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SPECIAL ISSUE

The Sun – Liquid or Gaseous?
A Thermodynamic Analysis

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LETTERS

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A Thermodynamic History of the Solar Constitution — I: The Journey to a Gaseous Sun

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History has the power to expose the origin and evolution of scientific ideas. How did humanity come to visualize the Sun as a gaseous plasma? Why is its interior thought to contain blackbody radiation? Who were the first people to postulate that the density of the solar body varied greatly with depth? When did mankind first conceive that the solar surface was merely an illusion? What were the foundations of such thoughts? In this regard, a detailed review of the Sun's thermodynamic history provides both a necessary exposition of the circumstance which accompanied the acceptance of the gaseous models and a sound basis for discussing modern solar theories. It also becomes an invitation to reconsider the phase of the photosphere. As such, in this work, the contributions of Pierre Simon Laplace, Alexander Wilson, William Herschel, Hermann von Helmholtz, Herbert Spencer, Richard Christopher Carrington, John Frederick William Herschel, Father Pietro Angelo Secchi, Hervé August Etienne Albans Faye, Edward Frankland, Joseph Norman Lockyer, Warren de la Rue, Balfour Stewart, Benjamin Loewy, and Gustav Robert Kirchhoff, relative to the evolution of modern stellar models, will be discussed. Six great pillars created a gaseous Sun: 1) Laplace's Nebular Hypothesis, 2) Helmholtz' contraction theory of energy production, 3) Andrew's elucidation of critical temperatures, 4) Kirchhoff's formulation of his law of thermal emission, 5) Plücker and Hittorf's discovery of pressure broadening in gases, and 6) the evolution of the stellar equations of state. As these are reviewed, this work will venture to highlight not only the genesis of these revolutionary ideas, but also the forces which drove great men to advance a gaseous Sun.

1 On the history of solar science

Pondering upon the history of solar science [1–14], it becomes apparent that, in every age, the dominant theory of the internal constitution of the Sun reflected the state of human knowledge. As understanding of the physical world grew, the theories of old were slowly transformed. Eventually, under the burden of evidence, ancient ideas were destined to disappear completely from the realm of science, relinquished to the sphere of historical curiosity [2]. What was once considered high thought, became discarded.

If science is to advance, historical analysis must not solely reiterate the progress of civilization. Its true merit lies not in the reminiscence of facts, the restatement of ancient ideas, and the reliving of time. Rather, scientific history's virtue stems from the guidance it can impart to the evolution of modern research.

Historical compilations, dissected with contemporary scientific reasoning, have the power to expose both the truths and the errors which swayed our formation of a gaseous Sun [15–21]. These models have evolved as a direct manifestation of mankind's physical knowledge in the 19th and 20th centuries. Through historical review, it can be demonstrated that virtually every salient fact which endowed the Sun with a gaseous interior has actually been refuted or supplanted by modern science. Astrophysics, perhaps unaware of the historical paths followed by its founders [1–14], has at times overlooked the contributions and criticisms of "non-astronomers". Perhaps unable to accept the consequences stemming from the discoveries of the present age, it has continued to perpetuate ideas which can no longer hold any basis in the physical world.

2 Pillars of a gaseous Sun

Five great pillars gave birth to the gaseous Sun in the middle and late 19th century. They were as follows: 1) Laplace's nebular hypothesis [22, 23], 2) Helmholtz' contraction theory [24, 25], 3) Cagniard de la Tour's discovery of critical phenomena [26,27] and Andrew's elucidation of critical temperatures [28, 29], 4) Kirchhoff's formulation of his law of thermal emission [30–32], and 5) the discovery of pressure broadening in gases by Plücker, Hittorf, Wüllner, Frankland, and Lockyer [33–37]. Today, the last four of these pillars have collapsed, either as scientifically unsound (pillar 4), or as irrelevant with respect to discussions of the internal constitution of the Sun and the nature of the photosphere (pillars 2, 3, and 5). Only the first argument currently survives as relevant to solar theory, albeit in modified form. Nevertheless, each of these doctrines had acted as a driving force in creating a gaseous Sun. This was especially true with regards to the ideas advanced by Helmholtz, Andrews, Kirchhoff, and those who discovered pressure broadening.

A careful scrutiny of history reveals that, beyond these factors, the greatest impulse driving mankind to a gaseous Sun was the power of theoretical models. In fact, given that all the great experimental forces have evaporated, astrophysics is left with the wonder of its theoretical formulations. Hence, a 6th pillar is introduced: the stellar equations of state [15–17]. It is an important foundation, one which remains intact and whose influence continues to dominate virtually every aspect of theoretical astrophysics.

2.1 Laplace's nebular hypothesis

Laplace's nebular hypothesis [22,23] was often proposed as a starting point for stellar formation in the 19th century. It became the seed for Helmholtz' contraction theory [24, 25], as will be seen in Section 2.2. Laplace's hypothesis was based on the idea that the Sun and the solar system were created by the slow contraction of a nebulous mass. It was initially outlined in very general terms [38] by Emanuel Swedenborg [39, p. 240–272]. Swedenborg, a Swedish philosopher and theologian, believed himself capable of supernatural communication [40, p. 429]. He made numerous contributions to the natural sciences, but in astronomy, the ideas which brought forth the nebular hypothesis may not be solely his own. Rather, Swedenborg might have simply restated the thoughts of the ancient philosophers [2, 38–40]. Still, for the astronomers of the 19th century, Laplace's name stands largely alone, as the father of the nebular hypothesis.

At present, the Solar Nebular Disk Model (SNDM) [41] has largely replaced the nebular hypothesis, although it maintains, in part, its relationship with the original ideas of Laplace. Space limitation prevents our discussion of these concepts. The point is simply made that, despite the passage of more than two centuries, there remains difficulties with our understanding of the formation of the solar system, as Woolfson recalls: "*In judging cosmogonic theories one must have some guiding principle and that oft-quoted adage of the fourteenth-century English monk, William of Occam, known as Occam's razor, has much to commend it. It states 'Essentia non sunt multiplicanda praeter necessitatem' which loosely translates as 'the simplest available theory to fit the facts is to be preferred'. The characteristics of the SNDM is that it neither fits the facts nor is it simple*" [42].

As for Laplace's nebular hypothesis, it was never specific to a particular solar phase (gas, liquid, or solid). Thus, even Kirchhoff had recourse to the ideas of Laplace in arguing for a solid or liquid photosphere [43, p. 23]. The theory could be applied to all solar models and finds prominence in many discussions of solar formation throughout the 19th century. Logically, however, the concept of a slowly contracting gaseous nebular mass enabled a continuous transition into Helmholtz's theory and the stellar equations of state. This was an aspect not shared by the liquid or solid models of the Sun. Hence, Laplace's ideas, though not counter to the liquid or solid Sun, were more adapted to a gaseous solar mass.

2.2 Helmholtz' contraction theory

Helmholtz' great contraction theory dominated solar science almost since the time it was elucidated at a Königsberg lecture on February 7th, 1858 [24, 25]. The mathematical essence of this lecture was rapidly reprinted in its entirety [24]. Prior to the birth of this theory, solar energy production was based on the meteoric hypothesis as introduced by J.R. Mayer [44], one of the fathers of the 1st law of thermodynamics [45]. The meteoric hypothesis was then championed by Lord Kelvin [46, 47]. Hufbauer provided an excellent description of the evolution of these ideas [14, p. 55–57]. Despite the statures of Mayer [44,45] and Thomson [46,47], the meteoric hypothesis quickly collapsed with the dissemination of Helmholtz' work [24, 25]. The contraction theory became a dominant force in guiding all solar models from the middle of the 19th century through the beginning of the 20th. Given the relative incompressibility of liquids and solids, Helmholtz' concepts were more compatible with the gaseous models. The 1660 law of Boyle [48] and the law of Charles [49], published in 1802 by Gay-Lussac, had just been combined into ideal gas law by Claperon in 1832 [50]. Consequently, it was more logical to assume a gaseous interior. Helmholtz' theory was consequently destined to prominence.

When formulating his contraction hypothesis, Helmholtz emphasized the contraction of nebular material, as advanced by Laplace [24, p. 504]. He stated: "*The general attractive force of all matter must, however, impel these masses to approach each other, and to condense, so that the nebulous sphere became incessantly smaller, by which, according to mechanical laws, a motion of rotation originally slow, and the existence of which must be assumed, would gradually become quicker and quicker. By the centrifugal force, which must act most energetically in the neighborhood of the equator of the nebulous sphere, masses could from time to time be torn away, which afterwards would continue their courses separate from the main mass, forming themselves into single planets, or, similar to the great original sphere, into planets with satellites and rings, until finally the principle mass condensed itself into the Sun*" [24, p. 504–505].

The contraction theory of energy production would not easily yield its pre-eminent position in solar science, surviving well into the 20th century. Still, practical difficulties arose with Helmholz' ideas, particularly with respect to the age of the Earth. Eventually, the concept became outdated. Nuclear processes were hypothesized to fuel the Sun by Arthur Eddington in his famous lecture of August 24th, 1920 [51]. This dramatic change in the explanation of solar energy production [52] would produce no obstacle to maintaining a gaseous Sun. This was true even though Helmholtz' theory had been so vital to the concept of a gaseous interior, both in its inception and continued acceptance. Astrophysics quickly abandoned Helmholtz' contraction hypothesis and adopted an alternative energy source, without any consequence for the internal constitution of the Sun. Ultimately, the advantages of condensed matter in solar fusion were never considered. This remained the case, even though the internuclear proximity within the solid or liquid might have held significant theoretical advantages for fusion when combined with the enormous pressures inside the Sun.

2.3 Andrews and critical temperatures

Addressing the role of Andrews and critical temperatures [28, 29] for solar theory, Agnes Clerke stated: "*A physical basis was afforded for the view that the Sun was fully gaseous by Cagniard de la Tour's experiments of 1822, proving that, under conditions of great heat and pressure, the vaporous state was compatible with considerable density. The position was strengthened when Andrews showed, in 1869, that above a fixed limit of temperature, varying for different bodies, true liquefaction is impossible, even though the pressure be so tremendous as to retain the gas within the same space that enclosed the liquid*" [11, p. 188]. A. J. Meadows echoed these ideas when he later added: "*Andrews showed that there existed a critical temperature for any vapour above which it could not be liquefied by pressure alone. This was accepted as confirming the idea, evolved in the 1860's, of a mainly gaseous Sun whose gas content nevertheless sometimes attained the density and consistency of a liquid*" [13, p. 30].

In the second half of the 19th century, the interior of the Sun was already hypothesized to be at temperatures well exceeding those achievable on Earth in ordinary furnaces. It became inconceivable to think of the solar interior as anything but gaseous. Hence, the gaseous models easily gained acceptance. Even today, it is difficult for some scientists to consider a liquid sun, when confronted with a critical temperature for ordinary hydrogen of −240.18 C, or ~33 K [53, p. 4–121]. In view of this fact, the existence of a liquid photosphere seems to defy logic.

However, modern science is beginning to demonstrate that hydrogen can become pressure ionized such that its electrons enter metallic conductions bands, given sufficiently elevated pressures. Liquid metallic hydrogen will possess a new critical temperature well above that of ordinary hydrogen. Already, liquid metallic hydrogen is known to exist in the modern laboratory at temperatures of thousands of Kelvin and pressures of millions of atmospheres [54–56]. The formation of liquid metallic hydrogen brings with it a new candidate for the constitution of the Sun and the stars [57–60]. Its existence shatters the great pillar of the gaseous models of the Sun which the Andrew's critical point for ordinary gases [28, 29] had erected. It seems that the phase diagram for hydrogen is much more complex than mankind could have imagined in the 19th century. The complete story, relative to hydrogen at high temperatures and pressures, may never be known. Nevertheless, it is now certain: the foundation built by Andrews [28] has given way.

2.4 Kirchhoff's law of thermal emission

Gustav Kirchhoff thought that the solar photosphere was either liquid or solid [43]. He based his belief on the continuous nature of the solar spectrum, adding that its generation by condensed matter was "*the most probable proposition*" [43]. In hindsight, Kirchhoff should have been even more forceful, as the existence of a continuous solar spectrum produced by condensed matter was indeed *the only possible proposition*. Kirchhoff held the answer in his hands nearly 150 years ago, but through the erroneous formulation [61–66] of his law of thermal emission [30–32] he allowed his insight on the state of the photosphere to be usurped by scientific error.

In speaking on the physical constitution of the Sun, Kirchhoff referred to his law of thermal emission in stating: "*for all bodies begin to glow at the same temperature. Draper has ascertained experimentally the truth of this law for solid bodies, and I have given a theoretical proof for all bodies which are not perfectly transparent; this, indeed, follows immediately from the theorem, concerning the relation between the power of absorption and the power of emission of all bodies*" [43, p. 26]. Of course, Kirchhoff's extension of Draper's findings from solid bodies to liquids and gases enabled the creation of a fully gaseous Sun in the 20th century. Kirchhoff's law stated that, within an adiabatic or isothermal opaque cavity at thermal equilibrium, the radiation would always be represented by a universal blackbody spectrum whose appearance was solely dependent on temperature and frequency of observation, irrespective of the nature of the walls (provided that they were not transparent) or the objects they contained [30–32]. Kirchhoff's law argued, by extension, that a gas could produce a continuous blackbody spectrum. Provided that the Sun could be conceived as following the restrictions for enclosure as required by Kirchhoff's law, there could be no problems with a gaseous structure for the production of the continuous solar spectrum. As such, Kirchhoff had already condemned his liquid photosphere [43] three years earlier, when he formulated his "law of thermal emission" [30–32]. According to Kirchhoff's law, liquids and solids were not required to obtain a blackbody spectrum. This unintended error would permeate physics throughout the next 150 years.

The problems with Kirchhoff's law were not simple to identify [61–66] and Planck himself [67, 68] echoed Kirchhoff's belief in the universal nature of radiation under conditions of thermal equilibrium [69, p. 1–25]. Planck did not discover Kirchhoff's critical error. Furthermore, his own derivation of Kirchhoff's law introduced arguments which were, unfortunately, unsound (see [61, 64, 65] for a complete treatment of these issues). In reality, the universality promoted by Kirchhoff's law involved a violation of the first law of thermodynmaics, as the author has highlighted [65, p. 6].

The acceptance of Kirchhoff's law, at the expense of Stewart's correct formulation [70], enabled the existence of a gaseous Sun. Its correction [61–66] immediately invalidates the existence of a gaseous photosphere. Condensed matter is required to produce a continuous thermal spectrum, such as that emitted by the solar photosphere. Blackbody radiation was never universal, as Kirchhoff advocated [30–32] and much of astrophysics currently believes. If Kirchhoff's law had been valid, scientists would not still be seeking to understand the nature of the solar spectrum [71–73] after more than 150 years [74–76]. In reality, the most important pillar in the erection of a gaseous Sun was defective.

2.5 Pressure broadening

Despite the existence of Kirchhoff's law, physicists in the early 1860's understood that gases did not produce continuous spectra. Gases were known to emit in lines or bands. As a result, though Kirchhoff's law opened the door to a gaseous Sun, it was not supported by sound experimental evidence. It was under these circumstances, that the concept of pressure broadening in gases entered astrophysics.

In 1865, Plücker and Hittorf published their classic paper on the appearance of gaseous spectra [33]. They reported that the spectrum of hydrogen could assume a continuous emission as pressures increased: "*Hydrogen shows in the most striking way the expansion of its spectral lines, and their gradual transformation into a continuous spectrum… On employing the Leyden jar, and giving to the gas in our new tubes a tension of about 60 millims, the spectrum is already transformed to a continuous one, with a red line at one of its extremities. At a tension of 360 millims. the continuous spectrum is high increased in intensity, while the red line Hα, expanded into a band, scarcely rises from it*" [33, p. 21–22]. Wüllner quickly confirmed pressure broadening in gaseous spectra [34,35]. Relative to hydrogen, he wrote: "*As the pressure increases, the spectrum of hydrogen appears more and more like the absolutely continuous one of an incandescent solid body*" [35].

During this same period, Frankland [36] and Lockyer made the critical transition of applying line broadening explicitly to the Sun [37]. Much of this discussion was reproduced in Lockyer's text [5, p. 525–560]. They proposed that pressure alone resulted in spectral broadening, excluding any appreciable effects of temperature. This was something which, according to them, had escaped Plücker and Hittorf [33]. They refuted Kirchhoff's solid or liquid photosphere: "*We believe that the determination of the above-mentioned facts leads us necessarily to several important modifications of the received history of the physical constitution of our central luminary — the theory we owe to Kirchhoff, who based it upon his examination of the solar spectrum. According to this hypothesis, the photosphere itself is either solid or liquid, and it is surrounded by an atmosphere composed of gases and the vapours of the substances incandescent in the photosphere… With regard to the photosphere itself, so far from being either a solid surface or a liquid ocean, that it is cloudy and gaseous or both follows both from our observations and experiments*" [37].

Unfortunately, the concept that the spectrum of a gas can be pressure broadened had little relevance to the problem at hand. The line shape was not correct, though this difficulty escaped scientists of this period. The full solar spectrum was not available, until provided by Langley in early 1880's [71–73]. The spectrum of the Sun was not simply broadened, but had the characteristic blackbody appearance, a lineshape that gases failed to reproduce, despite the insistence of Kirchhoff's law to the contrary. In 1897, W. J. Humphreys published his extensive analysis of the emission spectra of the elements [77]. The work only served to re-emphasize that not a single gas ever produced a blackbody spectrum [67–69] through pressure broadening. As a result, the fifth pillar had never carried any real relevance to solar problems.

Hence, astrophysics has had to contend with the inability to generate a Planckian spectrum [67–69] from gases. The spectrum so easily obtained with graphite or soot [61, 65] remained elusive to gaseous solar models, unless recourse was made to a nearly infinite mixture of elemental species and electronic processes [74–76]. As a mechanism, pressure broadening would fall far short of what was required. *A priori*, it shared nothing with the fundamental mechanism existing in graphite and soot, the two best examples of true blackbodies in nature. Consequently, the intriguing discovery of pressure broadening in the 1860's has failed solar science. In reality, the search for the origin of the solar spectrum using gaseous emission spectra has continued to evade astrophysics until the present day, as evidenced by the very existence of The Opacity Project [74, 75].

2.6 The stellar equations of state

Many scientists have not recognized that a slow transformation is taking place in the physical sciences. In large part, this is due to the elegance of the stellar equations of state [15–21] as they continued to evolve from the seminal thoughts of Lane [78], Schuster [79, 80], Very [81], and Schwarzchild [82]. As such, astronomy continues to advocate a gaseous Sun. In doing so, it sidesteps the consequences of solar phenomena and attempts to endow its gaseous models with qualities known only to condensed matter. Simplicity beckons the liquid photosphere through every physical manifestation of its state [57–60]. But, solar physics remains bound by the gaseous plasma.

3 Historical account of the constitution of the Sun

3.1 William Herschel, speculation, and the nature of scientific advancements

Throughout scientific history, the nature of the Sun has been open to changing thought (see Table 1) and, in hindsight, often wild speculation. Even the strangest ideas of our forefathers possess redeeming qualities. It is almost impossible, for instance, to escape the intellectual delight which day-

author	year	sunspots	photosphere	solar body
Thales [5, p. 2]	600 B.C.	?	?	solid
Galileo [101, p. 124]	1612	clouds	fluid	?
Descarte [100, p. 147]	1644	opaque solid mass	fluid	fluid
de la Hire [98, p. 391]	1700	opaque solid mass	fluid	fluid
J. Lalande [98]	1774	opaque solid mass	fluid	fluid
A. Wilson [84]	1774	cavities in photosphere	fluid	dark and solid
W. Herschel [83]	1795	cavities in photosphere	luminous cloud layer	inhabited solid
W. Herschel [88]	1801	cavities in photosphere	luminous cloud/reflective cloud	inhabited solid
F. Arago [89, p. 29]	1848	openings in photosphere	gaseous	solid
J. Herschel [93, p. 229]	1849	cavities in photosphere	luminous cloud/reflective cloud	dark solid
H. Spencer [104, 105]	1858	cyclones	incandescent liquid	gaseous
G. Kirchhoff [43]	1862	clouds	incandescent liquid	solid or liquid
W. Thomson [47]	1862	?	incandescent liquid	incandescent liquid
A. Secchi [95, 96]	1864	openings in photosphere	gaseous with condensed matter	gaseous
J. Herschel [97]	1864	cavities in photosphere	gas?/vapour?/liquid?	dark solid
H. Faye [111, 112, 120]	1865	openings in photosphere	gaseous with condensed matter	gaseous
de la Rue, Stewart, Loewy [133]	1865	openings in photosphere	gaseous with condensed matter	gaseous
Frankland and Lockyer [37]	1865	openings in photosphere	gaseous with condensed matter	gaseous
H. Faye [119]	1872	cyclones	gaseous with condensed matter	gaseous
Modern theory	present	gaseous (magnetic fields)	gaseous	gaseous

Table 1: A partial summary of humanity's concept of the Sun.

dreams of William Herschel's 'solarians' invoke [83]. An inhabited solid solar surface might seem absurd by our standards, but such beliefs dominated a good portion of 19th century thought, at least until the days of Kirchhoff and the birth of solar spectral analysis [30–32, 43]. If Herschel's solarians are important, it is not so much because their existence holds any scientific merit. The solarians simply constitute a manifestation of how the minds of men deal with new information.

As for the concept that the Sun was a solid, the idea had been linked to Thales [5, p. 2], the Greek philosopher, who is said to have pondered upon the nature of the Sun in the 6th century B.C., although no historical evidence of this fact remains [2, p. 81–84]. Lockyer provided a brief discussion of ancient thought on the Sun [5, p. 1–12], in which we were reminded of the words of Socrates that "*speculators on the universe and on the laws of the heavenly bodies were no better than madmen*" [5, p. 5]. Relative to a solid Sun, Herschel did not deviate much from the thoughts of the ancient philosophers whose conjectures were, at times, fanciful [2].

With regard to the photosphere and the "outer layers of the Sun", Herschel placed his distinct mark on solar science. In doing so, he built on the foundation advanced by his predecessor, Alexander Wilson, in 1774 [84]. Herschel wrote: "*It has been supposed that a fiery liquid surrounded the sun, and that, by its ebbing and flowing, the highest parts of it were occasionally uncovered, and appeared under the shape of dark spots; and in that manner successively assumed different phases*" [83, p. 48] … "*In the instance of our large spot on the sun, I concluded from the appearances that I viewed the real solid body of the Sun itself, of which we rarely see more than its shining atmosphere… The luminous shelving sides of a spot may be explained by a gentle and gradual removal of the shining fluid, which permits us to see the globe of the Sun*" [83, p. 51] … "*The Sun, viewed in this light, appears to be nothing else than a very eminent, large, and lucid planet, evidently the first, or in strictness of speaking, the only primary one of our system; others being truly secondary to it. Its similarity to the other globes of the solar system with regard to its solidity, its atmosphere, and its diversified surface; the rotation upon its axis, and the fall of heavy bodies, lead us to suppose that it is most probably also inhabited, like the rest of the planets, by being whose organs are adapted to the peculiar circumstances of that vast globe*" [83, p. 63].

Herschel believed that the Sun was a solid globe surrounded by a photosphere made from an elastic fluid which was responsible for light production: "*An analogy that may* *be drawn from the generation of clouds in our own atmosphere, seems to be a proper one, and full of instruction. Our clouds are probably decompositions of some of the elastic fluids of the atmosphere itself, when such natural causes, as in this grand chemical laboratory are generally at work, act upon them; we may therefore admit that in the very extensive atmosphere of the sun, from causes of the same nature, similar phaenomena will take place; but with this difference, that the continual and very extensive decomposition of the elastic fluids of the sun, are of a phosphoric nature, and attended with lucid appearances, by giving out light*" [83, p. 59].

Though Herschel first described an inhabited star in 1795, he soon discovered infrared radiation [85–87] and realized that the Sun would provide an uncomfortable setting for its population. In a valiant attempt to save his solarians in 1801, Herschel advanced that the luminous layer of the photosphere, floating like a cloud above the solid solar surface, was positioned beyond an inferior reflective cloud which could channel the heat of the photosphere away from the inhabitants of the Sun [88]. Herschel incorporated a new fact, the discovery of infrared radiation [85–87], with a new concept, the reflective layer [88], in order to salvage an existing theory, the inhabited solid Sun [83]. A study of Herschel reminds us that theories are able to undergo many alterations in order to preserve a central idea, even if the sum of new facts has, long ago, shattered its foundation.

3.2 Alexander Wilson's queries and conjectures

It is noteworthy that, unlike William Herschel, Alexander Wilson, in 1774 (see Table I), displayed uncharacteristic caution for speculation. In elucidating his ideas about the constitution of the Sun, the great astronomer placed the entire text in a section devoted to "*Queries and Conjectures*" [84, p. 20–30]. In fact, he dismissed much of the work of his predecessors as hypotheses without sound scientific basis. He was cautious to highlight the speculative nature of his theory on the constitution of the Sun when he wrote: "*When we consider, that the solar spots, some of whose properties have just now be enumerated, are so many vast excavations in the luminous substance of the Sun, and that, wherever such excavations are found, we always discern dark and obscure parts situated below; is it not reasonable to think, that the great and stupendous body of the Sun is made up of two kinds of matter, very different in their qualities; that by far the greater part is solid and dark; and that this immense and dark globe is encompassed with a thin covering of that resplendent substance, from which the Sun would seem to derive the whole of its vivifying heat and energy? And will not this hypothesis help to account for many phaenomena of the spots in a satisfactory manner? For if a portion of this luminous covering were by means displaced, so as to expose to our view a part of the internal dark globe, would not this give the appearance of a spot?*" [84, p. 20]. He continues: "*And from this may we not infer, that the luminous matter gravitates, and is in some degree fluid…*" [84, p. 22]. Wilson brought forth a solid solar body surrounded by a gaseous or liquid photosphere. He was well aware of the limitations of his own knowledge relative to the photosphere, stating that: "*we may never have a competent notion of the nature and qualities of this shining and resplendent substance…*" [84, p. 21]. Wilson was prudent in the manner by which he proposed new ideas. He closed his address by stating with respect to "*many such other questions, I freely confess, that they far surpass my knowledge*" [84, p. 30]. At the same time, Wilson wrote his "*Queries and Conjectures*" precisely because he realized that they formed a basis for further discovery and questioning. In a field as complex as astronomy, devoid of direct contact with the subject of its attention, mankind could adopt no other logical course of action.

3.3 François Arago, John Herschel, and the constitution of the Sun in the mid-1800's

By the middle of the 19th century, there seemed to have evolved both a popular conception of the Sun and a more "scientific" outlook. François Arago [89, 90], the premier astronomer in France during this period, shed light on the growing divide between popular thought and professional astronomy. He discussed the constitution of the Sun in these terms: "*Many conjectures have been offered in explanation of these spots. Some have supposed that the Sun, from which so vast a quantity of light and heat is incessantly emanating, is a body in a state of combustion, and that the dark spots are nothing else than scoriae floating on its surface. The faculae, on the contrary, they suppose due to volcanic eruptions from the liquified mass. The grand objection to this hypothesis is, that it does not suffice to explain the phenomenoa: it has not obtained admission among astronomers. The opinion most in favor in the present day, regards the Sun consisting of an obscure and solid nucleus, enveloped by two atmospheres — the one obscure, the other luminous. In this case, the appearance of the spot is explained by ruptures occurring in the atmosphere, and exposing the globe of the Sun to view…*" [89, p. 29].

Arago's position constituted essentially a restatement of William Herschel [88]. Only the solarians seemed to have disappeared and the inner atmosphere became obscure, rather than reflective. In order to strengthen his position, Arago then added: "*This opinion, however strange it may appear, has the advantage of perfectly explaining all the phenomena, and it acquires a high degree of probability from the consideration, that the incandescent substance of the Sun cannot be either a solid or a liquid, but necessarily a gas*" [89, p. 29]. Arago justified his position for a gaseous photosphere, well ignorant of the discoveries to come, both of his own time and in the years to follow. He stated: "*It is an established fact that rays of light, issuing from a solid or liquid sphere in a state of incandescence, possess the properties of polarization, while those emanating from incandescent gases are devoid of them*" [89,

p. 29]. He immediately emphasized that polarization experiments support this position affording "*proof that the light of the Sun's edges is as intense as that at its center*" [89, p. 29]. Further, "*But from the fact that the light from the edges of the Sun's disk is as intense as that from the center, there follows another consequence; namely, that the Sun has no other atmosphere outside the luminous one; for otherwise the light of the edges, having a deeper layer to penetrate, would be found more weakened*" [89, p. 29].

Of course, François Arago was incorrect in stating that "*light of the Sun's edges is as intense as that at its center*" [85, p. 29]. In fact, the converse was first observed in the days of Galileo [7, p. 274]. Arago's contemporary, Sir John Herschel, wrote: "*The deficiency of light at the borders of the visible disc is in fact so striking, whether viewed through coloured glasses or without their intervention, by projecting its image through a good achromatic telescope on white paper, that it seems surprising it should ever have been controverted*" [91, p. 434]. Yet, Arago had the notion that a difference in path length through gas would account for differences in observed solar brightness. This was not far removed from the modern concept of optical depth which explained the same phenomenon [79–82, 92]. However, in this instance, it is the light visualized from the center of the Sun which is from deeper, and therefore warmer, regions. For modern solar astronomy, differing path lengths into the Sun permit the sampling of warmer areas. In any case, Arago's arguments, relative to polarization as restated in his *Popular Astronomy* [90, p. 457], would be eventually refuted (see below).

As for John Herschel [91, 93, 94], over most of the course of his life, he viewed the constitution of the Sun through the eyes of his father, William: "*But what are the spots? Many fanciful notions have been broached on this subject, but only one seems to have any degree of physical probability, viz. that they are the dark, or at least comparatively dark, solid body of the Sun itself, laid bare to our view by those immense fluctuations in the luminous regions of its atmosphere, to which it appears to be subject*" [93, p. 229]. He stated that the "*more probable view has been taken by Sir William Herschel, who considers the luminous strata of the atmosphere to be sustained far above the level of the solid body by a transparent elastic medium, carrying on its upper surface... a cloudy stratum which, being strongly illuminated from above, reflects a considerable portion of the light to our eyes, and forms a penumbra, while the solid body shaded by the clouds, reflects none*" [93, p. 229]. The same citation can be found in the 10th edition of his work, published in 1869 [94, p. 314–315]. However, in 1864, along with Father Angelo Secchi [95, 96], John Herschel became one of the first professional astronomers to advance the concept that the Sun was gaseous when discussing sunspots in April of that year: "*while it agrees with that of an aggregation of the luminous matter in masses of some considerable size, and some degree of consistency, suspended or floating at a level determined by their ... gravity in a non-luminous fluid; be it gas, vapour, liquid, or that intermediate state of gradual transition from liquid to vapour which the experiments of Gagniard de la Tour have placed visibly before us*" [97]. In so doing, John Herschel was the first to propose that critical phenomena [26–29] may be important in understanding the structure of the Sun [57]. Oddly, he did not deem these ideas of sufficient merit to modify his popular text. In a public sense, John Herschel remained faithful to his father, even though nearly seventy years had elapsed in the "progress" of science.

3.4 Early thoughts of a fluid Sun

Unlike Alexander Wilson [84] and William Herschel [83, 88], who both advocated a solid solar body, the French astronomer Joseph Jérôme Le Français de Lalande thought that the Sun was a fluid. In his *Abrégé d'astronomie* of 1774 [98], Lalande reiterated the sentiment of his French predecessor, M. de la Hire. In 1700 and 1702, de la Hire stated that a sunspot was most likely the result of "*protrusion of a solid mass, opaque, irregular, swimming in the fluid material of the Sun, in which it sometimes dove entirely*" [98, p. 391]. René Descartes [99, 100] expressed essentially the same ideas in his *Principia Philosophiae*, published in 1644 [100, p. 147–152]. Descartes' contributions were outlined in Karl Hufbauer's classic text [14, p. 21].

Lalande also described how Galileo and Johannes Hevelius viewed the Sun as a fluid: "*Galileo, who was in no manner attached to the system of incorruptibility of the heavens, thought that Sun spots were a type of smoke, clouds, or sea foam that forms on the surface of the Sun, and which swim on an ocean of subtle and fluid material*" [98, p. 390–391]. In 1612, Galileo wrote: "*... I am led to this belief primarily by the certainty I have that that ambient is a very tenuous, fluid, and yielding substance from seeing how easily the spots contained in it change shape and come together and divide, which would not happen in a solid or firm material*" [101, p. 124]. Galileo differed from Lalande in advancing that sunspots were gaseous or cloudy versus solid [101, p. 98–101]. But, Galileo was not attached to this aspect of his work: "*for I am very sure that the substance of the spots could be a thousand things unknown and unimaginable to us, and that the accidents that we observed in them -their shape, opacity, and motion- being very common, can provide us with either no knowledge at all, or little but of the most general sort. Therefore, I do not believe that the philosopher who was to acknowledge that he does not and cannot know the composition of sunspots would deserved any blame whatsoever*" [101, p. 98]. It was the act of locating the spots on, or very close to, the surface of the Sun, that Galileo held as paramount [101, p. 108–124]. Thus, Galileo refuted Scheiner: "*I say that for the present it is enough for me to have demonstrated that the spots are neither stars, nor solid matters, nor located far from the Sun, but that they appear and disappear around it in a manner not dissimilar to* *that of clouds*" [101, p. 294–295]. Scheiner, Galileo's constant detractor, believed that special stars strangely coalesced to create sunspots [101, p. 98].

3.5 Kirchhoff, Magnus, Kelvin, and the liquid photosphere

In 1862, Gustav Kirchhoff elucidated the idea of a solid or liquid photosphere: "*In order to explain the occurrence of the dark lines in the solar spectrum, we must assume that the solar atmosphere incloses a luminous nucleus, producing a continuous spectrum, the brightness of which exceeds a certain limit. The most probable supposition which can be made respecting the Sun's constitution is, that it consists of a solid or liquid nucleus, heated to a temperature of the brightest whiteness, surrounded by an atmosphere of somewhat lower temperature. This supposition is in accordance with Laplace's celebrated nebular-theory respecting the formation of our planetary system*" [43, p. 23]. Kirchhoff explained how the Sun, like the planets, was formed through contraction. The Sun remained at the temperature of "white heat" as a result of its greater mass. Kirchhoff cited Arago extensively and was well aware of the work on sunspots by Alexander Wilson. Since the photosphere acted on the body of the Sun, Kirchhoff argued that it must also be heated to the point of incandescence. Relative to the constitution of the Sun, Kirchhoff's entire driving force was the solar spectrum itself. The argument must be echoed, even in the present day.

Unfortunately, it was in speaking of sunspots that Kirchhoff confused the issue: "*But the phenomena exhibited by the solar spots, for whose benefit the hypothesis of a dark solar nucleus was started, may, I believe, be explained more completely and more naturally by help of the supposition concerning the constitution of the sun, which the consideration of the solar spectrum has led me to adopt*" [43, p. 26]. Kirchhoff then advanced that sunspots were the results of layers of clouds which cut off the heat emitted by the incandescent surface of the Sun. Kirchhoff's thoughts were reminiscent of Galileo's [101, p. 98–101], a point not missed by Secchi [3, p. 16], and Faye [5, p. 51–61]. Therefore, Alexander Wilson's cavities were replaced by clouds. Kirchhoff invoked Secchi's work and convection currents to explain why sunspots appear only at certain latitudes and tried to bring understanding to the origin of faculae. This entire portion of the text was somewhat nebulous in logic for a man like Kirchhoff. It would undermine his idea that the photosphere must be solid or liquid based on its continuous spectrum [43].

As an expert in thermal emission, Kirchhoff rapidly objected to Arago's polarization arguments against the liquid. Emphatically, he maintained that Arago's "*statement that incandescent gas is the only source of non-polarized light, is, however, incorrect, for Arago himself mentions that the common luminous gas-flame emits perfectly unpolarized light; and the light in this case is almost entirely caused not by glowing gas, but by incandescent particles of solid carbon which are liberated in the flame. An incandescent haze consisting of solid or liquid particles must act in a manner precisely similar to such a flame*" [43, p. 30]. Kirchhoff further explained that a liquid Sun, whose seas are in continuous motions, would emit light from its surfaces in different directions with respect to our eyes. This destroyed any polarization. The argument was a powerful one, but as will be seen below, it was Kirchhoff's explanation of sunspots which his contemporaries, Secchi and Faye, would reject. In so doing, they would dismiss Kirchhoff's entire vision for the constitution of the Sun. This move on their part reflected, perhaps, their all too hasty conclusions with regards to thermal emission. The error continued to this day.

Heinrich Gustav Magnus [102] also believed that the Sun was a liquid. He was a great supporter of Kirchhoff [43]. On July 11th, 1861, he delivered Kirchhoff's memoire on the chemical constitution of the Sun's atmosphere before the Berlin Academy [103, p. 208]. Magnus demonstrated that the addition of caustic soda (sodium hydroxide) to a non-illuminating gaseous flame generated a tremendous increase in its luminosity [102]. He noted the same effect for the salts of lithium and strontium. In 1864, according to Magnus: "*These studies demonstrate that gaseous bodies emit much less heat radiation than solid or liquid bodies; and that, by consequence, one cannot suppose that the source of solar heat resides in a photosphere composed of gas or vapours*" [102, p. 174]. Magnus' argument was powerful and, for the next 50 years, it continued to impact the constitution of the Sun. It was because of Magnus that photospheric theory would preserve some aspects of condensed matter well into the beginning of the 20th century. It would eventually take the theoretical arguments of men like Schuster [79,80], Very [81], Schwarzschild [82], Eddington [51], and Milne [92] to finally set aside Magnus' contributions [102] and cast the concept of condensed matter out of the photosphere [43].

Kirchhoff liquid Sun was also echoed by William Thomson himself. Lord Kelvin states: "*It is, however, also possible that the Sun is now an incandescent liquid mass, radiating away heat, either primitively created in his substance, or, what seems far more probable, generated by the falling in of meteors in past times, with no sensible compensation by a continuance of meteoric action*" [47]. By the time these words were written, Thomson no longer believed that the Sun could replenish its energy with meteors and wrote: "*All things considered, there seems little probability in the hypothesis that solar radiation is at present compensated, to any appreciable degree, by heat generated by meteors fallings in; and, as it can be shown that no chemical theory is tenable, it must be concluded as most probable that the Sun is at present merely an incandescent liquid mass cooling*" [47]. In the same paper, Thomson discussed Helmholtz' contraction theory, as an extension, it seemed, of the meteoric hypothesis [47]. The contraction and meteoric models of energy generation would eventually prove to be unsound. But, for the time being, Thomson continued to view the Sun as liquid in nature, as did Kirchhoff and Magnus.

At the same time, it is ironic how Kirchhoff, through his law of thermal emission, unknowingly provided for astrophysics the very basis for the downfall of his liquid model. Currently, the entire concept of a gaseous Sun rests on the presumed validity of Kirchhoff's formulation. Nonetheless, early gaseous models of the Sun always placed either solid or liquid constituents in the region of the photosphere, as shall soon be outlined. Not until the early 20th century would the Sun become fully divested of condensed matter. In so doing, astrophysics would endow the gaseous plasma with emission properties it failed to possess on Earth. Regrettably, few of Kirchhoff's contemporaries supported his idea that the Sun was a liquid. Visual observations, and the view that Kirchhoff was an outsider to astronomy, would become ruinous to his model. Critical temperatures [28] also dictated that the Sun was simply too hot to allow this phase. Spectroscopic evidence became of secondary importance and the journey to a gaseous Sun formally began.

4 On to a gaseous Sun

4.1 Men, ideas, and priority

Throughout the history of astronomy, there is perhaps no more controversial figure than Herbert Spencer. As an independent philosopher, not formally trained in science, he became the first to advance that the interior of the Sun was completely gaseous [104–106]. He was also a staunch supporter of evolution and elucidated the concept of "*survival of the fittest*" [107]. In academic circles, Spencer was widely criticized for the views he held, both in ethics and in sociology [108]. By his supporters, he seemed highly admired [108] and compared to other polymaths including the likes of Goeth, Humbolt, and Whewell [103, p. 198]. Unfortunately, many of Spencer's social thoughts were unfounded and promoted concepts of imperialistic superiority and outright discrimination [107, p. 481–483]. His contributions on the constitution of the Sun [104, 105] were essentially ignored by professional astronomy, even though he corresponded with Sir John Herschel and Sir George Airy, the Astronomer Royal [106]. In addition, Spencer was a close friend of the great physicist John Tyndall who became, in like manner, a prominent evolutionist [106]. Spencer's political and social views were so counter to those espoused by men of the period that he remained ever outside the mainstream of astronomy.

Spencer eventually argued for priority over Hervé Faye with respect to his ideas of a gaseous Sun [105]. His defense was in response to review articles by Norman Lockyer published in the magazine *The Reader* [109, 110], about the Frenchman's *Comptes Rendus* papers [111, 112]. Nine years later, Lockyer reprinted these articles in his classic text [5, p. 44–62], without reference to Spencer's letter [105]. In doing so, Lockyer approached misconduct. He added a footnote crediting Balfour Stewart and Gustav Kirchhoff for a thermodynamic argument which the record well demonstrated was first expounded in Spencer's letter, as will be discussed in Section 4.6 [105]. But since Lockyer was the cause of Spencer's 1865 letter [105], he could not have been unaware of its contents.

Bartholomew advanced a somewhat disparaging analysis of Spencer's contributions to solar physics [106]. He attempted to justify Spencer's rejection by professional astronomy. Though he gave Spencer qualities, he charged him with being simply an amateur, a surprisingly desultory reader, and of incorporating in his own writings facts and ideas acquired in other ways [106]. He even accused Spencer with making the Nebular hypothesis the starting point of his discussion, justifying the same behavior by men like Kirchhoff and Faye as merely supportive and confirmatory [106, p. 22]. Though Bartholomew brought forth several other reasons why Spencer was ignored, many of which were perhaps valid, his central argument was summarized as follows: "*Rather, at the mid-nineteenth century a criterion of acceptability for scientific pronouncements was beginning to emerge that was linked to the notion of professionalism; only those who had credentials in their subject through training and research could expect to have their speculative theories taken seriously. As this standard gradually asserted itself, Spencer's work in astronomy lost much of its claim for attention*" [106, p. 21]. This aspect of 19th century thought, beginning to permeate science in Spencer's day, had also been proposed while discussing Robert Chambers' *Vestiges on the Natural History of Creation* which was one of the first works on evolutionary reasoning: "*the reaction to Vestiges was not simply a profession of empiricism: it was an attempt to restrict the privilege of theoretical speculation to a small circle of recognized researchers*" [113, p. 22].

Relative to the Sun, a review of the documents of the period showed no more theoretical brilliance in the works of Secchi [95, 96, 114–118] and Faye [109–112, 119, 120] than in those of Spencer [104, 105]. This was reality, despite the fact that Spencer was charged with being ill-trained in thermodynamics, astronomy, and mathematics [106]. While Secchi was a magnificent observational astronomer [3], all three men were profoundly mistaken in many of their ideas regarding the Sun and sunspots. Furthermore, in light of modern analysis, their differences hinged on the trivial. Few of the early works of either Secchi or Faye were mathematical in nature [95, 96, 109–112, 114–120].

The nature of sunspots had immediately become a focus of contention between Spencer [105] and Faye [120]. In fact, Secchi and Faye would criticize Kirchhoff on the same subject, although they were far from being his equal in theoretical prowess. In *Comptes Rendus*, the battle between Faye and Kirchhoff on sunspots was protracted, extensive [121–126], and would yield many of the modern ideas for a gaseous Sun. Faye and Secchi's defense against Kirchhoff was somewhat justified, relative to sunspots not resting as clouds above the photosphere. But they did not sufficiently appreciate the importance of the German's arguments for condensed matter [43]. For many decades, the contributions of these two men, on the constitution of the Sun, were highly cited and praised. Spencer, their British colleague, continued to be essentially ignored [106].

Consequently, had the scientific community merely erected a means of self-promotion and preservation, with respect to theoretical speculation, by rejecting Spencer's work? This is unlikely to be the only explanation. It was obvious that many despised Spencer's social, ethical, and evolutionary thoughts. Competitive pressures must also have been involved. Hervé Faye clearly became acquainted with Spencer's work, given the three articles presented in *The Reader*. Still, the Frenchman long delayed to cite Spencer. Yet, it was unlikely that mere "scientific exclusivity" could account for Faye's and Lockyer's treatment of Spencer, as Bartholomew proposed. Hervé Faye defended religion and argued on moral grounds against the merits of evolution in addressing both science and God in his classic text which emphasized: "*Coeli enarrant gloriam Dei*" [127, p. 1–4]. As such, it appears that Faye consciously refused to confer upon Spencer the credit he deserved. This was especially true given the struggle for priority and Faye's time in history [127, p. 1–4]. The situation was perhaps clearer for Father Secchi. Secchi likewise echoed "*Coeli enarrant gloriam Dei*" [128, p. 1] and, on his deathbed, paraphrased Saint Paul (2 Timothy 4:7–8): "*I have finished my course, I have fought the good fight. Throughout my entire life and in my scientific career, I have had no other goal but the exultation of the Holy Catholic Church, demonstrating with evidence how one can reconcile the results of science with Christian piety*" [128, p. vii]. It must be remembered that, when the Jesuits would be expelled from Rome, Secchi was defended by the world scientific community. Only Secchi, with his assistants, was allowed to remain in the city and continued to work at the Observatory of the Roman College [128, p. xxii-xxiii]. Did Secchi know in advance of Spencer's *Westminster Review* article [104]? In 1869, Secchi had mentioned, with respect to Lockyer, that "*As to what regards his work, I admit that I have knowledge of only those which were published in Comptes Rendus, or in Les Mondes*" [5, p. 500]. The situation is not definitive however, as Secchi does mention his knowledge of the recent work by William R. Dawes in *Monthly Notices* in his first letter [95]. Nonetheless, it was doubtful that the Director of the Observatory of the Roman College knew of Spencer's works when he wrote his key papers of 1864 [95, 96]. The surest evidence was the lack of similarity between the ideas of Secchi [95, 96] and Spencer [104]. Conversely, this was not the case for Faye's classic papers [111,112], including those dealing with the defense of his sunspot theory [119–126]. The problem for Faye would be three fold: 1) extensive scientific similarity, 2) eventual and certain knowledge of Spencer's rebutal letter in *The Reader* [105] and 3) his claim of simultaneous discovery with respect to Secchi, as will be soon discovered. For Faye at least, it is difficult to argue against deliberate scientific disregard relative to Spencer and his ideas.

Relative to issues of faith, it is also notable that many learned men of the period shared Faye's and Secchi's dual affection for religion and science. In fact, even Max Planck would be counted in their company [129]. Bartholomew failed to address any of these points. It is unlikely that the dismissal of Spencer can be solely attributed to his lack of training, amateur status, and "*an attempt to restrict the privilege of theoretical speculation to a small circle of recognized researchers*" [113, p. 22]. The reality remained that some of Spencer's ideas continued to be objectionable (e.g. [107, p. 481–483]) and that the quest for priority was powerful.

Nonetheless, one must question the persistent failure [7, 13, 14] to give Spencer credit for advancing the earliest model of the gaseous Sun. Bartholomew's discussion [106], in trying to justify the past with the privilege of scientific position and "right to speak", did nothing to advance truth. This was especially highlighted, when contrasted with Galileo's free acknowledgement of Benedetto dei Castelli's contributions to the projection of sunspots [101, p. 126]. It was further expounded by the remembrance of Charles' law by Gay-Lussac [49], even though the former had not written a single word and the experiments were done fifteen years earlier. If the name of *Charles' law* exists, it is only because of Gay-Lussac's profound honesty. As such, the refusal to credit Spencer for his contributions should not be justified by modern writers [106], but rather, must be condemned as an unfortunate injustice relative to acknowledging the genesis of scientific ideas [130]. The reality remains that the birth of a gaseous Sun was accompanied by bitter rivalry throughout professional astronomy, much of which was veiled with struggles for priority. In this expanded context, and given his social views, Spencer's isolation was not surprising.

4.2 Herbert Spencer and the nebular hypothesis

In reality, Spencer's contributions were noteworthy for their dramatic departure from the ideas of Herschel and Arago (see Table 1). Much like other works of the period, Spencer's thesis contained significant scientific shortcomings. Still, his writings were on par with those of his contemporaries and were, it appears without question, the first to outline both a gaseous solar body and a liquid photosphere. Spencer advanced this model in an unsigned popular work entitled *Recent Astronomy and the Nebular Hypothesis* published in the *Westminster Review* in 1858 [104]. He began his thesis by imagining a "*rare widely-diffused mass of nebulous matter, having a diameter, say as great as the distance from the Sun to Sirius*" [104, p. 191] and considered that mutual gravitation would eventually result in the "*slow movement of the atoms towards their common center of gravity*" [104, p. 191]. He argued that, as the nebular mass continued to contract, some

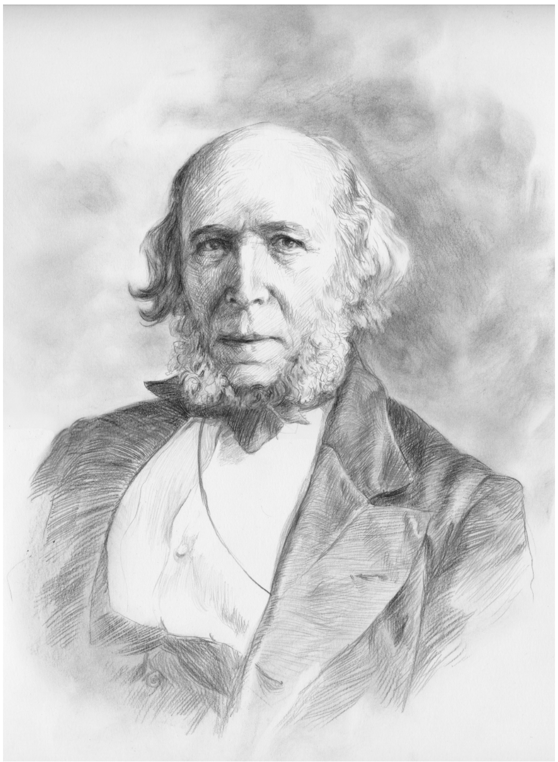

Fig. 1: Herbert Spencer (April 27th, 1820 — December 8th, 1903), was a polymath who advanced the first gaseous model of the Sun, in 1858 [104]. He conceived of a "Bubble Sun", a gaseous interior of variable density surrounded by a fully liquid photosphere. (Drawing by Bernadette Carstensen — used with permission.)

of the internally situated atoms entered into chemical union. With time, as the heat of chemical reaction escaped the nebular mass, the latter began to cool. The binary atoms would then precipitate and aggregate into "*flocculi*" [104, p. 192]. Spencer described how *flocculi* formation resulted in centripetal motion of the nebula and eventually condensed into a larger internal and external aggregate masses. The latter developed into planets and comets. Spencer summarized Laplace's nebular hypothesis as follows: "*Books of popular astronomy have familiarized even unscientific readers with his [Laplace's] conceptions; namely, that the matter now condensed into the solar system once formed a vast rotating spheroid of extreme rarity extending beyond the orbit of Neptune; that as it contracted its rate of rotation necessarily increased; that by augmenting centrifugal force its equatorial zone was from time to time prevented from following any further the concentrating mass, and so remained behind as a revolving ring; that each of the revolving rings thus periodically detached eventually became ruptured at its weakest point, and contracting upon itself, gradually aggregated into a rotating mass; that this like the parent mass, increased in rapidity of rotation as it decreased in size, and where the centrifugal force was sufficient, similarly through off rings, which finally collapsed into rotating spheroids; and that thus out of these primary and secondary rings arose the planets and their satellites, while from the central mass there resulted the Sun*" [104, p. 201].

Spencer succinctly outlined his thoughts on the Sun when he defended himself in *The Reader*. He opened as follows: "*The hypothesis of M. Faye, which you have described in your numbers for January 28 and February 4, is to a considerable extent coincident with one which I ventured to suggest in an article on 'Recent Astronomy and the Nebular Hypothesis,' published in the Westminster Review for July, 1858. In considering the possible causes of the immense differences of specific gravity among the planets, I was led to question the validity of the tacit assumption that each planet consists of solid or liquid matter from centre to surface. It seemed to me that any other internal structure, which was mechanically stable, might be assumed with equal legitimacy. And the hypothesis of a solid or liquid shell, having its cavity filled with gaseous matter at high pressure and temperature, was one which seemed worth considering, since it promised an explanation of the anomalies named, as well as sundry others*" [105]. He continued: "*The most legitimate conclusion is that the Sun is not made up of molten matter all through; but that it must consist of a molten shell with a gaseous nucleus. And this we have seen to be a corollary of the Nebular Hypothesis*" [105].

Throughout the article in *The Reader*, Spencer cited extensively from his prior work [104]. The resemblance to Faye's 1865 papers [111, 112] was difficult to justify as coincidental. Spencer argued strongly for the existence of convection currents within the Sun: "*. . . hence an establishment of constant currents from the center along the axis of rotation towards each pole, followed by a flowing over of accumulation at each pole in currents along the surface to the equator; such currents being balanced by the continual collapse, towards the center, of gaseous matter lying in the equatorial plane*" [105]. The presence of convection currents was to become a central aspect of Faye's model. Nonetheless, Spencer was arguably one of the first to invoke true convection currents within the Sun.

There were several elegant strokes in Spencer's original paper in the *Westminster Review* [104], including his anticipation of the contraction hypothesis which he re-emphasized in *The Reader*: "*Supposing the Sun to have reached the state of a molten shell, enclosing a gaseous nucleus, it was concluded that this molten shell, ever radiating its heat, but ever acquiring fresh heat by further integration of the sun's mass, will be constantly kept up to that temperature at which its substance evaporates*" [105]. He advanced two strata of atmosphere above the molten solar surface, the first "*made up of sublimed metals and metallic compounds*" and the second of "*comparatively rare medium analogous to air*" [105].

Spencer was concerned with the specific gravity of the sun, insisting "*but the average specific gravity of the Sun is about one*" [105]. He ventured: "*The more legitimate conclu-*

sion is that the sun's body is not made up of molten matter all through, but that it consists of a molten shell with a gaseous nucleus... the specific gravity of the Sun is so low as almost to negative the supposition that its body consists of solid or liquid matter from the center to surface, yet it seems higher than is probable for a gaseous spheroid with a cloudy envelope" [105]. Spencer reached this conclusion because he considered only the specific gravity of the metals and materials on Earth. He never realized that the Sun was mostly made of hydrogen. As such, given his building blocks, Spencer was left with a gaseous interior. The insight was profound. In fact, the objection which Spencer made, with respect to the improbability of a gaseous spheroid, would be repeated by the author, before he became acquainted with Spencer's writings [57].

Specific gravity has become a cornerstone of the modern liquid metallic hydrogen model of the Sun [57–60]. At the same time, science must marvel at the anticipation which Spencer gave of the current gaseous models of the Sun when he wrote: "*... but that the interior density of a gaseous medium might be made great enough to give the entire mass a specific gravity equal to that of water is a strong assumption. Near its surface, the heated gases can scarcely be supposed to have so high a specific gravity, and if not, the interior must be supposed to have a much higher specific gravity*" [105]. This is precisely what is assumed by astronomy today, as it sets the photospheric density to $\sim 10^{-7}$ g/cm^3 and that of the solar core to ~ 150 g/cm^3 [57]. With respect to convection currents and intrasolar density, it could be argued that Spencer led astrophysical thought.

Spencer closed his defense by restating his theory of sunspots. He initially advanced that the spots were essentially cyclones and credited John Herschel with the idea [105]. He then stated that cyclones contained gases and that the effects of refraction could account for their dark appearance. Spencer would modify his idea over time, but he continued to focus on cyclones. His conjectures regarding sunspots would have no redeeming features for the current understanding of these phenomena. As such, suffice it to re-emphasize the novelty of Spencer's *Bubble Sun* as a significant departure from the solid model of the period, with the introduction of convection currents and arguments regarding internal solar density.

4.3 Angelo Secchi and the partially condensed photosphere

Angelo Secchi [3] first outlined his ideas regarding the physical constitution of the Sun in the *Bullettino Meteorologico dell' Osservatorio del Collegio Romano* in two 1864 manuscripts [95, 96]. John Herschel followed suit in April of the same year [97]. Secchi's January work, represented a gentle rebuttal of Gustav Kirchhoff, initially relative to sunspots: "*Signor Kirchoff rejects both the theory of Herschel and that of Wilson. We will first permit ourselves the observation that it is one thing to refute Herschel's theory, and quite another to refute Wilson's, and that when the first is laid to rest, the second one hardly collapses*" [95]. Secchi also disagreed with Kirchhoff relative to thermal emission, disputing that all objects at the same temperature produce the same light: "*Kirchoff relies greatly on the principle that all substances become luminous at the same temperature in order to prove that the core of the sun must be as bright as the photosphere. Here it seems to us that two quite different matters have been conflated: that is, the point at which bodies begin to excite luminous waves capable of being perceptible to the eye, and the fact that all [substances] at the same temperature should be equally luminous. We can accept the first of these propositions, and wholly reject the second. In furnaces we see gases of entirely different luminosity from that of solids, and the strongest [hottest] flame that is known — that is, that of the oxyhydrogen blowpipe — is it not one of the least luminous?*" [95]. In this respect, Secchi was actually correct, as Kirchhoff had inappropriately extended his law to liquids and gases. Secchi realized that gases could not follow Kirchhoff's supposition. This was a rare instance in the scientific literature where the conclusions of Kirchhoff were brought into question. Secchi also expounded on his theory of the

Fig. 2: Father Angelo Secchi, S.J. (June 29th, 1818 — February 26th, 1878), was one of the foremost solar astronomers of his day and the Director of the Observatory of the Roman College. In 1864, Secchi advanced a solar model wherein the photosphere was formed of solid or liquid particulate matter floating on the gaseous body of the Sun [95, 96]. (Drawing by Bernadette Carstensen — used with permission.)

Sun in his classic text [95, p. 37]. Nonetheless, considering Secchi's position, his first article displayed a certain sternness with respect to Kirchhoff, closing with the words: "*We wanted, therefore, to say these things less to object to such a distinguished physicist, than to prevent science from taking a retrograde course, especially since history shows that persons of great authority in one branch of knowledge often drag along, under the weight of their opinion, those who are less experienced, even in matters where their studies are not sufficiently deep and where they should not have such influence*" [95]. Secchi appeared to be arguing, much like Bartholomew [106], that astronomy had become too specialized for the non-professional, even if represented by Kirchhoff himself.

The heart of Secchi's conception of the Sun was outlined in his November 1864 paper [96]. Secchi was concerned with the physical appearance of the solar surface: "*The grid-like solar structure seemed to us to offer nothing regular in those parts of the disc that are continuous, and thus the term granular appears very appropriate. Nevertheless, in the vicinity of the sunspots, that of willow leaf remains justified, because we actually see a multitude of small strips which terminate in rounded tips, and which encircles the edge of the penumbra and of the nucleus, resembling so many elongated leaves arranged all around. The granular structure is more visible near the spots, but it is not recognizable in the faculae; these present themselves like luminous clusters without distinguishable separation, emitting continual light without the interruption of dots or of that black mesh*" [96]. He then clarified his model of the solar photosphere: "*Indeed this appearance suggests to us what is perhaps a bold hypothesis. As in our atmosphere, when it is cooled to a certain point, there exists a fine substance capable of transforming itself in fine powder and of forming clouds in suspension, (water transforming into so-called 'vesicular' vapor or into small solid icicles), so in the enflamed solar atmosphere there might be an abundance of matter capable of being transformed to a similar state at the highest temperatures. These corpuscles, in immense supply, would form an almost continuous layer of real clouds, suspended in the transparent atmosphere which envelopes the sun, and being comparable to solid bodies suspended in a gas, they might have a greater radiant force of calorific and luminous rays than the gas in which they are suspended. We may thus explain why the spots (that are places where these clouds are torn) show less light and less heat, even if the temperature is the same. The excellent results obtained by Magnus, who has proved that a solid immersed in an incandescent gas becomes more radiant in heat and light than the same gas, seem to lend support to this hypothesis, which reconciles the rest of the known solar phenomena*" [96]. Secchi's model differed from Spencer's [104, 105] in that his photosphere was not a continuous layer of liquid. Rather, Sechhi's Sun was essentially gaseous throughout. In his photosphere, solid matter was suspended within the gas. Secchi adopted this model as a result of his visual observations and of Magnus' work on the thermal emission of caustic soda in the transparent gas flame [102]. In this regard, Secchi demonstrated a relatively good understanding of thermal emission.

Over the years, Secchi refined his model of the Sun, but the discussions would be highly centered on the nature of Sun spots. Secchi was a prolific author with more than 800 works to his name [128, p. xvi]. A partial listing of these, compiled at his death, included more than 600 publications [128, p. 95–120]. By necessity, the focus will remain limited to only five of his subsequent contributions on the Sun [114–118].

In the first of these publications [114], Secchi examined sunspots and largely confirmed Wilson's findings [84] that sunspots represented depressions on the solar disk. For both Secchi and Faye, this became a key objection to Kirchhoff's "*cloud model*" of sunspots [43].

In the second article, published in 1868 [115], the astronomer was concerned with the observation of spectral lines in the corona, but he concluded with a defense of the gaseous Sun. Secchi referred to a "*famous objection*" against his model, but never named the source. In actuality, for Secchi, the source of the objection must have been Kirchhoff's *Comptes Rendus* article, which appeared the previous year: "*From the relation which exists between the emissive and absorptive power of bodies, it results in an absolutely certain manner, because in reality the light emitted by the solar nucleus is invisible to our eye, this nucleus, whatever its nature may be, is perfectly transparent, in such a manner that we would visualize, through an opening situated on the half of the photosphere turned in our direction, through the mass of the solar nucleus, the internal face of the other half of the photosphere, and that we would perceive the same luminous sensation as if there was no opening*" [121, p. 400]. Kirchhoff's objection was almost identical to that first leveled by Spencer in 1865 [105, p. 228]: "*But if these interior gases are non-luminous from the absence of precipitated matter must they not for the same reason be transparent? And if transparent, will not the light from the remote side of the photosphere, seen through them, be nearly as bright as that from the side next to us?*" Kirchhoff had strong ties with Guthrie, Roscoe, and the English scientific community. In addition, in light of the previous incident between Kirchhoff and Stewart on priority in thermal emission [61, 138] it is difficult to imagine that the German scientist was unaware of Spencer's work. Two years had already passed.

In response to Kirchhoff, Secchi stated: "*The objection consisted in holding that, if Sun spots were openings in the photosphere, one should be able to see through a gaseous solar mass the luminous photosphere on the other side: as a result, Sun spots would be impossible, since they are not luminous, but black*" [115]. Secchi advanced two lines of defense: "*1) that sunspots, even in their nucleus, are not deprived of light and 2) that for the entire solar mass to be able to produce an absorption capable of preventing the visualiza-*

tion of the other side, it suffices that the interior of the Sun possess an absorbing power identical to its external atmosphere" [115]. Here was perhaps the conclusion of one of the first discussions concerning internal stellar opacity. It reflected why Spencer's complaint was central to the history of astronomy.

Secchi's third work in this series [116] was surprising for two reasons. First, Secchi described that he "*even believes he has seen traces of water vapour in the Sun, especially near the sunspots*" [116, p. 238]. Secondly, and most importantly, Secchi appealed to the French scientific community and to Mr. Sainte-Claire Deville to work on observing the incandescent light emitted by hydrogen under conditions of high pressure [116]. Sainte-Claire Deville immediately followed Secchi's letter with an affirmative response. Secchi thus highlighted the importance of line broadening in hydrogen [33–37] for astrophysical thought [116, p. 238].

In the fourth work of this series, Secchi once again argued that "*sunspots are cavities in the photosphere in whose interior the absorbing layer is thicker*" and continues that "*the brilliant lines that often traverse their nucleus could well be the direct lines of that gas which I have signaled constitutes the gaseous mass of the interior of the Sun*" [117, p. 765]. Secchi was completely mistaken, as these lines do not originate from inside the solar body. His 1869 argument [117] was also counter to that which he already outlined when speaking on stellar opacity a year earlier [115].

In the final work of interest, Secchi described four possible aspects of the chromosphere including: "*The first aspect is one of a layer clearly terminated, as would be the free surface of a liquid... sometimes, especially in the region of faculae, the surface is diffuse*" [118, p. 827]. Secchi completed his 1872 work with a detailed visual description of prominences.

Secchi also entered into a prolonged confrontation in *Comptes Rendus*, initiated by Lockyer, over the constitution of the Sun (reprinted in [5, p. 500–515]). The arguments were spectroscopic in nature and focused on the photosphere, the reversing layer, and the chromosphere. The rivalry, surrounding the gaseous models, had become intense.

In summary, a detailed review of Secchi's work reveals that he was truly an "observational astronomer". Though his initial contributions on the Sun were devoid of mathematical arguments, he displayed a keen sense of deduction, a broad scientific knowledge, and a profound honesty. Unlike Spencer [104, 105], Secchi did not bring to prominence the presence of convection currents inside his gaseous Sun. He based his solar model on the appearance of the solar surface and the work of Magnus [102]. Secchi opposed Kirchhoff [43] on the appearance of sunspots, correctly arguing for Wilson's cavities [84]. Secchi also disputed Kirchhoff's law [30–32] as experimentally unfounded relative to gases [95]. In his book, Secchi provided a discussion of thermal radiation [3, p. 311–319], reminding us of the work of Melloni who demonstrated that: "*different substances possess a particular and elective absorbing force, each of which acts on different heat rays, absorbing some while permitting others to pass, much like colored media acts on white light*" [3, p. 311]. Herein lays Secchi's objection to the universality of Kirchhoff's formulation [30–32]. He recognized the emphasis of his day on line broadening [33–37] and was one of the first to invoke significant stellar opacity [115]. Unfortunately, he advanced seeing water on the solar surface [116, p. 238]. Eventually, mankind would indeed discover water on the Sun [131], but Secchi and his model, by then, would be long forgotten.

4.4 de la Rue, Stewart, Loewy, Frankland, and Lockyer

Shortly after Secchi published his commentaries in *Bullettino Meteorologico* and in *Les Mondes* [95, 96], Warren de la Rue, Balfour Steward, and Benjamin Loewy made their famous report on their theory of sunspots on January 26, 1865. Armed with the sunspot observations of Carrington [132], they expanded on his discoveries [133–137]. Carrington led a tragic life [138, p. 117–128] and was an amateur [13, p. 32]. His observational work, unlike Spencer's ideas, became a cornerstone of astronomy. Presumably, this was because Carrington established the differential rotation of the Sun [132]. He also stayed clear of controversial philosophy and of theorizing on the internal constitution of the Sun. As for de la Rue, Stewart, and Loewy, their contributions with the photoheliograph at Kew were significant. As professional scientists, they ventured into a discussion on the constitution of the photosphere. Historically, their classic paper [133], like Faye's [111, 112], also appeared immediately after the *Les Mondes* translation of Secchi's seminal work [96].

Nonetheless, de la Rue, Stewart, and Loewy were the first [133] to propose that the continuous solar spectrum was consistent with a *fully* gaseous atmosphere. They were quickly endorsed by Frankland and Lockyer who, after believing they had disarmed Kirchhoff, wrote: "*That the gaseous condition of the photosphere is quite consistent with its continuous spectrum. The possibility of this condition has also been suggested by Messrs. De la Rue, Stewart, and Loewy*" [37]. The argument was based on the existence of pressure broadening, observed with hydrogen under conditions of high pressure [37]. It was here that pressure broadening became permanently linked to the gaseous models of the Sun. However, the idea of a fully gaseous photosphere would not truly take hold until much later. For most scientists, the photosphere continued to have at least traces of condensed matter. As for the concept that hydrogen, under pressure, could create a Planckian blackbody spectrum, it was always erroneous. Gases could never produce the required emission [77]. Frankland and Lockyer could not have established this fact with the experimental methods of 1865. They merely observed that the hydrogen lines became considerably broadened, completely unaware of their incorrect lineshape. Irrespective of this shortcoming, the paper by Frankland and

Loewy impacted scientific thought for the rest of the century and became highly cited by the astronomical community. As such, Frankland and Lockyer, along with de la Rue, Stewart, and Loewy who had so magnificently photographed the Sun, hold a preeminent role in the history of solar science [37, 133–137].

Addressing faculae, de la Rue and his team reported: "*It would thus appear as if the luminous matter being thrown up into a region of greater absolute velocity of rotation fell behind to the left; and we have thus reason to suppose that the faculous matter which accompanies a spot is abstracted from that very portion of the sun's surface which contains the spot, and which has in this manner been robbed of its luminosity*" [134]. Based on such observations, they ventured: "*From all of this it was inferred that the luminous photosphere is not to be viewed as composed of heavy solid, or liquid matter, but is rather of the nature either of a gas or cloud, and also that a spot is a phenomenon existing below the level of the sun's photosphere*" [134]. The proposal resembled Secchi's [95, 96]. With these words, Kirchhoff's thermodynamic reasoning, regarding the continuous solar spectrum, became supplanted by visual observations and the Sun adopted the gaseous state.

Given Stewart's earlier conflict with Kirchhoff [61, 139], it would not be unexpected if the Scottish astronomer, at the side of de la Rue and Loewy, had agreed to dispense with Kirchhoff's condensed photosphere [133–135]. However, this was not to be the case. Stewart, a man of strong moral character [140, 141], immediately abandoned de la Rue's gaseous sun, as we will come to discover in Section 4.7.

Beyond Stewart, a historical review of the period reveals that virtually every prominent astronomer voiced public disapproval of Kirchhoff's liquid photosphere. In a real sense, Kirchhoff stood essentially undefended against much of the scientific community. Yet, were the arguments of men like Secchi, Faye, de la Rue, and Lockyer truly sufficient to eventually advance a fully gaseous photosphere? Note in this regard, the *faux pas* by de la Rue, Stewart, and Loewy as to the cause of sunspots in their very next paper: "*the behavior of spots appears to be determined by the behavior of Venus*" [134]. Though Kirchhoff might have misjudged the nature of sunspots, the fault was minor and irrelevant today when compared to the error of assigning an improper phase to the entire Sun. In this respect, Galileo's words in his first letter to Welser come to mind: "*For the enemies of novelty, who are infinite in number, would attribute every error, even if venial, as a capital crime to me, now that it has become customary to prefer to err with the entire world than to be the only one to argue correctly*" [101, p. 89].

4.5 Hervé Faye and loss of the solar surface

Hervé Faye opened his classic presentation on the constitution of the Sun on January 16th, 1865, by stating that the solar phenomena had been well popularized [103]. Therefore, he reduced his historical discussions to the strict minimum and limited himself to the simple analysis of current facts and conjectures [111]. He set the stage by recalling the gaseous envelope and the polarization arguments of Arago [111, p. 92–93]. At the same time, he recognized the importance of Kirchhoff's spectroscopic studies and wrote: "*But incandescent solids and liquids alone give a continuous spectrum, while the gases or the vapors supply but a spectrum reduced to only a few luminous rays*" [111, p. 93]. Faye then argued against Kirchhoff's view of sunspots, as rejected, even by Galileo [111, p. 94]. He proposed that sunspots were produced by clearings in the photosphere, thereby exposing the nucleus of the Sun. Interestingly, Faye argued for the oblateness of the Sun based on the fluidity of the photosphere. Unfortunately for him, the slight oblateness of the Sun [142] supported a condensed photosphere, not one with a gaseous composition [57]. In his seminal communication [111], Faye did not actually advance a complete solution for the nature of the photosphere. He reserved this critical step for his second paper [112].

Fig. 3: Hervé Faye (October 1st, 1814 — July 4th, 1902) was a prominent French astronomer with a distinguished career in science and public service as a minister of education. In early 1865, Faye echoed Secchi's solar model wherein the photosphere was formed of solid or liquid particulate matter floating on the gaseous body of the Sun [111, 112]. (Drawing by Bernadette Carstensen — used with permission.)

Throughout his first work [111], Faye cited many notable figures, but failed to mention either Magnus or Spencer and, more importantly, Secchi's model [111]. Faye studied under the tutelage of François Arago who, as discussed in Section 3.3, visualized a divide between professional astronomy and popular thought, even in the first half of the 19th century. As such, Bartolomew's arguments for the failure to cite Spencer might be given some weight [106]. But what of Faye's failure to mention Secchi's model?

Secchi was an established scientist and well recognized throughout the western world, especially in Roman Catholic France. Secchi's first Italian paper in the *Bullettino Meteorologico* had already been published for nearly one year [95] by the time Faye gave his address [111]. Secchi's second paper on the constitution of the photosphere was immediately translated into *Les Mondes* by l'Abbé Moigno. It appeared in Paris on December 22nd, 1864 [96]. This was nearly one month prior to Faye's presentation before *l'Académie des Sciences* on January 16th. Faye's first paper was silent on this point. Nonetheless, in his second paper, presented on January 25th of the same year, Faye reported that "*I have seen, a few days ago, a correspondence by Father Secchi, who has much too studied the Sun to share the popular view reigning today on the liquidity of the photosphere, that our corresponding scientist has arrived from his side to an explanation of sunspots founded on the same principle*[1]" [112, p. 146]. The footnote in Faye's sentence referred to Moigno's translation of Secchi's second paper [96].

Faye's second paper began with a discussion of solar rotation and particularly of the work of Carrington [112, p. 140–142]. He then discussed Helmholtz' contraction hypothesis [112, p. 143] and highlighted the enormous temperatures inside the Sun as a cause of the complete dissociation of its constituents. These gases rose to the solar exterior where they condensed into non-gaseous particles susceptible to incandescence. Faye reasoned that the formation of the photosphere was simply a consequence of the cooling of internal gases [112, p. 144]. He reconciled Arago's argument on polarization with Kirchhoff's need for a continuous spectrum [112, p. 145]. In so doing, he advanced a photosphere based essentially on Secchi's model when he described: *incandescent particles, floating on a gaseous medium*" [111, p. 145]. Faye then highlighted that sunspots were produced by the visualization of the gaseous solar interior [112, p. 146]. This became the source of Spencer's "famous objection" in *The Reader* [105] and reflected Faye's incomplete comprehension of thermal emission.

Faye closed his second paper with an elaborate description of the vertical convection currents which he postulated were present inside the Sun. He replayed much of Spencer's ideas on the Nebular hypothesis and solar cooling. The Frenchman stated that, given sufficient time, the photosphere would become very thick with the "*consistence of a liquid or a paste*". Herein, he directly linked his ideas to Spencer's liquid photosphere [104]. Hence, along with the arguments based on convection currents, Faye introduced another source of priority claims for the British scholar. Faye's initial exposition [111, 112] was more extensive than Secchi's [95, 96], but not significantly superior to Spencer's [104, 105].

Once his papers on the Constitution of the Sun were presented to the Académie, Faye published a slightly different work in *Les Mondes* [143] in which he again stated that Father Secchi arrived at the same conclusion regarding the photosphere. The Frenchman sought Secchi's approbation [143, p. 298]. As for Secchi, he gallantly responded to Faye's *Les Mondes* article in a letter published in *Comptes Rendus*, on March 6th, 1865 [144]. Secchi wrote in most charitable terms, as if delighted by Faye's claim of simultaneous discovery. If anything improper had occurred, it was silently forgiven. A few years later, in 1867, Secchi would receive *la croix d'officier de la Légion d'Honneur* from the hand of Napoleon III [128, p. iii, 208].

Faye first addressed the sunspot problem in his model within his third paper on the constitution of the Sun, published in 1866 [120]. He began the discourse by praising English astronomy and citing every prominent British astronomer of the period, including Herschel, Carrington, Dawes, Nasmyth, Stone, Huggins, de la Rue, Stewart, Thomson, and Waterston. Spencer was absent from the list. Still, the focus of Faye's work was a direct address of Spencer's complaint with respect to solar opacity: "*The difficulty is relative to the explanation of sunspots. We know that gases heated to the point of becoming luminous never rise to the point of incandescence; the latter being a property of solid particles, even when they are reduced to the same tenuousness*" [120]. Faye restated Secchi's idea that the photosphere was made of fine condensed incandescent particles floating in a gaseous medium. If these particles were missing from a region, it would necessarily become obscure. This was his explanation of sunspots: regions devoid of these incandescent particles. Faye then raised the "famous objection", without mentioning Spencer's name, as if the charge had come from nowhere: "*In this we object that if gases emit but little light, by consequence they are transparent. If then an opening was made in the photosphere, one should see, across the gaseous internal mass of the Sun, the opposite region of the same photosphere with a brilliance barely diminished; as a result there would no longer be any spots*" [120]. It was only later, in 1867, that Faye was finally forced to acknowledge Spencer as a source of the complaint [122, p. 404]. He did so in a footnote, while insisting that the reproach had first been brought to his attention by the editor of *Comptes Rendus*. This was the most assured means of preventing impropriety. In the same work, Faye remained silent on Spencer's convection currents, variations in solar density, and justified priority claim for a gaseous solar interior.

Faye addressed the complaint by arguing that, in fact, it was a property of gases or vapors to extinguish light as well as an opaque body, provided that the thickness of the gas was sufficient. Faye was essentially invoking optical thickness and, once again, foreshadowing the modern stellar opacity problem. In answering Secchi [144], Faye presented his idea that the interior of the Sun could be viewed as concentric layers of gas [145, p. 296]. The thought was to remain associated with the treatment of the internal constitution of the Sun and was also used by Eddington in advancing his theoretical treatment of the problem [19].

As for Faye's debate with Kirchhoff, it was less than cordial. The battle began when Faye improperly described Kirchhoff's model in the literature [120]. Kirchhoff would rebuke Faye for maintaining that horizontal convection currents did not occur at the level of the photosphere: "*Mr. Faye then rejects the existence in the solar atmosphere of horizontal currents which, in my hypothesis, must explain the different movements of sunspots*" [121, p. 398]. Unlike Kirchhoff, Faye invoked internal convection currents with a vertical displacement. On the surface of the Sun, he wanted voids to obtain the spots, not horizontal currents [122, p. 403]. Faye responded to the father of spectral analysis in the most inappropriate tone: "*I congratulate myself in having received a personal intervention from Mr. Kirchhoff, because his letter explains to me something of which I have always been profoundly astonished, to know the persistence with which a man of such high merit can sustain a hypothesis so incompatible with the best known facts*" [122, p. 401]. Faye, of course, referred to Kirchhoff's cloud model of sunspots. In any case, Faye's arrogance in the published article was met eventually by a sound defeat at the hand of Kirchhoff [124].

Faye was so concerned by Kirchhoff's first letter of objection that he drafted a second response, which was mathematical in nature [123], even before the German had the opportunity of reply to his first answer [122]. In this letter, the Frenchman invoked that the nature of sunspots was similar to the darkened grid associated with solar granulation. He went on to dispute, like his mentor Arago (see Section 3.3), the existence of the corona [123]. Both statements were erroneous. Then, Faye opened a new line of defense for his sunspot theory and the controversy relative to seeing through the Sun. He believed that he could counter Kirchhoff and Spencer by advancing that the gas density inside the Sun was not homogeneous. He began by arguing that the interior of the Sun was highly variable in density [123, p. 222–223]: "*In consequence this central density must be many hundreds or even thousands of times superior to that of the superficial layer which forms the photosphere*". Once again, he failed to credit Spencer, this time regarding varying internal solar densities [105]. Faye then proposed a gaseous internal medium which could be viewed as spherical layers of material [123, p. 222–223]. He advanced the same idea a year earlier during a discussion with Father Secchi [146]. The concept has remained in astronomy to the present.

Finally, Faye made his critical misstep. He invoked that a ray of light which hit the higher density of the mass inside the Sun was refracted inward and unable to escape. The astronomer then audaciously charged Kirchhoff with failing to understand the consequences of a non-homogeneous solar interior.

Kirchhoff was severe in his defense. Using his law of thermal emission, Kirchhoff disarmed Faye. He reminded the scholar that the radiation inside an opaque enclosure must be black [124]. As such, Kirchhoff was, ironically, the first person to postulate that the radiation inside a gaseous Sun, surrounded by an enclosing photosphere, must be black. In reality, Kirchhoff's conclusion was only partially correct. The solar photosphere produced a thermal spectrum. However, it was not truly black, since the Sun maintained convection currents which prevented this possibility. Nonetheless, if the photosphere was condensed and perfectly enclosed a gaseous solar body, then that interior would have to contain the same thermal radiation as emitted on the solar surface. Still, Kirchhoff was mistaken in believing that the radiation would have to be black. It would take many years before this reality became apparent [61–66]. In any case, Kirchhoff's arguments, though not completely sound, well surpassed Faye's physical knowledge of the problem. With time, the modern theory of the Sun eventually applied Kirchhoff's ideas to the problem of internal stellar opacities. In doing so, it removed the condensed nature of the photosphere as a primary source of photons. Therefore, there was a great difference between the problem addressed by Faye and Kirchhoff and the current gaseous models of the stars. Kirchhoff and Faye were dealing with photons produced initially by condensed matter in the photosphere. The modern theory holds that such photons could be generated in the solar core, without recourse to condensed matter and without having the Sun enclosed by its condensed photosphere.

The great battle between Faye and Kirchhoff over the nature of sunspots and the solar constitution would end with a whimper. Faye advanced [125] that Kirchhoff had abandoned his model, because the German failed to defend it in his rebuttal letter [124]. Kirchhoff retorted by emphatically arguing that he continued to defend his solar theory [126].

As for Faye, he was completely unable to respond to Kirchhoff's closing argument on the presence of blackbody radiation inside a gaseous solar model. In 1872, he finally abandoned his first theory of sunspots, replacing it with cyclonic formation, an idea for which he once again failed to credit Spencer. Yet, in closing the openings he had created in the photosphere, Faye finally referred to Spencer [119] for his "famous objection". By this time, the problem of internal solar opacity had become irrelevant. Mankind became, at least for the moment, theoretically unable to "see within the Sun". The fully gaseous models, advanced in the 20th century, reintroduced the concept that scientists could visualize differing depths within the Sun. Despite the lack of the enclosure, as required by Kirchhoff in his 1867 letter [124], the modern solar interior has been hypothesized to contain blackbody radiation [15–17].

As a point of interest, the differences between Faye's, Secchi's, and Lockyer's concepts of sunspots have been reviewed in the 1896 version of Young's classic text [8, p. 182–190]. Today, nearly all of these ideas have been abandoned. Much of the controversies which called for the dismissal of Kirchhoff's condensed photosphere have long ago evaporated. The Wilson effect alone remains [84], as a standing tribute to that great English astronomer, who unlike Faye and many of his contemporaries, was so careful relative to queries and conjectures.

4.6 Discord, stellar opacity, and the birth of the gaseous Sun

Imagine a gaseous Sun. The idea was so tantalizing for men of the period that it became a source of instant quarrel for priority. Secchi gently rebuked Kirchhoff [95], absolved Faye [144], and defended himself against Lockyer [5, p. 500–515]. Faye, in turn, battled with Kirchhoff [121–127] and after securing the blessing of Father Secchi [144], was quick to announce his innocence before the Académie: "*This letter [from Secchi] demonstrates that we followed at the same time, Father Secchi and I, a train of ideas which was altogether similar...*" [145, p. 468]. Like his English counterparts, Faye acted as if he was also unaware of John Herschel's 1864 article [97]. But what could be said of this coincidence of ideas? Was it really possible that, in the span of a few months, Secchi, Herschel, Faye, Lockyer and Frankland, and de la Rue along with Stewart and Loewy all independently conceived of the same idea? Faye addressed the question: "*With respect to the analogies that Father Secchi signals with reason between his ideas and mine, coincidences of this type offer nothing which can surprise, identical ones [ideas] are produced every time that a question is ripe and is ready for a solution*" [145]. But surely, the argument could not be extended to every prominent astronomer of the period. Being first and very likely ignorant of Spencer's English text [104], only Secchi could claim truly independent thought.

After hearing from the Jesuit astronomer, Faye finally cited Magnus [145, p. 471], the scientific element which was central to his model, but which, unlike Secchi, he had so neglected in his earlier works. However, if one accounted for Spencer's and Secchi's ideas in Faye's famous papers [111, 112], there was not much left as original thought. The most significant exception was Faye's idea that the photosphere of the Sun was devoid of a real surface [13, p. 42], also advanced in *Les Mondes* [143]. Faye believed that the "*presence of the photosphere does not interrupt the continuity of the [central] mass*" of the Sun [143, p. 301] and insisted that "*This limit is in any case only apparent, the general milieu where the photosphere is incessantly forming surpasses without doubt more or less the highest crests or the summits of the incandescent clouds*" [143, p. 298]. Such was the first consequence of the gaseous models: there could be no defined solar surface. The problem continues to haunt astrophysics to this day [57, 146]. With Faye, the Sun lost its distinct surface.

It is evident that Faye never properly acknowledged Spencer [120, p. 235]. Nonetheless, he remained delighted that his works had been immediately reviewed in *The Reader* by Lockyer, as evidenced by his 1865 letter [145]. As such, it is doubtful, as early as 1865, that he never knew of Spencer's rebuttal [105]. Faye behaved as if concerns against his "*transparent solar interior*" originated exclusively from Kirchhoff as late as 1866 [121]. In fact, it was clear that the criticism of seeing through the Sun had been swiftly leveled by Spencer [105, p. 228]. Since Kirchhoff was a friend of Roscoe [61], it was not unlikely that he quickly became aware of *The Reader* series. Once again, Spencer wrote: "*But if these interior gases are non-luminous from the absence of precipitated matter must they not for the same reason be transparent? And if transparent, will not the light from the remote side of the photosphere, seen through them, be nearly as bright as that from the side next to us?*" [105, p. 228]. Meadows argued that this criticism of Faye's work originated from Balfour Stewart [13, p. 41–42], but did so without citation. In fact, the reference to Balfour Stewart was provided by Norman Lockyer, when he reprinted his letters, in 1874, and added a footnote giving credit to Balfour Stewart over Kirchhoff [5, p. 57], well after Spencer made his case. This was how Lockyer distorted the scientific record using a footnote: "*This note was added to the article as it originally appeared, as the result of a conversation with my friend Dr. Balfour Stewart. I am more anxious to state this, as to him belongs the credit of the objection, although, as it was some time afterwards put forward by Kirchhoff, the latter is now credited with it, although it was noticed by Faye, Comptes Rendus, vol. lxiii, p. 235, 1866. The idea is this: — If the interior solar gases are feeble radiators, then, on the theory of exchanges, they must be feeble absorbers; hence they will be incompetent to absorb the light coming through the hypothetically gaseous Sun from the photosphere on the other side (1873)*" [5, p. 57]. One can only wonder why the discoverer of Helium, one of the great fathers of spectral analysis, and the founder of the journal *Nature*, insisted on altering the historical record. Apparently, Spencer was not as weak in thermodynamics, as previously argued [106].

4.7 Stewart, Kirchhoff, and amateurs

Stewart had been an author on the initial paper with de la Rue and Loewy [133–135]. But suddenly, he detached himself from this position when he discussed the photosphere, without invoking the presence of a gas: "*Next with regard to the photosphere or luminous envelope of the Sun, this surface, when viewed through powerful telescopes, appears granulated or mottled... But besides this there is reason to believe that great defining as well as magnifying power discloses the fact that the whole photosphere of the Sun is made up of detached bodies, interlacing one another, and preserving a great amount of regularity both in form and size*" [147]. Thus, when Stewart wrote independently, it was obvious that he actually believed that the photosphere was a liquid or solid. In this respect, he became aligned with Spencer and Kirchhoff on the condensed nature of the photosphere.

In his *Lessons in Elementary Physics*, Stewart persisted in breaking from de la Rue and Loewy [148, p. 279]. This was the case even in the edition published closest to the end of his life. In this classic text for its day, Stewart stated: "*If we throw upon the slit of our spectroscope an image of the Sun or of one of the stars, with the view of obtaining its spectrum, we find a large number of black or dark lines in a spectrum otherwise continuous, and we argue from this that in the Sun or stars we start with a solid or liquid substance, or at any rate with some substance which gives us a continuous spectrum, and that between this and the eye we have, forming a solar or stellar atmosphere, a layer of gas or vapours of a comparatively low temperature, each of which produces its appropriate spectral lines, only dark on account of the temperature of the vapours being lower than that of the substance which gives the continuous spectrum*" [148, p. 279]. Again, there was no mention of a gaseous photosphere supporting condensed matter precipitates in this description of the problem. In fact, this passage echoed Kirchhoff's explanation [43], as Stewart was all too aware of the nature of thermal emission in gases [149].

Hence, the Scottish physicist very much desired that the photosphere be condensed, as evidenced initially in his 1864 article: *On the Origin of Light in the Sun and Stars* [150]. In this work, Stewart advanced that planets could alter the brightness of stars by modifying the amount of sunspots. He tried to answer the question "*From all this it is evident that in the case of many stars we cannot suppose the light to be due to an incandescent solid or liquid body, otherwise how can we account for their long continued disappearance?*" [150, p. 452]. The entire manuscript was aimed at accounting for this disappearance, even if the photosphere was solid or liquid. He stated in this regard "*if it can be proved, as we think it can, that a disc full of spots is deficient in luminosity*" [150, p. 452]. Stewart made this conjecture to explain the occurrence of variables [150]. For him, the photosphere had to be liquid or solid. But variable stars posed a tremendous scientific difficulty. As a result, he required something like planets to modify their emission cycles [150]. Stewart reconciled his desire for a liquid or solid photosphere within these types of stars by stating: "*the approach of a planet to the Sun is favourable to luminosity*" [150, p. 454]. His desire for condensed matter was so powerful that Stewart advocated the scientific error that Venus itself can modify the appearance of sunspots [150, p. 454]. Regrettably, Stewart would eventually discover Loewy's misconduct while producing mathematical reductions relative to the work at Kew [151, p. 361]. This would place a considerable tarnish on the Kew group, and Stewart would never again speak on planetary effects relative to sunspots.

Earlier, in *Origin of Light* [150, p. 450–451] Stewart had viewed sunspots as cavities on the Sun, produced by an opening in the photospheric matter revealing the dark nucleus of the interior. In 1864, just prior to the paper with de la Rue and Loewy, Stewart stated that the Sun possessed with a solid body [150, p. 451]. The concept was similar to Wilson [84].

Despite Loewy's misconduct [151], Stewart could not long maintain a fully gaseous photosphere, given his extensive knowledge of thermal emission in gases [149]. Clearly, he had not embraced de la Rue's model [133–135] and the claim by Lockyer, discussed in Section 4.7, that the photosphere could be completely gaseous and devoid of any condensed matter [37]. On the same note, Stewart's entire discussion on thermal radiation, in his classic physics text, is well worth reading [148, p. 270–297]. It revealed his profound knowledge of such processes and also his understanding that gases cannot produce the continuous spectrum required.

Stewart maintained support for what is essentially Kirchhoff's liquid photospheric model. He did so despite his previous adversity with the German [61, 139]. In this regard, he was being guided by the same scientific reasoning as his former detractor [43]. The Scottish scientist also held profound values [140, 141, 150]. As such, it is comforting to notice how, in some sense, the two men were now reconciled. Stewart's continued support for Kirchhoff's condensed photosphere, was astounding as it *de facto* dismissed any previous arguments relative to Andrew's critical temperature [28] and line broadening [37]. For Stewart, the primary determinant of the phase of the photosphere was its thermal emission. The same held true for Kirchhoff. Yet, Stewart's insistence was important because it continued well after critical temperatures and line broadening had entered the halls of astronomy. Those who maintained that the photosphere was gaseous, therefore, continued alone on their journey. They marched on without the support of the two great experts in thermal radiation: Gustav Kirchhoff and Balfour Stewart.

As for Spencer, if there was any merit in his work, other than his obvious and justified claim of priority, it was that he foresaw internal convection currents, variable solar density, and the tremendous problem of internal stellar opacity. The last of these, contained in the "famous objection", remains a key problem with the idea of a gaseous Sun, despite all attempts to rectify the situation [69, 70]. But what is most fascinating about this philosopher, remains his amateur status in astronomy. Karl Hufbauer has commented on the contributions of amateurs to astrophysics [152]. Bartholomew argues as though there was little room for Spencer and his theoretical ideas in solar science [106]. In this regard, he stands in profound opposition to George Hale, one of the greatest solar observers and the founder of the *Astrophysical Journal*. In 1913, Hale defended the special place of amateurs in astronomy when he drafted the moving obituary of Sir William Huggins: "*If it be true that modern observatories, with their expensive equipment, tend to discourage the serious amateur, then it may be doubted whether the best use is being made of the funds they represent. For the history of sci-*

ence teaches that original ideas and new methods, as well as great discoveries resulting from the patient accumulation of observations, frequently come from the amateur. To hinder his work in any serious way might conceivably do a greater injury than a large observatory could make good… Every investigator may find useful and inspiring suggestions in the life and example of Sir William Huggins. Their surest message and strongest appeal will be to the amateur with limited instrumental means, and to the man, however situated, who would break new ground" [153].

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Dedication

This work is dedicated to my youngest son, Luc.

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On the Theory of Sunspots Proposed by Signor Kirchoff

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Eileen Reeves (Department of Comparative Literature, Princeton University, Princeton, New Jersey, 08544) and Mary Posani (Department of French and Italian, The Ohio State University, Columbus, Ohio, 43221) provide a translation of Father Pietro Angelo Secchi's classic work "*Secchi A. Sulla Teoria Delle Macchie Solari: Proposta dal sig. Kirchoff*" as it appeared in *Bullettino Meteorologico dell' Osservatorio del Collegio Romano*, 31 January 1864, v.3(4), 1–4. This was the first treatise to propose a particulate photosphere floating on the gaseous body of the Sun. The idea would dominate astrophysical thought for the next 50 years. Secchi appears to have drafted the article, as a response to Gustav Kirchhoff's proposal, echoing early Galilean ideas, that sunspots represented clouds which floated above the photosphere. Other than presenting a new solar model, noteworthy aspects of this work include Secchi's appropriate insistence that materials do not emit the same light at the same temperature and his gentle rebuke of Kirchhoff relative to commenting on questions of astronomy.

We gestured in passing in the second number of volume II [of the *Bullettino Meteorologico dell'Osservatorio del Collegio Romano*] to the theory offered by Signor Kirchoff, as a substitution for the current view, about sunspots. This theory has been something of a sensation, since it is the view of a scientist who has rightly gained immense popularity and esteem for his magnificent discoveries concerning the solar spectrum. For this reason, some consideration of his theory is in order, and we will avail ourselves of the various studies that have recently appeared.

Signor Kirchoff rejects both the theory of Herschel and that of Wilson. We will first permit ourselves the observation that it is one thing to refute Herschel's theory, and quite another to refute Wilson's, and that when the first is laid to rest, the second one hardly collapses. Herschel maintained that the solar nucleus was solid, dark, and covered by two layers of luminous clouds, one a certain distance above the other, separated from each other by a non-luminous layer, and he attributed the sunspots to ruptures in these layers. The nuclei formed the body of the sun, which was relatively darker, and visible through the openings in both of these atmospheres; the penumbras were caused, according to Herschel, by the larger rupture in the second luminous layer. Signor Kirchoff does not like the idea of these two atmospheres, and in truth, we have never accepted them either, because they were not necessary, and they were always obliged to rupture together. As a result of our numerous studies, carried out with powerful instruments and with close attention, we concluded that the penumbra was for the most part formed by filamentous currents of the single photosphere that enveloped the sun, or of the same material, rendered so thin that it was transparent. We called attention to the presence of hazes and cirri, lighter than the nuclei, but darker than the penumbras, that were sometimes found within the sunspots; in this we confirmed the discovery of Signor Dawes, who has justifiably complained that until now, no one who studies this phenomena has paid attention to this matter.

Among the issues that have most recently engaged the attention of solar observers is the structure that Signor Nasmyth has called the "willow-leaf" shape. That is, when one observes the sun using reflectors of great size and oculars without darkened lenses,* but in which the light has been weakened, in order to render it tolerable to the eye, by the reflection of a strip of glass, the structure of the sun looks as if it is formed of many elliptical and luminous pieces, elongated in the shape of leaves, and piled one upon the other. They appear more isolated and detached from each other around the penumbras, where they resemble numerous leaves crossing each other, and they are extended in more isolated fashion within the very core of the nucleus.

We have not yet had the opportunity to observe this [willow-leaf] pattern, but we see that even Signor Dawes is in the same circumstances: he finds that the solar structure described by Sir John Herschel, that is, composed of a sort of luminous flakes, is what most closely resembles the appearances observed over the course of many years of research, and in regard to the penumbras, he agrees that there are bright parts, like currents that make their way into the nuclei crossing through the penumbra and retaining all the splendor of the photosphere, and not of the penumbra. This squares with what we ourselves have always observed, and we likewise have always insisted on the three types of substances that are to be seen in each spot: the true nucleus, the penumbra, and the semi-luminous cirri. In order to explain these phenomena, there is no need to rely on two strata of luminous clouds. What suffices, instead, is a simple incandescent photosphere, mixed with less luminous vapors — as one sees in eclipses

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*[*] *Offuscanti* refer to the dark colored lenses of the type Christoph Scheiner and others put on telescopes if they were observing rather than projecting sunspots.*

— in which the ruptures develop, for reasons difficult to ascertain but easy to conjecture, and through which tears one could see the central and less bright part of the star.

But it is precisely this central and darker part that appears a great absurdity to Signor Kirchoff. He asks how it can be maintained that upon contact with such an incandescent body, and under radiation as strong as that of the photosphere, the nucleus itself has not also reached incandescence and fusion. That is [in his view] an absurdity contrary to all the laws of physics. With all the respect that is due to such a distinguished scientist, we believe that this is an exaggeration. First of all, no one has ever said that the nucleus was cold, and if it is dark, it is only in relative terms; Galileo himself said as much in his own epoch, and photography proves the chemical intensity of the nuclei [of sunspots] is so active that in order to obtain an image, one must act instantly, for otherwise the nuclei also are indistinguishable from the photosphere. The difference, therefore, has little to do with their luminosity, and if we were to see one of these nuclei in isolation, perhaps we would hardly be able to distinguish it from an adjacent portion of the sun. Kirchoff relies greatly on the principle that all substances become luminous at the same temperature in order to prove that the core of the sun must be as bright as the photosphere. Here it seems to us that two quite different matters have been conflated: that is, the point at which bodies begin to excite luminous waves capable of being perceptible to the eye, and the fact that all [substances] at the same temperature should be *equally luminous*. We can accept the first of these propositions, and wholly reject the second. In furnaces we see gases of entirely different luminosity from that of solids, and the strongest [hottest] flame that is known — that is, that of the oxyhydrogen blowpipe — is it not one of the least luminous? Thus the conclusion that the parts that form the solar nucleus should be as luminous as the photosphere can hardly be maintained. Nor does it follow that what we call “nucleus” should be either *solid*, or notably less elevated in temperature, but only in a less luminous state; it could even be liquid or gaseous, and only in this state will those lively specific actions that take place in the photosphere fail to occur. The analogy with all planets, as Soret has rightly observed, tells us that the heavier parts should accumulate on the lower stratum, and the lighter ones on the surface, and between these are the gases and the more tenuous materials from whose modifications sunlight is produced. Thus there no longer remains the much-sung absurdity of admitting that beneath the extraordinarily incandescent layer of the photosphere there could be another stratum, perhaps equally warm, but less luminous than it, and that makes itself visible to us when the more incandescent layer of the photosphere itself ruptures.

Moreover, if we reflect carefully, it is not possible to concede an absolute identity in temperature in the various parts of the sun. Indeed, the continuous labor that takes place in that body and the continuous emission of heat suppose that one part must remain in an ongoing state of chemical alteration, and another must be on the verge of entering it; the former might be the photospheric part, and the latter the central and less luminous region, precisely as we observe in ordinary fires. And we would not like to omit the fact that if we were to concede the argument of someone in favor of a sun where all parts are of an equal temperature, that the same could be concluded, following the same logic, about our own furnaces. We are not saying this as if the sun were actually a furnace in which wood were burned; we are saying, rather, that the work itself that takes place to conserve solar activity supposes the existence of some parts that are more intense, and others that are less so. Were this not the case, we would risk regarding the sun as a merely incandescent body, which Thomson has demonstrated could not remain luminous for even a few thousand years.

Treating Wilson’s theory as absurd shows that this notion has been confused with that of Herschel, when in fact there is some difference between the two. Wilson said only that the sunspots were cavities, and subsequent observations have verified this *fact*. But no one ever said that these cavities had within them a void, in the rigorous sense of that word; rather, the cirri that can be observed across [the cavities] show that they are full of a less incandescent gas, but that sometimes can be very clearly seen turning in vortices and currents. Now if this is the case, what are these cavities if not simply spaces full of less luminous, and thus less incandescent, material? Signor Kirchoff prefers to imagine them as clouds or rather cooler masses. There is not, in fact, much to distinguish the two hypotheses, finally, provided that the terms are well defined. The difference is further diminished if we see the origin of such clouds that is attributed to vortices and cataclysms, which is the cause that we, too, have often attributed to the origin of the sunspots.

The only point of controversy that remains is to decide if that black [part] that is called the “nucleus” is a part of that general ground that remains beneath the photosphere, or if it was produced by the opacity of a cloud or a cooler mass which prevented the rays from the more luminous part beneath from reaching us.

This issue can only be resolved after scrutiny of the shapes and the phases of the sunspots themselves, and not in *a priori* fashion. Now the study of their shapes does not agree at all with that of clouds as far as we can judge from what happens in our atmosphere and what can reasonably be imagined to take place in an incandescent atmosphere such as that of the sun.

In fact, sunspots present themselves to us from the outset like black pores, in which it would not be difficult to recognize the idea of clouds, but soon enough all analogy vanishes. Because if the pore expands until it has the appearance of a spot, it can be observed that its edges are ragged, and the penumbra is formed *entirely of very fine rays converging towards the middle of the shape*. The nucleus does not always present the outlines of the penumbra in rigorous fashion, as has been said several times, but rather, a *protruding* angle of the luminous material against the nucleus corresponds to an angle *sloping* into the penumbra, just as would a cascade of material that fell from the walls into the nucleus, which would leave a scarp (*talus*) whose slope would increase as greater amounts of material flowed. These are the phases of all sunspots as long as they are in the first stage, which seems to be that of formation and complete development, after which the phase of dissolution follows.

Thus it is apparent that this first phase cannot show us anything that is similar to what should happen when a cloud forms. The cloud should appear like a less luminous mass, and should be either decisively separated from other warmer ones as are our cumulus clouds, or shaded on the edges like our stratus clouds; that radiating shape and the appearance of currents running into a cavity and forming a distinct scarp will not ever be observed, in any guise, at least in what we can perceive and reasonably conjecture about our clouds. Whatever the theory of sunspots might be, their appearance must first of all be explained, and this appearance has yet to be explained by any theory that compares them to clouds.

When the sunspot has reached its full development, it shows vast black surfaces in which brilliant threads erupt like radiant torrents all around the photosphere, twisting in long contorted lines within the nuclei and breaking, as noted earlier. Now if we were to judge what is happening there on the basis of what happens in our atmosphere, these eruptions of warm masses within cold ones, occurring in such fashion that they remain distinct and constantly separated, cannot be observed by us at all, nor does it appear that they can be formed, because the cloudy opaque mass would either block them from our view, or the mass itself would diminish the light, thus cooling down the torrent that penetrates within [the nuclei] with that linear movement. Now as we have already observed several times, and as Signor Dawes has recently repeated as well (in the latest number of the *Monthly Notices*) the filaments of the photosphere that penetrate into the nuclei maintain an extremely brilliant light, as bright as the photosphere itself. Such a structure for the sunspots hardly confirms the idea of clouds.

When the sunspot is in the last phase of dissolution, the penumbra is less regularly radiated, and it seems formed of the thinnest and most tenuous part of the photosphere itself. In this phase it can be said that it has some analogy with clouds, but a theory, of course, must give an account of all the phases. There is, moreover, a circumstance of which the analogy with clouds explains nothing, and that is the presence of faculae that surround the sunspots.

These faculae are nothing other than the crests of the tempestuous waves excited by the photosphere, waves whose peaks emerge from the denser stratum of the solar atmosphere, as I have shown at length in other publications. They seem in fact formed by the photospheric matter that has been hurled about by the internal force that creates the sunspot. If the sunspot were nothing but a cloudy formation, there would be no explanation for why its contours should be agitated and violently thrown into disarray. Everything indicates that the sunspots are centers in which the temperature is less, and I have demonstrated as much with the thermoscope. But it is also clear that the source of these lacunae is rather an eruption coming from the inside of the nucleus, rather than a simple drop in temperature produced in the photosphere by factors analogous to those in terrestrial meteorology, which would be difficult to imagine in the sun, whereas internal eruptions cannot be avoided in a body placed in such conditions.

But there is something more: Herschel, in order to explain the penumbras proposed two layers to the photosphere, just as Signor Kirchoff proposed two layers of clouds which were always obliged to appear together, the one above the other. These two strata are surely a pure expedient to explain the penumbras, of whose composition we have already spoken, and which can be explained merely by proposing a simple photosphere with those features that are inseparable from fires of this sort. The hypothesis of the clouds has been frequently been raised, but always by those who either have not carried out much solar study, or who have undertaken it with imperfect and mediocre instruments. Thus this hypothesis has always been rejected by those who had at their disposal better means of observation. There is no need of the goal of proposing a less luminous nucleus, nor of that effort (as perhaps has been excessively emphasized) to revive the old fantasies of the habitability of the sun, because if the Creator had wanted to make this star habitable there would have been no need to place men of flesh and blood like us there, as they would be incinerated within a few seconds; nor is there any need to imagine, for that reason, that the black layer is like a tent to shelter such inhabitants from excessive rays. These matters might be useful to amuse the readers of a treatise of Fontenelle or of those who follow in his tracks. We are saying only that without contradicting the laws of physics, first, that the photospheric layer might possess a brilliance greater than that of the internal nucleus; second, that what we call "nucleus" absolutely does not need to be imagined either solid or liquid, but might even be gaseous alone, but more dense; third, that in spite of the proximity of the photospheric layer, it might have not only a different light, but also a different temperature; and fourth, that the appearances of the different shapes of the sunspots absolutely rule out cloud-like structures, and we see nothing in the sunspots that has sufficient analogies with the way in which our clouds are formed, or the changes through which they go.

We wanted, therefore, to say these things less to object to such a distinguished physicist, than to prevent science from taking a retrograde course, especially since history shows that persons of great authority in one branch [of knowledge] often drag along, under the weight of their opinion, those who are less experienced, even in matters where their studies are not sufficiently deep and where they should not have such influence. We hardly pretend to have given a true theory of the sunspots, but we believe merely, as has been demonstrated, that the notion that they are clouds is surely one of the most infelicitous of hypotheses that can be imagined.

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On the Structure of the Solar Photosphere

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Mary Posani (Department of French and Italian, The Ohio State University, Columbus, Ohio, 43221) and Eileen Reeves (Department of Comparative Literature, Princeton University, Princeton, New Jersey, 08544) provide a translation of Father Pietro Angelo Secchi's classic work "*Sulla Struttura della Fotosfera Solare*" as it appeared in *Bullettino Meteorologico dell' Osservatorio del Collegio Romano*, 30 November 1864, v.3(11), 1–3. Secchi's paper was immediately translated into French by l'Abbé François Moigno appearing on December 22nd, 1864 (*Sur la structure de la photosphère du soleil. Les Mondes*, 1864, v.6, 703–707). Moigno's translation prompted significant interest in the nature of the Sun throughout Europe, with rapid claims of simultaneous discovery by Harvé Faye (Faye H. *Sur la constitution physique du soleil — premiere parti*. Les Mondes, v.7, 293–306) and others. In this article, Secchi reiterated that the photosphere was composed of solid corpuscles floating on the transparent atmosphere of the Sun. Secchi concluded that the body of the Sun was gaseous based on his visualization of solar granules or "*willow leaves*" described by Nasmyth (Nasmyth J. *On the Structure of the Luminous Envelope of the Sun — In a letter to Joseph Sidebotham. Memoirs of the Literary and Philosophical Society of Manchester*, 1862, 3rd Series, v.I, 407–411). Secchi also referred to Magnus' work on solid particles in the gaseous flame (Magnus G. *Notiz über die Beschaffenheit der Sonne. Poggendorff's Annalen der Physik und Chemie*, 1864, v.121, 510–512; also in French *Notice sur la constitution du soleil. Archives des science physique et naturelles (Genève)*, 1864, v.20, 171–175). The works by Sechhi, Nasmyth, Magnus, and Faye would dominate astrophysical thought for the next 50 years.

In the first number of the *Bulletin* of this year we mentioned in passing the discovery by Signor Nashmyth concerning the structure on the sun which he named *willow leaves*, and which was subsequently confirmed by other astronomers, who found it preferable to call them *grains of rice*, because of the greater irregularity in the forms of the oval parts that they observed.

We said then that we did not yet have the means to examine this structure, because we lacked an ocular which would enable us to examine the solar image without a black lens, or darkener, and with a full aperture of the objective lens, an apparently essential condition for the accurate observation of these details. Recently, however, having received such an ocular through the kindness of Warren de la Rue, we made, as soon as the atmospheric circumstances permitted it, a series of observations, which we now report, reserving for another occasion a more extensive exposition.

The eyepiece of which we speak was formed with a plate of dark red glass inclined at 45° that reflects a small portion of the luminous rays, while it transmits a large portion of the others, and especially those of caloric value; the axis of the eyepiece by consequence remains at a right angle with the axis of the telescope. In the more northern climates, and especially in England, one can with this simple means of weakening of the rays look at the sun without danger to one's eyesight; but for us it is impossible, and we must add a slight darkener. The method of observing the reflection was proposed also by Signor Porro and P. Cavalleri: they had used, instead, two glasses under the angle of polarization, where, because the reflecting planes, were at right angles, the light becomes tolerable to the eye without any other adjustment, and remains white.

With one or the other method, one can visualize the sun much better than with colored glasses; the light remains white, and thus we can distinguish many details that were lost in the older method. However the polarizing system, introducing a double reflection, requires a great perfection in the optical reflectors, and thus, it is difficult to apply it to large instruments, in which the reflective surfaces should be rather broad. Instead of the reflective colored glass, one can substitute a prism by reflecting rays on the hypotenuse externally (and not by total reflection). However one cannot use a simple strip with parallel faces, because the second surface would give an image that could disturb that of the first.

Applying therefore this new eyepiece to Merz's great refractor, maintaining an aperture of its nine inches, we could immediately recognize a structure that truly differed greatly from what is commonly observed. The bottom of the solar disc appeared to be formed of a fine black mesh whose links were very thin and full of bright points. It was not so much the shape of the grid that surprised us — for we had seen it also at other times with older methods — as its blackness, which was truly extraordinary. It was such that we suspected some illusion, but in concentrating on certain darker points and finding them of unchanging and precise forms, we no longer remained in doubt about the reality of the aspect. Of this grid-like structure we can give an approximate idea in saying that the sun looked like a ordinary piece of rough paper seen through a strong microscope; on this paper the prominences are numerous and irregular, and where the light falls rather obliquely, the bottom of the grooves are almost black compared to the more elevated parts, which appear extremely white.

The grid-like solar structure seemed to us to offer nothing regular in those parts of the disc that are continuous, and thus the term *granular* appears very appropriate. Nevertheless, in the vicinity of the sunspots, that of *willow leaf* remains justified, because we actually see a multitude of small strips which terminate in rounded tips, and which encircles the edge of the penumbra and of the nucleus, resembling so many elongated leaves arranged all around. The granular structure is more visible near the spots, but it is not recognizable in the *faculae*; these present themselves like luminous clusters without distinguishable separation, emitting continual light without the interruption of dots or of that black mesh.

In the end, we have found the granular structure more notable and easy to distinguish in the middle of the disc than near the limb, and in the zones near the sun's equator, more than in the polar zones. The first [of these features] is without doubt an effect of the sun's refraction: in fact, the transparent atmosphere which encircles the sun must, because of its thickness and greater agitation, produce a greater confusion near the limb. We seemed to have recognized a trace of the effect of the refraction of this atmosphere in some of the systematic irregularities of the place of the spots near the edges, found by Signor Carrington in his admirable recent publication about sunspots. The polar regions, as is known, have a lower temperature and less agitation, and the spots do not appear there. This grid suggests that the spots are but an exaggeration of the minute holes which riddle the solar surface.

These are, in summary, the observations, which certainly raise a great number of questions. First of all, are these new findings? We believe that, in the end, these are the same granulations that have long since been pointed out by observers, under the name of "lucules" and "pores", and that with the new method they can better be distinguished. Because, since we can in this manner utilize a large aperture, the phenomenon of dilatation of luminous points or circles of diffraction that the objective lens forms are considerably diminished, and, as a consequence, we can better recognize the details, because each luminous center remains completely separated.

In the second place the rounded tips which surround the nuclei and the penumbrae are not new — at least not us — but rather are those that we have always indicated as evidence of the luminous currents that run to fill the emptiness of the spots. They are those types of currents that accumulate around the *nucleus*, and render the light appearing there greater in intensity than in the remoter regions of the penumbrae, just as the spokes of a wheel are more crowded together near the axle than towards the circumference. However, we must not omit the fact that with this means of observation, the appearance of a continuous current seems in many cases rather interrupted, and takes on instead the aspect of a multitude of torn fragments, or as Dawes says, of truncated straws that run towards the nucleus. But in any case, the more we study with attention these phenomena, the more it is *unacceptable* to us the idea that the spots are clouds. We do not hesitate to say there is still much that is mysterious in this structure, but certainly it has nothing to do with clouds, unless we wanted to say that the clouds are rather what form the luminous element, and that this incandescent material rushes in *cumulus* and in *cirrus* in the void of the spots, as we see sometimes in our atmosphere the *cumulus* and the *cirrus* run and fill in voids [in the sky].

Indeed this appearance suggests to us what is perhaps a bold hypothesis. As in our atmosphere, when it is cooled to a certain point, there exists a fine substance capable of transforming itself in fine powder and of forming clouds in suspension, (water transforming into so-called "vesicular" vapor or into small solid icicles), so in the enflamed solar atmosphere there might be an abundance of matter capable of being transformed to a similar state at the highest temperatures. These corpuscles, in immense supply, would form an almost continuous layer of real clouds, suspended in the transparent atmosphere which envelopes the sun, and being comparable to solid bodies suspended in a gas, they might have a greater radiant force of calorific and luminous rays than the gas in which they are suspended. We may thus explain why the spots (that are places where these clouds are torn) show *less* light and less heat, even if the temperature is the *same*. The excellent results obtained by Magnus, who has proved that a solid immersed in an incandescent gas becomes *more radiant* in heat and light than the same gas, seem to lend support to this hypothesis, which reconciles the rest of the known solar phenomena.

In the third place, one will ask oneself if such appearances are constant. It seems that we should say yes, since it has been discussed for two years, but to observe them takes no small practice and good instruments. Ours was extremely well made, with a red lens from England, but it showed little resistance to our [Italian] sun, and exploded into many pieces. Now we have substituted a prism, but it emits too much reflected light, and its surface is perhaps not perfectly polished. Nonetheless, we continue to see with clarity a grid and the other phenomena mentioned above. But the principal obstacle is the agitation of the air, which by mixing all these small shapes, makes a general confusion and flattens everything, for which reason they are only seen intermittently on those days that are anything short of perfectly calm. However, by moving the telescope slowly we can see the granulations much more easily than when we hold it fixed, and once they are recognized, it is easy to follow them and to study their forms.

May these indications suffice for now; the numerous other questions raised by this new method of observation and by this structure will be resolved with time. For now it is certain that this mode of observation can be said to have truly been a new conquest of practical astronomy.

Acknowledgement

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A Note on the Constitution of the Sun

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Patrice Robitaille (TAV College, Montreal, Canada) provides a translation of Heinrich Gustav Magnus' classic work *Notiz über die Beschaffenheit der Sonne*, as it appeared in March 1864 within *Poggendorff's Annalen der Physik und Chemie*,1864, v.131, 510–512. The article had previously been translated into French: *Notice sur la constitution du soleil. Archives des science physique et naturelles (Genève)*, 1864, v.20, 171–175. This work formed the basis of the present translation. Heinrich Gustav Magnus (May 2, 1802 – April 4, 1870) was a professor at the University of Berlin and had studied in Paris under Joseph Louis Gay-Lussac. He would count amongst his students Wilhelm Beetz, Hermann Helmholtz, Gustav Wiedemann, John Tyndall, Rudolph Weber, and Adolph Wüllner (Heinrich Gustav Magnus, *Platinum Metals Review*, 1976, v.20(1), 21–24). In his *Notiz*, Magnus demonstrated that the addition of sodium hydroxide to the gaseous flame resulted in a tremendous increase in luminosity. Magnus' work would inspire Father Secchi to propose that the Sun was a gaseous globe whose photosphere contained condensed particulate matter (Secchi A. *Sulla Struttura della Fotosfera Solare. Bulletino Meteorologico dell' Osservatorio del Collegio Romano*, 1864, v.3(11), 1–3; English translation in *Progr. Phys.*, 2011, v.3, 30–32). Magnus' report on the constitution of the Sun would continue to impact solar physics for two generations.

Already in 1795, W. Herschel[*] advanced the idea that the Sun is formed of an obscure nucleus surrounded by an atmosphere or photosphere from which light and heat are emitted. Between this photosphere and the nucleus, he also admits the presence of a reflective atmosphere whose reflection prevents the light of the photosphere from reaching the nucleus. Arago[†] in exposing this hypothesis which he gives as generally accepted[‡], remarks that the photosphere determines the outer edge of the Sun, but that the photosphere is itself surrounded by a diaphanous atmosphere; he comes to this conclusion through the observation of the protuberances [flares and prominences] during total eclipses of the Sun. Herschel[§] says that the photosphere is neither liquid nor gaseous, but that it is made up of luminous clouds. According to our current knowledge of the radiation of light and heat, it is difficult to admit that the photosphere, from which solar heat emanates, does not heat to the point of incandescence the nucleus that it surrounds. The intermediate reflecting atmosphere, whose existence was assumed, could very well stop the passage of light but not the progressive heating of the nucleus. It is therefore with reason that Mr. Kirchhoff[¶] says that this hypothesis which was devised to explain sunspots, is in such total contradiction with our knowledge of physics, that we should reject it even if we cannot come to make comprehensible, in another way, the phenomenon of sunspots.

Mr. Kirchhoff was guided by his research on the solar spectrum to admit that the Sun consists of a solid or liquid nucleus, brought to the highest incandescence and surrounded by a diaphanous atmosphere with a slightly inferior temperature.

I do not know that we have as yet deduced from the nature of the heat that emanates from the Sun, a conclusion on its constitution; we could but mention the observations of Reverend Father Sechi[‖] relating that the poles emit less heat then the Sun's equator. Some of the experiments that I have conducted on calorific radiation, allow, I think, for new views on the constitution of this celestial body.

If we observe the heat that emanates from a non-luminous gas flame, and if we introduce a bit of sodium hydroxide which, as we know, renders it extremely luminous, we see at the same time, that the calorific radiation increases. The experiment was carried out in such a way that we were always comparing a predetermined place of the sodium hydroxide flame, with the same place of the non-luminous flame, and this in such a way that the sodium hydroxide introduced into the flame, could not radiate over the thermo-electro battery used for observation. Evidently, in this case, part of the heat of the flame was used to bring to incandescence or to vaporize the sodium hydroxide and the platinum blade on which it was found in such a way that, in the end, the flame had a lower temperature than before when it was not luminous, and yet it emitted about a third more of the heat that it had previously.

It can be that the sodium hydroxide was contained within the flame in a state of vapour or that particles removed from that body that augmented the illuminating power. Whatever the case may be, I choose, to shorten the discourse, the des-

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[*] *Philosophical Transactions* for 1795, page 42.

[†] *Astronomie populaire*, Vol. II, page 94.

[‡] *Ibid.*, page 143.

[§] *Philosophical Transactions* for 1795, page 71.

[¶] *Denkschriften der Berliner Acad. Der Wiss.*, 1861, page 85.

[‖] *Comptes rendus de l'Acad. Des sciences*, Vol. XXXV, page 606 and Vol. XXXVI, page 659.

ignation of sodium hydroxide vapour.

By introducing in the place of this vapour a platinum disk in the same area of the flame that was being studied, the heat that the flame emitted became even more considerable than previously recorded. The platinum blade evidently removed from the flame even more heat than the sodium hydroxide, but it, however, radiated even more. With the blade that I was using and whose diameter was 55 mm, the radiation became nearly twice as strong then when the flame did not throw off any light. We did not observe any fundamental difference by making the blade thicker or thinner, so long as the diameter remained the same.

But if, instead of making the blade thicker, we covered it with sodium carbonate, then the radiation increased again considerably; it became fifty percent stronger than with the platinum blade without any sodium hydroxide.

The radiation would rise even more, when, apart from the platinum blade being covered with sodium hydroxide, there was also sodium hydroxide vapour in the flame, this being obtained by introducing in the lower part of the flame some sodium hydroxide on a platinum blade, in the same way as was done previously, that is to say without having the sodium hydroxide radiating over the battery.

In this case, the flame being completely filled with sodium hydroxide vapour coupled with the platinum blade covered with sodium hydroxide, the flame emitted close to three times more heat than the flame that was not luminous. Lithium hydroxide and strontium hydroxide behaved like sodium hydroxide.

These experiments demonstrate that gaseous bodies emit far less heat than solid or liquid bodies; and that, by consequence; one cannot assume that the seat of solar heat resides in a photosphere made up of gas or vapours. They also demonstrate that, and this is especially striking, that incandescent sodium hydroxide has a much greater radiative power for heat than platinum at the same temperature.

Also, they demonstrate that sodium hydroxide vapour or sodium hydroxide particles absorb only a small part of the heat emitted by incandescent solid or liquid bodies. In fact, the radiation of the solid body in the flame filled with sodium hydroxide vapour was, it is true, always smaller than the sum of the radiations of the solid body alone and of the vapour introduced alone in the non luminous flame, but the difference was small.

This manner in which incandescent liquid or vaporous sodium hydroxide behaves confirms in a striking way the views of Mr. Kirchhoff on the nature of the Sun.

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On the Physical Constitution of the Sun — Part I

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Patrice Robitaille (TAV College, Montreal, Canada) provides a translation of Hervé August Etienne Albans Faye's classic report *Sur la constitution physique du soleil*, as it appeared in February 1865 within *Les Mondes*,1865, v.7, 293–306. Hervé Faye (October 1, 1814 – July 4, 1902) led a distinguished life, both in science and public service. He was widely regarded as one of the premier astronomers of his day. He had studied under the great François Arago. In 1843, he became a *Chevalier de la Legion d'Honneur* and, in 1877, served as the French Minister of Education (Catholic Encyclopedia, 1913). Faye's report *On the Physical Constitution of the Sun* was a crucial milestone in the history of astronomy. It was through this paper, that the Sun became viewed as devoid of a distinct surface. The work was also interesting as it presented Faye's early conception of the gaseous Sun. In addition, through its submission, Faye had sought the approbation of Father Secchi relative to claims of simultaneous discovery (see P. M. Robitaille. *A Thermodynamic History of the Solar Constitution — I: The Journey to a Gaseous Sun. Progr. Phys.*, 2011, v.3, 3–25). Faye's work would continue to impact solar physics until the 1920s.

Why do astronomers have so much trouble describing the physical constitution of the Sun? Why so many contradictory conjectures? One tells us that the Sun is an opaque globe, obscure, cold like ours, perhaps even inhabited, but surrounded by a radiant aureole, from which is emitted the heat and the light which, for thousands of centuries, has given life and movement on our little world of planets. Yet another affirms that it is a liquid globe, incandescent, surrounded by a vast atmosphere where float clouds of iron, sodium and magnesium vapor, etc.

It is in such a way that the sciences make their first appearance when they possess but a small number of facts and laws. The human spirit needs conjectures in order to take interest in the things that are beyond reach. But the question of the Sun cannot remain where it is after two and a half centuries of diligent observation. We have gathered, on this matter, the main elements of a rational solution; it is now time to address it.

What is the difference between a conjectural solution and a rational solution?

The first is quite simple; you have observed two or three facts: to explain them, imagine as many particular entities as there are facts, and try to coordinate them in a way to avoid that they contradict each other. Before the telescope, the only thing we knew about the Sun was its extremely powerful heat and its unwavering brightness; the conjecture consisted to say that this celestial body was formed of a subtle element, incorruptible, infinitely more noble than our terrestrial flames which smoke and die out miserably. Also, the discovery of sunspots would strongly appall the partisans who believed that the heavens were incorruptible; and when Father Scheiner, to whom we owe such remarkable work on these phenomena, went on to mention these to his superior, the latter replied to him: "I have read and reread Aristotle, but I haven't found anything there touching the things you tell me; go, my son, hold your spirit to rest; there are no spots on the Sun other than those that are created by the defects of your eyes or of your telescopes".

But the conjecture had to yield before the facts. These facts, are described here in all their simplicity: black spots are produced on that shining pool of fire; they are born, take about two weeks to cover the distance of the solar disk, and then pass over to the other side; we see them again at the end of another two weeks; sometimes they persist for months; normally, they disappear after a few weeks. These spots really look like holes; we can even distinguish, using powerful telescopes, a less brilliant part that typically resembles the embankments of these holes. The bottom seems completely black. Black holes in a pool of fire! It is apparent that the brilliant part is only a rather thin envelop of a very mobile luminous fluid, covering a black core, and here lies the second conjecture. We have lived a long time on that one and it has its merit. The preceding one itself, I mean the incorruptibility of celestial bodies, also had its own merit, since it represented a great fact, still true today, as in the time of the scholastics.

Lastly, in more recent times, a capital discovery revels the minds with much admiration: The rays of the solar spectrum are explained; we reproduce them in the laboratory by placing metallic vapors on the path of a beam of light that emanates from an incandescent solid or liquid.

Let us conjecturally transport this experience to the sky: the Sun will become an incandescent solid or liquid surrounded by a vast atmosphere of metallic vapors. But what about sunspots? How can black holes form themselves in a liquid or solid? Here, we must avoid an absurdity; the spots will be produced by something exterior, precisely by the clouds of that atmosphere, clouds formed of metallic vapors that begin to condense. Whatever can be said on the matter, this latest idea, which seems to violate all facts, except one, nevertheless answers to one of the most admirable discoveries of our time, that of spectral analysis, which permits the pronouncement, by the appearance of a light, on the chemical nature of the environment through which it has travelled.

During this time, the facts were multiplying, I am not saying at the time of Aristotle when we did not have telescopes, but since Fabricius, Galileo, and Father Scheiner. Today, the enumeration of observed facts offers a magnificent total. We must ask ourselves, I repeat, whether, in the presence of these facts, it is not time to renounce conjectures and to try a little simple reasoning. This second method is that which definitively constitutes science: it only comes after the first, but it must also have its turn.

Here, it is no longer a question of guessing, but of linking the phenomena through laws known in the physical world to some simple and very general fact that we would not be tempted to set aside. I do not know if I have succeeded, I am certain, at least, that the time has come and, since it is a question of pure logic, another, reasoning better, will succeed if I have failed.

My starting point will naturally be solar heat. Everything proves to us that this heat must be enormous; it must enormously surpass the highest temperatures that we can produce in our laboratories. However, the former suffices already to break down a large number of bodies. We must therefore consider chemical phenomena as being capable of occurring beginning at a certain temperature scarcely remote from those we can produce, but not above them. Above them, the elements mix, but do not combine. In the same manner, the phenomena of electricity, magnetism, life, occur at a certain temperature, but not above it. There is reason to believe that the Sun is at a temperature of universal chemical and physical dissociation, that its heat much surpasses all affinities, all molecular attractions, in such a way that its entire mass reduces itself to a gaseous mixture, to a true chaos of entirely separated atoms. That is my starting point, of which the complete justification, based on the dynamic theory of heat would require much too lengthy developments. I then place on one side the most characteristic known facts, on the other the consequences of my premises; if the starting point is accepted, if the facts can be successively identified with the consequences, we will have drafted a theory and no longer a conjecture.

This mass is undergoing cooling, since nothing comes from the outside to restitute the heat that it throws off daily into space, the stellar radiations being extremely weak; from there the successive phases which are convenient to analyze first.

In fact, the enormous heat that we have just mentioned is that of the entire mass; at the surface, there where cooling operates with the most energy, it can fall far below the internal heat, and make way for the initiation of chemical activity. Is this deduction true, can it be applied to the Sun? To find out, let us consult the facts. The heat emitted has been measured: it has been calculated that it does not exceed 30 or 40 times the heat contained in the furnace of a locomotive when it actively draws energy. On the other hand, the most intense heat furnaces produced by man do not emit a light incomparably weaker than solar light. We can therefore admit that, on the surface, chemical actions start to produce themselves, at least those that give birth to the most stable components. There are two ways, in fact, to have affinities react in a mixture of gas and vapors; by heating, if the mixture is cold; by cooling, if the mixture has gone beyond the temperature of dissociation.

Thus, in this environment, particulate clouds will be produced that will no longer be gaseous, but liquids or solids, like magnesia in a mixture of vaporous oxygen and magnesium and, in another sense, like the carbon in our lighting flames. Now these particles, becoming incandescent, will radiate enormously more than the gaseous environment itself, at the same temperature, because their emissive power is much superior to that of elementary gases or vapors. As a result, by the sole fact of superficial cooling, any gaseous mass primitively brought to a temperature of dissociation will surround itself at the surface with a continuous or discontinuous luminous cloud.

To these conclusions answers, item by item, as we shall see, the photosphere of the Sun.

There is, however, one difficulty. In a hot gaseous mass, isolated in space and which is cooling, there can and there must be established after a certain time, and following interior movements, a certain equilibrium that temporarily opposes the transport of some portion of the mass from one layer to another. Admitting, therefore, that chemical action occurs at a given instant in the exterior layers, following this cooling, how would it be maintained? How could the photosphere, which is produced momentarily, renew itself continuously and regularly? Here is the answer. The non gaseous particles that form the photosphere's luminous clouds are much heavier than the gaseous environment from which they are born; they will obey the attraction of the entire mass, and will fall vertically until they reach a layer that is hot enough to reproduce the dissociation of their elements. But then; in that layer, the gases and vapors due to this dissociation will break the equilibrium and will force a certain part of the mass of this layer to elevate itself to superior layers. From this, results a double incessant current that would produce itself only on long intervals and in a tumultuous manner, if the mass remained gaseous everywhere, if the chemical activities did not intervene to modify all at once the density of the superficial parts. This double current therefore incessantly brings to the surface part of the internal heat that is dispensed rapidly, thanks to chemical activity; while the incandescent particles, because of their excess density, fall once again within the deeper layers and lower, little by little, the temperature. There lies, to my liking, the rational explanation of that marvelous constancy of solar radiation, first phenomenon that hit the ancient [philosophers], whose long-lasting conjectures have never tried to take into account. How could the Sun, considering only historical times, support its enormous radiation with such a luminous envelop, thick of only a few leagues, being the seat of the most curious phenomena? The combustion of all the elements composing the Sun would not represent heat capable of supplying this radiation during half of that short period. Do you adopt the second conjecture, that of Mr. Kirchhoff? The thing would become even less possible still, because a liquid envelope would be quickly cooled; it would encrust itself at the surface, while the interior would maintain a high temperature that would have no other outlet but the weak conductibility of the outer crust. Conversely, the rational explanation of the photosphere gives for the energetic constancy of the radiation the only admissible reason, by showing that the entire mass participates in this heat expenditure and not only the superficial area. It must be remembered that the entire mass is enormous and that the originating temperature is equally enormous.

If I insist on this point, it is because here lays the heart of the problem. Everything else will easily follow if, on this point, one is willing to permit me to advance my cause. This old problem that the ancient school had resolved in its own way by proclaiming the incorruptibility of the heavens, was simply set aside by modern thinkers, until the creators of the dynamic theory of heat decided to revive the discussion. But their solution, so scholarly and so ingenious, was just one more conjecture: they believed they had to invent an artificial means to maintain this enormous caloric expenditure that equates to the incessant production of a 75,000 horse-power force for every square meter of solar area, while it suffices to represent a mode of cooling such that the internal mass is constantly called to supply to the superficial area the heat that it emits.

So then the exterior surface of the Sun, which from far appears so perfectly spherical, is no longer a layered surface in the mathematical sense of the word. The surfaces, rigorously made up of layers, correspond to a state of equilibrium that does not exist in the Sun, since the ascending and descending currents reign there perpetually from the interior to the superficial area; but since these currents only act in the vertical direction, the equilibrium is also not troubled in that sense, that is to say, perpendicularly to the leveled layers that would form if the currents came to cease. If, therefore, the mass was not animated by a movement of rotation, (for now we will make of it an abstraction), there would not be at its heart any lateral movement, no transfer of matter in the perpendicular direction of the rays. The exterior surface of the photosphere being the limit that will attain the ascending currents which carry the phenomenon of incandescence in the superior layers, a very-admissible symmetry suffices in a globe where the most complete homogeneity must have freely established itself, to give to this limit surface the shape of a sphere, but a sphere that is incredibly uneven.

This limit is in any case only apparent: the general milieu where the photosphere is incessantly forming surpasses without doubt, more or less, the highest crests or summits of the incandescent clouds, but we do not know the effective limit; the only thing that one is permitted to affirm, is that these invisible layers, to which the name atmosphere does not seem to me applicable, would not be able to attain a height of $3'$, the excess of the perihelion distance of the great comet of 1843 on the radius of the photosphere.

If you compare now these deductions to the best known facts of detail, you will find a remarkable agreement. The incessant agitation of the photosphere, the black points or rather the little interlaced black lines that cover the surface, the spots and the faculae are easily understood if we refer to the action of the vertical currents that we have just described. What shines in fact are the products of the chemical activity, that occurs in the photosphere on matter that is constantly renewed by the currents, and not the gaseous environment where these incandescent phenomena take place. To properly understand this difference, it would suffice to observe, through one of those obscuring glass plates that astronomers use to observe the Sun, the flame of pure hydrogen, or the one produced by a Bunsen burner, next to a flame produced by magnesium vapor. The first would be completely invisible, that is to say black; the other would be as white as snow. If, therefore, for one reason or another the incandescent clouds of the photosphere come together in a given place, there the visual ray will only meet but the general gaseous mass of the Sun endowed with a very weak emissive power, while a little further the photosphere will appear with its intense radiation and dazzling brightness. Father Secchi, recently came to a similar explanation of sunspots which makes me hope that the ideas I have just presented on the formation of the photosphere will meet his approbation. As for the faculae, there is nothing simpler assuredly that such level differences at the extreme limit of our ascending currents, and nothing so difficult to understand for those who admit the liquid photosphere. Persistent ridges of 100 or 200 leagues high on the extreme surface level of a liquid layer are not easy to justify.

But the high point of this theory, is the reconciliation of the two famous and contradictory experiments of Arago and Kirchhoff. Basing himself on the polariscopic analysis of the light of the Sun, Arago concluded that the photosphere had to be gaseous; basing himself on spectral analysis, Mr. Kirchhoff concluded that the photosphere is solid or liquid. The only way to have these opposed conclusions agree is to admit the photosphere I have proposed. Non gaseous but incandescent particles, floating like a cloud in the midst of a gaseous environment, would in fact emit natural light under all angles of incidence; they would also emit rays of all refrangibility with the exception of those that the gaseous environment interposed between the particles is capable of absorbing. The second point is the only one that needs a few developments:

the light so emitted is not purely superficial, it comes from a great depth; by consequence the largest part of the rays incurs on the part of the general environment, a very strong absorption. It would be different if the light was emitted only from the superficial area, then an exterior environment would be required, interposed between that superficial area and us, in order to produce the required absorption; as it is seen in the vast atmosphere that Mr. Kirchhoff places around the Sun; but then the spectrum of the outer edges of the Sun would be considerably different from the spectrum of the center, because of the thickness of the atmosphere that would be much greater on the edges than in the center. However, the experiment by Mr. Forbes and the more recent and even more decisive work of Mr. Janssen establish that there is no difference between the two spectra; so the absorption comes mainly from the cause that I have assigned, and far less of the layers exterior to the photosphere, these being in reality but the far restrained continuation, in my opinion, of the general gaseous mass. It suffices to admit that the effective depth is the same in all directions where emission operates, and that it is then the same in the center and on the edges of the visible disk, a result to which I concluded some years ago through many other considerations.

To this gaseous mass, let us restitute now the more or less slow rotational movement it must have acquired, at the same time as its heat, through the gathering of the matter that constitutes it; the ascending and descending currents will incur, because of this rotation, a certain deviation. Originating from a great depth, the ascending currents reach the surface with a linear speed which is reduced since the rays of their primitive parallels were smaller. The photosphere whose matter is constantly renewed by these currents, must therefore be behind on the general movement of rotation; on the other hand, the theorem of areas requires that the sum of the projections of the areas described at a given time by the vector rays of the molecules remain constant, no matter the interior movements. This means that if the exterior layer is lagging the general angular movement, there will be, through compensation, an advancement of this angular movement for a few interior layers, and this is immediately understood, because the ascending currents cannot exist without, at the same time, the existence of descending currents that carry back the superficial materials towards the interior with the excess linear speed due to their greater parallels. Falling towards the center, this matter will therefore transfer this excess of speed to the layer where it has just incurred the dissociation of its elements. From this, there will be two layers to distinguish: a superficial layer that lags behind, and an internal layer that runs ahead of the angular movement that the entire mass would take if vertical equilibrium came into being. But some zone, in a rotating fluid must tend to approach the axis if it is lagging behind, and distance itself from it if it is running ahead on the speed of the general movement; so that the exterior layer will have a tendency to flow little by little toward the poles, while the interior layer which is in advance, will express the opposite tendency and elevate itself toward the equator. From this results a significantly complex modification of vertical currents that we first considered in all their simplicity, and I imagine that things will occur as if the interior layers from which they emanate were a lot closer to the center toward the poles than at the equator itself. If this deduction were founded, and one cannot argue with the fact that the term *layer* has a variety of meanings, it would manifestly result that the superficial rotation should vary from the equator to the poles and slow down, more and more, without, however, that the exterior feature would substantially cease to differ from the primitive spherical form.

Thus, the photosphere would be constituted of successive zones, parallel at the equator, animated by a decreasing angular speed in a way that is more or less continuous from the equator to the poles, while the inverse would produce itself in a certain deeper layer. In this complex phenomenon, that would be impossible to subject to calculation, the movements would operate mainly in the direction of the parallels either to the opposite, either in the direction of the general rotation, without this bringing about strongly marked currents in the direction from the equator to the poles or inversely. This is, therefore, a considerable phenomenon, a very special mode of troubled rotation that the planets could not present an exact equivalent, since the conditions there are so different.

In the case of the planets, in fact, one must make a distinction that does not need to be made in the case of the Sun, between the solid body of the planet and its atmosphere: the solid body turns altogether; it would be the same for the atmosphere, if an exterior action, the solar heat, did not intervene at every instant. Equilibrium therefore cannot exist in that atmosphere, but the phenomena that are produced there being regulated mainly by a notable difference in temperature between the poles and the equator, the movements being hindered by the presence of an unchanging solid or liquid surface (the surface of the solid globe on which rests the atmosphere), it is principally produced a lateral call of the atmospheric masses in the direction of the meridians, from the poles toward the equator. A superior counter-current is established in the same time in the inverse direction, in the layers that are further from the ground. Nothing like this happens on the Sun because the presence of the photosphere does not interrupt the continuity of the [central] mass, because there is no resistant ground to deviate the currents, because there is no exterior cause to trouble the equilibrium of the layers in the lateral direction. In order to illustrate the difference, I would say that, in the photosphere, the rotation only generates currents that are approximately directed along the parallels in the inverse sense of the rotation, while that, on the planets, the currents in the inverse sense of the rotation result as a medial or indirect effect of the superficial transfer of air masses in the direction of the meridians.

In short, it results, because of the appearance and the up-holding of an atmosphere, in a gaseous mass animated by a rotational movement, that the surface must be delayed relative to the internal mass, in such a way that the superficial currents act only in the direction of the parallels save a slightly marked tendency toward the poles; and that this superficial delay must go increasing from the equator to the poles following a certain law that would be impossible to assign ahead of time, but of which we know this, that the direction of the rotational axis must not be substantially altered. Let us examine if the facts are in agreement with these consequences.

Here, it is good to restate things from a higher perspective. The astronomers naturally started by treating the Sun's rotation with the simplest hypothesis, that is to say, admitting that the entire mass turns as a single unit altogether, as if it consisted of the Earth or any other planet. In that case, the accidents of the surface would be animated with the same angular speed, no matter what was their position next to the pole or next to the equator, above the visible surface or below it. But this conjecture, the basis of all the work carried out in that sense from 1610 to 1840, was too far away from the truth for us to approach satisfactory results. If the astronomers generally agreed on the direction of the axis of rotation, they would reach the most discordant results concerning its duration. In the end, Delambre, discouraged by this failure, would console himself by saying that, after all, the subject had little importance, that it was good for training beginners. That was disregarding too hastily one of the most important phenomena of our solar world and one of the most verified laws in the history of the sciences, that is to say that all well-observed discordance carries with it the seed of a discovery. Finally, an astronomer was able to rid himself of these preconceived ideas in order to observe the phenomena for and in themselves. Mr. Laugier observed that every spot gave, so to speak, a specific value for the duration of the rotation: for 29 spots observed by him with all the refinements of precision, he observed that the completed rotations varied from 24 to 26 days, a difference far superior to the little uncertainty of the observations. This could mean two things: either the spots were animated by strong proper movements, or the successive zones of the photosphere did not possess the same rotational movement. Mr. Laugier left these things in that state, but he broke the ice, as we commonly say it, without mentioning the definitive elements that he had given to science for the direction of the solar axis. What needed to be done in order to pursue the work so nicely initiated? The spots had to be observed in a continuous manner, someone had to devote himself exclusively to this work for many long years, in order to discover the law of these specific variations; above all, a less dangerous method of observation for the eyes had to be devised by sacrificing partly the precision of the measurements.

That is what undertook Mr. Carrington, already known by astronomers through the great breath and extreme value of his work. Seven years and a half of continuous observation, 5 290 solar spot positions with the enormous quantity of drawings needed to conduct the discussion; there is the material that he accomplished. The definitive result can be formulated in the following manner: the determined rotation by the movement of sunspots is the same for all of the spots located at the same latitude, be it at the north, be it at the south of the equator, but it varies in a continuous fashion with latitude and becomes slower and slower towards the poles. Mr. Carrington tried to represent the complex phenomenon empirically with the following formulation: The duration of the rotation, obtained by dividing 216 000 by the movement of a spot expressed in minutes, this diurnal movement is equal to $865' - 265' \sin \frac{7}{4} l$, l designating the heliocentric latitude of the spot, and the quotient representing the average solar days. I do not know of any modern discovery that treats a matter more considerable than this one. We will not suppose, in fact, that the spots, simple clearing in the photosphere, could have such rapid proper movements (2 000 leagues per day at the 35th degree, for instance) and that they displace themselves this way within the environment where they are formed. A clearing, in a cloudy sky, can certainly displace itself and can displace itself at a great speed, but with the condition of being carried by the general movement of the ambient mass, which does not exclude specific modifications in the form and in the situation. We could not refuse ourselves to conclude from the nice work of Mr. Carrington that the photosphere moves with a varied angular movement whose slowness increases from the equator and up to the 15th degree and beyond and that this movement constitutes a mode of rotation quite different from that of the planets and their satellites.

Can this movement be assimilated to the trade winds and to the monsoons of our atmosphere? Observation answers negatively [to this question]. Trade winds originate from the transport of polar air masses toward the equator; the masses animated by a speed of rotation that is linearly less than the parallels met successively, appear to be blowing in the inverse direction of the terrestrial rotation, but here the essence of the phenomenon is not in the east-west sense of our trade winds, but the north-south direction (for our hemisphere); the first is but a consequence of the second, and the east-west movement would not exist if the movement from the north to the south disappeared or became too weak. However, on the Sun, we do not find any constant trace of this general movement from the poles to the equator, but rather an inverse tendency, starting from the 15th degree of latitude, from the equator to the poles, the identical tendency to the one that results from our above reasoning. Hence, the analogy that was naturally suspected at first does not exist, and we essentially remain before a new perturbation in a movement of rotation. It is up to the reader to decide if this great and beautiful phenomenon corresponds to the consequences that we have deduced from our theory.

One will surely note that these consequences end up being a little uncertain; this occurs because the facts themselves are not completely known. The formula provided by Mr. Carrington is purely empirical; the spots are so rare in the first degrees of the equatorial zone and from the 55th to the 50th degree of latitude, that the relative determinations in these zones are far from deserving the degree of confidence that can be given to the rotations concluded for the zones found between 5° and 35°. There is therefore a new work to undertake to complete the work of the English astronomer, but I do not think we can fully succeed without the help of photography whose introduction in the observatories is now a matter of factual use with our neighbors across the English Channel.

In short, conjectures no longer serve progress; they can only hinder it from now on. To the very simple idea associated with the cooling of a gaseous mass brought to a temperature such that its elements find themselves in a state of complete dissociation, except at the surface, where the chemical forces begin to exert themselves it is possible to logically link:

The constancy and the long duration of solar radiation;
The production and the maintenance of the photosphere;
The apparent contradictory experiments of Arago and Kirchhoff;
The explanation of sunspots and faculae;
And the mode of rotation particular to the Sun.

P.S. "I ask for permission to indicate here a coincidence or rather a remarkable agreement between the diverse conditions of organic life on the surface of the planets and our solar world. These conditions are of two kinds: 1) the mechanical stability of the system; and 2) the permanence of solar radiation. Either one or the other stability, even though they are of very different types, essentially rest on the enormity of the mass of the central celestial body".

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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

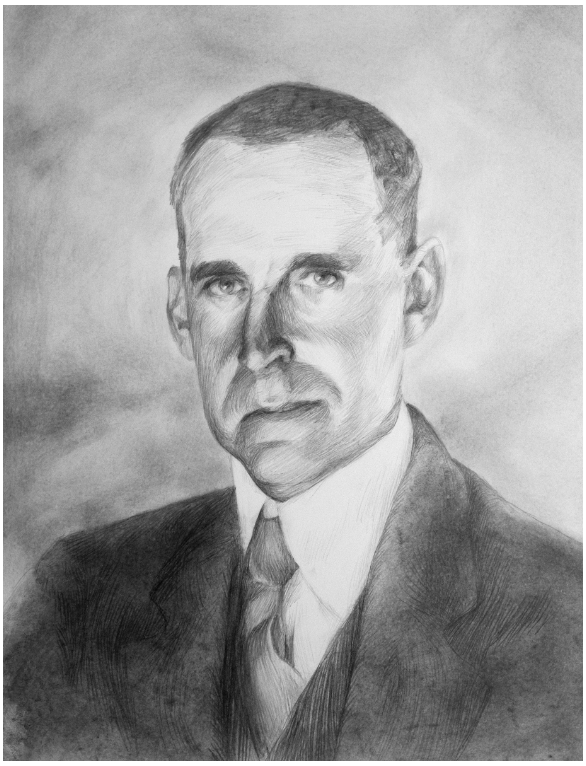

Fig. 1: Sir Arthur Stanley Eddington (December 28th, 1882 — November 22nd, 1944) was an outstanding theoretical physicist. He would become known for his approach to the gaseous stars. He derived a mathematical formulation which could account for the mass-luminosity relationship of the stars and was the 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].

Notes and acknowledgements

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Dedication

This work is dedicated to my youngest son, Luc.

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Liquid Metallic Hydrogen: A Building Block for the Liquid Sun

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Liquid metallic hydrogen provides a compelling material for constructing a condensed matter model of the Sun and the photosphere. Like diamond, metallic hydrogen might have the potential to be a metastable substance requiring high pressures for formation. Once created, it would remain stable even at lower pressures. The metallic form of hydrogen was initially conceived in 1935 by Eugene Wigner and Hillard B. Huntington who indirectly anticipated its elevated critical temperature for liquefaction (Wigner E. and Huntington H. B. On the possibility of a metallic modification of hydrogen. *J. Chem. Phys.*, 1935, v.3, 764–770). At that time, solid metallic hydrogen was hypothesized to exist as a body centered cubic, although a more energetically accessible layered graphite-like lattice was also envisioned. Relative to solar emission, this structural resemblance between graphite and layered metallic hydrogen should not be easily dismissed. In the laboratory, metallic hydrogen remains an elusive material. However, given the extensive observational evidence for a condensed Sun composed primarily of hydrogen, it is appropriate to consider metallic hydrogen as a solar building block. It is anticipated that solar liquid metallic hydrogen should possess at least some layered order. Since layered liquid metallic hydrogen would be essentially incompressible, its invocation as a solar constituent brings into question much of current stellar physics. The central proof of a liquid state remains the thermal spectrum of the Sun itself. Its proper understanding brings together all the great forces which shaped modern physics. Although other proofs exist for a liquid photosphere, our focus remains solidly on the generation of this light.

1 Introduction

Decidedly, the greatest single impetus for a fully gaseous Sun [1, 2] was the elucidation of critical temperatures by Thomas Andrews in 1869 [3, 4]. Since ordinary gases could not be liquefied at the temperatures associated with the Sun, it was inconceivable that the photosphere was made from condensed 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… 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 a liquid interior to the Sun is next to an impossibility*" [5, p. 36-37]. As a result of such logic, the idea that the Sun was gaseous flourished. Though Father Angello Secchi and Hervé Faye had already proposed a gaseous solar model [1], Andrews' discovery served to significantly validate their conjectures. Given the logic of the period, the body and photosphere of the Sun could not be liquid [1].

At the same time, scientists of the late 19th and early 20th century remained puzzled with respect to the solar spectrum [1, 2]. Because graphite was the prime source of blackbody radiation on Earth [6], G. Johnstone Stoney placed liquid or solid carbon on the surface of the Sun in 1867 [7]. It would remain there for the next 50 years [2]. Armed with graphite, it became simple to explain why the solar photosphere emitted a thermal spectrum resembling a blackbody. Over time, the enthusiasm for carbon began to wane. Charles Hastings argued that condensed carbon could not be present on the Sun. The temperatures involved did not permit such a hypothesis. Hastings required an alternative: "*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*" [8]. Hastings did not elaborate on these properties, but it was clear that he was searching for a substance with unbelievable refractory characteristics, something with the structure of graphite. A material capable of producing the thermal spectrum of the Sun had to exist in the condensed state at tremendous temperatures.

Eventually, theoretical astrophysics dispensed of the need for condensed matter. In so doing, the stellar opacity problem was created [9]. It was Schuster's *Radiation through a Foggy Atmosphere* [10] which began to cast condensed matter out of the photosphere [2]. Schuster postulated that all gases, if sufficiently thick, emitted as blackbodies: "*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*" [10, p. 6]. Schuster's conclusion was not supported by the gaseous nebula. These celestial objects had long been known to emit line spectra [11, p. 87] and, though they were assuredly thick, blackbody lineshapes were not produced. As previously outlined by the author [2], Schuster's error consisted in resting his derivation upon the premise that Kirchhoff's law of thermal emission was valid [12].

Gustav Kirchhoff insisted that, given thermal equilibrium with an enclosure, a blackbody spectrum could be produced by any object [12]. Yet, if Kirchhoff's law was correct, his contemporaries should not have refused to adopt a fully gaseous Sun throughout the 19th century [1, 2]. They would not have insisted on the need for graphite. If graphite was viewed as less than optimal, they would not have invoked pressure broadening as a means to produce the solar spectrum [1]. Kirchhoff's formulation, after all, was independent of pressure. It would become evident that something was not quite right with Kirchhoff's deductions. The author has outline why Kirchhoff's law of thermal emission was erroneous [13, 14]. On the simplest level, it constituted a violation of the first law of thermodynamics. In addition, as was outlined relative to the stellar opacity problem, gases remain unable to emit a blackbody spectrum [9]. This was the surest evidence that Kirchhoff's law was invalid.

As a result, if gases could not produce the solar spectrum, astrophysics should have returned to the condensed state. At the beginning of the 20th century, Jeans promoted liquid stars [15] based on stability arguments, only to discard them at the end of his life [2]. If Jeans abandoned liquids, it was likely due to his lack of a proper building block [2]. He conceived of stars as composed of heavy elements such as uranium and radium [2]. When the Sun was shown to contain large amounts of hydrogen [16–18], Jeans was left without a proper structural material. He did not anticipate that metallic hydrogen could exist [19] and that the substance provided the perfect candidate for a fully condensed Sun. In proposing the existence of metallic hydrogen [19], condensed matter physics would unknowingly provide Jeans with a suitable material for liquid stars [2]. Andrews' critical temperature in ordinary gases became inconsequential [20]. More intriguing was the observation that the layered lattice of condensed metallic hydrogen possessed tremendous similarity with graphite [19]. Could the layered form of metallic hydrogen finally replace Stoney's solid carbon on the Sun [2, 7]? Was this the strange material sought by Hastings for generating the solar spectrum [2, 8]?

2 Metallic hydrogen

Eugene Wigner (1963 Nobel Prize in Physics [21]) and Hillard B. Huntington [22] were the first to advance the existence of metallic hydrogen in 1935 [19]. They opened their classic paper by stating that "*Any lattice in which the hydrogen atoms would be translationally identical (Bravais lattice) would have metallic properties*" [19]. Their work focused on the body centered lattice. Recognizing the difficulties in obtaining the pressures required to form this lattice, they proposed that the layered form of metallic hydrogen would be more accessible. According to Wigner and Huntington "*it was J. D. Bernal who first put forward the view that all substances go over under very high pressure into metallic or valence lattices*" [19]. For the body centered cubic form of metallic hydrogen, they predicted a density of 0.8 g/cm^3 versus 0.087 g/cm^3 for molecular hydrogen in solid form [19]. This was nearly a tenfold increase in density. Wigner and Huntington concluded their paper as follows: "*The objection comes up naturally that we have calculated the energy of a body-centered metallic lattice only, and that another metallic lattice may be much more stable. We feel that the objection is justified. Of course it is not to be expected that another simple lattice, like the face-centered one, have a much lower energy, — the energy differences between forms are always very small. It is possible, however, that a layer-like lattice has a much greater heat of formation, and is obtainable under high pressure. This is suggested by the fact that in most cases of Table I of allotropic modifications, one of the lattices is layer-like*^{19}*. . .* " [19]. The footnote in the text began: "*Diamond is a valence lattice, but graphite is a layer lattice. . .* " [19]. Thus, in the first paper on metallic hydrogen, the layered structure of graphite (see Figure 1), so critical to producing the blackbody spectrum on Earth, was promoted. A solar spectrum explained through dense hydrogen was certain to eventually rise to prominence.

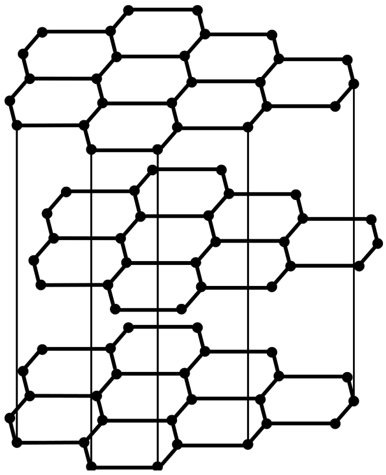

Fig. 1: Schematic representation of the layered lattice of graphite. Wigner and Huntington [19] would propose that most energetically favorable form of metallic hydrogen would assume this crystal structure.

2.1 Properties of metallic hydrogen

Initially, Wigner and Huntington estimated that the metallic state of hydrogen, in its most energetically accessible form (layered lattice), could be achieved at pressures in the 250,000 atm range (~25 GPa) [19]. This value was much too optimistic.

The most astounding property of metallic hydrogen would be its tremendous critical temperature. It was well in excess of anything Thomas Andrews and his contemporaries could have imagined in 1869 [3,4]. While the complete phase diagram for hydrogen may never be fully known, several attempts have been made to outline its general characteristics, both in condensed matter physics [23–25] and as related to astrophysics [26–28]. Franck [29] listed many of the early contributions to the hydrogen phase diagram, including the work by Alexey A. Abrikosov [30]. Abrikosov eventually won the 2003 Nobel prize in physics while at Argonne National Laboratories.

The critical point of metallic hydrogen has been constantly revised towards ever higher values. Ebeling and Richert [23] provided an overview of these estimates through the 20th century. In 1980, Franck [29] arrived at a critical temperature for metallic hydrogen in the 6,000–9,000 K range. In 1983, Ronik and Kundt [26] gave a critical point at a unprecedented 19,100 K and 24 GPa. A slightly more conservative 16,500 K and 22.5 GPa was soon published [23]. Beyond critical temperatures, the transition pressures in moving from molecular to metallic hydrogen have constantly been revised upwards. At present, the values have moved to the 400–600 GPa range: "*Although quantum chemistry calculations have been developed to a high degree of sophistication, and in general, there is a close correlation between theory and experiment, this is not the case for hydrogen. Phase transition calculations that seek the structure with the lowest lattice energy have difficulty handling the zero-point energy contributions to the total energy and zero-point energy is very important for hydrogen. As a result, the predicted critical transition pressures have an enormous variation, from as low as 0.25 Mbar to over 20 Mbar, while recent predictions are in the 400 to 600 GPa range*" [25].

2.2 The theory of metallic hydrogen

Several authors have reviewed the metallic hydrogen literature [31, 32]. In a landmark 1968 publication, Neil Ashcroft hypothesized that metallic hydrogen might be a high temperature superconductor [33]. Ashcroft consequently became one of the most important theoretical physicists with respect to understanding dense hydrogen in its molecular and metallic forms [24, 33–50]. Ashcroft's prediction relative to high temperature superconductivity was rapidly echoed by Schneider and Stoll [51]. Depending on lattice configurations, they calculated that metallic hydrogen would become superconductive with operational temperatures ranging from 67 to 200 K [51]. Barbee et al. confirmed these calculations, obtaining a temperature of 230±85 K [52]. Metallic hydrogen had the potential to be the highest temperature superconductor known. The point was emphasized in 2001, when Maksimov and Savrasov used *ab initio* calculations to conclude that metallic hydrogen at high pressure might have a superconducting critical temperature of 600 K [53].

Ashcroft also examined the ground state of metallic hydrogen at zero temperature under conditions of changing spatial densities achieved by varying pressures from ~1 Mbar to ~75 Mbar [34, 35]. At the highest densities ($r_s = 0.8$, 1.2, 1.36, and 1.488), he discovered that crystalline phases were preferred [35]. However, at the lowest lattice density studied ($r_s = 1.64$), he found that metallic hydrogen was metastable between the solid and liquid forms [34, 35]. He postulated that the existence of a liquid ground state could not be excluded, but that it was not established [34]. Ashcroft continued this line of investigation in 1981 and 1982 [36, 37]. He gathered that liquid metallic hydrogen might become essentially devoid of structure and that the protons and electrons would simply act as interpenetrating fluids [36]. The Cornell scientist had theoretically constructed a two-component Fermi-liquid from protons and electrons [36].

Still, there was no direct evidence that metallic hydrogen at absolute zero would ever completely lose all structural integrity. As a theoretical physicist, Ashcroft could not really establish if metallic hydrogen at absolute zero 1) acted as a two component Fermi liquid, 2) behaved much like the unusual theoretical one-component plasma [54, 55], or 3) retained the essential characteristic of a Bravais lattice, an ordered proton field with fully degenerate electrons. Nonetheless, in his 1981 communication, Ashcroft was careful to mention that his conclusions were "*assuming that it [the hypothetical state of liquid metallic hydrogen] is normal*" [36]. He highlighted: "*that in assuming the existence of a liquid phase, the very interesting question still remains of whether or not it exhibits some form of magnetic, momental, or even spatial (e.g. liquid crystal) ordering... We do not attempt at this time to resolve the important questions of the existence or properties of possible "ordered" liquid metallic phases of hydrogen*" [36]. In the ninth footnote to his 1982 treatment, Ashcroft repeated the warning: "*The possibility that liquid metallic hydrogen exhibits some kind of momental (e.g. superconductive), magnetic, or even spatial (e.g. liquid crystal) ordering has not been ruled out*" [37]. Only experimental evidence could answer such questions, but none was available, as liquid metallic hydrogen remained an elusive material [25, 31, 32, 56].

Astrophysics was quick to infer that Ashcroft had chosen a path eventually leading to some form of degeneracy of matter [57]. In fact, careful reading of these articles suggested otherwise. Ashcroft's liquid was a reflection of what theoretical condensed matter physicists were able to calculate at the time. A liquid with spatial order, thoughtfully preserved in the text [36] and in the footnotes [37] of his papers, was well beyond the reach of computational approaches in the absence of laboratory guidance.

Soon after Ashcroft published his groundbreaking papers [34–37], MacDonald and Burgess also wondered about the absence of crystallization in metallic hydrogen [58]. They insisted that, since electronic screening was important in the solid state but negligible in the liquid state, metallic hydrogen would remain fluid at all pressures. Solid metallic hydrogen could not be stable at any pressure [58]. Ashcroft answered that "*The prospect of a relatively low-density quantum melted phase of hydrogen, over a wide range of densities, is a fascinating one. However, we would like to bring up the following difficulties with concluding too definitely the existence of this phase for all densities*" [38]. Ashcroft then argued that such a state would exist only over limited densities whose range would be difficult to predict, as the solid and liquid phases are both close in energy and widely separated in configuration [38].

When Ashcroft returned to the ground state of metallic hydrogen in 1984, he assumed that the protons occupied the sites of a rigid Bravais lattice [39]. Using the Wigner-Seitz approximation which he regarded as physically appealing, Ashcroft calculated a lower bounds on the density of metallic hydrogen at its transition pressure. This density would be on the order 0.60 g/cm^3 corresponding to $r_s = 1.65$ [39]. Metallic hydrogen, if it was stable at all, would have to possess a greater density.

Given the nature of metallic hydrogen, both as a theoretical problem and as a prized material, significant Russian and Ukranian contributions were made in this area [32,53,59–65], beginning with Alexey Abrikosov [30]. In an important communication, Abrikosov was one of the first to examine the destruction of an atomic lattice under high compression [59]. He noted: "*that at sufficiently small volumes the positive zero-point oscillation energy exceeds the negative Coulomb energy, and this leads to a destruction of the crystal lattice*" [59]. Abrikosov remarked that "*the inter-atomic distances at the transition point are greater than the nuclear dimensions only for the lightest elements, hydrogen and helium. Thus, such a transition can take place only in these two elements*" [59]. It seemed as though elevated pressures might lead to the destruction of the crystal lattice, but Abrikosov never considered that fusion might act to relieve the stresses of compression. Beyond a certain point, perhaps crystals became incompressible. It was unclear if the small volumes required to give prominence to the zero-point oscillations in metallic hydrogen might ever be reached.

After Abrikosov's classic paper was released [59], Brovman et al. were the first to hypothesize that metallic hydrogen might be a metastable substance [61]. Kagan's group [32,61] advanced that metallic hydrogen synthesized at elevated pressures might be completely stable even at zero pressure. This behavior would be much like diamond, the metastable form of carbon. Brovman et al. [61] calculated that the most stable lattice of metallic hydrogen would be hexagonal with a triangular string structure [60] . The conjecture would spawn the possibility of industrial and propellant roles for metallic hydrogen [25]. Many years later, Kaim et al. [64] would once again address the metastable nature of metallic hydrogen and essentially confirm Kagan's findings [61].

However, the most interesting facet of Kagan's work [61] was the observation that metallic hydrogen displayed liquid tendencies: "*there occurs in metallic hydrogen a unique tendency towards the formation of a family of structures with very close energies… In a certain sense the picture recalls the situation with graphite, but is apparently even more strongly pronounced… the formation of the planar family is evidence of the unique liquid-like tendencies that take place in metallic hydrogen under pressure*" [61]. They continued: "*As a result it is impossible to exclude beforehand, in principle, the possibility that the transition from the molecular phase to the metallic phase is a transition into the state of a liquid metal. (It may turn out that the situation will be different in hydrogen than in deuterium.) The phase diagram could have in this case a very special character. For example, with increasing pressure, the liquid phase could go over into the crystalline phase, but at extremely high densities a liquid would again be produced, but now as a result of the predominant role of the energy of the zero point oscillations (see the paper by Abrikosov*^{7}*). The metastable state could remain crystalline in this case*" [61]. The footnote referred to the work just discussed above by Abrikosov [59]. Relative to the liquid metallic hydrogen model of the Sun, the work by Brovman et al. [61] would remain landmark.

Barbee et al. [52, 66] continued the quest to calculate the most stable structure for hydrogen in solid form. The work supported Wigner and Huntington's [19] contention that a layered Bravais lattice form of metallic hydrogen was the most stable in the 380±50 to 860±100 GPa range [66]. Above such values, the body centered cubic was preferred. Below 380±50 GPa the molecular non-metallic hexagonal-close-packed arrangement was most stable. The authors highlight some of the difficulties faced by theoretical condensed matter physics: "*A metal-insulator phase is expected near 200 GPa, in the m-hcp phase, but this transition pressure is harder to predict because of the shortcomings of local-density theory and the fact that structures with similar enthalpies (e.g. diamond and graphite) may have completely different band structures*" [66].

At about the same time, an interest developed in theoretical physics for examining the mono-, di-, and trilayered forms of atomic hydrogen [67–69]. While it could be argued that such structures were not physically realistic, their study generated additional insight into metallic hydrogen. Significantly, they demonstrated that very small changes in lattice parameters could alter the conductive behavior substantially, creating insulators from metals.

For his part, Neil Ashcroft maintained his interest in the structure of hydrogen. In 1993, he once again examined the metal-insulator transition in this element [40]. At this time, Ashcroft moved increasingly towards the idea that dense hydrogen might lack local structure at the lower densities. It seemed as if the stability of crystal forms was becoming questionable for him, even at the higher densities: "*At sufficiently high densities (*$r_s \leqslant 1.5$*), the predicted states of H (eq. 1) certainly include monatomic crystalline arrangements [6], at least where the dynamics of the protons can be ignored*" [40].

Though recognizing the presence of crystalline forms, he emphasized the dynamics of the protons. Observing that the proton pairing in molecular hydrogen was robust, Ashcroft eventually proposed that molecular metallic hydrogen might be energetically preferred [42]. This was a material very different than first proposed by Wigner and Huntington [19]. At low temperatures and at pressures less than 110 GPa, Ashcroft argued that molecular hydrogen existed as a rotational crystal [40,42]. At low densities ($1.5 < r_s < 2$), he envisioned that hydrogen might become a low temperature quantum fluid [45].

Ashcroft moved further towards the idea that, at the proper density, liquid hydrogen was a superfluid [47, 48]. In doing so, he revisited the ideas elucidated when first dealing with two component Fermi liquids [36, 37] and expanded on his work with Moulopoulos [41]. Ashcroft appropriately highlighted that experiments up to 300 GPa proved that molecular H-H stretching modes continued to exist at these high pressures [47]. He insisted that both proton-proton and electron-electron pairing could become the dominant interaction, given the proper conditions [41]. The concept that liquid metallic hydrogen was a two gap superconductor was also promoted by Babaev [70]. In such a superfluid, both protons and electrons could flow in the same direction, providing mass transfer without charge transfer. Alternatively, the system could result in a superconducting mode wherein proton and electrons flowed in opposite direction, resulting in the flow of both mass and charge [48]. Ashcroft then emphasized that "*the neutral superfluid mode does not couple to an external magnetic field, while the charged superconducting mode does*" [48]. The work did not address metallic hydrogen in its densest form. Ashcroft mentions that: "*Above any superconducting transition temperature (and above any Bose condensation temperature) liquid metallic hydrogen and deuterium should begin to adopt properties similar to those of conventional liquid metals, at least in the structural characteristics important to electron scattering*" [47].

Ashcroft's hypothesis that metallic hydrogen might exist as a quantum fluid immediately gained theoretical support [71]. Given increasing compression, Bonev et al. [71] calculated that solid molecular hydrogen [72] would be transformed into a quantum fluid state. Additional pressure would then lead to the monatomic crystal [19, 71]. With increasing pressure, it could be computed that hydrogen might undergo a transition from a liquid-molecular state into a non-molecular liquid [71]. This would become known as the liquid-liquid transition [71]. By extending the work of Brovman et al. [61], it was possible to visualize that hydrogen had a zero-temperature structured liquid ground state. With enough pressure, hydrogen could then move from the two component Fermi liquid [36, 37, 41, 46–48, 70], to the crystalline solid [19], and finally into a zero-temperature structured liquid state [61]. Alternatively, metallic hydrogen might move from a two component Fermi system directly either into a structured liquid metal [61] or into the solid classical form of metallic hydrogen [19]. A wide array of theoretical possibilities now existed for the state of hydrogen under dense conditions.

While the theory of liquid metals [73] has remained a fascinating branch of condensed matter physics, hydrogen liquid metals, though they appear simple on the surface, continued to offer unequalled challenges. With only sparse experimental data (see Section 2.4), theoretical condensed matter physics had little guidance from the laboratory. Even so, progress was being made, if only in the realization that metallic hydrogen was a material filled with mystery and promise. Modern condensed matter theory persisted in providing exciting results, often from the most prestigious groups [74–81].

Relative to solar physics, it was clear that the superfluid form of metallic hydrogen [36,37,41,46–48,70], devoid of all structure, could never be found on the surface of the Sun. The material required a very specific critical density along with low temperatures not found on the solar surface. Superfluid metallic hydrogen resembled nothing of the layered structure [19, 61] which mimicked graphite and was most likely to generate the solar spectrum. Superfluid metallic hydrogen [36, 37, 41, 46–48, 70] might never be found anywhere.

Fillinov et al. [74] studied dense hydrogen states at temperatures ranging from 10,000 to 100,000 K examining plasma phase transitions. Interestingly, at 10,000 K, they noticed droplet formation at certain densities (10^{23} cm^{-3}). But at the highest densities studied (10^{26} cm^{-3}), they observed an ordering of protons into a Wigner crystal. These were tremendous densities on the order of ∼150 g/cm^3. Militzer and Graham extended theoretical calculations to the petapascal range, a full eight orders of magnitude beyond the pressures of the molecular phase [76]. Such computations were appropriate only for the interior of astrophysical objects. Militzer and Graham [76] considered astounding hydrogen densities (2100 g/cm^3), but, in contrast to Abrikosov classic paper [59], the lattice was not destroyed and the calculations open serious questions as to the nature of the solid state.

Remaining in the realm of physically attainable pressures, Attaccalite and Sorella [77] demonstrated that the molecular liquid phase of hydrogen should be stable at pressures on the order of 300 GPa at ∼400 K. The melting curves for hydrogen and its phase boundaries have likewise been addressed [78, 79] revealing that theoretical approaches have remained difficult and open to new discoveries. Miguel Morales, while working with David Ceperley and Carlo Pierleoni [80], recently addressed the problem of metallic hydrogen by considering a range of temperatures and densities ($2,000 \leqslant T \leqslant 10,000$ K; $0.7 \leqslant \rho \leqslant 2.4$ g/cm^{-3}). Such conditions were appropriate for liquid metallic hydrogen devoid of structure, much like the one-component plasma [54, 55]. At elevated temperatures and densities, the system was observed to be a fully metallic liquid plasma [80]. However, a combination of lower densities and temperatures resulted in formation of an insulator [80]. Ceperley's group also considered electrical conductivity in high pressure liquid metallic hydrogen [81]. The work was noteworthy, as it tried to examine the liquid-semiconductor to liquid metal transition first reported experimentally by Weir et al. [82, 83]. Using either 32 or 54 atom cells, they calculated the transition density to be near $r_s \sim 1.65$, a value very close to the experimentally determined number ($r_s = 1.62$) [82, 83]. These calculations assumed that the liquid was devoid of any structure. In addition, David Ceperley examined hydrogen at ultra high pressures, $P \geqslant 20$ TPa [84], a value considerably lower than that of Militzer and Graham [76]. Furthermore, the Urbana-Champaign scientist studied the phase diagram for hydrogen in the ground state [85]. However, the theoretical procedure utilized was best suited to tritium and deuterium, as infinitely massive protons were hypothesized to be present. This work presented an excellent literature review and a remarkable array of potentially significant new structures for the ground state of hydrogen as a function of increasing pressure up to 5 TPa [85].

2.3 Metallic hydrogen in astrophysics

Soon after Wigner and Huntington [19] published their classic paper, liquid metallic hydrogen entered the realm of astrophysics. Its introduction as a constituent of the giant planets and the white dwarfs far preceded any experimental confirmation. Liquid metallic hydrogen would eventually occupy a peripheral position in astronomy, well removed from the Sun and most stars of the main sequence.

In 1946, Kronig et al. [86] proposed that metallic hydrogen existed at the center of the Earth. Their work was motivated by a recent report postulating that the Earth's center was composed of residual solar matter containing up to 30% hydrogen. Kronig et al. [86] calculated a density for metallic hydrogen of 0.8 g/cm^3. The result was apparently independent of Wigner and Huntington [19] as they seemed unaware of this previous communication. Then in 1950, W.H. Ramsey extended the study of metallic hydrogen to the planets and the white dwarfs [87]. According to Ramsey, at the International Astronomical Union meeting in Paris of 1935, H. N. Russell [88] had pointed out: "*that both the planets and the white dwarfs are cold in the sense that the density at any interval point is determined by the pressure at that point. In other words, the influence of temperature is so small that it can be neglected to a good approximation. Thus, in the accepted theory of the white dwarfs it is assumed that the electrons constitute a degenerate Fermi gas at absolute zero temperature*" [87]. The minutes of the meeting highlight how Russell believed that the maximum radius of a cold body was equal to one tenth of the solar radius, or about the diameter of Jupiter [88, p. 260]. It was a crucial statement which linked studies of the giant planets with those of the white dwarfs. At the pressures inside white dwarfs and giant planets, all solids were viewed as metallic [87]. Hydrogen was no exception. In the end, Ramsey deduced that metallic hydrogen could not be produced inside a small planet like the Earth [87]. Hence, it was primarily because of this work [87] that the quest for liquid metallic hydrogen would be extended simultaneously to the celestial objects with features of mass and density lying to either side of the Sun. In these objects, the study of liquid metallic hydrogen [26–28, 89, 90] progressed quickly to the fully degenerate liquid state (i.e. — states where both protons and electrons were unrestricted by lattice confinements).

Astrophysical bodies are not pure laboratory samples. They are an assembly of mixtures and alloys. As such, once scientists gained interest into the composition of the planets [91–95] and the white dwarfs (see [96] for a short review relative to ^{22}Ne), hydrogen/helium mixtures [97, 98] and their alloys [49, 50, 99] were certain to attract attention. Along with Ashcroft, Eva Zurek and her coworkers [50] discovered that lithium had the capability of greatly stabilizing the metallization of hydrogen. Even the phase diagram for carbon under extreme conditions grew in importance, as potentially relevant to understanding Neptune, Uranus, and the white dwarf [100, 101]. A vast number of publications flourished, but they shared one common factor: the paucity of laboratory data. Nellis et al. extended results from the laboratory to interior of Jupiter [94, 95], well before his findings [82] were independently confirmed. Nellis' work on the production of liquid metallic hydrogen (see Section 2.4) at 140 GPa and 3,000 K was supported by conductivity measurements [82], although the merits of these measurements were to remain in doubt. In any case, astrophysics continued to insist that the large planets and white dwarfs were constituted of liquid metallic hydrogen devoid of structure and existing in fully degenerate states. At pressure of ~500 GPa (5 Mbar), William Nellis maintained that materials were either semiconductors or fully degenerate metals [102]. Experimental confirmation of a fully degenerate state for liquid metallic hydrogen at such pressures was unproven. In the laboratory, all forms of metallic hydrogen remained ethereal with theoretical predictions far surpassing experimental reality.

2.4 Laboratory quests for metallic hydrogen

Throughout the 20th century, the study of extraordinary states of matter has represented one of the most fascinating aspects of physics [102, 103]. The generation of extremes in temperatures, pressures, and densities has always involved complex and sophisticated experimental resources, often attainable only through national or multinational initiatives [103]. Nonetheless, with regards to metallic hydrogen [102], many efforts have been conducted in university level laboratories. Frederic Golden has provided an excellent review of the search for metallic hydrogen which Ho-Kwang Mao dubbed the "Holy Grail" of condensed matter physics [104]. Golden touches on the early Russian and American attempts to synthesize the material, along with a general description of methods [104]. Given the prize [56], experimental progress has been limited.

In June 1989, Ho-Kwang Mao and Russell Hemley, from the Geophysical Laboratory of the Carnegie Institution, reported evidence of metallization for hydrogen at 77 K and 250 GPa in the journal *Science* [105]. The key finding was the near opaqueness of the sample at the highest pressures. Isaac Silvera, working at the Lyman Laboratory of Physics at Harvard, was studying the metallization problem in parallel with Hemley [106–111]. He rapidly contested the validity of Hemley's claims and submitted a letter to Science [106] to which Mao and Hemley responded [107]. Silvera argued that visual darkening provided insufficient evidence for metallization and that further tests were needed [106]. Mao and Hemley defended their result, but in the end, conceded that "*The observations and spectroscopic measurements clearly indicate that significant changes in solid hydrogen occur with increasing pressure, but further work is needed to characterize in detail its optical, electrical, and structural properties under these conditions*" [107]. Silvera soon reported that there was no evidence of metallization up to 230 GPa from 77 to 295 K [110]. Metallic hydrogen had slipped away, but Ho-Kwang Mao, Russell Hemley, and Isaac Silvera would come to rank amongst the experimental leaders in the struggle to synthesize the material.

A few years later, Weir, Mitchell, and Nellis reported anew that metallic hydrogen had been produced [82]. Using shock compressed experiments [102, p. 1510–1514], the metallization of fluid molecular hydrogen was thought to have been achieved at 140 GPa and 3,000 K [82]. The communication was supported through conductivity measurements [82] a vital link in establishing metallization. The results were once again contested [112], though Nellis and Weir maintained their position [113]. In arguing against metallization, Besson brought in data with deuterium suggesting that its samples might represent highly degenerate material, something very different from molecular metallization in hydrogen [112]. Beyond this, Besson was concerned that the Al_2O_3 windows had affected the experiment [112]. Nellis and Weir countered that "*Our experiment and analysis yield the simple picture of a dense metallic fluid comprised primarily of molecular H_2 dimers and a relatively low dissociation fraction of ~5% of H monomers*" [115]. The entire sequence of observation was on the order of just a few hundred nanoseconds [102, p. 1512], hardly time to conduct detailed structural analysis, while introducing tremendous difficulties in properly measuring both pressures and conductivities. William Nellis once again addressed his metallization experiments, but this time with Neil Ashcroft as a coauthor [114]. During the discussion which followed the paper, Nellis admitted that "*the exact nature of this unusual fluid needs to be determined*" [114, p. 135]. Though Nellis eventually claimed that "*Metallic fluid H is readily produced by dynamic high pressures*" [102, p. 1564], only questionable evidence existed for this state [82]. The shock experiments of metallic hydrogen from this group produced no additional results and other groups never confirmed the findings. The lack of lattice structure was debatable and mankind was no closer to metallic hydrogen. For his part, William Nellis moved to arguments of degeneracy, without solid experimental grounds [102].

In 1996, a collaboration between the University of Paris and the Geophysical Laboratory at the Carnegie Institution would make the next vital step forward [115]. Loubeyre et al. [115] examined both solid hydrogen and deuterium with X-ray diffraction at pressures just exceeding 100 GPa at 300 K. They discovered that solid hydrogen "*becomes increasingly anisotropic with pressures*" [115]. In like manner, the layered structure of graphite was considered anisotropic. Loubeyre et al. [115] tried to generate the equation of state for hydrogen as a function of temperature and pressure. They concluded that their results differed substantially from ab initio calculations "*indicating that theoretical understanding of the behavior of dense hydrogen remains incomplete*" [115]. Narayana et al. then studied solid hydrogen up to 342 GPa at 300 K [116]. These were pressures similar to those at the center of the Earth [117], but no evidence of metallization was found. The findings confirmed Ramsey's conclusion that the interior of the Earth could not support the metallic state of hydrogen [87]. In 2002, Loubeyre et al. again presented evidence that solid hydrogen became black, this time at 320 GPa and 100 K [118]. These values were not far removed from the 250 GPa used by Mao and Hemley in 1989 [105]. By observing the vibron mode, they maintain that molecular hydrogen in the solid form existed at least until 316 GPa, but Narayana had just reported that solid hydrogen remained transparent up to 342 GPa at 300 K [116]. Two of the world's major groups were again at odds with one another. Perhaps the discrepancies could be explained by difficulties in recording proper pressures at such values [102, p. 1514–1533]. After all, these studies were far from trivial in nature. Loubeyre et al. [118] refrained from stating that metallization had been achieved. Rather, they predicted that the process should occur near 450 GPa [118].

Mankind has remained unable to synthesize metallic hydrogen in the laboratory. However, as pressures rose and experimental settings improved, the characteristics of dense hydrogen did become increasingly established [119–125]. Great attention was placed on constructing phase diagrams for hydrogen (see [119] for a review). Determination of the peak in the melt line of this element has consequently been the subject of intense study (e.g. [121–124]). By this time, the broken symmetry and hydrogen-A phase for dense hydrogen were reasonably established, but neither form was metallic (see [122] for a brief review). Blackbody radiation finally entered such studies, with the goal to properly establish temperatures [122]. Along these lines, statements such as: "*we have shown that the emissivity of platinum is essentially independent of temperature in the temperature region of our study*" [122] would only serve as a reminder that not all was correct with our understanding of blackbody radiation [13, 14]. For its part, metallic hydrogen continued to be ephemeral.

2.5 Commentary on liquid metallic hydrogen

As was seen in Section 2.3, within astrophysics, liquid metallic hydrogen is believed to exist as fully degenerate matter within the interior of white dwarfs and giant planets such as Jupiter or Saturn. Some have suggested that these planets also possessed liquid metallic helium, or a liquid metallic alloy of hydrogen and helium. Solid metallic hydrogen would have no role in astrophysics [27], as every hypothesis was either a molecular or a fully degenerate liquid. The conjecture that condensed matter could become degenerate in the large planets was far from what Chandrasekhar had envisioned when he first promoted degeneracy [57]. As a fully degenerate material, liquid metallic hydrogen could not sustain any useful current or magnetic field. Positive charges in liberal motion along with negative charges do not seem very amicable, either to potential generation or net current flow. At the same time, current flow with mass transfer seemed unreasonable in astrophysical objects. Direct laboratory observations remained much too elusive to reach any confirmation of these theoretical ideas. Some element of structure might always exist in metallic hydrogen independent of temperature. The superfluid form could remain ever theoretical, as Ashcroft had first carefully cautioned in the work with Oliva [36, 37].

The application of fully degenerate matter to the large planets and the white dwarfs was an unusual concept in light of a fully gaseous Sun. If Jupiter contained metallic hydrogen as degenerate matter and the same was true for the white dwarf, then it would not be unreasonable to place at least some condensed hydrogen on the Sun. Solar temperatures would prevent degenerate states and thus layered liquid metallic hydrogen represented a remarkable constitutive element.

When it was first conceived, the most energetically accessible form of metallic hydrogen was the layered lattice arrangement similar to that of graphite. Solid metallic hydrogen was viewed almost as a one component plasma [54, 55], wherein all electrons were degenerate and distributed over a hexagonal Bravais lattice formed from ordered protons [19]. In this sense, solid metallic hydrogen was considered as degenerate only relative to the flow of its electrons. Today, theoretical astrophysics has abandoned early thoughts of solid or liquid metallic hydrogen possessing a Bravais lattice [19], opting instead for fully degenerate materials where both protons and electrons flow freely. Conversely, experimentalists hope to harness metallic hydrogen for processes as varied as earthly fusion and rocket propulsion [25]. Such processes would not be easily approachable with a fully degenerate material. Hence, many experimental physicists are likely to be skeptical of a fully degenerate state for metallic hydrogen.

The progress towards dense hydrogen states has been an intriguing aspect of condensed matter physics. Ashcroft's two component Fermi liquid has remained a fascinating substance. However, given the combination of low temperatures, exact densities, and atypical conductive properties, it could have little practical role in human advancement. Current flow involving mass displacement was a concept which seemed to oppose structural stability, even though it could sustain magnetic fields. Conversely, when proton and electron displacement occurred in the same direction, there could be no current or the generation of magnetically interesting properties.

Theoretical condensed matter physics promoted hydrogen at extreme densities [76, 84], but hydrogen might not be compressible to such levels. In permitting essentially infinite compression of the lattice, it was debatable whether or not condensed matter physics had adopted a behavior similar to the ideal gas. Moreover, if compression was great enough, the solid might resist further attempts at reducing lattice dimensions. Fusion might relieve the stresses associated with compression.

3 Lessons from the Sun

Though the Sun would always remain devoid of the great advantage of our earthly laboratories, it has historically provided us with an amazing insight into nature. When Sir Joseph Lockyer and Pierre Jules César Janssen independently observed the lines of helium within solar spectra acquired in 1868 [126–130], they must have wondered if this unknown element would ever be discovered. Lockyer named this element Hēlios, the Greek name for the Sun god and the Sun [126]. Eventually, William Ramsay would isolate helium from cleveite [131–133], and the Sun would be credited for providing the first indication that helium existed. The identification of Coronium would follow a parallel story [134–136]. It took nearly three quarters of a century for Bengt Edlén and Walter Grotrian to finally identify Coronium from transition lines produced by highly oxidized iron, like Fe^{+13} and Fe^{+14} [136, p. 170]. Hence, a combination of earthly science and celestial observations became critical to the development of astronomy. This spirit of discovery has taught astronomers how to tackle even the most perplexing problems. The understanding of the solar spectrum should not be an exception.

3.1 Graphite, metallic hydrogen, and the solar spectrum

If graphite played a critical role, both in the construction of blackbodies [14], and historically in the structure of the Sun itself [2], it was because science has always recognized that graphite possessed a unique ability towards the production of Planck's spectrum [6, 13, 14]. Hastings was searching for a material which would possess many of the properties of graphite [8]. Graphite, the layered form of carbon, differed significantly in optical properties from its cubic counterpart, diamond. Structure was vital to the production of spectra. That materials were condensed was not sufficient, but a distinct lattice arrangement seemed central [9]. As a consequence, it would be expected that the layered form of metallic hydrogen would resemble graphite itself in its optical properties. In contrast, fully degenerate forms of hydrogen [36, 37, 41, 46–48, 70–72] could never approach such optical behavior. Devoid of a true lattice, such a substance, if it truly existed anywhere, would be completely unable to generate a blackbody spectrum [6]. These are the lessons from our earthly laboratories, after examining thousands of materials over extreme ranges of frequencies and temperatures [13, 14, 137]. The structural lattice of graphite and soot was to remain unique in its thermal properties [13, 14]. It should serve as a guide for the nature of any condensed material placed either on the photosphere or within sunspots. The generation of a thermal spectrum with a blackbody lineshape has been solely a quality of condensed matter, not of gases, degenerate matter, or any other state which physicists might create.

Unlike the giant planets, the Sun possessed a unique feature: the ability to generate tremendous internal pressures and temperatures. Based on the solar spectrum [138–140] and other physical evidence [141], it was therefore reasonable to postulate that liquid metallic hydrogen must constitute the bulk of the solar mass and specifically the photospheric material [20, 142–149]. In considering a solar building block, thermal emission required a distinct lattice [150], as the absence of such structure would lead to the stellar opacity problem [9]. The author has previously made the point: "*As a result, the photosphere must be treated as condensed matter. Unfortunately, it is counterintuitive than an object at extreme temperatures can possess lattice structure. Nonetheless, given the evidence for condensed matter*^{4}*, the solar constitutive element (primarily H) must form a lattice. The presence of powerful solar magnetic fields and gravitational forces make liquid metallic hydrogen a distinct possibility for the condensed state of the photosphere. In this case, the hydrogen nuclei can be viewed as arranged in an array forming an essentially incompressible solar lattice. The hydrogen electrons are contained within the metallic conduction bands. The inter-nuclear distance is being maintained by the need to keep the quantum conditions such that metallic conduction bands can be produced. Hydrogen contains no inner shell electrons. All the electrons are completely delocalized within the metallic conduction bands. As such, hydrogen in this state is not only a liquid metal (reminiscent of liquid sodium) but can also be viewed as a liquid metallic plasma*" [149]. The footnote referred to reference [141] in this work.

In the solar framework, the electrons would translate freely within the confines of conduction bands formed by the Bravais lattice of the protons. Though not a one-component plasma in a theoretical sense [54, 55], liquid metallic hydrogen could be considered as a one-component plasma in the physical sense since the electrons were delocalized. But liquid metallic hydrogen would possess a true Bravais lattice and, perhaps, even liquid crystal behavior [151–153]. In this regard, Ashcroft had left open the possibility that liquid metallic hydrogen was a liquid crystal in 1981 and 1982 [36, 37]. Ashcroft had been unable to exclude the possibility when he advanced the two-component Fermi liquid [36, 37]. Liquid metallic hydrogen could well have an ordered lattice which oscillates between structural forms. The finding by Brovman et al. [61] that metallic hydrogen, much like graphite, could adopt a family of structures with nearly the same energy should be considered in this regard.

In any event, it would be difficult to conceive that conduction bands could truly exist without a lattice and the importance of the Bravais lattice in the formation of metals should not be dismissed. To a large extent, liquid metallic hydrogen should preserve the layered structure of solid metallic hydrogen as anticipated by Wigner and Huntington [19]. But the metallic character might be somewhat reduced in the low pressures of the photosphere. In fact, this could be advantageous for emission, better resembling graphite. Indeed, if the graphitic spectrum was to be produced, the structure and conductive properties of liquid metallic hydrogen should resemble graphite as much as possible. This is because graphite represents the premier laboratory model.

3.2 Metallic hydrogen and solar structure

Metallic hydrogen, with its critical temperatures in the thousands of degrees Kelvin [23–26], overcomes all concerns raised regarding a liquid Sun based on Andrews [20] and his findings in ordinary gases [3, 4]. A liquid Sun composed of metallic hydrogen benefits from elevated critical temperatures for liquefaction, permitting hydrogen to adopt a condensed state even within an object like the Sun. Along these lines, it is doubtful that metallic hydrogen could really become infinitely compressed. Such a scenario appears unlikely, as the presence of conduction bands involves quantum restrictions on the lattice. If the internuclear distances are not ideal, quantum mechanical conditions should fail to support conduction. Two boundary conditions should exist. If the interatomic distance becomes too large, the substance should become an insulator. Similarly, if the interatomic distance becomes too small, the crystal should collapse [59] and conduction cease. In this respect, it would be important to note that the Sun has dynamo action and maintains large magnetic fields. Both of these phenomena make destruction of the conducting lattice unlikely [141].

It remains unclear why condensed structures resist compression, but invoking fusion as a means of releasing the strain of compressions should be a viable solution. This is especially the case if compared to the destruction of the crystal [59] and the creation of fully degenerate matter [36, 37, 41, 46–48, 70]. Degeneracy removes all of the forces which lead to fusion. As such, it should be more reasonable to maintain the relative incompressibility of condensed matter. The Sun, after all, has a very ordinary density of 1.4 g/cm^3 [141] and the same is true for the giant planets. Thus, Jeans' idea that the Sun represents a rotating liquid mass of reasonably constant density should not be dismissed [2]. Condensed matter and metallic hydrogen provide a framework for ordinary densities, even in light of enormous pressures. The reward of such an approach is threefold leading to: 1) a reasonable framework to generate the solar spectrum, 2) a decent ability to impart structure, and 3) a practical path towards fusion.

A Sun composed of metallic hydrogen provides an interesting model to explain sunspots and other structural elements. The photospheric material in this case might be considered as liquid metallic hydrogen where the lattice dimensions are relaxed at lowered pressures. Perhaps, the material exists much like graphite at the limits of conductive behavior. Conversely, within sunspots, pressures would be more elevated, and liquid metallic hydrogen might assume a more compact lattice, with increased metallic behavior. This would help account for the stronger magnetic fields observed within sunspots. As a result, scientists could be considering the conversion from a Type I lattice in the photosphere to a Type II lattice in the sunspots [141]. Such a scenario has great advantages in terms of simplicity.

Gases have always been an unsustainable building material for an object like the Sun. Gases know no surface and cannot, even momentarily, impart structure. Hence, one cannot be surprised to find that there is no physical evidence which supports a gaseous Sun, while ample evidence [141] has been revealed for its condensed state [20,142–149]. In order to bring structure to the gas, astrophysics must depend on the action of magnetic fields. However, strong magnetic fields themselves are a property of condensed matter, not gases [141]. In order to maintain a gaseous Sun and impart it with structures, astrophysics must therefore have recourse to phenomena best produced by condensed matter.

A simple illustration of these issues can be focused on the understanding of solar prominences. Such objects appear as sheet-like structures in images captured by NASA's SOHO satellite (see Figure 2). In a Sun built from layered metallic hydrogen, it can be envisioned that a layer of material simply peeled away from the surface to form a prominence. In contrast, within a gaseous body, the creation of such overwhelming structures would remain difficult to explain, even with magnetic fields forming and maintaining these entities. Perhaps it would be more logical to presume that magnetic fields were simply associated with the presence of metallic hydrogen, whether on the surface of the Sun itself or within the prominences.

Moreover, the active photosphere and chromosphere supports structural features [154]. Prominences contain fine structure [155, 156], which would be easier to explain if a condensed solar model was adopted. For more than one century [157, p. 104], prominences have been known to emit continuous spectra in addition to the line spectra which characterize the quiescent state [158–161]. Eilnar Tandberg-Hanssen has long studied prominences and has provided an excellent review of the subject matter [160]. Like other solar physicists, because the Sun was considered as a gas, he viewed prominences as gaseous in nature [160]. Tandberg-Hanssen maintained that the continuous spectrum associated with some quiescent prominences was being generated by the scattering of light emitted from the photosphere [161]. This was because gaseous prominences could have no means of generating continuous spectra by themselves. They should have produced only line spectra. Conversely, if the Sun was made from condensed metallic hydrogen, the prominences could directly produce the continuous spectrum. No scattering would need to be invoked. If the density of the prominence material in some cases could not sustain a continuous spectrum, then only line spectra would be generated. Thus, as the prominence dissipated with time, it would be expected that the continuous spectrum might weaken or become absent. It is possible to consider that prominences are formed by layered metallic hydrogen separating from the inferior levels of the photosphere. A slight change in density could account for such actions reflecting an abrupt transformation from a more compact lattice to a less dense form. This hypothesis might explain why entire sheets of material appear to be ejected, some-

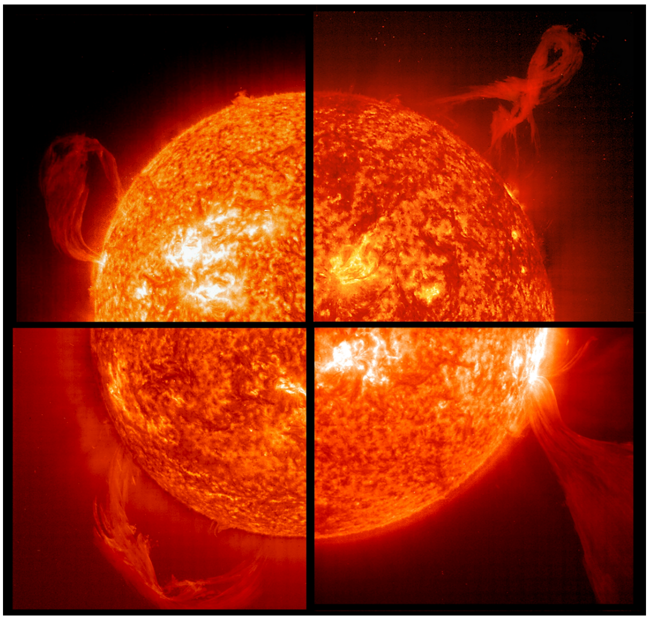

Fig. 2: Sheet like appearance of solar prominences. NASA describes the image as follows: "*A collage of prominences, which are huge clouds of relatively cool dense plasma suspended in the Sun's hot, thin corona. At times, they can erupt, escaping the Sun's atmosphere. For all four images, emission in this spectral line of EIT 304Å shows the upper chromosphere at a temperature of about 60,000 degrees K. The hottest areas appear almost white, while the darker red areas indicate cooler temperatures. Going clockwise from the upper left, the images are from: 15 May 2001; 28 March 2000; 18 January 2000, and 2 February 2001*". Courtesy of SOHO/[Extreme ultraviolet Imaging Telescope (EIT)] consortium. SOHO is a project of international cooperation between ESA and NASA. http://sohowww.nascom.nasa.gov/gallery/images/promquad.html (accessed May 31, 2011).

thing which would be difficult to understand otherwise.

It is possible, one further observation worth pondering involves a figure presented by Fortov in his new text [103]. The figure in question (Figure 7.7 in [103]) consists of a plot of the log of object diameter versus the log of mass. On such a plot, a straight line passes through all astrophysical objects within our solar system, from the smallest comic dust, to the meteorites, to the comets, to the asteroids, to the satellites of planets, to the planets, and finally to the Sun [103, p. 192]. This plot provides another line of evidence that the Sun should be viewed as condensed matter. Every object on the graph can be considered as condensed. Uranus and Neptune are currently viewed as having metallosilicate cores and mantles of ices [103, p. 193]. Jupiter and Saturn are largely liquid metallic hydrogen or helium in either molecular or atomic form [103, p. 193]. As the only remaining fully gaseous object in the solar system, it may be reasonable to suggest that the Sun should not stand alone on such a graph.

4 Conclusion

Relative to the Sun, a condensed approach brings interesting contrasts and dilemmas versus the gaseous models. The latter are endowed with tremendous mathematical flexibility [1, 2], but their physical relevance appears limited. Gases cannot by themselves impart structure and the solar spectrum is not easily explained in a gaseous framework [9]. The gaseous stars suffer from the stellar opacity problem [9]. Conversely, a liquid metallic hydrogen model imparts a wonderful ability to explain the origin of the solar spectrum relying on the layered structure held in common with graphite [141–149]. Metallic hydrogen possesses a very high critical temperature and can exist as condensed matter even on the solar surface accounting for many features of the Sun best characterized by material endowed with a lattice [141]. Most of the physical attributes of the Sun are more simply explained within the framework of a liquid model [141]. However, a condensed Sun is not as open to theoretical formulations. The advantages of a liquid Sun are now so numerous [20, 141–149] that it is difficult to conceive why the model was not proposed long ago. This speaks to the allure of the gaseous Sun and the mathematical beauty of the associated equations of state.

In closing, it should be highlighted that there is currently an effort to describe the Sun as "liquid-like" (e.g. [162]). In the end, the author believes that such terminology should be avoided. If the Sun is condensed, it should be viewed as liquid, not "liquid-like". Even gases could be "liquid-like". Such terms cannot be sufficient, since a real lattice is required for production of the solar thermal spectrum. No compromise can be made on this point for those who have studied thermal emission in real materials. "Liquid-like" might refer to anything from a gas, to a plasma, to fully degenerate matter, to supercritical fluid and none are necessarily endowed with a lattice. The contention of this work remains that the photosphere of the Sun is liquid, with *true lattice structure and ordered interatomic distances*. The adoption of liquid metallic hydrogen as a solar constituent brings with it a wealth of possibilities in describing solar structures and understanding the solar spectrum. Central to this advancement, the lattice must remain the foremost element in all of condensed matter, whether here on Earth, within the Sun, and even, in the firmament of the stars.

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Luc Robitaille is acknowledged for producing a rendition of graphite's layered lattice.

Dedication

This work is dedicated to my son, Christophe, and his wife, Lindsey.

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On the Presence of a Distinct Solar Surface: A Reply to Hervé Faye

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In this exposition, the existence of the solar surface will be briefly explored. Within the context of modern solar theory, the Sun cannot have a distinct surface. Gases are incapable of supporting such structures. The loss of a defined solar surface occurred in 1865 and can be directly attributed to Hervé Faye (Faye H. Sur la constitution physique du soleil. *Les Mondes*, 1865, v.7, 293–306). Modern theory has echoed Faye affirming the absence of this vital structural element. Conversely, experimental evidence firmly supports that the Sun does indeed possess a surface. For nearly 150 years, astronomy has chosen to disregard direct observational evidence in favor of theoretical models.

Herbert Spencer was the first to advance that the body of the Sun was gaseous [1], but he believed, much like Gustav Kirchhoff [2], that the photosphere was liquid [3, 4]. For his part, Father Angelo Secchi [5, 6] promoted the idea that the Sun was a gaseous body with solid or liquid particulate matter floating within its photosphere. Soon after Father Secchi's second Italian paper [6] was translated into French by l'Abbé François Moigno, Harvé Faye made claims of independent and simultaneous discovery [3, 7, 8].

Harvé Faye almost immediately published his own work in *Les Mondes* [9]. In this communication, he deprived the Sun of its distinct surface. He based the loss of a solar surface on the gaseous nature of the interior and the associated convection currents. The salient sections of Faye's classic 1865 article stated: "*So then the exterior surface of the Sun, which from far appears so perfectly spherical, is no longer a layered surface in the mathematical sense of the word. The surfaces, rigorously made up of layers, correspond to a state of equilibrium that does not exist in the Sun, since the ascending and descending currents reign there perpetually from the interior to the superficial area; but since these currents only act in the vertical direction, the equilibrium is also not troubled in that sense, that is to say, perpendicularly to the leveled layers that would form if the currents came to cease. If, therefore, the mass was not animated by a movement of rotation, (for now we will make of it an abstraction), there would not be at its heart any lateral movement, no transfer of matter in the perpendicular direction of the rays. The exterior surface of the photosphere being the limit that will attain the ascending currents which carry the phenomenon of incandescence in the superior layers, a very-admissible symmetry suffices in a globe where the most complete homogeneity must have freely established itself, to give to this limit surface the shape of a sphere, but a sphere that is incredibly uneven*" [9].

In the same article, Hervé Faye emphasized that the photospheric surface was illusionary: "*This limit is in any case only apparent: the general milieu where the photosphere is incessantly forming surpasses without doubt, more or less, the highest crests or summits of the incandescent clouds, but we do not know the effective limit; the only thing that one is permitted to affirm, is that these invisible layers, to which the name atmosphere does not seem to me applicable, would not be able to attain a height of 3′, the excess of the perihelion distance of the great comet of 1843 on the radius of the photosphere*" [9]. Though astronomy has denied the existence of a distinct solar surface as a question of utmost complexity involving opacity arguments [10], the conjecture was actually proposed by Faye in 1865 within a framework of questionable value [9]. Hervé Faye's contributions to solar theory have been extensively addressed [3] and many, like his famous *Les Mondes* communication [9], were not supported by mathematics. Early solar theory rested on vague hypotheses.

It was only much later that Faye's ideas would gain the support of mathematical formulation. In 1891, August Schmidt of Stuttgart wrote a small pamphlet which solidified Faye's conjectures [11]. Within two years, Schmidt received the support of Knoft and, in 1895, Wilczynski published a detailed summary of their ideas in the *Astrophysical Journal* [11]. The illusionary nature of the solar surface was finally supported by mathematics. James Keeler was the first to voice an objection to Schmidt's theory, responding immediately to Wilczynski's article [12]: "*But however difficult it may be for present theories to account for the tenuity of the solar atmosphere immediately above the photosphere, and however readily the same fact may be accounted for by the theory of Schmidt, it is certain that the observer who has studied the structure of the Sun's surface, and particularly the aspect of the spots and other markings as they approach the limb, must feel convinced that these forms actually occur at practically the same level, that is, that the photosphere is an actual and not an optical surface. Hence it is, no doubt, that the theory is apt to be more favorably regarded by mathematicians than by observers*" [12]. Twenty years after Schmidt proposed his ideas, they had still not gained the support of observational astronomers such as Charles Abbot, the director of the Smithsonian Observatory: "*Schmidt's views have obtained considerable acceptance, but not from observers of solar phenomena*" [13, p. 232].

In 1896, Edwin B. Frost [14] discussed Wilson's theory [15] in which sunspots represented depressions on the solar photosphere [3]. He maintained that the theory was not yet well established and required further investigation. Nonetheless, the highlight of his paper would be a comment relative to the existence of a true solar surface. Frost's work [14] formed an appropriate reminder that the presence of the solar surface had been long denied by those who, by advocating gaseous solar models, must reject solar structure as mere illusion: "*In speaking of levels we must proceed from some accepted plane of reference; and the most natural plane, or surface of reference, would be the solar photosphere. Here we are abruptly confronted by the theory of Schmidt, elaborated in a convenient form by Knoft, according to which the photosphere is merely an optical illusion, produced by circular refraction in the Sun itself, supposed to be a globe of glowing gas without a condensed stratum. Prominences, faculae, spots, and granulation are explained as effects of anomalous refractions due to local changes of density somewhere in the gas ball. This theory, worked out as it is by careful mathematical reasoning, deserves and has received respectful consideration. Nevertheless, in view of the physical improbability of Schmidt's primary assumption that in its outer portions the gaseous mass maintains its state without condensation, the physicist will feel obliged to reject the theory, which also suffers from the fundamental defect of failing to account for the solar spectrum on the accepted principles of physics. Moreover, any one who has with some continuousness studied the phenomena of the solar surface must affirm that he has observed realities, not illusions. The perspective effects on prominences as they pass around the limb, the motion and permanence of the spots, the displacements of the spectral lines on the approaching and receding limbs, and in fact all the phenomena concerned with solar rotation, are distinctly contradictory to Schmidt's theory. In dismissing it from further consideration, however, we shall take with us the important inference that refraction within and on the Sun itself may modify in some considerable degree the phenomena we observed*" [14].

Though Faye and Schmidt denied the presence of a distinct surface on the Sun, it was clear that observational astronomers were not all in agreement. The point was also made in 1913 by Edward Walter Maunder, the great solar physicist: "*But under ordinary conditions, we do not see the chromosphere itself, but look down through it on the photosphere, or general radiating surface. This, to the eye, certainly looks like a definite shell, but some theorists have been so impressed with the difficulty of conceiving that a gaseous body like the Sun could, under the conditions of such stupendous temperatures as there exist, have any defined limit at all, that they deny that what we see on the Sun is a real boundary, and argue that it only appears so to us through the effects of the anomalous refraction or dispersion of light. Such theories introduce difficulties greater and more numerous than those that they clear away, and they are not generally accepted by the practical observers of the Sun. They seem incompatible with the apparent structure of the photosphere, which is everywhere made up of a complicated mottling: minute grains somewhat resembling those of rice in shape, of intense brightness, and irregularly scattered. This mottling is sometimes coarsely, sometimes finely textured; in some regions it is sharp and well defined, in others misty or blurred, and in both cases they are often arranged in large elaborate patterns, the figures of the pattern sometimes extending for a hundred thousand miles or more in any direction. The rice like grains or granules of which these figures are built up, and the darker pores between them, are, on the other hand, comparatively small, and do not, on the average, exceed two to four hundred miles in diameter*" [16, p. 28].

That same year, Alfred Fowler [17] the British spectroscopist who trained as Lockyer's assistant, commented on problems in astronomy [18]. Fowler served as the first secretary of the International Astronomical Union [17]. Fowler's writings reflected that the ideas of Hervé Faye [9] and August Schmidt [11] continued to impact astronomy beyond 1913 [3, 4], even though observational astronomers were not convinced: "*The apparently definite bounding-surface of the Sun which is ordinarily revealed to the naked eye, or seen in the telescope, has such an appearance of reality that its existence has been taken for granted in most of the attempts which have been made to interpret solar phenomena… Thus the photosphere is usually regarded as a stratum of cirrus or cumulus clouds, consisting of small solid or liquid particles, radiating light and heat in virtue of their state of incandescence… An effort to escape from this difficulty was made in the view suggested by Johnstone Stoney, and vigorously advocated by Sir Robert Ball, that the photospheric particles consist of highly refractory substances carbon and silicon (with a preference for carbon), both of which are known to exist on the Sun...The photosphere is thus regarded as an optical illusion, and remarkable consequences in relation to spots and other phenomena are involved. The hypothesis appears to take no account of absorption, and, while of a certain mathematical interest, it seems to have but little application to the actual Sun*" [18]. It was well known that Johnstone Stoney [19] advocated that the solar photosphere contained carbon particles [4].

Even in the 21st century, astronomy has maintained that the Sun's surface is an illusion. For instance, in 2003, the National Solar Observatory claimed that "*The density decreases with distance from the surface until light at last can travel freely and thus gives the illusion of a 'visible surface'*" [20].

Nonetheless, spectacular images of the solar surface have been acquired in recent years, all of which manifest phenomenal structural elements on or near the solar surface. High resolution images acquired by the Swedish Solar Telescope [20–23] reveal a solar surface in three dimensions filled with structural elements. Figure 1 displays an image which is publicly available for reproduction obtained by the Swedish So-

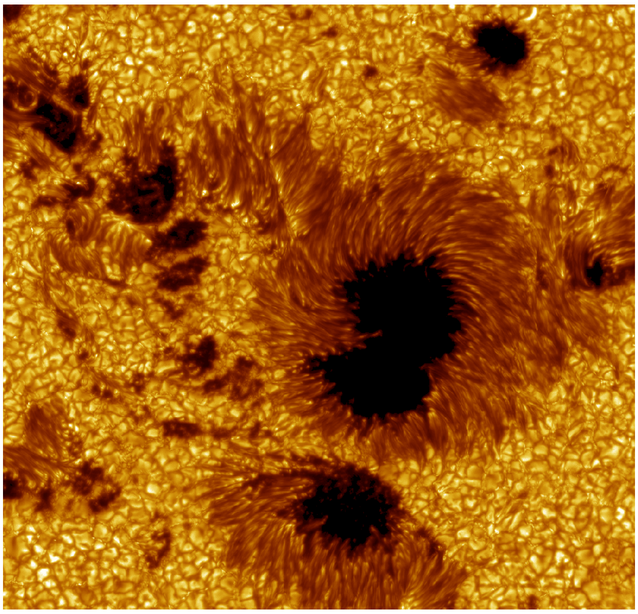

Fig. 1: Part of a sunspot group near the disk center acquired with the Swedish 1-m Solar Telescope by Göran B. Scharmer, Boris V. Gudiksen, Dan Kiselman, Mats G. Löfdahl, and Luc H. M. Rouppe van der Voort [21]. The image has been described as follows by the Institute for Solar Research of the Royal Swedish Academy of Sciences: “*Large field-of-view image of sunspots in Active Region 10030 observed on 15 July 2002. The image has been colored yellow for aesthetic reasons*” http://www.solarphysics.kva.se

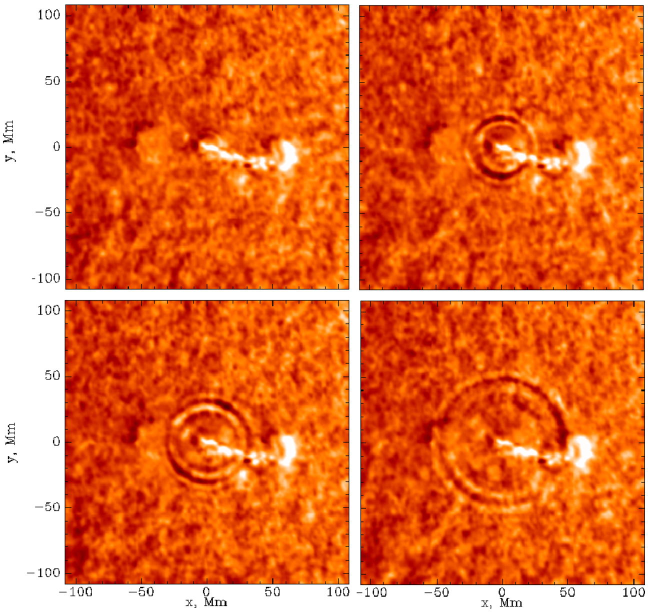

Fig. 2: Doppler image of a solar flare and the associated disturbance on the solar surface acquired by the NASA/ESA SOHO satellite. Such data was described as “*resembling ripples from a pebble, thrown into a pond*” [25]. Courtesy of SOHO/[Michelson Doppler Imager] consortium. SOHO is a project of international cooperation between ESA and NASA.

lar Telescope of the Royal Swedish Academy of Science. The author has previously commented on these results: “*The solar surface has recently been imaged in high resolution using the Swedish Solar Telescope [24, 25]. These images reveal a clear solar surface in 3D with valleys, canyons, and walls. Relative to these findings, the authors insist that a true surface is not being seen. Such statements are prompted by belief in the gaseous models of the Sun. The gaseous models cannot provide an adequate means for generating a real surface. Solar opacity arguments are advanced to caution the reader against interpretation that a real surface is being imaged. Nonetheless, a real surface is required by the liquid model. It appears that a real surface is being seen. Only our theoretical arguments seem to support our disbelief that a surface is present*” [24]. References [24] and [25] in the quotation referred to [21, 22] in the current work. A study of Lites et al. [23] illustrates how these authors hesitated to regard the solar surface as real, precisely because they considered that the Sun was gaseous in nature: “*However, since the angular resolution of the SST* [Swedish Solar Telescope] *is comparable to the optical scale of the photosphere (about one scale height), we may no longer regard the photospheric surface as a discontinuity; optical depth effects must be considered*” [23]. Though the authors reported three-dimensional structure, they added quotation marks around the word “surface” precisely because a gaseous Sun cannot support such a feature. They referred to the “optical depth unit surface”, a concept inherently tied to gaseous models of the Sun. At the same time, the authors displayed a qualified desire for condensed matter: “*This gives the (perhaps false) visual impression of a solid surface of granules that protrude up a considerable distance from the surface, and that a raised structure is “illuminated” by a light source in the vicinity of the observer*” [23].

Beyond the evidence provided by the Swedish Solar Telescope and countless other observations, there was clear Doppler confirmation that the photosphere of the Sun was *behaving* as a distinct surface [25, 26]. In 1998, Kosovichev and Zharkova published their *Nature* paper *X-ray flare sparks quake inside the Sun* [25]. Doppler imaging revealed transverse waves on the surface of the Sun, as reproduced in Figure 2: “*We have also detected flare ripples, circular wave packets propagating from the flare and resembling ripples from a pebble, thrown into a pond*” [25]. In these images, the “optical illusion” *was now acting as a real surface*. The ripples were clearly transverse in nature, a phenomenon difficult to explain using a gaseous solar model. Ripples on a pond are characteristic of the liquid or solid state.

Hervé Faye’s contention that the Sun was devoid of a real surface has never been supported by observational evidence; the solar surface has long ago been established. Though theory may hypothesize a gaseous Sun, it must nevertheless support observational findings. Perhaps, now that a reasonable alternative to a gaseous Sun has be formulated [27], astrophysics will discard the idea that the solar surface is an illusion, embrace the liquid nature of the Sun, and move to better comprehend this physical reality.

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Dedication

This work is dedicated to my eldest son, Jacob.

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On Solar Granulations, Limb Darkening, and Sunspots: Brief Insights in Remembrance of Father Angelo Secchi

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Father Angelo Secchi used the existence of solar granulation as a central line of reasoning when he advanced that the Sun was a gaseous body with a photosphere containing incandescent particulate matter (Secchi A. Sulla Struttura della Fotosfera Solare. *Bullettino Meteorologico dell'Osservatorio del Collegio Romano*, 30 November 1864, v.3(11), 1–3). Secchi saw the granules as condensed matter emitting the photospheric spectrum, while the darkened intergranular lanes conveyed the presence of a gaseous solar interior. Secchi also considered the nature of sunspots and limb darkening. In the context of modern solar models, opacity arguments currently account for the emissive properties of the photosphere. Optical depth is thought to explain limb darkening. Both temperature variations and magnetic fields are invoked to justify the weakened emissivities of sunspots, even though the presence of static magnetic fields in materials is not usually associated with modified emissivity. Conversely, within the context of a liquid metallic hydrogen solar model, the appearance of granules, limb darkening, and sunspots can be elegantly understood through the varying directional emissivity of condensed matter. A single explanation is applicable to all three phenomena. Granular contrast can be directly associated with the generation of limb darkening. Depending on size, granules can be analyzed by considering Kolmogoroff's formulations and Bénard convection, respectively, both of which were observed using incompressible liquids, not gases. Granules follow the 2-dimensional space filling laws of Aboav-Weiner and Lewis. Their adherence to these structural laws provides supportive evidence that the granular surface of the Sun represents elements which can only be constructed from condensed matter. A gaseous Sun cannot be confined to a 2-dimensional framework. Mesogranules, supergranules, and giant cells constitute additional entities which further support the idea of a condensed Sun. With respect to sunspots, the decrease in emissivity with increasing magnetic field strength lends powerful observational support to the idea that these structures are comprised of liquid metallic hydrogen. In this model, the inter-atomic lattice dimensions within sunspots are reduced. This increases the density and metallic character relative to photospheric material, while at the same time decreasing emissivity. Metals are well known to have lowered directional emissivities with respect to non-metals. Greater metallicity produces lower emissivity. The idea that density is increased within sunspots is supported by helioseismology. Thus, a liquid metallic hydrogen model brings with it many advantages in understanding both the emissivity of the solar surface and its vast array of structures. These realities reveal that Father Secchi, like Herbert Spencer and Gustav Kirchhoff, was correct in his insistence that condensed matter is present on the photosphere. Secchi and his contemporaries were well aware that gases are unable to impart the observed structure.

1 Introduction

The appearance of sunspots has fascinated mankind for centuries [1–8] and while limb darkening [9–11] has been documented from the days of Galileo [3, p.274], the phenomenon only became well-established in the 1800's [7, 12]. Solar granulations have also long captivated solar science [13, 14]. Although humanity has gazed at the Sun since time immemorial, our understanding of these phenomena remains limited. In a large measure, this reflects the unassailable nature of the Sun. At the same time, our lack of understanding mirrors the incapacity of the gaseous models to properly address questions related to solar structure. Gases will always remain devoid of structural attributes.

Strangely, if Father Angelo Secchi [2] first advanced that the Sun was constituted of a gaseous body surrounded by a photosphere containing particulate matter [16, 17], it was because he was searching to understand photospheric structure. The nature of solar granulations troubled Secchi [2, 17]. He solved the problem by endowing the body of the Sun with a gaseous nature while maintaining a partially condensed photosphere. Secchi's proposed photosphere could not adhere to the full properties of condensed matter. Sixty years later, theoretical physics advocated a completely gaseous solar model.

As a result, it has been nearly impossible to synthesize a realistic and cohesive portrayal of sunspots, granulation, and limb darkening, even though a cursory review of the question suggests otherwise.

2 Granulations and the gaseous models

2.1 Ideas of the 19th century

Secchi built his solar model on two driving forces: 1) Nasmyth's early description of solar granulation [18, 19] and 2) Magnus' demonstration that solid sodium hydroxide increased the luminosity of the gaseous flame [20]. Based on Magnus [20], Secchi advanced [17] that some condensed matter was present within the photosphere, as gases were devoid of the emissive power required to produce the solar spectrum [2]. Secchi considered that the darker appearance of intergranular lanes reflected the inferior radiative ability of the gaseous solar body. He believed that Nasmyth's discovery was noteworthy [18, 19], though remarking that granular features had previously been observed on the solar surface: "*First of all, are these new findings? We believe that, in the end, these are the same granulations that have long since been pointed out by observers, under the name of "lucules" and "pores" and that with the new method they can better be distinguished*" [17]. Secchi's description of granulation was important to the history of astronomy, as the Jesuit scientist was regarded as one of the leading solar observers of his time [2, 21]. His representations of granules depicted in his classic text [21, p.31–34] (reproduced in part within [14, p.4] and [1, p.143–145]) were nothing short of astounding. In 1870, Secchi presented drawings which remain respectable by today's standards and which far surpassed the illustrations which had made James Nasmyth famous only a few years before (see drawings reproduced in [13]).

In the mid-1860s, considerable controversy erupted between James Nasmyth [22] and the Reverend William Rutter Dawes [23] over the appearance of the solar granulation [13]. Nasmyth supported the notion that granules had a consistent structure and resembled regular overlapping "*willow leaves*". For his part, Dawes maintained that they had been discovered long before Nasmyth and that the term "*willow leaves*" was inappropriate as the features displayed an irregular form [13]. The discussion then involved George Airy as the Astronomer Royal, Warren de la Rue, John Herschel, William Huggins, Father Angelo Secchi, and others [13]. Much of the debate would once again transpire in *The Reader* [2]. In 1865, no less than ten letters appeared in the popular magazine and included contributions from Secchi himself [24–33]. Scientists took the controversy beyond conventional journals into the public forum.

With time, Dawes' view [13, 30] rose to prominence and the concept of "*willow leaves*" faded from solar physics. With respect to granulations, Dawes reminded his readers that: "*Their existence was well known to Sir W. Herschel*" [30]. He cited Herschel directly [30]: "*There is all over the Sun a great unevenness in the surface which has the appearance of a mixture of small points of an unequal light*" [34]. Dawes elaborated on his own position: "*I have proposed to term them granules or granulations, as more suitable than any more definite appellation, and therefore unlikely to mislead*" [30]. Nasmyth discovered nothing new [13, 18, 19], but he generated tremendous interest in the nature of solar granules. In turn, this prompted Secchi to put forth his solar model [16, 17]. Dawes did not live to see the resolution of the conflict.

As for Secchi, he observed both the granules and the intergranular lanes. He addressed the appearance of the solar surface as follows: "*The bottom of the solar disc appeared to be formed of a fine black mesh whose links were very thin and full of bright points. It was not so much the shape of the grid that surprised us — for we had seen it also at other times with older methods — as its blackness, which was truly extraordinary. It was such that we suspected some illusion, but in concentrating on certain darker points and finding them of unchanging and precise forms, we no longer remained in doubt about the reality of the aspect. Of this grid-like structure we can give an approximate idea in saying that the Sun looked like a ordinary piece of rough paper seen through a strong microscope; on this paper the prominences are numerous and irregular, and where the light falls rather obliquely, the bottom of the grooves are almost black compared to the more elevated parts, which appear extremely white... The grid-like solar structure seemed to us to offer nothing regular in those parts of the disc that are continuous, and thus the term granular appears very appropriate. The granular structure is more visible near the spots, but it is not recognizable in the faculae; these present themselves like luminous clusters without distinguishable separation, emitting continual light without the interruption of dots or of that black mesh. In the end, we have found the granular structure more notable and easy to distinguish in the middle of the disc than near the limb, and in the zones near the sun's equator, more than in the polar zones*" [17].

It was based on these observations that Secchi advanced his model of a gaseous Sun with a partially condensed photosphere: "*Indeed this appearance suggests to us what is perhaps a bold hypothesis. As in our atmosphere, when it is cooled to a certain point, there exists a fine substance capable of transforming itself in fine powder and of forming clouds in suspension, (water transforming into so-called "vesicular" vapor or into small solid icicles), so in the enflamed solar atmosphere there might be an abundance of matter capable of being transformed to a similar state at the highest temperatures. These corpuscles, in immense supply, would form an almost continuous layer of real clouds, suspended in the transparent atmosphere which envelopes the sun, and being comparable to solid bodies suspended in a gas, they might have a greater radiant force of calorific and luminous rays than the gas in which they are suspended. We may thus ex-*

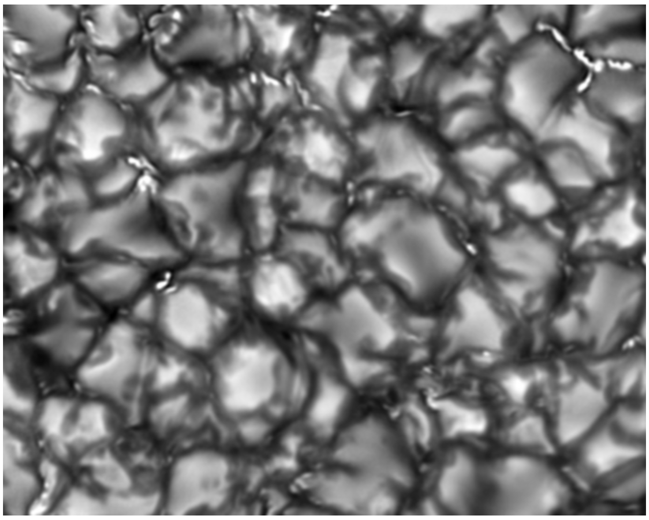

Fig. 1: High resolution image of solar granules acquired by Vasco Henriques on May 23, 2010 using the Swedish 1-m Solar Telescope (SST). "*The SST is operated on the island of La Palma by the Institute for Solar Physics of the Royal Swedish Academy of Sciences in the Spanish Observatorio del Roque de los Muchachos of the Instituto de Astrofísica de Canarias*", http://www.solarphysics.kva.se.

plain why the spots (that are places where these clouds are torn) show less light and less heat, even if the temperature is the same. The excellent results obtained by Magnus, who has proved that a solid immersed in an incandescent gas becomes more radiant in heat and light than the same gas, seem to lend support to this hypothesis, which reconciles the rest of the known solar phenomena" [17]. With Secchi's words, others quickly followed suit [2, 25] and the Sun became viewed as having a gaseous body [2]. Such was the authority of Father Angelo Secchi in astronomy.

Objects which appeared as "*rice grains*" or "*willow leaves*" on the Sun's surface offered a rather poor foundation for scientific advancement. Chacornac would distance himself from these concepts: "*As to the form of the objects observed a subject so warmly discussed at the present time — I did not see, with the large instrument of the Paris Observatory, nor have I ever yet seen, that the form is limited to one only, either "willow leaf" or "rice grain". I have always seen the "crystals" of the photospheric atmosphere entangled (enchevêtrés) in a thousand ways, and connected among themselves by one or many points in their peripheries; I have always observed these photospheric clouds affecting forms reminding one of the flocculent mass in an incandescent metal, in suspension in a liquid... I have always in my descriptions compared the "crystals" of the photospheric matter to this silver solder in a state of fusion*" [25]. With these words, Chacornac became one of the first to invoke crystalline structure on the surface of the Sun. In the same letter [25], he echoed Secchi's model published in *Les Mondes* [17] three days prior, without properly referencing Secchi: "*... they constitute one of the essential conditions of the nature of this luminous matter, of which the elements are contained in the exterior atmosphere of the Sun as vapour is contained in our air*" [25]. Chacornac's description of the crystalline structure of granules would be revisited using theoretical analysis, more than 130 years later [35].

Scientists of the 19th century advocated that convection currents were the cause of granular formation. Gaseous material rose from deep within the Sun and then condensed on the photospheric surface before sinking once again in the gaseous atmosphere back towards the interior. The modern gaseous models promote similar hypotheses, but do not permit the condensation of matter. In 1881, Hastings described granules as follows: "*In our theory, then, the granules are those portions of upward currents where precipitation is most active, while the darker portions, between the bodies, are where the cooler products of this change with accompanying vapors are sinking to lower levels*" [36]. The convective nature of the granular field was well recognized, even though solar physicists lacked the mathematical tools required to address such problems.

2.2 Modern concepts of granules

The careful analysis of the solar granulation is important, as such studies reveal that the photosphere possesses objects with defined structures. The presence of such features provides compelling evidence that the Sun is constituted from condensed matter. Today, the study of solar granulation involves sophisticated image acquisition (see Figure 1) and data processing [14, 15, 35, 37–51]. Granules are widely regarded as the result of convective phenomena, wherein subsurface heat is being transported to the solar surface [14, 15, 37, 44, 50]. Convective processes move material upwards within the granule. Following radiative cooling, matter then sinks into the intergranule lanes [43]. The velocities of up and down flows can reach 1200 m/s in granular centers and intergranular lanes [43]. According to the gaseous models of the Sun, once the material reaches the surface layer, radiative heat losses result in greatly lowered opacity and the atmosphere of the Sun becomes transparent [37].

Granules vary in size from ~0.3–4 arcsec with most having a rough diameter of 1–2 arcsec giving a mean of ~1.35 arcsec (~1,000 km) [14, 37, 38]. Del Moro finds that no granule has an area larger than 1 Mm^2 [48]. Other investigators obtain maximal values in the 3-5 Mm^2 range [38, 45]. Small granules are very numerous, but they do not account for much of the solar surface [38]. They tend to be concentrated in downdraft regions, whereas the larger granules are located in areas of strong up currents [45]. The intergranular distance is on the order of 1.76 arcsec [38] and by some measures the darker intergranular lanes account for about 32% of the solar surface [42]. Conversely, Abdussamatov and Zlatopol'skii report that on a mesogranular scale (see below) the intergranular lanes can occupy as much as 55% of the photospheric area [44]. Roudier and Muller provide an excellent review of many key facts relative to granules [38]. The structures tend to be irregular in shape, although they can be properly described as polygons with a slight prevalence of pentagons over hexagons [35].

If the log of the number of granules of a given size is plotted against the log of their area, two distinct lines can be used to fit all granules with a critical diameter of 1.31 arcsec (see Figure 7 in [38]). This suggest that "*granules are self-similar*" [15, 38, 45] which then implies structure. Smaller granules fit the first line and are thought to be produced by turbulent phenomena of a "*Kolmogorov-type*" [38]. Because they are believed to be the result of turbulent eddy motions, Roudier and Muller argue that these small structures should be viewed as "*photospheric turbulent elements*" [38], an idea consistent with their more prevalent occurrence in the downdraft regions [44]. Conversely, they state that only medium and larger structures should be viewed as true "*granules*" as these alone properly transport convective energy [38].

Mean granular lifetimes range from ~5 minutes to 16 minutes with a maximum of approximately 30 minutes [14, 46]. Granules are subject to three evolutionary mechanisms. Most often, they are produced through the fragmentation of larger systems [39, 40, 46, 49]. They often "*die*" through the merger of smaller entities [46]. They seldom appear from, but frequently dissolve into, the background [46]. The larger granules tend to have the largest lifetimes [48]. Granules that are "*long lived*" have a tendency to form clusters [49]. Dark dots often form within granules and these result in violent fragmentation of the structure producing "*exploding granules*" [39, 47, 51]. The formation of these dark dots results in fragmentation within a couple of minutes and the features have no link to magnetic fields [39,47]. Only very large granules explode [48]. Exploding granules are often very bright, initially suggesting the upward flow of matter followed by great expansion [39]. Their dark dots eventually evolve into intergranular dark regions which are indicative of downward flow even though some have argued, using opacity arguments, that dark dots represent upward material displacement [40].

Mark Rast proposed that exploding granules "*can be better understood if granulation is viewed as downflow-dominated-surface-driven convection rather than as a collection of more deeply driven upflowing thermal plumes*" [51]. Though not mentioned by Rast, such an idea would benefit from the presence of a real solar surface which only a condensed model of the Sun could provide [52].

The smaller the granule, the more likely it is to die without fragmentation or merging [40]. Conversely, if the granule is large, it is likely to merge or fragment [40]. The brightest region and the strongest upflows within large granules tend to be near the intergranular lanes and consequently are not located near the center of the structure [53]. A family of granules shares either fragmentation or merging and can have a lifetime approaching 46 minutes [40].

Granules can be organized into larger assemblies: mesogranulation, supergranulation, and giant cells [41–45]. Such assemblies share common and simultaneous changes in size, temperature, or other parameters [43]. Mesogranulation areas usually tend to be brighter, more dynamically active [42]. They are thought to represent a greater uplifting of matter and can span from 6–9 arcsec [43] and have lifetimes ranging from 30 minutes to 6 hours [48]. They are viewed as connected to common convective origins located at depths of 3,000–8,000 km [43]. Supergranular cells are believed to have their origins at depths of 20,000–30,000 km, while giant cells might stem from convective processes located as deep as 200,000 km below the surface [43]. These hypothetical depths are inherently linked to the gaseous models of the Sun.

Giant cells divide successively into supergranular and mesogranular structures [43]. However, Rast believes that mesogranulation and supergranulations are "*secondary manifestations of granulation itself*" [51]. He provides an excellent review of the solar granulation and these structures [53]. Granules tend to have limited vertical flows on the order of 1 km/s while the mesogranulation with their ~5,000–10,000 km diameters, can have flows approaching 60 km/s [53, 54]. Ikhsanov et al. suggest that the solar surface supports protogranules which are intermediate in size between granules and mesogranules [54]. Supergranulations possess diameters of ~30,000 km, display a 20 hour lifetime, and can manifest horizontal flows on the order of 400 km/s [53]. Such horizontal flows are contrary to a fully gaseous model of the Sun, as highlighted by the author (see §10 in [55]). Recently, Arkhypov et al. have found that Kolmogorov turbulence determines large scale surface activity on the photosphere [56] and claim these indicate that sub-surface convection motion can be detected through photospheric activity of supergiant complexes.

Granules display varying emissivities, but most studies simply report values for the granules and the intergranular lanes (e.g. [44] reports +8±7.5% for granules and −7±5.5% for the intergranular lanes). These descriptions appear to be over simplified as a smooth transition exists between the maximum brightness of a granule and the darkest point of the intergranular lane. As a result, considerable variability can be expected in such values.

Center to limb variations in granular intensities have also been investigated [57, 58]. Initially, Hidalgo et al. reported that granular contrast increased slightly towards the limb up $\mu = 0.6$, followed by a decrease in contrast moving further away from the solar center [57]. It is not clear if this change was due to an increase in brightness. Later, in a wavelength dependent study (0.8 μm and 1.55 μm), Cuberes et al. observed a monotonic decrease in contrast from the center of the solar surface ($\mu = 1$) towards the limb ($\mu = 0.3$) [58]. The change was steeper at the lower wavelength [58]. No peak was observed in contrast variation at either frequency [58]. The contrast at the center of the solar surface was dependent on wavelength, with larger contrast (6.1%) at 0.8 μm, while only 2.9% at 1.55 μm [58].

Title et al. [59, 60] have studied the formation of granules in association with magnetic fields and discovered significant differences relative to size, intensity variation, and lifetime.

Recently, Getling et al. published a series of stunning reports implying that the solar surface possesses a series of ridges and trenches [61–64]. On first inspection, the results appear valid and the authors have gone through considerable lengths to eliminate artifacts [64]. If these findings are genuine, they suggest that the solar surface contains "*quazeregular*" structural systems of great breath and regularity [61–64]. Nonetheless, it is currently unclear if these fascinating results will withstand scrutiny. If so, they would constitute additional support for the condensed nature of the photosphere.

Solar granulations have been the subject of intense theoretical work (e.g. [65]). From the onset [66–68], such studies have been subject to the charge that they can, at times, constitute "*little more than an exercise in parameter fitting*" [67]. Clearly, the gaseous models of state do offer significant flexibility with respect to the number of usable parameters [69]. Given enough variables, fits can almost always be achieved. Nonetheless, this brief review of solar granulation reveals that these elements are filled with structural properties based on size, behavior, and lifetimes. In this regard, it is instructive to consider how solar granulations conform to the laws of convection, turbulence, and structure as obtained in condensed matter (see §3, §4, and §5).

3 Granules and the laboratory

The analysis of granulations as convective processes has always rested on the science of liquids. In 1900, Bénard convection was first observed in the liquid state [70, 71] and the process continues to be a property of condensed matter. Bray et al. re-emphasized that Bénard convection was dominated by surface tension, not buoyancy [14, p.116].

Bénard (or Bénard-Marangoni) convection [72–74] is characterized by hexagonal structures. In fact, such features are properties of both Bénard convection [70–74] and many solar granules [14]. It is difficult to discount the presence of these structural elements on the surface of the Sun as coincidental, even though many solar physicist deny the presence of Bénard convection. Yet, even the laws of Kolmogoroff turbulence are strictly applicable only to an *incompressible* fluid [14, p.14], a framework well-beyond that afforded by the gaseous Sun. Still, since solar physicists currently endorse a gaseous model of the Sun, granular convection has always been viewed as a buoyancy driven phenomenon. Bénard convection cannot occur on the surface of the Sun if a gaseous body is to be preserved. To propose otherwise automatically requires surface tension, an impossibility for gaseous models. Nonetheless, it is particularly troubling that most laboratory experiments used to treat granulation have been performed on incompressible liquids [14, p.116]. To avoid surface tension, experimentalists study incompressible liquids placed between rigid plates [14, p.116]. Such a setting is hardly the equivalent of the hypothetical and illusionary gaseous solar surface.

4 Granulations and crystal structure

Beyond these applications of liquids to the treatment of granular convection, Noever has used the methods of statistical crystallography to analyze the solar surface [35]. He has reported that the granular field displayed a remarkable similarity to crystals [35]. Solar granulation followed both the laws of Aboav-Weaire and of Lewis [75–77] for space filling structures in two dimensions. The agreement with the Aboav-Weaire law had an R value of 0.998, indicating "*a correlation which does not extend beyond the nearest neighbor cells*" [35]. Noever also found that granules followed the perimeter law, suggesting that many sided structures have larger perimeters ($R = 0.987$) [35]. Adherence to the perimeter law implied that "*energy is carried by the cell boundaries*" [35]. Noever stated: "*It is particularly noteworthy that prior to grain fragmentation, a dark region of low luminosity typically appears near this predicted low energy core of each cell. The perimeter law predicts this outcome derived not from any specific fluid parameters but from a statistical picture of lattices alone*" [35]. With these words, Noever accounted for the origin of exploding granules without any recourse to convection, based solely on structural energy considerations. Structure led to behavior and this directly implied that the granulations are condensed matter. Noever further demonstrated that granules obey Lewis' law which relates two dimensional area and cell sidedness ($R = 0.984$) [35]. This places a restriction on granulation based on the need to fill two dimensional space entirely [35]. Gases cannot assume two dimensional space filling forms and cannot follow the laws of structure. Liquids alone can truly account for the convective and structural nature of granules.

Regrettably, Noever's work has been largely neglected [78–81] receiving only one citation relative to solar science [81]. Nonetheless, it represented a critical contribution in the understanding of granulations, precisely because it implied that granules are condensed matter.

5 Emissivity: A common link for solar surface structures

5.1 Metals and sunspots

Non-metals are known to possess directional spectral emissivities which monotonically decrease with increasing angle as illustrated schematically in Fig. 2 [82–84]. Their normal emissivity is typically higher than their directional spectral emissivity. Conversely, metals tend to have lower normal spectral emissivities relative to their directional spectral emissivities. For metals, the directional spectral emissivities usually rise with increasing angle until they fall precipitously as

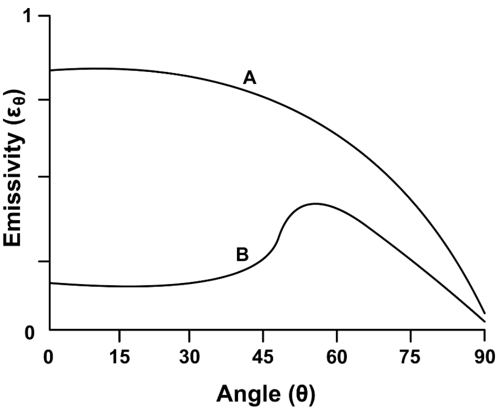

Fig. 2: Schematic representation of directional spectral emissivities for non-conductors (A) and conductors (B). Note that in non-metals, the spectral emissivity decreases monotonically with viewing angle. Conversely, in metals, while the normal emissivity can be substantially reduced, the emissivity can rise with increasing angle before precipitously dropping (adapted from [83]).

orthogonal viewing is approached [82,83]. These simple considerations provide tremendous insight to the structure of the photosphere in the context of a condensed solar model [52].

Consider the liquid metallic hydrogen model of the Sun [52]. When first proposed [85], liquid metallic hydrogen was hypothesized to assume a layered graphite-like structure. This lattice was subsequently adopted for the solar photosphere [52].

Since graphite itself is a great emitter, but only a modest conductor, one can hypothesize that liquid metallic hydrogen on the surface of the Sun is not highly metallic [52]. The inter-atomic distance in the lattice must be such that the photosphere displays little metallic character, but great graphite-like emissivity. This would correspond to the Type-I lattice structure previously discussed by the author [52, 55]. However, within sunspots, the interatomic distance would contract and liquid metallic hydrogen would increase its metallic character while at the same time, lowering its emissivity. In the limit, this would correspond to the Type II lattice [52].

The point can be amplified by examining the emissive behavior of sunspots with respect to magnetic field intensity [86, 87]. Leonard and Choudhary have reported that the emissivity of sunspot umbral regions drops with magnetic field strength suggesting the approach to a saturation limit (see Figure 2 in [86]). They stated: "*Although there is a large scatter, it is tempting to infer that the sunspot umbral intensity attains a maximum value beyond which the magnetic field increases without substantial intensity drop, resulting in a 'saturation effect'*" [86]. While more data of this nature is required, these preliminary findings imply that a limiting structural lattice might be reached within sunspots.

Sunspots are known to have substructure [88] and, as they can be the source of powerful magnetic fields [89], such observations [86] further support the notion that they are metallic in character [52]. The dark nuclei of sunspots clearly have lower emissivities and possess the highest magnetic fields [8, p.80]. Conversely, the light bridges display higher emissivities and lower magnetic fields [8, p.85–86], implying that they are less-metallic in character. The dark cores detected in sunspot penumbral filaments might be a reflection of increased metallicity in these elements [90].

Supportively, helioseismology reveals that sound waves travel much faster through sunspots than through normal photospheric matter [91, 92]. This suggests that the modulus of elasticity is higher within sunspots, in accordance with the hypothesis that the material is both more metallic and slightly denser than photospheric matter.

Consequently, greater attention might be placed on evaluating directional emissivity within sunspots. Measurements from these regions are already giving hints that emissivities may be increasing with angle of visualization. This is reflected in the "*problems of stray light*" into the sunspots [8, p.75–77]. The effect of "*stray light*" acts to increase the observed emissivity of sunspots in precisely the same manner that an increased metallic character would produce (see Eq. 8 in [8, p.75]). As a result, such data may already be affirming the metallic character of sunspots by mimicking the behavior manifested in Fig. 2. "*Stray light*" arguments might have been introduced simply to address a finding which could not be explained otherwise by the gaseous solar models. The observation of large sunspots at high resolution should enable scientists to clearly establish the directional emissivity of sunspots without any "*stray light*" effects and thereby possibly affirm their metallic nature.

It is appropriate to consider that sunspots might represent liquid metallic hydrogen whose lattice density has increased along with a corresponding rise in metallic nature: the stronger the metallic character, the stronger the associated magnetic fields and the weaker the emitted light intensity. This is precisely what one observes in sunspots [86]. Emissivity is strongly dependent on magnetic field intensity. As magnetic field intensity increases, sunspot emissivity progressively falls until a plateau region appears to be reached [86]. This would correspond to the limit of compressibility of the lattice. Beyond this point, liquid metallic hydrogen should become essentially incompressible, the Type II lattice having been reached.

Along these lines, it is interesting to note that liquid graphite displays two lattice forms which differ in spatial dimensions, densities, and metallic character [93]. Liquid graphite [93] appears to provide an interesting parallel with the two structural lattice Types required in a liquid metallic hydrogen model of the Sun [52].

These results can only be explained with difficulty using the gaseous models. After all, the presence of magnetic fields by themselves can have no effect on emissivity. It is well known that a piece of iron does not change its emissivity on becoming magnetized. Emissivity changes demand changes in structure [94] and the gaseous solar models afford none.

5.2 Emissivity, granulation, and limb darkening

Frank Very was the first to monitor the limb darkening of the Sun [12] as a function of frequency. Very examined the photosphere at 7 wavelengths ranging from 0.416 μm to 1.5 μm [12]. He found that limb darkening was much more pronounced at shorter wavelength [12]. Since that time, extensive studies of limb darkening have been performed (e.g. [9–11]). Pierce and his collaborators provided an detailed list of coefficients for polynomial representations of limb darkening spanning a wide range of frequencies [9, 10]. Overall, these functions demonstrated that the photosphere behaves as a non-metal.

Today, limb darkening constitutes a central pillar of the gaseous solar models. The phenomenon remains linked to solar opacity arguments [95]. Nonetheless, when Very first considered the frequency dependence of limb darkening [12], he did not ponder only upon opacity arguments. He questioned whether limb darkening could be explained by the granulated aspect of the solar surface [12].

Solar granules display emissive characteristics which change towards their periphery as the dark intergranule lanes are reached. They also display center to limb variations [57, 58]. In fact, it is likely that the same phenomenon is being observed both locally near the granules and over the expanse of the entire solar surface as the limb is visualized. Granules manifest a brightness which fades in the intergranule lanes in the same manner as darkening manifests itself from the center of the solar body to the limb. As such, higher spatial resolution on granules may soon reveal that they individually exhibit the same features as observed globally in limb darkening. This would be expected if the emissivity of the Sun simply reflected the constitution of its condensed surface. Each individual granule would become a local manifestation of the limb darkening observed over the entire solar disk.

6 Conclusions

From the days of their discovery by William Herschel [34], granules have offered solar science a vast and fascinating array of structural forms which follow specific evolutionary paths and predetermined timelines. By every measure, granules are real entities, not illusions. They obey the laws of two-dimensional structures and manifest themselves as objects which can be analyzed, categorized, and mathematically evaluated. They appear and behave as condensed matter.

Conversely, a gaseous Sun should be devoid of structural elements: sunspots, granules, prominences, and flares which rupture the solar surface. It should be a blob, a haze, a nondescript mass — not a body filled with structure, as Secchi so elegantly described in his classic text [21]. A brief study of granulations and sunspots demonstrates that these are real structures which follow in every manner the behavior of condensed matter. The issues are not only structural, but involve the ability to have variable emissivities and powerful magnetic fields. On the Earth, the generation of strong magnetic fields remains associated with metallic character [55]. Gases can never produce magnetic fields of themselves. They simply respond to such phenomena.

The fact that sunspots possess strong magnetic fields might guide the synthesis of liquid metallic hydrogen on the Earth [52]. If the Sun is really made of liquid metallic hydrogen, then our study of sunspots implies that the material is easily endowed with magnetic properties. Therefore, it is possible that the synthesis of metallic hydrogen on the Earth could benefit by placing the entire experimental setting within a modest magnetic field on the order of 0.5 Tesla. This would correspond to the maximal 5,000–6,000 gauss field observed within sunspots [86, 87]. Large bore human magnetic resonance imaging (MRI) magnets currently operate up to fields of 9.4 Tesla, thereby confirming that suitable magnet technology exists for such studies [96].

At the same time, it is clear that the proper study of granular and sunspot emissivity will require much stronger optical space telescopes devoid of the "*seeing problems*" [1, p.23–25] when visualizing the Sun from the Earth. Resolutions must be increased tremendously such that emissivity can be properly mapped across an individual granule or sunspot umbra. When studying granulations, such maps should be married with Doppler imaging of the solar topology in order to link emissivity to angular changes in the surface. In this manner, solar physicists should be able to directly associate observed darkening with the emissive behavior of the solar surface itself, whether locally on the granular scale, or globally, as observers compare the solar center to the limb. In addition, the study of directional emissivities in sunspots should eventually affirm their metallic nature making investments in powerful space solar telescopes vital to the proper understanding of the solar surface.

As we continue to ponder the nature of the Sun, it is appropriate to close by recalling the brilliance of Father Secchi as an astronomer. Above all, Secchi valued observations. He painstakingly generated drawings of the Sun in an attempt to describe solar structures. Through his writings, he demonstrated that observation must lead theory. Short of data, we know nothing of the Sun. Therefore, should solar physics advance, the tradition of careful observation which Secchi inspired must be imitated. Even 140 years after the publication of *Le Soleil* [21], Secchi continues to astound, as Sobotka highlights [8]: "*In 1870 appeared the first edition of a fundamental work in solar astronomy by P. A. Secchi: Le Soleil. Most of the basic concepts of the sunspots' morphology can be found there. Secchi made his visual observations from 1865 to 1870 with a resolution approaching to* $0''.3$ *in some cases. In his wonderful drawings he presented not only the basic morphological features like multiple umbrae,* *light bridges, and penumbral filamentary structure, but also "knots" in bright penumbral filaments (penumbral grains) and internal structure of light bridges. He also noticed spatial variations in umbral brightness and the darkest regions — "holes" — in the umbra (dark nuclei). In three of his drawings even some umbral dots can be seen, although he did not describe them*". Now, endowed with the gifts of modern technology, solar physicists must be better equipped to properly describe what Secchi himself could only observe in awe using a simple telescope.

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Dedication

Cet ouvrage est dédié à celles qui ont été parmi mes premières enseignantes, les Filles de la Sagesse.

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On the Temperature of the Photosphere: Energy Partition in the Sun

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In this note, energy partition within the Sun is briefly addressed. It is argued that the laws of thermal emission cannot be directly applied to the Sun, as the continuous solar spectrum ($T_{app} \sim 6,000$ K) reveals but a small fraction of the true solar energy profile. Without considering the energy linked to fusion itself, it is hypothesized that most of the photospheric energy remains trapped in the Sun's translational degrees of freedom and associated convection currents. The Sun is known to support both convective granules and differential rotation on its surface. The emission of X-rays in association with eruptive flares and the elevated temperatures of the corona might provide some measure of these energies. At the same time, it is expected that a fraction of the solar energy remains tied to the filling of conduction bands by electrons especially within sunspots. This constitutes a degree of freedom whose importance cannot be easily assessed. The discussion highlights how little is truly understood about energy partition in the Sun.

The discussion of energy partition in materials may be considered to be so complex at times that, perhaps, the most prudent course of action rests in avoiding the entire subject. In the laboratory, the evaluation of energy partition demands years of study involving many hurdles for meager rewards. Nonetheless, before progress can be made in any field, the issues at hand must be identified. It is worthwhile to highlight some general ideas relative to energy partition in the Sun which would eventually afford a detailed mathematical approach to the question. Relative to solar physics, energy partition is complicated by the presence of both conduction and convection on the solar surface.

The interior of the Sun is currently hypothesized to approach temperatures of ~15,600,000K, while the corona manifests values on the order of 2,000,000–3,000,000 K [1, p.10]. Solar physicists maintain that the solar photosphere exists at a temperature of ~5,780K [1, p.10] in an apparent violation of the second law of thermodynamics [2–4]. This surface temperature is based on the application of the laws of thermal emission [5–7] to the solar spectrum [1, p.3–9] as first recorded in its entirety by Langley [8–10]. Still, the assignment of a temperature to the photosphere has not been without controversy.

Throughout the 19th century, great variations existed with respect to the temperature of the photosphere (see [11, p.268–279] and [12, p.48–52] for reviews). In 1898, Scheiner brought apparent unification to the problem when he applied Stefan's law [6] to data acquired by Pouillet, Secchi, Violle, Soret, Langley, Wilson, Gray, Paschen, and Rosetti [13]. Scheiner demonstrated that these previously discordant studies (see [14] for many of the original values) resulted in calculated solar temperatures of 5,000 to 6,200 K, with only one observation standing at 10,000 K [13]. Scheiner believed in a gaseous model and insisted that, even though the Sun's layers supported differing temperatures, it might be viewed as a blackbody. However, such an object did not meet the equilibrium conditions required by Kirchhoff [15, 16]. This immediately brought into question any temperature derived from such methods.

Scheiner was not alone in advocating that the laws of thermal radiation could be applied to the Sun. Two years earlier, in order to justify the extraction of the photospheric temperature from the laws of thermal radiation, Ebert stated that: "*With respect to electromagnetic radiation, the principal mass of the Sun acts like a black body*" [17]. In 1895, most scientists believed that Secchi's model of the Sun [18,19] was valid. Ebert considered this framework when he initially expressed doubt about the blackbody nature of the Sun: "*There remains only the question, whether we can regard the incandescent particles of the Sun, which yield the continuous spectrum, as comparable to a black body with respect to their total radiating capacity*" [17]. Frank Very [20] was more adamant in questioning the applications of the laws of emission to solar data when, in 1908, he stated in *Science*: "*It is doubtful whether radiation formulae obtained from measures through a limited range of temperature for solid bodies, composed of complex molecules, are applicable to solar conditions at the photospheric level, where it is improbable that any molecules remain undissociated. Extrapolations from Stefan's law of the proportionality of total radiation from a black body to the fourth power of the absolute temperature, are therefore not certainly applicable to the problem, even though the law has been verified through a range of some hundreds of degrees*" [20]. Nonetheless, Very immediately applied Stefan's law to the Sun [20].

The sternest warning against applying the laws of radiation to the Sun would come from Max Planck [21]. The father of modern physics removed all doubt relative to his position when he wrote: "*Now the apparent temperature of the Sun is obviously nothing but the temperature of the solar rays, de-*

pending entirely on the nature of the rays, and hence a property of the rays and not a property of the Sun itself. Therefore it would be not only more convenient, but also more correct, to apply this notation directly, instead of speaking of a fictitious temperature of the Sun, which can be made to have meaning only by the introduction of an assumption that does not hold in reality" [22, §101]. If Planck was so forceful in his comment, he rested his case on solid grounds: "*It is only in the case of stable thermodynamic equilibrium that there is but one temperature, which then is common to the medium itself and to all rays whatever color crossing it in different directions*" [22, §101]. Planck recognized with these words that the Sun was not in thermal equilibrium and hence he refused to accept the concept of "*apparent*" or "*effective*" solar temperatures [22, §101].

Perhaps more than anyone, Max Planck recognized that the laws of thermal emission had been obtained in settings involving complete thermal equilibrium. Kirchhoff's formulation was restricted to radiation within a rigid enclosure [15, 16,22] sustaining full thermal equilibrium. There could be no net conduction or convection processes present. Based on his objection, Planck recognized that the Sun supported convection currents. Carrington's differential solar rotation had been well known for over fifty years [18] and the convective nature of granular field was also firmly established [23]. In view of Planck's warning, a more considered approach should be adopted relative to applying the laws of thermal emission to the Sun.

Max Planck specifically excluded conduction when treating radiation, on the grounds that it's presence violated thermal equilibrium: "*Now the condition of thermodynamic equilibrium requires that the temperature shall be everywhere the same and shall not vary with time. Therefore in any given arbitrary time just as much radiant heat must be absorbed as is emitted in each volume-element of the medium. For the heat of the body depends only on the heat radiation, since, on account of the uniformity in temperature, no conduction of heat can take place*" [22, §25]. Like conduction, convection reduces emissivity. It is known that the emissivity of gases can fall with temperature in clear violation of Stefan's law [24]. These two realities, the presence of conduction and convection on the photosphere, are likely to explain Planck's hesitation to state anything about the Sun, based solely on the acquisition of its spectrum. Nonetheless, perhaps it is possible to extract something of value from the solar spectrum with respect to energy distribution within the Sun.

Relative to thermal radiation, the availability of electrically conductive paths can alter emissivity. In metals, normal emissivity can be substantially reduced [25–27]. Silver is an excellent conductor, but a poor emitter [28]. In fact, polished silver has one of the highest coefficients of reflection. It can be concluded that electronic conduction reduces emissivity.

When energy enters or escapes from an object, it does so by filling or vacating available degrees of freedom [24]. Without considering nuclear processes, the degrees of freedom are either translational, vibrational, rotational, or electronic [24]. As a rule, electronic degrees of freedom become particularly important at elevated temperatures. Within a gaseous Sun, constituent atoms are viewed as existing in a dissociated state. Such monoatomic species can have recourse only to translational and electronic degrees of freedom. Vibrational and rotational degrees of freedom are restricted to species which are at least diatomic.

In a solid, such as graphite at room temperature, the dominant degrees of freedom are likely to be vibrational [24]. Graphite displays a reasonable thermal conductivity in the hexagonal plane (390 W/m×K for *ab* direction) [29, p.44–57]. This compares well with the thermal conductivity of silver (420 W/m×K) [29, p.57]. Conversely, the thermal conductivity of graphite drops substantially between layers (~2 W/m×K) [29, p.57]. In graphite, thermal conductivity is linked to the vibrations of the lattice and these degrees of freedom [29, p.56].

Relative to electrical conductivity, graphite is a "semimetal" [29, p.57]. Its resistivity is $\sim 3\times10^{-3}$ ohm×m between layers making it is good insulator [29, p.61]. However, in the hexagonal plane, graphite has a resistivity of approximately $2.5\text{–}5\times10^{-6}$ ohm×m [29, p.61] making it reasonably metallic, but still well below silver which has an electrical resistivity of $\sim 1.59\times10^{-8}$ at 293 K [30, p.12–40]. Even in its favored plane, graphite is a significantly inferior conductor relative to silver. Consequently, the electrical conductivity of silver must be responsible for its weak emissivity, since its thermal conductivity is similar to graphite at least in one plane. This leads to the conclusion that the vibrational degrees of freedom are responsible for the excellent emissivity of graphite. Assuming that the object is at rest, the graphitic lattice does not permit translations or rotations, while the electronic degrees of freedom are unlikely to be significantly populated. As a result, when emissivity is properly coupled to temperature, it appears that the vibrational state of the sample primarily dominates [24].

In the gaseous models of the Sun, hydrogen and helium must exist as isolated atoms, many of which are devoid of electrons. Since the gaseous Sun has no lattice, it cannot support either thermal conduction through such a structure or energy transfer through electronic conduction bands. It cannot have recourse to lattice vibrations as a degree of freedom. Consequently, a gaseous Sun must rely almost exclusively on translational and electronic degrees of freedom as receptacles for energy. Yet, laboratory experience dictates that these degrees of freedom cannot support thermal emission of a Planckian nature [7]. Such is the great flaw of gaseous models which solar opacity approaches cannot reconcile [31]. To explain solar thermal emission, a mechanism similar to that which exists in graphite must be invoked. The dominant degrees of freedom in graphite are vibrational and linked to the existence of the lattice itself. In contrast, a gaseous Sun has no lattice and therefore cannot produce a thermal spectrum. Opacity arguments do not suffice to rectify these problems in a gaseous solar model [31].

Conversely, within a liquid metallic hydrogen model of the Sun [32], a lattice exists. In fact, from the days when it was first proposed by Wigner and Huntington [33], metallic hydrogen has been hypothesized to be able to assume a layered lattice similar to graphite. Such a lattice configuration will possess vibrational degrees of freedom which mimic those found in graphite, as required to properly account for the production of the solar spectrum. Accordingly, the thermal spectrum itself should be regarded as one of the strongest proof that the Sun is condensed matter, as its generation requires a lattice which dictates the interatomic spacing of condensed matter.

It appears that the solar spectrum is reporting only a small fraction of the true energy content of the photosphere, providing information which is limited to the vibrational state of the solar lattice. Much more substantial energy is stored in the translational degrees of freedom. This is manifested by the convection currents of the granules [23] and the differential solar rotation observed by Carrington [18]. Moreover, there is strong evidence to suggest that sunspots are metallic [23] and, therefore, maintain electronic conduction bands with their own associated energy.

These realities explain why the temperature of the solar photosphere does not constitute a violation of the second law of thermodynamics. The 5,780 K [1, p.10] measured is linked only to the vibrational degrees of freedom of the photospheric lattice. However, the true energy of the photosphere is dominated by its translational degrees of freedom. This helps to account for the production of X-rays in association with solar flares rupturing the photospheric surface [34]. When this occurs, we are likely to be monitoring some measure of the translational energy associated with the photosphere, as matter moves horizontally across the surface and collides orthogonally with the flare's vertical displacement of material. In a sense, the flare is providing resistance to the horizontal flow of matter on the photosphere. As surface matter collides with the flare, its energy is revealed and X-ray emissions are obtained [34]. Similarly, the temperatures of the corona in the 2,000,000–3,000,000 K range [35, p.3–10] reflect a coupling of these atoms to the translational degrees of freedom on the photosphere. No violation of the second law exists. The energy content of the photosphere is likely to correspond to temperatures of ∼7,000,000 K, when properly accounting for all of these phenomena as the author has previously stated [36]. In that case, the photospheric spectrum may be considered as reporting an apparent temperature, with little relevance to the real temperature of the surface [36]. Alternatively, it is also possible to reconcile the emission spectrum to the real temperature of the photosphere. The approach would be similar to that adopted when dealing with the microwave background problem [37] and, unfortunately, involves a reconsideration of Boltzmann's constant [38].

The consideration of energy partition in the Sun opens new avenues of discovery in physics. Most notably, it brings into question the universality of blackbody radiation, as first advocated by Gustav Kirchhoff [15, 16]. *A priori*, the gaseous Sun fails to meet Kirchhoff's requirement for thermal equilibrium with an enclosure, as Max Planck recognized [22, §101]. Regrettably, Kirchhoff's law itself is unsound [39, 40], destroying any perceived ability of gases to emit blackbody spectra. The issue is critical to the survival of the gaseous solar models. If local thermal equilibrium and its extension of Kirchhoff's formulation fails to guarantee that a blackbody spectrum is produced at the center of the Sun, then the gaseous models have no mechanism to generate its continuous emission. In part, this forms the basis of the solar opacity problem [31].

Dedication

This work is dedicated to the memory of Professor David G. Cornwell (10/8/1927–3/23/2011).

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Stellar Opacity: The Achilles' Heel of the Gaseous Sun

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The standard gaseous model of the Sun is grounded on the concept of local thermal equilibrium. Given this condition, Arthur Milne postulated that Kirchhoff's law could be applied within the deep solar interior and that a blackbody spectrum could be generated in this region, based solely on equilibrium arguments. Varying internal solar opacity then ensured that a blackbody spectrum could be emitted at the photosphere. In this work, it is demonstrated that local thermal equilibrium and solar opacity arguments provide a weak framework to account for the production of the thermal spectrum. The problems are numerous, including: 1) the validity of Kirchhoff's formulation, 2) the soundness of local thermal equilibrium arguments, 3) the requirements for understanding the elemental composition of the Sun, and 4) the computation of solar opacities. The OPAL calculations and the Opacity Project will be briefly introduced. These represent modern approaches to the thermal emission of stars. As a whole, this treatment emphasizes the dramatic steps undertaken to explain the origins of the continuous solar spectrum in the context of a gaseous Sun.

1 Introduction

The mechanism by which the solar spectrum is produced has long preoccupied astrophysics [1–4]. Though Langley established that the photosphere's emission [5–7] generally conformed to a blackbody lineshape [8,9], two lines of reasoning initially prevailed as to its formation. It was hypothesized that the photosphere contained condensed carbon [1, 2], as graphite was the premier blackbody source on Earth [3, 4]. Alternatively, it was believed that the pressure broadening of hydrogen could account for the spectrum [1, 2]. Although Kirchhoff had formulated his law of thermal emission in 1859 [10], observational astronomers appeared dissatisfied with the idea that Langley's spectrum [5–7] could be produced by assuming thermodynamic equilibrium and enclosure [9, p.1–45]. They insisted on placing carbon particles on the Sun for sixty years [1,2] and essentially dismissed any notion that Kirchhoff's law afforded a sufficient framework to generate the solar spectrum.

It would take the work of men [11] like Schuster [12], Schwarzschild [13], Eddington [14–17], Rosseland [18, 19] and Milne [20–23] to finally remove graphite from the Sun [2]. These communications [12–23] formed the foundation of radiation transfer within stars. They consequently came to represent the heart of modern stellar physics. As a group, these authors used elegant approaches, but without exception [12–23], their mathematical treatments relied on thermal equilibrium and the validity of Kirchhoff's law [10]. In addition, since the standard model of the Sun was deprived of condensed matter, astronomers would have to account for the production of the solar spectrum with physical atoms, ions, and electrons. Graphite was gone, but the theoretical alternative, solar opacity arguments, provided a questionable replacement.

2 Kirchhoff's law and local thermal equilibrium

Arthur Milne [2] was perhaps the first to advocate that the interior of the Sun could be regarded as existing in a state of local thermal equilibrium [20–23]. Milne's definition became central to astrophysical thought and will, therefore, be largely recalled: "*It is convenient to have a phrase to describe the circumstances under which the relation* $j_\nu = k_\nu B_\nu(T)$ *holds exactly. When a small portion of matter has a definite temperature T, and is behaving, i.e. emitting, as if it formed a part of an equilibrium enclosure at temperature T, we shall say that it is in "local thermodynamic equilibrium" at temperature T. We shall examine later in particular cases the conditions under which material is in local thermodynamic equilibrium. It is not necessary that the temperature shall be uniform. In an non-isothermal state, we may still have local thermodynamic equilibrium everywhere. The temperature may vary from point to point, but each point may be characterized by a definite temperature T and the element of matter at each point may be behaving as if in thermodynamic equilibrium at temperature T*" [23, p.81]. Milne's treatment was centered on Kirchhoff's law: $j_\nu = k_\nu B_\nu(T)$ [10]. Nonetheless, there was a risk that Milne's setting was so broad that virtually any non-equilibrium process, no matter how violent, could be considered in local thermal equilibrium, provided that sufficiently small volumes of matter were being considered. No restriction was placed on confirming the validity of these arguments.

Much like Milne, Chandrasekhar described local thermal equilibrium as follows: "*. . . we often encounter physical systems which, though they cannot be described as being in rigorous thermodynamical equilibrium, may yet permit the introduction of a temperature T to describe the local properties of the system to a very high degree of accuracy. The interior* *of a star, if in a steady and static state, is a case in point. For, even if the temperature at the center of the Sun, for instance, were* 10^8 *degree, the mean temperature gradient would correspond to a change of only 6 degrees in the temperature over a distance of* 10^4 *cm. This fact, coupled with a probably high value for the stellar absorption coefficient, enables us to ascribe a temperature T at each point P such that the properties of an element of mass in the neighborhood of P are the same as if it were adiabatically inclosed in an inclosure at a temperature T*" [24, p.205]. Similar points were raised in Clayton's classic text [25, p.175]. These discussions were focused strongly on assumptions which pertain to a gaseous model.

On the surface, it would seem that Chandrasekhar's temperature gradient of only 6 degrees across 100 meters could be considered quite small [24, p.205]. Yet, the oceans of the Earth sustain convection currents based on much smaller temperature gradients. In fact, oceanographers might reject equilibrium arguments globally for the oceans, even though these temperature gradients are on the order of just a few degrees over spans of thousands of kilometers. The oceans contain convection currents as a direct manifestation of their lack of thermal equilibrium. Convection precludes the existence of equilibrium. As a result, a temperature variation of 6 degrees over a span of 100 meters should be treated as an enormous temperature gradient, not a condition approaching thermal equilibrium. The oceans demonstrate that Chandrasekhar's conditions, even if relaxed 1,000 fold, would still constitute powerful driving forces for convection, thereby eliminating all possibility of viewing the solar interior as existing in a state of thermal equilibrium.

Well before the days of Chandrasekhar, Milne elaborated further on local thermal equilibrium in the gaseous framework: "*The interior of a star is in a state of local thermodynamic equilibrium of this character. As we approach the boundary from the inside, the state of local thermodynamic equilibrium gives place to an entirely different state, in which the influence of external radiation on an element is paramount. It will be shown that when an element at temperature T is subjected to radiation, which is not black radiation of temperature T, the extent to which it behalves as if in thermodynamic equilibrium locally depends on the relative importance of collisions as a cause of atomic absorptions and emissions. If the atoms are sufficiently battered about by colliding with one another, they assume a state (distribution of stationary states) characteristic of thermodynamic equilibrium at temperature T; if they are not sufficiently battered about, their "temperature" becomes irrelevant and they emit and absorb at a rate which is determined by the incident radiation. It is clear that collisions will be the more numerous, and therefore likely to be more effective, the higher the density. This permits us to see in a general way why the state of local thermodynamic equilibrium in the interior of a star breaks down as we approach the surface...This assumption will certainly be satisfied in the far interior, since in the limit at great distances the conditions are those of an enclosure... It follows that the intensity of radiation at* $d\sigma$ *in the direction* θ *is* $B_\nu(T)$*, the intensity of black radiation for temperature T*" [23, p.81–83].

The argument advanced by Milne was framed in the context of the laws of gases. Milne saw the rapid collisions occurring at the center of the Sun as sufficient to establish equilibrium, but the requirements set forth by Kirchhoff [10] and Planck [8, 9] required something more significant. They demanded that the walls of the enclosure be rigid [9].

If a gas is highly compressed, the collisions with neighboring particles will enable the flow of heat through conduction. Gold has a density of 19.3 g/cm^3 [26, p.12–205] and many solids [26, p.12–80] have densities which are just slightly more than one order of magnitude (about a factor of 30) below the 150 g/cm^3 currently hypothesized for the center of the Sun [27, p.10]. When heat enters solids, it can travel through conduction, either thermally through its vibrational lattice or electronically through its conduction bands. Clearly, gases cannot sustain conduction bands, but they are subject to thermal conductive processes, especially at these densities. As such, when an atom in the gaseous model vibrates at the center of the Sun, it can transfer its energy to its "*non-rigid*" neighbor. Milne cannot assume that the atoms at the center of the Sun are devoid of collisional energy exchange, precisely because the atoms are not rigid. The center of the Sun cannot meet the requirements for a rigid enclosure as set forth by Kirchhoff and Planck [8–10]. The arguments of enclosure and "*local thermal equilibrium*" are invalid based on these considerations.

At the same time, Planck required that the source of blackbody radiation was found in material particles. Planck's entire *Heat Radiation* [9] was based on the analysis of a material oscillator not present at the center of the gaseous Sun: "*For among all conceivable distributions of energy the normal one, that is, the one peculiar to black radiation, is characterized by the fact that in it the rays of all frequencies have the same temperature. But the temperature of a radiation cannot be determined unless it be brought into thermodynamic equilibrium with a systems of molecules or oscillators, the temperature of which is known from other sources. For if we did not consider any emitting and absorbing matter there would be no possibility of defining the entropy and temperature of the radiation, and the simple propagation of free radiation would be a reversible process, in which the entropy and temperature of separate pencils would not undergo any change. Now we have deduced in the preceding section all the characteristic properties of the thermodynamic equilibrium of a system of ideal oscillators. Hence, if we succeed in indicating a state of radiation which is in thermodynamic equilibrium with the system of oscillators, the temperature of the radiation can be no other than that of the oscillators, and therewith the problem is solved*" [9, §144].

Max Planck required that a perfect absorber be present in order to produce blackbody radiation. Milne neglected this important line from *Heat Radiation*: "*Hence in a vacuum bounded by totally reflecting walls any state of radiation may persist*" [9, §51]. Planck then argued that, if an arbitrarily small quantity of matter was introduced, the radiation in the enclosure will change to a new state. However, it will not be a blackbody state unless the substance is not transparent for any frequency. Planck chose a piece of carbon to ensure blackbody radiation [9, §51]. The desired radiation does not simply appear [9, §51], as Milne and his contemporaries surmised. The presence of an enclosure, by itself, could never satisfy the requirements for the production of blackbody radiation. Planck insisted throughout *Heat Radiation* on the need for a physical oscillator and he reminded his readers that only "*material particles*" can be involved in emission [9, §4] and absorption [9, §12]. A physical oscillator which acted as a perfect absorber must be present. Milne has not advanced such a species at the center of the Sun.

Instead, Milne, like Schuster [12], Schwarzschild [13], and Eddington [14–17] before him, automatically presumed that the invocation of Kirchhoff's law provided sufficient proof that the interior of the Sun harbored black radiation, despite the absence of the rigid enclosure required by Kirchhoff [10]. Blackbody radiation was inserted at the center of the Sun without any requirement on the material generating the needed photons. All that was required was enclosure (even if not strictly rigid) and a newly hypothesized "*local thermodynamic equilibrium*". For Milne, the presence of an enclosure was insured by the hypothesis that the density at the center of the Sun was sufficiently elevated to restrict photonic and atomic diffusion [20–23].

In reality, Milne's idea fell far short of the requirements to produce blackbody radiation. He was considering a setting where conduction, not radiation, could dominate heat exchange. Consequently, his arguments relative to radiative heat transfer were without strong scientific justification. Milne had neglected the observation that the collision of adjacent atoms constituted the universally accepted exchange mechanism for thermal conduction, not equilibrium. It was for this reason that Planck insisted on a rigid enclosure.

A careful review of blackbody radiation has revealed that the production of such a spectrum always requires the presence of a perfect absorber [3]. Planck himself constantly brought forth the carbon particle as inherently linked to the validity of his arguments [3]. Kirchhoff's reasoning that an adiabatic enclosure could contain black radiation has been exposed as flawed and his law of thermal emission as erroneous [3, 4, 28–30]. The universality of blackbody radiation simply does not exist [3, 4, 28–30]. Yet, even if Kirchhoff's law was valid, Milne's argument was fallacious, as he lacked both the rigid enclosure and the materially perfect oscillator required by Max Planck to ensure that a blackbody spectrum could be produced at the center of the Sun.

3 Solar and stellar opacity

Solar opacity [22, 31, 32, 34–39] plays a vital role in all modern gaseous models of the Sun [24, 25, 40–46] and is currently at the center of our understanding of the stars. Therefore, the study of solar opacity has far reaching implications throughout modern astronomy.

Opacity, κ, refers to the ability of a material to absorb incoming radiation. Monochromatic opacity, κ_ν, is associated with a single frequency. The extinction coefficient, α (cm^{-1}), is equal to the opacity, κ (cm^2/g) multiplied by the density of the material, ρ (g/cm^3).

To calculate opacity within the solar interior, solar physicists first accept that the Sun can radiate internally. By itself, this constitutes a notable departure from the rest of Earthly physics. For all objects on Earth, internal heat transfer occurs through conductive and convective paths, not internal radiation. Radiation allows objects to achieve thermal equilibrium with one another, not within themselves. As a result, the idea that the Sun transfers internal energy through radiation directly implies that astrophysics treats the solar interior as the sum of its individual atomic, ionic, and electronic species. The Sun as a single object does not exist in the gaseous models. Only in such a scenario would internal radiation permit the transfer of energy between the constituent objects which make up the Sun. Still, Milne required that, within the center of the Sun, atoms, ions, and electrons were packed such that collisions occur. This scenario rendered conduction probable, greatly impacting any radiative field.

In gaseous solar models, thermal photons at X-ray frequencies, with a characteristic blackbody appearance, are believed to be produced at the center of the Sun. Over the course of thousands of years, Eddington stated that these thermal photons slowly leaked out of the solar body [16]. As they traversed increasingly elevated layers of the solar mass, photons gradually lost some of their energy. The entire solar spectrum was shifting from the X-ray to the visible range, while preserving a blackbody appearance [16].

3.1 Opacity mechanisms

Stellar opacity involves the removal of energy from a beam of photons originating in the core of the Sun through four mechanisms: 1) bound-bound, 2) bound-free, 3) free-free, and 4) scattering processes (see [41, p.137–141] for an excellent description). Bound-bound processes rely on spectroscopic line absorption, either within an atom or an ion. Bound-free mechanisms result in the dissociation of a previously bound electron by an incoming photon. The electron becomes completely free of the atom or ion. Free-free processes are inverse Bremsstrahlung mechanisms, whereby a free electron and an ion interact during which time the combined species is able to absorb a photon [41, p.138]. In scattering mechanisms, the momentum of the photon is being transferred to a scattering electron. Theoretical astrophysics calculates opacities for the Sun by taking the summation of these processes, for all atoms, ions, and electrons at all temperatures within the solar interior.

The negative hydrogen ion was advanced as a significant determinant of solar opacity by Wildt [47]. The concept immediately received the support of Chandrasekhar who calculated that the negative hydrogen atom within the context of a gaseous solar model would contribute greatly to solar opacity in the 4,000–24,000 Å range [48–51]. Of course, the negative hydrogen ion spectrum extended over much of the photospheric emission (~2,500–25,000 Å).

Nonetheless, the negative hydrogen ion could never, by itself, generate the continuous solar spectrum with its characteristic thermal appearance. For gaseous models, the production of the thermal spectrum involves the slow conversion of a hypothetically X-ray blackbody spectrum produced in the solar interior to the visible spectrum observed at the photosphere. Thus, if a blackbody spectrum did exist at the center of the Sun, it would be characterized by a Wien displacement temperature of ~15,000,000 K. Such a spectrum would be centered in the X-ray region. It would then have to be gradually shifted, while always maintaining its thermal appearance, to much lower frequencies.

Consequently, astrophysics is requiring that a perfect mixture of atoms, ions, and electrons exists at all layers within the Sun. In each layer, these mixtures could then produce the desired local blackbody spectrum. Within each solar layer, a new perfect mixture must exist in order that its absorptive characteristics enable the production of a new shifted thermal spectrum.

Therefore, despite Chandrasekhar's findings [48–51], the computation of solar opacity has remained a tremendously complex undertaking. For example, the American astrophysics community has invested heavily in calculating the opacity contributions from neutral and ionized gases. In a project involving international collaboration, the Los Alamos National Laboratory led Opacity Project [33, 34] provided an absolutely phenomenal treatment of nearly every possible atomic species inside the stars, in widely varying states of oxidation. Similar findings have been obtained at the Lawrence Livermore National Laboratories. These studies have resulted in the OPAL opacity values [35–39], but none of the opacity mechanisms considered by these methods can be used to explain the origin of the blackbody spectrum in graphite. This suggests that these mechanisms are not truly related to the production of the solar spectrum.

3.2 Rosseland mean opacities

The determination of internal solar opacity values must be performed at each individual frequency of interest, since the production of a blackbody spectrum always remains frequency dependent. The problem becomes so overwhelming that astrophysics has chosen to adopt Rosseland mean opacities [18, 19]. Through Rosseland's approach, a single frequency independent value of opacity can be obtained for each solar level.

On the surface, it could be argued that Rosseland mean opacities merely reduce an otherwise intractable problem. They lower computational requirements and greatly simplify the presentation of opacity data. Rosseland mean opacities enable solar physics to sidestep the reality that, at each level of the solar interior, it is impossible to generate a purely blackbody spectrum with strict adherence to Planckian behavior at all frequencies. It is not feasible to build a blackbody spectrum from the sum of non-blackbody processes. For instance, during the computation stage, a single bound-bound transition will introduce a "*spike-like*" contribution in the calculated spectrum. Each "*spike*" being associated with line absorption. Such a "*spike*" must then be compensated by using the sums of processes (other bound-bound processes, or bound-free, free-free and scattering mechanisms) whose existence will always remain in doubt at the levels required to incorporate the initial "*spike*" into the final solution for the blackbody lineshape. The entire process becomes an exercise in parameter fitting, devoid of confirmatory physical evidence.

Still, Rosseland mean opacities remain at the heart of modern solar models [24, 25, 40–46]. Within each layer in the Sun, a mean opacity can be inferred based on expected atomic, ionic, and electronic species. However, the sum of the processes (bound-bound, bound-free, free-free, scattering) utilized in Rosseland mean opacity computations cannot be infinite. Thus, rather than analyze mean opacities, scientists can convince themselves of the futility of these approaches by taking the mean opacity solutions and using the same species and concentrations to calculate the associated *frequency dependent spectra*. Such solutions will not correspond to black body spectra. As a result, Rosseland mean opacities form a weak foundation for the gaseous solar models. The summation of numerous spectral processes which are individually unrelated to thermal radiation can never give rise to a truly black spectrum.

3.3 Elemental compositions

To further complicate matters, the computation of solar opacity, as a function of depth, requires that the elemental composition of the Sun [52] remains independent of spatial position. Such, a requirement can never be justified. Our current understanding of the solar composition rests, and will always rest, on that which can be evaluated at the level of the photosphere. All extensions of the solar composition to the solar interior and all claims of constant elemental constitution with depth should be regarded as scientific conjecture.

4 Conclusion

Through opacity considerations, solar physicists believe that an X-ray based blackbody spectrum, produced at the center of the Sun, can be emitted at the solar surface in the visible range. However, from the moment that the Sun was hypothesized to exist in the gaseous state in the mid-1800s, objections were raised as to the ability of gases to emit a blackbody spectrum [1]. The interior of a gaseous Sun was thought to be essentially transparent to radiation. This was the position advocated by Herbert Spencer when he complained that, if sunspots were openings in the photosphere, one should be able to see through them to the other side [1]. In fact, the same "*famous objection*" was voiced by Kirchhoff himself [1]. According to Kirchhoff, the interior of the Sun could only sustain blackbody radiation if it was surrounded by a condensed photosphere [1]. Kirchhoff well understood that no gas, in isolation, ever produced a blackbody spectrum. The presence of condensed matter was always required.

In support of Kirchhoff's liquid photosphere [1], there are numerous lines of evidence that the photosphere is condensed matter [53]. Granules, sunspots, and limb darkening provide additional evidence [56]. Sunspot emissivities are highly suggestive of metallic character [56] strengthening the case for condensed matter. All of these factors should be considered when advancing the proper phase of the photosphere and the mechanism associated with solar thermal emission.

Nonetheless, despite clear violations with regards to enclosure, thermal equilibrium, and the presence of a perfect absorber as required by Max Planck [9], solar physics has tried to account for the generation of the Planckian spectrum. Yet, none of the mechanisms advanced can be used to explained the simple thermal spectrum of graphite itself. In fact, although physics advocates an understanding of internal thermal radiation within the Sun, it has produced no mechanism by which the simplest earthly spectrum can be explained. This constitutes a powerful reminder that tremendous difficulties remain relative to the science of blackbody radiation [3, 4]. In the end, stellar opacity calculations represent a myriad of physical impossibilities. None of the suggested opacity mechanisms (bound-bound, bound-free, free-free, and scattering) are related to the emission of a single photon by graphite.

As such, beyond an inability to support structure, the shortcomings of any gaseous solar model rests on opacity. Even though Milne and his predecessors were incorrect in inferring that a blackbody spectrum could be produced at the center of the Sun, the gaseous models contain numerous other stumbling blocks on their way to generating a continuous spectrum at the solar surface. A truly remarkable thesis has been advanced to explain the photospheric spectrum within the gaseous model. In the end, astrophysics has championed a solution for obtaining the solar spectrum which cannot survive the careful scrutiny of the spectroscopic scientific community.

Each spectroscopic signature in nature is linked to a unique physical process. For instance, a Lyman or a Balmer series can only be produced by electronic transitions within the hydrogen atom. Similarly, atomic line spectra are unique to each individual elemental or ionic species. Nuclear magnetic resonance (NMR) spectra are obtained from particular spin transitions within a well defined physical and experimental context. Physics does not search for the Lyman series in NMR spectra. One process is electronic, the other nuclear. Within the gaseous Sun, modern astrophysics currently believes that it can produce the graphitic spectrum using processes which do not exist in graphite. It is improper to advance that a blackbody spectrum can be produced in the Sun using physical mechanism which are not present on Earth within all the blackbodies currently studied in our laboratories [3, 4]. The use of a nearly infinite sum of atomic, ionic, and electronic processes which can alter their absorption and emission precisely in a manner which preserves the blackbody appearance of the solar spectrum at all depths within the Sun represents a non-scientific exercise based solely on the desire to salvage the gaseous equations of state. It is well-known that thermal emissivity in gases can drop with increasing temperature. Neither pressure broadened gases nor any of the atomic, ionic, and electronic processes advocated in the interior of the Sun have a fourth power of temperature behavior. Furthermore, the gaseous models depend on knowledge of the internal constitution of the stars based on the solar elemental constituents. Mankind will always lack such information.

As a result, this work constitutes an invitation to reconsider the phase of the Sun [53–55]. The gaseous models suffer from two insurmountable weaknesses: 1) the inability to account for photospheric structures [56], and 2) the lack of a proper mechanism to generate the solar spectrum. Observational astrophysics has long documented the existence of features of the solar surface which demand the presence of condensed matter [56]. The belief that opacity arguments can account for the illusionary nature of the solar surface and all associated structures, discounts the realization that the photosphere also *behaves* as condensed matter [56, 57]. Helioseismology demonstrates that the Sun acts as a resonant cavity [53]. On Earth, resonant cavities are manufactured from condensed matter [4]. It is not reasonable to expect that a gaseous Sun can create an illusionary surface in the visible range using negative hydrogen ion opacity, while at the same time and in the same layer, produce a surface which is nearly perfectly reflecting for wavelengths which extend over many thousands of meters. Such are the requirements, if the Sun really acts as a resonant cavity [58, p.60]. Perfect resonators sustain standing waves which are never absorbed [4]. Accordingly, the photosphere of the gaseous Sun must be strongly opaque in the visible region while powerfully reflecting in the sub-audio. In addition, the gaseous models must account for the presence of transverse waves on the surface of the Sun when gases are known to sustain only longitudinal waves [53, 57]. It remains the case that seismology is a science of condensed matter [53]. To account for seismological behavior in a gaseous Sun using opacity arguments constitutes a significant departure from accepted Earthly physics.

Given the problems which surround solar opacity, it remains difficult to understand how the gaseous models of the Sun have survived over much of the twentieth century. Local thermal equilibrium does not exist at the center of the Sun. Both Kirchhoff and Planck require rigid enclosure which is not found in the Sun [9, 10]. Planck has also warned that the Sun fails to meet the requirements for being treated as a blackbody [59]. Milne's rapid collisional regime constitutes a path to conduction, not equilibrium [20–23]. Milne and his contemporaries cannot infer that a blackbody spectrum exists at the center of the Sun based on Kirchhhoff's law [10], even if the law was valid [60]. Unfortunately, not only does the Sun fail to meet the requirements for enclosure and local thermal equilibrium, but Kirchhoff's law itself is erroneous [3,4]. The production of a blackbody spectrum requires the presence of a perfect absorber. Max Planck appeared well-aware of this reality [3,59]. Gaseous opacity arguments will always fall far short of what was required. In the end, the mechanism used to generate the solar spectrum should be shared with graphite itself. The most likely physical cause remains the vibration of atomic nuclei within the confines of a layered graphite-like lattice [28,55].

Dedication

This work is dedicated to my sister, Lydia, and to her children, Fabienne and Louis.

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Lessons from the Sun

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In this brief note, the implications of a condensed Sun will be examined. A celestial body composed of liquid metallic hydrogen brings great promise to astronomy, relative to understanding thermal emission and solar structure. At the same time, as an incompressible liquid, a condensed Sun calls into question virtually everything which is currently believed with respect to the evolution and nature of the stars. Should the Sun be condensed, then neutron stars and white dwarfs will fail to reach the enormous densities they are currently believed to possess. Much of cosmology also falls into question, as the incompressibility of matter curtails any thought that a primordial atom once existed. Aging stars can no longer collapse and black holes will know no formative mechanism. A condensed Sun also hints that great strides must still be made in understanding the nature of liquids. The Sun has revealed that liquids possess a much greater potential for lattice order than previously believed. In addition, lessons may be gained with regards to the synthesis of liquid metallic hydrogen and the use of condensed matter as the basis for initiating fusion on Earth.

> "*Young people, especially young women, often ask me for advice. Here it is, valeat quantum. Do not undertake a scientific career in quest of fame or money. There are easier and better ways to reach them. Undertake it only if nothing else will satisfy you; for nothing else is probably what you will receive. Your reward will be the widening of the horizon as you climb. And if you achieve that reward you will ask no other.*"
>
> Cecilia Payne-Gaposchkin [1]

When Cecilia Payne [1] discovered that the stars are primarily composed of hydrogen [2], she encountered strong opposition from Arthur Eddington, her first mentor, and from Henry Norris Russell [3]. Nonetheless, Cecilia Payne's work engendered a new age in astronomy: hydrogen became the building block of the universe. Russell would eventually come to echo Payne's position [4]. In those days, it was natural to assume that a hydrogen-based Sun would be gaseous [5, 6]. Ten years after Payne published her classic report, Wigner and Huntington proposed that condensed metallic hydrogen could be synthesized [7]. In so doing, they unknowingly provided James Jeans with the material he had lacked in constructing liquid stars [5]. Still, though liquid metallic hydrogen became a component of the giant planets and the white dwarf [8], the concept of condensed matter was kept well removed from the Sun.

Now that liquid metallic hydrogen has been advanced as a solar building block (see [8] and citations therein), it is likely that opposition will be raised, for many will foresee unsettling changes in astronomy. A liquid Sun brings into question our understanding of nearly every facet of this science: from stellar structure and evolution [9], the existence of black holes [10], the premordial atom [11], dark energy [12], and dark matter [13]. It is not easy to abandon familiar ideas and begin anew.

However, some scientists will realize that a liquid metallic hydrogen model of the Sun [8], not only opens new avenues, but it also unifies much of human knowledge into a cohesive and elegant framework. A liquid metallic Sun invites astronomy to revisit the days of Kirchhoff [14] and Stewart [15], and to recall the powerful lessons learned from studying the thermal emission of materials [16,17]. It emphasizes that our telescopes observe structural realities and not illusions [18, 19]. In recognizing the full character of these structures, all of the great solar astronomers from Galileo [20], to Secchi [21], to Hale [22] are honored. These observers knew that solar structures (granules, sunspots, pores, flares, prominences, etc…) were manifesting something profound about nature.

For astrophysicists, the Sun imparts lessons which may well have direct applications for mankind. For instance, the solar body holds the key to fusion. If the Sun is made from condensed matter [8], then our experiments should focus on this state. Sunspots may also guard the secret to synthesizing metallic hydrogen on Earth [8]. If sunspots are truly metallic [18], as reflected by their magnetic fields [22], then attempts to form liquid metallic hydrogen on Earth [8] might benefit from the presence of magnetic fields. Our analysis of the photospheric constitution and the continuous thermal spectrum should be trying to tell us something about liquids and their long range order. It is currently believed that liquids possess only short term order [23]. In this regard, perhaps physics has lacked caution in bombarding the fragile liquid lattice with X-rays and neutrons [24, 25]. These methods may fail to properly sample the underlying structure. Gentler approaches may reveal structure where none was previously believed to exist. The solar spectrum implies long range order, much like that observed in graphite [16, 17, 26]. As such, liquid metallic hydrogen on the photosphere could provide the framework for long range order, despite the fact that its only binding force lies in the need to maintain electronic conduction bands (see [8] and references therein). Most importantly, however, the Sun might be trying to tell us that we still do not properly understand thermal emission [16, 17, 27]. If gaseous models exist to this day, it is because the mechanism which produces the blackbody spectrum in graphite continues to be elusive [16, 17, 27]. Of all spectroscopic signatures, blackbody radiation remains the only one which has not been explained fully. These problems constitute serious and important questions for humanity. Unlocking these mysteries is certain to keep scientists occupied, as we continue to ponder upon the lessons discerned from the Sun.

Dedication

This work is dedicated to the memory of Miss Beckly [28, p.134], Annie Scott Dill Russell [28, p.144–146], Margaret Huggins [29], Henrietta Swan Leavitt [30, 31], Annie Jump Cannon [32–34], Antonia Maury [35], Williamina Paton Stevens Fleming [36–38], Cecilia Payne-Gaposchkin [1] and the forgotten women of astronomy [39, 40].

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LETTERS TO PROGRESS IN PHYSICS

Pierre-Marie Luc Robitaille: A Jubilee Celebration

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We celebrate the 50th birthday anniversary of Prof. Pierre-Marie Robitaille, the author of Progress in Physics who is one of the leading experts in the Nuclear Magnetic Resonance Imaging. Prof. Robitaille is known as the designer of the most world's first Ultra High Field MRI scanner. Prof. Robitaille still continues his creative research activity in the field of thermal physics, connected to the origin of the Microwave Background and astrophysics.

July 12, 2010 marks the 50th birthday of Professor Pierre-Marie Robitaille. He was born in North Bay, Ontario, the third of ten children to Noel Antoine Robitaille and Jacqueline Alice Roy. Noel Robitaille had moved to Ontario from his native Quebec when he was stationed as a physician in the Royal Canadian Air Force. Eventually settling in northern Ontario, he served the villages of Massey and Espanola. In his role as a local doctor, Noel Robitaille would also care for the Ojibway population of the region. In 1964, he would be honored by the Ojibway Nation, becoming the first white man to bear the distinction of Ojibway chief of the Spanish River Band. His Indian name, *Ke-chutwa-ghizhigud*, meaning "Chief Holiday" [1].

Raised by French-Canadian parents, Pierre-Marie Robitaille attended L'École St. Joseph in Espanola, Ontario, where he studied primarily in his native tongue. Upon completion of the 8th grade, he attended Espanola High School, where education was conducted in English. As an adolescent, he often served as an altar boy during daily mass at St. Louis de France Catholic Church, the French parish of his community. Surrounded by the forests of Northern Ontario, he enjoyed ice fishing, hunting, and building log cabins in the woods.

In 1978, just as Robitaille was completing his secondary education, his father relocated to Cedar Falls, Iowa. Mrs. Robitaille and her children were to remain in northern Ontario. In order to maintain ties with his father, Robitaille enrolled at the University of Iowa in Iowa City. It was there that he met his future wife, Patricia. Though he relocated to Iowa for the 1978–1979 school year, Robitaille rarely saw his father. Therefore, he moved to Cedar Falls, Iowa. He would graduate from the University of Northern Iowa, in 1981, with a degree in general science.

At that time, Robitaille entered a Ph.D. program in biochemistry under the tutelage of Dr. David E. Metzler at Iowa State University, obtaining an M.S. degree in 1984. His masters thesis involved NMR equilibrium analysis of polyamines with vitamin B6. At the same time, Robitaille realized that in-vivo NMR was beginning to grow. He sought unsuccessfully to convince Dr. Metzler to enter this promising new area of biochemistry and, eventually, entered the field on his own.

Prof. Pierre-Marie Robitaille.

He transferred his graduate appointment to the Department of Zoology, where he brought in-vivo NMR methods to the laboratory of George Brown, an electron microscopist. It was there that he acquired a set of standards for in-vivo ^{31}P-NMR [2] and conducted some of the first studies of isolated sperm cells with ^{31}P-NMR [3, 4]. At the same time, Robitaille enrolled in the Inorganic Chemistry doctoral program, under the guidance of Professor Donald Kurtz. He graduated from Iowa State University with a Ph.D. in 1986, holding majors in Zoology and Inorganic Chemistry. His dissertation was divided into two parts which he would defend in front of separate committees, one for each major.

Following his Ph.D. training, Pierre-Marie Robitaille joined the in-vivo NMR laboratory of Professor Kamil Ugurbil at the University of Minnesota. There, he conducted work in cardiac spectroscopy, operating one of the first small animal 4.7T/40cm magnetic resonance instruments in the United

States. It was Professor Ugurbil who urged Robitaille to apply for faculty positions in magnetic resonance imaging and spectroscopy. Ultimately, he accepted the position of Director of Magnetic Resonance Research and Assistant Professor of Radiology at The Ohio State University, with a startup package well in excess of $1 million. He was 28.

While at Ohio State, Professor Robitaille established himself as a leader in cardiac spectroscopy and magnetic resonance [5, 6]. He would eventually design and assemble the world's first Ultra High Field MRI instrument [7–16]. The results obtained from this scanner would propel MRI into a new era in imaging technology. Professor Allan Elster, the Editor of the Journal of Computer Assisted Tomography recognized the magnitude of the contribution and arranged for a special issue of the journal to be published outlining some of the first 8 Tesla results. In is editorial comments relative to this issue, Dr. Elster wrote:

> "This is a landmark issue of the Journal of Computer Assisted Tomography. Contained within its pages are amazing images and technical descriptions of the world's first whole body human clinical magnetic resonance scanner operating at 8 Tesla. Congratulations to Pierre-Marie Robitaille and his co-workers in Radiology and Engineering at The Ohio State University for constructing a device some experts said would be impossible to build. The total stored magnetic energy in this 30,000 kg magnet is a remarkable 81 megajoules. To put this value into perspective, 81 MJoules is the kinetic energy of a 200-metric ton locomotive barreling down the track at 100 km per hour! The human images obtained so far are also astounding (Fig. 1), especially considering that the system has only been operational for a few months and many radio frequency coil and pulse sequence issues remain to be worked out. The Ohio State team has proposed a number of interesting theories concerning susceptibility effects and dielectric resonance phenomena within the human head at 8 Tesla. Some of these theories challenge traditional tenets in MR physics and are admittedly controversial. As more measurements are obtained and experiments are conducted, these theories will be refine, improved, or discarded. Robitaille et. al. have led us to a new frontier in clinical MR imaging. Perhaps one day in the not-so-distant future, 1.5 Tesla will be considered low-field imaging" [14].

The next month, Professor Robitaille established a new record for high resolution imaging in MRI, once again published in JCAT, with the following editorial note:

> "Pierre-Marie Robitaille and the Ohio State University MRI Team have done it again! In this issue they present the world's first MR images obtained at 2,000×2,000 resolution — in honor of the new millennium of course. In case you missed it, please check out the Journal of Computer Assisted Tomography's November/December 1999 issue. Here Robitaille and colleagues have published 10 landmark articles describing the design and construction of their 8 Tesla whole-body MR scanner, as well as additional remarkable images of the brain. If you wish to download some of the images directly (they look even better on a video monitor), please see the JCAT website at www.rad.bgsm.edu/jcat/supp.htm. Happy Y2K from all of us at JCAT!" [15].

The birth of Ultra High Field MRI represented a paradigm shift for many in the MRI community who had previously believed that human images could never be acquired at such field strengths [16, 17]. Relative to the creation of the first UHFMRI systems, Paul Lauterbur (Nobel Prize in Medicine and Physiology, 2003) wrote:

> "In the early machines, low radiofrequencies of 4 MHz or so meant that RF coil designs were simple (even inexperienced undergraduates could design and build such circuits with little knowledge of more than DC electrical circuits), and the forces on gradient coils were small. The effects of magnetic susceptibility inhomogeneity in and around the object being imaged were negligible, and RF penetration depths were not a problem for human-scale samples. Everything began to change as higher fields and higher frequencies came into use, and the earlier idyllic simplicities began to seem quaint. The trend continued, however, driven by the increased signal to noise ratios and the resultant higher resolution and speed available, and sophisticated engineering became more and more essential, not only for magnets but for gradient systems and radiofrequency transmitters and receivers, but also for better software for modeling and correcting distortions. Experts who had said, and even written, that frequencies above 10 MHz would never be practical watched in amazement as scientists and engineers pushed instrument performances to ever-higher levels at ever-increasing magnetic field strengths, as this volume demonstrates" [18].

Prior to assembling the 8 Tesla instrument, Professor Robitaille envisioned that his career would remain firmly grounded in MRI. However, the first results at 8 Tesla relative to RF power requirements in MRI profoundly altered his scientific outlook. He began to think about MRI as a thermal process. In the early days of NMR, the T1 relaxation time was referred to as the "thermal relaxation time". As a result, Professor Robitaille advanced the idea that, if MRI was thermal process, it should be possible to extract the temperature of the human head using the laws of thermal emission, in the same manner that Penzias and Wilson had measured a temperature of ~3 K for the microwave background [19]. Unfortunately, such an approach yielded a Wien's displacement temperature of less than 1 K for the human head. Surely, something was incorrect.

Professor Robitaille viewed magnetic resonance as enabling scientists to examine the reverse of the emission problem in the infrared, as studied by Planck and his predecessors [20]. Therefore, he turned his attention to thermal radiation and astrophysics. Soon, he published an abstract which questioned the assignment of the microwave background to the cosmos [21]. Then, in a bold step, he placed an ad in the New York Times [22] announcing the Collapse of the Big Bang and the Gaseous Sun. The response from the popular press and the scientific community was immediate and sometimes harsh [23–25]. Despite claims to the contrary [23], Professor Robitaille's advertisement in the New York Times had nothing to do with the concurrent debate in Ohio relative to evolution [23]. The timing was purely coincidental.

Following the ad in the New York Times, Professor Robitaille turned to Progress in Physics and began outlining his ideas in a series of papers which spanned a very broad area of fundamental physics. His papers on the WMAP [26] and COBE [27] satellites are amongst the most viewed by the journal audience and, eventually, his position was found to merit some consideration by the astrophysics community [28].

The study of Kirchhoff's Law of Thermal Emission has been the driving force behind Prof. Robitaille's work in astrophysics. Robitaille has demonstrated the invalidity of this law and its subsequent claims for universality [29–33]. Prof. Robitaille has also argued that the proper analysis of thermal emission should be attributed to Balfour Stewart [32]. Resting on the knowledge that Kirchhoff's Law was invalid, Robitaille argued for a liquid model of the Sun [34] and advanced simple proofs to strengthen his position [35]. Robitaille maintains that the emission of a thermal spectrum from the Sun, by itself, comprises all the proof necessary for a liquid model. Given the error within Kirchhoff Law, the Sun cannot be a gaseous plasma. It must be condensed matter.

Robitaille has also based his re-assignment of the microwave background to the Earth on Kirchhoff's Law [28]. He has shown that astrophysics did not properly consider the emission of water itself when contemplating the background [36, 37]. His recent paper analyzing the Planck satellite [38] further builds on his position, along with papers by the authors, Rabounski and Borissova [39]. Finally, Robitaille has questioned the validity of Boltzmann's constant [40]. This is the result of the correction of Kirchhoff's Law [28, 29–33] and the re-assignment of the microwave background to the Earth [36–39].

Robitaille maintains a quite lifestyle in Columbus, Ohio. He has been married to Patricia for 30 years, and they have three sons: Jacob, Christophe, and Luc. Dr. Robitaille enjoys sailing his Flying Scot and is an avid builder of timberframe structures.

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