A Thermodynamic History of the Solar Constitution — II: The Theory of a Gaseous Sun and Jeans' Failed Liquid Alternative

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In this work, the development of solar theory is followed from the concept that the Sun was an ethereal nuclear body with a partially condensed photosphere to the creation of a fully gaseous object. An overview will be presented of the liquid Sun. A powerful lineage has brought us the gaseous Sun and two of its main authors were the direct scientific descendants of Gustav Robert Kirchhoff: Franz Arthur Friedrich Schuster and Arthur Stanley Eddington. It will be discovered that the seminal ideas of Father Secchi and Hervé Faye were not abandoned by astronomy until the beginning of 20th century. The central role of carbon in early solar physics will also be highlighted by revisiting George Johnstone Stoney. The evolution of the gaseous models will be outlined, along with the contributions of Johann Karl Friedrich Zöllner, James Clerk Maxwell, Jonathan Homer Lane, August Ritter, William Thomson, William Huggins, William Edward Wilson, George Francis FitzGerald, Jacob Robert Emden, Frank Washington Very, Karl Schwarzschild, and Edward Arthur Milne. Finally, with the aid of Edward Arthur Milne, the work of James Hopwood Jeans, the last modern advocate of a liquid Sun, will be rediscovered. Jeans was a staunch advocate of the condensed phase, but deprived of a proper building block, he would eventually abandon his non-gaseous stars. For his part, Subrahmanyan Chandrasekhar would spend nine years of his life studying homogeneous liquid masses. These were precisely the kind of objects which Jeans had considered for his liquid stars.

1 The search for a continuous thermal spectrum: Carbon particles on the Sun?

Consider particulate matter floating on a gaseous globe. Such was the idea advanced by Father Angelo Secchi and Hervé Faye as they described the photosphere of the Sun [1]. But what was this particulate matter? For Faye, a subtle allusion was made to carbon within the gaseous flame [2, p. 296]. As a result, the marriage between Faye's model and graphite was almost immediate. Graphite, or at least some form of condensed carbon, remained on the surface of the Sun until the 1920's. Even the pioneering treatment of a gaseous Sun, by Jonathan Homer Lane, referred to the carbon envelope of the photosphere, as demonstrated in Section 2.2. Thus, it was only through Eddington and his inception of a *fully* gaseous Sun [3] that particulate matter was finally removed from the photosphere.

If carbon played a pre-eminent role in solar theory, it was because of the need to understand the continuous spectrum of the photosphere. On Earth, only graphite and soot were known to produce such a spectrum. As the common form of condensed carbon, graphite possessed outstanding refractory properties. The material did not melt. Rather, it sublimed at extreme temperatures [4]. It seemed to be the perfect candidate for introducing condensed matter on the Sun in order to generate the solar spectrum. Moreover, from the earliest studies on thermal radiation [5, 6], graphite and soot played a dominant role [7]. Balfour Stewart [8] who, along with Gustav Kirchhoff [9], was one of the fathers of thermal emission, emphasized the crucial role of carbon in heat radiation: "*Indeed, it is only the light from a black body that represents by itself the brightness of the enclosure, and such a body, when taken out and hastily examined in the dark, without allowing it time to cool, will be found to give out rays having a brightness in all respects the same as that of the enclosure in which it was placed, because being opaque and non-reflective, all the light which it gave out in the enclosure was proper to itself, none having passed through its substance or been reflected from its surface; it therefore retains this light when taken into the dark, provided its temperature is not in the meantime allowed to fall*" [10, p. 277–278]. Experimental blackbodies of the 19th century were manufactured using either graphite, or soot [7], precisely because such carbon surfaces were not transparent and exceeded all others in being devoid of reflection.

In 1867, less than two years after Secchi and Faye [1] had conceived their solar model, G.J. Stoney explicitly placed carbon on the Sun: "*We have strong reasons for suspecting that the luminous clouds consists, like nearly all the sources of artificial light, of minutely divided carbon; and that the clouds themselves lie at a very short distance above the situation in which the heat is so fierce that carbon, in spite of its want of volatility, and of the enormous pressure to which it is there subjected, boils. The umbra of a spot seems never to form* *unless when the region in which carbon boils is carried upwards, or the hot region above the clouds is carried downwards, so as to bring them into contact, and thus entirely obliterate the intervening clouds…*" [11]. Stoney's proposal introduced graphite particles in the photosphere, while reaffirming Faye's contention that the Sun was devoid of a distinct surface [1]. These words were to guide solar physics for two generations.

For instance, in 1891, during his Inaugural Address before the British Association, William Huggins stated: "*The Sun and stars are generally regarded as consisting of glowing vapours surrounded by a photosphere where condensation is taking place, the temperature of the photospheric layer from which the greater part of the radiation comes being constantly renewed from the hotter matter within… Consequently, we should probably not go far wrong, when the photosphere consists of liquid or solid particles, if we could compare select parts of the continuous spectrum between the stronger lines, or where they are fewest… The brightness of a star would be affected by the nature of the substance by which the light was chiefly emitted. In the laboratory, solid carbon exhibits the highest emissive power. A stellar stage in which radiation comes, to a large extent, from a photosphere of solid particles of this substance, would be favourable for great brilliancy… It may be that the substances condensed in the photosphere of different stars may differ in their emissive powers, but probably not to a great extent*" [12, p. 375–376].

Overall, the *Inaugural Address* amplified the search to understand the continuous nature of the solar spectrum. Huggins was a central figure in the history of solar astronomy and lived just prior to the conceptualization of a *fully* gaseous Sun. As such, it is almost as if his mind was suspended between two separate physical realities. He oscillated between a carbon containing photosphere as a source of light and a continuous spectrum produced exclusively by gases: "*We must not forget that the light from the heavenly bodies may consist of the combined radiations of different layers of gas at different temperatures, and possibly be further complicated to an unknown extent by the absorption of cooler portions of gas outside*" [12, p. 373]. The presentation by Huggins demonstrates a strained application of logic. Immediately after stating that: "*Experiments on the sodium spectrum were carried up to a pressure of forty atmospheres without producing any definite effect on the width of the lines which could be ascribed to the pressure. In a similar way the lines of the spectrum of water showed no signs of expansion up to twelve atmospheres; though more intense than at ordinary pressures, they remained narrow and clearly defined*" [12, p. 373], he writes: "*It follows, therefore, that a continuous spectrum cannot be considered, when taken alone, as a sure indication of matter in the liquid or the solid state*" [12, p. 373]. The experiments just described were contrary to the result sought. Ultimately, there could be no evidence that a gas could produce a blackbody spectrum simply by being pressurized. The spectrum may well have gained a continuous nature, but never with the proper blackbody shape. Huggins continued: "*Not only, as in the experiments already mentioned, such a spectrum may be due to gas when under pressure, but, as Maxwell pointed out, if the thickness of a medium, such as sodium vapor, which radiates and absorbs different kinds of light, be very great, and the temperature high, the light emitted will be of exactly the same composition as that emitted by lampblack at the same temperature, for the radiations which are feebly emitted will also be feebly absorbed, and can reach the surface from immense depths*" [12, p. 373]. In bringing forth these ideas from Maxwell, Huggins was abandoning the carbon containing photosphere.

James Maxwell wrote extensively about the theory of heat radiation [13]. He was well acquainted with Stewart and claimed: "*Professor Balfour Stewart's treatise contains all that is necessary to be known in order to make experiments on heat*" [13, p. vi]. In this regard, Maxwell's text contains many of the same ideas [13, p. 210–229] found in Stewart's works [14]. Maxwell's treatise also contained the classic lines previously invoked by Huggins [12, p. 373]: "*If the thickness of a medium, such as sodium-vapour, which radiates and absorbs definite kinds of light, be very great, the whole being at a high temperature, the light emitted will be exactly the same composition as that emitted from lampblack at the same temperature. For though some kinds of radiation are much more feebly emitted by the substance than others, these are also so feebly absorbed that they can reach the surface from immense depths, whereas the rays which are so copiously radiated are also so rapidly absorbed that it is only from places very near the surface that they can escape out of the medium. Hence both the depth and the density of an incandescent gas cause its radiation to assume more and more the character of a continuous spectrum*" [13, p. 226]. This conjecture, by Maxwell, was never validated in the laboratory. Sodium gas could not approach the blackbody spectrum under any circumstances, especially in the absence of a perfectly absorbing material. Even modern high pressure sodium lamps [15] could not produce the required spectrum. Their real emission was far from continuous and not at all like a blackbody [15, p. 23]. Nonetheless, Maxwell's theory became an anchor for those who believed that gases, if sufficiently thick, could produce a blackbody spectrum.

Astrophysics stood at an impasse between the need for carbon and its elimination from the solar body. Soon after Huggins delivered his famous address, William Wilson would approach the same subject in these words: "*Solar physicists have thought that the photosphere of the Sun consists of a layer of clouds formed of particles of solid carbon. As the temperature of these clouds is certainly not below 8000° C., it seems very difficult to explain how carbon can be boiling in the arc at 3500° and yet remain in the solid form in the Sun at 8000°. Pressure in the solar atmosphere seemed to be the most likely cause of this, and yet, from other physical rea-*

sons, this seemed not probable" [16]. Wilson goes on to state: "*carbon may exist in the solid form at very high temperatures although the pressures are comparatively low*" [16]. He was arguing in favor of solid carbon on the Sun despite the elevated temperatures. In 1897, along with George FitzGerald, Wilson would reaffirm his conviction while advancing an alternative for sunspots: "*Dr. Stoney called attention to an action of this kind that might be due to clouds of transparent material, like clouds of water on the Earth, but in view of the high solar temperature it seems improbable that any body, except perhaps carbon, could exist in any condition other than the gaseous state in the solar atmosphere; so that it seems more probable that Sun-spots are due, at least partly, to reflections by convection streams of gas, rather than by clouds of transparent solid or liquid particles*" [17].

Despite Huggins' *Inaugural Address*, Robert Ball, the Lowndean professor of astronomy and geometry at Cambridge, also reemphasized the central role of carbon in the structure of the Sun at the end of the 19th century: "*The buoyancy of carbon vapor is one of its most remarkable characteristics. Accordingly immense volumes of the carbon steam in the Sun soar at a higher level than do the vapors of the other elements. Thus carbon becomes a very large and important constituent of the more elevated regions of the solar atmosphere. We can understand what happens to these carbon vapors by the analogous case of the familiar clouds in our own skies… We can now understand what happens as the buoyant carbon vapors soar upwards through the Sun's atmosphere. They attain at last to an elevation where the fearful intensity of the solar heat has so far abated that, though nearly all other elements may still remain entirely gaseous, yet the exceptionally refractory carbon begins to return to the liquid state. At the first stage in this return, the carbon vapor conducts itself just as does the ascending watery vapor from the earth when about to be transformed into a visible cloud. Under the influence of a chill the carbon vapor collects into a myriad host of little beads of liquid. Each of these drops of liquid carbon in the glorious solar clouds has a temperature and a corresponding radiance vastly exceeding that with which the filament glows in the incandescent electric lamp. When we remember further that the entire surface of our luminary is coated with these clouds, every particle of which is thus intensely luminous, we need no longer wonder at that dazzling brilliance which, even across the awful gulf of ninety-three millions of miles, produces for us the indescribable glory of daylight*" [18].

The idea that the photosphere consisted of carbon containing luminous clouds would be echoed by almost every prominent astronomer of the 19th century, from Simon Newcomb [19, p. 269] to Charles Young [20, p. 194]. The finest spectroscopists, including John Landauer and John Bishop Tingle [21, p. 198–200], joined their ranks. Even in 1913, the ideas of Johnstone Stoney [11] were mentioned throughout much of professional astronomy, as reflected by the writings of Edward Walter Maunder [22]. Mauder, who had discovered the great minimum in the sunspot cycle, wrote about the solar constitution in these words: "*The Sun, then, is in an essentially gaseous condition, enclosed by the luminous shell which we term the photosphere. This shell Prof. C. A. Young and the majority of astronomers regard as consisting of a relatively thin layer of glowing clouds, justifying the quaint conceit of R. A. Proctor, who spoke of the Sun as a "Bubble"; that is a globe of gas surrounded by an envelope so thin in comparison as to be mere film. There has been much difference of opinion as to the substance forming these clouds, but the theory is still widely held which was first put forward by Dr. Johnstone Stoney in 1867, that they are due to the condensation of carbon, the most refractory of all known elements. Prof. Abbot, however, refuses to believe in a surface of this nature, holding that the temperature of the Sun is too high even at the surface to permit any such condensation*" [22].

Change was eminent and graphite was soon irrevocably cast out of the photosphere. In their 1885 classic text *On Spectrum Analysis*, Henry Roscoe and Arthur Schuster [23, p. 229–264] had already chosen to neglect the prevailing ideas relative to solar constitution. Arthur Schuster [24, 25] was soon to prepare his report on *Radiation through a Foggy Atmosphere* [26, 27]. With its publication, the decisive step towards the fully gaseous Sun would be taken and graphite soon forgotten.

2 The rise of theoretical astrophysics

Through Secchi and Faye [1], observational astronomers gazed upon a gaseous Sun. They could only dream of what they had created, as the concept of an ethereal star had evolved virtually in the complete absence of mathematical guidance. At the same time, though the photosphere maintained some semblance of condensed matter, the introduction of a tenuous solar interior provided a compelling invitation to theoretical study. If the Sun was truly a gas, then perhaps some understanding could be harnessed through the ideal gas law, which had been discovered by Clapeyron [28]. In contrast, William Herschel's solid Sun was devoid of such appeal [1]. The same was true true for Spencer's model. Though his solar interior was gaseous, his photosphere was liquid [1].

As for a fully gaseous Sun, the idea was full of theoretical promise. But was the interior of the Sun truly gaseous? For men of the late 19th and 20th century, there could be no question of this reality, in light of Andrews' discovery of critical temperatures [29]. Alfred Fisen would leave no doubt as to the importance of critical phenomena for solar models: "*The question as to the physical conditions existing in the interior of the Sun is attended with graver difficulty… When the necessity for the interior heat of the Sun being at least as high as that of its exterior became recognized, the solid globe was generally replaced by an ocean of molten matter. It is, however, scarcely possible to regard as existing in the interior of* *the Sun, matter in either the solid or in the liquid condition... It was for a time regarded as barely possible that the enormous pressure that must exist at great depths in the interior of the Sun might be effective in maintaining matter in the solid or liquid condition in spite of the high temperature, since it is a familiar fact in laboratory experience, that liquefaction of a gas is in every case assisted by pressure, and may in many instances apparently be affected by it alone. Since, however, it became apparent from the classic research of Dr. Andrews in 1869, that there exists for every element a critical temperature, above which it is impossible for it under any conditions of pressure to assume the liquid state, it has generally been regarded that the liquid interior to the Sun is next to an impossibility*" [30, p. 36–37]. Armed with Andrews' discovery, the path seemed clear. Much of theoretical physics adopted a gaseous solar interior. They would eventually move forward to a *fully* gaseous structure, undaunted by the prospect that graphite or soot remained unchallenged as unique sources of blackbody spectra on Earth.

2.1 Friedrich Zöllner's protuberances: The laws of gases and the solar constitution

Zöllner was amongst the first scientists to apply the laws of gases to the study of the solar constitution [31, 32]. He attempted to understand the nature of solar protuberances, considering both eruptive flares and prominences. These works were important for two reasons: 1) Zöllner mathematically addressed the internal temperature for the Sun [33, 34] and 2) he highlighted that flares could not be easily explained when the Sun was considered *fully* gaseous. Using an atmospheric temperature of 27,700°C, Zöllner surmised that, at a depth lying 1/36th of the solar radius from the surface, the solar temperature approached 68,400°C [31].

Zöllner reasoned that eruptive protuberances, or solar flares, must occur because "*of a difference in pressure between the gases in the interior and those on the surface of the Sun*" [31]. In order to have an interior and an exterior, a boundary was certainly needed. Zöllner envisioned: "*Respecting the physical constitution of this layer, the further assumption is necessary that it is in some other state than the gaseous. It may be either solid or liquid. In consequence of the high temperature the solid state is excluded; and we must therefore conclude that the layer of division consists of an incandescent liquid*" [31]. Zöllner actually considered two models: "*Respecting the mass of hydrogen enclosed by this liquid layer, two suppositions appear to be possible*" [31]. The first was essentially a restatement of Spencer's "Bubble Sun" [35, 36] — a liquid photosphere with a gaseous interior [1]: "*The whole interior of the Sun is filled with glowing hydrogen, and our luminary would appear like a great bubble of hydrogen surrounded by an incandescent atmosphere*" [31]. At the same time, he considered a second situation in which the Sun was essentially liquid throughout while containing pockets of gas: "*The masses of hydrogen which are thrown out in these volcanic outburts are local aggregations contained in hollow spaces formed near the surface of an incandescent liquid mass, and these burst through their outer shell when the increased pressure of the materials in the interior reaches a certain point*" [31].

Zöllner would look back to Kirchhoff [37] and created a strange mix with the ideas of Secchi [38, 39] and Faye [40]. He placed the *fully* liquid layer, required in the interior of the Sun, at the level of the umbrae of sunspots [31, p. 319–320]: "*Hence it follows that the radius of the visible disk need not be necessarily identical with that of the supposed layer of separation, but that this latter may probably be assumed to lie below the point at which the hydrogen gas under compression evolves a continuous spectrum*" [31]. In doing so, Zöllner maintained the importance of the liquid layer in a manner completely independent of the need to generate the thermal spectrum. The enclosure provided by the liquid was required for the generation of flares. In fact, Zöllner argued against the need for condensed matter in producing the thermal spectrum: "*It is thus clear that it is not necessary, in order to explain the presence of dark lines in the solar spectrum, to assume that the continuous spectrum is produced by the incandescence of a solid or liquid body; for we may with equal right consider that the continuous spectrum is produced by the glowing of a powerful compressed gas*" [31]. By introducing this new layer, Zöllner advanced another reason why the Sun must possess condensed matter.

In treating the second scenario, that of a fully liquid Sun with pockets of gas, Zöllner made several arguments leading to a liquid solar interior: "*If we assume that the highest limit of specific gravity of this layer is the mean specific gravity of the Sun, we shall have to assume that all the deeper-lying layers, and therefore the sill deeper-lying gaseous layer, have the same temperature. But then the interior of the Sun would not consist of a gas, but of an incompressible liquid... In this case, however, the first supposition change into the second, according to which the Sun consists of an incompressible liquid...*" [31]. After completing several calculations, he then argued that pressures were rapidly increasing towards the solar interior. On this basis, the Leipzig professor rendered plausible the concept that the interior of the Sun could be liquid, despite high temperatures [31, p. 324].

In his second treatment on the solar constitution, Zöllner concentrated on determining the temperature of the chromosphere [32] and on refining the mathematical approach he had previously adopted. The 1873 article emphasized that line broadening could be affected by pressure, temperature, and optical thickness of the sample [32]. In this regard, Zöllner was concerned with the quantity of luminous particles in the line of sight of the observer. As such, he elucidated the complex considerations involved in obtaining temperatures and densities from the line widths of gases near the solar surface. Zöllner's second treatise was devoid of the complex solar theories which had characterized his first work [31].

2.2 Jonathan Homer Lane: A gaseous Sun endowed with condensed matter

In his *Memoire*, Cleveland Abbe presented a detailed picture of J. Homer Lane [41]. Lane considered Helmholtz's theory and Espy's theory of storms, while applying the ideal gas law to the Sun [42]. In so doing, he became the first scientist to build a truly mathematical model of a gaseous star. Like Einstein, Lane had worked as a patent examiner. He was said to have been quiet and lacking the fluency of speech [41]. Lane was never married and he was personally known to only a few people [41, p. 259]. He was deeply religious and he displayed many marks of simple nobility. Cleveland recounted these in the words of Byron Sunderland: "*Of the propriety, integrity, and simplicity of his life, of his exceeding conscientiousness and carefulness and his modest shrinking from all self-assertion or ostentation, we all well know. He was not what we should style a demonstrative man. He lived quietly within himself, and his life was engrossed in scientific pursuits. The nature and construction of his mind was purely mathematical. This was evident in the exactitude of his language, even in the most casual conversations and the most trivial subjects*" [41, p. 261].

Stevenson-Powell provided a detailed and extensive review of Lane's classic work on the theoretical modeling of a gaseous Sun [43]. In his approach to science, Lane was not unlike Eddington [44] and chose to consider the Sun as a theoretical physicist. He proposed a model and then considered the ramifications [43, p. 190], tackling a question by extrapolating from the known laws of physics. At the same time, "*Lane had little interest in the physical appearance of the Sun, and none at all in the spectral discoveries that increasingly influenced ideas about the Sun during the 1860s*" [43, p. 183]. The same could be said of Eddington [44].

Lane was responsible for advancing the first of the polytropic gas spheres. He was followed in this endeavor primarily by August Ritter [45], William Thomson (Lord Kelvin) [46], and Robert Emden [47]. Subrahmanyan Chandrasekhar provided a detailed treatment of polytropes in his classic text *An Introduction to the Study of Stellar Structure* [48, p. 84–182] whose bibliographical notes included excellent summaries of all key contributions in this subject area. Eddington also discussed the polytropes in *The Internal Constitution of the Stars* [44, p. 79–96].

Lane based his theoretical contribution on the ideal gas and Espy's theory of storms, advanced more than twenty years earlier [42]. But, the concept that the Sun was an ideal gas created obstacles. Stevenson-Powell recounted this fact, citing Arthur Eddington: "*In Lane's time there was no evidence that any star existed for which the theory of a perfect gas would be applicable*" [43, p. 190]. While the work of Andrews on critical temperatures was already well recognized [29], many failed to completely abandon the idea that the Sun contained at least some condensed matter.

In spite of these difficulties, the American scientist viewed the Sun as a gaseous sphere possessing a condensed exterior. He opened his classic paper as follows: "*Some years ago the question occurred to me in connection with this theory of Helmholtz whether the entire mass of the Sun might not be a mixture of transparent gases, and whether Herschel's clouds might not arise from the precipitation of some of these gases, say carbon, near the surface, with the revaporization when fallen or carried into the hotter subjacent layers of atmosphere beneath; the circulation necessary for the play of this Espian theory being of course maintained by the constant disturbance of equilibrium due to the loss of heat by radiation of the precipitated clouds*" [42]. Lane was replaying the ideas of Stoney, Secchi, and Faye [11, 38–40]. Nonetheless, the study of Lane's private notes revealed an unpublished paper from 1867 *The Sun viewed as a gaseous body* [43, p. 186]. In these unpublished notes, Lane claimed priority of ideas and wrote: "*The within formulae were written down about the year 1863 (perhaps earlier) considering the credibility of the Sun being a gaseous body, sustaining its heat by the descent of its mass in cooling, and keeping up by its circulation a continual precipitation of (carbon?) vapor in the photosphere, and the continual re-vaporization of the carbon? in the interior, after the philosophy of terrestrial storms as explained by Espy. Conclusion: it seemed evident the Sun's gaseous constitution could not be credibly referred to the laws of the gases, so far as they are known. J.H.L. May 1867*" [43, p. 187]. It appeared that Lane might have conceived of a gaseous Sun independently, in 1863. However, it would be difficult to conceive that such similarity with the well-known works of Secchi and Faye was purely coincidental [38–40]. Lane properly claimed that Faye's theory was "*seriously lacking*" [42]. The 1865 articles, by the French author, were devoid of mathematical treatment [1]. Through Lane's work, carbon was once again mentioned. Hence, even in the first truly theoretical work on a gaseous Sun [42], the emissivity of graphite maintained its powerful undercurrent.

2.2.1 Lane and convective equilibrium

Interestingly, Lane used the concept of convective equilibrium as a footnote to his first equation [42]. William Thomson had proposed the existence of convective equilibrium in 1862 and applied the idea to a gaseous Sun in 1887 [46]. By this time, Lord Kelvin had abandoned his original idea that the Sun was liquid [1]. Convective equilibrium would become one of the great building blocks of the theory of a gaseous Sun. Chandrashekhar would cite Kelvin's understanding of convective equilibrium in his classic text [48, p. 85]: "*If a gas is enclosed in a rigid shell impermeable to heat and left to itself for a sufficiently long time, it settles into the condition of gross-thermal equilibrium by 'conduction of heat' till the temperature becomes uniform throughout. But if it were stirred artificially all through its volume, currents not considerably disturbing the static distribution of pressure and* *density will bring it approximately to what I have called convective equilibrium of temperature. The natural stirring produced in a great fluid mass like the Sun's by the cooling at the surface, must, I believe, maintain a somewhat close approximation to convective equilibrium throughout the whole mass*" [46].

Convective equilibrium was a strange allusion, given that convection, by definition, was a non-equilibrium process. Convection existed as a result of the second law of thermodynamics, a principle first outlined by Clausius [49, 50] and ironically, by William Thomson [51]. To call for convective equilibrium "artificially" implied a violation of the first law of thermodynamics. To invoke it on the Sun, was a violation of the second law. Convective equilibrium could never exist, either on or within the Sun precisely because, by its very nature, convection was a non-equilibrium process. True system equilibrium required that both conduction and convection be absent. In Lane's case, recourse to convective equilibrium for his mathematics was particularly unusual, given that he had opened his manuscript with the statement that: "*the circulation necessary for the play of this Espian theory being of course maintained by the constant disturbance of equilibrium due to the loss of heat by radiation of the precipitated clouds*" [42]. How could a theory of storms ever form the basis for invoking convective equilibrium?

2.2.2 Lane and the temperature of the solar surface

The final portion of Lane's paper centered on elucidating the temperature at the upper visible solar surface. He reached the conclusion that this number must not be too far from 54,000°F and raised an objection to Faye's model: "*It must be here recollected that we are discussing the question of clouds of solid or at least fluid particles floating in a non-radiant gas, and constituting the Sun's photosphere. If the amount of radiation would lead us to limit the temperature of such clouds of solids or fluids, so also it seems difficult to credit the existence in the solid or fluid form, at a higher temperature than 54,000° Fah. of any substance that we know of*" [42].

Though Lane adopted Faye's model as a point of departure, he was open, though non-committal, to the idea that the Sun was fully gaseous: "*Dr. Craig, in an unpublished paper, following the hint thrown out by Frankland, is disposed to favor the idea that the Sun's radiation may be the radiation of hot gases instead of clouds. At present, I shall offer no opinion on that point one way or another, but will only state it as my impression that if the theory of precipitated clouds, as above presented, is the true one, something quite unlike our present experimental knowledge, or at least much beyond it, is needed to make it intelligible*" [42]. Craig was referring to the classic paper by Lockyer and Frankland discussed in Part I of this work [1]. Clearly, Lane had strong reservations relative to Faye's model, even though it formed the basis for much of his own presentation.

Lane advanced two ideas to uphold the precipitated cloud theory. In the first, he invoked Clausius' work on the specific heat of gases, using the idea that hydrogen might be able to exist, either in atomic or molecular form [42]. This was a novel concept at the time and Lane believed that the precipitated cloud model could be preserved through its introduction. However, the most fascinating defense was found in his second hypothesis which he believed was not very sound and dismissable with very little reflection [42]. Interestingly, in this hypothesis, Lane abandoned varying densities in the solar interior and created the requirements for a liquid Sun, apparently without realizing the obvious change in phase and the profoundness of his own writings. Lane advanced the possibility that "*in the Sun's body the average length of the excursion made by each molecule between two consecutive collisions, becomes very short compared to the radius of the sphere of repulsion of molecule for molecule, and with the average distance of their centers at nearest approach. This way of harmonizing the actual volume of the Sun with a temperature of 54,000° Fah. in the photosphere, and with the smallest density which we can credit the photosphere, would involve the consequence that the existing density of almost the entire mass of the Sun is very nearly uniform and at its maximum possible, or at all events that any further sensible amount, comparatively, of renewed supplies of heat, for the obvious reason that this hypothesis carries with it almost the entire neutralization of the force of gravity by the force of molecular repulsion*" [42]. Lane, without direct reference, was calling for a liquid Sun. He concluded: "*Another thing involved in this second hypothesis is the fact which Prof. Peirce has pointed out to the Academy, viz: that the existing molecular repulsion in the Sun's body would immensely exceed such as would be indicated by the modulus of elasticity of any form of matter known to us*" [42]. With these words, Lane reminded his readers that the conditions within the Sun were very different than those predictable at the time using terrestrial physics. Given the pressures within the Sun, the possibility of unusual materials had to be considered. For Lane, this extended to a material approaching a liquid in behavior, even though such conjectures were viewed as unlikely.

2.2.3 Lane's law: Stars which cannot cool

In his 1870 treatment of the Sun [42], Lane advanced an elegant approach to the gaseous Sun. From his mathematics, he was able to obtain a relationship between solar density and radial position using two equilibrium conditions. Today, these are referred to as 1) mechanical or hydrostatic equilibrium and 2) convective equilibrium. At the same time, Lane deduced a central solar density of 7 to 28 g/cm^3 depending on the assumptions applied [42]. Yet, the most important conclusion of Lane's paper was a law, not discovered by Lane but by Ritter [45]. In fact, Chandrashekhar would state that "*almost the entire foundation for the mathematical theory of stellar structure was laid*" by Ritter [48, p. 179].

As for Lane's law, it proposed that the product of a gaseous star's radius and its radial temperature was a constant [43, p. 194]. If the star contracted, its temperature increased, provided that it remained an ideal gas. Fisen commented as follows: "*In a very remarkable paper, published in 1870, Mr. Homer Lane has shown that if the Sun were entirely gaseous, and if the gases composing it were under such physical conditions that the laws of 'perfect gases' should be applicable to them, the heat developed by shrinkage must not be merely equal but must so far exceed that radiated to effect it, that the temperature of the whole must actually rise in consequence, and must continue to do so for so long as a perfectly gaseous condition is maintained*" [30, p. 38]. Professor Benjamin Peirce would restate the same ideas: "*Gaseous bodies in the process of radiating light and heat condense and become hotter throughout their mass*" [52, p. 197–198]. Today, "*Lane's Law*" is referred to as Lane-Emden equation, even though Ritter discovered the formula and Lane never wrote it down [43, p. 196]. As a result of the Lane-Emden relation, gaseous stars could never cool. They continued to emit massive amounts of heat radiation. In so doing, gaseous stars actually contracted and heated up. Eddington was astounded at the "*striking result that if a star contracts the internal temperature rises so long as the material is sufficiently diffuse to behave as a perfect gas*" [44, p. 5].

2.2.4 An independent discovery of Lane's law

Lane's law was also independently discovered by T.F.F. See [53, 54]. See provided a detailed description of his experiences with Lane's law. The discourse was both credible and instructive [54]. See's treatment of Lane's law advanced a straightforward derivation from Helmholtz' ideas and placed much of the history of Lane's law in perspective. Ritter's work was not very well known by the astronomical community. After deriving Lane's law, See recognized its profound importance and wrote to many astronomers to establish if there were priority claims to the formulation. Eventually, an English astronomer mentioned Ritter's 1881 communication [54]. Examining the reference, See argued that Ritter only used "language" to describe Lane's law. In fact, as Chandrashekhar stated [48, p. 178], Ritter first arrived at the law in the key 1878 paper [45]. Unfortunately for See, the Englishman was poorly aware of the German literature. In large measure, See's own work, would simply become an independent confirmation of Ritter.

However, See's papers were both elegant and well written [53, 54]. See argued that star-like masses, formed from nebular bodies, could not become infinitely compressed. Eventually, they must reach the liquid state: "*From these considerations we see that when the gaseous nebula is infinitely expanded the temperature is the absolute zero of space, and that the maximum temperature results when the mass is contracted to the smallest radius consistent with the laws of gaseous constitution. After the mass has condensed so far that liquefaction sets in, free contraction is obstructed by molecular forces, or practically ceases; the temperature falls, and the body eventually cools down to obscurity. Such it would seem, must be the history of the temperature of cosmical bodies formed by the gravitational condensation of nebulous matter*" [54]. For theoretical astrophysics, it was difficult to account for such a phase transition.

2.3 Charles Hastings: A photosphere made of silicon?

When Charles Hastings developed his theory on the constitution of the Sun, he was surely unaware of the great impact he would have on solar theory [55]. Though Hastings' contribution was devoid of mathematics, it advanced many novel ideas which became the genesis for new theoretical formulations. Amongst his contributions was the concept that line widths could be explained by considering various layers within the photospheric atmosphere. For Hastings, line widths were directly related to pressure [55]. In order to arrive at increasing values, it was simply required that the lines originated from deeper layers within the photosphere.

Hastings opposed Faye's model of the Sun on two grounds: "*1) To produce dark lines in a spectrum by absorption, the source of the absorbed light must be at a higher temperature than that of the absorbing medium and 2) There is an inferior limit of brightness below which the course of absorbed light cannot go without the spectral lines becoming bright*" [55]. In the second of these objections, Hastings was referring to the reversing layer of the Sun observed during total eclipses.

Hastings advocated that "*it is not a priori improbable that we receive light from many hundreds of miles below the outer surface of the photosphere*" [55], a concept still utilized in the modern age to explain limb darkening. Hasting applied the idea to explain the linewidths of dark lines in the solar spectrum and proposed an alternative approach to account for limb darkening. Hastings also advocated that solid or liquid carbon could not be present on the Sun: "*Granting this, we perceive that the photosphere contains solid or liquid particles hotter than carbon vapor, and consequently not carbon*" [55]. He suggested that the material might be silicon. Hastings made the bold pronouncement: "*At any rate, we are sure that the substance in question, so far as we know it, has properties similar to those of the carbon group*" [55]. But what properties? Hastings was not clear on this point. Nonetheless, the idea was important and Hastings' point will be addressed in an upcoming contribution [56].

2.4 Frank Very: Frequency dependent limb darkening

In 1902, Frank Very published a detailed analysis of limb darkening as a function of frequency [57]. The work would be monumental in astronomy. Very was once Samuel Langley's trusted assistant [58] and had been with Langley in the days when the solar spectrum was first recorded in its entirety [59–61]. In his classic report [57], Very documented that the Sun's radiance was darkening towards the limb in a frequency dependent manner. He studied 7 wavelengths ranging from 0.416 μm to 1.5 μm, and demonstrated that shorter wavelengths produced more dramatic limb darkening [57]. In the violet wavelengths (0.416 μm), the edge of the solar disk was radiating only 10% of the intensity found at the center. As one moved towards the red (1.50 μm), the decrease was much smaller with 75% of the radiation remaining [57].

Very attempted to explain his findings by invoking atmospheric absorption of radiation, primarily by the corona [57, p. 80]. Very advanced the scattering of radiation in the corona and its reflection by carbon particles [57, p. 82]. Of course, graphite makes for a very poor reflector. Very considered diffraction: "*We can subject the hypothesis of an extensive envelope, depleting the rays by selective diffraction*" [57]. Finally, Very advanced that the phenomenon was produced by the irregularity of the Sun's photosphere, invoking its granulated structure [57, p. 86]. The idea was never pursued.

Immediately following the publication of Very's discovery, Arthur Schuster attempted to explain the strange frequency/position dependent variation of solar radiation [62]. In so doing, he began to develop the logic which led to his famed communication on *Radiation through a Foggy Atmosphere* [26, 27]. Very's work became a source of motivation for theoretical physics.

2.5 Arthur Schuster and the solar atmosphere

Sir Arthur Schuster was one of the most influential scientists of his time [24, 25]. He attended Balfour Stewart's classes and, following the counsel of Henry Roscoe, completed his dissertation with Gustav Kirchhoff [24,25]. At the Cavendish Laboratory, Schuster worked under both James Clerk Maxwell and Lord Rayleigh [24]. He also studied with Weber and Helmholtz [25]. In 1888, he succeeded Balfour Stewart as the Langworthy Professor of Physics at Owen's College and remained in this chair until 1907 [25]. Eventually, Schuster was elected secretary of the Royal Society [24]. If George Hale was regarded as the "*father*" of the International Union for Solar Research, it has been argued that Schuster was its "*mother*" [25]. Schuster counted amongst his students Sir J. J. Thomson (Nobel Prize 1906), John William Strutt (Lord Rutherford, Nobel Prize 1904), and Sir Arthur Eddington, [24]. As a consequence, Eddington became a direct scientific descendent of Gustav Kirchhoff.

Schuster's seminal contributions began in 1902 with a report on *The Solar Atmosphere* published within the *Astrophysical Journal* [62]. *The Solar Atmosphere* was written in response to Frank Very's detailed examination of solar radiation [57] (see Section 2.4). In turn, it was subjected to a letter of criticism authored by Very [63] to which Schuster would reply [64].

Schuster's reply, *The Temperature of the Solar Atmosphere* [64], summarized his position and exposed some rather prominent errors in logic. Schuster believed that he could account for the law of variation of solar radiation by invoking two layers within the Sun: 1) a photospheric layer radiating as a blackbody at 6,700° and 2) an absorbing layer at 5450°. The sum of the two layers produced the Sun's apparent temperature at 6,000°. Schuster stated that within *The Solar Atmosphere* [62], he had used a fourth power of temperature relationship, when a fifth power was more appropriate. Additionally, and this was perhaps most troubling, Schuster maintained that the radiative layer was emitting as kF, where F was the blackbody function and k was a wavelength dependent constant which could adopt any value between zero and infinity. In so doing, he removed all restrictions on the ability of bodies to emit radiation and operated well outside the bounds of physics. As a student of Kirchhoff, Schuster insisted that: "*Everybody knows that the function of temperature and wavelength which expresses the radiation of a blackbody is a fundamental function which must enter into every discussion of radiation and absorption*" [64]. Yet, through his mathematics, Schuster essentially disregarded the blackbody function itself. Schuster could provide no physical justification for the behavior of k, his magical constant. Its presence made any extended discussion of mathematics pointless. Schuster further broadened the boundary of proper mathematical treatment highlighting: "*As misunderstandings seem so easily to arise, it is perhaps worth pointing out that, although for the purpose of facilitating mathematical analysis it is sometimes necessary to treat the upper portion of the same body as made up of distinct layers, having different temperatures and possibly different absorbing qualities. . .*" [64]. With these words, Schuster removed even more restrictions for the gaseous solar models relative to ability to emit radiation. Given unbridled mathematics, all could be explained in a gaseous framework.

Very seemed more mindful of physical realities: "*It is a fact that, at the photospheric level, some form of matter exists which does radiate indiscriminately through a wide range of wavelengths, and whose particles are presumably coarse enough to act non-selectively in other respects*" [63]. He championed an idea that was to permeate theoretical astrophysics: "*From the depths of the Sun, radiations composed mainly of very short waves tend to proceed, and a very extensive scattering atmosphere acts almost like a reflector, send nearly all the rays back again. In this case the medium will not be heated much in the process. Only a small fraction of the incident rays will be absorbed by the fine particles; the greater part is assumed diffracted. Still, as the course of the rays through such an extensive scattering medium is a zigzag one, the scattering being repeated over and over again, some cumulative action and some absorption of energy by the medium must result. Consequently, it is not possible to separate completely the two causes — absorption and scattering*" [63]. Almost the exact arguments would be repeated by Eddington in the 1920's [44].

2.6 Classic papers in stellar radiation transfer

Donald Menzel prepared a compilation of *Selected Papers on the Transfer of Radiation* [66], wherein he reprinted the great contributions on the subject, but regrettably, without offering a commentary. By assembling these articles in one text, Menzel implicitly reminded the reader of their importance in the history of theoretical astrophysics.

The study of radiation in stellar atmospheres was primarily driven by the need to explain the continuous solar spectrum. While many works describe the transfer of radiation within stars [67–69], the entire problem was introduced into astronomy by the desire to account for thermal emission in a gaseous framework. The understanding of internal stellar opacity was directly associated with the act of building a star without recourse to condensed matter. Ironically, it also became essential to account for physical structure using a phase of matter, which on Earth, was devoid of structural potential [70]. In adopting a gaseous foundation, astrophysics was immediately confronted with two dilemmas: 1) how could a gas provide a continuous blackbody spectrum like graphite? and 2) how could structure and activity, like granulations, sunspots, flares, and prominences be understood using a fully gaseous entity? To solve these great questions, only theoretical approaches were available.

2.6.1 Schuster and the foggy atmosphere

Arthur Schuster initially presented an abridged version of his *Radiation through a Foggy Atmosphere* in 1903 [27]. The complete paper appeared in 1905 [26]. Schuster attempted to explain the bright lines of the reversing layer above the photosphere and the dark lines which usually typify the solar spectrum. For Kirchhoff, the bright lines were being produced by species which were at a higher temperature than the liquid photosphere, while the dark lines required lower temperatures. Though Kirchhoff's student, the German-born British physicist preferred an alternative explanation.

Schuster viewed as *foggy* an atmosphere which sustained a considerable amount of scattering. The basis of the presentation was the emission of radiation from a surface towards an overlaying atmospheric layer, wherein both scattering and absorption occurred. Accordingly, Schuster required that the Sun possess a distinct surface [26]. The point was also made by Milne [70] in his description of Schuster's contribution to the understanding of solar emission. For Schuster, scattering and absorption within the foggy atmosphere could modify the light emitted from the lower surface, permitting only certain frequencies to pass through which accounted for the bright or dark lines on the solar spectrum. The derivation assumed that the coefficient of absorption in the scattering layer was a function of wavelength dependent on the density of the absorbing species in the medium. Likewise, the coefficient of scattering also depended on the number of scattering particles in the medium which may or may not be the same as those used in absorption.

Schuster considered the Sun much like Faye [2]. The photosphere was composed of particulate matter floating above a gaseous solar body [1]. It was this particulate matter which would allow for the treatment of the scattering process. Schuster insisted on the validity of Kirchhoff's law as the proper starting point for all work in thermal emission. Though he recognized many of the weaknesses of his approach, Schuster never questioned Kirchhoff [26, p. 5]. Consequently, Schuster demonstrated that when the absorption coefficient of the layer was large with respect to the coefficient of scattering, the radiation observed from a large cloud of gas was the blackbody function: "*The radiation in this case becomes equal to that of a completely black surface, which agrees with the well-known law that absorption irrespective of scattering tends to make the radiation of all bodies equal to that of a black body when the thickness is increased*" [26, p. 6]. The result unfortunately, while mathematically appealing, was logically flawed.

Schuster expressed that the radiation emitted by the absorbing layer was the product of the absorption coefficient, k, multiplied by the blackbody function, E, and the thickness of the layer, dx: $kEdx$ [26, p. 3]. The absorption coefficient, k, in this case, was dependent on the wavelength of observation, the nature of the gas, and the density of the medium. In reality, Schuster needed to use an arbitrary function, like Γ, obtaining $k\Gamma dx$. In this case, Γ could be viewed as equal to $k'E$. Such an approach would more appropriately reflect the complexity involved in this problem. Schuster never established that E equaled Γ, the step critical to maintaining his conclusion. His *a priori* invocation of the blackbody function for the gas layer, though appearing mathematically correct because of the multiplication with k, ensured the result sought. Repeating the same derivation using Γ would completely alter the conclusions.

Once Schuster assumed that the blackbody function could be directly applied to represent the emission of the gas, a great thickness guaranteed that blackbody radiation was produced, even if the coefficient of absorption was small, merely because the coefficient of scattering was much smaller (see Eq. 14 in [26]). The result was impossible as it violated the first law of thermodynamics. It would have been more reasonable to derive that great thickness would simply result in obtaining the arbitrary function Γ. Schuster would have obtained this tempered finding, reminiscent of the line spectrum, such as that of the gaseous nebula in Orion [71, p. 87], if he had not insisted upon using the blackbody function as a point of departure.

The lineshapes of emission spectra for condensed matter do not change simply because objects become large. Yet, this was what Schuster was implying for the gas. This conclusion was very far reaching and would propagate throughout the astrophysical literature without correction. Arbitrary radiation never becomes black within adiabatic enclosures [72] and gases do not become black simply because they are expansive — a lesson learned from the gaseous nebula [71, p. 81–92]. The size of objects remains secondary to the nature of radiation, if diffraction effects can be neglected [73].

2.6.2 Schwarzschild and radiative equilibrium

As was seen in Section 2.2, Lane's gaseous Sun [42] achieved stability through convective equilibrium. But for Arthur Eddington, radiative equilibrium became an important means of achieving the same result [3, 44]. The concept of radiative equilibrium was initially advanced, as Eddington recalls [44, p. 9], by R. A. Sampson in 1894 [74]. Still, it was Karl Schwarzschild (October 9th, 1873 — May 11, 1916) [75] who, in 1906, would give it prominence in theoretical astrophysics [76].

Schwarzschild was a gifted theoretical physicist who died at the age of 42 in the course of World War I: "*The war exacts its heavy roll of human life, and science is not spared. On our side we have not forgotten the loss of the physicist Moseley, at the threshold of a great career; now from the enemy, comes news of the death of Schwarzschild in the prime of his powers. His end is a sad story of long suffering from a terrible illness contracted in the field, borne with great courage and patience. The world loses an astronomer of exceptional genius, who was one of the leaders in recent advances both in observational methods and theoretical researches*" [75]. Many surely believe in the impossibility of reading Schwarzschild without gaining some reverence for the beauty of the human mind. Schwarzschild's treatment of radiative equilibrium within stars would not set a lower standard [76].

Milne reviewed Schwarzschild's contribution to radiative equilibrium in his Bakerian lecture [70]. This elegant treatment, as mentioned in Section 2.6.1, also addressed Schuster's approach [70].

Schwarzschild began his discussion of limb darkening on the solar surface by assuming that radiative equilibrium existed [76]. He also considered adiabatic equilibrium, referred to by Lane as convective equilibrium [42]. According to Schwarzschild: "*radiative equilibrium in a strongly radiating and absorbing atmosphere will be established when radiative heat transfer predominates over heat transfer due to convective mixing*" [76]. The theoretical formulation adopted resembled Schuster's [70]. Schwarzschild almost perfectly accounted for limb darkening using radiative equilibrium, demonstrating accordingly, that this assumption was valid for a gaseous Sun. The final result was independent of wavelength, dealing only with the total heat emitted, as measured with a bolometer [76]. Schwarzschild further proved that limb darkening could not be accounted for using convective equilibrium (see the table in [76]). The finding was impressive. Like Schuster before him, Schwarzschild based his conclusion on the validity of Kirchhoff's law [9]. Thus, the result was critically dependent on the soundness of Kirchhoff's conclusion. In addition, since it was based on an ideal gas, Schwarzschild's derivation implied that the Sun was devoid of a real surface and the solutions obtained extended to infinity [76]. Radiative equilibrium, sustained within a gaseous Sun, would form the basis of Eddington's treatment of the internal constitution of the stars [3, 44, 77].

2.6.3 Rosseland and mean opacities

Before discussing Eddington's application of radiative equilibrium to the stars, a sidestep should normally be made in order to briefly cover Rosseland and the formulation of the mean opacities [78, 79]. First proposed in 1924, Rosseland mean opacities enabled the next great advance in theoretical astrophysics [78, 79]. However, the topic will be passed over for the time being, reserving it instead for an upcoming work [80].

3 Eddington and Jeans: The clash of the titans

In writing the biography of Arthur Stanley Eddington, Subrahmanyan Chandrasekhar chose the following title: *Eddington: The Most Distinguished Astrophysicist of his Time* [81]. Chandrasekhar was not far from the mark. However, another contender for the title existed: James Hopwood Jeans. In fact, Edward Arthur Milne [82], who along with Ralph Fowler [83] worked with Eddington at Cambridge, would spend the last days of his life writing the biography of Sir James Jeans [84]. The work would be published after Milne's death. No one can truly dissect the merits of each man. Eddington and Jeans were giants in the world of theoretical astrophysics. Each made brilliant strides and, like all men, each committed regrettable scientific errors.

Matthew Stanley provided an outstanding account of the great battle which engulfed Eddington and Jeans [85]. Stanley outlined the vivid debates over the nature of the stars and the vastly differing philosophical approaches. He emphasized that much of what theoretical astrophysics would become dependent on Eddington's phenomenological outlook [85]. Jeans, for his part, dismissed Eddington's approach as not even science [85]: "*Eddington argued that his phenomenological approach opened up new avenues of investigation in astronomy, but Jeans argued that this was a violation of the very rigor and discipline that made astronomy so powerful*" [85]. Albert Einstein shared in Jeans' position stating: "*Eddington made many ingenious suggestions, but I have not followed them all up. I find that he was as a rule curiously uncritical towards his own ideas. He had little feeling for the need for a theoretical construction to be logically very simple if it is to have any prospect of being true*" [86, p. 40]. Einstein wrote these words in a private letter and made no such statements publicly. After all, it was Eddington who first worked to confirm Einstein's theory of relativity [87]. Jeans was even more critical: "*All Eddington's theoretical investigations have been based on assumptions which are outside the laws of physics*" [88]. As for Eddington, he was described

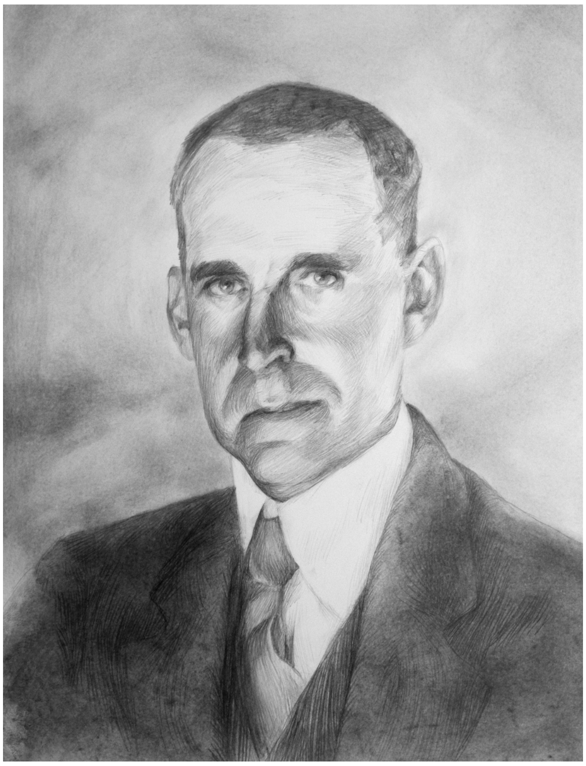

Fig. 1: Sir Arthur Stanley Eddington (December 28th, 1882 — November 22nd, 1944) was an outstanding theoretical physicist. He would become known for his approach to the gaseous stars. He derived a mathematical formulation which could account for the mass-luminosity relationship of the stars and was the first to propose that stars were fueled by nuclear processes. Eddington also conducted key experiments validating Einstein's theory of relativity.

Fig. 2: Sir James Hopwood Jeans (September 11th, 1877 — September 16th, 1946) was the last modern advocate of liquid stars. He believed that such objects were constructed from heavy elements obtaining their energy through fission, rather than fusion. Beyond astronomy, he was best known for his work on the partition of energy between matter and radiation — a solution leading to the Jeans-Rayleigh ultraviolet catastrophe. Jeans served as Secretary of the Royal Society from 1919–1929.

as a pragmatist [85]. He used "*whatever knowledge and tools were useful, instead of worrying about whether they were 'really true'*" [85]. In his defense against Jeans' constant detractions, Eddington claimed: "*although a reasonable degree of rigour is required, the laborious exploration and closing of every loophole is of secondary importance* [85]. But, with regards to the Sun, who was to assess if an element of theory was merely a question of closing a loophole or a fatal and irrecoverable logical flaw? Eddington and Jeans would outline scientific and philosophical problems which remained unanswered to the present day.

Milne, perhaps better than anyone, was in a position to highlight the great loss to science that the discord between Jeans and Eddington produced: "*It is much to be regretted that these two titans, Eddington and Jeans, should not have co-operated in their assaults on the grand subject of stellar structure, instead of being opposed to one another, during the most constructive periods of their careers. The blame has to be divided between them. Jeans mistakenly attacked Eddington's mathematics instead of accepting his mathematics and then providing the correct interpretation; Eddington resented what he considered to be aspirations on his competency as a mathematician, and never understood the difficulties of a philosophical kind that surrounded his own interpretation of his results. Astronomers on the whole have favoured Eddington's side of the controversy — mistakenly in my opinion. This is due, in addition to the reasons mentioned above, to the fact that Eddington had more of a feeling for the physics of a situation than Jeans had, whilst Jeans had more of a feeling for the mathematics of a situation than Eddington had; the result was that Eddington's stars had a physical plausibility that Jeans' lacked, and the astronomer who did not wish to go into the rights and wrongs of the mathematical situation could see the physical likelihood of Eddington being correct*" [84, p. 28].

3.1 Arthur Stanley Eddington

Though Eddington was a great proponent of the gaseous Sun, in 1910, he noted that "*the stars might be solid, liquid, or not too rare a gas*" [85]. He was a Quaker by birth and had earned a bachelor's degree with Arthur Schuster at Owens College [85]. As such, he was a direct scientific descendent of Kirchhoff. Eddington maintained that the value of theory was in its ability to prompt further study, not in its relation to the established facts [85].

In his classic paper *Radiative Equilibrium in the Sun and Stars*, Eddington wrote about the laws of emission: "*There are some physical laws so fundamental that we need not hesitate to apply them to the most extreme conditions; for instance, the density of radiation varies as the fourth power of temperature, the emissive and absorbing power of a substance are equal, the pressure of a gas of given density varies as its temperature, the radiation-pressure is determined by the conservation of momentum — these provide a solid foundation for discussion*" [77]. Unfortunately, Eddington dispensed with the qualifiers so critical to make such statements hold true. In reality, only the emission of graphite or soot varied as the fourth power of temperature [7, 72, 73, 89]. Even for these cases, the relationship depended on the frequency of interest and the specific mineralogical origin of the material. The gas Eddington considered could never adopt such behavior [89]. In fact, the emissivity of gases could actually drop with increasing temperature [89], a clear violation of Stefan's 4th power of temperature law [90]. Unlike graphite, gases utilize convection currents in an attempt to reach thermal equilibrium. In any event, Kirchhoff's law [9] required two restrictions: a rigid enclosure and thermal equilibrium [7, 72, 73]. Eddington's gaseous Sun could provide neither. Outside the strict confines of thermal equilibrium, even the statement that emission equaled absorption was invalid. Jeans also made the point: "*In a gaseous star it is probable that much more energy is transferred by radiation than by ordinary gaseous conduction, so that an accurate determination of the laws of radiative transfer is a necessary preliminary to many problems in stellar physics*" [91]. Jeans based his thesis on theoretical grounds, while the laws of radiation for gases must be determined experimentally. In any case, even the slightest conduction and/or convection, both of which are undeniably present in stars, rendered all conjectures of radiative equilibrium invalid.

Despite all these considerations, Eddington was able to make what appeared to be surprisingly powerful advances in theoretical astrophysics. While assuming that absorption was constant within stars, the triumph of his gaseous models rested on the confirmation of the mass-luminosity relationship [44, p. 145–179] and the explanation of Cepheid variables [44, p. 180–215]. Eddington's paper, *On the Relation between the Masses and Luminosities of the Stars*, became an instant classic in theoretical astrophysics [92]. Eddington justified his theoretical approaches by invoking the work of Jacob Halm [93] who was the first to state that "*intrinsic brightness and mass are in direct relationship*". Halm was soon followed in this concept by Ejnar Hertzsprung who, in 1919, also established a relationship between these two variables [94]. An excellent historical review on the subject exists [95]. For theoretical astrophysics, Eddington's confirmation of the mass-luminosity relationship was not simply an affirmation of Halm and Hertzsprung [93, 94]. It represented the birth of the fully gaseous Sun and of theoretical astrophysics.

The derivation of the mass-luminosity relationship would become a direct confirmation that Eddington's entire approach was correct. Stars, it seemed, must be gaseous. The argument was powerful. Still, it remained strangely dissociated from all physical observations of the Sun itself. In order to reproduce the mass-luminosity relationship, Eddington had only one requirement: the line he would draw would be guided by passing through a single star — Capella [92]. Jeans was not convinced. In 1925, he argued that the mass-luminosity relationship itself was nothing but an illusion: "*. . . there is no general relation between the masses and luminosities of stars. . .* " [85, p. 67].

Despite Jeans' objection, Eddington was quick to gain broad acceptance of his views. He would soon write a highly read popular work, *Stars and Atoms* [96]. It would provide a powerful look at both his philosophy and his scientific positions. In *Stars and Atoms*, Eddington stated that "*The Sun's material, in spite of being denser than water, really is a perfect gas. It sounds incredible, but it must be so*" [96, p. 38]. Further, Eddington would invoke Ralph Fowler in claiming that the gas was "*superperfect*" and "*more easily compressed than an ordinary gas*" [96, p. 40]. He would go on to state: "*It is now well realized that the stars are a very important adjunct to the physical laboratory — a sort of high temperature annex where the behavior of matter can be studied under greatly extended conditions. Being an astronomer, I naturally put the connexion somewhat differently and regard the physical laboratory as a low temperature station attached to the stars. In it the laboratory conditions which should be counted as abnormal*" [96, p. 83]. These words, of course, echoed Jeans' claim that Eddington had abandoned the laws of Earthly physics. Milne was forceful regarding Eddington: "*No words are needed to praise Eddington's achievement in calculating the state of equilibrium of a given mass of gas, and in calculating the rate of radiation from its surface. What was wrong was Eddington's failure to realize exactly his achievements: he had found a condition for a star to be gaseous throughout; by comparison with the star, Capella, he had evaluated the opacity in the boundary layers; and he had made it appears unlikely that the stars in nature were gaseous throughout. His claims were the contrary; he claimed to have calculated the luminosity of the existing stars; he claimed to show that they were gaseous throughout; and he claimed to have evaluated the internal opacity of the stars. Jeans deserves great credit for being the first critic to be skeptical about these claims of Eddington's theory, in spite of the attractive plausibility with which the theory was expounded*" [84, p. 27].

Recently, Alan Whiting presented a review of *Stars and Atoms* [97, p. 215–229]. Whiting claimed that Eddington was carefully aware of observational physics, particularly with regards to the mass-luminosity question [97]. Whiting created an interesting contrast with Stanley [85] relative to the Jeans-Eddington battle. Whiting was highly critical of Jeans, but much more reverential towards Eddington [97, p. 215–229]. Perhaps this was with good reason as Eddington had championed the gaseous stars. This was to become the prevailing theory. Jeans defended the liquid alternative [97, p. 187–214]. Eventually though, even Jeans abandoned the liquid [97, p. 231–246] in favor of Eddington's gaseous models.

3.2 James Hopwood Jeans

Milne said of Jeans that "*he never wrote a dull page of mathematics in his life*" [84, p. 15]. Thus, in every respect, Jeans was a fitting adversary for Eddington. While an undergraduate at Cambridge, he received outstanding scores on his entrance exams to Trinity College and, along with G. H. Hardy, he would become the first student to take Part I of the Mathematical Tripos in only two years [84, p. 4–5]. A brilliant mathematician, Jeans' first great contribution to theoretical physics would be his study of the partition of energy between matter and radiation [98–100]. The papers demonstrated that Planck's quantum mechanical formulation [101], devoid of the Jeans-Rayleigh ultra-violet catastrophe, was the proper solution to the blackbody problem. Milne reviewed Jeans' contribution to the energy partition problem [84, p. 89–98]. Milne also provided perhaps the best condensed review of Jeans' position on liquid stars [84, p. 99–124]. In doing so, he reminded us that one of Jeans most beautiful works was his Adams Prize Essay [102]: "*Jeans Adams Prize Essay of 1919 was and remains a classic, even where subsequent discoveries have proved it wrong*" [84, p. 57]. The Essay was Jeans' first great venture into liquid stars.

Jeans was not the first to consider the problem of rotating homogeneous masses. As shall be seen in Section 3.3, the problem had been addressed by many of the finest minds in science. For Jeans, this included Poincaré [103] and George Darwin [104–108], the Cambridge physicist who had judged the *Adams Prize Essay* [84, p. 11]. Schwarzschild had also devoted time to this problem [109] and his approach remains important [110].

For Jeans, the starting point for liquid stars appears to have been the observation that a very large portion of these bodies existed as binary systems. The prevalence of binary stars would open the *Adams Prize Essay* [102, p. 2–4]. It would become a central part of *Astronomy and Cosmogony* [111, p. 20–23] and of his popular *The Universe Around Us*, both in its First Edition of 1933 [112, p. 38–53] and in the dramatically different Fourth Edition of 1944 [113, p. 37–51]. Relative to the formation of binaries, he wrote: "*In brief every rotating body conducts itself either as if it were purely liquid, or as if it were purely gaseous; there are no intermediate alternatives. Observational astronomy leaves no room for doubt that a great number of stars, possibly even all stars, follow the sequence shown in fig. 11. No other mechanism, so far as we know; is available for the formation of the numerous spectroscopic binary systems, in which two constituents describe small orbits about one another. In these stars, then, the central condensation of mass must be below the critical amount just mentioned; to this extent they behave like liquids rather than gases*" [112, p. 215]. Figure 11 represented the pear-shaped Darwin sequence of stellar evolution.

Three major problems preoccupied Jeans: 1) the purely rotational problem of a homogenous liquid, 2) tidal problem wherein a primary mass was affected by a secondary object, and 3) the formation of binary stars and maintenance of binary stars [84, p. 110]. For Jeans, the entire problem of the stars was one of physical stability. His work on liquids was surprisingly sparse of the radiative considerations which had characterized Eddington's entire approach to gaseous stars.

Jeans argued in Astronomy and Cosmogony that gaseous stars were inherently unrealistic [111, p. 64–104]: "*. . . we investigated the internal equilibrium of the stars on the supposition that they were masses of gravitating gas, in which the gas-laws were obeyed throughout. The investigation was abandoned when it was found to lead to impossibly high values of atomic weights of the stellar atoms. This created a suspicion that the hypothesis on which it was based was unfounded, and that the gas-laws are not obeyed in stellar interiors*" [111, p. 136]. He had previously attacked the stability of gaseous stars in the 1925 *Monthly Notices* [114]. He claimed that stars which generate energy as a function of temperature and density, would be violently unstable to radial oscillations [114]. Cowling refuted Jeans' claims [115, 116] and Whiting recently followed suit [117]. In the end, the instability of gaseous stars would survive scrutiny.

By the time Astronomy and Cosmogony was published, Jeans still refused to accept that the mass luminosity relationship was valid [111, p. 83]. Rather, he held that the mass-luminosity law could not be real, but that it was "*a consequence merely of the special assumption that kG is constant, and cannot have reference to actual stellar conditions*" [111, p. 83]. Jeans viewed the entire relation as a mathematical trick [85, p. 75]. Already, Jeans believed that stars were driven by the fission of materials such as uranium [111, p. 83]: "*But if the star has a liquid, or partially liquid, centre, this strip of safe land is so wide that, consistently with stability, the stellar material may have exactly the property that we should à priori expect to find, namely that its annihilation proceeds, like radio-active disintegration, at the same rate at all temperatures. If the substance of the star has this property, the star can no longer be in danger of exploding, for a mass of uranium or radium does not explode whatever we do to it*" [112, p. 287]. The amount of emitted light depended on the nature of the stellar constituents, not on a star's mass. Still, Jeans did not relate the ability to emit radiation to the phases of matter.

When Jeans first wrote *The Universe Around Us* [112], he postulated that, in order for a star to be stable, it must contain, at the minimum, a liquid central region: "*And mathematical analysis shews that if the centre of the star is either liquid, or partially so, there is no danger of collapse; the liquid center provides so firm a basis for the star as to render collapse impossible*" [112, p. 287]. He advanced two postulates: "*1. That the annihilation of stellar matter proceeds spontaneously, not being affected by the temperature of the star. 2. That the central regions of stars are not in a purely gaseous state; their atoms, nuclei and electrons are so closely packed that they cannot move freely past one another, as in a gas, but rather jostle one another about like the molecules of a liquid*" [112, p. 287]. Jeans' concept of a liquid star was based not only on the stability of the resulting structures, but also on its constitutive materials and the need to provide the energy dissipated in the Sun's thermal radiation.

In his *Hindsight and Popular Astronomy*, Whiting [97] addressed at length the differences between Jeans' two Editions of his classic text *The Universe Around Us* [112, 113, p. 83]. These two editions were drastically at odds with one another. The first made the case for liquid stars, while the second advocated gaseous entities. Jeans completely removed any reference to liquid stars from the index of the 1944 edition [113]. The listing had many entries in the previous editions. Thus, it appears that a great transformation occurred for Jeans between 1933 and 1944. The evolution of Jeans' ideas were not recorded in the scientific literature. Jeans' last technical paper [84, p. 60] was entitled: "*Liquid Stars, a Correction*" [118]. It was published in 1928 at the same time as *Astronomy and Cosmogony* [111], but did not address liquid stars. Rather, it tackled Jeans' concerns relative to the instability of gaseous stars.

Why did Jeans abandon liquid stars? The answer will probably remain elusive. It was clear that Jeans had advocated that liquid stars were constituted of heavy elements which derived their energy from fission. As a result, when evidence gathered that hydrogen was the principle constituent of stars like the Sun [119–121], Jeans was left without a building block and without a means to generate energy. It was inconceivable to a person in Jeans' day that hydrogen could exist in liquid form, provide the requisite building material for a liquid star, and maintain the Sun's energy through fusion [56]. Furthermore, Jeans had to contend with the critical temperature arguments based on Andrews [29]. Given the need for hydrogen, it must have seemed to Jeans that liquid stars were doomed.

3.3 Subrahmanyan Chandrashekhar and rotating fluid masses

Subrahmanyan Chandrasekhar (October 13th, 1910 — August 21, 1995) [122] was Ralph Fowler's student at Cambridge. He was well acquainted with Eddington, Jeans, and Milne. Eventually, he would become the recipient of the 1983 Nobel Prize in physics. His text, *Introduction to the Study of Stellar Structure* remains an authoritative treatment of the subject matter and is widely considered a classic in astrophysics [48]. Chandrasekhar also wrote a lesser known volume on *Ellipsoidal Figures of Equilibrium* [124]. Rotating fluid masses captivated Chandrasekhar for a period of nine years [124, p. 241]. The father of modern solar astrophysics makes two points with regards to his time investment: 1) "*the subject had attracted the attention of a long succession of distinguished mathematicians and astronomers*" and 2) "*the method of the virial is not restricted to homogeneous masses*" [124, 241].

Except for a single chapter, *Ellipsoidal Figures of Equilibrium* was entirely devoted to homogeneous liquid masses. His *Historical Introduction* [124, p. 241] provided a magnificent review of the field which outlined the seminal contributions of men like Newton, Maclaurin, Jacobi, Meyer, Liouville, Dirichlet, Dedekind, Riemann, Poincaré, Cartan, Roche, Darwin, and Jeans.

Chandrasekhar believed that the problem of the homogeneous liquid mass "*had been left in an incomplete state with many gaps and omissions and some plain errors and misconceptions*" [124, p. 241]. This was the prime motivation for his text. The most significant gap in the theory of the homogeneous rotating liquid was addressed with Chandrasekhar's discussion of the Darwin ellipsoids [124, p. 218–239]. In a chapter devoted to the Roche ellipsoids, he demonstrated that such structures are unstable over the entire Darwin sequence [124, p. 218–239]. Chandrasekhar's conclusion was a partial setback for Jeans' work, in that the latter had speculated, as seen in Section 3.2, that binaries were formed through the evolution of the Darwin sequence [112, p. 247–253]. Both Jeans and Darwin had recognized that the pear-shaped figure was unstable [112, p. 252], though they did not suspect that this was the case for the entire sequence. As a result, the extensive presence of binaries in the sky, Jeans' primary argument for liquid stars, could not be easily explained by the liquid models he had advocated after all. Relative to binaries, it seems that neither liquid nor gaseous models have offered a definitive answer. Lebovitz argued that "*the viability of fission theory remains unsettled to this day*" [125, p. 131].

4 Conclusions

Throughout the ages, as new physical discoveries occurred, attempts were made to mold them into the prevailing model of our star. Secchi's Sun, with its particulate photospheric matter floating on a gaseous globe, was not easily abandoned [38, 39]. Faye's insistence that the Sun was devoid of a true surface has remained accepted to this day [2]. Stoney's sprinkling of graphite particles on the Sun would prevail for 60 years [11]. But when Stoney was eventually abandoned, could modern man really endow a gas with features found only in condensed matter? Could the solar spectrum truly be accounted for by the mathematics linked to gaseous stars? These were the questions that begged for answers, although they could not be resolved solely through historical review. They would require instead a careful analysis of the stellar opacity problem [80].

It has always been true that current solar models far surpass in validity those advanced by previous generations. Therefore, modern science must be called to greater caution. It is noteworthy that, while Laplace's nebular hypothesis and Helmholtz' contraction theory have long ago been abandoned [1], the influence they carried in forging a gaseous Sun did not wane. In like manner, Kirchhoff's law of thermal emission [9, 73], though never validated in a gas, has remained a pillar of modern solar theory [1]. This has been the case, even though no gas has ever emitted a continuous spectrum which varied as the 4th power of temperature. Thermal emissivities in gases tended to drop with temperature, not to dramatically increase [89]. Invoked as one of the early pillars of the gaseous Sun, the broadening of hydrogen has never assumed a blackbody line shape. In the gaseous state, despite increased pressure, hydrogen cannot emit with a 4th power relationship [89]. In 1869, Andrews [29] was unaware that liquid metallic hydrogen existed [56]. The existence of this material [56], has delivered a devastating defeat to the limiting aspect of critical temperatures [29] measured in ordinary gases, relative to forming a gaseous Sun [1]. Given these considerations, what can be said about our solar models?

With the publication of Arthur Eddington's *Internal Constitution of the Stars* [3] and the subsequent work *An Introduction to the Study of Stellar Structure* by Subrahmanyan Chandrasekhar [48], astrophysics seemed to have taken unprecedented steps in understanding the stars. Eddington's classic work advanced a cohesive gaseous model. It also brought forth the phenomenal mass-luminosity relation, so prized by theoretical astrophysics. For his part, Chandrasekhar would propel our knowledge of stellar evolution with his introduction of degeneracy and his tremendous treatment of the white dwarf, leading to the limit which bears his name [48]. Given the powerful theoretical framework which surrounded the gaseous stars, most envision that a perfect marriage of physical observation and mathematical prowess had resulted in a level of sophistication well beyond that reached in ages past.

In spite of all this, as a celestial body, the Sun has structure: a photosphere, a chromosphere, a corona, granulations, sunspots, prominences, etc. However, by their very nature, gases are unable to impart structure. Long ago, Jeans complained that "*All of Eddington's theoretical investigations have been based on assumptions which are outside the laws of physics*" [88]. The criticism may be overly harsh, but it must be remembered that many astronomers of the period, unlike Eddington, placed a strong emphasis on physical observation. For his part, Eddington essentially dismissed physical findings. Hence, it is not surprising that animosity arose between these two men. As the author previously stated: "*Eddington believed that the laws of physics and thermodynamics could be used to deduce the internal structure of the Sun without any experimental verification. In 1926, he would speak hypothetically about being able to live on an isolated planet completely surrounded by clouds. In such a setting, he thought he would still be able to analyze the Sun without any further knowledge than its mass, its size and the laws of physics*" [126]. Eddington himself realized the risks he was taking when he wrote that: "*We should be unwise to trust scientific inferences very far when it becomes divorced from opportunity for observational tests*" [44, p. 1]. Since Eddington was trying to understand stellar interiors, there could be no observational confirmation of his mathematics. In addition, Eddington's treatment completely sidestepped the structural features on the Sun. Moreover, Eddington assumed the same average coefficient of absorption throughout a star despite fluctuations in temperatures and densities [44]. He treated all opacities, for both dense stars and sparse ones, as corresponding to the opacity within the Sun itself [44]. His model could not be tested using data from the Sun.

Eddington sought to establish the mass-luminosity relationship as a manifestation that at least some merit could be gained from his approach. This relationship was enticing, but its acceptance would come at a great price. Theoretical astrophysics would be brought to the uncomfortable position of minimizing the importance of direct physical evidence for the state manifested by the Sun. This was the cost of embracing stellar, rather than solar, data. Direct solar observations received less weight than distant stellar findings. This was the case even though stellar measurements were obtained, following assumptions and manipulation from stars positioned light years, if not thousands of light years, away. Additionally, by adopting Eddington's conclusion, the chemical nature of the star itself was quietly dismissed as immaterial [44]. Yet on Earth, the thermal emission of all materials was determined strictly by their chemical makeup and physical structure [127]. These facts should not be overlooked. It was improper for Eddington to discount earthly laboratories, as seen in Section 3.1, because mankind could trust no other venue.

If Eddington struggled in certain areas, his approach was not without precedent. As described earlier [1], those who studied solar physics, from Galileo to Wilson to Herschel to Spencer to Secchi and Faye, had no alternative course of action. Eddington was correct: given our limitations, educated speculation was the only avenue. Furthermore, it would prove much easier, in making progress in science, to rebuke known ideas, rather than to speculate on the unknown. Eddington's attempt to forge new ground was laudable and such will remain the case through the ages.

Though Jeans philosophically disagreed with Eddington's approach [85], he was unable to truly offer an alternative. Many of his claims were incorrect. He continued to believe in Helmholtz' theory of contraction for energy production, well after many had abandoned the idea [85]. He advocated liquid stars as a mechanism for producing binaries, when more prudent mathematical treatments would cast doubt upon his arguments [124]. He advocated that gaseous stars were unstable to oscillations [114]. He advanced that liquid stars had to be formed from uranium and radium [112, p. 287]. In the battle with Eddington, he showed a lack of restraint in charging that his colleague's approaches were not even science. Who, from sole authority, could establish what was or was not science? Rather, as Milne highlighted, Jeans and Eddington should have made a concerted effort to work together [84, p. 28]. The questions were much too complex for isolated approaches and both men would have been well served to collaborate.

As this review of the *Thermodynamic History of the Solar Constitution* comes to a close, one can only wonder at the beauty of solar science. Stellar astrophysics remains a relatively small island in the sea of science. Nonetheless, so many aspects of earthly physics and chemistry touch the subject. In this regard, and given the task ahead, there is much to contribute to the subject area, even for non-astronomers. Thus, we leave the subject by pondering, once again [1], upon the wisdom offered by the magnificent solar astronomer, George Hale [128]. In writing the obituary for Arthur Schuster [24], the founder of the *Astrophysical Journal* [128] was sickly and approaching the end of his own life. Hale reminded us of the need to work together in order to arrive at a deeper understanding of the world around us. A study of the history of solar science echoes Hale. The contributions of many were required to arrive at some semblance of the truth: "*A Galileo or a Newton or an Einstein cannot be produced by an International conference, nor can lesser men who have nevertheless contributed enormously to original thought. How then are we to reconcile our co-operative projects with the prime necessity for personal freedom?* [24, p. 101] … "*One of the most important needs of science is to establish closer relationships between workers in different fields. It is comparatively easy to bring together specialists in given subjects and to secure their friendly co-operation. But to fill the gaps between various branches of science is a more difficult task, in spite of the obvious possibilities of advance. Such possibilities are shown by the development of astrophysics, geophysics, biochemistry, and many other subjects. However, the fact remains that countless opportunities are lost because instruments, methods, and ideas which have originated in some particular field are unknown or at least unused in other fields*" [24, p. 102].

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Dedication

This work is dedicated to my youngest son, Luc.

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