The Early Scientific Contributions of J. Robert Oppenheimer

Why did the scientific community miss the black hole opportunity?

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Abstract

We aim to carry out an assessment of the scientific value of Oppenheimer’s research on black holes in order to determine and weigh possible factors to explain its neglect by the scientific community, and even by Oppenheimer himself. Dealing primarily with the science and looking closely at the scientific culture and the scientific conceptual belief system of the 1930s, the present article seeks to supplement the existent literature on the subject by enriching the explanations and possibly complicating the guiding questions. We suggest a rereading of Oppenheimer as a more intriguing, ahead-of-his-time figure.

Background

The 1930s witnessed a tremendous growth in our understanding of stars. Not only did Hans Bethe and others solve the long-standing problem of stellar energy production by means of nuclear fusion, but the recently discovered neutron (1932) allowed for speculation about the existence of more extreme physics. In this way, Fritz Zwicky and Lev Landau considered the possibility of stars composed entirely of neutrons. Along similar lines, J. Robert Oppenheimer became deeply interested in the problem of stellar stability, leading to an acute interest in total stellar collapse. Oppenheimer invented the concept of black holes.

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1We have greatly benefited from discussions with Barton Bernstein, which led to the organization of a multidisciplinary conversation with historians, philosophers, and physicists, among others, at Stanford University’s Hansen Experimental Physics Laboratory on January 31, 2014, with a follow-up on January 30, 2015 in the History Dept. These Stanford sessions were themselves continuations of earlier conversations at Universidad de Costa Rica. The insightful and thorough accompanying article by Barton Bernstein (which has the merit that does not shy away from the science) and this paper complement each other and are best read together. To the date March 12, 2017, Barton Bernstein’s paper has not been published yet.
Apparently, though, Oppenheimer’s move was too extreme. Despite the fact that these ideas are considered milestones today, in 1939 and for reasons that are not completely understood they fell into oblivion for two decades, failing to capture the attention of most physicists (Landau being a notable exception).

Freeman Dyson calls Oppenheimer’s black hole work his “only revolutionary contribution to science.” Furthermore, Dyson considers “the outstanding mystery in Oppenheimer’s life” the fact that even Oppenheimer failed to grasp the importance of his own discovery.2

Indeed, Oppenheimer never regained interest in the topic, a potential Nobel winner. When biographer Abraham Pais asked him what his most important contribution to science had been, he referred to his electron/positron work, not a word on astrophysics.3

Main Question: Why Did the Scientific Community Miss the Black Hole Opportunity?

The present multidisciplinary collaboration aims to carry out an assessment of the scientific value of Oppenheimer’s research on black holes in order to determine and weigh possible factors to explain its neglect by the scientific community, and even by Oppenheimer himself.4

Not that there is a lack of easy ways to dismiss, or to address, this question. For example, by arguing that Oppenheimer’s discovery was beyond experimental/observational corroboration and thus scientifically uninteresting. But that answer ignores the fact that physics seems many times not to care about this circumstance, and that theoretical corroborations and elaborations were doable in the 1930s even when the observational ones were not feasible.

We believe, then, that much insight can be gained from plunging into the question of the present paper’s subtitle. This subtitle question hints at another question, namely: What would it have taken for the black hole concept to become an active field of research in 1939?

This article differs from previous treatments of the subject in its emphasis.

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2Dyson, F. (2013, August 15), *Oppenheimer: The shape of genius*, retrieved from http://www.nybooks.com/articles/archives/2013/aug/15/oppenheimer-shape-genius/

3Pais, A. (2006), *J. Robert Oppenheimer, a life*, New York: Oxford University Press, 33.

4This version of the paper can be read without having technical knowledge of general relativity.
This essay a) deals primarily with the science, b) attempts to be situated in time (i.e., forgetting what came after 1939), and c) looks closely at the scientific culture and conceptual belief system of the 1930s, in particular it considers what “good science” meant back then.

This paper therefore complements studies using other perspectives, such as career choices, network analyses (including very counterproductive enmities), German-Jew frustrated liberal idealism, the whole “bag” of personality traits (Oppenheimer’s peculiar intellectual impatient style, his “pathological” interest in everything, his constant desire to be at the center of things, his fierce independence and Sitzfleisch\(^5\) problem), in addition to purely contingent factors: war, anti-Semitism, nationality issues, etc.

**Review of the Literature**

In addition to the well-known biographies of Oppenheimer, the subject of the contextualized stellar science of Oppenheimer has been touched upon by several authors with different backgrounds.

The most detailed account, to our knowledge, is the one given by historian-of-science Karl Hufbauer,\(^6\) which constitutes our starting point (see next section). In addition, the black hole science is briefly commented on books by physicist/journalist Jeremy Bernstein\(^7\) and historian-of-science David Cassidy.\(^8\)

Kip Thorne, an astrophysicist, presents a comprehensive view of the circumstances surrounding the black hole conception, including Oppenheimer’s confrontation with theoretical physicist John Wheeler in 1958 in Brussels.\(^9\) Thorne’s account is the most complete from a scientific point of view with the caveat of being seen through modern eyes.

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\(^5\) According to Dyson (ref. 2), 19, and literally meaning “sit still,” this term refers to Oppenheimer’s inability to sit still and work quietly to finish a difficult calculation.

\(^6\) Hufbauer, K. (2005), J. Robert Oppenheimer’s path to black holes, in C. Carson & D. A. Hollinger (Eds.), *Reappraising Oppenheimer, Centennial Studies and Reflections* (pp. 31–47), Berkeley: University of California, Berkeley.

\(^7\) Bernstein, J. (2004), *Oppenheimer: Portrait of an enigma*, Chicago: Ivan R. Dee.

\(^8\) Cassidy, D. (2005), *J. Robert Oppenheimer and the American century*, New York: Pi Press.

\(^9\) Thorne, K. (1994), *Black holes and time warps: Einstein’s outrageous legacy*, New York: Norton, 209; details of the confrontation can be found in Israel, W. (1987), *Dark stars: the evolution of an idea*, in S. Hawking & W. Israel (Eds.), *300 Years of Gravitation* (pp. 199–276), Cambridge: Cambridge University Press, 229.
Finally, we must mention Freeman Dyson’s lucid review\textsuperscript{10} of biographer Ray Monk’s book on Oppenheimer.\textsuperscript{11} Dyson actually reviews, albeit briefly, the whole Oppenheimer’s science debate.

**Summary of Hufbauer’s Article**

The article by Hufbauer represents, to our knowledge, the most comprehensive historical study of the black hole quest by Oppenheimer.

Hufbauer carefully explicates the path of events that led to the publication of the three relevant papers (Oppenheimer & Serber 1938, Oppenheimer & Volkoff 1939, Oppenheimer & Snyder 1939\textsuperscript{12}), including how Oppenheimer became interested as early as 1933 in high-density stellar physics. This was facilitated by his simultaneous interest and competence in both particle physics and astronomy, a rather American trait. (For the benefit of readers not familiar with the papers, there is a brief description of each in Appendix A.)

In addition, Hufbauer describes Oppenheimer’s efficient use of available resources, including talking to prominent figures like his Caltech colleague Richard Tolman. Hufbauer also describes the way in which Bethe “scooped” Oppenheimer on the topic of stellar energy. Hufbauer then discusses the main results of the Oppenheimer & Snyder paper: not only the surprising collapse, but also how time freezes at the Schwarzschild radius.

Finally, Hufbauer offers five reasons for the early neglect of Oppenheimer’s papers, in the form of a contrast with Bethe’s more successful experience. Unlike Bethe’s research on stars, Oppenheimer a) was not addressing a well-defined problem with a large following; b) had no data and was invoking the little-used theory of general relativity; c) offered a solution that was completely counterintuitive; d) did not reach out to potential audiences; e) published his paper just as the war began.

\textsuperscript{10}Dyson (ref. 2).

\textsuperscript{11}Monk, R. (2013), *Robert Oppenheimer: His life and mind*, New York: Doubleday.

\textsuperscript{12}Oppenheimer, J. R., & Serber, R. (Oct 1, 1938), “On the stability of stellar neutron cores,” *Physical Review*, 54, 540; Oppenheimer, J. R., & Volkoff, G. M. (Feb 15, 1939), “On massive neutron cores,” *Physical Review*, 55, 374–381; and Oppenheimer, J. R., & Snyder, H. (Sept 1, 1939), “On continued gravitational contraction,” *Physical Review*, 56, 455–459.
The Value of Oppenheimer’s Work

There is no real doubt that Oppenheimer’s work on black holes is considered good science according to our modern point of view. The internal logic of the decade-long development of the ideas about denser and denser astrophysical entities is very clearly expounded in Thorne’s book using nontechnical language.\textsuperscript{13} In addition, the citation record of the paper by Oppenheimer & Snyder shows a clear delayed recognition of their ideas, in the 1960s.

The real question is whether Oppenheimer’s work was considered good science according to the standards of the time. A second, related question is whether he was preeminent or not among scientists along this line of research.

The following quote, taken from a long, authoritative (“a bible in the field”) stellar evolution review from 1962 (and therefore written more than two decades after Oppenheimer’s work) is helpful in this respect.\textsuperscript{14}

A possibility of stellar evolution leading to these extremely dense configurations [more dense than neutron stars] may not be denied, but it will be highly more probable that, before the star reaches such a configuration, its mass will be reduced below its Chandrasekhar limit by mass ejection from its surface, due to an increase in the centrifugal force in the course of contraction.

In the sentence immediately preceding this quote in the review, the work of Oppenheimer with Snyder and Volkoff is referenced, but not Albert Einstein’s related article\textsuperscript{15} (described in Appendix A), or any other author’s. This shows that, even when the scientific community as a whole (if we take this comment as representative) still did not believe in black holes, Oppenheimer’s work was considered authoritative. The authors could have easily been more dismissive of Oppenheimer, who had after all disappeared from the field of (what we would now call) astrophysics.\textsuperscript{16}

\begin{itemize}
  \item \textsuperscript{13}Thorne (ref. 9), 187–197 and 209–219.
  \item \textsuperscript{14}Hayashi, C., Hoshi, R., & Sugimoto, D. (1962), “Evolution of the Stars,” \textit{Progr. Theoret. Phys. Supp.}, 22, 95. See also footnote 16.
  \item \textsuperscript{15}Einstein, A. (1939), “On a stationary system with spherical symmetry consisting of many gravitating masses,” \textit{Annals of Mathematics}, 40, 922–936.
  \item \textsuperscript{16}By 1962, Chushiro Hayashi was already 42 years old and a prestigious scholar who had received a Professor appointment at Kyoto University five years before. This strongly reduces the probability of him having paid lip service to Oppenheimer. According to the American Astronomical Society, Hayashi’s review with Hoshi and Sugimoto was considered “...a bible in the field of stellar evolution for a long time, and may be so still.” See https://aas.org/obituaries/chushiro-hayashi-1920-2010.
\end{itemize}
Further proof of Oppenheimer being considered as a preeminent scientist is the fact that Landau allegedly included the Oppenheimer & Snyder paper in his “Golden List” of classic papers in 1939. What we do know for certain is that Landau and Lifshitz cite the work of Oppenheimer with Snyder in their widely read 1951 (Russian) edition of Statistical Physics. (The corresponding English edition came out in 1958, and constitutes to the best of our knowledge the first critical citation of Oppenheimer’s black hole work in the Western World.)

In this Russian book, Landau and Lifshitz fully support the relevant ideas:

Such a study [the one by Oppenheimer and Snyder] has been carried out only for the simplest case of the equation of state $P = 0$, i.e. for a sphere consisting of a very thin substance; it probably gives also a correct indication of the nature of the process for the general case of an exact equation of state [emphasis added].

One should also point out that there is not a single published attack to Oppenheimer’s ideas on black holes until the publication of a paper by Tullio Regge and Wheeler in 1957 (see immediately below), in which the attack is tacit as Oppenheimer is not referenced. The only piece resembling an attack on black holes before 1957 was Einstein’s 1939 paper, but this article faded away quickly. It was not cited until 1953, and then only to be torn apart by Amalkumar Raychaudhuri.

The fact that Oppenheimer’s ideas survived Einstein’s assault is significant. Also significant is the fact that no other scientist published anything else on the subject of black holes until the late 1950s. A careful scrutiny of the articles written by Landau, Zwicky, Bethe, Richard Tolman, George

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17 Explained in Hufbauer (ref. 6), 46 and footnote 77; Thorne (ref. 9), 219.
18 Landau, L., & Lifshitz, E. (1951), Statisticheskaya Fizika, Moscow: Fizmatgiz.
19 Landau, L., & Lifshitz, E. (1958), Statistical Physics, Oxford: Pergamon.
20 There are a few earlier citations of the work of Oppenheimer with Snyder, but these are made in passing and refer not to star collapse but to more normal stellar dynamics. See Johnson, M. (1946), “Atomic possibilities underlying stellar catastrophe,” The Observatory, 66, 248–254; Borst, L. B. (1950), “Supernovae,” Physical Review, 78, 807–808; and Vaidya, P. C. (1951), “Nonstatic solutions of Einstein’s field equations for spheres of fluids radiating energy,” Physical Review, 83, 10–17.
21 Regge, T., & Wheeler, J. A. (1957), “Stability of a Schwarzschild singularity,” Physical Review, 108, 1063–1069.
22 Raychaudhuri, A. (1953), “Arbitrary concentrations of matter and the Schwarzschild singularity,” Physical Review, 89, 417–421.
Gamow, Robert Serber, George Volkoff, and Hartland Snyder show nothing on this.

The first paper dealing with the subject is the one by Regge and Wheeler in 1957 mentioned above, where the authors proposed wormholes as a way to avoid total collapse of the star. It is important to stress that even though this 1957 paper does not reference Oppenheimer explicitly, it is clear that the paper is presented as a criticism of Oppenheimer’s ideas on indefinite contraction: there is a bold and unnecessary emphasis on the concept of “stability” all throughout the paper, including the first word in the title and the last sentence of the paper. A casual reader could be thus forgiven for thinking that the paper is not so much about discussing wormhole physics as being a defense of stellar stability under extreme conditions.

David Finkelstein wrote a paper in 1958 where, though not directly addressing total collapse, he established the Schwarzschild radius as a surface of “no return.” Finkelstein did not reference Oppenheimer either.

In 1960, Wheeler wrote a paper on behalf of Martin Kruskal in which black holes are finally acknowledged. It is significant that Wheeler is not listed as co-author of the paper even though he did the actual writing, and that Oppenheimer went unreferenced one more time.

In addition to these publications, there is unpublished work of Wheeler and (independently) Yakov Zel’dovich in the late 1950s, using computers, as reported by Thorne. One must also not forget about the 1958 Brussels confrontation of Wheeler with Oppenheimer mentioned above.

Four Arguments

We now plunge into the question in the subtitle of this paper: Why did the scientific community miss the black hole opportunity? We note that, even though the five reasons listed by Hufbauer are sensible and generally agreed upon, we believe that they could benefit from being elaborated (as in the “too esoteric” and “not earned the right” arguments below) and extended (as in the “wrong episteme” and “wrong relativistic ontology” arguments).

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23Finkelstein, D. (1958), “Past-future asymmetry of the gravitational field of a point particle,” Physical Review, 110, 965–967.

24Kruskal, M. (1960), “Maximal extension of Schwarzschild metric,” Physical Review, 119, 1743–1745.

25Wheeler, J. A., & Ford, K. (2000), Geons, Black Holes & Quantum Foam: A Life in Physics, New York: Norton, 745.

26Thorne (ref. 9), 197 and 240.
The last two arguments are of a Kuhnian, “history of ideas” flavor and are offered here to complement more conventional approaches. Even if somewhat Foucauldian, they try to provide a fresh perspective on how the conceptual framework of knowing and discovery could have been very different back then.

Before starting, we discuss some general considerations (in the next section) and make the perhaps unnecessary proviso that the four arguments below are not independent among themselves nor with extra-scientific factors.\footnote{Part of the conundrum’s answer is clearly extra-scientific. To give but one example, take Oppenheimer and (Caltech colleague) Zwicky’s refusal even to acknowledge each other’s papers. Oppenheimer never used the word “neutron star.” See Thorne (ref. 9), 206.}

\section*{General Considerations}

\textit{The past is a foreign country: they do things differently there.}
L. P. Hartley

For a trained scientist, the main difficulty in a project like this one is effectively situating oneself in the conceptual framework of the time, forgetting what came after 1939.

For us, a black hole is an exciting opportunity. Back then, it was a nuisance in need of quick repair. Just to begin, think how a physicist living and working in the 1930s would have perceived an intellectual world very different from ours:

In the first place, the disciplinary landscape was very different. There was no “solid-state” discipline, and there were no “astrophysics” or “cosmology” disciplines in the institutional sense, in sharp contrast with the prestigious particle and nuclear physics disciplines. This lack of disciplinary affiliation made it difficult for a person like Gamow, even as late as the 1950s, to find a scientific audience for his cosmology ideas. On top of everything, the United States was not a scientific power like it is today.\footnote{A general reference for the statements made in this section of the paper is Kragh, H. (2002), \textit{Quantum generations: A history of physics in the twentieth century}, Princeton, NJ: Princeton University Press.}

In the second place, most of the related basic knowledge we take for granted today was absent: The mechanism for stellar energy was unknown, only being teased out in 1938 by Bethe and Carl von Weizsäcker. The neutron
was a new thing, and the muon did not appear until 1937. Astronomers had not quite finished digesting the fact that galaxies were not nebula in the Milky Way.\textsuperscript{29}

In the third place, the name “black hole” with all its psychological and metaphorical implications (e.g., a hole lets you move somewhere else—perhaps into new physics) did not exist.\textsuperscript{30} Instead, the literature would talk about “frozen stars,” a quite anticlimactic term.

The “Too Esoteric” Argument

*General relativity was the “string theory” of the 1930s*

“Very odd” is how Oppenheimer described in writing his new results to George Uhlenbeck.\textsuperscript{31} This is probably an understatement.

Three reasons made this odd situation even odder. In the first place, astronomy in general was much more distant from physics than it is today. It had a natural history ring to it. Oppenheimer was working on the margins of physics.

Secondly, the influential Arthur Eddington had given an esoteric twist to astronomy and cosmology, invoking arguments that at times were perceived as too philosophical.

Thirdly, and most importantly, the abstruse character of general relativity did not help either. As late as 1960, Alfred Schild said that “Einstein’s theory of gravitation ... is moving from the realm of mathematics to that of physics”\textsuperscript{32} and even as late as 1958, Wheeler (the eventual champion of black holes) did not feel comfortable at all with the concept of black holes (which led to the famous Brussels confrontation with Oppenheimer that year).\textsuperscript{33}

This situation became worse in the United States, as Cassidy says, where theoretical research was supposed to aid experimentalists, not become in-

\textsuperscript{29}We are grateful to Prof. James Bjorken (Stanford) for his comments on this particular issue and for his interest in this paper’s discussion. He recalls how, as late as 1950, the multi-galaxy idea was still hard to take in general.

\textsuperscript{30}Wheeler thrust the term “black hole” in 1967. See Wheeler, J. A. (1968), “Our universe: The known and the unknown,” *American Scholar, 37*, 248–274. It is important to note also that the term had appeared in print as early as 1964. See Ewing, A. (1964), “Black holes’ in space,” *Science News Letter, 85*, 39.

\textsuperscript{31}Cassidy (ref. 8), 176.

\textsuperscript{32}Quoted in Kragh (ref. 28), 362.

\textsuperscript{33}See footnote (9).
volved in radical, creative, German-style speculations.\textsuperscript{34}

On top of everything, one has to add the fact that Oppenheimer worked with idealized spherical symmetry conditions in his treatment of black holes. Even though this approach is not considered particularly grave today, back then spherical symmetry was considered a special, probably physically irrelevant case.\textsuperscript{35}

One also has to keep in mind the precedent of Eddington’s bashing of Chandrasekhar’s ideas (referred to as “stellar buffoonery”) on the collapse of white dwarfs in 1935.\textsuperscript{36} One may wonder just how influential this case might have been as Oppenheimer was trying to expound his position.

The “Not Earned the Right” Argument

\textit{Intellectual seniority matters}

We could phrase this argument thus: if you had already succeeded at prestigious physics (which in that time meant something nuclear or particle), as in the case of Bethe,\textsuperscript{37} then you earned the right to do something unorthodox in the border of physics and be taken seriously.

Since Oppenheimer liked to be at the center of things, and was (intellectually) moving all the time, he had never quite achieved fame in anything before publishing his paper with Snyder (Oppenheimer’s papers with Max Born in 1927\textsuperscript{38} and Melba Phillips in 1935\textsuperscript{39} had presumably been his most famous, but these papers, with only 15 citations each\textsuperscript{40} in their respective first ten years, could not be called truly revolutionary). This was made worse by his distancing from physics into philosophy, literature and left-wing pol-

\textsuperscript{34}Cassidy (ref. 8), 179.

\textsuperscript{35}We are grateful to Prof. Robert Wagoner (Stanford) for his comment on this particular issue and for his interest in this paper’s discussion. He mentioned that a similar opinion of special-case irrelevance surfaced with Kerr’s solution for black holes. Prof. Robert Wald (University of Chicago), to whom we are also grateful, offered further the case of Big Bang cosmology as an example along these lines of special cases.

\textsuperscript{36}Israel (ref. 9), 217.

\textsuperscript{37}Bethe, H., & Fermi, E. (1932), “Über die Wechselwirkung von Zwei Elektronen,” \textit{Zeitschrift für Physik}, 77, 296–306.

\textsuperscript{38}Born, M., & Oppenheimer, R. (1927), “Zur Quantentheorie der Molekeln,” \textit{Annalen der Physik}, 389, 457–484.

\textsuperscript{39}Oppenheimer, J. R., & Phillips, M. (1935), “Note on the transmutation function for deuterons,” \textit{Physical Review} 48, 500–502.

\textsuperscript{40}Google Scholar Citations.
itics, so this trend of going to extreme stellar physics could be seen as part of a movement away from mainstream physics.

The “Wrong Episteme” Argument

The scientific world had already enough infinities to deal with

Why is it that Einstein never accepted the black hole consequences of his theory? One possibility is that black holes did not belong to the correct episteme of the time.

Michel Foucault uses the term “episteme” to refer to the implicit assumptions about how we know the world. More precisely, it refers to “...the assumptions about knowledge, method, and theory which at any given time period are shared across “discursive formations” (which as a first approximation can be translated as “disciplines”).” An episteme differs from a Kuhnian paradigm in part in that it is transdisciplinary.

These statements are best explained by examples. According to Foucault’s ideas, not just physics but the whole realm of academic knowledge in the beginning of the twentieth century was marked by the episteme of equilibrium and closedness. One sees it in biology (population equilibrium theory), economics (classical, pre-Keynesian theory), linguistics (syntax rather than evolution), the social sciences (structuralism), and physics, as in Bohr’s atom.

To these examples discussed by anthropologist of science David Hess, one may add how Einstein develops his general theory of relativity immersed in this episteme. Einstein’s model of the universe needs (by Einstein’s own later account) an artificial “cosmological term” in order to preserve the equilibrium episteme.

Oppenheimer’s stellar “indefinite contraction” did not belong to this episteme. Was he ahead of his time, sensing the forthcoming episteme of open processes?

It is revealing that Einstein published a paper with the intention of killing the Schwarzschild singularity once and for all. The paper was en-

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41 Foucault, M. (1970), *The order of things*, New York: Random/Vintage.
42 Hess, D. J. (1995), *Science and technology in a multicultural world*, New York: Columbia University Press, 87.
43 Ibid., 94.
44 Einstein (ref. 15).
45 In 1916, Karl Schwarzschild found a solution of Einstein’s equations which were not
titled “On a stationary system with spherical symmetry consisting of many gravitating masses.” It used a stationary argument to show that black holes were impossible. What he actually proved was only that there are no stable solutions to Schwarzschild radius stars (and therefore his original intention was frustrated), but for some reason Einstein thought this proof was sufficient. A case could be thus made for his tacit commitment to the equilibrium episteme.

Along similar equilibrium-episteme lines, Eddington

...like virtually every relativist of the time, considered the Schwarzschild radius to be both a singularity and an impassible barrier. The image that he conjures up of the star ‘at last finding peace’ is of a body frozen at the Schwarzschild radius...

We might speculate what would an out-of-the-equilibrium-episteme attitude look like for a person living within the equilibrium episteme. Perhaps an “equilibrium epistemist” would simply consider a person like Oppenheimer as somewhat lost, not confident, confused. The following 1967 quotation from particle physicist Isidor Rabi (born in 1898, and therefore only six years Oppenheimer’s senior) is useful:

[I]t seems to me that in some respects Oppenheimer was overeducated in those fields which lie outside the scientific tradition, such as his interest in religion, in the Hindu religion in particular, which resulted in a feeling for the mystery of the Universe that surrounded him almost like a fog. He saw physics clearly, looking toward what had already been done, but at the border he tended to feel that there was much more of the mysterious and novel than there actually was. He was insufficiently confident of the power of the intellectual tools he already possessed and did not drive his thought to the very end because he felt instinctively that new ideas and new methods were necessary to go further than he and his students had already gone [emphasis added].

well behaved at certain points. See Schwarzschild, K. (1916), “Über das Gravitationsfeld eines Massenpunktes nach der Einsteinschen Theorie,” Sitzungsberichte Königlich Preuss. Akad. Wiss. Berlin, Phys.-Math. Klasse, 189–196.

46Israel (ref. 9), 219.
47Quoted in Thorne (ref. 9), 208.
Rabi thus felt the need to explain Oppenheimer’s unassertiveness as something having nothing to do with science, but rather with his other, extra-scientific inclinations.

It is appropriate to finish this section with an intriguing comment. A different reading of Wheeler’s initial attitude towards Oppenheimer could be made (and it is one that does not necessarily contradict this article’s main argument) in which it was Wheeler’s strong commitment to a particle physics point view which would have intensified his lack of interest in Oppenheimer’s work. We are referring in particular to Wheeler’s work on geometrodynamics and his aversion to singularities, and on how gravity, considered as part of the particle physics puzzle, could have helped to solve fundamental difficulties in the theory. In such a reading, Wheeler’s disregard of Oppenheimer’s ideas would be less dramatic and more of a pragmatically nature. The details of such a study are to be carried out elsewhere.

The “Wrong Relativistic Ontology” Argument

*The spell of geometry*

The set of ten equations of general relativity,

\[ G_{ab} = 8\pi T_{ab}, \]

can be interpreted in different ways. If one reads them from right to left, then the matter (through the momentum-energy tensor \( T_{ab} \)) determines the geometry (described by Einstein tensor \( G_{ab} \)). If, on the other hand, one chooses to read them from left to right, then geometry would be ontologically primal: geometry dictates how matter must behave.

Even though in either interpretation one must have of course exactly the same equations, from a cognitive point of view, and even from a mathematical point of view, it makes a huge difference what interpretation you adhere to.

The original interpretation was the geometrical one, even to the point that Einstein’s crafting of general relativity is imbued with quite a bit of implicit space-time reification, called “substantivalism” in the literature, a

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48 We are in debt to Prof. Robert Wald (University of Chicago) and Randall Espinoza (University of Illinois at Chicago) for this particular point.

49 Wheeler, J. A. (1957), “On the nature of quantum geometrodynamics,” *Annals of Physics*, 2, 604–614.

50 Janssen, M. (2007), What did Einstein know and when did he know it?, in J. Renn (Ed.), *The Genesis of General Relativity*, vol. 2 (pp. 785–837), Dordrecht: Springer, 825.
curious state of affairs indeed since Einstein was an enemy of absolute space.

In any event, nowadays general relativity applications follow a more “matter first” approach. In this sense, Oppenheimer appears to be again ahead of his time. The crucial point is that the geometrical approach biases your understanding and your problem searching towards more static/stationary situations.

To make this point clearer, consider a system of two masses rapidly rotating around each other. This system will produce oscillating space-time ripples moving away from them. If one starts from the two masses, then it is straightforward to calculate the surrounding oscillating geometry. However, the opposite problem of reconstructing the masses’ movements from the geometrical ripples is a fantastically complicated problem. This is an example of a problem that does not lend itself to be formulated if one starts from a geometrical viewpoint.

The consequence of all this is that your aesthetical judgment (“geometry first”) is going to have an effect on the type of problems you tackle. If you unite this effect with the equilibrium episteme one (described in the previous section) the result is devastating for Oppenheimer, as collapsing stars are thus doubly denaturalized: they are not in equilibrium, and they are not “geometry first.”

Oppenheimer was an outsider to this geometrical ontology. As Cassidy says, “the few active general relativity theorists were interested not in the astrophysics of a star collapsing into a mathematically awkward singularity but in the more elegant and well-behaved geometry of continuous, nonsingular curved space-time” (emphasis added).51

Discussion: Oppenheimer’s Black Hole vis-à-vis Einstein’s EPR Paradox and Zwicky’s Dark Matter

It is helpful to perform a comparison between the black hole idea as developed by Oppenheimer’s group (in 1939) with the Einstein-Podolsky-Rosen Paradox (in 1935) and Zwicky’s concept of dark matter or Dunkle Materie (in 1933).52 (For the benefit of readers not familiar with these scientific concepts,

51 Cassidy (ref. 8), 177.
52 This discussion actually originated in lively fashion during the January 2014 Stanford meeting. Einstein, A., Podolsky, B., & Rosen, N. (1935), “Can quantum-mechanical description of physical reality be considered complete?,” Physical Review, 47, 777; Zwicky, F. (1933), “Die Rotverschiebung von extragalaktischen Nebeln,” Helvetica Physica Acta, 6, 110–127.
there is a brief description of each in Appendix B.)

All three theoretical concepts appeared in the 1930s. They all have in common that the related ideas were put aside for several decades before they were taken seriously, when one could say experiments made them inevitable.

The similarities stop there, though. The EPR Paradox was really not a discovery of a new entity, but rather a gedankenexperiment designed with the sole purpose of pointing out an inconsistency in the looming (for Einstein) conceptual edifice of quantum mechanics. Einstein would have been happy if he had caused the dismissal of the Copenhagen interpretation of quantum mechanics; there was no actual intent, or interest, of carrying out the experiment or having somebody else carrying it out.

Zwicky’s dark matter was also more about pointing out an inconsistency than discovering a new substance. The fact that we are currently, 80 years later, looking for dark matter should not distract us from this point.

Both Zwicky’s dark matter and Einstein’s EPR Paradox are more what one would call “anomalies” in the Kuhnian sense (as is the case of Mercury’s perihelion precession) than true proposals/discoveries of new physical entities or phenomena. This makes a huge difference, since anomalies tend to be treated with respect, and kept along in their unresolvedness.

This is, we believe, what makes the history of Oppenheimer’s black holes much more intriguing than Einstein’s and Zwicky’s counterparts.

Final Words

Something rather interesting happened in physics in the late 1930s. In what was to prove (judged in retrospect) as his last shot at intellectual glory, Oppenheimer, with what might be termed the tacit complicity of the whole physics community, missed a chance to fully discover black holes—not observationally, but theoretically. Says Werner Israel about Oppenheimer’s work with Snyder: “[it] has strong claims to be considered the most daring and uncannily prophetic paper ever published in the field.”\(^{53}\) Thorne says: “This line of reasoning [what happens when a neutron star cannot hold its own weight] is so obvious in retrospect that it seems amazing that Zwicky did not pursue it, Chandrasekhar did not pursue it, Eddington did not pursue it.”\(^{54}\)

We have to add that once it was initially pursued, by Oppenheimer, it was then ignored by the community until the late 1950s, many years after the

\(^{53}\)Israel (ref. 9), 226.

\(^{54}\)Thorne (ref. 9), 178.
war was over.

In this paper we have tried to address the issue of why is it that this new idea did not receive the benefit of the doubt in the same sense that other oddities did (such as many in particle physics, e.g., the uncertainty principle), even though there is plenty of evidence that Oppenheimer’s work was considered authoritative by at least some of his contemporaries, such as Hayashi in 1962 (in addition to what was discussed about Landau above).

In this paper we have tried to go beyond previous discussions on this topic. The way we did so was by adding an additional layer of a more history-of-ideas, Foucaldian nature. We entertained the possibility that at least some of the explanations might have to do with idiosyncratic aspects (of the scientific culture, that is), in particular to a tacit commitment to the equilibrium episteme and a geometry-first ontology. In contrast with the EPR Paradox and the ingenuity of the dark matter concept, the black hole idea is not so much about pointing out an anomaly as adding a new object to our universe.

In this essay, we have sought to supplement the existent literature on the subject by enriching the explanations and possibly complicating the guiding questions. We suggest a rereading of Oppenheimer as a more intriguing, human, ahead-of-his-time figure.

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APPENDIX A
Brief Description of the Relevant Papers

Oppenheimer & Serber, October 1, 1938
*On the stability of stellar neutron cores*

This one-page, no-formula letter is a critique of Landau’s work on “condensed neutron cores”—as it was believed back then that a neutron star (a “neutron core”) could lie in the interior of stars like the Sun.\(^{55}\)

The main point raised by the authors had to do with the necessary inclusion of strong nuclear forces considerations (which were absent in Landau’s papers). This inclusion was problematic because it came in a moment in history in which there was “no existing nuclear experiment or theory [giving] a complete answer to this question.”\(^{56}\)

Oppenheimer & Volkoff, February 15, 1939 (received January 3)
*On massive neutron cores*

Here the authors continue commenting on improvements on Landau’s ideas, this time emphasizing the importance of using a general relativistic approach rather than a Newtonian one. The reason for this is that neutron cores have an extremely high density and require thus a relativistic approach. Stars that would be stable in a Newtonian world are unstable once general relativity is considered.

For the first time, the *indefinite contraction* fate for heavy enough stars in mentioned.

Oppenheimer & Snyder, September 1, 1939 (received July 10)
*On continued gravitational contraction*

In this paper, the authors apply the equations of general relativity to prove that, at least under some simplifying conditions (non-rotating star, no pressure, no outward radiation), a large enough star will contract indefinitely.

This is the debut of black holes. The authors describe how time freezes at the Schwarzschild radius (of a few kilometers), while it does not freeze for an infalling observer.

\(^{55}\)Landau’s ideas appear on the following two papers: Landau, L. (1932), “On the theory of stars,” *Phys. Z. Sowjetunion*, 1, 285; Landau, L. (1938), “Origin of stellar energy,” *Nature*, 141, 333–334.

\(^{56}\)Oppenheimer & Serber (ref. 12), 540.
Einstein, October 1939 (received May 10)

On a stationary system with spherical symmetry consisting of many gravitating masses

After criticizing simpler treatments on the subject, Einstein uses a stationary argument (cluster of particles in circular paths) to argue that black holes are impossible. However, what he actually proves is that very compact stars are unstable.

APPENDIX B

Brief Description of the EPR Paradox and Zwicky’s Dark Matter

Einstein-Podolsky-Rosen Paradox

The EPR Paradox is a thought experiment designed to show that there is a theoretical inconsistency within quantum mechanics if one holds that it is a complete theory. Imagine a pair of particles originating from a common source. According to the Copenhagen interpretation of quantum mechanics, under some conditions the state of particles 1 and 2 remain fundamentally undetermined until one decides to measure one of them. When one does measure one of them, say particle 1, then either a) particle 2 has a definite state which, however, is not included in the theory, rendering thus the theory incomplete, or b) particle 2 acquires, immediately after performing the measurement on particle 1, certain definite physical property, thus provoked an action-at-a-distance effect, which is contrary to the principles of special relativity.

Einstein et al. assumed that option b) is untenable and thus quantum mechanics must be incomplete—ruining thus the Copenhagen interpretation of the theory. Experiments performed from the 1980s on have, however, corroborated option b).

Zwicky’s Concept of Dark Matter

The concept of “dark matter” was postulated in order to solve a breach between theory and observation in astrophysics. As stars move around the center of galaxies (including our own), their speeds are higher than expected, as if there were a substantial amount of matter not accounted for: invisible matter—hence the name “dark.” More precisely, theory requires that 85% (by modern calculations) of the mass be in the form of dark matter. Or else, there is something fundamentally wrong with our theories of gravity. The dark matter problem has not been solved yet.
APPENDIX C

Timeline of Events

Main events relevant to the discussion. Note the gap between 1939 and 1957.

1930s heyday of nuclear physics, not so much of astrophysics and cosmology (which did not exist)
1930s stellar energy production problem solved by Bethe
1932 discovery of the neutron
   Zwicky, Landau ask: Are there neutron stars (or neutron cores inside stars)?
1938 JRO & Serber: do not forget to include nuclear forces, Landau
1939 JRO & Volkoff: do not forget to include general relativity
1939 JRO & Snyder: a large enough star will contract indefinitely
   (at least under some simplifying assumptions)
1939 Einstein tries to show that black holes are not feasible, but what he actually
   proves is that very compact objects are unstable
1939 Landau allegedly adds JRO & Snyder paper in his Golden List of classic papers
1939 JRO & Volkoff: a large enough star will contract indefinitely
   (at least under some simplifying assumptions)
1939 JRO tries to show that black holes are not feasible, but what he actually
   proves is that very compact objects are unstable
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