the limiting mass from 0.91 to 1.44 solar masses (Chandrasekhar,1935:216-220;Wali,1982:35).

from the upper left-hand corner of the graph – in much the same way as the line born of Fowler's non-relativistic considerations. However, by approximately 0.60 solar masses, Chandrasekhar's theoretical line begins to noticeably deviate from Fowler's model, and by approximately 1.31 solar masses the deviation between the two lines is very pronounced. Indeed, whereas Fowler's theoretical line begins to stabilize as it nears the critical mass of 1.44 solar masses, Chandrasekhar's commences to flow downward to the lower right hand corner of the graph and intersects the abscissa at exactly 1.44 solar masses. At this point, the corresponding radius is zero. Since a star cannot physically have a radius of zero, the analysis clearly implied that only stars with a mass less than 1.44 solar masses could exist as white dwarfs in a stable configuration Chandrasekhar's derivation of an exact theory of white dwarfs resulted in the generation of two, very detailed papers that he sent to the RAS (Chandrasekhar,1935:218-222; Gupta,1995:200-201; Horgan,1994:500-501; Lankford,1997:144-145; Nityanandra,1995:554; Parker,1995: 106-107; Penrose,1997: 62-63; Wali,1982: 36-37).

Interestingly enough, two months before Chandrasekhar had completed his general white dwarf model, he had published a brief article in *The Observatory* which highlighted some preliminary details of his analysis which seemed to grant strong support to his view that the white dwarf was not the terminal stage for all stars. As Chandrasekhar wrote,

> ... The detailed investigations with full tables of solutions will be published elsewhere, but the purpose of writing this article was to show how the setting up of an exact differential equation to describe the degenerate state has led to an almost complete solution of the general problem of stellar structure ...
>
> Finally, it is necessary to emphasize one major result of the whole investigation, namely, that ***it must be taken as well established*** that the life-history of a star of small mass must be essentially different from the life-history of a star of large mass. For a star of small mass the natural white-dwarf stage is an initial step towards complete extinction. A star of large mass (>M) cannot pass into the white-dwarf stage, and one is left speculating on other possibilities (Chandrasekhar,1934:377; emphasis added).

It is curious – if not bold – for Chandrasekhar to have declared that the differing evolutionary paths of small and large stars to be a well established notion, especially in the light of the serious

reservations both Milne and Fowler had concerning this particular line of investigation. No doubt, from Chandrasekhar's perspective, his theoretical work had firmly convinced him that only a select group of stars evolved into white dwarfs. However, given the reality that scientific knowledge is fundamentally social knowledge, the validity and legitimacy of an idea or body of research is never in the hands of a single individual, but in the hands of a collective (Mendelsohn,1977:3-4; Merton,1968:59-60; Shapin,1994: 5-6). As Shapin writes,

> ...What counts for any community as true knowledge is a collective good and a collective accomplishment. That good is always in others' hands, and the fate of any particular claim that something 'is the case' is never determined by the individual making the claim. This is a sense in which one may say that truth is a matter of collective judgment and that it is stabilized by the collective actions which use it as a standard for judging other claims...(Shapin,1994:5-6).

In Chandrasekhar's case, elite members of the astrophysical collective perceived his work as flawed and highly problematic. The fact that his research had been published in respectable journals – though not without some degree of struggle – probably gave Chandrasekhar a sense that his research had acquired some measure of authority. Yet, the fact remained that key astrophysicists within the community did not accept Chandrasekhar's conclusions, and as such, his discovery of the limiting mass, while "well-established", was by no means well accepted. This posed a serious problem for Chandrasekhar for in science, only knowledge that is effectively embraced and used by others can be seen as achieving a state of "truth" and being allowed to exist within the purview of conventional wisdom in peace. Since Chandrasekhar's research had not been elevated to this prominent level, his seemingly solid, theoretical framework remained highly vulnerable to criticism as he was soon to learn (Merton,1968:59-60; Shapin,1994:5-6; Wali,1991:124-127).

4.4 Eddington's Response

Within days of receiving his papers detailing the general, white dwarf theory, the RAS invited Chandrasekhar to present his findings before its membership at the January 11th 1935 session. At

this point in its history, the RAS decided the content of each meeting a few days prior to the session, the program containing the title of each paper to be presented, and its respective author, distributed to the gathered members only at the meeting itself. Interestingly enough, when Chandrasekhar finally received the program he was surprised to learn that Eddington was scheduled to give a paper directly after his own presentation (Horgan,1994:501; Wali,1991:124). In an interview conducted with his biographer, Chandrasekhar recalls the events leading up to the meeting,

> Then I went to dine in the college and Eddington was there. Somehow I thought Eddington would come to talk with me, so I did not go over to talk with him. After dinner I was standing by myself in the combination room where we used to have coffee, and Eddington came up to me and asked me, "I suppose you are going to London tomorrow?" I said, "Yes." He said, "You know your paper is very long. So I have asked Smart (the secretary of the RAS) to give you a half hour for your presentation instead of the customary fifteen minutes." I said, "That's very nice of you." And he still did not tell me that he too was presenting a paper. So I was a little nervous as to what the story was.
>
> The next day at Burlington House, at the usual tea before the meeting, McCrea and I were standing together and Eddington came by. McCrea asked Eddington, "Well, Professor Eddington, what are we to understand by 'Relativistic Degeneracy'?" Eddington turned to me and said, "That's a surprise for you," and walked away (Chandrasekhar cited in Wali,1991:124).

After this peculiar encounter, both Chandrasekhar and Eddington proceeded to take their seats at the RAS meeting. At the start of the session, the minutes of the last gathering were read and confirmed. Then, the floor was opened up to various astronomers and astrophysicists who proceeded to discuss the newly discovered star, Nova Herculis. In the wake of this presentation, Dr. F.J.M. Stratton, a prominent astrophysicist and President of the society, invited Chandrasekhar to give an account of his recent investigation of stellar configurations. At this point, Chandrasekhar rose to discuss his controversial research (M/RAS[21],1935:37). Following Chandrasekhar's presentation, Milne rose to comment on his junior colleague's work, stating,

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[21] Meeting of the Royal Astronomical Society.

> I have had an opportunity of seeing Dr. Chandrasekhar's paper. We have both been working on the same problem. I had intended to present a paper, written around Mr. Fairclough's latest numerical results, to this Meeting of the Society, but it has been unavoidably delayed. In many ways the methods pursued and the results obtained are the same as Dr. Chandrasekhar's. I have pursued a cruder method of analysis, but I believe that my method gives more insight into the fundamental physical postulates underlying the work, takes account of our ignorance of the behavior of degenerate matter, ***and gives a more rational picture***. A result common to our theory and Dr. Chandrasekhar's is that the more massive a star, the smaller its radius when completely collapsed....(Milne cited in M/RAS,1935:37; emphasis added).

In effect, Milne was informing his colleagues that his own research duplicates the essential components of Chandrasekhar's work, specifically, that the radius of a star is inversely proportional to its mass; however, Milne points out that his research is not only more comprehensive than that of the young Fellow, but provides a much more logical, conceptual vision of stellar evolution, namely, that his research does not suggest that degeneracy cannot exist in stars of a certain mass, nor does it postulate the existence of alternative terminal stages for a star other than the white dwarf configuration, the only known terminal stage at the time.

After Milne concluded his remarks, the President invited Arthur Eddington to take the floor. Eddington's presentation is as follows,

> Dr. Chandrasekhar has been referring to degeneracy. There are two expressions commonly used in this connection, "ordinary" degeneracy and "relativistic" degeneracy ... I do not know whether I shall escape from this meeting alive, ***but the point of my paper is that there is no such thing as relativistic degeneracy!***
>
> I would remark first that the relativistic formula has defeated the original intention of Prof. R.H. Fowler, who first applied the theory of degeneracy to astrophysics. When in 1924, I suggested that owing to ionization we might have to deal with exceedingly dense matter in astronomy, I was troubled by a difficulty that there seemed to be no way in which a dense star could cool down. Apparently it had to go on radiating for ever, getting smaller and smaller. Soon afterwards Fermi-Dirac statistics were discovered, and Prof Fowler applied them to the problem and showed that they solved the difficulty; but now Dr. Chandrasekhar has revived it again. Fowler used the ordinary formula; Chandrasekhar, using the relativistic formula which has been accepted for the last five years, shows that a star of mass greater than a certain limit M remains a perfect gas and can never cool down. ***The star has to go on radiating and radiating and contracting and contracting until, I suppose, it gets down to a few km. radius, when gravity becomes strong enough to hold in the radiation, and the star can at last find peace.***
>
> Dr. Chandrasekhar had got this result before, but he has rubbed it in his last paper; and, when discussing it with him, I felt driven to the conclusion that this was

> almost a reductio ad absurdum of the relativistic degeneracy formula. Various accidents may intervene to save the star, but I want more protection than that. ***I think there should be a law of Nature to prevent a star from behaving in this absurd way!***
>
> If one takes the mathematical derivation of the relativistic degeneracy formula as given in astronomical papers, no fault is to be found. One has to look deeper into its physical foundations, and these are not above suspicion. ***The formula is based on a combination of relativity mechanics and non-relativity quantum theory, and I do not regard the offspring of such a union as born in lawful wedlock.*** I feel satisfied myself that the current formula is based on a partial relativity theory, and that if the theory is made complete the relativity corrections are compensated, so that we come back to the "ordinary" formula.
>
> ... In wave mechanics, the electrons are represented by waves. There are two kinds of waves, progressive and standing. In the ordinary analysis of matter into electrons one is dealing with progressive waves; but in the analysis which leads to the Exclusion Principal (used in deriving the degeneracy formula) the electron is represented by a standing wave. Now an electron represented by a standing wave is a quite different sort of entity from the electron represented by a progressive wave. The former is constantly changing its identity. I might compare the progressive wave with Professor Stratton and the standing with the President of the Royal Astronomical Society; only to make the analogy a good one, the Society would have to change its President gradually and continuously, instead of suddenly every two years. The formulae which apply to such a President would be different from the formulae which apply to an ordinary individual; and this point has a definitive bearing on the question. The electron represented by a progressive wave can be brought to rest by a Lorentz transformation, and then it becomes a standing wave. This transformation introduces a factor into the equation, which is not needed if the waves referred to are standing waves originally. ***My main point is that the Exclusion Principle presupposes analysis into standing waves, and this has been wrongly combined with formulae which refer to progressive waves*** (Eddington cited in M/RAS,1935:37-39; emphasis added).

At this point the President of the RAS stated that the arguments put forth in Eddington's work would need to be carefully considered before any discussion can take place. He then asked the members to return thanks to Arthur Eddington. All further comments on this topic were, thereby, closed, and Chandrasekhar had no opportunity to defend his research (M/RAS,1935:39).

In essence, Eddington's critique of Chandrasekhar's work was designed to point out a number of critical problems with the new model of stellar evolution. Firstly, the prominent scientist begins his lecture by emphasizing that Fowler's research had concisely and efficiently solved the perplexing cooling and structural features associated with white dwarfs. However, Chandrasekhar's investigation had, in effect, thrown a theoretical wrench into this nice, neat

conceptual picture. As such, one was faced with a potentially bizarre scenario in which a massive stellar object, with very little nuclear fuel to feed its enormous bulk and forced to obey the perfect gas law, could, at least in theory, find itself being compressed indefinitely into an entity only a few kilometres in diameter, or even smaller; imagine an object with the mass of a number of suns, yet possessing a diameter equivalent to that of a small city. From Eddington's perspective, and from the perspective provided by 1930s astrophysical knowledge, this was an outrageous theoretical picture, and hence his statement that this constituted a line of reasoning carried to the point of absurdity.

Secondly, Eddington argues that Chandrasekhar's integration of special relativity and quantum mechanics was flawed, as he attempts to combine *relativity* with *non-relativistic* quantum theory; by undertaking this particular path, Eddington seems to imply that his junior colleague was essentially doing the mathematical equivalent of adding two inconsistent, conceptual frameworks together and ending up with an erroneous, nonsensical conclusion.

This leads Eddington to a discussion of standing and progressive wave-functions[22] where he argues that the Exclusion principle assumes that electrons behave as standing waves; now, what Chandrasekhar had done in his derivation of the limiting mass, Eddington proclaims, was combine the formulae for electrons as standing waves (e.g. the Exclusion Principle) with a formulae which represents electrons as progressive waves when he introduced relativity into the analysis. In effect, Eddington painted Chandrasekhar's unorthodox conclusions concerning stellar evolution as a by product of technical and conceptual error (Douglas,1956:160-162; Evans,1998:143-144; Nityananda,1995:554; Parker,1995:108; Penrose,1997:63; Wali,1982:37; 1991:126).

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[22] A ***progressive wave*** can best be described with the following cognitive image: suppose you have an infinitely long piece of rope tied to a wall; if the rope is given a certain amount of energy, the wave-form it generates, will propagate forever.

In the case of a ***standing wave***, the situation is similar, except the wave form will eventually hit another wall and reflect back to the first wall, thereby setting up a stationary pattern (Sundaresan,2000).

According to Wali, Chandrasekhar was quite taken aback by Eddington's criticism, surprised by the senior scientist's strong attack on the integrity of his work. To add to his undoubtedly depressed spirits, those in attendance had apparently come up to Chandrasekhar after the session had ended to tell him that it was 'too bad' that he had expended so much effort in constructing what appeared to be a flawed model. Not soon after, Chandrasekhar ran into Milne who was seemingly in very high spirits, as he believed that Eddington's analysis had invalidated the notion of the limiting mass, and had, indirectly, boosted the viability of his own belief that all stars had degenerate cores irrespective of their mass.

While Chandrasekhar was upset with Eddington's appraisal of his work, he also firmly believed that his research held merits and was built upon a strong theoretical foundation. From Chandrasekhar's perspective, while it was possible that he had overlooked something in his derivation of the white dwarf model, he could not find anything wrong with its conceptual or technical structure, a model he had designed with a minimum of assumptions and approximations. In addition, he thought many of Eddington's comments made little sense, especially his remark that the limiting mass had been derived through a combination of relativity and non-relativistic quantum theory and that this was somehow forbidden by the laws of mathematical physics, since the exclusion principle represents electrons as standing waves, and in relativistic systems, electrons are represented as progressive waves; as such, integrating these two models was erroneous. Yet, there existed no rule which proclaimed that the exclusion principle could not be used in relativistic systems.

As a matter of fact, in 1928, approximately seven years before Chandrasekhar's construction of his comprehensive white dwarf theory, Paul Dirac had successfully combined quantum mechanics with special relativity to explain a number of peculiar features of electrons such as their spin and magnetic moment; this research led to Dirac being awarded the Nobel Prize in 1933. Indeed, a fairly prominent theoretical physicist, Rudolf Peierls, who thought Chandrasekhar's work was technically sound, tells Wali in a private communiqué that the fact

Dirac had very effectively integrated relativity and quantum theory had been pointed out to Eddington, but the senior scientist had simply brushed off this comment (Eddington,1935:194-195; Fowler,1926:114-115; Hawking,1988:67-68; Parker,1995:108; Peierls,1936:780-784; Serway,1994:906; Wali,1991:129-132,135). As Peierls writes,

> ... When it was pointed out that the combination of relativity and quantum mechanics had proved itself in Dirac's theory of the hydrogen atom, he argued that this was a question of symmetry. Relativity was all right for a problem with spherical symmetry, such as the hydrogen atom, but in statistical mechanics you were considering a rectangular volume element dxdydz, and that had a different symmetry (I would have thought a star was more like a spherical box than a rectangular one!) He also brought in the difference between standing and progressive waves[23].
>
> I did not know any physicist to whom it was not obvious that Chandrasekhar was right in using relativistic Fermi-Dirac statistics, and who was not shocked by Eddington's denial of the obvious, particularly coming from the author of a well-known text on relativity.... (Peierls cited in Wali,1991:135).

However, in many respects, the damage had already been done. Scientists who had attended the meeting felt that Eddington's comments had proven that the notion of the limiting mass was highly flawed. Scientists who published works in astrophysics on stellar structure in the aftermath of the January 11th 1935 RAS meeting would point out that Chandrasekhar's work had been critiqued by none other than Eddington himself (Gupta,1995:200; Horgan,1994:501; Nityananda,1995:554; Penrose,1997:62-63; Wali,1991:126). Indeed, as Milne wrote in a follow-up piece immediately after Eddington's lecture before the RAS, "... ***Sir Arthur Eddington's investigations may now confer on our work a justification*** to which it is only accidentally entitled ... The work evidently coincides in aim, and partly in results, with the similar work by Dr. S. Chandrasekhar..." (Milne,1935:137; emphasis added).

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[23] Interestingly enough, in his monograph, *Relativity Theory of Protons and Electrons (1936)*, Eddington commences his detailed discussion of the harmonisation of relativity and quantum theory for the purposes of investigating the relativistic degeneracy formula employed by Chandrasekhar in deriving his paradoxical conclusions concerning stellar evolution, by asking the reader to "Consider a rectangular block of matter (e.g. in the interior of a star) of dimensions $l_1 \times l_2 \times l_3$, and containing $\sigma l_1 l_2 l_3$ electrons" (Eddington,1936:234). It was, essentially, this curious example Rudolf Peierls was referring to in his private comments to Professor Wali.

Similarly, in a piece written by the famous observational astronomer, Henry Norris Russell, about seven months after the memorable RAS meeting, he explains the logic of Chandrasekhar's work, and the notion of the limiting mass in some detail; afterwards he goes on to say, "All this sounds pretty speculative. But it is based on precise calculation, founded on a theory which is generally accepted – ***though Eddington has recently criticized it*** (in a paper comprehensible only to a very small group)..."(Russell,1935:19; emphasis added).

Lastly, in 1936, Ralph Fowler published a comprehensive monograph of statistical mechanics; in a footnote in a chapter discussing stellar theory, he points out that validity of Chandrasekhar's relativistic degeneracy formula was questioned by Eddington. As Fowler wrote, "***It has recently been contended by Eddington ... that [equation] 1805 is invalid*** and does not apply to an electron in any stationary state in an enclosure, but only to electrons represented by progressive waves. If his contentions are correct then the formulae derived from 1805 are meaningless and we must always use formulae derived from $E = p_{\bullet}^{2}/2m$ (i.e. Schrodinger's equation) for electrons represented by standing waves in an enclosure" (Fowler,1936:652; emphasis added).

Thus, in almost every paper immediately published after the RAS meeting to touch upon the concept of stellar structure and the limiting mass, the respective authors pointed out that Eddington had serious problems with Chandrasekhar's work on this specific field of inquiry. The only exception to this pattern was an article written by Peierls, who tried to counter Eddington's arguments that the exclusion principle could not be applied to relativistic systems, by mathematically proving that such a combination did not make any of the conceptual violations that Eddington had claimed. As Peierls wrote, in a paper communicated to the RAS by Chandrasekhar himself,

> The problem of a degenerate relativistic gas has recently become of importance for work on stellar structure. Some controversy has arisen as to whether there is an equation of state in the usual sense of the word, *i.e.* whether the pressure-density relation of such a gas enclosed in a certain volume would be independent of the shape of this volume. This might seem sufficiently obvious to make a proof unnecessary, but in view of the controversy it is perhaps worth while to give a proof (Peierls,1936:780).

In any event, Peierls' effort seems to have made little difference in Eddington's opinion that the limiting mass was at the very least, an unrealistic phenomenon in practice (Horgan,1994:501; Nityananda,1995:554; Parker,1995:108; Peierls,1936:780-784; Wali,1991:135).

For Chandrasekhar this was a serious problem. As a very young scientist who had yet to firmly establish himself as a scholar, he had made a literally surprising claim that strongly contradicted conventional wisdom concerning the physical and evolutionary nature of stars, a claim that had been quite elegantly and forcefully attacked by one of the most famous scientists of the time, a scholar whose reputation in astrophysics was beyond doubt. As such, Eddington's remarks no doubt cast Chandrasekhar's credibility into question. Acutely aware that his integrity as a theorist had been damaged to some degree by the elder scientist's critique, Chandrasekhar embarked on a mission to settle the validity of his work once and for all.

4.5 The Appeal to Authority

Bruno Latour notes that during the tenure of many scientific controversies, the researcher who is the author of an unorthodox discovery – a *Mr. Anybody* – and faced with a particularly prominent researcher – a *Mr. Somebody* – who is sceptical or hostile to his/her results, will tend to find allies either within the discipline in question, or from a related field – so called "aliens" as Harry Collins refers to them. Yet, no ordinary ally will do; the particular ally sought should be a scientist – or more preferably scientists – who has a strong reputation as a scholar, someone whose opinions cannot be easily brushed aside (Collins,1999; Latour,1987:30-32). Under such circumstances, "... Mr. Somebody's tone of voice has been transformed. Mr. Anybody is to be taken seriously since he is not alone anymore: a group, so to speak, accompanies him. Mr. Anybody has become Mr. Manybodies! This appeal to higher and more numerous allies is often called the **argument from authority**..."(Latour,1987:31).

In Chandrasekhar's case, he knew that his words alone could do little to neutralize the intellectual authority of someone of Eddington's stature. Given this reality, Chandrasekhar believed that a strong verdict from prominent and famous physicists like Niels Bohr, Paul Dirac,

and Wolfgang Pauli would succinctly settle the matter once and for all; of course, Chandrasekhar realized that these distinguished scientists could potentially point out to him that Eddington's critique of his model did have strong merits. Nevertheless, either way, at least the dispute would find closure.

And so, over the course of the next few weeks immediately following the RAS meeting, Chandrasekhar sought the advice of a number of very renowned theoretical physicists. His first contact was with Leon Rosenfeld who was in Copenhagen at the time assisting Bohr with his research. Chandrasekhar gave a brief account of his work to his friend in a personal letter, and asked him to seek Bohr's advice on the matter. Chandrasekhar wrote yet another memo to Rosenfeld after he had a long conversation with Eddington in which the elder scientist gave an interpretation of Pauli's exclusion principle that seemed to clash with that held by theoretical physicists. Within days, Rosenfeld responded, informing Chandrasekhar that both he and Bohr saw no technical error in Chandrasekhar's work, but found Eddington's remarks with respect to the exclusion principle to be baffling.

In the wake of these comments, Chandrasekhar approached Eddington once again to try to see if he could devise an argument that would enable Eddington to see the significance of his conclusions with respect to stellar evolution. However, Eddington continued to hold firm to his opinion that Chandrasekhar's research was highly flawed on technical grounds. Faced with this situation, Chandrasekhar wrote Rosenfeld again telling him that he was sending a copy of Eddington's thoughts on his research, and asking him if he could persuade Bohr to write a definitive letter that would counter Eddington's comments and reinforce the validity of his [Chandrasekhar's] model, a letter he could perhaps publish, or at least make public.

Yet, Rosenfeld responded that Bohr was extremely busy and could not spare the time to get involved in a new topic. Nevertheless, Bohr had a proposal that would undoubtedly satisfy Chandrasekhar's demand for an authoritative statement on the matter. He suggested that Eddington's argument against the young scientist's work be forwarded to Wolfgang Pauli

himself, the discoverer of the exclusion principle and one of the pioneers of quantum mechanics, for an expert assessment. Chandrasekhar readily agreed to the proposal (Evans,1998:144-145; Parker,1995:108; Wali,1991:126-132). While awaiting Pauli's verdict, Rosenfeld wrote Chandrasekhar a follow up letter, "... After having courageously read Eddington's paper twice, I have nothing to change in my previous statements; it is the wildest nonsense" (Rosenfeld, cited in Wali,1991:131).

Before long, Pauli responded to Chandrasekhar's inquiries concluding that Eddington's remarks concerning the legitimacy of applying the exclusion principle to relativistic systems were completely unfounded as there was no rule that prohibited such a combination, nor was such an avenue of inquiry clouded with ambiguity as Eddington seemed to suggest. Paul Dirac, another pioneer in the realm of quantum mechanics, and a Nobel Laureate, also voiced his support for Chandrasekhar's work, stating that it was technically flawless (Dalitz,1995:xvii; Debus,1968:463; Evans,1998:144-145; Hawking,1988:55-56,68; Parker,1995:108; Serway,1994:906-907; Wali,1991:132).

Yet, much to Chandrasekhar's frustration, none of the aforementioned theoretical physicists were willing to publicly criticize Eddington's interpretation of quantum mechanics and its application to relativistic systems[24]. Part of the problem was due to the historical context in which this controversy transpired. Indeed, that at this point in history, astrophysics was viewed as a particularly problematic field of inquiry by most theoreticians, a field not susceptible to experimental testing, filled with untested assumptions and ambiguities. Indeed, as there was no way to empirically test the merits of Chandrasekhar's model, Bohr et. al no doubt felt that had they entered the debate, they would be dragged into an endless series of arguments that would force them to invest much of their time learning a wholly new area of inquiry which they felt was plagued with countless theoretical and methodological problems to begin with. This reality,

[24] Chandrasekhar was eventually successful in getting Rosenfeld to co-author a very brief, two page paper with him that was published in the prominent scientific journal, *Nature*, in which they re-state the apparent legitimacy of the limiting mass (Chandrasekhar & Rosenfeld,1935:998-999).

coupled with the prospect of confronting a prominent colleague who, from their point of view, had convinced himself of some truly strange ideas would amount to a fruitless exercise (DeVorkin,1989; Evans,1998:144-145; Wali,1991:132).

In truth, it is questionable how much of an impact Bohr, Dirac and Pauli would have made among astrophysicists had they decided to intervene in this particular debate. While these scientists possessed great reputations and stature within the physics community, they were not astrophysicists; in contrast, Eddington's authority in astrophysics was enormous and many fellow astrophysicists seemed quite pleased with Eddington's assessment of Chandrasekhar's work. Indeed, William McCrea, who had attended the RAS meeting in which Eddington critiqued his friend's work, informs Wali in a personal letter that the elder scientist's comments concerning the limiting mass sounded legitimate at first. As McCrea wrote,

> When I listened to Eddington ... I could not immediately weigh up all implications of what he said, but my instinct seemed to tell that he might be right....
>
> What I am ashamed of is not having tried to get to the bottom of the sort of argument Eddington produced. ***Had anyone other than Eddington produced such arguments, I suppose I should have done so***. But they were superficially satisfying to me, and since they satisfied Eddington, I confess that I was content to let it go at that.... (McCrea cited in Wali,1991:134; emphasis added).

Edward A. Milne, too, seemed very much at ease with Eddington's critique of Chandrasekhar's white dwarf model. However, Chandrasekhar wrote to Milne that he had sought out the advice of some of the most esteemed names in theoretical physics who all unanimously agreed that his conclusions concerning stellar evolution were founded on solid, technical grounds. This exercise greatly annoyed Milne who did not appreciate the fact that theoretical physicists – *outsiders* – were offering opinions concerning astrophysical phenomena (Chandrasekhar,1969:583; Evans,1998:144-145; Parker,1995:106,108; Wali,1991:132). As Milne wrote to Chandrasekhar,

> Your marshalling of authorities such as Bohr, Pauli ... impressive as it is, leaves me cold. If the consequences of quantum mechanics contradict very obvious, much more immediate considerations, then something must be wrong with the principles underlying the equation of state derivation ... ***To me it is clear that matter cannot***

> ***behave as you predict***... A theory must not be used as a doctrine to *compel* belief ... Eddington is nearly always wrong in his work in the long run, and I am quite prepared to believe that he is wrong here, in his details. But I hold by my general considerations (Milne cited in Chandrasekhar,1969:584; emphasis added).

As Kennefick argues, one of the main dilemmas facing a researcher who generates a highly unorthodox finding is to find that key argument or proof that will persuade his/her opponents of the legitimacy of the claims being made. But when one's critics happen to be eminent scientists whose beliefs concerning a given topic area are well entrenched, such a task is often an uphill battle. In Chandrasekhar's case, he had tried a number of ways to convince Eddington, Fowler and Milne of the significance of his research, but to no avail (Kennefick,2000:26; Pinch,1994:96-97).

Finally, Chandrasekhar persuaded Christian Moller, a noted physicist who had made important contributions to relativity and quantum mechanics to co-author a paper with him in which they derived the relativistic degeneracy formula anew, using an alternative approach which sought to explicitly address the technical arguments Eddington had mentioned concerning the union of relativity and the non-relativistic quantum theory. The essence of this paper was to demonstrate that even if they were to use Dirac's relativistic quantum theory, and taking into consideration Eddington's own objections to Chandrasekhar's work, that the relativistic degeneracy formula Chandrasekhar had employed in his research was in fact valid. In their conclusion, the authors adopt a most diplomatic tone, almost if they are trying not to offend Eddington, yet, at the same time registering their disagreement with the views put forth by the eminent scientist (Moller & Chandrasekhar,1935:673-676). As they wrote,

> ... In conclusion we wish to state that we do not intend this note as a reply in any sense to Eddington's papers. We thought it of interest, however, to point out that, starting with the energy-stress tensor as is defined in *relativistic* quantum mechanics and following Eddington's own procedure for calculating the pressure, we are simply led back to the relation between P [pressure] N [total number of electrons] one had earlier derived ... by directly inserting in it the relation between E [kinetic energy of the electron] and p [momentum] given by relativistic mechanics (Moller & Chandrasekhar,1935:676).

Almost immediately upon publication, Eddington followed it with a rebuttal. In his two page critique, Eddington stated that Moller and Chandrasekhar's paper contained a serious contradiction that invalidated their argument from the start (Eddington,1935; Moller and Chandrasekhar,1935; Wali,1991:133). Now, this contradiction Eddington wrote of was as follows: in their article, Moller and Chandrasekhar make the following argument,

> To go over to the case of N electrons in a finite volume V which satisfy the exclusion principle.... Let $\psi_{p,s}$ be a suitable normalized eigen solution ... with a definite value for the momentum p and spin s. ***Since the electrons are confined in a finite volume, p takes on only discrete values*** (Moller & Chandrasekhar,1935:674; emphasis added).

Eddington counters that the final sentence in this statement is technically flawed as it seriously contradicts the logic of the uncertainty principle. As he wrote,

> Thus at the outset they contradict the Uncertainty Principle, which asserts that when ***the position of an electron is known within finite limits the momentum p has not a definite value ...***
>
> ***My case against former proofs was that they conflict with relativity theory; my case against this proof is that it conflicts with wave mechanics.***
>
> The Exclusion Principle was first enunciated for electron orbits or states in an atom; for applications of this type it has been abundantly verified. Undoubtedly there exists a generalization of it applicable to the statistics of large assemblies of particles. But the generalization cannot be of the form assumed by Moller and Chandrasekhar, which conflicts with the Uncertainty Principle. In my treatment (and, I think, in most earlier treatments) the generalization is supposed to apply to standing waves. For standing waves the unit solutions correspond to discrete values of p^2; and p has no definite value – as the Uncertainty Principle insists (Eddington,1935:20; emphasis added).

Suffice it to say, Chandrasekhar found Eddington's rebuttal utterly perplexing, as he seemed to be interpreting a statement in the body of his [Chandrasekhar's] paper in a completely erroneous fashion.

It should be emphasized that the *Pauli Exclusion Principle* states that one cannot have two identical particles in the same quantum state at the same time; a quantum state refers to a set of conditions describing the speed and location that a particle is allowed to occupy. In contrast, the *Uncertainty Principle*, formulated by Werner Heisenberg in 1927, states that it is impossible to

take measurements of both a particle's position and momentum (p) with infinite accuracy. In practical terms, the uncertainty principle argues that there is an inversely proportional uncertainty between a particle's position and momentum; thus, the more precisely you try to determine a particle's position, the less certain you are of the particle's momentum; conversely, the more accurately you try to measure a particle's momentum, the less certain you are of its position (Kaufmann,1988:417,578; Serway,1994:825,860; Sullivan,1979:34).

Now, it appeared that Eddington was interpreting Chandrasekhar and Moller's statement that, since the electrons in a star were confined within a ***finite volume of space, a definitive value for their momentum could be assigned***, *as implying* that the positions of these electrons had been allotted a precise position, thereby contradicting a fundamental axiom of the uncertainty principle.

For Chandrasekhar's part, this reading of his and Moller's paper came across as truly bizarre, for to say that electrons are confined to a specific volume is not the same as stating their exact location; while the electrons in a white dwarf are restricted to the physical boundary of the star, this does not mean their individual locations were assigned a discrete value in the analysis. But, for some reason, Eddington, who should have easily known this distinction, read the statement in exactly this fashion. When Chandrasekhar went to discuss the matter with the elder scientist, Eddington insisted that his interpretation was perfectly consistent with the conceptual elements of quantum mechanics. Chandrasekhar apparently left the meeting completely exasperated (Evans,1998:144-145; Parker,1995:106-108; Wali,1991:133).

While Chandrasekhar was unable to construct an argument that persuaded Eddington of the significance of his discovery, Eddington enjoyed plenty of success on the lecture circuit in critiquing the younger scientist's work. For instance, in July of 1935, the International Astronomical Union meeting was held in Paris, France. Once again, Eddington gave a lecture in which he attacked the legitimacy of the limiting mass, arguing that every star, irrespective of its mass, settled down in its old age as a white dwarf. Chandrasekhar, who was in attendance at the

conference, made an attempt to rebut Eddington's comments, but Henry Norris Russell who was chairing the session would not allow it. About one year later, in 1936, at the Harvard Tercentenary Conference of Arts and Sciences, Eddington once again criticized his junior colleague's work (Eddington,1937:131-144). As he remarked,

> The high density of the companion of Sirius was duly confirmed by Professor Adams – but this puzzle remained. Shortly afterward Prof. R.H. Fowler came to the rescue in a famous paper, in which he applied a new result in wave mechanics which had just been discovered. It is a remarkable coincidence that just at the time when matter of transcendently great density was discovered in astronomy, the mathematical physicists were quite independently turning attention to the same subject. I suppose that up to 1924 no one had given a serious thought to abnormally dense matter; but just when it cropped up in astronomy it cropped up in physics as well. Fowler showed that the newly discovered Fermi-Dirac statistics saved the star from the unfortunate fate which I had feared.
>
> I will say a word or two about Professor Fowler's explanation. My colleague Fowler was in his youth a pure mathematician, and I am afraid he never really recovered from this upbringing. Consequently, although his paper contained reassuring equations, it did not clearly reveal the simple physical modification of ideas which wave mechanics brought about. He proved that the star would manage all right. But, as you may have inferred from Professor Hardy's revelations, I am not an extreme worshipper of proof. I want to know *why*; a proof does not always tell you that. As Clerk Maxwell used to ask, "What's the go of it?" Well, in this case the "go of it" was that whereas the older theory said that atoms could only be ionized by high temperature the new wave mechanics said that high temperature was not essential because they could also be ionized by crushing them under high pressure. Several writers tumbled to it, before I did, that that was what Fowler's other mysterious result really meant; but I think that it is still not at all generally known. You see this allows the star to cool down and still retain its enormous density – which the older quantum theory did not.
>
> Not content with letting well alone, physicists began to improve on Fowler's formula [e.g. Anderson and Stoner]. They pointed out that in white dwarf conditions the electrons would have speeds approaching the velocity of light, and there would be certain relativity effects which Fowler had neglected. Consequently Fowler's formula, called the *ordinary* degeneracy formula, came to be superseded by a newer formula, called the *relativistic* degeneracy formula. All seemed well until certain researches by Chandrasekhar brought out the fact that the relativistic formula put the stars back in precisely the same difficulty from which Fowler had rescued them. The small stars could cool down alright, and end their days as dark stars in a reasonable way. But above a certain critical mass (two or three times that of the sun) the star could never cool down, but must go on radiating and contracting until heaven knows what becomes of it. ***That did not worry Chandrasekhar; he seemed to like the stars to behave that way, and believes that is really what happens***. But I felt the same objections as 12 years earlier to this stellar buffoonery; ***at least it was sufficiently strange to rouse my suspicion that there must be something wrong with the physical formula used***.

> "I examined the formula – the so-called relativistic degeneracy formula – and the conclusion I came to was that it was the result of a combination of relativity theory with a nonrelativistic quantum theory. I do not regard the offspring of such a union as born in lawful wedlock. ***The relativistic degeneracy formula – the formula currently used – is in fact baseless***; and, perhaps rather surprisingly, the formula derived by a correct application of relativity theory is the ordinary formula – Fowler's original formula which every one had abandoned.... (Eddington,1937:137-138; emphasis added).

Once again, Eddington tried to stress the fact that given the highly peculiar implications of Chandrasekhar's work, namely, the possibility that a sufficiently massive star could collapse into some mysterious entity other than a white dwarf, one which astronomers had yet to detect, or even more bizarre, a mathematical singularity, there had to be something significantly wrong with Chandrasekhar's analysis.

Chandrasekhar did not attend the Harvard conference and thus did not hear Eddington's now familiar reasons for discounting his white dwarf model. Instead, he was preparing to permanently leave England for the United States, which he finally did in December of 1936. However, this did not mean his controversy with Eddington was over. Indeed, a few years later in 1939, Chandrasekhar decided to attend the Paris Conference on white dwarfs and supernovae, knowing full well that he would cross paths with Eddington once again. At the conference, Chandrasekhar presented his general, white dwarf model in complete detail, even providing extremely detailed numerical calculations. Chandrasekhar ended his lecture by stressing that his research should be read as tentative as opposed to a definitive statement on stellar evolution, an interesting change in tone from his earlier published comments on the issue which were decidedly bold in nature. As he wrote,

> For stars of mass less than M_3 [limiting mass] we can tentatively assume that the completely degenerate state represents the last stage in the evolution of stars – the stage of complete darkness and extinction ... For $M > M_3$ no such simple interpretation is possible ... Since degeneracy cannot set in, in the interior of such stars, continued and unrestricted contraction is possible, in theory ... The above remarks ... are made with due reserve and no definiteness is claimed for them (Chandrasekhar,1939:244-245).

Eddington, who was present in the audience made no comment.

On the following day, a Saturday, Eddington was scheduled to present. Not surprisingly, in his lecture Eddington reiterated his familiar objections to Chandrasekhar's theoretical research into stellar evolution. After the presentation, a noted observational astronomer, Gerard Kuiper, asked Eddington if there were any observational tests that could be performed that would permit a resolution to the debate. Eddington apparently took issue with this question, arguing that there did not exist two conceptual stellar models, but only one, namely Fowler's white dwarf paradigm. He further argued that while observational tests could facilitate the choosing of one model over another, it was little use in discriminating between rival conclusions that epitomize the same hypothesis. At this point, Chandrasekhar tells his biographer, he became quite angry (Colloque International d'astrophysique,1939:269-289; Wali,1991:133-138,181-182). In his words,

> ... I asked, 'Well, Professor Eddington, how can you say that? Last week we were at a discussion together with Dirac and Pryce, and Dirac and Pryce told you they did not accept your theory of degenerate matter. And if Dirac does not accept your theory, I do not see how you can claim, with respect to an observational astronomer, that there is only one theory (Chandrasekhar cited in Wali,1991:137).

At this point in the heated discussion, Henry Norris Russell who was presiding over the conference, as he did over the first Paris meeting of 1935, ended all further debate. In many respects, this specific conference was a memorable one as it would be the last time Chandrasekhar and Eddington would ever meet, as Eddington would eventually die of cancer in 1944. Chandrasekhar confides in his biographer his final conversation with the elder scientist, a conversation that is most note worthy for clearly displaying the personal conviction of both Eddington and Chandrasekhar's in the legitimacy and correctness of their own ideas, and the personal tension this intellectual stance generated between the two colleagues (Colloque International d'Astrophysique,1939:289; Debus,1968:506; Douglas,1956; Evans,1998:145; Parker,1995:108; Wali,1991:137-138). As Chandrasekhar said,

> After the meeting ended, there was a gala lunch at the City Hall ... I was sitting way in the corner somewhere, really extremely upset and annoyed, because of the way in which the whole discussion had gone. After lunch, I was standing entirely by myself waiting to leave in the next hour ... Eddington suddenly appeared next to me. He said, "I am sorry if I hurt you this morning. I hope you are not angry with what I said." I said, "You haven't changed your mind, have you?" "No," he said. "What are you sorry about then?" I said and turned away. Eddington sort of stood there for a few moments and walked away (Chandrasekhar cited in Wali,1991:138).

Chandrasekhar tells his biographer that he came to regret his harsh, final words to Eddington, a scholar he continued to admire despite their academic differences. In the wake of their last encounter, Chandrasekhar left Paris for his newly adopted country of the United States. Soon after reaching home, a comprehensive monograph he had started after the memorable 1935 RAS meeting, *An Introduction to the Study of Stellar Structure*, was published in late 1939. After the publication of this text, Chandrasekhar permanently left the realm of white dwarf research and moved into other realms of investigation; at this point, as far as he was concerned the controversy with Arthur Stanley Eddington was over.

Indeed, the astrophysical literature seemed to share Chandrasekhar's sentiments, as references to the famous debate between Eddington and his junior Fellow are virtually absent, as are any peer reviewed articles discussing the limiting mass in the years following the 1935 RAS meeting. This is not surprising, as Eddington's stature within astrophysics convinced many in the community that the notion of the limiting mass appeared too incredible to believe it represented an actual physical phenomenon (Horgan,1994:501; Nityananda,1995:554; Parker,1995:108; Penrose,1997:63; Wali,1982:40; 1991:143-146,189).

Harry Collins argues that one of the most effective strategies that orthodoxy can initiate in the face of a controversial discovery is to ignore it, hoping it will fade away due to lack of intellectual input either positive or negative (Collins,1985:150). As he writes, "Even to criticize an idea in a devastating way is to start to bring about its institutionalization" (Collins,1985:150).

In this respect, since Chandrasekhar's work had been continuously attacked in the literature by scientists such as Milne and Eddington, it would appear its institutionalization was well under way. Indeed, despite Eddington's criticism of the limiting mass and his apparent success in convincing many of his peers that it was an absurd notion, this did not mean that scientists completely and utterly ignored Chandrasekhar's model in its entirety. As a matter of fact, scientists such as Marshak (1940), Wares (1944) and Reiz (1949) generated papers in the years following the 1939 Paris Conference in which they wrote of Chandrasekhar's white dwarf theory in a positive light. Indeed, Marshak's article was the first published work in the literature to demonstrate that the white dwarf mass-radius prediction of Chandrasekhar's theory was in fact supported, *at least tentatively*, by observational data. He points out the paradigm seems to accurately predict the observed radius of the white dwarf star, 40 Eridani B, though in the case of Sirius B there appears to be a significant discrepancy (Mashak,1940:322,352-353). In any event, in the final analysis he declares that the "... theory seems to be well founded"(Marshak,1940:352).

It should be pointed out that while the above-cited works treat Chandrasekhar's model in a favourable fashion, these sentiments primarily reflect intellectual opinion concerning the theoretical mass-radius prediction of Chandrasekhar's work, and do not extend to the notion of the limiting mass, a topic on which the articles are silent. This is not surprising given Eddington's stand on the issue, and the absence of any other known bizarre, stellar masses whose characteristics surpassed the peculiar structural features of white dwarfs, entities that should exist if the limiting mass was in fact a valid physical phenomenon.

Nevertheless, Chandrasekhar's model received unexpected support, in a theoretical sense, when the gifted Berkeley physicist, J. Robert Oppenheimer and a Russian colleague, G.M. Volkoff, postulated that a sufficiently massive star, one whose initial mass was much greater than

the limiting mass[25], would not become a white dwarf but would instead evolve into an entity composed entirely of neutrons[26]. The results of this research implied the existence of an incredibly compact object with a diameter of roughly 30 kilometers (18.6 miles) and a density of 400 trillion grams per cubic centimetre. Suffice it to say, these results were so fantastic that they were not taken seriously by physicists of the time, and the astrophysical community had little or no reaction to such speculation other than to ignore it; part of the problem was that – just as in Chandrasekhar's case – the results appeared too bizarre to be easily digested against an already well established theoretical and observational background; in addition, the predicted diameters of these objects were so small, that no technological apparatus of the period had the necessary power to detect their existence either directly or indirectly[27] (Hawking,1988:85; Kaufmann,1988:449; Lasota,1999:40-43; Oppenheimer & Volkoff,1939:375,381).

In essence, in the wake of the Chandrasekhar-Eddington controversy, Chandrasekhar's work came to be paradoxically perceived by the astrophysical community as a flawed model of stellar evolution, but also as a framework that held some conceptual merits with respect to specific issues concerning the structural composition of white dwarfs. Yet, its critical prediction that not all stars would end their days as a white dwarf constituted a dubious notion whose legitimacy would only come to be slowly accepted after the passage of many decades.

In many ways, this controversy is most instructive for demonstrating how scientific practice is a combination of both technical and social factors, an endeavour that is in one sense seemingly impervious to sociological or psychological concepts, but in another, strongly influenced by such

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[25] They referred to Landau's work in this respect and not Chandrasekhar's (Oppenheimer & Volkoff,1939:374-375).

[26] In truth, the idea of a star composed entirely of neutrons was first proposed in 1933 by Fritz Zwicky and Walter Baade. Their inspiration for this strange, conceptual creature was the experimental discovery of the neutron a year earlier in 1932 (Chandrasekhar,1972:166; Kaufmann,1988:449).

[27] It should be noted that Chandrasekhar was well aware of Oppenheimer & Volkoff's research and implicitly referred to it at the 1939 Paris conference when he stated, "... If the degenerate cores attain sufficiently high densities (as is possible for these stars) the protons and electrons will combine to form neutrons. This would cause a sudden diminution of pressure resulting in the collapse of the star onto a neutron core giving rise to an enormous liberation of gravitational energy. This may be the origin of the Supernova phenomenon" (Chandrasekhar,1939:245).

extra-scientific factors. The complex, socio-political character of science is by no means a new phenomenon, though it always, for some reason or another is perceived as such when an author attempts to articulate a specific debate or controversy from a sociological perspective. As Trevor Pinch states, "There is no turning the clock back to some mythical Golden Age when scientists were all true gentlemen (they never were anyway, as the history of science has taught us in recent years) ... It is our image of science that needs changing, not the way science is conducted" (Pinch,1994:99).

5.0 The Theoreticians' Regress

As Trevor Pinch (1977) has demonstrated in his analysis of the Bohm-von Neumann controversy, mathematical proofs do not always provide a convincing, final solution to a specific, theoretical problem. Indeed, in many ways, what is considered a satisfactory proof is a function of factors that often reside outside the boundaries of the strictly logical and rational, guided by the same extra-scientific criteria that are frequently employed to judge the quality and validity of a controversial, experimental discovery. As such, when faced with a scenario in which a particular proof is not perceived by a community of scholars to prove its case, the sociologist of science is forced to examine not only the technical reasons behind the rejection of the proof, but also socio-cultural variables such as the social status of both the designers of the proof and his/her critics, the intellectual training of the various parties involved, and the characteristic style and approach with which the participants in a mathematical dispute approach a given conceptual puzzle, all of which may hold the seeds to potential explanations of a specific academic disputation (Collins,1975,1985,1994;Kennefick,2000; Pickering,1981,1995; Pickering & Stephanides,1992; Pinch,1977,1981,1994; Simon,1999).

Of course, when dealing with a situation in which a proof gives rise to an unexpected discovery, one is forced to first consider the possibility that the analysis was flawed. Indeed, in any complex derivation a scientist is required to perform a series of mathematical operations, the more intricate the procedures and the more steps that have to be taken in the completion of the proof, the greater the probability that an error will occur either in the logic of the analysis, the overlooking of critical values, or even a basic arithmetical mistake. The luxury of such proofs is that they are often done by hand and scientists can trace each individual step of the analysis in order to flesh out any analytic mistakes that might have given rise to a problematic conclusion.

In contrast, a majority of the research problems being investigated in contemporary physics is done using numerical methods in which a given calculation is performed on a computer which in turn is guided by an exhaustively designed program that might be thousands, or millions of lines

in length. If this mathematical model should happen to generate an unexpected finding, it becomes extremely difficult to know from a practical standpoint whether this result reflects a "true" discovery, or whether it constitutes the by-product of a faulty algorithm. Unlike pen and paper researchers, numerical analysts do not have the convenience of checking each individual step in the calculation given the enormity of the code. As such, they must test their program under conditions in which the answer is known beforehand; if the program generates a result that agrees with pre-existing results, then the researcher will gain some measure of confidence in the validity of the software.

Yet, given the reality that computer programs take many months to design, perfect and test, the author of the code develops a familiarity with the software – known as *tacit knowledge* – that is very difficult to communicate to others. Indeed, since programs are written in such different manners, often reflecting the style and approach of its creator, it becomes virtually impossible for others to exactly replicate the code in order to determine whether any unorthodox phenomenon predicted by the model is in fact legitimate; this problem is further exacerbated if the designer of software and those attempting to duplicate it come from differing intellectual traditions and have divergent experiences and approach to the conceptual problem under investigation.

Given the above socio-cognitive reality, the proponents and detractors of a troublesome theoretical discovery will inevitably find a host of technical as well as conceptual issues that provide ample academic fodder for heated debate, a debate in which a resolution may prove elusive and virtually impossible to obtain. This is especially true if experimental or observational tests to verify the unorthodox results are not available during the tenure of a controversy. As such, what inevitably results is the manifestation of a circular pattern of logic in which both sides in a specific dispute claim that their interpretation of the results are sound and allege that the other side is not behaving in a rational manner; this phenomenon has been termed by Kennefick as the theoreticians' regress (Collins,1985:84; Kennefick,2000:6,8,24-25,27-29; Pinch,1994:93,99). In essence, the theoreticians' regress "... expresses the near impossibility of discriminating between

the results of rival calculations which fail to agree, by the process of criticizing the calculations. Such a process of rebuttal is open-ended, because the number of ways in which a calculation can get the wrong result are practically inexhaustible" (Kennefick,2000:34).

While the notion of the regress may, upon first inspection, appear more appropriate to the investigation of a disputation occurring in a field in which mathematical modelling plays a critical role, as opposed to a realm of inquiry in which pen and paper analysis is predominant, the various factors cited above, and in an earlier section of the thesis, that contribute to communication and perceptual problems between two groups in a controversy (e.g. differential training, varying style and approach to a problem, personal experience in investigating topics in a given field), are similarly evident in traditional analytic work as the research of Pinch (1977), Pickering and Stephanides (1992) and Pickering (1995) have demonstrated.

Indeed, given the fact that Chandrasekhar and his opponents were unable to construct an argument capable of convincing one another of the validity of their respective views, this particular controversy represents a classic case of the theoreticians' regress; one side claims that the calculations prove the legitimacy of the limiting mass, while the other claims that such a conclusion is erroneous. And thus, a circular pattern of logic manifests itself which seems practically impossible to break.

Yet, what is curious about the Chandrasekhar-Eddington controversy is that *some* of the factors postulated to be necessary for the establishment of the theoreticians' regress are not evident in this particular dispute. Indeed, recall that Chandrasekhar's initial discovery and later refinement of his white dwarf model were done through the time-honoured practice of pen and paper analysis, a practice in which each and every step in the calculation can be traced and investigated thoroughly; in this respect, "replication" of Chandrasekhar's model hardly posed an insurmountable obstacle as it does in disputes involving computer simulations in which thousands if not millions of calculations are involved, thus making it extremely difficult for others to ascertain whether the result is valid or is the by-product of computational error.

Secondly, both Chandrasekhar and his opponents were explicitly socialized into the culture of astrophysics and thus, effectively shared the same disciplinary backgrounds as well as conceptual approach for tackling astrophysical problems. Indeed, in order to emphasize this fact one need only remember that Chandrasekhar's discovery of the limiting mass was almost wholly based on the established works of Fowler and Eddington, though involving a critical, but not very significant theoretical expansion of this particular body of research. Furthermore, it is important to emphasize that in all of Chandrasekhar's controversial papers on white dwarfs, he unequivocally states that the article in question is simply an extension of stellar models put forth by scientists such as Milne, Fowler or Eddington, as opposed to a body of research that employs a very different approach or methodology. As such, the strong disagreement that arose between Chandrasekhar and the astrophysical elite over the notion of the limiting mass cannot be strongly attributed to issues of highly divergent methods or intellectual training (Chandrasekhar,1930,1932,1935; Evans,1998; Gupta,1995; Horgan1994; Kennefick,2000:6,28; Nityananda,1995; Parker,1995; Penrose,1997).

However, while the methodology adopted by Chandrasekhar in his controversial research was not highly incongruent with the pre-existing conceptual strategies of astrophysicists in dealing with stellar phenomenon, his specific technical approach – style – to examining white dwarf evolution resulted in a conclusion that strongly challenged the personal experiences of both Milne and Eddington with respect to the calculations involved in explaining degenerate matter. Indeed, one could argue that these two scientists had built up an intuitive understanding of stellar dynamics after many years of intense research on the topic; this tacit knowledge undoubtedly gave Eddington and Milne great confidence that they had a complete understanding of how stars evolved and passed away, and provided a level of physical insight that enabled them to evaluate the merits of any new explanation relating to stellar structure, as was the case with Ralph Fowler's novel approach to deciphering the peculiar structural characteristics of white dwarfs (Kennefick,2000:25,28-29).

Indeed, recall that Ralph Fowler had employed Fermi-Dirac statistics in order to decipher the puzzling structural features of white dwarfs; his research revealed that under the extremely dense conditions found in such stars, the electrons would be compressed so closely one to another that any further compression would not be permitted by the exclusion principle; in light of this reality, the electrons comprising the white dwarf would give birth to a powerful repulsive force that prevented the star from total collapse. This solution appeared to neatly fit the observation data and pleased not only Fowler, but also Milne and Eddington who believed the analysis to be quite sound; in addition, Fowler's work seemed to reinforce the existing belief that white dwarfs constituted the only terminal stage for a star, regardless of its mass.

In contrast, as previously stated, Chandrasekhar examined the issue of white dwarf structure in a more detailed manner, investigating the issue from a slightly different perspective, stating that the exclusion principle would propel the electrons away from one another's presence with such force that they would reach speeds that would constitute a significant fraction of the speed of light; as such, it was necessary to incorporate special relativity into the analysis. And it was specifically this approach that acted as the primary catalyst for the dispute between Chandrasekhar and Eddington, as the elder scientist viewed the combination of relativity and Fermi-Dirac statistics as technically unsound in the context of investigating stellar phenomenon (Eddington,1935:195;1936:235-255; Evans,1998:139; Kennefick,2000:33-34; M/RAS,1935:37-39; Parker,1995:108;Pinch,1994:93,99). Indeed, as Eddington wrote in his written response to Chandrasekhar's work,

> I do not think that any flaw can be found in the usual mathematical derivation of the formula. But its physical foundation does not inspire confidence, since it is a combination of relativistic mechanics with non-relativistic quantum theory.
>
> In the present paper this unholy alliance is examined. The conclusion is reached ... that the "relativistic" formula is erroneous, and that the correct formula is $P_e = K\delta^{5/3}$ [Pressure is equal to a constant times the electron density]. That is to say, the corrections to the "ordinary" formula introduced by partially relativistic treatments are compensated in the fully relativistic theory (Eddington,1935:195).

Thus, Eddington viewed his junior colleague to have made a serious, technical error in his analysis, thus giving rise to an unsound conclusion. Eddington points out that if Chandrasekhar had combined relativity in an appropriate and full manner with the ideas inherent in the exclusion principle, the standard degeneracy formula born of Fowler's research would manifest itself once again (Douglas,1956:162; Eddington,1935:195,200).

Chandrasekhar rejected Eddington's interpretation of the relation between relativity and quantum mechanics, responding in a paper co-authored with Moller that he did not understand Eddington's comments in this respect, and that the senior scientist is not exactly clear with regards to the logic of his criticism of the relativistic degeneracy formula (Moller and Chandrasekhar,1935:156-157).

While it would be simple to take an asymmetrical stance in this case and argue that Eddington was simply behaving in an irrational manner, adopting an interpretation of quantum mechanics and relativity that was at odds with that held by theoretical physicists, if not most physicists in general, a more symmetrical analysis reveals that the cultural-cognitive environment in which Chandrasekhar and Eddington's disagreements over the technical derivation of the limiting mass were embedded, offers a more intricate explanation for this intellectual regress.

As previously stated, the way a theorist approaches a problem and the methodology that theorist employs in the performance of a calculation may differ sufficiently from orthodox conventions to make disagreements over the validity of the specific approach almost inevitable. Indeed, it was always Eddington's contention that Chandrasekhar's combination of relativity and the Fermi-Dirac statistics – a non-relativistic quantum model – was erroneous as these two models represented electrons as two distinct wave functions (progressive and standing) and their integration thus, gives rise to a technically unsound conclusion; in essence, Eddington argued that the exclusion principle conflicted with the principles of relativity (Eddington,1935a;1935b; M/RAS,1935). The reader will recall Eddington's closing words before the RAS, "My main point is that the Exclusion Principle presupposes analysis into standing waves, and this has been

wrongly combined with formulae which refer to progressive waves" (Eddington cited in M/RAS,1935:39).

Now, Chandrasekhar had received assurances from the principle architects of quantum mechanics that Eddington's comments in this matter were unjustified. Yet, despite this, both Eddington and Milne were not convinced that Chandrasekhar's calculation was valid.

Part of the problem lay with the fact that quantum mechanics itself was a relatively new way of conceptualizing the physical world, having been devised and refined by scientists such as Werner Heisenberg, Erwin Schrodinger, Wolfgang Pauli and Paul Dirac only in the mid-1920s, just a few years before Chandrasekhar's initial discovery of the limiting mass in 1930. Quantum mechanics views the world in a very different fashion than classical physics; whereas in classical mechanics, a definite value could be predicted for an observation, quantum theory only states the outcome of a result in probabilistic terms. Thus, quantum mechanics perceives reality in a non-deterministic fashion, a quality that offended Albert Einstein greatly, even though his own research gave birth to many of these concepts. In 1928, Dirac expanded the theoretical structure of quantum mechanics when he developed a version of the model that incorporated Einstein's special relativity into its framework, thus giving birth to a relativistic-quantum mechanical theory of nature. Despite the fact that some scientists like Einstein had metaphysical objections to the intrinsic structure of quantum physics, most other scientists embraced the theory since it was incredibly successful in explaining experimental data (Hawking,1988:55-56,67-68; Serway,1994:827,834,860-861,906).

While theoretical and experimental physicists were highly enthusiastic about quantum mechanics, many astrophysicists were still trying to assimilate the often-complex mathematical formulism of this new theory into the existing theoretical apparatuses employed in astrophysical research at the time. Furthermore, since quantum mechanics was a relatively novel approach to comprehending the physical world, the conceptual implications of its use in understanding various problems in astrophysics were still very much unknown to scientists in the field.

In light of the above remarks, it is not difficult to understand why Chandrasekhar and his opponents could not agree on the merits of his derivation of the limiting mass. Indeed, both Eddington and Milne were comfortable in Fowler's use of non-relativistic Fermi-Dirac statistics in deciphering the puzzling structure of white dwarfs, as it appeared to provide a concise, neat explanation to this specific problem. However, Chandrasekhar's specific approach had introduced relativity into Fowler's work, generating a startling result that contradicted orthodoxy's expectations with the whole topic of stellar evolution, one that immediately cast suspicion upon the technical methodology Chandrasekhar had employed – the union of relativity and non-relativistic quantum statistics. In this respect, given the quantum model's very unique theoretical structure, its status as a very new framework, questions concerning its generalizability in understanding large scale structures such as stellar objects, the inexperience of many astrophysicists with the theory, and in applying it in investigating astrophysical issues, constituted significant factors that generated reasonable doubts in the minds of scientists like Milne and Eddington concerning the legitimacy of Chandrasekhar's finding. Since there were a number of technically vital issues involved in Chandrasekhar's derivation of the limiting mass, issues that were far from being terribly clear, the above-cited variables provided countless opportunities for Chandrasekhar and his opponents to debate the validity of the young scholar's conceptual research. In addition, given the critical reality that there were no observational means – no independent criterion – by which the legitimacy of Chandrasekhar's or Eddington's views on stellar evolution could be assessed, it became virtually impossible to break the circular pattern of logic that had subsequently come to characterize this controversy (Collins,1985:84; DeVorkin,1989; Evans,1998:144-145; Kennefick,2000:6; Lankford,1997:74-75; Penrose,1997:62-63; Pinch,1994:93; Russell,1935:18-19;Wali,1991:132). Professor Wali comments on how the startling, unexpected nature of Chandrasekhar's findings, in conjunction with the relatively novel status of quantum mechanics contributed to the debate over the legitimacy of the limiting mass:

> ... R.H. Fowler had demonstrated unequivocally the relevance of quantum mechanics and Fermi-Dirac statistics to explain the behaviour of degenerate matter inside the White Dwarfs. Eddington had no quarrel with that. Special Relativity then became essential because of high densities and to satisfy FD [Fermi-Dirac] statistics, electrons had to acquire relativistic energies ... Chandra's derivation was straightforward enough, but it raised the unsettling question regarding the fate of massive stars. What happens to them? There was no easy experimental tests at the time. Recall that QM [quantum mechanics] was new, and its application in conjunction with relativity (although Dirac's theory of the electron had been set up) appeared too far fetched.

In addition, Professor Sundaresan also highlights the fact that the full implications of using quantum statistics – given its highly non-deterministic nature – and relativity in comprehending large scale structures such as stellar matter was not well understood at the time of the controversy, especially as it pertained to the evolution of celestial objections such as stars. As such, when confronted with a conclusion that confounded their expectations, Milne and Eddington had to struggle with the technical merits and implications of a calculation whose validity could not be assessed in an empirical fashion. As Dr. Sundaresan stated:

> ...see, the problem with quantum mechanics is that there is a lot of uncertainness about it; there is no continuous evolution of things, especially if you are dealing with something that is in the form of atoms, or in a very similar state. At the time, it was not clearly understood what kind of conceptual evolution quantum mechanics would bring. See, classically, systems evolve in a nice, natural way; everything is continuous. Now, quantum mechanically, what happens is there are discontinuous processes that occur, but there is a different theme by which evolution occurs; on the average, evolution is very, very continuous, but not for every state of the system. Now, that was not appreciated at the time very much ...

This physicist went on to explain that:

> The Fermi-Dirac distribution was derived from applying quantum mechanical principles to large scale systems, where you have a large number of particles; and, when you do that, you get this distribution, and that distribution has in it energy and momentum quantities pertaining to a particle, and in these energy and momentum relationships, you'll get the Boseman equation, Boseman distribution. But, if you went beyond it and you explicitly said that the energy-momentum relationship is not one of this sort because they have very high velocities, then, you get completely different predictions from this new distribution; they're insignificantly different in the region where non-relativistic energy-momentum relationships hold, but when the energy-momentum relationship becomes a relativistic one, then, there is a completely different result from what you expect based on classical statistics....

Thus, when Chandrasekhar attempted to combine relativity with non-relativistic Fermi-Dirac statistic, he constructed a theoretical apparatus – composed of a set of theoretical tools and procedures – whose structure, while differing only subtly from the apparatus employed by Fowler, nevertheless generated a conclusion that proved very difficult for astrophysicists to assimilate into their background knowledge of stellar evolution; this fact alone served to polarize individuals on both sides of the debate, permitting the manifestation of an intellectual environment that made resolution over the outcome of the calculation of the limiting mass extremely difficult. Indeed, while Chandrasekhar, too, had been surprised by his startling conclusion, he nevertheless felt his model was fairly sound on technical grounds, and thus strongly believed his unorthodox result held merit (Chandrasekhar,1969:582; Kennefick,2000:6). As Chandrasekhar wrote, many decades after his dispute with Eddington had ended,

> ... I was puzzled by the emergence of the critical mass when I first obtained it. But by October of that year [1930] it was clear that what was happening was that the relation $R \propto M^{-1/3}$ [a star's radius is inversely proportional to the cube root of its mass] given by the nonrelativistic theory was modified by the inclusion of the relativistic effects....(Chandrasekhar,1969:582).

In contrast, as his dispute with Chandrasekhar continued, Eddington expanded his critique of his junior colleague's research, believing that the validity of the Fermi-Dirac statistic, while having been substantiated in the confines of strictly controlled experimental trials, its extrapolation to comprehending the behaviour of an enormous quantity of particles as found in stellar matter was problematic as its validity under such large scale conditions was unknown (Eddington,1935b:20). As he wrote, responding to a paper written by Moller and Chandrasekhar (1935) in which the two scientists tried to defend the legitimacy of the relativistic degeneracy formula through an alternative mathematical proof which employed relativistic quantum mechanics,

> ... The Exclusion Principle was first enunciated for electron orbits or states in an atom; for applications of this type it has been abundantly clarified. Undoubtedly there exists a generalization of it applicable to the statistics of large assemblies of particles. But the generalization cannot be of the form assumed by Moller and

> Chandrasekhar, which conflicts with the Uncertainty Principle....(Eddington,1935b:20).

In this respect, given the differing theoretical apparatuses and conceptual rationale both scientists adhered to and firmly believed was the correct one, direct comparison of Fowler's Fermi-Dirac model of white dwarfs and Chandrasekhar's relativistic degeneracy model proved highly problematic, as were attempts by both sides to effectively communicate their respective perception of the problem to each other.

As the above exposition reveals, when faced with a disputed result whose validity cannot be checked through empirical means, theorists are forced to search for alternative avenues by which to judge the reliability of a controversial calculation. One path, as we have seen, is to be sceptical of the technical merits of an answer, especially if it predicts the existence of a physical phenomenon that strongly contradicts conventional wisdom. A secondary path scientists can take is to assess the reputation and credibility of the scholar who is the author of the controversial result (Collins,1976,1985; Kennefick,2000:6,27; Pinch,1994:96-97). As Pinch states, "The struggle between proponents and critics in a scientific controversy is always a struggle for credibility. When scientists make claims that are literally "incredible,"...they face an uphill struggle"(Pinch,1994:96).

In this case, Chandrasekhar had to overcome the social reality that he was a very young scholar who had yet to solidly establish his mark in the field of astrophysics; in addition, while Chandrasekhar could lay claim to possessing a certain degree of expertise in the field of stellar structure, having published ten articles and defended a doctoral dissertation on the topic at the time of the memorable 1935 RAS meeting, he could not do the same with respect to his knowledge of relativity and quantum mechanics, and it was precisely in this area that the challenge to Chandrasekhar had been most severe (Pinch,1994:97).

In contrast, Chandrasekhar's main opponent, Arthur Eddington, represented one of the most prominent scientists of the time, having played a critical role in the development of modern

theoretical astrophysics. His contributions to the field of stellar evolution and structure were required reading for anyone hoping to pursue a career in astrophysical research. In time, Eddington's considerable stature in physics grew even larger as he became intricately involved with Einstein's special and general theories of relativity. An enthusiastic supporter of Einstein's revolutionary research from the start, he wrote what would be the first ever analysis of the general relativity theory to be produced in the English language, and which was subsequently published in 1918. One year later, Eddington became a principal participant in an attempt to test a specific prediction of general relativity, one that hypothesized that light passing near an intense gravitational field such as that generated by the sun, would be bent as it entered the star's gravity well. The observational evidence Eddington gathered during the course of this particular research endeavour provided the first empirical verification of the validity of Einstein's comprehensive theory of gravity. This result solidified Eddington's already strong belief in the model, and convinced him that the relativistic model of nature constituted a path toward which scientists would achieve a true comprehension of the structure of the physical world.

In the years following his successful verification of the general theory, Eddington proceeded to publish a number of articles and books on the topic. Indeed, from 1920 to 1923, he generated no less than nine articles and two books on the special and general theories of relativity. What was interesting about Eddington's writing on the relativistic framework was that his exposition of the theory was so clear, vivid and imaginative, written in such an engaging style that he soon became in great demand to present Einstein's work to scientists and non-scientists alike. Indeed, Albert Einstein himself considered Eddington's presentation of his theories among the finest ever to be set in print (Douglas,1956:39-42,193,195-196; Evans,1998; Russell,1928:83; 1945:134).

In light of Eddington's incredible intellectual authority, expertise and track record in not only stellar dynamics, but relativity as well, his judgment that Chandrasekhar's combination of relativity and the quantum model was unsound was one other astrophysicists could not easily ignore; indeed, William McCrea's comment to Wali that he felt content with Eddington's

explanation of why he did not accept Chandrasekhar's controversial discovery since they seemed to please the prominent scholar, indicates the influence Eddington possessed within astrophysics (Evans,1998:143; Wai,1991:134). As Professor Sundaresan explains, commenting on the dilemma Chandrasekhar faced in getting colleagues to take his research seriously,

> ... See the main problem was that the bizarre consequences could not be – there was no experimental way to verify that it was true or not, at that point ... And it was purely a theoretical concept ... So, in this situation, if Eddington says, no, that's not a thing that can exist [the limiting mass], and somebody says, yes, it does, what do you do? How do you take sides? ...Eddington's reputation was so high, and Chandrasekhar was basically unknown in this area, therefore, there was the possibility that if anybody was in the right and anybody was in the wrong, the chances were Eddington would be right and Chandrasekhar would be wrong....

Yet, it was for the specific reason of countering Eddington's enormous prestige that Chandrasekhar had solicited the support of theoretical physicists whose – as Trevor Pinch would state – social capital rivalled that of the famous astrophysicist. As indicated earlier, however, while the giants in theoretical physics concurred with the technical validity of Chandrasekhar's calculation, they refused to enter the debate in a public fashion, partly because of the difficulties in empirically verifying an abstract notion such as the limiting mass which might not exist in any physical sense despite what a mathematical proof might claim, but mainly because theoreticians had very little respect for astrophysics (Evans,1998:144;Pinch,1977:178-180; Wali,1991:132). In Professor Wali's words:

> As I have explained in the book [Chandrasekhar's biography], great physicists of the time were not interested in astrophysical problems. They thought the subject was too clumsy. Great discoveries were taking place in physics. Also, they did not have great respect for Eddington. Still, Peierls and Moller did write articles arguing against Eddington's objections.

Yet, despite the public show of support Peierls and Moller gave Chandrasekhar, this did very little to diminish the influence of Eddington's critique; despite the fact that both these scientists were relatively important figures in theoretical physics, they were still intellectual outsiders whose lack of experience and credibility in dealing with astrophysical issues probably did little to

help Chandrasekhar's case, even though their debate with Eddington was on purely technical grounds which focused exclusively on the physics involved in the derivation of the limiting mass (Evans,1998:144-145). However, it is difficult to believe that Eddington's critique of Chandrasekhar's work would not have lost some of its shine had the giants in theoretical physics decided to enter the controversy. While the participation of these famous scientists might not have swayed Eddington's opinion on the matter, it might have influenced other astrophysicists and theoreticians to get involved in the debate, thus widening the social and academic parameters of the dispute. In the words of Professor Sundaresan, when asked if the intervention of elite scientists such as Bohr, Dirac and Pauli might have made a significant difference in the dynamics of this controversy:

> ... [it] might have made a difference in the sense that Eddington would have to take – the controversy might have become wider rather than with Chandra. It's much like the Einstein-Bohr controversy on quantum mechanics, where, because you have two giants who were in controversy, then there were other people who got involved, and a much wider circle could discuss who was right and who was wrong. In this situation, they were not two equals at the time unlike Einstein and Bohr who were well established figures. It would have made a difference if some of these people [Bohr, Dirac, Pauli] had said that on the basis of physics – on the basis of quantum mechanics, on the basis of relativity, Chandra's research is completely right; that would have provided some solace to Chandra at that point. He knew in his heart of hearts that he was right, but he could not get other people to publicly say he was right.

Yet, even the intercession of elite theoretical physicists would not have resolved the theoreticians' regress; if anything, their entrance into the controversy might have intensified the dispute even more. Indeed, given the reality that Eddington – and Milne – had acquired a strong intuitive and stylistic comprehension of the behaviour of stellar matter based on years of amassed tacit knowledge, it would have been extremely difficult for any argument posted by intellectual outsiders who did not possess the sharply refined intuition of someone of Eddington's stature regarding astrophysical phenomena, to have influenced Eddington or Milne's views on the notion of the limiting mass in the slightest (Evans,1998:143-145; Kennefick,2000:30).

As we have seen, a theorist's style – how s/he approaches a problem, the methods employed when analyzing it – can contribute to serious disagreements over the validity of a new discovery, especially if the methodology used generates a result that forcefully contradicts orthodox expectations. Issues of style, however, may further hamper the communication of a novel result if the logic of a particular mathematical proof that gave rise to a controversial theoretical finding is encapsulated in a format that is so intricate and detailed that one cannot clearly follow or assimilate the arguments being made (Kennefick,2000:27-29).

Indeed, in Chandrasekhar's case, his initial derivation of the limiting mass which was published in 1931, was relatively simple due to the fact that he had used a number of approximations and assumptions to facilitate his analysis, and thus amounted to only two short pages. In contrast, the derivation of his exact theory of white dwarfs which he first presented before the RAS in 1935, consisted of nineteen pages of many complex mathematical operations, detailed numerical calculations and intricately drawn graphs, all of which must have overwhelmed many astronomers whose experience with the equations and calculations governing stellar mechanics at the time did not fully prepare them to comprehend the novel, relativistic-quantum model of stellar evolution Chandrasekhar was presenting; this reality was compounded by the fact that many astronomers of the time were not specialists in theoretical matters in the same sense as Eddington, and so, were not in a position to fully digest what Chandrasekhar was attempting to explain (Chandrasekhar,1931;1935; DeVorkin,1989; Evans,1998:145; Lankford,1997). As David Evans, an astrophysicist who had attended Eddington's lectures at the undergraduate and graduate levels at Cambridge, and had later taught at the University, explains that many astrophysicists had a hard time coming to grips with the young scientist's derivation of his white dwarf theory:

> Things were not helped by Chandra's expository style, which included diagrams difficult to interpret, and the use of units, the result of so many algebraic transformations that readers found it difficult to put them in relation to more familiar ones (Evans,1998:145).

Indeed, faced with such circumstances, any meaningful dialogue is very problematic, the logistics involved in winning an opponent's favour given an enormous perceptual and technical gap so great that further attempts at communication may do seriously more harm than good.

As we have seen, issues of style, intuition and tacit knowledge can play a critical role in preventing two theorists from resolving a disagreement over the nature of a controversial, conceptual discovery. However, another variable that can play a significant function in generating and perpetuating the theoreticians' regress, is one that is based on metaphysical principles, as opposed to the purely technical and conceptual.

While metaphysical matters do not often play an influential role in scientific controversies, sociologists and philosophers of science have investigated disputations in which this concept has constituted a not insignificant factor (e.g. Klein,1964; Pinch,1977; Wynne,1976). In these instances, firm adherence by the proponents or critics of a controversial theoretical perspective to a given world view made constructive communication between the respective camps virtually impossible.

Indeed, a theorist's metaphysical beliefs can strongly influence his or her judgment of the validity of a specific, controversial theoretical discovery. In this respect, if the particular finding in question predicts the existence of a physical phenomenon whose nature forcefully contradicts the personal convictions of a given scientist, then arguments made in favour of the phenomenon may have virtually no impact on a theorist who firmly believes the conceptual result to be wrong on metaphysical grounds.

As we have seen, both Edward Milne and Arthur Eddington had their own strong beliefs concerning the structure of stellar matter, beliefs that had embroiled them in their own private version of the theoreticians' regress. As the reader will recall, in the case of Milne, he was most adamant that the outer layers of a star were crucial in understanding its internal composition; thus, from this perspective, one could not learn anything important of a star's interior without first understanding its exterior characteristics. This particular conceptual stance led Milne to

conceptualize all stars as entities that were composed of degenerate cores enveloped in layers of gaseous material that obeyed the perfect gas law. In contrast, Eddington believed these theoretical views to be nonsensical, arguing that the conditions existing in the interior of a star constituted the critical factors in comprehending stellar dynamics. Yet, despite their disagreements, both scientists firmly believed that the white dwarf stage represented the only terminal stage in the course of a star's evolution as it passed from youth to old age. Given this epistemological position, it is not surprising that Eddington and Milne reacted so critically to Chandrasekhar's unorthodox conclusion that only stars of a given mass could progress into the white dwarf phase (Evans,1998:133; McCrea,1951:166-167; Milne,1929;1930a;1930b; Wali,1991:119-120). As Evans writes, commenting specifically on Eddington's entrenched beliefs concerning stellar evolution, and his dismay of Chandrasekhar's discovery,

> Now, to upset the almost fifty-year-old doyen of stellar physics comes this young man in his early twenties to destroy his senior's cherished preconceptions. Eddington was essentially an inspirational thinker, who could often see his goal with certainty before he knew how to reach it. Eddington knew in his bones how things ought to be, and could not abide the particular part of the younger man's work which told him differently....(Evans,1998:143).

Professor Wali, too, brought up the issue of the role personal convictions played in this scientific dispute, saying:

> Milne had his own ideas. Chandra showed that Milne was wrong in assuming that every star had a degenerate core. Milne was more interested in proving Eddington wrong ... Eddington had his own pet ideas about nature and how it should behave....

Thus, a deep metaphysical commitment to a specific world view can seriously hamper the ability of two groups of theorists in establishing a constructive dialogue to try and resolve serious disagreements over the outcome and implications of a given disputed calculation. Indeed, perhaps, Milne best summed up how important a role beliefs played in this specific controversy when he told Chandrasekhar, "...To me it is clear that matter cannot behave as you predict...."(Milne cited in Chandrasekhar,1969:584).

While Milne's was convinced by his own confidence in his beliefs on stellar structure that Chandrasekhar's notion of the limiting mass had to be erroneous, the metaphysical basis for Eddington's reluctance to accept the controversial conclusions of his junior colleague, though very similar to Milne's, was in truth slightly more complicated in nature; in fact, one can argue that the distinguished scientist's rejection of Chandrasekhar's work was, essentially, intertwined in a very complicated philosophical perspective on the nature of the physical world. In order to address this issue, a brief discussion of Eddington's epistemological perspective is required at this time.

In approximately 1919 – the year he helped verify the empirical validity of the general theory of relativity – Eddington became preoccupied with understanding the intricate structure of reality. He wondered if there existed a mysterious link connecting cosmic and atomic phenomena, between general relativity and quantum theory, and pondered whether the constants of physics constituted arbitrary empirical quantities, or were somehow correlated with one another in a cabalistic fashion? And so, over the course of two decades, Eddington commenced to develop what he deemed a fundamental theory of nature, a mathematically and philosophically complex conceptual model whose main goal was to address the questions cited above. This work was of such immense scope that it inspired two related works that were published as monographs, specifically *Relativity Theory of Protons and Electrons (1936)* and *The Philosophy of Physical Science (1939)*.

What is important to note is that in the former monograph, Eddington presented his own metaphysically inspired model of atomic phenomena in which he deduced the exact number of protons and electrons in the universe as being 1.29×10^{79}, the ratio of the mass of the proton to the mass of the electron, and the fine-structured constant – a value generated by the product of Planck's constant and the velocity of light, divided by the charge on the electron[28]; in addition, he

[28] Interestingly enough, while many of Eddington's deduced values differed from experimental values by only modest amounts, the general consensus was that the prominent scientist appeared to be manipulating

discussed his specific views concerning the union of relativity and quantum mechanics. As he wrote,

> I have sought a harmonisation, rather than a unification, of relativity and quantum theory. I do not set out to obtain an all-embracing formula, but the investigation shows in detail how to combine the conceptions of the two theories in the solution of specific problems, which would be outside the range of either theory separately (Eddington,1936:v).

Indeed, in this respect he gave yet another detailed account of how he believed relativity theory and the Fermi-Dirac quantum statistic should be integrated in the investigation of the structure of white-dwarfs; this more accurate application of the two models neatly reproduced Fowler's original conclusion that all stars, irrespective of mass, would pass into oblivion as white dwarfs (Douglas,1956:161-162; Eddington,1936:235-255; Evans,1998:150-152). In his Harvard Lecture, he discussed how this particular work upset a number of colleagues such as Chandrasekhar, saying,

> I was not surprised to find that in announcing these conclusions I had put my foot in a hornet's nest; and I have had the physicists buzzing about my ears – but I don't think I have been stung yet. Anyhow, for the purposes of this lecture, I will assume that I haven't dropped a brick (Eddington,1937:138-139).

In many respects, Eddington saw his exploration of the physical world to be of profound importance, one that revealed to him the aesthetic and elegant structure of the universe. As he wrote, "We may look on the universe as a symphony played on seven primitive constants as music is played on the seven notes of a scale" (Eddington,1936:v)[29].

In later writings, he expounded upon this view, arguing: "... I think that progress of the epistemological method has assured us that the constants of nature (apart from our arbitrary units)

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his deductions in a fashion that resonated with empirical results (Douglas,1956:151,164-165; Evans,1998:151-155).

[29] These constants were the mass of an electron, the mass of a proton, Planck's constant, the electric charge of an electron, the velocity of light, the gravitational constant, G, and the cosmological constant (Eddington,1936:v; Douglas,1956:163).

are numbers introduced by our subjective outlook, whose values can be calculated a priori and stand for all time...."(Eddington,1967 [1939]:78).

Yet, many physicists did not accept Eddington's philosophically inspired views of reality, often commenting on its perplexing and bewildering logic (Douglas,1956:175-179; Evans,1998:149-152; Lankford,1997:200-201). As David Evans states, highlighting scientist's bafflement with the senior scholar's epistemological writings,

> As a newly-minted bachelor of arts, I attended Eddington's lectures on what became the topic of his book, *The Relativity Theory of Protons and Electrons*, and though impressed by the dexterity and beauty of his E-number algebra, remained baffled as to the object of the whole business. As a geriatric retiree, I am equally baffled by the string of eleven Royal Society papers, and two others published elsewhere, listed in Eddington's bibliography. I am not baffled by the intention of these mathematical fireworks and densely imagined arguments. That is perfectly clear. The intention was to refine, bolster, and seek acceptance for, the original inspiration. I am convinced that, like myself, nobody was sure what Eddington was trying to get at in the first place (Evans,1998:151).

Yet, despite the lack of support this line of investigation received from his colleagues, Eddington persevered with constructing his fundamental model of the physical world, though communal scepticism of his research did greatly annoy him, as can be seen in the following letter he wrote to a colleague:

> I am continually trying to find out why people find the procedure obscure. But I would point out that even Einstein was considered obscure, and hundreds of people have thought it necessary to explain him. I cannot seriously believe that I ever attain the obscurity that Dirac does. But in the case of Einstein and Dirac people have thought it worthwhile to penetrate the obscurity. I believe they will understand me all right when they realize they have got to do so – and when it becomes the fashion 'to explain Eddington'(Eddington cited in Crowther,1952:194).

In essence, Eddington was convinced that his expansive and difficult research into the structure of reality would be in demand at some point in time, in much the same way as Einstein's work. However, many physicists of the period expressed a strong suspicion of a purely deductive theory founded upon philosophical principles as opposed to physical hypotheses, a theory that provided no original predictions concerning the natural world, but mainly gave an account of

phenomena already studied and explored in great depth by scientists. As physicist Edmund T. Whittaker wrote of the *Relativity Theory of Protons and Electrons*,

> No one who studies it can doubt that its author is a man of genius; but whether it produces conviction is another matter... In judging the value of a speculative theory, one enquires how far it has helped on the actual development of knowledge, by predicting or suggesting or interpreting the results subsequently obtained by others (Whittaker,1937:15).

Thus, while criticism of his metaphysical approach to comprehending reality continued to mount, Eddington persevered with his research. However, as fate would have it, Eddington never did finish his fundamental theory, his health deteriorating to the point that further work on the topic was no longer possible. Two years after his death in 1944, Edmund Whittaker published what work Eddington had completed on his cherished model in a monograph entitled *Fundamental Theory* (1956 [1944]). This specific work contained previously published material as well as a great deal of new research into the structure of the physical world and represents an incredibly complicated exposition on a wide spectrum of topics ranging from quantum mechanics to gravitation. Suffice it to say, many of Eddington's colleagues found this latest monograph from their distinguished associate to be immensely difficult to comprehend; furthermore, those who were able to follow Eddington's arguments found his logic at times to be convoluted (Bondi,1952:157; Crowther,1952:195; Evans,1998:162; Kilmister & Tupper,1962; Wilson,1945). As physicist A.H. Wilson wrote,

> Eddington's work, if correct, is extremely important, but most of those who have tried to read his work have not been able to agree with his conclusions. His papers are very clear up to a point and then at the critical moment they become obscure, to become clear again after the important results have been deduced. There is certainly no logical deduction of the conclusions from explicitly stated axioms and hypotheses, and Eddington himself was aware of this. Once after a long discussion with the present writer, which achieved very little, Eddington said: "I can't quite see through the proof, but I am sure the result is correct"(Wilson,1945:171).

In similar fashion, the theoretical physicist, J.C. Crowther stated,

> Eddington's "unified theory," apart from its obscurity, explains too much; indeed it explains everything, and hitherto such theories have generally been found ultimately to explain nothing. At best, it is a fragmentary work, which contains flashes of insight that will be appreciated by future generations, like Leonardo da

> Vinci's scientific researches, probably after what is significant in them has been more completely discovered by very different paths and methods (Crowther,1952:195).

While it would appear the scientific community rejected much of Eddington's research into uncovering the intricacies of physical phenomena, the distinguished scientist firmly believed in the validity of his analysis, one that came to influence Eddington's whole approach to understanding nature. As his biographer, fellow physicist, Alice Douglas, wrote,

> In evolving his Fundamental Theory from his epistemological premise, Eddington believed that it should be possible to deduce the various and manifold relations by pure reason. His motto might well have been *Quocumque lux ducit.* As he progressed, one detail after another encouraged him, but it was the grand sweep of the general panorama that fascinated him (Douglas,1956:177).

In light of these comments, it is fair to argue that Eddington's rejection of Chandrasekhar's work was not solely based on the fact that the concept of the limiting mass was incongruent with the knowledge base of astrophysics of the 1930s, but also because Chandrasekhar's unification of relativity and quantum mechanics contradicted Eddington's metaphysical beliefs of how the two models should be integrated – or harmonized – for the purposes of investigating natural phenomena, beliefs that were themselves grounded in a much wider philosophical worldview which he very strongly felt was correct (Eddington,1936:v; Evans,1998:144; Penrose,1997:63). Indeed, as Eddington once wrote, commenting on the important role personal beliefs often play in the investigation of scientific problems "In science we sometimes have convictions as to the right solution of a problem which we cherish but cannot justify; we are influenced by some innate sense of the fitness of things...."(Eddington,1930:337).

Indeed, in this respect, Chandrasekhar wrote that Eddington once informed him that his objection to the young scientist's work was ultimately based upon a holistic understanding of the structure of the natural world; as Eddington told Chandrasekhar, "... You look at it from the point of view of the star; I look at it from the point of view of nature"(Eddington cited in Chandrasekhar,1987:140).

Thus, it would appear, by his comments to Chandrasekhar that Eddington was informing his junior colleague that his critique of the limiting mass was founded upon a rationale that transcended the mere question of stellar structure and evolution, one that was located in a much more panoramic conceptual outlook whose goal was to ultimately decipher the composition of reality itself. While Chandrasekhar states he found Eddington's comment on this matter to be bewildering, he should have realized what the senior scientist was alluding to, and no doubt he did. Indeed, Chandrasekhar was more than aware of Eddington's epistemological work, and not surprisingly, he viewed this particular line of analysis as deeply flawed. At one point in 1937, Chandrasekhar informs his biographer that he had asked Eddington to what extent his views on relativistic degeneracy were informed by his research into the theory of protons and electrons. According to Chandrasekhar, Eddington allegedly responded, "Absolutely fundamental ... If my ideas on relativistic degeneracy are wrong, my entire theory of electrons and protons is wrong"(Wali,1991:142).

Thus, given the strong stance taken by both these theorists with respect to stellar evolution, a stance born ultimately of stylistic, intuitive and metaphysical considerations – more so for Eddington than Chandrasekhar – it would seem that any attempt by the two scientists to convince one another of the validity of their views was doomed to failure from the start, the communication gap between the upholders of each perspective too great to be bridged through compromise. As such, it comes as no surprise that Chandrasekhar never did persuade Eddington – or Milne – of the legitimacy of the one, critical conclusion of his general, white dwarf model, namely that only stars of a certain mass could ever evolve into white dwarfs (Evans,1998:145; Wali,1991:138).

Yet, one of the essential predictions of the theoreticians' regress is that this kind of controversy *can be settled* if and only if members on either side of the dispute open a constructive dialogue with one another, a series of conversations in which the principal participants attempt to reach an agreement on the proper technical apparatus to employ in the solution of a specific

problem. In the absence of such a collaboration, the theoreticians' regress will perpetuate itself for years if not decades, until one viewpoint simply fades from the academic landscape (Kennefick,2000:30-31).

Yet, I argue, in light of the preceding comments, that it is a rare occurrence during the tenure of a heated debate over a disputed result for scientists located on both sides of the issue to come together and to adopt an intellectual viewpoint and language that will permit a orderly resolution to the controversy; indeed, if anything, the history of science shows us that such diplomatic and pragmatic behaviour is few and far between (Kennefick,2000:31;Pinch,1994:99). As such, I propose an alternative picture by which the theoreticians' regress is broken: according to this particular conceptual perspective, another means by which the theoreticians' regress can be effectively broken is when one party involved in a dispute, because of this scientific structure, is able to successfully construct the conceptual conclusions of its opponent as being so implausible that it convinces a majority of members in the field touched by the controversy, and eventually forces those championing the alternative perspective to completely withdraw from the debate. Indeed, the empirical evidence in the Chandrasekhar-Eddington controversy appears to suggest that this is exactly what happened in this particular case, and thus, it is to the whole issue of the construction of scientific plausibility that I turn to next.

6 The Construction of Plausibility

In the previous chapter, I have argued that the inability of Chandrasekhar and his opponents to convince each other of the legitimacy of their respective arguments represents a classic example of the theoreticians' regress, a socio-cognitive phenomenon in which the validity of two competing theoretical calculations cannot be decided by simply criticizing the calculation. Such a process is unable to provide a neutral, objective path whereby the controversy can be concisely settled, because the participants on both sides of the debate have dissimilar experiences, styles and metaphysical convictions with respect to the physical problem or calculation in question (Kennefick,2000:33-34; Pinch,1994:96).

While the notion of the regress provides a parsimonious explanation as to why Chandrasekhar, Milne and Eddington were unable to establish a meaningful dialogue in order to settle their strong disagreements with each other's research concerning the evolutionary fate of white dwarfs, it does not fully explain why Chandrasekhar was so unsuccessful in convincing other astrophysicists that the concept of the limiting mass was a valid one. Thus, in order to provide a more holistic conceptual accounting of Chandrasekhar's failure in this respect, it is necessary to discuss how the concept of plausibility played a significant role in the context of this specific controversy.

From a relativistic perspective, to proclaim that a given discovery in science is perceived to be plausible implies that it is, at some cognitive level, also accepted as representing the true state of affairs of the physical phenomenon under scrutiny; the conceptual advantage of this approach is that an idea need not prove its validity through overwhelming empirical support, but only convince members of the scientific community that it is more plausible than any alternative viewpoint by drawing upon various socio-cultural and technical resources.

Indeed, one critical factor in the construction of plausibility rests with the actions of scientists themselves, in how they react to a given finding, in the interpretations of the empirical or theoretical result they wish to pursue and the judgments they chose to pass. The choices scientists

make when faced with a discovery is, however, by no means random, but is significantly influenced by the culture[30] in which they were socialized and trained. In this respect, the term culture refers to the vast reservoir of intellectual resources scientists draw upon in the course of their research; this reservoir not only moulds and shapes a scientist's perception of the physical world, but it also sets clear conceptual boundaries upon the type of explanations that can be invoked to account for a given phenomenon; without such limits any result or finding may be assimilated into the knowledge base of science.

As such, which findings are seen as plausible depends on whether or not that finding has qualities and characteristics that resonates with the socially accepted conceptualizations of the natural world that permeate a given scientific discipline; if a specific result does not possess these merits, then in all probability it will be rejected as implausible. In such an instance, when a community of scholars abandons a particular conclusion wrought by one of its own, this is in itself a significant social act, for it constitutes a powerful proclamation that the membership has strong faith in the fundamental concepts and principles of their field, i.e., in the prior social agreements upon which these variables were founded (Collins,1975:217; 1985:135-136,150; Harvey,1981:105-106,124; Pickering,1981:89;1992:2-3).

The conceptual utility of the above statements are perhaps most evident when examining a disputed experimental or theoretical result. Indeed, under such circumstances the authors of the controversial finding are forced to construct arguments that attempt to demonstrate the plausibility of their discovery to their peers; the purpose of these arguments is to try and persuade colleagues that the conclusion reached resounds with some actual physical process and is not an artifact of poor methodological design or technical error. As the previous section has demonstrated, however, in order for such arguments to be effective, all parties involved in a dispute must be able to speak a common language and possess similar experiences of a given problem, otherwise constructive dialogue becomes virtually impossible.

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[30] Please refer to section 2.3 for a definition of this term.

In the face of a communications gap, scientists must deal with an unorthodox finding by drawing upon their knowledge and familiarity with a specific topic – that is, their culture. In doing so, scientists will inevitably compare the technical and conceptual structure of the novel finding against prior agreements concerning the theoretical composition and nature of the phenomenon in dispute. If the controversial result is found to be highly incongruous with this background knowledge, then it is likely that the notion of plausibility will be distributed in such a fashion as to construct this particular conclusion as being highly improbable.

It should be emphasized, however, that the mechanisms by which a knowledge claim is perceived as implausible is by no means distinctly different from how scientific findings are judged to be plausible; in both instances, decisions are made and intellectual paths are chosen that determine how a specific discovery will be effectively portrayed to others in the scientific community. In this respect, the allocation of plausibility is, ultimately, a function of social action. As such, one can argue that the side which emerges as the victor in a scientific dispute, does so, not necessarily because it was able to show overwhelming evidence that its conceptualizations of nature were empirically sound, but because it was able to successfully monopolize the notion of plausibility in its favour (Collins,1985:135-136; Harvey,1981:96,106,124-125; Kennefick,2000; Pickering,1981:83,88-89; Simon,1999:79). This social process is succinctly expressed by Bart Simon when he wrote, "... Mainstream scientific culture produces truth [or arguably, plausibility] not only because some knowledge claims are made into facts through the actions and associations of certain people and things, but also because in the same motion some other knowledge claims are made into artifacts or fictions"(Simon,1999:79).

With respect to the Chandrasekhar-Eddington controversy, the entire debate had taken place against a cultural background in which the only known terminal stage for a fading star was that of a white dwarf. This statement, in effect, highlights the historical context in which this particular dispute emerged. Indeed, observational evidence accumulated in the 1920s, as well as theoretical knowledge of stellar structure, indicated that white dwarfs were very small, incredible dense stars

whose once enormous physical dimensions had contracted substantially due to the depletion of nuclear fuel. All the observational surveys catalogued up to this point in time in the astrophysical literature, failed to reveal any alternative star-like objects whose qualities were so divergent from the white dwarf that a new conceptual category would need to be created. As such, academic consensus slowly grew that white dwarfs represented the final stage of a star's life-history, a view that solidified once Fowler deciphered the peculiar structural characteristics of this class of star[31] (DeVorkin,1989; Evans,1998; Lankford,1997; Wali,1991).

As the reader will recall, in reviewing observational data produced in the mid-1920s that appeared to confirm that white dwarfs possessed incredible densities, Eddington became troubled by what would ultimately happen to these peculiar objects. The phenomenal densities found in such stars was only possible if the atoms composing the star had been completely ionized; but, the only way this was possible was in the face of enormously high temperatures; however, white dwarfs, having exhausted much of their nuclear fuel, do not emit much energy, what energy they do emit radiating away into space, presumably forcing the star to become smaller and smaller. Yet, Eddington reasoned that with the passage of time, as the white dwarf depleted its remaining sub-atomic fuel, it would cool down, but this would require a tremendous increase in the volume of the star, requiring an immense expenditure of energy to counteract the crushing force of gravity, energy the star did not have. This scenario led Eddington to make his paradoxical statement that such an entity was continuously losing heat but did not have enough energy to grow cold.

Soon thereafter, Fowler seemed to have solved the cabalistic physical nature of white dwarfs in elegant fashion by employing the recently designed Fermi-Dirac statistics. Eddington interpreted Fowler's research as suggesting that either a white dwarf would one day expand to a normal configuration, or that it would exist as a white dwarf until the end of its days. In any

[31] The point is, had Chandrasekhar made his surprising prediction that star's had a limiting mass in the context of modern astrophysical research with its sophisticated technical and observational equipment, scientists might not have been so quick to reject his result as implausible.

event, Fowler's research provided a highly satisfactory and wholly plausible conceptual model which seemed to concisely account for the observational data accumulated on white dwarfs. As Eddington commented, "Fowler showed that the newly discovered Fermi-Dirac statistics saved the star from the unfortunate fate which I had feared" (Eddington,1937:137).

In this respect, one can argue that Fowler's use of non-classical statistics resulted in an *exemplary* solution to the puzzling question of white dwarf structure, one which served to guide future practice in investigating issues in theoretical stellar mechanics, a model which found strong support among esteemed theorists like Eddington and Milne. As such, Fowler's white dwarf model soon became identified with the actual, physical evolution of stars, as opposed to being exclusively attached to its author; in effect, this knowledge became a socially acceptable fact that soon became entrenched with the culture of astrophysics, leaving scientists in the field with a clear social constraint in how they could conceptualize the fate of stellar matter, one that allowed virtually no latitude for alternative interpretations.

While it is true that both Anderson and Stoner had published papers suggesting that stars did in fact have a limiting mass, these works were not accorded a great deal of attention by elite scientists such as Eddington and Milne, perhaps, because they did not make any explicit statements, or went into as much detail as Chandrasekhar's work, concerning the fate of stars whose masses exceeded the limiting mass. As such, in the wake of Fowler's work, matters concerning the evolutionary fate of the stars seemed permanently settled, and astronomers moved onto other avenues of inquiry (Eddington,1937:137-138; M/RAS,1935:37-39; Evans,1998:138-139; Pickering,1981a:128-131;1981b:73).

However, Chandrasekhar's independent discovery of the limiting mass, and his exact theory of white dwarf stars, strongly suggested that only stars under 1.44 solar masses could ever hope to retire as white dwarfs; those stars whose masses were greater than this limiting mass would journey down a decidedly different evolutionary path, their fates resting in the hands of gravity and the perfect gas law. This conclusion implied that a sufficiently massive star could, at least in

theory, collapse to an incredibly small entity only a few kilometres across, or perhaps, in an extreme scenario, even a singularity, a point where the entire mass of the star has been crushed to infinite density, giving birth to a gravity well of equally infinite strength (Chandrasekhar,1934:377; Evans,1998:143-144; Hawking,1988:46-47; Kaufmann,1988:472-473; Penrose,1997:62-63).

It is clear that Chandrasekhar's findings seriously challenged the socially agreed upon conceptualization of stellar evolution of the time, one that introduced notions that astrophysicists had a difficult time assimilating with their prior experience and knowledge of the topic. Thus, when faced with what appeared to be an anomalous theoretical conclusion, one that had no means of being verified in any empirical sense, astrophysicists had no option but to draw upon the socially-agreed to interpretive resources of their culture in order to evaluate the legitimacy of Chandrasekhar's work. Indeed, Eddington effectively used these resources to construct the notion of the limiting mass to be more fiction than fact, and convinced many in the astrophysical community that Chandrasekhar's conclusion that only some stars passed into oblivion as a white dwarf to be highly implausible. Eddington's success in this respect was founded upon the fact that he, and his colleagues shared a similar intellectual culture, a culture whose underlying conceptual conclusions concerning stellar evolution had been settled at a point in the past, conclusions whose validity astrophysicists had come to accept and trust as final (Harvey,1981:96,113,124; Pickering,1981:89).

Indeed, in their construction of the limiting mass as an untenable concept, both Milne and Eddington constantly emphasized to their colleagues that the critical problem with this particular notion was that it generated an *irrational* picture of how stars evolved over time, and ultimately died. Fowler's model had provided a neat theoretical image in which stars of any mass settled into old age as white dwarfs until they exhausted their fuel reserves and became extinct. However, Chandrasekhar's work implied that a sufficiently massive star could *never* find a state of equilibrium, and must keep radiating and contracting until it evolved into a new kind of bizarre

entity, such as a neutron star, or it collapsed indefinitely to form some kind of point mass, a wholly outrageous conclusion as far as the knowledge base of 1930s astrophysics was concerned (Chandrasekhar,1939:245; Evans143-144; Penrose,1997:62-63; M/RAS,1935:37-39).

As Milne informed the RAS in 1935, his own model of stellar evolution, which was not based on relativistic principles, generated essentially the same result as Chandrasekhar's, namely, that the more massive the star, the smaller its radius; however, since the model had ignored the incorporation of relativistic effects into its conceptual framework, it stopped short of implying the troubling notion that only stars of a given mass could transform into the white dwarf stage. Milne argued that his methodology not only presented a more detailed exposition of stellar structure than Chandrasekhar's, but a more plausible theoretical picture of stellar evolution (M/RAS,1935:37; 1935:52). As Milne stated, "... I have pursued a cruder method of analysis, but I believe that my method gives more insight into the fundamental physical postulates underlying the work ... and gives *a more rational picture*...."(Milne cited in M/RAS,1935:37).

Similarly, in every public and written response to Chandrasekhar's theory, Eddington, too, stressed the fact that Chandrasekhar's conclusion that some stars had a limiting mass generated a very problematic and disturbing conceptual picture for extremely massive stars. According to Chandrasekhar's theory, an enormous stellar object could not appeal to Fermi-Dirac statistics to halt its collapse once it had exhausted its fuel reserves; as such, it would be continuously losing heat, and subsequently, being forced by its tremendous gravity into a smaller and smaller volume of space, becoming ever denser with the passage of time. Such a star, in theory, could only find peace when it became so small – perhaps, as small as a planet, or the moon, or an asteroid, or smaller still – and its gravitational field so intense, that radiation could no longer escape from its surface and it would reach a state of equilibrium (Eddington,1935; Eddington,1937; M/RAS,1935: 37-39; Russell,1935:19). As Eddington argued before the RAS,

> ... The star has to go on radiating and radiating and contracting and contracting until, I suppose, it gets down to a few km. radius, when gravity becomes strong enough to hold in the radiation, and the star can at last find peace ... Various

> accidents may intervene to save the star, but I want more protection than that. ***I think there should be a law of Nature to prevent a star from behaving in this absurd way!*** (Eddington cited in M/RAS,1935:37; emphasis added).

And as Eddington stated at the Harvard Conference of 1936, Chandrasekhar's work defeated Fowler's research which showed that all stars could end their days in a *reasonable* fashion. As Eddington said,

> ... The small stars could cool down all right, and end their days as dark stars in a reasonable way. But above a certain critical mass (two or three times that of the sun) the star could never cool down, but must go on radiating and contracting until heaven knows what becomes of it. ***That did not worry Chandrasekhar; he seemed to like the stars to behave that way, and believes that that is what really happens.*** But I felt the same objections as 12 years earlier to this stellar buffoonery.... (Eddington,1937:138; emphasis added).

There is little doubt that when Eddington made these statements he was thinking of the enormous star, Betelgeuse, whose physical characteristics, first deduced in around 1920, indicated that it had a diameter of roughly eight-hundred times the diameter of the sun. In 1926, Eddington had speculated that if Betelgeuse – which has a very low density – had a density reading as high as the sun's, it's gravitational field would be so great that it would twist the very fabric of space-time around its mass to such an extent as to draw in the cosmos, leaving humans in a state of physical limbo. As Eddington wrote,

> The great bulk of these giant stars is due to low density rather than great mass. Betelgeuse for example has a radius of the order 250 million km. and a volume 50 million times greater than the sun. But the mass, or amount of matter contained in it, is probably between 10 and 100 times greater, so that the density is about a million times less. It is rather interesting to notice that Einstein's theory of gravitation has something to say on this point. According to it a star 250 million km. radius could not possibly have so high a density as the sun. Firstly, the force of gravitation would be so great that light would be unable to escape from it, the rays falling back to the star like a stone to the earth. Secondly, the red-shift of the spectral lines would be so great that the spectrum would be shifted out of existence. Thirdly, ***the mass would produce so much curvature of the space-time metric that space would close up round the star, leaving us outside (i.e. nowhere)***....(Eddington,1926:6;emphasis added).

This was, for Eddington an incredibly preposterous, unrealistic image, and perhaps, this was the very conceptual picture that first occurred to him when he read Chandrasekhar's conclusion that a sufficiently massive star could not appeal to the Fermi-Dirac statistics to avoid collapse into a potential singularity (Chandrasekhar,1932:64; Eddington,1926:6; Eddington,1935:153; Evans,1998:120,146; Kaufmann,1988:357,421). As Evans writes, "... If this is what he had in mind, in the white dwarf case, it is easy to understand his repugnance"(Evans,1998:146).

In similar fashion, the mathematical physicist, Roger Penrose, renowned for his work in general relativity, wrote the following concerning the difficulty of reconciling the stunning consequences of Chandrasekhar's research with the conventional wisdom of 1930s astrophysics. As Penrose stated, with respect to Chandrasekhar's white dwarf model,

> ... There are, indced, still many possible loopholes in the arguments which lead to the final conclusion that has now become an accepted implication of present-day theory – that, at least in some cases, ***the fate of a body in gravitational collapse must be to encounter a space-time singularity, representing, for the constituents of that body, an end to time!***
>
> The issue had been at the root of his difficulties with Eddington ... Eddington, also, was aware of the implications of Chandra's findings, but regarded this as a *reductio ad absurdum* and preferred to move along his own highly speculative route towards a fundamental theory.... (Penrose,1997:62-63; emphasis added).

Thus, in both Milne and Eddington's presentations, the central argument was that the implications of the limiting mass were just too fantastic as to be plausible. The convictions of these scientists flowed from their socialization in a collective intellectual culture, a culture which effectively fostered a shared tacit knowledge of stellar evolution – knowledge of what was and was not a reasonable fate for stars. The justifiability of this stance was perfectly logical within the social parameters of the astrophysical community whose members already accepted the more rational, theoretical model of how stars became extinct elucidated by Fowler.

In this respect, we see how strongly cultural factors influence a scientist's perception and interpretation of a specific discovery. In truth, the prior investigations of Anderson and Stoner clearly demonstrated that there was the *potential* possibility that all stars did not evolve into white

dwarfs as they grew old if one incorporated relativistic effects into Fowler's model. Milne's own research concluded that the more massive a star, the smaller its radius when it became a white dwarf; indeed, in this sense, the idea that an enormous stellar object could collapse into a very tiny volume of space, an idea Milne found problematic when placed in the context of the limiting mass concept, was already embedded in an implicit fashion in his more *rational* theoretical framework. Furthermore, Eddington himself did not appear to have great problems with Anderson and Stoner's attempts to integrate relativity with Fermi-Dirac statistics, though he was clearly annoyed by the exercise (Eddington,1937:138; Harvey,1981:106,113; Milne,1935:52; M/RAS,1935:37). As Eddington stated,

> ... Not content with letting well alone, physicists began to improve on Fowler's formula. They pointed out that in white dwarf conditions the electrons would have speeds approaching the velocity of light, and there would be relativity effects which Fowler neglected. ***Consequently Fowler's formula, called the ordinary degeneracy formula, came to be superseded by a newer formula, called the relativistic degeneracy formula. All seemed well*** until certain researches by Chandrasekhar brought out the fact that the relativistic formula put the stars back in precisely the same difficulty from which Fowler had rescued them.... (Eddington,1937:138; emphasis added).

Indeed, Henry Norris Russell, writing on Chandrasekhar's discovery in the July 1935 issue of *Scientific American* appears to indicate that the relativistic degeneracy formula employed by the young theorist – initially developed by Anderson and later refined by Stoner – did have a certain measure of social acceptance, though Eddington's problems with the formula arose from how it was explicitly employed by Chandrasekhar to make bold, problematic predictions concerning the fate of large stars. As Russell wrote, Chandrasekhar's model, "... is based on precise calculation, founded on a theory which is generally accepted[32] – though Eddington has recently criticized it...."(Russell,1935:19).

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[32] It is not clear what Russell (1935) meant when he said that Chandrasekhar's model was based upon a theory that was generally accepted. It would appear he was talking about the relativistic-degeneracy formula first introduced into the literature by Anderson and Stoner. If this is the case, it is odd that he would make such a statement considering the fact that prominent theorists like Fowler, Milne and Eddington had serious reservations about the validity of the relativistic-degeneracy formula.

Thus, in light of the above statements, while it would have been intellectually permissible for Milne and Eddington to have interpreted the limiting mass as a potential – if not problematic – consequence of relativistic-Fermi-Dirac statistics that should be investigated further, they chose, instead, to interpret it in a fashion that was consistent with the socially-accepted conceptualization of stellar evolution dominating astrophysics at the time, one that did not violate prior agreements, nor challenged the personal conceptual commitments of both scientists concerning the ultimate fate of stars.

While it is true that Milne strongly disagreed with Eddington's theoretical stance regarding the physical structure of stellar matter, he was more than willing to support Eddington in his battle with Chandrasekhar since both scientists shared the same conceptual outlook and had great faith in the orthodox view that white dwarfs represented the terminal stage for all stars irrespective of their mass. Furthermore, in supporting Eddington, Milne need not re-evaluate, nor abandon his own personal commitment to his belief that all stars had degenerate cores (Evans,1998:132-133; Pickering,1981:73,78-79,87; Wali,1991:119-122; Wynne,1976:336-337). As Milne informed Chandrasekhar,

> ... ***To me it is clear that matter cannot behave as you predict*** ... Eddington is nearly always wrong in his work in the long run, and I am quite prepared to believe that he is wrong here, in his details. ***But I hold by my general considerations*** (Milne cited in Chandrasekhar,1969:584; emphasis added).

The reality that the relativistic-degeneracy formula which Chandrasekhar had used in his work, had been socially articulated by Anderson and Stoner many years earlier, and tentatively supported in a limited sense, did provide the basis for an alternative conceptualization of stellar evolution, one that could have been cautiously entertained if the astrophysical elite had chosen to do so, is touched upon by Penrose when he wrote that Eddington had rejected "... the sound reasoning ***within the accepted tenets of procedure*** that had characterized what Chandra had achieved...."(Penrose,1997:63; emphasis added).

In similar fashion, Professor Sundaresan also remarks on the fact that Eddington need not have accepted the bizarre consequences of Chandrasekhar's work, but could have left the whole matter open to further research given the pedigree of the relativistic-degeneracy formula,

> ...But, I think what was sad about it was the way it was done, namely, in a meeting where Eddington makes some pronouncements about it, and essentially, tears it down. That was unfortunate because, he could have very easily said, look, this is so novel, and this is so intriguing, it is well worth the time of all sorts of people who have an open mind to investigate further, whether it was right or wrong, rather than say, this is nonsense, and that is the difference. He was so convinced of his own knowledge of various things that he could not conceive of the possibility that he could be wrong – he would not even allow the possibility, which is what I say was sad.

Yet, as we have seen, conviction in the plausibility of a particular conceptual view is a direct function of personal experience and knowledge of a specific field of inquiry, and of socialization into a given shared scientific culture, a culture which sensitizes one to perceive a physical phenomenon in a fashion that resonates with well established beliefs. This is not to say that scientists are blind to alternative conceptualizations of nature, but that these conceptualizations are given very little weight against an already well-established and consensual view of reality. In this respect, critics of Chandrasekhar's research were able to draw upon these cultural resources to strongly influence the distribution of plausibility surrounding the notion of the limiting mass, and preserve orthodox ideas concerning stellar matter and its behaviour under extreme physical conditions (Collins,1985:136,150; Harvey,1981:105-106; Pickering,1981:78-79).

Perhaps, the most critical observation one can make with respect to the Chandrasekhar-Eddington controversy, is the critical socio-cognitive role Fowler's model played in not only facilitating the social construction of the limiting mass to be the by-product of technical error, but also constituting the conceptual epicentre for the genesis of the dispute in the first place.

Indeed, it is probably a reasonable argument to make that the primary reason this controversy erupted was not because Chandrasekhar had combined relativity with non-relativistic quantum theory, but because the specific union of these theories gave rise to a conclusion the challenged

pre-existing agreements concerning stellar evolution. Thus, the whole of Eddington's objections to Chandrasekhar's white dwarf model, as well as his rhetorical statements that the combination of relativity and Fermi-Dirac statistics was a union born in unlawful wedlock, was a direct function of the bizarre consequences generated by notion of the limiting mass. It is hard to imagine that Eddington would have made such statements had Chandrasekhar's work simply resulted in a relativistic generalization of the Fowler model, preserving the original notion that all stars passed into old age as white dwarfs. Indeed, Eddington himself had demonstrated that a *correct* application of relativity theory with Fermi-Dirac statistics preserved the well-established theoretical picture first enunciated by Fowler (Douglas,1956:162; Eddington,1937:138; M/RAS,1935:37). As his biographer wrote, "... Thus he assured himself that nature would deal justly with the white dwarfs!"(Douglas,1956:162).

At this point, it is important to point out that Chandrasekhar himself first believed that in using a relativistic approach would result in a neat, generalized, conceptual picture of white dwarf structure. When this expectation did not materialize, he writes that it proved to be of great puzzlement (Chandrasekhar,1969:582; Wali,1991:76). As he wrote, "... I was puzzled by the emergence of the critical mass when I first obtained it...."(Chandrasekhar,1969:582).

Following from this, it can be concluded that Chandrasekhar's puzzlement of his discovery, was born not of an initial examination of technical matters, but of an assessment of its phenomenal prediction which postulated a theoretical scenario that existed outside the boundaries of a narrow range of socially feasible phenomena. In this respect, Fowler's white dwarf model constituted the only socially-available theoretical benchmark Chandrasekhar – and his critics – could use in order to check the validity of his analysis. It seems logical to suggest that had Fowler's model been unavailable, Chandrasekhar's conclusions concerning the fate of stars probably would not have surprised him as much as they did, and would, no doubt, have made it a been a bit more digestible to the orthodox establishment, but perhaps, not completely so, as it would have still postulated the incredible possibility of a truly massive star collapsing to a

singularity, a conceptual scenario that would have remained a deviant idea as far as the cultural resources of astrophysics was concerned, whether Fowler's research had existed or not (Harvey,1981:124; Pickering,1981:87-89).

Nevertheless, we have seen how influential cultural variables are in constraining not only a scientist's perception of the validity of a predicted phenomenon, but also the validity of the theoretical apparatus used in the generation of the controversial result. Indeed, Eddington's key point of attack was to argue that since the results of Chandrasekhar's work presented such an implausible, theoretical picture concerning the future of large stars, there *had* to be something wrong in the technical derivation of the limiting mass. As Eddington stated before his peers at the 1939 Paris Conference,

> ...in stars of mass greater than the critical masses mentioned by Dr. Chandrasekhar there is no limit to the contraction, so that if the star is symmetrical and not in rotation, it would contract to a diameter of a few kilometres, until, according to the theory of relativity, gravitation becomes too great for the radiation to escape. This is not a fatal difficulty, ***but it is nevertheless surprising; and, being somewhat shocked by the conclusion***, I was led to reexamine the physical theory and so finally to reject it....(Eddington,1939:289; emphasis added).

Yet, in truth, Eddington and Milne were not the only prominent astrophysicists who felt that the union of relativity and non-relativistic Fermi-Dirac statistics must be in error given the ludicrous, conceptual vision it presented. Indeed, the Russian physicist, Lev Landau, who independently derived the limiting mass of stars, employing essentially the same mathematical methodology as Chandrasekhar, found the theoretical implications of this concept to be so contrary to common sense, that he was willing to adopt an intellectual position that argued that the laws of quantum mechanics broke down in enormous stars in order to preserve the more rational conceptualization of stellar evolution postulated by Fowler (Landau,1932; Lightman,1984:34-35). As Landau wrote,

> ... For $M>M_o$ [critical mass] there exists in the whole quantum theory no cause preventing the system from collapsing to a point (the electrostatic forces are by great densities relatively very small). ***As in reality such masses exist quietly as stars and do not show any such ridiculous tendencies*** we must conclude that all

> stars heavier than 1,5 ⊙ [sic; solar masses] certainly possess regions in which the ***laws of quantum mechanics (and therefore of quantum statistics) are violated. As we have no reason to believe that stars can be divided into two physically different classes according to the condition M> or <M_o*** we may with great probability suppose that all stars possess such pathological regions. It does not contradict the above arguments, which prove only that the condition $M>M_o$ is sufficient (but not necessary) for the existence of such regions. It is very natural to think that just the presence of these regions makes stars stars.... (Landau,1932:287; emphasis added).

As physicist, Alan Lightman[33] stated, commenting on Landau's interesting rationalization of his unconventional discovery,

> What is shocking about the 1932 paper is that Landau, without warning and in a single sentence, dismisses a major branch of physics ... By 1932 the laws of quantum mechanics had been firmly established and ranked beside Einstein's relativity as the foundation for modern physics. To Landau's dismay, his calculations predicted that burned-out stars cannot avoid complete inward collapse if slightly more massive than the sun. That is, in sufficiently massive cold stars no amount of internal pressure can counterbalance the inward crush of gravity, leading to a frantic contraction of the star from a sphere a million miles or more across down to a point ...
>
> It seems Landau found his theoretical result (which was actually one of the first predictions of black holes) so preposterous, ***so disturbing to common sense***, that he was willing to abandon the celebrated theory that produced the result.... (Lightman.1984:34-35; emphasis added).

Indeed, Lightman argues that Landau's rejection of his startling result is a clear example of how bias and prejudice at times influences scientific practice. Yet, Lightman makes no comment

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[33] Lightman cites another – though this time – very famous example in which cultural constraints played an influential role in the perception of a surprising theoretical prediction. In this instance, the case revolves around Albert Einstein's general theory of relativity which predicted a dynamic, expanding universe. Einstein was so convinced that the universe was a static entity, that he engaged in some mischievous mathematical juggling. Thus, into the field equations of the general theory, he introduced a term known as the cosmological constant, a constant that represented a repulsive force which increased in strength with the distance between objects. Since the notion of a static universe could be traced back to the days of Aristotle, one that had become a permanent fixture of Western thought for so many centuries, and empirical evidence of the time did not appear to contradict this view, Einstein no doubt strongly felt that the predictions of his theory in this respect were clearly erroneous against the socially-agreed to conceptualization of the cosmos.

However, in 1929, a young astronomer by the name of Edwin P. Hubble produced observational data that very forcefully suggested that the galaxies were fleeing from one another at extraordinarily high velocities; if the galaxies were moving farther and farther apart with the passage of time, this implied that the *space between the galaxies* was also increasing with each passing moment, leading scientists to infer that the very structure of the universe was expanding. In light of this evidence, Einstein abandoned his cosmological constant in 1930 (Clark,1972:264-270; Hawking,1988:39; Kaufmann,1988:551; Lightman,1984:35-36; Pickering,1981).

on how such bias arises in the first place, appearing to suggest that it's a purely psychological phenomenon related to a particular scientist. However, a relativist-constructivist perspective would argue that such beliefs do not emerge in a vacuum, but are a direct consequence of socialization in a specific scientific tradition in which scientists are exposed to a body of well established and communally accepted knowledge regarding a specific topic area. As such, scientists immersed in such an intellectual culture feel confident that they know what theoretical results appear reasonable and which seem ludicrous (Collins,1976:206-207; Lightman,1984:34-35; Harvey,1981; Pickering,1981).

In this respect, the above-cited quotation from Landau clearly demonstrates that his beliefs concerning what was ridiculous and reasonable with respect to stellar structure, were influenced by his faith in a number of fundamental assumptions located in the culture of astrophysical research related to the investigation of stellar material and the behaviour of degenerate matter. Landau knew that the physical nature, characteristics and evolutionary paths of stars had been socially articulated over the course of many years, and had come to constitute a socially-accepted interpretive resource, one that, obviously, resonated with his own personal experiences in the field of astrophysical analysis. Thus, in concluding that his discovery of the limiting mass implied the existence of pathological regions in a star, Landau was forced to manipulate his analysis in such a fashion in order to produce an explanation that served to preserve prior social agreements, and his own tacit knowledge concerning the fate of all stellar matter, even though in doing so, he was essentially willing to cast strong suspicion upon the validity of two theoretical frameworks that enjoyed a tremendous amount of support within the scientific community as a whole.

Thus, in effect, the similar conclusions reached by Eddington, Milne and Landau, and the ability of Eddington to convince his colleagues that the limiting mass concept was highly implausible, was only possible because astrophysicists had been socialized into, and constituted members of, a highly specialized cultural group, one that legitimated and placed conceptual boundaries upon a particular conceptualization of the natural world. Indeed, in the absence of

such a shared cultural background, it is conceivable that the allocation of plausibility regarding the limiting mass would not have been skewed so strongly against Chandrasekhar's work, but might have fostered a socio-cognitive environment in which his ideas were seen as representing a potentially new and exciting means of comprehending the life-cycle of stars (Collins,1975:207;1985:139;Harvey,1981:106,113; Lightman,1984:34-35; Pickering,1981:73,78-79). To moderately paraphrase a statement once made by Harry Collins, the realm of ideas is not circumscribed by the limits of human thought but by the limits that a given social group places upon the interpretation and conceptualization of natural phenomena (Collins,1985:150).

In this respect, one can put forth the equitable supposition that the reason Chandrasekhar's opponents were so effective in discrediting their junior colleague's alternative, theoretical vision of stellar evolution was not because they were able to present overwhelming, empirical proof, or even observational evidence in any significant sense, that their particular contentions concerning the fate of white dwarfs represented the truth, but because they were able to monopolize the notion of plausibility by consistently and effectively drawing upon well-established cultural resources. Articulated in a slightly different fashion, one can argue that the critical explanation that accounts for Chandrasekhar's inability to convince his peers of the plausibility of his model, lies with the fact that he was unable – or unwilling – to present his findings in terms of a very narrow and limited range of socially conceivable concepts (Collins,1985:136,138; Pickering,1981:89).

As we have shown, the examination and assessment of the plausibility of a controversial discovery is not an exercise governed solely by strict, rational rules or guided by the intangible hand of logic. In truth, it constitutes a social activity in which the viability of an unorthodox finding is often fiercely negotiated by two sides, and while that negotiation concentrates on cognitive issues, its scope and scale is inevitably limited by the socio-cultural context in which it exists. While Chandrasekhar was able to publish his unorthodox model – though not without struggle – and was given plenty of opportunities to present his work to his colleagues, he was

unsuccessful in convincing astrophysicists that the unconventional predictions of his model held merit, if only in a tentative, speculative sense. In light of the socio-cognitive dynamics of astrophysics of the time, it would appear that one lecture or paper written by Arthur Eddington spoke much louder than the many public presentations or articles written by Chandrasekhar, or those written by his supporters in theoretical physics.

This reality raises an important point, and that being that the plausibility of an anomalous finding is a function of the social, as well as cultural, context into which it emerges. That is to say, in most controversies the proponents and opponents of an unorthodox discovery do not commence debate on an equal footing – one camp may possess numerous socio-cultural and technical advantages, such as being constituted by very prominent scientists, access to the latest, cutting-edge technology, a deep financial reservoir with which to pursue a given line of inquiry to completion, or, as was the case in the Chandrasekhar-Eddington controversy, an air-tight grip on the notion of plausibility. While it may be presumptuous to argue that the outcome of this particular disputation could have been predicted well in advance, the evidence seems to indicate that Chandrasekhar was faced with a powerful social and cultural current flowing against him from the moment he had discovered the limiting mass concept (Collins,1985:150; Harvey,1980:161; 1981:124-125; Pickering,1981:65).

In this respect, in a hypothetical social context in which a very distinguished scientists such as Eddington had felt strongly about the validity of Chandrasekhar's research, and had argued with the same rhetorical and authoritative flair in favour of this unconventional model as he did against it, social perception of the young scientist's research might have been decidedly different given the enormous social stature Eddington possessed in astrophysics. This is not to say that all astrophysicists would have blindly agreed with Eddington, for they too had been socialized to conceptualize stellar evolution as a phenomenon that ended with the white dwarf. Nevertheless, Chandrasekhar's surprising predictions might have been accorded closer scrutiny by other practitioners, and everyone might not have been so quick to assume that the whole issue of the

ultimate fate of stars had been conclusively resolved in 1926, and that it was time to move on to other matters (Harvey,1980:161).

But, it was precisely because of the weight of collective opinion amassed against his work, an opinion that had formed and solidified because of the actions of one specific individual, that Chandrasekhar decided to completely withdraw from the debate. He felt that since most astrophysicists viewed the notion of the limiting mass as so implausible and unrealistic in practice, a view that was being passionately championed by one of the most famous scientists of the twentieth century, there was little to be achieved in prolonging his efforts to get others to believe in the validity of his work. In this respect, Chandrasekhar's decision to leave the controversy – though not necessarily admitting defeat – not only ended the dispute in a pragmatic sense, but also, from a sociological perspective, led to the breaking of the theoreticians' regress (Gupta,1995:200; Horgan,1994:501; Nityananda,1995:554; Parker,1995:108; Penrose,1997:63; Wali,1991:127,135,138,145). Chandrasekhar, in a conversation with his biographer, explains his reasons for leaving the controversy with Arthur Eddington:

> It was a personal decision I made at the time. I felt that astronomers without exception thought that I was wrong. They considered me as a sort of Don Quixote trying to kill Eddington. As you can imagine, it was a very discouraging experience for me – to find myself in a controversy with the leading figure of astronomy and to have my work completely and totally discredited by the astronomical community. I had to make up my mind as to what to do. Should I go on the rest of my life fighting? After all I was in my middle twenties at that time. I foresaw for myself some thirty to forty years of scientific work; and I simply did not think it was productive to constantly harp on something that was done. It was much better for me to change the field of interest and go into something else. If I was right, then it would be known as right. For myself, I was positive that a fact of such clear significance for evolution of the stars would in time be established or disproved. I didn't see that I had a need to stay there, so I just left it (Chandrasekhar cited in Wali,1991:145-146).

Chandrasekhar's decision to withdraw from his dispute with Eddington and thus, essentially put an end to the controversy is an approach that is, in ways, somewhat unique in comparison with how most scientific disputes appear to find closure. Indeed, in most cases, a scientific controversy ends either because the discoverers of an unorthodox result conclude that their

finding does not represent a novel discovery as initially believed, as was the case in the monopole incident discussed by Pickering, or because the anomalous result comes to be perceived as so implausible that it gradually fades from the mainstream scientific consciousness, leaving a small band of proponents who continue to fight to the bitter end in establishing the validity of their problematic claim, as is the case with the cold fusion controversy[34], or because a consensus slowly develops with the accumulation of empirical evidence that one side's theoretical stance regarding a specific issue most succinctly accounts for the data, as was the case in the Devonian controversy (Gieryn,1999; Pickering,1981; Pinch,1994; Rudwick,1985; Simon,1999).

In Chandrasekhar's case, he fought the good fight to have his research accepted by the astronomical community, but only up to a point. While he could have continued to question the validity of Eddington's arguments against his work, and made further attempts to mobilize elite theoretical physicists to join him in a public show of solidarity against the prominent astronomer, he decided not to undertake this particular path, and chose instead to not only withdraw from the controversy, but also to completely divorce himself from all further investigations into the structure of white dwarfs and let history judge the validity of his research. This particular decision, as is evident by the specific elements of this case and Chandrasekhar's own words, was not based on any technical grounds, but on purely social factors (Pinch,1994).

In essence, the Chandrasekhar-Eddington controversy has demonstrated that the plausibility of an idea is, in certain contexts, as much a function of social and cultural variables as it is of technical ones.

At this point it is important to emphasize that this project constitutes a single case study of a scientific controversy; as such, it is not only problematic to generalize the findings in this report to other similar intellectual disputes, but also to use this study as a foundation for constructing a

[34] Cold fusion research is still being pursued in countries such as Japan with a certain degree of intensity (Simon,1999).

theoretical model for how the plausibility of a novel discovery is assessed and judged by scientists.

Nevertheless, the case study approach does allow one to study, research and achieve a detailed understanding of complex phenomena in a fashion that is not amenable to the traditional hypothetico-deductive approach; the insights realized with this strategy often permit one to make approximate generalizations that can form the foundation for a more rigorous, future investigation (see Cunningham,1997; Dyer & Wilkins,1991; Eisenhardt,1991; Shrader-Frechette & McCoy,1994).

In light of these statements, and with respect to this study, it is, perhaps, appropriate to state that the plausibility of a given argument or conceptualization of nature cannot be examined as existing in a social vacuum for the structure and composition of such arguments and perceptions of the physical world are constrained and limited by the socio-cultural context in which they arise.

Indeed, as we have seen, Eddington and Milne's conviction that the limiting mass concept was implausible flowed from their socialization in a highly specialized disciplinary culture, a culture that constrained their views of what was or was not an acceptable evolutionary fate for stars. Thus, the faith these scientists had in the fundamental principles and concepts of their specialty allowed them to perceive their beliefs that all stars ended their lives as white dwarf stars as being certain and well-established even in the absence of any empirical evidence to suggest that this really was the case. As such, they were quite strongly convinced that the notion of the limiting mass could not possibly exist in nature as an actual, physical phenomenon but had to be the by-product of technical error. This belief led them to constantly and effectively draw upon cultural resources in such a fashion as to contrast the pragmatic, rational qualities of their vision of stellar evolution, with the seemingly irrational predictions of Chandrasekhar's research. In this way, Eddington and Milne were able to not only influence social perceptions of the plausibility of their junior colleague's work, but preserve prior social agreements concerning white dwarf structure,

as well as their own specific obligations in the field, founded upon long standing expertise and faith in a given conceptual tradition. Indeed, it would appear that in rejecting Chandrasekhar's white dwarf model, these elite scientists were acting primarily "...on the strength of human commitments, rather than being guided by 'impersonal' norms"(Wynne,1976:337).

In effect, one can state that orthodoxy's victory in this particular case was not due to its ability to conclusively prove its case through any independent, objective means, but because it was able to successfully monopolize the notion of plausibility. This finding suggests that what a community perceives as truth is, ultimately, contingent upon social and conceptual action (Collins,1985:136,150; Harvey,1981:124-125; Pickering,1981:73,89; Wynne,1976:336-337).

I must emphasize that I do not mean to imply in any way that Chandrasekhar's critics *should* have accepted the startling theoretical implications of his research. While it is very tempting for scientists[35] residing in the early twenty-first century to look back into the early years of the twentieth century in which this controversy took place, and to proclaim that Eddington and Milne were being unreasonable in critiquing Chandrasekhar's theory, the fact was that the limiting mass introduced truly surprising theoretical possibilities that extended beyond the cultural parameters of 1930s astrophysics; as a result, scientists had a hard time assimilating Chandrasekhar's bold statements concerning stellar evolution with their own conceptual training and experience in the field.

Indeed, one wonders how much more open-minded astrophysicists living in the year 2001 would have been to Chandrasekhar's results if they, like Chandrasekhar's opponents, had been socialized into the culture of astrophysics as it existed over seventy-years ago in which theoretical conceptualizations of stellar structure and evolution were firmly entrenched; I would suspect that these individuals would have in all probability been more far more likely to perceive the

[35] One astrophysicist who sent me his informal thoughts on the Chandrasekhar-Eddington debate via e-mail, believed Eddington should have accepted Chandrasekhar's research, and should have known better than to have argued that the combination of relativity and non-relativistic quantum theory was not legitimate. This seems a somewhat unfair statement given the conventional wisdom of the time concerning stellar evolution and the novelty of applying quantum mechanics to stellar models.

arguments put forth by the prestigious Arthur Eddington and Edward Milne as being far more plausible than those articulated by the relatively unknown Chandrasekhar.

7 Conclusion

In summary, I wish to make a number of points concerning the findings of this project. Firstly, I have examined a scientific controversy that occurred in astrophysics over seventy years ago concerning the fate of stellar matter, and I have attempted to comprehend this particular dispute from the conceptual perspective offered by modern SSK research.

In essence, the sociology of scientific knowledge (SSK) is interested in comprehending how scientists construct and assess knowledge-claims of the physical world, and the role socio-cultural factors play in this endeavour. One of the curious, if not controversial, conclusions of this research is that the social and cultural context in which a scientist performs his/her duties has a strong influence on how that scientist perceives and interprets empirical data or theoretical findings. In this respect, a specific body of knowledge comes to be labelled as true and plausible, whereas an alternative conceptualization is perceived as incorrect and flawed, not necessarily due to an impartial, rigorous examination, but because of the socio-cultural context in which knowledge claims evolve. By arguing that the content of scientific knowledge is shaped by social variables, this field of study suggests that scientific positivism – the notion that science is a discipline governed by rational rules, deciphering the mysteries of nature in a purely objective fashion – is a dubious concept in practice. As such, SSK analysis tends to emphasize the relativistic nature of knowledge, and how certain knowledge claims are shaped, moulded and acquire meaning through social interactions (Collins,1985:1-12; Harvey,1980:139-140;1981:95-96; Jones,2000:317-318).

In this respect, I have tried to use the conceptual tools of SSK to shed some light on the social and cognitive dynamics inherent in the Chandrasekhar-Eddington controversy. I have argued that the inability of Chandrasekhar and his critics to persuade one another of the merits of their respective arguments, represents a classic example of the theoreticians' regress. In short, this articulates the notion that it is virtually impossible to conclusively resolve the validity of a disputed theoretical prediction by simply criticizing the calculation which initially generated the

problematic result, since the proponents and opponents of an unorthodox discovery might possess varying experiences with a specific problem or calculation, as well as differing styles that strongly affects how s/he approaches a given topic area, and even certain metaphysical beliefs concerning the phenomenon in question, all of which can lead to a serious communication gap that may prove impossible to bridge. As a result, such a process of debate inevitably gives rise to a circular pattern of argument in which both sides staunchly defend the legitimacy of their respective positions. Once initiated, this form of conceptual regress is exceedingly difficult to break by simply appealing to the norms of logic and objectivity.

However, a critical prediction of the theoreticians' regress is that a controversy of this nature can be broken if the participants in the dispute come together and develop a unified approach that attempts to seriously consider the technical structure of the unorthodox prediction; in this way, a consensus might arise that either characterizes the problematic theoretical discovery as possessing intellectual merit or as an artifact of mathematical error (Kennefick,2000:6,30-34).

Yet, I argued that during the tenure of a scientific controversy, such diplomatic behaviour on the part of scientists is a rare sight, indeed. Instead, I proposed an alternative model of how disputes of this kind find closure is when one side is able to successfully construct the views of the other as being so implausible, that its arguments come to be nearly universally perceived as representing the truth, and thus, essentially forcing its rivals to withdraw from the debate, though not necessarily conceding defeat. In this respect, the winning side proclaims victory, not so much because it was able to conclusively prove by reference to overwhelming empirical evidence that it was right, but because it was able to monopolize the notion of plausibility to its benefit. The mechanisms by which this is achieved often sees the victorious party drawing upon cultural resources in such a fashion that it effectively constructs the troublesome finding as being too paradoxical to be believed. The ability to discursively weave such an argument, and its social effectiveness could only be possible if scientists have been socialized into a shared culture where certain fundamental principles and beliefs are accepted as well-established. This cultural

immersion thus, inevitably, influences how scientists observe the physical world, interpret data, and perceive anomalous findings.

In effect, scientists conviction in the legitimacy of specific, theoretical concepts and models is born of their early immersion in their speciality, a socialization that sees them learning how to use the conceptual tools of their field to solve a range of practical problems first introduced to them in textbooks and examinations. These problem-solutions, which Kuhn refers to as exemplars, are significant for they demonstrate the concrete utility of a cultural product – i.e., theoretical framework, experimental methodology – in solving a particular intellectual puzzle. As such, by doing problems, the formulas, equations and models that students of physics learn are transformed from the realm of the abstract and become linked with a physical reality, which Pickering refers to as 'out-thereness' (Kuhn,1970:187-188; Pickering,1980:108-109). In this respect, exemplars reinforce the validity of a set of conceptual tools, and "... refers back, from the problem to be understood at the research front, to some existing body of practice...."(Pickering,1980:109) and knowledge.

Phrased in this fashion, the solving of exemplars adds to a scientists experience with a specific cultural resource, guides future practice, and reinforces his/her faith in a particular conceptualization of the natural world; consequently, this faith also permits the scientist to judge the legitimacy of an anomalous experimental or theoretical finding, to ascertain whether or not a given discovery has merit or is highly implausible (Harvey,1981:106-107,113).

As we have seen, Chandrasekhar's critics were very strongly convinced that the limiting mass notion had to be a by-product of technical error and could not possibly exist in practice. This conviction was only possible due to their shared immersion in the culture of 1930s astrophysics, a culture which entrenched in them a certain degree of knowledge and experience as to the structure and behaviour of stellar matter, knowledge that essentially influenced their conceptualizations as to what constituted a reasonable or plausible theoretical picture with respect to stellar evolution. In this respect, Fowler's application of the Fermi-Dirac statistics to the white-

dwarf problem had resulted in an exemplary solution, reinforcing the utility of the new statistics in investigating astrophysical problems, but also allowing Fowler's model to become identified with the natural life-cycle of stellar matter, permitting it to become entrenched in the culture of astrophysics as a socially-acceptable conceptualization of how all stars ended their lives (Pickering,1980:128-130; 1981:88-89).

As such, when Chandrasekhar first discovered the critical mass, and later persisted with his arguments that this notion was a legitimate conceptual entity, he was, in effect, seriously questioning a number of fundamental principals and beliefs embedded in the culture of astrophysics. This reality, coupled with, Chandrasekhar's bold statements concerning the fate of stars ended up stepping upon the cherished views of one of the most distinguished astrophysicists of the era who had pioneered so much theoretical research into stellar structure, one who felt he knew for certain how all stars ended their respective lives. While Chandrasekhar, no doubt, believed that his work was an honest attempt at articulating the composition of a surprising conceptual discovery, it was somewhat naïve of the young scholar to think that challenging the beliefs of such a powerful, intellectually established figure would not be answered with a strong rebuttal (Collins,1985:136; Evans,1998:143-144; Harvey,1981:105,113,124; Pinch,1994:96-97; Wali,1991:126-127).

Indeed, in responding to Chandrasekhar's findings, Arthur Eddington constantly stressed how absurd and irrational the theoretical implications of his junior colleague's model were, for they implied that a sufficiently massive star would only find gravitational peace when it had collapsed to a relatively small dimension, perhaps as small as a planet, or asteroid, or even a mathematical point. In this fashion, Eddington discursively and effectively placed Chandrasekhar's work in contrast with the socially-agreed to conceptualizations of stellar evolution dominating astrophysics at the time, one in which Fowler's white dwarf model constituted the only socially conceivable means by which a star would end its days. As the reader will recall, Fowler's research suggested that *all stars* irrespective of their mass would evolve into white dwarf stars,

and would remain in this state until they exhausted all of their nuclear fuel and faded into oblivion.

Not surprisingly, against such a cultural background, and in the light of a very narrow reservoir of socially-condoned interpretive resources concerning white dwarf structure, Chandrasekhar's conclusion that only some stars could retire in this fashion, leaving the more colossal stars to a fate in which they collapsed into neutron stars, or collapsed even indefinitely, appeared highly implausible to the astrophysical community, to say the least.

Thus, with his white dwarf model socially perceived as erroneous, due to the concentrated actions of one prominent scientist Chandrasekhar decided to completely withdraw from the debate and pursue other research interests in the field of astrophysics, thus, resulting in the breaking of the theoreticians' regress that had characterized this highly specialized topic area for approximately four years (Eddington,1937:137-139; Harvey,1981:124; M/RAS,1935:37-39; Penrose,1997:63; Pickering,1981:88-89; Wali,1991:145-146).

The above statements highlight how assessing the plausibility of a novel finding can be influenced by social, as well as cultural context. Indeed, it is highly conceivable that had Eddington chosen to do so, he could have made a case for his junior colleague's theory, albeit a cautious one. However, in doing so, he would have been forced to compromise a number of prior agreements concerning theoretical research into the physical configuration of white dwarfs, as well as his own firm conceptual and metaphysical commitments regarding stellar matter and the natural world. Nevertheless, the point is, with his towering stature and prominence in astrophysics, Eddington, of all people, had the luxury of cloaking Chandraseker's research with a certain measure of credibility had he wished, but he obviously did not, for socio-cultural reasons as cited above. Yet, this reality reinforces the argument posited in an earlier chapter and that being, what is or is not plausible is, at times, more strongly linked with social action than an objective, impartial examination of data or a mathematical calculation (Collins,1985:150; Harvey,1980:148-152; 1981:97-98; Pickering,1981:83).

Naturally, any discussion of what might have been, had Eddington chosen to support Chandrasekhar's intriguing discovery, rests purely in the realm of speculation and conjecture. However, it is important to consider alternative endings to the actual resolution of a controversy, otherwise that ending can, potentially, be perceived as the by-product of rational thought and debate (Harvey,1980:158). As Harvey argues,

> ...unless we are prepared to consider alternatives to the *actual* outcome, that outcome can easily be interpreted as inevitable and unproblematic. The whole process of social change within science then becomes either inaccessible to sociological analyses, or else describable only as the straightforward application of a generalized scientific methodology....(Harvey,1980:158).

A curious epilogue to this whole affair is that, many decades after his controversy with Eddington faded into memory, Chandrasekhar remained puzzled and seemingly exasperated over Eddington's behaviour with respect to his theory. As he wrote, commenting on Eddington's presentation before the RAS in which he characterized Chandrasekhar's conclusion that stars possess a limiting mass as a *reductio ad absurdum* of the relativistic degeneracy formula,

> It is clear from this statement that Eddington fully realized, already in 1935, that given the existence of an upper limit to the mass of degenerate configurations, one must contemplate the possibility of gravitational collapse leading to the formation of what we now call black holes. But he was unwilling to accept a conclusion that he so presciently drew; and he convinced himself that "there should be a law of nature to prevent a star from behaving in this absurd way!"
> ... For my part I shall only say that I find it hard to understand why Eddington, who was one of the earliest and staunchest supporters of the general theory of relativity should have found the conclusion that black holes may form during the natural course of the evolution of stars, so unacceptable.... (Chandrasekhar,1987:135).

In addition, in conversations with his biographer, Chandrasekhar speaks of how he believes that Eddington's staunch resistance to the notion of the limiting mass seriously delayed advanced research in the field of stellar mechanics for approximately sixty years. As Chandrasekhar said,

> It is quite an astonishing fact ... that someone like Eddington could have such an incredible authority which everyone believed in, and it is an incredible fact that in the framework of astronomy there were not people who had boldness enough and understanding enough to come out and say Eddington was wrong. I don't think in

> the entire astronomical literature you will find a single sentence to say Eddington was wrong. Not only that, I don't think it is an accident that no astronomical medal I have received mentions my work on white dwarfs ... The Heineman Prize in physics was the first one to mention it. It is ridiculous to talk about these things, but I personally believe that the whole development of astronomy, of theoretical astronomy, particularly with regard to the evolution of stars and the understanding of the observations relating to white dwarfs, were all delayed by at least two generations because of Eddington's authority (Chandrasekhar cited in Wali,1991:145).

It is clear from the above quotations that Chandrasekhar believed that Eddington *should have* accepted the theoretical implications of the limiting mass, and that his resistance to this idea led to a serious intellectual drought in theoretical understanding of stellar structure. As this thesis has demonstrated, however, there really was no overwhelming theoretical or empirical reason for Eddington to have embraced his junior colleague's findings, especially given the socio-cultural context in which it first emerged. Furthermore, even if Eddington had accepted the concept of the limiting mass, it is very difficult to say how this act would have affected astrophysical research; *recall, there was no way to empirically verify the validity of this abstract notion at the time.* Perhaps, it would have allowed Chandrasekhar's research to assimilate into the prevailing belief system of astrophysics a bit more rapidly, but this scenario is difficult to imagine given the fact that by its very existence, Chandrasekhar's work challenged prior social agreements concerning stellar evolution, and required a sharp re-conceptualization of a small corner of the natural world. As a result, there is no doubt that, in the very least, Edward Arthur Milne, whose own commitments in the field of stellar structure had been threatened by Chandrasekhar's analysis, would have strongly resisted acceptance of Chandrasekhar's theory, and a new regress would have, in all probability, emerged within the discipline.

In addition, a review of the literature following the years and decades after the 1939 Paris Conference, shows that scientists – despite Eddington's critique – were, nevertheless, using aspects of Chandrasekhar's comprehensive theory for the purposes of theoretical research into stellar structure and evolution. While research into the physical nature and composition of white

dwarfs were few and far between at first, their numbers grew steadily with the passage of time. Thus, perhaps, even if Eddington had endorsed Chandrasekhar's work, this pattern of scholarly production would not have changed by any significant amount.

Still, it is interesting to read Chandrasekhar writing how puzzled he was that Eddington could not see that the limiting mass was a consequence of a number of logical considerations. From the perspective of Chandrasekhar the scientist, Eddington's stance against the limiting mass is no doubt bewildering. However, from the perspective offered by SSK research, the specific academic position Eddington adopted in this particular controversy is understandable; in effect, it was a function of a number of factors ranging from problems in communication, metaphysical commitments to culture, all of which acted as obstacles to the dissemination and acceptance of the young Trinity Fellow's controversial research.

It is often difficult for scientists to acknowledge, let alone accept the notion that social dynamics play any role in the generation of scientific knowledge; Allan Franklin's criticism of Harry Collins' Experimenter's Regress, and Nobel Laureate Steven Weinberg's thoughts on the value of a sociological interpretation of scientific practice, seem to emphasize this point quite nicely[36] (Franklin,1994; Weinberg,1994).

Yet, I wonder, had Chandrasekhar lived to read this thesis, would he have accepted the sociological analysis put forth in this work concerning the controversy he had been engaged in with Eddington, or would he have dismissed the relevance of social factors with respect to this particular debate? I, for one, would like to believe he would have agreed, at least for the most part, with my interpretation of the controversy. I say this because, in all his writings on this

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[36] Ironically, as Collins points out, in his criticism of the Experimenters' Regress, Franklin inadvertently seems to substantiate the validity of this sociological phenomenon! (Collins,1994). As Collins writes, "In attacking the earlier account of the story of gravitational radiation, Franklin has effected a 'replication' of at least part of the history. This has confirmed part of my earlier account. It is a particularly strong confirmation, as Franklin is a powerful critic of the earlier interpretation. More powerful replications have also been carried out. These discover the same overall phenomena (experimenters' regress ...) in different areas of the sciences ... Thus, the notion of the experimenters' regress is not only one of the most thoroughly replicated phenomena in the social sciences, it has been replicated by some of its most hostile critics. Therefore Collins must be wrong!" (Collins,1994:503).

dispute, as well as his conversations with his biographer, Chandrasekhar implicitly and explicitly talks about the role that beliefs played in the controversy, as well as the notions of authority and prestige, and how they were arrayed against him; in effect, he essentially points to extra-scientific factors in his articulation of the debate – factors that almost sound sociological in nature[37](Chandrasekhar,1969;1987; Wali,142,144).

In his interesting semiotic analysis of a semi-popular monograph written by Albert Einstein to explain his theory of relativity, Bruno Latour talks about a particular style that all narrations employ in discussing a topic or scene, a style semioticians refer to as *shifting out* and *shifting in*; in the former case, the writer asks the reader to shift their attention away from the author to a specific element in the text; in the latter instance, the author, through the use of particular words, shifts attention back to him/herself (Latour,1988:3,5-6).

Now, one can use Latour's notions of shifting in and shifting out to argue that scientists reluctance to acknowledge the influence of social variables in the everyday practice of their work, is a direct result of their inability to *shift out* of the purely technical dimensions of their field, to view their practice in a holistic manner, in a fashion that would allow them to acutely witness the influence of sociological variables in the production of scientific knowledge. Indeed, one of the invaluable qualities of scientific controversies is that it explicitly reveals normally difficult to perceive social processes not only to sociologists, but scientists themselves, thus permitting a far

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[37] Chandrasekhar's explanation of the resistance he faced essentially concentrated on what he perceived as the arrogant certainty with which Eddington and Milne viewed reality. As he told Wali, "I don't think Eddington's tirade against me was derived from any personal motives ... You may attribute it to an elitist, aristocratic view of science and the whole world. Eddington was so utterly confident of his views that as far as he was concerned he was a Gulliver in a land of Lilliput ... The moral ... is that a certain modesty of approach toward science always pays in the end. These people [Eddington ... Milne], terribly clever, of great intellectual authority, terribly perceptive in many ways, lost out because they did not have the modesty to say, 'I am going to learn from what physics teaches me.' They wanted to dictate how physics should be"(Chandrasekhar cited in Wali,1991:142,144).

And, as he wrote about the controversy in an article published in the *American Journal of Physics*, "With the prestige of Eddington and Milne arrayed against me, I am afraid that I appeared to most astronomers of the thirties as Don Quixote" (Chandrasekhar,1969:584).

Of course, I have tried to present a more balanced – symmetrical – view of the whole controversy than that articulated by Chandrasekhar. Whether Chandrasekhar would have accepted my arguments in this respect, I do not know, but his comments above seem to tentatively suggest that he would not have opposed them.

more easier *shifting out* than would be customarily possible (Latour,1988; Pinch,1994:88). As Trevor Pinch wrote, "Under the lens of a scientific controversy the good, the bad, and the ugly within science come into focus as never before"(Pinch,1994:88).

In this respect, it would appear that Chandrasekhar was able to alter his perspective from the technical aspects of his controversy with Eddington, to the broader social context in which it existed, albeit in a limited sense. Nevertheless, at least in his commentary on his dispute with Eddington, Chandrasekhar seemed to acknowledge that cognitive activity does not occur in a social vacuum but is in effect, guided, moulded and negotiated in a socio-cultural context (Harvey,1981:125; Pickering,1981:88-89).

As is evident from the empirical features of the Chandrasekhar-Eddington controversy, Chandrasekhar's discovery of the limiting mass of stellar matter did not constitute a "revolutionary", alternative conceptualization to the dominant world-view of the ultimate fate of stars, in the true Kuhnian sense of the word; nor would it prove to be an insignificant framework for comprehending the behaviour of extremely dense matter; indeed, in the decades following his dispute with Eddington, Chandrasekhar's model slowly, yet steadily demonstrated its conceptual utility. As more and more white dwarfs came to be discovered, and their respective masses and radii were documented after much painstaking observational work, it was found that the observed masses and radii of every white dwarf star catalogued, succinctly followed the theoretical relationship predicted by Chandrasekhar in 1935. Moreover, with the discovery of the *pulsar* in 1967– a rapidly rotating neutron star[38] whose physical dimensions were many times smaller than a white dwarf – Chandrasekhar's work was elevated from the purely abstract and conceptual to the tangible and physical, in time fuelling advanced theoretical research into exotic objects such as black holes. Nevertheless, this vindication took approximately thirty-two years to come to pass, highlighting the reality that new ideas, ultimately, become entrenched in the culture of

[38] Neutron stars have diameters of only 30 to 60 kilometres (18.6 to 37.3 miles), and possess densities of approximately 400 trillion grams per cubic centimetre, in contrast to densities of one-million grams per cubic centimetre found in white dwarfs (Kaufmann,1988:433,438,449-451; Lasota,1999:42).

science via paths that are both natural and social in structure (Chandrasekhar,1972:167; Kaufmann,1988:438,450,618;Rudwick,1985:451;Wali,1982:40).

To this effect, the Chandrasekhar-Eddington controversy is an instructive case with which to understand how novel, conceptual ideas are assessed in a context in which the ability to empirically falsify anomalous claims is pragmatically difficult at best. In a time when theoretical astronomy was still in its youth, possessing a relatively small group of practitioners, with one particular individual endowed with an enormous amount of social capital, when answers to fundamental questions about astrophysical phenomena, such as how the sun generated its tremendous heat and temperature, were couched in highly speculative language, and where technological limitations prevented the gathering of sufficient quantities of empirical evidence to facilitate the assessment of rival, theoretical frameworks, the plausibility of a novel, conceptual discovery was strongly dependent on whether or not that discovery had qualities that resonated with the socially-acceptable interpretive resources of astrophysics, and was supported by authoritative practitioners in the field (Lankford,1997; Rudwick,1985:445,450-455).

In this respect, the Chandrasekhar-Eddington case study has reinforced past research (e.g. Collins,1975,185,1999; Harvey,1980,1981; Kennefick,2000; Pickering,1980,1981a,1981b,1992; Pinch,1994; Rudwick,1985; Simon,1999) which has shown the not insignificant influence socio-cultural forces play in the generation of knowledge claims, but, more importantly, has demonstrated how communal perception of the plausibility of a theoretical finding can be strongly shaped and moulded by the actions of one prominent, scientific figure, actions that effectively forced Chandrasekhar to withdraw from the dispute in light of the fact that social perception of his model – a perception that had been fostered and encouraged by Eddington – was decidedly negative.

Indeed, as suggested earlier, if Eddington had decided to support Chandrasekhar's intriguing finding, instead of spending so much energy in proving to his peers that it was a deviant idea, it is probable that other astrophysicists would have come to accept the surprising conclusion that not

all stars ended their lives as white dwarfs, as opposed to being convinced that it *must* be an erroneous discovery based on a flawed, technical apparatus.

When placed against the backdrop of past scientific controversies (e.g. Clark,1972; Frankel,1987; Gieryn,1999; Harvey,1980,1981; Klein, 1964; Mamiani,2000; Pickering,1981; Pinch,1977,1994; Rudwick,1987; Ruse,2000; Stewart,1986), Eddington's role in this particular controversy is unusual. Indeed, in most scientific disputes, two sides often engage in vigorous debate about the validity of a given knowledge claim; if the claim in question is highly surprising and challenges conventional beliefs, orthodoxy will usually put forth arguments stressing technical flaws in the logic of the opposing camp. While Eddington did the same in his dispute with Chandrasekhar, he seemed to go to an inordinate amount of trouble to stress the *heretical* nature of his opponent's work, using this as a foundation for his strictly technical objections to Chandrasekhar's model. Of course, one could interpret this discourse as a means of reinforcing the pre-existing belief that the white dwarf stage represented the terminal stage for all stars in the face of Chandrasekhar's alternative conceptualization, of solidifying the institutional validity of the orthodox view of stellar evolution, and the subsequent theoretical practice born of its entrenchment in the culture of astrophysics. Still, it was a curious strategy when placed against the historical backdrop of past intellectual disputations; why Eddington chose this particular style of academic debate is, perhaps, a question to which a definitive answer may not be possible (Collins,1985:150; Simon,2001).

In essence, the result of this study has suggested one possible sociological path by which the theoreticians' regress may be broken. Of course, an alternative conceptual path might have incorporated "interest-theory" in elucidating how the Chandrasekhar-Eddington controversy found closure; this model argues that scientists intellectual interests influences how they interpret experimental results (Barnes,1977;Galison,1987:10-11;Pickering,1980:236;1984; Shapin,1982:164-175; Woolgar,1981). As Pickering argues, "... scientific communities tend to reject data that conflict with group commitments and, obversely, to adjust their experimental

techniques and methods to "tune in" on phenomena consistent with those commitments..." (Pickering,1980:236).

In his examination of the debate between the validity of two, rival conceptual models in particle physics – charm and colour – Pickering argues that the charm model, eventually, won the debate because it was able to enrol the greatest number of professional interests spread across a wide network of sub-specialties, whereas the colour model failed to achieve a similar outcome. While the nature and structure of the "interests" involved in the Chandrasekhar narrative are not identical to Pickering's case study, it is possible to investigate this controversy from such a conceptual perspective (Galison,1987; Pickering,1980)

The following research agenda suggests future issues and hypotheses in the sociology of scientific knowledge that can be examined in detail to flesh out the arguments and concepts introduced in this project.

- With respect to what was stated above, one potential hypothesis related to the interest perspective could be investigated by future researchers is: ***a novel, theoretical finding that encompasses the greatest number and most diverse set of professional interests is more likely to be perceived as plausible, even in the absence of any empirical evidence, than a novel, theoretical finding which encompasses few or no professional interests.***

- It was argued that another potential variable that can play a critical role in giving rise to the theoreticians' regress – one which Daniel Kennefick did not discuss – was what I referred to as metaphysical[39] commitments. Indeed, Eddington – and to a lesser extent, Milne – had a firm metaphysical view

[39] For those with an interest in social psychology, the whole issue of metaphysical commitments and how those commitments can induce strong resistance to a novel, unorthodox finding, would appear to resonate with elements of Leon Festinger's cognitive dissonance theory. Indeed, Festinger argues that individuals need consistent knowledge about the reality that encompasses them. Inconsistent states of cognition often give rise to feelings of psychological tension and unease. In such circumstances, people feel compelled to neutralize such discomfort and resolve the inherent contradiction. With respect to the Chandrasekhar-Eddington controversy, one could argue that Eddington and Milne's strong reaction against Chandrasekhar's theory were a result of cognitive dissonance. Indeed, both scientists were no doubt bothered by the strong inconsistency between their firm belief in the validity of Fowler's model, and their knowledge that Chandrasekhar's comprehensive theory forcefully presented an alternative conceptual picture of stellar evolution. In this sense, both scientists neutralized the resulting dissonance by arguing that Chandrasekhar's theory was technically flawed and that an appropriate application of the special theory and quantum statistics would reaffirm the notion that all stars retired as white dwarfs (Festinger et. al,1956:3-6; Berkowitz,1980:88; McDonagh,1976; Penrod,1986:111). While the above application of cognitive dissonance to the current case study is simplistic, it does highlight the complex socio-psychological dynamics inherent in scientific controversies. While sociologists tend to ignore psychological factors in the examination of scientific practice, perhaps the time has come for a more co-operative approach between the two disciplines (Krohn,1980:xix).

concerning the structure of reality, a view that ultimately constituted a very influential factor in his criticism of Chandrasekhar's derivation of the limiting mass concept and its theoretical implications concerning the fate of white dwarf stars. As such, one is forced to wonder how such an epistemological perspective arises in the first place, and why different scientists, at times, subscribe to quite divergent metaphysical beliefs?

Andrew Pickering (e.g. 1980,1981a,1984;1992;1995) has exhaustively studied the social nature of theory construction, yet very few works, if any, have explored how and why *individual* scientists adopt the particular philosophical beliefs that they do concerning the composition of the physical world, and what role such beliefs play in the context of scientific disputes. I would argue that an answer to this train of thought cannot rest solely in the realm of the purely psychological, since many of the beliefs we hold as true and absolute are a function of social negotiation that transpired at some point in the past; the truth content of these beliefs are subsequently reinforced and legitimated due to the support of specific authority figures and institutions, allowing them to circulate within the social boundaries of a given culture much like currency (Harvey,1981:124-125; Shapin,1994:5-7; Simon,1999:79). In this sense, one can state that "truth is conceivable only as a socially organized upshot of contingent courses of linguistic, conceptual, and social behaviour"(McHugh cited in Harvey,1981:124).

Yet, the question remains, why do individual scientists socialized within a given specialized, disciplinary culture, and thus sharing a commitment to certain fundamental beliefs and principles of this culture, nevertheless, still adopt differing epistemological views concerning a specific topic or problem area in their specialty? Such a question cannot be effectively answered solely through the analysis of scientific controversies, for it touches on a wide spectrum of complex social, cultural and technical factors that scientists are exposed to during the course of their training and subsequent careers, and which become cognitively solidified long before the scientist finds him/herself embroiled in an intellectual controversy. As such, having touched upon the issue of metaphysics in this project, I would recommend a detailed exposition of the genesis, evolution, and even transformation, of metaphysical beliefs held by scientists, beliefs that guide not only how scientists perceive the physical reality they study, or approach a given problem, but how they respond to the emergence of knowledge claims that forcefully contradict these views.

In light of these comments, one could investigate the following hypothesis: ***the nature of a scientist's initial exposure, training and specialization in a given field, influences his/her metaphysical beliefs concerning a given physical phenomenon.***

- Lastly, this study has focussed exclusively on comprehending how the plausibility of a novel, theoretical concept was assessed, in a context in which recourse to observational techniques for testing the validity of that concept did not exist; in light of this reality, a circular pattern of argument manifested itself between the author of the anomalous finding and its primary critic, one that could not be broken by simple appeal to logic and objectivity. At this point, one is forced to wonder if it is easier to assess the plausibility of an unorthodox theoretical idea, if that idea is susceptible to experimental or observational testing? The instinctive answer may be, yes, but studies by Collins

> (1975,1985,1999), Galison (1987), Pickering (1981a,1981b,1984), Pinch (1981,1994), Simon (1999) have shown that experiments do not always provide neat, neutral, conclusive avenues by which the validity and reality of theoretical entities may be conclusively resolved.
>
> In truth, it would be most instructive if sociologists of science were to examine, in detail, the similarities and differences between how the plausibility of a novel theoretical concept was assessed when recourse to experimental or observational tests were available, in contrast to situations in which such tests were not feasible. Indeed, one could phrase a potential, workable hypothesis designed to explore this issue as follows: ***the plausibility of a new, theoretical concept is easier to assess if it can be subjected to empirical testing, then when it cannot be subjected to empirical testing.***

The argument and analysis presented in this project has demonstrated that assessing the plausibility of a surprising, theoretical idea is not a straight-forward, impartial process but is influenced by socio-cultural factors and the context in which a new concept emergences into the public domain. It has also presented one possible avenue by which the theoreticians' regress may be broken, and has suggested an alternative approach that may prove instructive to explore in future investigations on this topic. Lastly, this study has suggested a number of complex issues[40] often found in scientific controversies that need to be researched and fleshed out in further detail by scholars, issues that highlight the reality that the practice of science is an intricate, multifaceted endeavour that is sure to keep the interest of historians, philosophers and sociologists of science for a long time to come.

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[40] As a final note, one significant issue this thesis has highlighted is the complex personal dynamics between senior and junior scientists. In a very recent article, Jason Owen-Smith focuses upon this dynamic in the context of laboratory research, arguing that the skepticism of senior scientists toward research conducted by junior colleagues is a means by which these experienced researchers reinforce their intellectual authority to the younger generation. The full reference to this article is, **"Managing Laboratory Work Through Skepticism. Process of Evaluation and Control."** *American Sociological Review*, 66: 427-452, 2001.

APPENDIX A: STANDARD ETHICS PROTOCOL

My name is Alex Ipe, and I am a doctoral student working on a project entitled, ***"Plausibility and the Theoreticians' Regress: Constructing the Evolutionary Fate of Stars."***

This is a research project for the Doctoral Degree in Sociology, being conducted at Carleton University, Ottawa, Ontario, Canada. My thesis supervisor is Dr. Charles Gordon.

I am the principal investigator of the project and I may be contacted at this phone number: 728-4450, or at my e-mail address: aipe@ccs.carleton.ca, should you have any questions regarding the project.

Thank you for your willingness to participate in the research project. Your participation is greatly appreciated. Just before we start the interview, I would like to reassure you that as a participant in this project, you have definite rights.

First, your participation in this interview is entirely voluntary.

You are free to refuse to answer any question at any time.

You are free to withdraw from the interview at any time.

This interview will be kept strictly confidential, and will be available only to myself and my doctoral committee.

The interviews will be recorded on a tape recorder only with your permission.

Excerpts of the interview will be made part of the final dissertation report, and no identifying characteristics will be included in this report without your permission.

I would be grateful if you would sign this form to show that I have read you its contents.

_____ (signed)

_____ (printed)

_____ (dated)

Please send me a report on the results of this research project.

Yes No

Source: **McCracken,1998: Appendix A.**

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