

Stellar equilibrium vs. gravitational collapse

Carla Rodrigues Almeida^a

Department I Max Planck Institute for the History of Science, Boltzmannstraße 22,
14195 Berlin, Germany

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Abstract. The idea of gravitational collapse can be traced back to the first solution of Einstein's equations, but in these early stages, compelling evidence to support this idea was lacking. Furthermore, there were many theoretical gaps underlying the conviction that a star could not contract beyond its critical radius. The philosophical views of the early 20th century, especially those of Sir Arthur S. Eddington, imposed equilibrium as an almost unquestionable condition on theoretical models describing stars. This paper is a historical and epistemological account of the theoretical defiance of this equilibrium hypothesis, with a novel reassessment of J.R. Oppenheimer's work on astrophysics.

1 Introduction

Gravitationally collapsed objects are the conceptual precursor to black holes, and their history sheds light on how such a counter-intuitive idea was accepted long before there was any concrete proof of their existence. A black hole is a strong field structure of space-time surrounded by a unidirectional membrane that encloses a singularity. General relativity (GR) predicts that massive enough bodies will collapse into black holes. In fact, the first solution of Einstein's field equations implies the existence of black holes, but this conclusion was not reached at the time because the necessary logical steps were not as straightforward as they appear today. The theoretical and philosophical advances essential to take those steps arrived over the following two decades. This paper addresses the question of how these advances happened and what had to change in order for the idea of gravitational collapse to become plausible.

Though understood today as a strange consequence of general relativity, the idea of an astronomical body that traps light, or *dark stars*, was first considered in a Newtonian framework by both John Michell and Pierre Simon Laplace in the late 18th century. Their reasoning required light to be a massive particle, thus they dropped the idea after the discovery of the interference of light phenomenon, suggesting that light was a wave as opposed to a particle. It was only with Albert Einstein's general theory of gravitation in 1915 that the effects of gravity on light would be properly explained. Karl Schwarzschild gave the very first solution of Einstein's equations only one year after the birth of general relativity. It was the solution for a spherically

^a e-mail: calmeida@mpiwh-berlin.mpg.de

symmetric configuration with the presence of a *massive point*, *Massenpunktes*, and it foresaw two regions where the solution became ill-defined: a singular radius, the Schwarzschild radius, and the origin of the coordinate system.

In his paper [Schwarzschild 1916a], Schwarzschild comments that the punctual mass is an idealization, and in nature there could not be a body with such a high density. J. Droste independently found the same solution [Droste 1917], publishing it the following year. About the singular radius, he states that a “moving particle (...) can never pass the sphere” [Droste 1917, p. 201] because it “would require an infinite long time” [Droste 1917, p. 206] to do so. The relation between coordinates and singularities was still not well understood, so these findings served as reasons to avoid seriously considering the interior region of the sphere delimited by the singular radius, and the Schwarzschild radius came to be considered an impassible barrier.

On the observational side, astronomers were drawing a correlation between the density and luminosity of stars, concluding that the greater their density, the fainter they become. In the 1920s, stars with faint luminosity but with a mass similar to the sun received the name *white dwarfs* and their brightness implied a density that had never been observed before. This led to a survey for more of these objects by Williem J. Luyten in 1921. The subject got the attention of the renowned British astronomer Arthur S. Eddington, who proposed a star model that was the starting point of a debate on the properties of massive bodies over the following decades. This debate paralleled the attempt to validate general relativity by observation, in which Eddington was also involved, leading the expedition that in 1919 measured the deflection of light as estimated by Einstein. As a brilliant astronomer and a great advocate for general relativity, Eddington quickly understood that considering matter compressed into higher densities would lead to strong theoretical ramifications of gravitational *collapse*, which would imply the existence of regions closed within themselves, apart from the rest of the universe. As a philosopher, he could not accept this idea.

It was only in 1939 that J. Robert Oppenheimer and his student Hartland Snyder published a paper entitled *On the Continued Gravitational Contraction*, the first to confirm that gravitational collapse could happen for supermassive stars, a conclusion that would later evolve into the concept of black holes. This article was the third and last of a series published by Oppenheimer and collaborators on astrophysics; he never returned to this subject again. This trilogy—*The Stability of Stellar Neutron Cores*, in collaboration with R. Serber, *On Massive Neutron Cores*, with G.M. Volkoff, and the aforementioned *On the Continued Gravitational Contraction*—represents their increasing defiance of the scientific beliefs of that time, culminating with a rebellious description of the imminent continued contraction of a supermassive star. The conclusion was that in comoving coordinates the star would reach any positive radius in a finite proper time during the contraction, while a distant observer would see the star shrinking into its gravitational radius before losing all communication with its exterior. Oppenheimer’s interest in astrophysics was short-lived and his use of general relativity was controversial, but it was undoubtedly unapologetic and revolutionary.

The history of astrophysical and astronomical researches on dense objects from the beginning of the 20th century up to the 1940s was outlined by Luisa Bonolis in [Bonolis 2017], while Jean Eisenstaedt recounted how the Schwarzschild solution was perceived during the first years after the birth of general relativity in works such as [Eisenstaedt 1982] and [Eisenstaedt 1993]. Werner Israel delineated the evolution of the concept of dark stars into black holes in [Israel 1987], offering a narrative of the history up to the mid-1980s. Israel’s work is a good summary and acknowledgement of the facts leading up to the establishment of the definition of black holes in the mid-1970s and some further developments. His “retrospective look” had the intention of recording some key studies so as to prevent “losing all the detail that will in time become laundered and streamlined by Darwinian selection of citations into a

handful of names and dates of potted histories,” [Israel 1987, p. 199] presenting a state-of-the-art up to that point.

This article proposes a different approach from those just mentioned. The three works by Oppenheimer in astrophysics are today widely regarded as groundbreaking, especially those written with Volkoff and Snyder. They are celebrated as pioneering works in the history of black hole research. It would take over two decades, however, until the physics community could accept the idea of gravitational collapse. The present work is a historical reassessment of their status as pioneers: an analysis of Oppenheimer and his collaborators’ papers on astrophysics that identifies the novelty and the influences on their thinking as well as the later impact of their work. It will focus on the often forgotten contributions made by cosmologists and relativists and try to understand the shift in philosophy that eventually allowed the idea of gravitational collapse to be seen as plausible.

This paper is structured as following: Sections 2 and 3 give a historical account of the scientific research endeavors that led to Oppenheimer’s works, described in Section 4. Section 2 presents advances in the study of the Schwarzschild solution (gravitation) and Section 3 recounts the evolution of theoretical models for stars (astrophysics). After providing this historical context, the philosophical and sociological contexts will be discussed in Sections 5 and 6. Section 7 describes the state of large-scale-structure physics¹ after Oppenheimer’s publications and reactions to his gravitational collapse conclusion, analyzing the relevance of his works on astrophysics. Finally, Section 8 gives a summary of this paper.

2 Impassible barrier – Lemaître’s legacy

Karl Schwarzschild had been thinking about curved spaces since 1900. He opens the paper *On the permissible curvature of space* [Schwarzschild 1900] by saying: “If I presume to present a few remarks that have neither any real practical applicability nor any pertinent mathematical meaning, my excuse is that the topic we are considering has a particular attraction for many of you because it presents an extension of our view of things way beyond that due to our accessible experience, and opens the most strange prospect for later experiences. (...) [W]e are considering the possibility of curvature of space.”² Although he was not convinced that curved spaces would correspond to reality, when Einstein proposed the description of gravity as a curvature in a four-dimensional space, Schwarzschild was already familiar with the mathematics and could obtain a solution for Einstein’s field equations in less than a year.

In the first of two papers released in 1916, *On the gravitational field of a mass point according to Einstein’s theory*,³ Schwarzschild describes the solution for a gravitational field of a punctual body with mass M . This formulation presents two singularities: one at the origin of the coordinate system and a radial one that depends on the value of M . This last one became known as the Schwarzschild radius. Shortly after, the Dutch mathematician and physicist Johannes Droste independently obtained the same solution. Despite Schwarzschild’s and Droste’s comments on this problem, the concept of space-time singularities was not clear; it would evolve only later [Earman 1999, p. 235].

Naturally, regarding the Schwarzschild solution, the nature of the singularities was not a priority at first. Take the work of Paul Painlevé [Painlevé 1921] and Alvar Gullstrand [Gullstrand 1921], for example. They independently found a new set of

¹ Large-scale-structure physics means, in this case, all disciplines that deal with scales greater than Earth, like astronomy, astrophysics, cosmology, and gravitation.

² Translation by John M. Stewart and Mary E. Stewart [Schwarzschild 1998].

³ Translated by S. Antoci and A. Loinger [Schwarzschild 1999].

coordinates that regularizes the radial singularity, but neither addressed the issue of singularities, instead using their findings to criticize Einstein's theory. Painlevé found the solution for the spherically symmetric gravitational field of a mass M but with different coordinates, and accused Einstein's theory of being ambiguous, claiming that the two "different" solutions would encompass different physics [Painlevé 1921, pp. 8-9]. It was an issue with the interpretation of covariance, resulting from a general confusion in the early years of GR. "Not only [Painlevé] is still convinced of the possibility of choosing 'a better' coordinate system, but he believes that the predictions of the theory are sensitive to the chosen coordinate system."⁴ [Eisenstaedt 1982, p. 175]

This new set of coordinates later received the name of Gullstrand-Painlevé (GP) coordinates. The transformation from the GP coordinates to the Droste-Schwarzschild coordinates requires a non-trivial change in the temporal parameter, a difficult step to take since it would require a reinterpretation of time: "physicists were tempted, by favoring a particular coordinate system, to think of these coordinates as a physical measure, as a distance and not only as a reference."⁵ [Eisenstaedt 1982, p. 176] Gullstrand, on the other hand, said that GR would provide inconclusive results concerning the perihelion of Mercury [Gullstrand 1921, p. 13].

In 1924, Eddington proposed—and correctly interpreted—a temporal coordinate transformation for the Schwarzschild solution. He identified the confusion regarding covariance in a comparison between Whitehead's⁶ theory of gravitation [Whitehead 1922] and the general theory developed by Einstein. Considering two different coordinates, given by the line-elements dJ in Whitehead's theory and ds in Einstein's, with different time parameters t_1 and t , he writes: "Divergence can only arise in problems involving the exact metrical interpretation of the symbols—e.g. the shift of spectral lines. In Einstein's theory, the time as measured by a clock is ds , and neither dt_1 nor dt is pre-eminently the 'time.'" [Eddington 1924a]

He proposes that dJ and ds are the same, connected through a coordinate transformation, and he gives a proper interpretation of the time parameter in both cases: "Schwarzschild's t_1 corresponds to a synchronisation of time, at different parts of the solar system, by the condition that the outward velocity of light is equal to the inward velocity; Whitehead's t corresponds to a synchronisation by the condition that the outward (but not the inward) velocity is constant throughout the system." Eddington's coordinates also remove the condition of singularity on the Schwarzschild radius, but he too did not address this issue in the paper. Eddington's work on astrophysics and approaches to the subject by two of his general relativity students, Yusuke Hagihara and George Lemaître, suggest that he avoided touching upon the subject on purpose, as will be exposed later.

Japanese astronomer Yusuke Hagihara went to study at Cambridge in 1923 under Eddington and the English mathematician Henry F. Baker. In 1931, when he was already back in Japan, Hagihara wrote a treatise entitled *Theory of the Relativistic Trajectories in a Gravitational Field of Schwarzschild* [Hagihara 1931], in which he presented a study on the trajectories of particles under the influence of the gravitational field of a spherically symmetric astronomical body. He calculated that radial trajectories reach the gravitational radius in a finite proper time, but "the coefficient of dt^2 of Schwarzschild's line element square (...) vanishes at $r = \alpha$ [the gravitational radius]. This shows that the velocity of light becomes zero at this place. As r passes

⁴ "Non seulement il est encore convaincu de la possibilité de choisir 'un meilleur' système de coordonnées mais il croit que les prévisions de la théorie sont sensibles au système de coordonnées choisi."

⁵ "(...) des physiciens sont tentés, en privilégiant un système de coordonnées particulier, de penser ces coordonnées-là comme mesure physique, comme distance et non pas seulement comme repérage."

⁶ English philosopher and mathematician Alfred North Whitehead.

through this value, that coefficient changes its sign, the result being the same as to take t imaginary. This is inadmissible by the principle of relativity. Hence the motion for $r < \alpha$ should be excluded (...) [and] the region $r < \alpha$ does not belong to our world of events.” [Hagihara 1931, p. 107]

Hagihara justifies this statement by citing the improbability of matter condensing into great densities. “In order that the radius of a star with its mass comparable with our Sun be equal to the distance $r = \alpha$, its density ought to be about 10^{17} times that of water, while the densest star, the companion of Sirius, a white dwarf, the density is about 6×10^4 times that of water. There is no such diversity in the masses of the stars as to overcome this tremendous high magnitude of the critical density. Therefore the orbit inside $r = \alpha$ is physically highly improbable. Hence even the [radial trajectory of a particle moving along a Schwarzschild gravitational field] is physically a collisional orbit or an ejectional orbit, because the radius $r = \alpha$ is situated completely inside the star.” [Hagihara 1931, p. 107] His words still strongly suggest not only the idea of an impassible barrier but also disbelief in the contraction of matter into higher densities.

A friend of Hagihara and another student of Eddington, the Belgian priest Georges Lemaître, went further on the subject. Lemaître went to Cambridge in 1923 to study general relativity under Eddington. In the following year, he went to Massachusetts, attending many conferences in the USA and Canada before obtaining his Ph.D. degree at Harvard University in 1925 with his work on interesting problems in astronomy using GR, such as the De Sitter universe and pulsating stars.⁷ In 1927 Lemaître published a paper in French—with an English version arranged by Eddington in 1931—proposing a dynamical model of the universe in contrast to Einstein’s stationary model, an earlier version of his more famous paper entitled *The expanding universe*, published in 1933 [Lemaître 1933].⁸

On the subject of the Schwarzschild solution, Lemaître uses Friedmann’s solution for an expanding universe to “show that the singularity of the Schwarzschild exterior is an apparent singularity due to the fact that one has imposed a static solution and that it can be eliminated by a change of coordinates” [Lemaître 1997, p. 643]. This is the definitive answer to whether or not the Schwarzschild radius is an impassible barrier. General relativity does not forbid a body to contract beyond its gravitational radius, so the remaining question is, ‘What does prevent the continued contraction?’ Lemaître suggests that the answer lies in nuclear physics: “only the subatomic nuclear forces seem capable of stopping the contraction of the universe, when the radius of the universe is reduced to the dimensions of the solar system.” [Lemaître 1997, p. 679]

Lemaître travelled throughout the United States and Canada propagating his work. In the early 1930s, he spent some time with Howard P. Robertson at Princeton. Inspired by him,⁹ Robertson did an extensive study on the properties of Schwarzschild gravitational fields. With another coordinate change he also reached the conclusion that the singular radius is only apparent and proceeded to study the inward and outward trajectories of particles under the influence of this gravitational field. Robertson did not seem convinced that a body could exist with the density required for the Schwarzschild radius to be in its exterior, but he considered the possibility nevertheless. “Suppose that there existed an object which was smaller than its Schwarzschild radius 2μ . There are no known objects with so small a size, but suppose that somehow a star or a galaxy had collapsed to a size smaller than its Schwarzschild radius.” [Robertson 1968, p. 273]

⁷ For more on Lemaître’s life and work, see [Eisenstaedt 1993] and [Lambert 2015].

⁸ English version by M.A.H. MacCallum [Lemaître 1997].

⁹ In a letter to Synge by the end of the 1930s, Robertson cites Lemaître when discussing the subject, which indicates that he was a major influence for Robertson.

Because of this approach and the acknowledgement of Lemaître’s results, Robertson’s work takes a very different form from that of Hagihara: more complete and more precise. He focuses on the properties of the metric, and he does a thorough analysis of the trajectories of particles under the influence of Schwarzschild field. “[A]s the particle approaches the Schwarzschild radius (. . .) the observer’s time T of observation approaches $+\infty$. The observer never sees the particle reach [the gravitational radius], although the particle passes [it] and reaches $r = 0$ in a finite proper time!” [Robertson 1968, p. 252] Robertson also noticed that one can change the attractor nature of the central singularity to obtain a repulsive singularity.¹⁰ This indicates that the Schwarzschild solution was described through an incomplete topology, but he did not go further with this topological analysis. In fact, despite giving lectures and seminars on the subject, he never published his findings. It was published posthumously by one of his students and collaborators, T. W. Noonan.

Among the places that Lemaître visited during that time, perhaps the most susceptible to his influence was California. There he had insightful discussions with Edwin Hubble and Richard Tolman in the beginning of the 1930s. While it is undeniable that the achievements of Lemaître were of major importance to cosmology,¹¹ his interactions with Richard Tolman had a greater impact on the history of the Schwarzschild solution. Tolman became very open to Lemaître’s ideas, and soon after their encounter he published a paper “employ[ing] expressions for the [Schwarzschild] line element and its consequences which are equivalent to those recently developed by Lemaître for investigating the formation of nebulae.” [Tolman 1934a, p. 170] Considering comoving coordinates and Lemaître’s pressure-free model, he questions if homogeneous models are a good approximation for cosmological models, a procedure justified by its mathematical simplicity and by the latest measurements of matter distribution obtained by the telescope on Mount Wilson, where Hubble worked.

Naturally, Tolman also suggests that the singularity at the gravitational radius is only coordinate-related. He remarks that one can obtain “any desired initial relation between the radial coordinate r and distances as actually measured from the origin.” [Tolman 1934a, p. 172] But he does not dwell on this, and his studies mainly concern inhomogeneities in cosmological models, arguing against the “existence of any kind of gravitational action which would necessarily lead to the disappearance of inhomogeneities in cosmological models.” [Tolman 1934a, p. 175] In the same year, Tolman, a chemist by training, published a book entitled *Relativity, Thermodynamics and Cosmology* [Tolman 1934b], with the first account of thermodynamics using general relativity—using Lemaître’s model, to be specific. We will discuss more about it later.

Although Lemaître was not the first to obtain a coordinate transformation that regularizes the Schwarzschild singularity, he was the first to recognize that this could be done. Lemaître’s work showed that the impenetrability of the Schwarzschild radius was not an issue. The “magic circle,” as Eddington called it [Eddington 1920a, p. 98], is not an impassible barrier. Although this fact is relative to the laws of motion, saying nothing about the forces that could counter-balance gravity, Lemaître’s work made clear that one could no longer justify the presumable non-existence of a body with a radius smaller than Schwarzschild’s using GR. Eddington himself changed his stance after 1924, arguing in favor of nuclear physics to prevent the gravitational contraction. Then the problem became one of an astrophysical nature.

¹⁰ Later, this repulsive solution would evolve into the concept of *white holes*.

¹¹ The International Astronomical Union (IAU) recognized Lemaître’s influence on Hubble on 29 October, 2018, when it renamed the old law for the motion of astronomical objects due to the expansion of the universe to acknowledge Lemaître’s contribution. It has been renamed the Hubble-Lemaître Law.

3 Stars in equilibrium

So far we have discussed whether a particle moving along the Schwarzschild gravitational field can cross the singular radius or not. The question remains if nuclear forces *can* counter-balance the gravitational pull towards the interior of a star, that is, if stars will remain in equilibrium. However, on an interesting epistemological note, physicists at the time tried to address the problem by asking *how* nuclear forces can counter-balance gravitation, indicating that equilibrium was considered a fundamental property. To answer these questions, physicists adopted mainly two different approaches. On one hand, some would rely on gravitational forces in a star model with an oversimplified internal constitution, while those concerned with astrophysical properties would work on the ‘self-equilibrium’ of more complex stellar structures, disregarding gravity in the process.

The Schwarzschild solution was obtained for a massive point at rest, considered by Schwarzschild himself to be an idealized model. His follow-up paper, *On the gravitational field of a sphere of incompressible liquid, according to Einstein’s theory* [Schwarzschild 1916b]¹² presented the problem by considering a spherical body made of incompressible fluids—in his view, a more realistic application. His reasoning for considering specifically an incompressible fluid was that general relativity requires information not only on the quantity of matter but also on its energy. [Schwarzschild 1916b, p. 189] This choice provided a precise and simple energy-momentum tensor as an example of application. Schwarzschild assumed a static solution for the exterior, in this case the same as what he obtained before for a *Massenpunktes*. In 1923 George D. Birkhoff proved the more general case: the gravitational field outside any spherical distribution of matter is the Schwarzschild gravitational field. [Birkhoff 1923]

To derive conclusions about the density of the spherical body of incompressible fluids, Schwarzschild considered the interior of the star to be an homogeneous distribution of matter, the spatial metric of which turned out to be of constant positive curvature.¹³ The exterior—without matter—is described by the same solution as the one obtained for the *Massenpunktes*. In his calculations, he found that pressure plus density grows proportionally to the velocity of light at the interior of the body (in those coordinates), and noticed a break of continuity when the velocity reaches $8/9$ of the velocity of light at the gravitational radius. “This value sets the upper limit of the concentration; a sphere of incompressible liquid cannot be denser than this.” [Schwarzschild 2008, p. 31] This implies that a sphere cannot have a radius smaller than the singular one. Once again, a problem with the the choice of coordinates. Robertson similarly used the field equations to reach conclusions for the spherical distribution of a perfect fluid. “[S]ince we do not know the equation of state, we are forced to turn to the field equation.” [Robertson 1968, p. 252] However, Robertson found a limiting value for the radius instead of the density: a sphere of incompressible fluid cannot have a radius smaller than $9/8$ of its gravitational radius.

From a different perspective, the search for white dwarfs in the beginning of the 1920s was the basis of great progress in the study of stellar interiors, which Eddington expounded a few years later. Bonolis writes that “Eddington’s ‘standard model’ of stellar structure based on stars for which the perfect-gas law held and energy transport via radiation prevailed, yielded information on temperature and density in the interior of main-sequence stars and it was realized that ideal-gas equation of state was a good approximation for all these stars.” [Bonolis 2017, p. 318] This proposition marked the beginning of a long debate in astrophysics surrounding three main points: luminosity, energy production, and the stability of stars. At that time, he instigated

¹² Translation by Larissa Borissova and Dmitri Rabounski [Schwarzschild 2008].

¹³ I sincerely thank the referees for helping me with this information.

this debate by stating that gravitational forces would be annulled by “[e]lectrical-pressures, gas-pressures, and radiation-pressure” [Eddington 1924b, p. 321] inside the star, thus maintaining equilibrium.

Eddington’s star model, which he developed over the next years, became the standard model for research on star evolution and its properties. It did not make use of GR or the Schwarzschild solution, instead relying on thermodynamics to ensure equilibrium. He states that “[t]here is no theoretical reason to expect a change in stellar conditions as the star reaches a density 0.1–1; and the indications here found, that the condition of a perfect gas persists up to higher densities, are not to be dismissed as incredible. It would have been more puzzling if we found that it did not persist.” [Eddington 1924b, p. 322] With a GR-free approach, even those who did not sympathize with Einstein’s theory could have an opinion on the stability of stars. For those who embraced GR, Eddington’s model gave them another reason to avoid thinking about the Schwarzschild radius. It was buried inside the body where the Schwarzschild solution was no longer valid.

Also at Cambridge, Ralph Howard Fowler and Edward A. Milne started to apply statistical mechanics to investigate stellar atmospheres in 1923 [Fowler 1923], and after Eddington’s 1920 paper on the constitution of stars [Eddington 1920b], the enthusiasm around new developments in quantum mechanics and Fermi-Dirac statistics¹⁴ led Fowler to publish the first article applying them to an astrophysical context in 1926 [Fowler 1926]. By the end of the decade, Milne challenged Eddington’s perfect gas model by arguing that it would be unstable and that it would not explain the white dwarf configuration. “The present paper shows that a perfect-gas star in a steady state is in nature an impossibility, and that actual stars must either possess a small but massive core of exceedingly high density and temperature, or else must be almost wholly (that is, save for a gaseous fringe) at a very high density.” [Milne 1930, p. 4] The instability that Milne comments on was not caused by gravity, but rather internal reactions.

Opposing Eddington’s idea, Milne argued that the energy generation process and stellar equilibrium are independent. Of the two assumptions made by Eddington—using a perfect gas to model stars and considering thermodynamic equilibrium—Milne challenged the perfect gas structure in favor of equilibrium. Thus, he analyzed the stellar equilibrium first and then the energy production problem. He asked what the possible structures of a star can be in a steady state, trying to determine if there is any necessary relation between luminosity, mass, and radius that appears only under the conditions of equilibrium. “I wish to make it clear that the question of the physics of the energy-generating process and the question of stellar equilibrium are two separate matters in the first instance, and we can only make progress with either by so considering them *in the first instance*. We first make an analysis of stellar equilibrium; lastly, we must make a grand synthesis and consider the two together. The present paper is almost entirely devoted to the analysis of equilibrium.” [Milne 1930, p. 5] By steady state, he means that “each particle of the mass of material remains constant in position and constant in temperature,” [Milne 1930, p. 6] with a footnote that rotating stars were not under consideration.

Milne’s treatment of equilibrium as a fundamental property is understandable since he is primarily thinking about white dwarfs, even though he generalizes the results afterwards. This assumption allows him to analyze steady configurations of an arbitrary mass M , which is kept constant as the “total source-strength” L varies from zero upwards. On this quantity L , Milne says that “the total radiation to space is the prescribed amount L per second” [Milne 1930, p. 7], and from that point on he constantly interchanges the concepts of luminosity and energy. He concludes that, for steady states to exist, there should be a core with a minimum energy L_0 , in his words

¹⁴ Formulated independently by Enrico Fermi and Paul A.M. Dirac in 1926.

the *critical luminosity*, in which the laws of a perfect gas break down. If the observed energy L is such that $L > L_0$, then the configuration is “centrally condensed,” and if $L_0 > L$, it would be “collapsed.” Those correspond, he says, to the observed division of the stars into “ordinary stars” (giants and dwarfs) and “white dwarfs” [Milne 1930, p. 4]. This is the first time the term *collapse* is used in an astrophysics context. Milne remarks that he will “make no attempt to evaluate L_0 for actual stars” since “the evaluation of L_0 is a difficult business, as it requires an accurate knowledge of κ [the coefficient of absorption] right up to the boundary of the star.” [Milne 1930, p. 13]

Milne also adopts a perfect-gas configuration, but in a highly collapsed configuration to ensure stability. “From the results of many investigators, in real stars the state will be one of high ionization, the material consisting of stripped nuclei and free electrons. It will be as nearly the ‘ideal gas’ of theory as can occur in nature (apart from electrostatic corrections), but owing to its high density it will be an ideal gas in a ‘degenerate’ state (...). We use the term ‘degenerate’ as referring to the Fermi-Dirac statistics.” [Milne 1930, p. 30]. In an attempt to understand the relation between luminosity and radius for an arbitrary mass, Milne uses equations of mechanical equilibrium. The solution for pressure and density are given by *Emden polytropes*, which are power law relations between pressure and density with exponent n , the index of the polytrope. They can be interpreted as generic equations of state. The choice to investigate radius and luminosity as a function of the mass is the point that the young physicist Subrahmanyan Chandrasekhar would later oppose, treating mass as a function of luminosity.

Chandrasekhar showed high proficiency in physics during his undergraduate years in India. He was rewarded with a government scholarship in 1930, and he left Bombay to study under Fowler at Cambridge. He was 19. Having read Eddington’s and Fowler’s works on astrophysics, he wrote his first paper during this trip, a short report entitled *The Maximum Mass of Ideal White Dwarfs* [Chandrasekhar 1931a]. Later that year, he expanded it into *The Highly Collapsed Configurations of a Stellar Mass* [Chandrasekhar 1931b], where he used Milne’s core model focusing on collapsed configurations, that is, only stars with cores. In contrast to Milne, Chandrasekhar argues that relativistic effects should be predominant on a highly collapsed electron gas whose mass is above a certain limit, and thus one cannot consider an arbitrary mass. He then reinterprets Milne’s calculations while taking into consideration this mass constraint, re-obtaining Milne’s results both for *non-relativistic degenerate cases* and *completely relativistically degenerate configurations*, focusing on the mass and fixing the luminosity instead. For this last case, he finds that the solutions provide a maximum density for the star [Chandrasekhar 1931b, p. 461], solidifying the result of his first paper.

After establishing the properties of non-relativistic and completely relativistic degenerate cores, Chandrasekhar analyzes both composite configurations, a degenerate envelope with a non-relativistic core and with relativistic cores. He obtains the limiting masses for each case to happen and concludes that “the completely relativistic model considered as the limit of the composite series is a point-mass with $\rho_c = \infty!$ ” At the same time, he acknowledges that he obtained this result for the equation of state given by the standard model of dynamic equilibrium, the Emden polytropes, and “[w]e are bound to assume therefore that a state must come beyond which the equation of state (...) is not valid, for otherwise we are led to the physically inconceivable result that for $M = 0.92 \odot \beta^{-\frac{3}{2}}$, $r_1 = 0$, and $\rho = \infty$.” He finally concludes: “As we do not know physically what the next equation is that we are to take, we assume for definiteness the equation for the homogeneous incompressible material $\rho = \rho_{\max}$, where ρ_{\max} is the maximum density of which matter is capable.” [Chandrasekhar 1931b, p. 463] To Chandrasekhar’s disappointment, his paper was not given the attention it deserved.

Lev Landau was another brilliant student of the era. After graduating from Leningrad State University in 1927, Landau received a stipend in 1929 for a one-and-a-half year trip abroad for scientific collaboration, visiting places like Copenhagen, where he met Niels Bohr, and Cambridge, where he became familiar with Milne's proposal of stellar cores. Although he criticized Milne's mathematical approach in his 1932 paper [Landau 1932],¹⁵ he sympathized with the idea of stellar cores. He also assumed that a star must remain in internal equilibrium. As in the work of Milne and Chandrasekhar, gravitational forces play no role in assessing stability in Landau's work. "It seems reasonable to try to attack the problem of stellar structure by methods of theoretical physics, i.e. to investigate the physical nature of stellar equilibrium." [Landau 1975, p. 271]

To counter-balance Milne's mathematical approach, Landau adopted a perfect gas model and used thermodynamics and the principle of minimum energy in an equilibrium state to argue that it would not achieve a steady configuration. "[I]n the case of classical ideal gas, we obtain no equilibrium at all. Every part of the system would tend to a point." To justify the introduction of a core, he adds that "[t]he state of affairs becomes quite different when we consider the quantum effects." [Landau 1975, p. 271] What Landau called "quantum effects" are what Chandrasekhar called "relativistic effects," and both refer to using the Fermi-Dirac statistical method. Not surprisingly, Landau also finds a limiting mass M_0 for stable configurations. He calls it a *critical mass*. "For $M > M_0$ there exists in the whole quantum theory no cause preventing the system from collapsing to a point (the electrostatic forces are by great densities relatively very small). As in reality such masses exist quietly as stars and do not show ridiculous tendencies we must conclude that all stars heavier than $1.5 \odot$ certainly possess regions in which the laws of quantum mechanics (and therefore quantum statistics) are violated." [Landau 1975, p. 272] Arguing that there should be no reason for stars to differentiate themselves based on their masses, Landau proposed that all stars must have a region where the laws of quantum mechanics break down. This was his interpretation of the stellar core. While Chandrasekhar refused to propose an unknown equation of state, Landau firmly asserted that the known laws of physics should break down. Landau's results differ from Chandrasekhar's only in terms of the density considered. Today we know that Chandrasekhar's work refers to the density of white dwarfs, while Landau's corresponds to that of neutron stars, although the neutron particle would not be discovered until almost a year later.

In 1935, Chandrasekhar published a follow-up to his first paper titled *The Highly Collapsed Configurations of Stellar Masses (Second Paper)* [Chandrasekhar 1935]. Here, he detailed the results obtained in the first paper and presented a thorough description of the physical characteristics of degenerate spheres and the (mass-radius-)relation for highly collapsed configurations, hoping this time it would get the attention of the scientific community. This time, however, he reconsidered his introduction of a maximum density. "In [the first paper] this 'singularity' was formally avoided by introducing a state of 'maximum density' for matter, but now we shall not introduce any such hypothetical states, mainly for the reason that it appears from general considerations that when the central density is high enough for marked deviations from the known gas laws (degenerate or otherwise) to occur the configurations then would have such small radii that they would cease to have any practical importance in astrophysics." [Chandrasekhar 1935, p. 207] Chandrasekhar was evolving his understanding and confidence on the subject. While in the first paper [Chandrasekhar 1931b] he let open the fundamental question of what happens if matter is compressed indefinitely, the second [Chandrasekhar 1935] presented the implication that the radius will tend to zero as a certain mass limiting mass is reached.

¹⁵ Republished as [Landau 1975].

In fact, Chandrasekhar did get Eddington's attention, but not his approval. Although Chandrasekhar was careful with this conclusion, Eddington understood that it was a serious matter. At a meeting of the Royal Astronomical Society in January 1935, after Chandrasekhar presented his work, Eddington took the opportunity to attack Chandrasekhar's methods [Eddington 1935]. "Using the relativistic formula, he finds that a star of large mass will never become degenerate, but will remain practically a perfect gas up to the highest densities contemplated. When its supply of subatomic energy is exhausted, the star must continue radiating energy and therefore contracting—presumably until, at a diameter of a few kilometres, its gravitation becomes strong enough to prevent the escape of radiation. This result seems to me almost a *reduction ad absurdum* of the relativistic formula. It must at least rouse suspicion as to the soundness of its foundation." [Eddington 1935, p. 195] Eddington targeted precisely the point that Milne, Chandrasekhar, and Landau had left out: gravitational forces.

Although Eddington admitted that "any flaw can be found in the usual mathematical deviation of the formula," he concluded that "its physical foundation does not inspire confidence, since it is a combination of relativistic mechanics with non-relativistic quantum theory." He criticized this "unholy alliance," proposing a purely relativistic account of quantum mechanics to introduce a new equation of state based on Dirac's approach to quantum mechanics. Eddington was able to convince the audience that Chandrasekhar's work would not correspond to reality. In the end, he argued, the introduction of the gravitational force in Chandrasekhar's model would lead to instability and the collapse of heavier stars. In the oral presentation, he famously stated: "I think there should be a law of Nature to prevent a star from behaving in this absurd way!" [Thorne 1994, p. 160] After this backlash, Chandrasekhar gave up on the subject, focusing instead on other astrophysical problems, among other things, and only went back to it decades later.

While at Cambridge the tendency to combine statistical mechanics and special relativity was predominant in astrophysics, on the other side of the globe Richard Tolman was working on something entirely new. He considered "the extension of thermodynamics from special to general relativity together with its applications" and added that "the principle of relativistic mechanics themselves provide a justification for this new thermodynamics conclusion, since they permit the construction of cosmological models which would expand to an upper limit and then return with precisely reversed velocities to earlier states." [Tolman 1934b, pp. 4–5] With the application to cosmology in mind, Tolman acknowledged the effects of gravitation through general relativity in proposing a modification to thermodynamics. He wrote: "Future changes in the structure of theoretical physics are of course inevitable. Nevertheless, the variety of the tests to which the theory of relativity has been subjected, combined with its inner logicity, are sufficient to make us believe that further advances must incorporate enough of the present theory of relativity to make it a safe provisional foundation for macroscopic considerations." [Tolman 1934b, p. 9] Although he did not mention the matter of star structure, his work later became the basis for the research of Fritz Zwicky and J. Robert Oppenheimer on the subject.

4 Collapsed stars

Fritz Zwicky was a Swiss astronomer that immigrated to the United States in 1925, establishing himself at Caltech. Zwicky and Walter Baade, a German astronomer who worked at Mount Wilson Observatory, were the first to suggest the existence of the process of *Supernovae explosions*, which resulted in what they called a *neutron star*.

This was published in 1934 in a brief communication called *Supernovae and cosmic-rays* [Baade 1934]. Later, Zwicky would expand this idea using Tolman's account of thermodynamics, analyzing the collapse of regular stars into neutron stars. In his 1938 paper *On collapsed Neutron Stars* [Zwicky 1938], he states that a "limiting mass of stars exists for every given average density" and adds interestingly that "A star which has reached the Schwarzschild limiting configuration must be regarded as an object between which and the rest of the universe practically no physical communication is possible. For instance, the velocity of light on such a star is infinitely small, so that it requires light from this star an infinitely long time to reach any external point. Also, the gravitational red shift is complete in the sense that light originating on the star arrives at any external point with the energy zero. It is, therefore, impossible to observe physical conditions in stellar bodies which have reached the Schwarzschild limit." [Zwicky 1938, p. 523]

Unlike Chandrasekhar, Zwicky commented on what would happen to a mass greater than this limit; and unlike Landau, he did not exclude the possibility of it happening. His reasoning was purely based on GR, not on relativistic statistical methods. The impossibility of observing physical conditions in this case gave him a reason to avoid analyzing stars beyond the Schwarzschild limiting configuration. Zwicky himself did not have a good understanding of general relativity, and he emphasized the fact that these results were derived in collaboration with Tolman. Although he did not oppose the idea of a completely collapsed object, his results have nothing to say on this matter.

John Robert Oppenheimer had studied under Ralph Fowler at Cambridge University from 1925 to 1926 and finished his Ph.D. at Göttingen in 1929. He also worked with Wolfgang Pauli in Zurich on nuclear physics before settling in California at the beginning of 1930, where he became good friends with Richard Tolman. Around this time, Oppenheimer's interest in the stellar constitution problem increased, and in 1938 he and his collaborator, Robert Serber, published a one-page report on the limiting mass of *neutron cores*,¹⁶ based on Landau's work. *On the stability of neutron cores* [Oppenheimer 1938] suggests that "a condensed neutron core, which would make essential deviations from the Eddington model possible even for stars so light that without a core a highly degenerate central zone could not be stable, still seems of some interest." Then, Oppenheimer and Serber present a correction to Landau's calculations, arguing that "Landau's requirement is unnecessarily severe." Since Landau's work was first published prior to the discovery of the neutron, it is understandable why he placed conditions upon the nuclei that were too severe. In this work, there is no mention of general relativity, but this would change in Oppenheimer's next paper, again written with one of his students.

In February 1939, Oppenheimer and G.M. Volkoff published *On Massive Neutron Cores*, in which the authors reaffirm Landau's results and challenge the thermonuclear equilibrium proposed by Eddington: "Landau showed that for a model consisting of a cold degenerate Fermi gas there exist no stable equilibrium configurations for masses greater than a certain critical mass, all larger masses tending to collapse." They start the paper by clearly laying out their assumptions and goals. "Two objections might be raised against this result. One is that it was obtained on the basis of Newtonian gravitational theory while for such high masses and densities general relativistic effects must be considered. The other one is that Fermi gas was assumed to be relativistically degenerate throughout the whole core, while it might be expected that on the one hand, because of the large mass of the neutron, the nonrelativistically degenerate equation of state might be more appropriate over the greater part

¹⁶ Hufbauer credits Oppenheimer's insistence on using the term *neutron cores* instead of *neutron stars* to the feud he had with Zwicky, since Zwicky was responsible for the latter term. [Hufbauer 2005]

of the core, and on the other hand the gravitational effect of the kinetic energy of the neutrons could not be neglected. The present investigation seeks to establish what differences are introduced into the result if general relativistic gravitational theory is used instead of Newtonian, and if a more exact equation of state is used.” [Oppenheimer 1939a, pp. 374–375] Therefore, they adopted Tolman’s model of thermodynamics to re-evaluate Landau’s work, using GR instead of Newtonian mechanics with relativistic corrections.

Their conclusions are concordant with Landau’s Newtonian analysis, but in this case they were not so quick to dismiss the possibility of gravitational collapse: “There would then seem to be only two answers possible to the question of the ‘final’ behavior of the very massive stars: either the equation of state we have used so far fails to describe the behavior of highly condensed matter that the conclusions reached above [that there is a limiting mass for stars in equilibrium] are qualitative misleading, or the star will continue to contract indefinitely, never reaching equilibrium. Both alternatives require serious consideration.” [Oppenheimer 1939a, p. 381] At the end, the authors give some insight into how pressure can be considered in a way that could prevent the collapse. They also emphasize the need to investigate other scenarios, such as the contraction becoming slower and slower until this solution becomes quasi-static (although not in equilibrium).

The result of these investigations came a few months later in a paper by Oppenheimer and Hartland Snyder, *On Continued Gravitational Collapse* [Oppenheimer 1939b]. As a continuation of the prior works, they assume nonstatic solutions of the field equations and they consider only stars which have exhausted their nuclear source of energy and with masses greater than the limiting mass established in [Oppenheimer 1939a]. “If the mass of the original star were sufficiently small, or if enough of the star could be blown from the surface by radiation, or lost directly in radiation, or if the angular momentum of the star were great enough to split it into small fragments, then the remaining matter could form a stable static distribution, a white dwarf star. We consider the case where this cannot happen.” [Oppenheimer 1939b, p. 455] They also neglect deviations from spherical symmetry. Focusing on the properties of the energy-momentum tensor, they establish that the pressure would not become singular and thus it “is insufficient to support [the star] against its own gravitational attraction (...). The star thus tends to close itself off from any communication with a distant observer; only its gravitational field persists.” [Oppenheimer 1939b, p. 456] So they assume a null pressure to integrate the equations, adopting Tolman’s comoving solution from 1934, which is another take on Lemaitre’s null pressure solution for the universe.

Oppenheimer and Snyder’s final conclusion is that the contraction does not become slower, which would result in a quasi-static solution, but that in fact the time of collapse up to the gravitational radius is surprisingly short: “For a star which has an initial density of one gram per cubic centimeter and a mass of 10^{33} grams this time τ_0 [of collapse into the gravitational radius] is about a day.” [Oppenheimer 1939b, p. 459] They acknowledged the fact that it should be slower due to the effect of pressure, radiation, or rotation. The authors explained that for any radii the comoving time τ remains finite, even if the coordinate time tends to be infinite, and that after the contraction reaches the gravitational radius at the time τ_0 , “an observer comoving with the matter would not be able to send a light signal from the star; the cone within which a signal can escape has closed entirely.” Their choice to focus on the point of view of a distant observer, calculating the time of contraction up to the gravitational radius, shows that Oppenheimer was not willing to speculate any further.

Meanwhile, most likely unaware of Oppenheimer’s papers, Einstein also approached the subject a few months later in his 1939 paper *On a stationary system with spherical symmetry consisting of many gravitating masses* [Einstein 1939], where he calculated the possible paths of masses gravitating in spherical orbit. He

found that the minimum value for the radius of a circular orbit under these conditions is $(2 + \sqrt{3})\alpha$, where α is the Schwarzschild radius. This is a result similar to what Hagihara had already obtained in 1931. Einstein concluded that particles are *bounded* to stay outside of the sphere $r = (2 + \sqrt{3})\alpha$ [Einstein 1939, p. 924]. His mistake was to assume that the model of a cluster of particles in circular orbits was a good approximation for general cases, without even considering its internal reactions. He says: “Although the theory given here treats only clusters whose particles move along circular paths it does not seem to be subject to reasonable doubt that more general cases will have analogous results.” [Einstein 1939, p. 936]

Einstein’s investigations are probably a reaction to Robertson’s findings. Princeton physicist Peter Bergman recalled a conversation between him, Robertson, and Einstein where Robertson “told us that the Schwarzschild singularity (at $r = 2M$) might not be so bad. He used what is known as Finkelstein coordinates (...). In these terms it takes only a finite ‘time’ to get inside, but ‘forever’ to get out. Or the converse. We thought this was important but puzzling.”¹⁷ It seems that Robertson was able to convince Einstein of the apparent nature of the singularity at the Schwarzschild radius, since he states that the reason matter would not concentrate arbitrarily was not connected to the nature of this singularity, but rather to the fact that “otherwise the constituting particles would reach the velocity of light.”

5 The equilibrium hypothesis

The scientific context of the initial phase of investigations on stellar equilibrium and gravitational collapse is now in place, culminating in the works of J.R. Oppenheimer on astrophysics in 1938–1939. As previously revealed, all the elements necessary to derive Oppenheimer’s results were available as early as 1934, with Landau’s 1931 analysis on the limiting mass of what would later be determined to be neutron stars, Lemaître’s 1933 conclusion of the coordinate-nature of the Schwarzschild radius and his null-pressure configuration, and Tolman’s take on thermodynamics using GR in 1934. To understand the gap of five years between these results and the conclusion of Oppenheimer’s papers (as well as their relevance and later impact), it is necessary to situate their work within the social and philosophical context of the physics community of that time. Therefore, the next sections present a brief analysis of this context before addressing these questions directly.

The timeframe known as the low-water mark period of general relativity lasted approximately three decades, from the mid-1920s to the mid-1950s, and the late 1930s was in the midst of this interval. Its abstract character and few experimental possibilities made it difficult to comprehend and accept the theory and also to promote it scientifically. “It is that general relativity appears to many as a difficult (...) and ungrateful theory. Difficult to accept philosophically, difficult to understand epistemologically, difficult to interpret physically, difficult to work with technically, difficult to verify experimentally and therefore difficult to promote at the institutional level.”¹⁸ [Eisenstaedt 1986, p. 117] On the other hand, while GR was going through a stagnation period, quantum mechanics thrived, especially in Europe. It was definitely counter-intuitive, but it was more applicable and easier to come up with experiments to verify it. Despite their varying receptions in the scientific community, both theories were well received by philosophers of science. The subject of stellar equilibrium thus

¹⁷ Bergman to Eisenstaedt, 9 May 1986.

¹⁸ “C’est qu’alors la relativité générale apparaît à beaucoup comme une théorie difficile ... et ingrate. Difficile à, accepter philosophiquement, difficile à comprendre épistémologiquement, difficile à interpréter physiquement, difficile à travailler techniquement, difficile à vérifier expérimentalement et donc difficile à promouvoir au niveau institutionnel.”

becomes a good example of how social and philosophical components can constrain scientific research.

The culture of scientific interchange was strong within Europe, and it provided a way for the scientific community to stay up-to-date on the latest trends. It was common for physicists to spend some time with figures like Niels Bohr at Copenhagen, for example. Considering the research on disciplines of large-scale physics, the rise of quantum mechanics in Europe and the low-water mark period of general relativity turned the focus of the Cambridge community to astrophysics, to the internal constitution of stars, favoring quantum statistical methods instead of GR. Around this time Einstein went to the USA and, together with Robertson and Bergman, created an influential environment for relativists on the East Coast at Princeton University. Despite the stagnation period for GR, Lemaître's work presented interesting features for the Schwarzschild solution, and his close contact with astronomers from Mount Wilson Observatory turned the focus of the Californian large-scale physics community to cosmology and to gravity.

Between the empirical-philosophical tendencies of the time and the focus of the scientific community on more immediately applicable subjects, the issue of gravitational collapse remained on the margins. Though it had been suggested over a century prior, the idea behind the concept of collapsed stars was still very counter-intuitive. First, the density required to create such an object was far from any that had yet been observed. The estimate of the density of white dwarfs was already controversial and was a point of criticism of Eddington's model in the first half of the 1920s [Bonolis 2017]. Another point of discontent was the appearance of singularities in the Schwarzschild solution. Einstein could not bring himself to accept the idea, “[f]or a singularity brings so much arbitrariness into the theory that it actually nullifies its laws” [Einstein 1935, p. 73]. Until the beginning of the 1930s, the theoretical developments suggesting gravitational collapse were weak, with a lot of uncertainties that could potentially annul this conclusion.

In the first half of the last century, physicists had a closer relationship with philosophy. Einstein, for example, had an important role in the development of the logical empiricist movement [Howard 2005]. Likewise, in the 1920s Eddington had written various philosophical books on nature, science, and reality, and his influential work on astronomy and his natural charisma for science communication made him one of the most influential and recognized scientists of that time. He was the one who presented general relativity to the world, bringing Albert Einstein into the spotlight.¹⁹

Eddington had a very particular philosophy concerning nature. In his 1927 book *The Nature of Physical World*, when discussing the implications of the laws of gravity, he writes, “I shall have to emphasise elsewhere that the whole of our physical knowledge is based on measures and that the physical world consists, so to speak, of measure-groups resting on a shadowy background that lies outside the scope of physics. Therefore in conceiving a world which had existence apart from the measurements that we make of it, I was trespassing outside the limits of what we call physical reality. I would not dissent from the view that a vagary which by its very nature could not be measurable has no claim to a physical existence.” [Eddington 1927, p. 152]

Naturally, as his investigations of gravitation and astrophysics progressed, more indications pointing towards gravitational collapse appeared. Eddington fought against them. In 1920 he concluded, as Schwarzschild and Droste had before him, that the Schwarzschild singularity was insurmountable. “We start with r large. By-and-by we approach the point where $r = 2m$. But here (...) [t]here is a magic circle which no measurement can bring us inside.” This was not an unexpected conclusion for him. “It is not unnatural that we should picture something obstructing our closer approach, and say that a particle of matter is filling up the interior.” [Eddington 1920a, p. 98]

¹⁹ I thank Professor Helge Kragh for bringing this point to my attention.

In this quote we see a clear tendency to consider the matter an excuse to avoid the “problem” implied by the Schwarzschild solution.

On the Relation between the Masses and Luminosities of the Stars [Eddington 1924b] was published in March 1924, and *A Comparison of Whitehead’s and Einstein’s Formulae* [Eddington 1924a], where Eddington tackled the covariance interpretation, was released a month prior. He was actively working on the problem of a spherical symmetric system both from a gravitational (using GR) and an astrophysical point of view. The proximity of those publications suggests that he was aware that the Schwarzschild radius was apparent, but chose not to address this issue.

In the end, Eddington better understood the consequences of gravity for dense bodies. “It is rather interesting to notice that Einstein’s theory of gravitation has something to say on this point [density and gravity]. According to it a star of 250 million km radius could not possibly have so high a density as the sun. Firstly, the force of gravitation would be so great that light would be unable to escape from it, the rays falling back to the star like a stone to the Earth. Secondly, the red-shift of the spectral lines would be so great that the spectrum would be shifted out of existence. Thirdly, the mass would produce so much curvature of the space-time metric that space would close up round the star, leaving us outside (i.e. nowhere). The second point gives a more delicate indication and shows that the density is less than 0–001; for even at that density there would be a red-shift of the spectrum too great to be concealed by any probable Doppler effect. (. . .) A luminous star, of the same density as the Earth, and whose diameter should be two hundred and fifty times larger than that of the sun, would not, in consequence of its attraction, allow any of its rays to arrive at us; it is therefore possible that the largest luminous bodies in the universe may, through this cause, be invisible.” [Eddington 1926, p. 6] This is a very thorough description of gravitational collapse, but with a hint of disdain in his words.

Eddington was aware that a collapsed star would lose contact with its exterior, and to him this meant it would be far from the physical reality. This is probably why he chose not to pursue GR in this matter, focusing on nuclear physics instead. As he puts it, “[t]he advance in our knowledge of atoms and radiation has led to many interesting developments in astronomy.” [Eddington 1927, p. 5] The idea that the properties of matter would counter-balance the consequences of gravitational attraction became a constant in his research endeavors. His trust in a yet-to-be-discovered mechanism inside the star that would maintain equilibrium is a reflection of this philosophy.

To avoid an out-of-reach region of the universe, Eddington, perhaps unintentionally, imposed an *equilibrium hypothesis* on theoretical models for stars: the belief that there is something in nature that would prevent them from collapsing under their own gravitational influence. As said before, Eddington’s equilibrium hypothesis was hard to contradict without further assumptions about the constitution of stars. It also appealed to the common sense of the time. It sat well with the empirical philosophy predominant of that era, thus it was easy to accept. Regarding observations, there was no particular reason to question this hypothesis, since no known stars displayed the “ridiculous tendency,” in Landau’s words, of achieving a density even close to what was necessary to create a dark star. Thus, although theoretical research pointed to gravitational collapse, the equilibrium hypothesis became widely accepted.

6 Californian community

On the West Coast of the United States, far from Princeton and Cambridge, astronomical observations led the scientific community to focus more on cosmology than on astrophysics. Mount Wilson Observatory opened up opportunities for great astronomers to settle in California. Among them was Edwin Hubble, who wrote a

major work on the distance of galactic nebulae in 1929 [Hubble 1929], in which he presented the results that now carry his and Lemaître's names. With this tendency to place greater emphasis on cosmology and with Hubble's observations, Lemaître's ideas were welcomed in California. This fact is reflected in the works of Tolman, Zwicky, and Oppenheimer. It is interesting to notice that while in Europe general relativity was largely being ignored by the physics community, in California it found its home, propelling cosmology as a scientific field.

In 1932, Herbert Dingle, another prominent British astronomer, spent a year at the California Institute of Technology, where he met and collaborated actively with Richard Tolman on relativistic cosmology, helping him with some calculations for his upcoming work on relativistic thermodynamics. Dingle's interest in the philosophy of science started in the 1920s and it often prompted him to harshly criticize those whom he called "new Aristotelians" [Dingle 1937a, p. 785], including Eddington, Einstein, and Milne. "There is a vague rumor in the scientific atmosphere that the description of nature given by a theory should not include what is unobservable, and in the absence of a precise statement the requirement is considered to be met by amputation of the forbidden members. It is not even thought necessary to bind the wounds. Once express the principle in a positive instead of a negative form—that a theory should be a logical correlation of experience—and the invalidity of this procedure is apparent." [Dingle 1937b, p. 199] He opposed the tendency of theoretical physicists to subject reality to abstract laws derived only in the human mind, apart from any observation, which he called 'experimental philosophy.' [Dingle 1937a, p. 784]

Dingle especially did not agree with the philosophical views of Eddington, "the most significant of modern philosophers of science," according to him [Dingle 1937b, p. 27]. "I think one can see in Sir Arthur Eddington's philosophy an example of the kind of error into which a brilliant and successful scientist is most likely to fall. Success in scientific theory is won, not by rigid adherence to the rules of logic, but by bold speculation which dares even to break those rules if by that means new regions of interest may be opened up. (...) It seems to me that Sir Arthur Eddington has used as a foundation-stone the trowel which he wields so well. He would not like to have to prove its fitness, but if he were forced to do so I think he would find that the proof would be fatally at variance with the principles on which his super-structure is built. He has accepted without question the old definition of science, with its external world, and tried to make it fit the practice of science which recognizes nothing external." [Dingle 1937b, pp. 346–347]

Dingle was well connected to Tolman and served as a probable source of philosophical questioning for him. At the same time, Tolman's friendship with Oppenheimer led to numerous discussions on the advances of cosmology and astrophysics, and it was Tolman who gave Oppenheimer the right push towards applying general relativity to the problem of gravitational collapse, which Oppenheimer eventually did once he started to supervise doctoral students. There remains the question of why Oppenheimer was the one to put together all the pieces of this problem. Aside from personality traits, his background had a role in it. Oppenheimer had studied at Cambridge under Fowler and was very familiar with the discussion between Eddington and Milne on the constitution of stars. As Hufbauer explains, Tolman was also working towards a description of stars, but his methodical approach and desire to get more general results made him miss the conclusion of collapse [Hufbauer 2005, p. 42]. Oppenheimer reapplied Tolman's partial results to an astrophysical problem he was familiar with.

Summarizing the situation of the large-scale physics community of the time, the stagnation of general relativity right after the advance of quantum mechanics hit those with close connections to Cambridge, and they oriented themselves instead toward the boom of quantum theories arising in Europe. Meanwhile, Einstein was able to create an interesting school of general relativity at Princeton alongside Bergman

and Robertson, which would later re-flourish during the 1950s. In California, however, the situation was different. General relativity was being celebrated through cosmology while the authority of Einstein and Eddington was being questioned. The celebration of Hubble's observations and the criticism of Eddington's philosophy provided by Lemaître and Dingle, respectively, were an active part of the environment in which Oppenheimer developed the notion of gravitational collapse—as opposed to Chandrasekhar, who was in Cambridge, at the *lion's den*.²⁰ The obvious respect and influence of Eddington and Milne in Chandrasekhar's formative years made it impossible for him to go against them in this matter.

7 The aftermath

Oppenheimer and Snyder's proposal of the imminent collapse was published in the *Physical Review* on September 1, 1939. On the same day, the German army invaded Poland, setting another world war in motion. During the years of war, there were no new developments in large-scale-structure physics. Understandably, the mysteries of the cosmos were not a priority for anyone. World War II permanently changed the way physics was produced, especially the relation between physicists and the state.²¹ Oppenheimer became the chief of the Manhattan Project and did not return to astrophysics afterwards. Moreover, the war was not the only change in the scientific landscape. The older generation of prestigious and influential physicists was dying. In 1944 Sir Arthur S. Eddington and Ralph H. Fowler passed away, and the deaths of Edward A. Milne and Albert Einstein followed in 1950 and 1955, respectively.

Also, in 1939 the mechanisms of nuclear fusion [Bethe 1939] and nuclear fission [Bohr 1939] were unraveled by Hans Bethe, and Niels Bohr and John Archibald Wheeler, respectively. After the war these would be the basis for the works of Fred Hoyle [Hoyle 1946, Hoyle 1954] as well as George Gamow and Ralph Alpher [Alpher 1948] on the nucleosynthesis of the chemical elements. These works mark the beginning of high-level debates on cosmology, with Fred Hoyle arguing in favor of Einstein's stationary model and Alpher and Gamow adopting the expanding universe idea to explain the synthesis of the lighter elements. Cosmology was finally being recognized in the large-scale physics community and GR came to its assistance.

Oppenheimer was the first to affirm that the gravitational collapse of supermassive stars could happen, but he did not completely solve the problem. As he mentions in his paper with Volkoff, there are two possibilities left: either the equation of state fails and a new physics must be discovered, or the continued contraction will happen. Oppenheimer's decision to address the collapse seriously made this possibility plausible, challenging the philosophical belief in the equilibrium hypothesis. However, his papers were overlooked for almost two decades, except for by Landau, who promptly put these three papers on his Golden List of the most important physics research articles published anywhere in the world, according to Kip Thorne [Thorne 1994, p. 219]. Oppenheimer's argumentation thoroughly convinced Landau, who once deemed the idea of gravitational collapse absurd. Under his influence, the Russian school accepted gravitational collapse long before anyone else, jumping to the next step in the middle of the 1950s by investigating what a singularity is and if it really occurs in nature, and pairing their studies with the those of the theoretical relativists A.K. Raychaudhuri and A. Komar [Earman 1999].

In the Western world, although Oppenheimer continued to be ignored, the first progress on the subject after the war came with J.L. Synge's *On The Gravitational Field of a Particle* in 1950 [Synge 1950]. In this work, he admits that the idea of a

²⁰ See Section 6 of [Bonolis 2017].

²¹ For more on the way WWII changed the scientific arena, see [Fortun 1993].

body smaller than its Schwarzschild radius is strange, but that “[w]e have the right to ask whether the general theory of relativity actually denies the existence of a gravitating particle” [Synge 1950, p. 84]. In the 1950s, general relativity saw its renaissance [Blum 2016], fueled by the tradition of post-doctoral education created after the war, underwritten by governmental subsidies. By the end of the decade, D. Finkelstein [Finkelstein 1958], C. Fronsdal [Fronsdal 1959], M.D. Kruskal [Kruskal 1960] and G. Szekeres [Szekeres 1960] found analytical extensions for the Schwarzschild solution, all of them regularize the Schwarzschild radius.

Astrophysicists had more difficulty accepting Oppenheimer’s results. John Archibald Wheeler later recalled his sentiments towards this idea: “For years this idea of collapse to what we now call a black hole went against my grain. I just didn’t like it.” [Wheeler 1998, p. 294] At the Solvay Conference in 1958, whose theme was *The Structure and the Evolution of the Universe*,²² Wheeler reported a discussion on the subject with some of his Princeton grad students, who coauthored the article presented during the proceedings [Adams 1958]. He questioned the idealization of the model (spherical symmetry and null pressure) used in Oppenheimer’s work and suggested that some mechanism should exist that would allow the collapsing star to lose the exceeding mass in the form of radiation, thus ensuring stability and preventing a complete collapse. After the talk, Oppenheimer’s personal confidence in his assumptions clashed with Wheeler’s conclusions, and he questioned the need to introduce new, unknown laws of physics only to avoid the continued gravitational collapse when it would be simpler to acknowledge the consequences of the theory. To this, Wheeler replied that it was “very difficult to believe” in such an imminent collapse [Mehra 1975, pp. 147–148].

In essence, Wheeler acted similarly to Eddington when he dismissed Chandrasekhar’s results in 1935, but now in a different atmosphere. In 1935, Eddington spoke of the possibility of collapse in a very sarcastic manner, mocking Chandrasekhar for even suggesting it. Oppenheimer’s unapologetic publication may not have been well received at first, but his thorough calculations and serious considerations made it impossible for people to treat the subject lightly. Wheeler also had the mindset of “there must be something that prevents nature from behaving in such a bizarre way,” but he could not disregard this hypothesis with disdain as Eddington did.

The personality of Oppenheimer and his seriousness towards science becomes clear in this confrontation. Although Oppenheimer argued in the publication that one should consider both possibilities seriously (the breakdown of physical laws that would prevent the continued contraction as well as gravitational collapse), at the Solvay Conference years later he clearly favored the latter. As indicated by this episode at the conference, he was (or became) skeptical of the unknown; he trusted the equations. Apart from the equation-of-state breakdown possibility, deviations from spherical symmetry could still prevent the collapse, but those were problems solved only decades later. Today’s definition of black holes involves a topological understanding of the solutions that are not present in the works of Oppenheimer and his collaborators. His role was to challenge Eddington’s equilibrium hypothesis, establishing the plausibility of gravitational collapse.

Over the next decade, Wheeler became the most enthusiastic advocate of black holes, later popularizing the term. He admitted that Finkelstein and Kruskal greatly influenced him on the subject, and that new developments in computer simulations were crucial for him to accept Oppenheimer’s conclusions. Wheeler’s acceptance signaled the larger scientific community’s acceptance. As Kip Thorne observes, “[t]he torch was being passed to a new generation. Oppenheimer’s legacy would become Wheeler’s foundation; and in the U.S.S.R., Landau’s legacy would become

²² The original in French: *La structure and l’évolution de l’univers*.

Zel'dovich's foundation." [Thorne 1994, p. 238]. The 1960s saw a complete change of mentality, marking the beginning of the relativistic astrophysics era, or, as Thorne calls it, the golden age of general relativity.

8 Conclusion

The beginning of the history of black holes research is marked by purely theoretical work without evidence to support the idea. General relativity was still in its infancy, trying to prove itself to the scientific community. Meanwhile, white dwarfs were the major astronomical discovery, with an estimated density much higher than any observed on Earth. This broke all expectations, and it was hard to rationalize an even higher density. Eddington, an enthusiast of general relativity and one of the most influential astronomers of the time, understood that the problem went deeper than just an unimaginable density. The consequences of such a dense object would be a body that absolutely loses communication with its exterior, and this clashed with Eddington's philosophical view on nature. His proposal of a star model became popular, and it allowed his disbelief in gravitational collapse to persist in the form of an equilibrium hypothesis. This assumption complied with observations and with the philosophical views of that time. Apart from observational reasons, the idea of collapse also presented major theoretical challenges. Singularities were still hardly understood and the apparent nature of the Schwarzschild singularity was only revealed by Lemaître in 1933.

By the year 1934, all the elements necessary to conclude that gravitational collapse was possible were already in place: the mass upper limit of stable white dwarfs and neutron stars, the coordinate nature of the Schwarzschild singularity, and an account of thermodynamics using general relativity. The gap between these theoretical advances and the eventual conclusion that super massive stars could collapse can be explained by the resistance imposed by the philosophical views of that time, as represented by influential people such as Eddington and Einstein. Because the nuclear processes were still not understood, scientists stayed open to the possibility of an unknown mechanism that would prevent gravitational collapse from happening. The 1935 confrontation between Eddington and Chandrasekhar and the publication of Einstein and Rosen's paper against the existence of singularities in the physical reality attest to these figures' disdain for the idea of collapse.

Oppenheimer enjoyed working with nuclear physics, the opportunity to supervise Ph.D. students, and the environment he was in, which was open to exploring directions neglected by other important centers. These factors enabled him to organize all the elements and challenge the implicit equilibrium hypothesis. He and Snyder recognized that Landau's neutron core model was a good approximation of a star in its later stages of thermonuclear energy exhaustion, but they asserted that the phase in which the energy production is still relevant cannot be overlooked, and general relativity must be considered at this stage. As a result, they adopted Tolman's model of thermodynamics to re-evaluate Landau's work, using GR instead of Newtonian mechanics with relativistic corrections. Although there was still no new evidence pointing towards the existence of matter gathered at such high densities, the theoretical advances summarized by Oppenheimer and collaborators made it impossible to exclude the possibility.

As for why Oppenheimer's works were mostly ignored by the scientific community, we followed a different path from Hufbauer [Hufbauer 2005], avoiding the comparison with Bethe's more successful paper [Bethe 1939], since they are very distinct in nature—it is undeniable that the nuclear fusion theory was immediately applicable, while black holes were still highly theoretical. Instead, we focused on the events of

the following years. World War II broke out on the same day that Oppenheimer and Snyder's paper was published, and this event initiated a clear shift in goals for physics research; work on large-scale-structure physics was dropped during those years. In the years following the end of the war, the debate on the nucleosynthesis of elements started by Fred Hoyle and George Gamow heavily relied on cosmological models based on Einstein's theory of gravitation. While Hoyle defended Einstein's stationary model, Gamow was in favor of Lemaître's expanding universe. Both used general relativity as well as Bethe's results on nuclear fusion, which led to the conclusion that the major problem with Oppenheimer's results did not concern the use of GR but rather the lack of possibilities for validating them. Those who accepted this conclusion earlier, in particular the Russian school led by Landau, continued with a more mathematical approach, focusing on singularities, an issue distant from the astrophysical problem originally in focus.

After the war, the state of the scientific community changed considerably, as did the philosophy of its members. Einstein and Eddington's philosophical influence had diminished, in contrast to what Ortega-Rodríguez et al. contend [Ortega-Rodríguez 2017].²³ The lack of observations to support the notion of collapse was another important (and often ignored) aspect of the difficulty in substantiating the gravitational collapse idea. As for our account, Oppenheimer's works on astrophysics grew out of the cosmological advances made by Lemaître and the lack of prejudice towards GR in the Californian scientific community, as well as philosophical changes in the middle of the 1930s. This led Oppenheimer to re-evaluate advances in astrophysics and to admit the plausibility of gravitational collapse, culminating in his groundbreaking description of the continued contraction of a massive star.

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²³ Ortega-Rodríguez et al. give another factor for why the scientific community missed the opportunity offered by Oppenheimer's work to accept the black holes existence, which they summarize in four main arguments, which they called: the “too esoteric” argument, the “not earned the right” argument, the “wrong mindset” argument, and the “wrong relativistic ontology” argument.

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