History and Philosophy of Physics

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Oppenheimer and the Cosmos

by Virginia Trimble, University of California, Irvine

No one who ever met Robert Oppenheimer seems to have been neutral about him. Nor indeed were and are folks who never met him. Depending on who is speaking, he was:

- A brilliant theoretical physicist who created a stable of slightly younger acolytes at the University of California (Berkeley) and the California Institute of Technology before World War II, who (mostly) adored him
- An arrogant … who disrupted other physicists' colloquium talks and thought the country could not be run without his participation
- A quick study, who mastered enough German and Dutch to write in them during a brief stay in Europe and carried back an affectionate nickname, Opje, that quickly morphed into the American Oppie
- A man wildly attractive to women, to whom he was not entirely indifferent
- A Leader of Men, who herded the diva cats of Los Alamos·to produce both the U-235 and the Pu-239 bombs just after the nick of time for the intended target, Germany
- A fairly unsuccessful husband and father
- The director who built the Institute for Advanced Study into a world-class organization
- A sadistic mentor who cast several of his former students (Bernard Peters, Joe Weinberg, Rossi Lomanitz, and David Bohm) to the wolves
- A strong, almost card-carrying, Communist sympathizer, who should never have been entrusted with national secrets, and who never would be again.

He was also, briefly and presciently, an astrophysicist, a territory to which he seems to have tried to return again, briefly and sadly, near the end of his life. I never met Oppenheimer, whom I will often here call JRO, though the J. for Julius (according to his birth certificate) was not a name he ever used. As for being called "Oppie," when during the security hearings, he was asked whether a particular person had called him by his first name, his response was "You mean did he call me Robert?"

"Not meeting" was probably a fairly near miss. The January, 1967, Third Texas Symposium on Relativistic Astrophysics in New York was the first significant international meeting I attended (as a 3*rd* year graduate student, paying my own way). JRO and his younger brother, Frank, (by then at the University of Colorado) were both among the 278 registered participants at the First Texas Symposium (December 1963 in Dallas), and JRO would quite probably have attended the Third Texas if the throat cancer that killed him in February, 1967, had held off another year or two.

That (later called) First Texas Symposium had been widely advertised and was apparently open to anyone who wished to attend, including some local graduate students, according to the list of 278 participants in the published Proceedings [1] The

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March 2022 FHPP Session Report

"Pais Prize (2022): Tomorrow began yesterday: Why history matters"

By Patricia Fara, Department of History and Philosophy of Science, University of Cambridge, United Kingdom

Patricia Fara

I feel immensely honoured to have won this award, and I hope to take advantage of the privilege to campaign for greater equality among practising scientists as well as to present my own historical research. In devising my rather enigmatic title, 'Tomorrow began Yesterday', I wanted to emphasise that the past can have a crucial effect on the future, both for individual lives and for society as a whole. As a historian, I investigate the past in order to understand how we have reached the present – and for me, the whole point of doing that is to improve the future.

I often integrate my own life within my historical narratives because – like every historian – the questions I ask about the past stem from my personal experiences. In 1969, I graduated from Oxford with a degree in physics after encountering just one role model: Marie Skłodowska Curie, routinely presented as a scientific martyr who endured hunger, cold and loneliness in a leaky shed while repetitively sieving pitchblende. That was not a future I envisaged for myself. I also learnt about her wartime work with X-rays, for which she was portrayed as a familiar female stereotype – the caring nurse, the Florence Nightingale figure dressed in white. That was definitely not for me either.

At that stage, in England anyway, it seemed impossible for a good scientist to be regarded as a normal woman. In my year, there were about 8 women in 200 men, but of course I had known in advance that I would be in a minority. It was only after I moved to an IT position in London that I first experienced the pain of discrimination. To avoid exclusion by both men and women, I spent the next twenty years concealing my scientific background, until eventually enrolling for postgraduate work as a mature student.

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History and Philosophy *of* Physics
NEWSLETTER

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April 2022 FHPP Session Report

"The Century of Physical Cosmology"

By Donald Salisbury, Department of Physics, Austin College

From the left: P. James E. Peebles, Paul Halpern, Dennis Lehmkuhl, and Christopher Smeenk

P. James E. Peebles, Professor Emeritus in the Department of Physics at Princeton University, spoke first on "The social construction of physical cosmology". He shared the Nobel Prize in Physics in 2019 for his work on physical cosmology. His main focus was a well-documented verification in cosmology of a thesis that had been advanced by the sociologist Robert Merton concerning multiple scientific discoveries. It is remarkable that he did not reveal his own significant involvement in these social processes, beginning even with the theoretical and experimental evidence for the production of Helium in the Big Bang. Another involved the evidence for dark matter, the potential role of massive neutrinos and ultimately the currently dominant cold dark matter (CDM) scenario. And then finally he and others had advanced the proposition that a cosmological constant Λ could deliver a spatially flat universe in a manner that could match early baryon oscillations with observed anisotropies in the microwave background radiation. This was done before the experimental confirmation of the accelerating universe in the late 1990s and the resulting acceptance of the ΛCDM model. For many more details I would enthusiastically recommend his new book, *The Whole Truth – A Cosmologist's Reflection on the Search for Objective Reality*.

The second talk, "Theoretical cosmology in the 1960s," was delivered by Dennis Lehmkuhl, Professor of History and Philosophy of Physics at the University of Bonn. His starting point was the Bern conference of 1955, the first truly international general relativity meeting, and a precursor to the meeting in Dallas in December, 1963, that brought together relativists and astronomers. The just discovered quasars engendered lively discussion, and that was just the first step, following the discovery of the cosmic microwave background in 1965, in an expanding application of the Einstein's theory in cosmology. Dennis identified several steps in this evolution, citing the developing Petrov classification, Kerr's rotating black hole, Penrose's black hole theorem, and Hawking's theorem on cosmological singularities

Christopher Smeenk, Director of the Rotman Institute of Philosophy at Western University, followed with "Observational cosmology in the 1960s". He placed much emphasis on what the historian Jean Eisenstaedt has identified as the "low water mark" of general relativity following World War II, noting the expanding international community links that emerged in the 1960s. Several international general relativity and Solvay conferences played a role, and they helped in the development of new mathematical techniques that could address the challenges posed by the growing observational data. Among these were the Newman-Penrose formalism and its application to null geodesics. Much progress was made in this area by the related Hamburg and Texas groups. Also of particular interest were methods for dealing with inhomogeneous perturbations in the Lemaitre Friedman Robertson Walker models from the 1930s. One important outcome was a derivation of the relation between baryon density perturbations and cosmic microwave anisotropies modeled by the 1967 Sachs-Wolfe effect. Then there was of course much attention devoted to the galaxy structure formation and evolution.

FHPP Essay Contest Winners

The Forum held its sixth annual history of physics essay contest in 2022, and is pleased to recognize a winner and a runner up. The essays are linked from the Essay Contest webpage: https://engage.aps.org/fhpp/resources/essay-contest

Miguel Ohnesorge's winning essay is entitled "Newton as a Geodesist – The Problem of the Earth's Figure and the Argument for Universal Gravitation". He is a PhD Student at the University of Cambridge and Visiting Fellow at Boston University's Philosophy of Geoscience Lab. In his PhD project, he reconstructs how physical geodesists measure(d) planetary figures and explores the insights that this problem holds for the epistemology of scientific measurement. His other work focuses on the global history of physics and the ethics and epistemology of industry-funded science. You can learn more about his research on his website https://www.mohnesorgehps.com.

Miguel Ohnesorge
University of Cambridge, United Kingdom

Shraddha Agrawal
University of Illinois, Urbana-Champaign

Shraddha Agrawal's runner up essay is entitled "The sunny side of Anna Mani". She is a fifth-year Ph.D. student in the Department of Physics at the University of Illinois at Urbana-Champaign. Her research is in atomic physics, specifically using ultracold atomic gases to explore novel topological phenomena. Outside of research, she enjoys reading fiction, writing physics-related essays and stories, doing crosswords, and making good food.

FHPP News

Quantum Century update from Paul Cadden-Zimansky, Physics Program, Bard College

The FHPP-originated endeavor to mark the centennial of quantum mechanics in 2025 with a global public awareness and outreach campaign proceeds apace. Originally branded the Quantum Century Project, the APS and German Physical Society (DPG) have taken the lead in organizing the endorsements of over 40 scientific societies and institutes to lobby UNESCO and the United Nations to officially declare 2025 the International Year of Quantum Science and Technology. The country of Mexico is acting as lead sponsor of a declaration resolution, with three other countries so far, South Africa, Jordan, and Argentina, as co-sponsors. The resolution is expected to be taken up for consideration and voted on over the course of 2023, first by UNESCO and then at the United Nations General Assembly in December. More information can be found at https://quantum2025.org

March 2023 FHPP Sessions

Wednesday, March 8 | 3:00–6:00 PM

Politics and Techniques: Nuclear Testing in the Decades after World War II, Catherine Westfall (Chair),

- Retrospective of Nuclear Weapons Testing: 1945 to 1992, Alan Carr, Los Alamos National Laboratory, New Mexico
- Total Immersion in Computing: The 70th Anniversary of the Los Alamos MANIAC, Nicolas Lewis, Los Alamos National Laboratory, New Mexico
- Higher & Higher: Rockets for High-Altitude Nuclear Testing, Rebecca Ullrich, Sandia National Laboratories, New Mexico
- So Sophisticated and So Barbarous': Britain's Revival of Nuclear Testing, 1961-62, Richard Moore, King's College London, United Kingdom
- Knowing Better: Experts, the Public, and Above-Ground Nuclear Testing in Nevada, Catarina Tchakerian, Northeastern University, Boston, Massachusetts,

Thursday, March 9 | 3:00–6:00 PM

And It Was a Very Good Year!' Anniversaries of Breakthroughs in Physics and Astronomy, Virginia Trimble (Chair)

- Maxwell & Gibbs: 150 Years since the Foundations of Statistical Mechanics, Lena Zuchowski, University of Bristol, United Kingdom
- 1923: Harvard, Howard, and Gradual Inclusion, Virginia Trimble, University of California, Irvine, California
- Seventy-Five Years of QCD, Chad Orzel, Union College, New York
- Willie Hobbs Moore: the First African-American Woman Physics Ph.D., Donnell Walton, Corning Technology Center, Silicon Valley, California

April 2023 FHPP Sessions

Pais Prize Session on Galileo, Newton, Einstein, Michel Janssen (Chair),

- The History of Physics in Collaborations: Archimedes, Galileo, Einstein et al., Jürgen Renn, Max Planck Institute for the History of Science, Berlin, Germany
- Newton as Geodesist: The Problem of the Earth's Figure and the Argument for Universal Gravitation, Miguel Ohnesorge, University of Cambridge, United Kingdom
- In Elsa's Apartment, Einstein was Hiding an Escaped Soldier and Womanizer, Alberto Martinez, University of Texas, Austin, Texas

The Stern-Gerlach Experiment: Past and Future – Alberto Martinez (Chair), University of Texas, Austin

- A Century ago the Stern-Gerlach Experiment Ruled Unequivocally in Favor of Quantum Mechanics, Bretislav Friedrich, Fritz Haber Institute, Berlin, Germany
- The Stern-Gerlach Experiment, 1921–1940: From the Old Quantum Theory to Spin, Clayton Gearhart, Saint John's University, Minnestota
- Realization of a complete Stern-Gerlach interferometer: Towards a test of quantum gravity, Ron Folman, Ben Gurion University of the Negev, Israel

Crisis and Big Science, Paul Halpern (Chair)

- The Leak: Politics, Activists, and Loss of Trust at Brookhaven National Laboratory, Robert Crease, Stoney Brook University, New York
- Why Let a Good Crisis Go to Waste? Big Science Funding Lessons, Catherine Westfall, Michigan State University, Michigan
- Launching the Facility for Rare Isotope Beams (FRIB) at Michigan State University: Crisis, Challenge, Opportunity, Thomas Glasmacher, Facility for Rare Isotopes, Michigan State University, Michigan

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three Oppenheimer and (Serber, Volkoff, Snyder) papers are cited multiple times in the proceedings chapters in connection with models for QSRS and radio galaxy energy sources involving super-dense stars or gravitational collapse. Presence at that meeting in Dallas in December, 1963, shortly after JRO had received the Fermi medal from President Johnson was, perhaps, a mark of serious reviving interest in the universe.

But here are my second-hand connections. First, my late husband, Joseph Weber (1919-2000) spent portions of his sabbaticals of 1955 and 1962 at the Institute for Advanced Study during the Oppenheimer regime. Joe liked JRO, despite his first wife having fallen in love with him (He quoted her as saying that, if it weren't for Kitty - Mrs. JRO - she would make a play for him; her answer to Joe's response, "Hey, what about me?" has not been recorded). And apparently Oppenheimer liked Joe, because at some party Weber was the only man JRO would let dance with his daughter, Toni.

Second, and on the other side of the fence, was Edward Gerjuoy (who died only a couple of months short of his 100th birthday in 2018). He was among the last of the JRO students, and we became friends during the 2005 APS celebrations of the centenary of Einstein's miraculous year. Ed did not much like JRO and felt that JRO did not like him and did not treat him entirely fairly, in comparison with better-liked members of the group like Robert Serber and Philip Morrison. It is probably not wholly irrelevant that, when the Los Alamos team was being assembled, Ed at first told JRO that he didn't want to work on defense-related projects and, later, when he changed his mind and asked to join the Manhattan Project, JRO said he didn't want him, since Ed would be a half-hearted participant. Gerjuoy did his "war work" in Connecticut, working on sonar, and really had been a card-carrying Communist, though only as a member of the Young Communists' League (I saw the card).

A third connection was Philip Morrison (1915–2005), who, quite unexpectedly, offered a donation to support a lecture on the history of physics in honor of his thesis advisor, Oppenheimer, during a period when I was in the chair sequence of the Forum on the History of Physics of the American Physical Society.

This in her own words is Virginia Trimble in September, 1962, "about the time JRO was displaying some interest in the universe. The board on the right has Egyptian hieroglyphs of a "wen bunny," which appears in statements of the form "nen wen sgm-f", "there is not he hears". Sgm, to hear, is the prototype verb (like amo, amas, amat in Latin) in Egyptian textbooks. The expert in the photo is the late Dr. Prof. Miriam Lichtheim, born in Berlin in 1914. She earned her PhD there, and was one of the farsighted and lucky Jews who left in time. She died in Israel. The photograph was taken by Bill Ray for Life Magazine, which transferred copyrights to its photographers and authors when the Magazine ceased weekly publication. Bill (who was important enough to get a NY Times obituary) transferred copyrights to his subjects thereafter. It may be recalled that JRO himself had some knowledge of and interest in Sanskrit."

There exist an enormous number of books about Robert Oppenheimer, some overall biographies, others focused on the security hearings, his letters, late life, relationships with women, and so forth. I have not read anything like all of them and own only half a dozen or so, but of these can recommend: Two comprehensive biographies: David Cassidy,2005, J. Robert Oppenheimer and the American Century (New York, Pi Press) and Abraham Pais (with additional material from Robert Crease), 2006, J. Robert Oppenheimer: A Life (Oxford University Press) and a couple of the focused ones: Shirley Streshinsky and Patricia Klaus 2013, An Atomic Love Story: The extraordinary women in Robert Oppenheimer's Life, Turner Publishing (including the fascinating tidbit that the first husband of Katherine Vissering Puening Oppenheimer, Frank Ramseyer (m. 1932, annulled 1933) remarried and that his daughter Helene (Lanie) is an astronomer; Alice Kimball Smith and Charles Weiner, Eds, 1980, Robert Oppenheimer: Letters and Recollections (Harvard University Press), which includes a complete list of JRO's technical papers); Silvan Schweber, 2008, Einstein and Oppenheimer: The Meaning of Genius (Harvard University Press (the personal relationship was not an unmixed friendship); R. Polenberg, 2002, In the Matter of J. Robert Oppenheimer (Cornell University Press). Given this richness, I shall not attempt a biographical sketch, but instead move on to a not completely solved question:

How and why Oppenheimer came to astrophysics

Several "secondary sources" say that the definitive event was interacting with astronomers/astrophysicists at the California Institute of Technology. This cannot be quite true, as the only one there in the 1930s was Fritz Zwicky, who indeed was thinking about neutron stars, cosmic rays, and dark matter in the 1930s, but is not actually cited in any of the key JRO et. al. papers. The Pasadena astronomers were at the headquarters of Mt. Wilson Observatory, 813 Santa Barbara Street, where indeed astronomers are still to be found, now affiliated with the Carnegie Institution of Washington. The first author of those 1934 supernova-neutron star-cosmic ray papers [2]-[5] was indeed Walter Baade of Mt. Wilson.

Meanwhile, as it were, however, the University of California, Berkeley, had had an astronomy department since about 1870, as well as close connections with Lick Observatory on (relatively) nearby Mt. Hamilton. (The astronomers now mostly are located at UC Santa Cruz, though there are now some on every UC campus except the medical school in San Francisco).

In any case, JRO's early papers were all applications of the quantum mechanics of those years to vibration-rotation bands of molecules, the two-body-problem, continuous spectra, absorption spectra, alpha particles, polarization of impact radiation, aperiodic effects, the Ramsauer effect, and so forth, most particularly the Born Oppenheimer approximation [6]. They showed that when one particle in an interaction was much less massive and so faster-moving than the other, you could integrate out the (high frequency) electronic motion and get a wave-mechanical description of the nuclear behavior. Perhaps worth noting about his papers from his first years at CIT and UCB are that many are called "notes on" and that the authors are virtually always alphabetical, so that he came after C.F. Carlson in [7] as well as Wendell Furry, Leo Nadelsky, and Charles Lauritsen, but before Melba Phillips in [8], which concerns the capture by heavy nuclei of neutrons upon bombardment by deuterons, called the Oppenheimer Phillips process later, at least by his students.

These papers and the handful to be mentioned were sometimes submitted from UC Berkeley and sometimes from Caltech, in a pattern that must have varied with the seasons but is not easy to discern, since many of the submission dates are in spring. It was 1 June 1937 and the "postmark" said Caltech when Oppenheimer and Robert Serber submitted [9]. They had in mind that the intermediate-mass particle then just newly reported by Anderson and Neddermeyer [10] could be the particle envisioned by Hideki Yukawa [11] to describe the nuclear force by an exchange of particles. It wasn't (which is part of another story), leading to Cecil Powell receiving the 1950 Physics Nobel (right after Yukawa) for the discovery, also in cosmic ray secondaries, of the pi meson, work to which Cesare Lattes and Giuseppe Occhialini had made major contributions.

Should we deduce that the arrival of Robert Serber was the trigger for JRO taking an interest in physics of the cosmos? Probably not, since he had come (with an NRC postdoctoral fellowship) in 1934, more or less straight from earning his Ph D at Wisconsin, for work with John H. Van Vleck on "Some optical properties of molecules". If mixing of human scientific generations interests you, consider that it was another 43 years to 1977 when Van Vleck won his Nobel Prize (shared with Philip Anderson, discoverer of the muon, and Neville Mott).

Whatever the case, JRO turned briefly (but not exclusively) to astrophysics. The three key papers were [12-14]. [13] and [14] had garnered 2217 and 1128 Astrophysics Data System citations up to January 2, 2022. And it is not true that you had to have a surname beginning with P or later in the alphabet to be a JRO co-author. He comes second to someone with a surname beginning with C, but on a different subject, in the same years.

Robert Serber had come to Berkeley with a National Research Council postdoctoral fellowship after earning his Ph D in 1934 under Van Vleck at Wisconsin. He apparently acted as something of an additional advisor to JRO's students, who included Volkoff (whom I met) and Snyder (whom I did not). JRO and RS had already published [9] and there is a later cosmic ray paper [15]. The "state of the art' of stellar models at the time is well represented by S. Chandrasekhar's *Stellar Structure*. The backbone was the Eddington standard model with which JRO and Serber begin. It assumes that gravity is balanced by total pressure (a good bet) and that the ratio of radiation pressure to gas pressure is a constant through the star, and provided then and now a reasonable fit to the observed mass-luminosity and luminosity-temperature (Herzsprung-Russell) diagram of main sequence stars, including the sun. Further refinements were provided by Bengt Strömgren who was at Chicago also in the late 1930s.

JRO and Serber begin with the Eddington model and credit Gamow with the idea that there could be stars with degenerate (Fermi equation of state) cores. They also quote Landau on the subject but provide no reference. They say that the reactions called out by H.A. Bethe and C.H. Critchfield [17] are sufficient to power the sun and other relatively low mass stars, but that more massive ones require some more powerful source, and could this be condensation into/onto a neutron core? Probably not, for they conclude that even the most massive stars will not form a (stable) neutron core until practically all the sources of nuclear energy have been exhausted, at least in the center of the star. Modern work confirms this conclusion.

This brings us to Oppenheimer and Volkoff, who set out to determine the maximum possible mass of a neutron core in isolation. They began with the equation of state for a cold Fermi (degenerate) gas and the equations of general relativity (with $\Lambda = 0$!). You are most likely to have heard of a quantitative conclusion, that the mass of a neutron star cannot exceed 0.7 solar masses. But the qualitative conclusions are at least as interesting. The source most cited is Richard Chase Tolman's 1934 text [18]. That was the only book not required as a textbook that I purchased in graduate school. (It cost $10 in 1966). Much tattered it is here on my bookshelf, so that I could confirm the first qualitative conclusion, which is that the Oppenheimer-Volkoff solutions are like the analytic ones, which put "an upper limit on the possible size of a sphere of given (constant) density, and on the mass of a sphere of given radius." That mass is just the $2M = rc^2/G$ of the Schwarzschild solution. And the maximum radius for a given density, ρ, is (you must now be patient while I go away and fill in the G's and e's of $r^2=3/8\pi\rho$). Tolman notes on p. 247 that "these limits are very generous, and have so far led to no contradictions with astrophysical observations. The constant density is clearly going to be that of nuclear matter, of order 10^{15} g/cc, and while we are paused, let us note that JRO and Volkoff, quite astonishingly credit the 1.5 solar mass upper limit for degenerate atomic matter, which we call the Chandrasekhar limit, to Landau, now citing his papers from 1932 and 1938, though eventually also Chandra in [19]

Neither Chandrasekhar nor Landau fall within Tolman's field of view, though JRO is there for work from 1930 and 1933 (some with M. S. Plesset) which concluded that positive and negative electrons left together will turn quickly and efficiently to radiation, and Tolman is specifically thanked by JRO and Volkoff. Here is where we sneak in that Tolman's wife, about a decade older than JRO and a decade younger than her husband, is one of three women mentioned (along with Jean Tatlock and Katherine (Kitty) Puening Oppeneheimer as having been at least emotionally important to JRO. While we are at it, Kitty's first, annulled 1932-33 marriage was to Frank Ramseyer, who remarried, and one of his daughters became an astronomer Helene Dickel. She remains in the 2021 membership directory of the American Astronomical Society and kindly responded and confirmed that this was so. She and her astronomer-husband celebrated one of their wedding anniversaries by a balloon ascension.

Meanwhile, back at the Units Convocation, we have concluded that the missing factor has to be c^2/G. Restoring that and making the usual approximations ($3^2 = (pi)^2 = 10$), then the maximum radius allowed for matter at 10^{15} g/cm^3 is about 14 km. They are already saying that the fate of something exceeding the allowed neutron-core mass will be to contract indefinitely, ever more slowly and never reaching a true equilibrium.

But the second, striking, qualitative conclusion is that a different equation of state won't make much difference JRO and Volkoff are of the opinion that the force between two neutrons is weak and if anything attractive. They also explore briefly something repulsive, though never so repulsive as a true hard core potential, which in later work by others brings us to a Tolman-Oppenheimer-Volkoff limiting mass for neutron stars somewhere in the 2-3 solar mass territory. At the end of the paper, it is stated that G.M. Volkoff hopes to discuss in detail elsewhere the consequences of a repulsive n-n force at higher densities.

A paper that is arguably the one intended appeared within the year [20]. It contrasts a cold neutron sphere with the Schwarzschild solution, saying that the massive sphere won't collapse if its central temperature is less than 0 K, which would imply repulsive, negative mass at the center. This is presumably one of the exciting things one could do with a supply of negative mass (of which the most profitable would surely be running a reducing salon).

Now here are Oppenheimer and Snyder. They start with the realization that a neutron core of more than the TOV limit can never achieve equilibrium, and carry on with a time-dependent general relativistic calculation, using Tolman coordinates. The primary finding is that the radius of the star approaches asymptotically its gravitational (Schwarzschild) radius, and light from the surface progressively reddens and can escape over a progressively narrower range of angles. The collapse time will be of the order of a day, for a co-moving observer, though it will be slowed by pressure of matter and radiation (these were taken equal to zero to facilitate the integration of the equations) and by rotation, but not stopped unless so much material is cast off that the mass falls below the TOV limit. Tolman is again specifically thanked, and also a Mr. G. Omer.

Leonard Schiff (1915–1971) also came to JRO with an NRC postdoctoral fellowship for 1937–38, after carrying out Ph D research with Philip Morse at MIT, staying for two more years in California and having some contact with Lamb, Serber,and Snyder, according to the NAS memoire by F. Bloch.

And then there was War

Robert Serber, George Volkoff, and Hartland Snyder all went on to distinguished careers that can be traced on Wikis, various lists of publications, Google, and all. None of them in the pub-lished record ever touched astrophysics again. Schiff, in contrast, after time at the University of Pennsylvania, engagement in work relevant to submarine warfare, and a late call from JRO to Los Alamos (in April 1945) went on to a professorship at Stanford in the fall of 1947. There, much of his work for the last decade of his life was devoted to testing general relativity in new ways. Would antimatter fall down? Yes – remarkably difficult to demonstrate in the lab (if only electrons and positrons didn't have those pesky electric charges winning by 10^40 or so over gravity) –but an unavoidable conclusion from accurate equivalence principle experiments, since part of the effective masses of heavy nuclei come from virtual electron-positron pairs.

Starting in 1960, came The Gyroscope Experiment, described in Physical Review Letters [21], in American Journal of Physics [22], and described at the 1962 Third International Conference on General Relativity and Gravitation in Warsaw, and later at the GRG in Tbilisi, by which time (1968) William Fairbank and Francis Everitt were part of the team. The idea was that a very stable gyroscope in near earth orbit could detect the Lense-Thirring effect (dragging of inertial frames). The required earth orbiter took decades to get off the ground and indeed could and did measure Lense-Thirring precession, though with uncertainty too large to rule out some other theories of gravity.

Another of the JRO stable of students, Philip Morrison (1915–2005, about whom I have written elsewhere [23] [24]) completed a 1940 dissertation on "Three problems in atomic electrodynamics." He spent most of WWII in the Metallurgical Lab at the University of Chicago, participated in the Trinity test, went on to faculty positions at Cornell and MIT, and gradually shifted his primary teaching, research, and outreach efforts, including a very wide range of topics and the advising of many students.

Canadian-American physicist Robert F. Christy (1916–2012) followed Volkoff from the University of British Columbia south to Berkeley. His 1941 thesis with JRO addressed "Cosmic-ray burst production and the spin of the mesotron." He spent the war years at Chicago and Los Alamos, taking up a Caltech professorship (as JRO's replacement) in 1946 [25]. Initially he focused on nuclear physics, developing a gradual interest in stellar energy sources and structure, and moving firmly into astrophysics after a 1960 sabbatical at JRO's Institute for Advanced Study, where he began reading up on variable stars. Another sabbatical in 1968 at the University of Cambridge (Fred Hoyle's Institute of Theoretical Astronomy) led Christy to an interest in pulsars and so forth.

It is possible that other Oppenheimer students eventually came to astrophysics, with or without his influence, but I have not attempted to chase them all down.

Oppenheimer re-encounters the universe

JRO participated in three conferences on astronomy/astrophysics between 1961 and 1964 of which I own the proceedings (either purchased used for a different project or because my husband, Joseph Weber, was there). Let's take the easy one first, the December 1963 First Texas Symposium on Relativistic Astrophysics, with its proceedings edited by Ivor Robinson, Alfred Schild, and Engelbert Schucking [I. Robinson, A. Schild, E.L. Schucking 1965, Quasi-Stellar Sources and Gravitational Collapse, University of Chicago Press] Both Robert and Frank (by then at the University of Colorado) Oppenheimer were among the 278 registered participants. Neither appears as a presenter or submitter of discussion remarks. The original announcement of the meeting had, however, as the third of its four queries: "Does gravitational collapse lead, on our present assumptions, to indefinite contraction and a singularity in space time?" The published presentations then included one citation of Oppenheimer and Serber, Five of Oppenheimer and Volkoff, and six of Oppenheimer and Snyder. The presentations by John Archibald Wheeler and his Princeton colleagues appeared in a separate volume and must surely have included more such citations.

That conference, taking place only the month after the assassination of President John F. Kennedy in the same city, cannot have been emotionally neutral for JRO, who had known them both. But it is the 1961 and 1964 events that I described as "sad and sorry" in an earlier draft, because the proceedings suggest he had somehow lost the quickness and brilliance that characterized his earlier behavior. A proper comparison sample would be a conference on astrophysics (etc) held some time between 1946 and 1953, but nothing of the sort seems to exist. The last section of this paper therefore provides extracts from 1948 and 1961 symposia on high energy/ particle physics, his own topics. My intention is to separate the effects of venturing into long-abandoned territory and of the events of 1953-54 that resulted in JRO's loss of security clearance, chairmanship of the Atomic Energy Commission's General Advisory Committee, and so forth.

Both the 1961 and 1964 conferences belong to a vanished era of small meetings, with discussions after formal presentations included in some detail after in the proceedings.

First, The Distribution and Motion of Interstellar Matter in Galaxies took place at the Institute for Advanced Study on 10-20 April 1961, with Proceedings under that title, edited by L. Woltjer and published in 1962 by W.A. Benjamin Inc. The idea had come from Bengt Strömgren, then at Princeton University, who was "happy to receive strong encouragement from Dr. Robert Oppenheimer and [Strömgren's] colleagues in physics" at Princeton. It was timed for when Jan Oort (who acted as chairman of the conference), Donald Osterbrock, Otto Struve, and Lodewijk Woltjer (who acted as secretary and editor) would all be at IAU [26]}. Support for the conference and publication came from the Air Force Office of Scientific Research. There were 20 speakers (from North America, including including one woman, E. Margaret Burbidge) and a handful of other participants, who did not provide manuscripts (perhaps were not asked to). Most appear in the discussion remarks, strict silence having been observed only by Freeman J. Dyson, Mr. Wennersten, and Abbe Georges Lemaitre.

JRO appears precisely four times: (1) responding to a question from Woltjer by saying that the maximum detected cosmic ray energy is 5 X 10^19 eV, (2) agreeing with Woltjer (who spoke a lot at this meeting) that the ability of the Fermi acceleration mechanism for cosmic rays to give nearly all extragalactic radio sources the same spectral index (that is, the same distribution of electron energies for synchrotron radiation) "has always been really unsatisfactory." (3) and (4) responding twice to Lyman Spitzer, who expressed puzzlement that there aren't more electrons in the cosmic rays as seen from earth.

Those whose experiences of international conferences began only recently will be surprised both by the long duration of "Distribution and Motion" and by the small number of participants. My own conference participation crossed the dividing line between Old (small numbers of distinguished persons, each speaking at length) to New (all you have to do is pay the registration fee and submit a contributed poster or abstract), so that I have on occasion been the only female and the youngest person in a group of 20 or 60, not expected to say anything (in effect a gate-crasher) and the oldest person in a group of 200 or 300, with anything from 10 to 100 women, and still not expected to say anything!

The second volume representing a possible return of JRO to the universe is the Proceedings of the Thirteenth Conference on Physics at the University of Brussels, September 1964, The Structure and Evolution of Galaxies, that is the 13th Solvay, published in 1965 by John Wiley & Sons, Interscience Publishers. No editor is credited, and this may have some relevance to the apparent incomprehensibility of some of the remarks credited to JRO. Whoever was responsible was clearly not an astronomer, because for many of the presentations references to Ap.J (Astrophysical Journal) have been expanded to J. App. Phys.(Journal of Applied Physics)

The structure of Solvay conferences at the time was sufficiently complex to appear as an Appendix. AFOSR was still sponsoring astronomical research at multiple US institutions. Oppenheimer, who had in effect chaired the SOC, does not make a formal presentation, but appears 7 times in the discussions (versus a formal presentation and 24 discussion remarks from Woltjer).

On page 13, JRO kicks off the discussion of the first talk, by Ambartsumian, by asking him "Is it not true, that if one observes the non-nuclear characteristics of galaxies, they do not determine the properties of the nucleus?" Ambartsumian, who was committed to the view that galaxy formation occurred by expansion of the material out of the nucleus, concurred. We would somewhat disagree.

On page 63 he is telling Hoyle what Hoyle thinks about star formation: "your view is that stellar size [he means mass] is determined by the transition from optical thinness and an isothermal regime to optical thickness, and adiabatic conditions." Hoyle does not respond.

On page 104 there are two comments in response to a report by Hoyle on supernovae and their remnants. "(a) under the conditions you envisage, even roughly, particle production, neutrino losses, and high-energy radiation must occur on a vast scale, whether or not there is long wave electromagnetic radiation. (b) Our discussion may be made more difficult because of a lack of definition of the many different time scales involved. If anything much is to survive the implosion, some energy loss must be very fast." To which one can say, well yes, supernovae are very energetic and, by astronomical standards, fast. Particle production probably means particle acceleration. And getting the energy available from neutron star formation into a stellar shell to expel it is still something of a challenge to the modelers of core collapse.

On page 121 he responds to a question from Alfven about the time evolution of occurrence of radio galaxies in an evolution with a uniform space density of galaxies. "It must decrease the time markedly." If this is a small transcription error for "It must decrease with time markedly," then the comment is correct, and if it was a real-time deduction from the data on radio source counts that had just been presented, it represents a very quick mind at work once again! We would now say that radio galaxies were much commoner at z =
2-3 than they are now.

On page 160, as part of an extensive discussion of an extensive presentation by G.R. and E.M. Burbidge on theories of the origin of radio sources, JRO said "Often the centres of radio galaxies appear faint. Perhaps they are destroyed in producing the sources. That might amount to 10^9 solar masses." Well, 10^9 is the right sort of mass for some of the most powerful radio galaxies, like M87, and I suppose one could describe collapse to and feeding of a Schwarschild black hole as destroying the matter, though Freeman Dyson recalled in 1995 that JRO late in life had not been interested in black hotels [27].

On page 171 he leads off the discussion of a short presentation by Alfven on a cosmos with equal amounts of matter and anti-matter: "If there is antimatter in the Galaxy, would we not have antiprotons in cosmic rays?" and later, again to Alfven, "If mechanisms for separation and keeping together (the matter and anti-matter zones) which are imaginable, really do exist." And to Schatzman "Matter and anti-matter could have a large scale separation." This is actually the very end of the proceedings, with the last word going to Alfven, who declared "In this connection, I should like to point out the importance of studying the relativistic statistical mechanics, as Professor Prigogine and others are doing". Prigogine, you will find in the Appendix was the Directeur of the Commission Administrative. And there is no concluding "see you in three years" or other farewells from JRO

But Oppenheimer (on pages 168-169) had led off the general discussion. Here are some excerpts. "In this session we will discuss topics about which we know very little. Perhaps then we will all be treading on common ground." "We are going to discuss possible sources of very large energies...equivalent to masses of many millions of suns...released on these spectacular occasions." "I think it is perhaps true, certainly it is true for me, that everyone's report contained new and previously unknown things." He found strange and puzzling the outflow of the 3 kpc arm (of the Milky Way), the hydroxyl concentration close to the Galactic nucleus; polarization of radio emission; explosion in M82; and the quasi-stellars. One sentence is fairly incomprehensible: "I think the most remarkable impression I have is how much in the galactic story the very high degree of order places apart."

One feels here that some important words must have been left out or mistranscribed. About star formation, JRO opined, "It is true to say that we cannot prove that they do not form." (It remains true today that other phases of stellar life are better understood!) About stellar mass loss which must occur (he is thinking at least partly of white dwarfs as the only possible end point): "but we don't know where they do, or when, or how much, and here I think that probably we will have to look very closely at nature for clues." True: when, where, how much are now largely determined by observations, though some details of why/how TBD. The obvious candidates for great energies are nuclear energy and gravitational energy; neither has proved to be the answer...nor have we got proof that these are not adequate. He regards as puzzling the amounts, the time scale of energy release, and the form (that is radio and optical radiation), but does not mention the intermediaries of magnetic fields and relativistic particles.

About gravitation, Oppenheimer says: "I keep an open mind about the fact that there could be great surprises here, which would not in any way contradict the views that Einstein had about gravitation, except that they might not be as simple as he stated".

Oppenheimer then turned over the floor to, first, Alfven (to talk about matter-anti-matter theories of the universe and quasars), followed by Ambartsunia to present an idea from "F. Zeldovich" that the centers of quasi-stellars are dense clusters of compact stars that reach a run-away collapse, and finally one from I.D. Novikov [28] that they might be lagging cores in the explosion of the "Metaglaxay." This then tied up with Ambartsumian's own view that both galaxies and stars arose from expansions of primordial dense bodies.

For these two conferences and their proceedings, we have a sort of historic or "case controlled" comparison sample of Oppenheimer's contributions to presentations and discussions on elementary particles and quantum field theory, both of which were more or less his territory at the times they occurred (1948 and 1961)

A comparison sample and a somewhat hollow victory

If JRO participated in an astro-related conferences between 1945 and 1953, I have not found evidence thereof, but there were a pair of meetings on particle physics topics close to his interests before and after the security clearance hearings. These were the 8th Solvay in 1948 on Elementary Particles and the 1961 12th Solvay on Quantum Field Theory. Let's take a look at them, or at least their proceedings.

In 1948, the President was Lawrence Bragg (his first of five times in that position). His SOC included Bohr, de Donder, Richardson, Verschaffelt, and Kramers who participated, and Debye, Jaffe, Einstein, and Joliot, who did not. The rapporteurs were C.F. Powell (who got the Nobel Prize perhaps earned by Marietta Blau, Occhialini, and Cesare Lattes for use of nuclear emulsions to discover the pi meson in cosmic rays, and by Bose who first saw meson tracks), Auger, F. Bloch, Blackett, Bhabha, de Broglie (part of his presentation coming from Marie Antoinette Tonnelat), Peirels, Heitler, Teller, Serber, Rosenfeld, Rossi, and Bethe (the last two not there).

The invited participants were Bohr, Bloch, Casimir,Cockcroft, Debye, Dirac, Fermi, Ferretti, Frison, Klein, Laprince-Ringulet, Lisa Meitner, Moller, Perrin, Oppenheimer, Pauli, Scherra, Schroedinger, and invited but not there, Joliot-Curie, Oliphant, and Schwinger. Meitner was not quite the only woman, for Connie Dilworth (who started as a cosmic ray physicist before turning to infrared astronomy) was among the secretariat, and there are two women in the front row of the conference photo.

Oppenheimer had something to say after nearly every presentation and spoke himself at length on "electron theory" (pp. 269–281). His recent work is highly praised by Teller (a possible surprise when one remembers what happened a few years later). The "electron talk" credited quantum electrodynamics to Dirac, Heisenberg, and Pauli, and something about renormalization to Rosenberg, but he also mentioned the work of Schwinger and Feynman (a pair of adjacent papers in the 1948 Volume 73 of Physical Review), with also some credit to Tomonaga and Stueckelberg. Some of JRO's other comments addressed two-neutrino processes, what sorts of particles initiate the strongest cosmic ray showers, two kinds of meson (mu and pi we now say), and tensor forces. He also spoke at some length on two topics I had to look up, "contact transformations" [29] and the Maxwell-Yukawa analogy [30], and mentioned the tau meson.

At the end of the week, "Mr.Bragg" asked R. Oppenheimer to sum up the conference. This he did admirably, though with a lengthy intervention by Bohr (whose views by then were no longer very close to the mainstream of theoretical physics). That the JRO of 1948 might have been a strong candidate to succeed Bragg as the leader of future Solvay conferences is easy to believe. By 1961, not so much so.

In 1961, Oppenheimer was part of the organizing committee along with Amaldi, Moller, Mott, and Perrin. Rapporteurs (invited speakers) included Bohr, Feynman, Gell-Mann, Goldberg, Heitler, Kallen, Madelstam, and Pais, and invitees included

1964 Solvay Conference – Courtesy of the International Solvay Institutes, Brussels

Bohr, Chew, Cini, Dirac, Dyson, Heisenberg, Rosenfeld, Schroedinger, Schwinger, Tomonaga, Van Hove, Wick, Wightman, and Wigner, plus a few (including the invited Russians) who didn't actually get there. Unless they are hiding among the auditors with only initials, there were no women involved. The auditors were nearly all from Belgium (Brussels and Liege), plus Lemaitre from Louvain and Robert Herman from the US.

In the conference photo, JRO is seated in the very center of the front row (presumably indicating his position for the 1964 Solvay conference), but he contributed precisely three remarks during the week:

1. Following the talk by Abraham Pais on the weak interaction (p. 125), he briefly described the ideas of T.D. Lee (C.N. Yang was there - Lee not - but said very little)
2. Following the talk by Goldberger, he described something as a relativistic generalization of an approximation possible because of the small value of mu/M (p. 197).
3. Concerning some difference of opinion (p. 229) between Goldberger and Madelstam, he suggested the combination of high energy and neglect of cross-terms as relevant. Murph's answer was "Perhaps", in a tone of voice suggesting "when pigs fly."

Given this sample of participation in conferences on topics that were more or less his own, I think it reasonable to conclude that Oppenheimer's interest in physics in general did not entirely survive the 1954 verdict.

In an eerie coincidence, as this was being written, on December 17, 2022, the Department of Energy (successor to the Atomic Energy Commission) announced that the verdict "In the Matter of J. Robert Oppenheimer" was being vacated, leaving him with his security clearance, committee memberships, and all intact.He has been dead for 55 years.

Appendix

The structures of the post-world-war-II Solvay Conferences were sufficiently complex that we give the full details of one here, the 13th, with sketchier versions for others in the main text. For each there were six entities with names in French for the 13th, and in English later

1. Commission Adminstrative, with a president (e.g.the president of the Free University of Brusselles), Membres (including some Solvay descendents), a Directeur (Ilya Prigogine then), and a secretary, also from ULB.

2. A Comité Scientifique, with a President or Chair (Lawrence Bragg held his position for Numbers 8, 9, 10, 11, and 12, and R. Oppenheimer for 13 in 1964), and some members. For 13 these were Amaldi, Bragg, Gorter, Heisenberg, Møller, Mott, Perrin, Taam, and Tamonaga, with Geheniau of ULB as Secretaire. Of these, Bragg, Mott, and Taam did not attend. One or more invitees from the USSR who did not attend was a signature of conferences in that period.
3. Membres Rapporteurs who were invited to make major presentations. For 13 these were Ambartsumian (the one Soviet astronomer who habitually managed to get to conferences), John Bolton, Geoff and Margaret Burbidge, Hoyle, Rudolf Minkowski, Oort, Ed Salpeter, Maarten Schmidt, Lyman Spitzer, and Lodewijk Woltjer (of these I knew all but Ambartsumian). E.M. Birbidge was the only woman on these first three lists, another signature of conferences of the period.
4. Membres invités, who were allowed to make comments and ask questions about the major presentations. For 13 they were Alfven, Biermann, W.A. Fowler, Ginzburg, Greenstein, Kahn, Lindblad, Lovell, Morrison, Pontecorvo, Prendergast, Rossi, Sandage, Schatzman, Martin Schwarzschild, Shklovsky, Strömgren, and Zeldovich. Those not present (marked "excusé" in the proceedings) were the Russians Ginzburg, Pontecorvo, Shklovski, and Zeldovich, plus Greenstein, Morrison, and Schwarzschild (who was working on solar imagine from a balloon gondola).
5. Membres secretaires, three from ULB, plus Annie Baglin from IAP Paris and James Lequeux from Meudon, and
6. Membres auditeurs, meaning presumably they were supposed only to listen, not talk, though a couple of them appear in the proceedings. There were 25 of them, most from ULB (Including P. Bourgeois), a handful from Liege (including Pol Swings), Ed Spiegel later at Columbia, P. Ledoux, and R. Simon, these last two among the "listeners" who said something. Georges Lemaitre came from Louvain and Robert Herman (collaborator of George Gamow on nucleosynthesis in the hot, dense early universe, but then at General Motors Research Labs in Michigan.

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[29] Contact transformation: this is one that preserves certain structures, for instance if two curves had been tangent in the original coordinate system, they will still be so in the transformed system. But the mathematics was developed by Sophus Lie (1842-1899) so it is really more complex (in non-technical sense) than I can explain.

[30] Maxwell-Yukawa analogy is the effort to put some theory of the strong (nuclear) force into a form like Maxwell's equations for electromagnetism. This turned out not to be the winning strategy

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Continued from page 2

More recently, as a senior Cambridge academic, I have tried to ensure that young women are welcomed into the sciences, especially physics and engineering. One way of approaching that challenge is to uncover successful women who have been concealed in the archives. Although they had no influence on my own early career, I hope they will convince younger women that choosing science can be a rewarding option. Furthermore, scrutinizing the past can expose prejudices that still pervade modern society and so help to confront and tackle them.

For my talk at APS in Chicago, I focused on three of Curie's contemporaries: a physicist, a mathematician and an engineer. First came Edith Stoney from Dublin, daughter of a distinguished physicist who got first-class marks in her final examinations, but was unable to collect her degree: Cambridge women were banned from officially graduating until 1948. For a while she taught at the London School of Medicine for Women, the only one accepting women before the First World War, but as soon as hostilities started, she joined the Scottish Women's Hospitals and spent several arduous years overseas as a radiologist. This international initiative was launched by an enterprising Scottish doctor, Elsie Inglis, who appealed through the well-established suffragist network to assemble fully equipped medical teams. Although Britain's War Office laughed, her allies accepted eagerly.

Stoney started out in French field hospitals, learning how to wire machinery in sub-zero temperatures and provide illumination for female surgeons with candles protected by cocoa tins. Her most gruelling experiences were in Serbia, where she 'could carry heavy loads of equipment, repair electric wires sitting astride ridge-tents in a howling gale, and work tirelessly on an almost starvation diet'. She and her female colleagues endured appalling conditions: the weather was extremely hot in the summer, alternating with deep snow in winter. Malaria, typhoid and dysentery were rife, sanitation was non-existent, and they were forced to improvise their own equipment. Somehow, they converted an old, filthy silk factory into a functioning hospital where Stoney developed X-ray photographs: 'The dark room was partly in the flue of the tall factory chimney, and the blizzard streamed through the outhouse, where I was, and up the chimney. When I creaked up the ladders in stockinged feet to the loft where 54 of us now slept, there could be no thought of washing with ice already in the jug.'

Stoney was awarded an impressive array of medals, most of them foreign. The stories of these forgotten doctors and scientists make grim reading, but they often remembered those years as the most fulfilling of their lives: for the first and only time, these educated women could live independently and make their own decisions – just as if they were men.

In contrast, Ray Strachey (née Costelloe), stayed in England during the War but was an ardent committee woman who helped to administer the Scottish Women's Hospitals. After studying mathematics at Cambridge and engineering at Oxford, she became one of England's most eminent suffrage leaders. Of my three examples, I empathise with her most strongly because she demonstrates that leaving science can be a deliberate and positive decision, as it was for me; unfortunately, the 'Leaky Pipeline' model implies that women who shift careers are failures, unable to keep pace with the demands of hard science.

As a schoolgirl, Costelloe was in love with her subject: 'the more Mathematics I learn, the more I wonder that anyone can call them dull or useless. They seem to me to be the most exciting & beautiful ideas, exquisitely expressed.' But once at Cambridge, she became involved in equality activism: 'we have started a society which has now got more than ¾ of the college, & which has joined with Girton [Cambridge's other women's college] & amalgamated to the national Society… Cambridge has become a centre of activity – meetings, debates, plays, petitions etc all through the term.' During the War, she worked at the Women's Service Bureau in London, placing thousands of women a year into paid work as munitions workers, plumbers, and clerks, as well as launching a school for female welders.

Costelloe Strachey remained politically active for the rest of her life, publishing and campaigning for equality. She showed no sign of regretting the scientific life she had abandoned. Perhaps she compared her self-fulfilment and influence with the fate of her mentor, Hertha Ayrton (née Phoebe Marks), a talented engineer who was repeatedly rebuffed by the masculine world of science. Ayrton did, however, find solace in the friendship of Skłodowska Curie. Both married to a scientist, the two women supported each other through difficult times – Ayrton even taught maths to Curie's teenage daughter.

Like Stoney and Costelloe, she was a successful student denied the privilege of graduating. A prolific inventor, Ayrton registered 26 patents but a legacy from Barbara Bodichon, a prominent feminist, enabled her to dedicate herself to an unremunerated career in science. She became most famous for her work on electrical arcs, which was of great practical importance for making lights burn more evenly, while her research into sand ripples was judged so important that she was the first woman allowed to read her own paper at the Royal Society. She won the Society's prestigious Hughes' medal, awarded annually 'in recognition of an original discovery in the physical sciences.' But when she was nominated for a Fellowship, the men closed ranks and refused her application on the tenuous grounds that she was married.

This eminent scientist was also an enthusiastic suffrage campaigner. In 1911, along with many other protesters, she deliberately defaced her census form, scrawling angrily 'How can I answer all these questions if I have not the intelligence to choose between two candidates for parliament? I will not supply these particulars until I have my rights as a citizen.' She even persuaded Curie to sign her one and only petition.

In 1919, Ayrton declared to a *Daily News* journalist that 'I do not agree with sex being brought into science at all. The idea of "woman and science" is completely irrelevant. Either a woman is a good scientist, or she is not.' A wonderful objective – yet even now, more than a century later, relatively few women reach the upper echelons of scientific career structures. As every scientist knows, all sorts of inequalities survive despite legislation. Disheartening as this, much has changed over my own lifetime, and I agree with the basic optimism expressed by Strachey: 'The establishment of equality of pay and opportunity for women may lie far ahead in the future; but that it does lie there is beyond question. The day of economic emancipation will come, just as the day of political emancipation came.'