"Francis Perrin's Early-1939 Analysis of Uranium Criticality: A Look Back"

By Cameron Reed

Introduction

In the May 1, 1939, edition of *Comptes Rendus*, the weekly journal of the French Academy of Sciences, physicist Francis Perrin (1901-1992; Fig. 1) of the Collège de France published a 3-page paper containing the first numerical estimate of the critical mass necessary to achieve a fast-neutron chain reaction in a natural-abundance uranium compound [1]. His result was about 40 metric tonnes (40,000 kg), which he estimated could be reduced to about 12 tonnes with a suitable tamper. He did not seem particularly concerned with the enormous scale of this result – which would later prove to be about three orders of magnitude too large – but he did remark that since a fast-neutron reaction would not be easy to control, a better hope might be to use hydrogenic compounds to slow neutrons and so achieve a controllable reaction.

Perrin's calculation was soon eclipsed by deeper understanding of the mechanism of fission; it had no real relevance for a Hiroshima-type nuclear weapon, which requires essentially pure U-235. He was, however, apparently the first to publish a diffusion-theory-based quantitative estimate of the critical mass ("masse critique"), so it is interesting to take a retrospective look at his work to see both how his numbers arose and to explore the impact of his paper. As I will show, Perrin's physics was largely correct: Had he considered using pure U-235 and been armed with even rough numbers for the relevant cross-sections, he could have made a quite respectable estimate of the critical mass for a nuclear weapon nearly a year before Otto Frisch and Rudolf Peierls alerted British authorities to the possibility of "super-bombs" in their famous memorandum of March, 1940 [2]. In this article I examine Perrin's model, make some speculations as to the provenance of his parameter values, and consider how his work compared to later efforts.

Perrin's criticality model

To set the context of Perrin's work, it is helpful to briefly summarize what was known of fission in the spring of 1939. Otto Hahn and Fritz Strassmann had discovered fission via *slow* (moderated) neutron bombardment of uranium, but Perrin considered *fast* neutrons, a choice which may have been conditioned by the fact that he would not have to deal with the complicating effects of a moderator; also, it is clear from

the outset of his paper that he was interested in the possibility of explosively liberating large amounts of energy ("libérant une énergie énorme"). In February, 1939, Niels Bohr posited that it was the rare 235 isotope of uranium that was responsible for slow-neutron fission; perhaps Perrin was thinking that the much more abundant 238 isotope might suffer fast-neutron fission if there was some energy threshold involved. Bohr's hypothesis had been made in response to the differing fission properties of thorium and uranium uncovered by Otto Frisch and others [3, 4]; Perrin referenced neither Frisch nor Bohr, but it is hard to imagine that he would have been unaware of their work. Further research over the following year by Bohr and John Wheeler (among others) would reveal the poisoning role of inelastic scattering and capture of neutrons by U-238.

Perrin considered uranium oxide (U_3O_8) of natural isotopic abundance, and treated a chain reaction as an

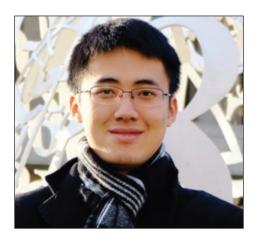
Continues on page 4

In This Issue	
Francis Perrin's Early-1939 Analysis of Uranium Criticality: A Look Back	1
FHP Essay Contest Winners	2
March and April 2020 FHP Sessions	2
April Session Reports	3
Book Review	8
Officers and Committees	9

FHP Essay Contest Winners

By Paul Cadden-Zimansky

The FHP Student Essay Contest received a record number of entrants in 2019, with submissions from four continents and essay writers ranging from high school students to graduate students. The winning essay "A Changing Dichotomy: The Conception of the 'Macroscopic' and 'Microscopic' Worlds in the History of Physics" was submitted by Zhixin Wang, a graduate student in applied physics at Yale University. Wang's essay examines scientists' shifting views on the what distinguishes micro and macro over four centuries, from seventeenth century hypotheses of hidden mechanical mechanisms as explanations for visible phenomena to more contemporary distinctions where it is not size, but quantum metrics that are often appealed to. For winning the contest Mr. Wang will receive a cash award of \$1,000, plus support for travel to an APS annual meeting to deliver a talk based on his essay. This year's runnerup essay "Isabelle Stone: breaking the glass ceiling with thin films and teaching" was submitted by Melia Bonomo, a Ph.D. candidate in applied physics at



Zhixin Wang, a graduate student in applied physics at Yale University

Rice University. Bonomo's essay charts the career of the largely unknown Stone who, under the supervision of Albert Michelson, became the first American woman to be awarded a physics doctoral degree in 1897 and went on to be a co-founder of the American Physical Society while making significant contributions to multiple schools of higher education that served women. Ms.



Melia Bonomo, a Ph.D. candidate in applied physics at Rice University

Bonomo will receive a \$500 cash award for her work. Both essays are posted on the FHP's website at aps.org/units/fhp/essay/index.cfm

March and April 2020 FHP Sessions

March 2020 FHP Sessions

Monday 3/2 2:30 pm History of Materials Science

Tuesday 3/3 8:00 am

History of Physics and Computation
(cosponsored with DCOMP)

Friday 3/6 8:00 am History of Physics in India

April 2020 FHP Sessions

Sunday 4/19 8:30 am *Science Policy and Science's History*

Sunday 4/19 1:30 pm History of Cosmology

Sunday 4/19 3:30 pm Pais Prize Session

Monday 4/20 1:30 pm

Author in Dialogue: Jeff Bub,

Bananaworld: Quantum Mechanics for

Primates

History of Physics

The Forum on History of Physics of the American Physical Society publishes this Newsletter biannually at http://www.aps.org/units/fhp/newsletters/index.cfm. Each 3-year volume consists of six issues.

The articles in this issue represent the views of their authors and are not necessarily those of the Forum or APS.

Editor

Robert P. Crease Department of Philosophy Stony Brook University Stony Brook, NY 11794 robert.crease@stonybrook.edu (631) 632-7570

Assistant Editor
Don Salisbury
Austin College
DSalisbury@austincollege.edu

APRIL SESSION REPORTS: "Secrecy and Espionage in Science"

By Dan Kennefick

t the April 2019 APS meeting the FHP and the Forum on Physics and Society cosponsored a session of invited talks on Secrecy and Espionage in Science. There was considerable interest in the topic, with the room being packed. Quite a few people were standing at the back. The large and engaged audience was treated to three excellent talks. The first was given by Audra Wolfe, a historian of science who is a member of the FHP's Executive Committee as well as being the author of Freedom's Laboratory: The Cold War Struggle for the Soul of Science. Her talk, entitled "Scientific Internationalism, Scientific Intelligence, Or Both?" concerned the efforts made by the United States government to wrestle with competing impulses in post-war science policy. On the one hand they wished to maintain secrecy, especially in the area of nuclear physics, and on the other hand they hoped to use science as an aid to Diplomacy in the cold war. Quite early on they recognized that secrecy was not palatable to the scientists themselves and that it would be unhelpful in efforts to attract scientists to the US and to encourage positive interactions between them internationally. She showed how the postwar emergence of the Central Intelligence Agency naturally suggested the idea that interactions with and between scientists should fundamentally be an intelligence matter, but that this conception was, to a considerable extent, trumped by the desire to let the State department handle science as an arm of international Diplomacy. Most interesting of all she shows how the Diplomats and the Spooks could find common ground on the idea of the science attaché as a scientific intelligence gatherer at American embassies abroad. Internal government documents reveal that a

vision of intelligence gatherers centralizing the latest scientific knowledge in government hands never became a reality, but considerable penetration of the scientific community by the intelligence apparatus was achieved. In her view "The United States' commitment to international scientific cooperation was never primarily about scientific values; it was scientific internationalism for the sake of anti-Communism."

Alex Wellerstein is a historian of science at the Stevens Institute of Technology and the creator of NUKEMAP (<u>nuclearsecrecy.com/nukemap/</u>) which permits the ordinary citizen to find out how their city would fare in a nuclear attack. His talk was entitled "The legacy of nuclear secrecy in the United States" and it discussed the scientific area in which the tensions addressed in Wolfe's talk were at their sharpest, namely nuclear research. In this subject even most of the scientists agreed about the need for secrecy and he discussed the early self-censorship of the small group working on fission chain-reaction in the early days of World War II and the role of major figures like Oppenheimer in formulating secrecy policy after the war. One particularly interesting point that he made was the extent to which secrecy efforts during the period of the Manhattan project itself were probably mostly aimed at keeping Congress in the dark, since they could have shut the project down. Leaks as such were unlikely to reach the Germans in a form which would be helpful to them and the Russians were still allies at that point. As with all of the talks, Wellerstein was confronted with a long queue of listeners at the microphone eager to ask questions when he concluded.

The final speaker was Douglas O'Reagan, whose talk was titled "Stealing Nazi Science: Allied Efforts to

Acquire German Science and Technology during and after the Second World War." He is the author of the forthcoming book Taking Nazi Technology: Allied Exploitation of German Science after the Second World War. His talk was focused on the spoils of German science and technology, which were greatly coveted by the wartime Allies, who hoped to be recompensed for their enormous wartime losses by benefiting from technology and scientific transfer from Germany to their countries. He demonstrated strikingly that their understanding of the problems involved in such a transfer was often on a very high level. There was considerable appreciation of the difficulties in simply transferring science and technology. Allied powers often recognized that taking material and personnel out of their own environment would probably merely result in the loss of the very "know-how" they sought to acquire. The very phrase "know-how", he confides, emerged in this period. Often they preferred to send their own people to work with German research groups within Germany, hoping to thereby transfer the knowledge thus absorbed back home. The French, he argues, saw the dangers in sending older scientists (too set in their ways) or younger people (too susceptible to putting down roots in Germany) and preferred to send mid-career scientists. Of course simply grabbing whatever wasn't nailed down (and some of what was) and transporting it home still had its attractions, especially to the Soviets, who felt the most in need of an infusion of technical equipment. It was a fascinating talk with which to end an engaging session.

All three session contributors are social media stars and can be followed on twitter at @ColdWarScience (Audra Wolfe), @wellerstein and @D_OReagan.

Continued from page 1



Francis Perrin (1902-1992) in 1951, Source: AIP Emilio Segrè Visual Archives, Physics Today Collection

application of diffusion theory, exactly the appropriate physics. He would have come to diffusion physics from years of expertise: his father, Jean Perrin, was awarded the 1926 Nobel Prize for Physics for his experimental determination of Avogadro's number, and Perrin *fils'* 1928 doctoral thesis was on the topic of Brownian motion [5].

Perrin considered the simplifying case where the mean free path of the neutrons is small in comparison to the characteristic size of the sample. While not strictly true for nuclear weapons materials, this is a commonly-made and reasonably accurate assumption. This allowed him to write a simple expression for the mean free path in terms of the number densities of uranium and oxygen atoms and their cross-sections for neutron transport and fission; this is equation (2) below. His terminology is somewhat confusing in that he uses the term "absorption" to refer to both the fission cross-section of uranium and the capture cross-section of a tamper material or the oxygen in U₃O₈, and defines what we now call the transport cross-section, the sum of the cross-sections for fission (if possible) and elastic scattering, as the "diffusion"

cross-section. Details of solving the diffusion equation can be found in [6]. Doing so for a spherical system along with the boundary condition that criticality occurs when the neutron density at the edge of the sphere falls to zero, he arrived at an explicit expression for the critical radius:

$$R = \pi \sqrt{\frac{\lambda_{core}}{3n_U \sigma_{fiss}(v-1)}}, \tag{1}$$

where n_U is the number density of uranium atoms, ν is the number of neutrons emitted per fission, and σ_{fiss} the fission-cross-section for uranium under fast-neutron bombardment. With σ^u_{trans} and σ^o_{trans} respectively representing the transport cross-sections for uranium and oxygen and n_O the number density of oxygen atoms, λ_{core} is given by

$$\lambda_{core} = \frac{1}{n_U \sigma_{trans}^U + n_O \sigma_{trans}^O}.$$
 (2)

 n_U and n_O are computed by first computing the number density of U_3O_8 molecules from its known bulk density and atomic weight, and multiplying by factors of three and eight. The true density of U_3O_8 is about 8.4 gr cm⁻³; Perrin assumed 4.2 gr cm⁻³.

Equation (1) appeared in Robert Serber's *Los Alamos Primer* four years later, in April, 1943 [7]. As Serber explained, demanding that the neutron density goes to zero at the edge of the bomb core is too restrictive a condition, and ends up leading to overestimating the critical mass. However, it does have the advantage that it leads to equation (1) for the critical radius; otherwise, one has to deal with a transcendental equation.

Perrin adopted $\sigma^u_{trans} = 6$ barns (modern value for U-235 = 5.8), $\sigma^u_{trans} = 2.0$ barns (2.7; Perrin neglected any neutron capture by oxygen), $\sigma_{fiss} = 0.1$ barns (1.2), and $\nu = 3$ (2.6). These numbers give $\lambda_{core} = 9.8$ cm (Perrin claims 10 cm), and R = 134 cm, corresponding to a mass of just over 42,000 kg.

But for his density and fission crosssection, Perrin's numbers are respectably close to what we would use today for U-235. The source of his fission cross-section may have been a paper by Herbert Anderson and his collaborators

at Columbia University which appeared in the March 1, 1939, edition of the Physical Review, where they report precisely the figure of 0.1 barns for naturalabundance uranium [8]. Perrin does not reference Anderson et al., nor does he give any source for his transport crosssections. A paper published in the April 15, 1939 Physical Review by Anderson, Fermi, and Hanstein did report a total capture cross-section for uranium of about 5 barns, but this was for slow neutrons [9]. Perrin does refer to a paper published by Hans von Halban and collaborators in the April 22, 1939, edition of *Nature* wherein they reported $\nu \sim 3.5$ \pm 0.7 neutrons per fission [10]. In view of the still-unfolding understanding the roles of U-235 and U-238 in the fission process, we cannot be critical of Perrin's small fission cross-section. Curiously, Frisch and Peierls would err in the opposite direction by assuming too large a fission cross-section for U-235, 10 barns, and significantly underestimating the critical mass. Since the critical radius behaves as $R\sim 1/\rho\sqrt{\sigma_{fiss}\sigma_{trans}}$, small changes in parameter values can have big effects on the mass.

Effect of tamper

Perrin then turns to the idea of surrounding the fissile core with a spherical metallic neutron-reflective tamper of thickness *L*. This has the effect of lowering the critical mass by returning neutrons that would otherwise escape back into the core and giving them fresh opportunities to induce fissions. He then presented the following transcendental equation for the critical radius *R*:

$$\lambda_{core}(1 - aR \cot aR) = \lambda_{tamp}(1 + bR \coth bL).$$
 (3)

Here, λ_{core} is as in equation (2). λ_{tamp} depends on the nuclear number density and transport cross-section of the tamper material:

$$\lambda_{tamp} = \frac{1}{n_{tamp} \sigma_{trans}^{tamp}}.$$
 (4)

a and b both have units of inverse length, and depend on the properties of the core and tamper materials. But for a factor of π , a is the reciprocal of the critical radius of equation (2):

$$r^2 = \frac{3 n_U \sigma_{fiss}(v-1)}{\lambda_{core}}, \tag{5}$$

and

$$b^2 = 3 n_{tamp}^2 \sigma_{trans}^{tamp} \sigma_{capt}^{tamp}, \qquad (6)$$

where σ_{copp}^{tamp} is the capture cross-section for the tamper material.

By considering the analysis of tamped cores presented in ref. [6], I have determined that equation (3) results from a standard solution to the neutrondiffusion equation in conjunction with assuming the continuity of the neutron density and flux at the core/tamper interface and that the neutron density goes to zero at the outer edge of the tamper, again a more restrictive but simpler condition than what applies in actuality. Perrin chose iron as the tamper, taking a thickness of 35 centimeters, and adopting $\sigma_{trans}^{tamp} = 3$ barns, and $\sigma_{capt}^{tamp} =$ 0.05 barns. He may simply have made a lucky guess here: the modern value for the fission-spectrum averaged elasticscattering cross-section for neutrons on iron is in fact almost exactly 3 barns; the radiative-capture cross-section is about 3.4 millibarns. In any event, the situation is still impractical: with the modern value for the density of iron (Perrin did not specify what value he used), this reduces the critical mass to just over 14,000 kg, but the tamper itself would have a mass of over 42,000 kg.

Subsequent developments

Two other papers on critical conditions soon followed Perrin's. In the June 6, 1939, edition of Naturwissenschaften, Seigfried Flügge presented a much lengthier analysis of the prospects for realizing nuclear energy [11]. He was much more concerned with neutron resonance-capture phenomena; he did not reference Perrin, and does not appear to have estimated a critical mass. For English-language readers, the most interesting impact of Perrin's work can be found in a paper published in October, 1939, by Rudolf Peierls [12]. Peierls, who explicitly referenced Perrin, simplified the situation by working with pure uranium and no tamper, took an integral-equation approach, and developed approximate expressions for the critical radius for when the number of secondary neutrons per fission is either very close to unity or very large. In the regime of practical interest, the two approximations do not differ drastically, and taking an average of his results gives a critical mass only about 14% high when compared to the results of later, more sophisticated Los Alamos diffusion theory [13]. Peierls's paper contains no numbers: He did not estimate a critical mass.

In his memoirs, Peierls related that after refining Perrin's calculation, he had some misgivings about publishing, and claims that he consulted with Otto Frisch on the advisability of doing so. Frisch was confident that Niels Bohr had shown that an atomic bomb was not a realistic proposition, and advised Peierls that there was no reason not to publish [14]. A few months later they would find themselves in a very different circumstance.

In the Los Alamos Primer, improved cross-section data allowed Robert Serber to estimate a critical mass of ~ 200 kg, which he reduced to ~ 60 kg upon considering a more refined boundary condition. Serber also considered a tamper, although he did not incorporate any capture-cross section for the tamper material, which changes the form of the solution for the diffusion equation from what Perrin used. Since the *Primer* was not (originally) intended for publication, it contains no references; we can have no idea as to what extent Serber was aware of or influenced by the work of Perrin or others. That the same problem would be considered in largely the same way by two very competent theoretical physicists is not surprising, but it is striking to see the connection from 1939 to 1943.

The overall conclusion is that Perrin conceived very clearly the concepts of both critical mass and tampering, and formulated the physics correctly but for the boundary-condition issue. The details would evolve, but by the opening days of World War II the key formulations had appeared in the open literature. In view of this, Werner Heisenberg's initially very muddled presentation of the issue during his postwar captivity at Farm Hall remains a mystery; his claim that he never studied the question of critical mass because he did not think it possible to separate U-235 does not seem fully convincing. The mystery is only deepened by the fact that an unsigned 1942 report to German Army Ordnance seems to indicate that somebody must have looked at the issue very closely [15].

As for Perrin, he enjoyed a distinguished career after the war, remaining as a professor at the Collège de France until 1972, serving as the High-Commissioner of the French Atomic Energy Commission from 1951 to 1970 (during which time that country developed its own nuclear weapons), and being involved with the discovery of the Oklo natural reactor in 1972. He passed away in Paris on July 4, 1992.

I am grateful to former Alma College student Cory Townes for translating Perrin's paper.

References

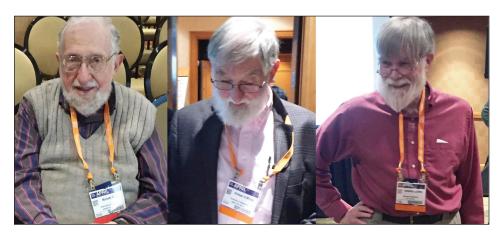
- F. Perrin, "Calcul relative aux conditions éventuelles de transmutation en chaîne de l'uranium," Comptes Rendus 208, 1394-1396 (1939).
- [2] J. Bernstein, "A memorandum that changed the world," Am. J. Phys. **79**(5), 440-446 (2011).
- [3] N. Bohr, "Resonance in Uranium and Thorium Disintegrations and the Phenomenon of Nuclear Fission," Phys. Rev. 55, 418-419 (1939).
- [4] O. R. Frisch, "Physical Evidence for the Division of Heavy Nuclei under Neutron Bombardment," Nature 143(3616), 276 (1939).
- [5] A copy of Perrin's thesis can be found at tel.archives-ouvertes.fr/tel-00278195/ document
- [6] B. C. Reed, *The Physics of the Manhattan Project* (3rd ed., Springer, Berlin, 2015). See Chapter 2.
- [7] R. Serber, The Los Alamos Primer (Berkeley, University of California Press, 1992), Sect. 10.
- [8] H. L. Anderson, E. T. Booth, J. R. Dunning, E. Fermi, G. N. Glasoe, and F. G. Slack, "The Fission of Uranium," Phys. Rev. 55, 511-512 (1939).
- [9] H. L. Anderson, E. Fermi, and H. B. Hanstein, "Production of Neutrons in Uranium Bombarded by Neutrons," *Phys. Rev.* 55, 797-798 (1939).
- [10] H. von Halban, F. Joliot, and L. Kowarski, "Number of Neutrons Liberated in the Nuclear Fission of Uranium," *Nature* 143 (3625), 680 (1939).
- [11] S. Flügge, "Kann der Energieinhalt der Atomkerne technisch nutzbar gemacht werden?" Naturwissenschaften 27, 402-410 (1939).
- [12] R. Peierls, "Critical Conditions in Neutron Multiplication," Proc. Camb. Philos. Soc. 35, 610-615 (1939).
- [13] B. C. Reed, "Rudolf Peierls' 1939 Analysis of Critical Conditions in Neutron Multiplication." Physics & Society 37(4), 10-11 (2008).
- [14] R. Peierls, Bird of Passage: Recollections of a Physicist (Princeton: Princeton University Press, 1985), 153.
- [15] Bernstein, "Heisenberg and the Critical mass," Am. J. Phys. 70(9), 911-916 (2002).

APRIL SESSION REPORTS: "Remembering Julian Schwinger" (DPF co-sponsor)

By Donald Salisbury

Telebrating somewhat tardily the centennial birthdate of Julian Schwinger on February 12, 1918, roughly forty attendees were treated to personal reminiscences on one of the leading theoretical physicists of the twentieth century. Touching a theme that would be repeated by all three presenters, Michael Lieber, whose title was "Divergence", was the first to recall the mesmerizing effect of Schwinger's lectures, characterizing them as like virtuoso violin performances. He asked Schwinger two years after having commenced his graduate studies at Harvard in 1957 if he would oversee a dissertation, and he agreed. Lieber experienced a couple of ``misstarts" involving first the 1955 renormalization approach of Bogoliubov and Shirkov, and then the Euclidean TCP theorem. After a respite in industry Lieber returned to Harvard and to Schwinger. The latter had famously exploited the link between Lorentz transformations and Euclidean rotations in his 1957 paper. He had of course also very early emphasized an almost universal role for Green functions, and the link between these themes formed a basis of Lieber's dissertation, completed in 1967, in which he constructed a Coulomb Green function by exploiting the O(4) symmetry of the non-relativistic hydrogen atom as analyzed by Schwinger in 1964. The result was an alternative derivation of Schwinger's original Lamb shift calculation. Not incidentally, this project also involved the use of variational techniques he had learned from his advisor, and it was Schwinger himself who recommended that since Larry Spruce at NYU was working on variational techniques related to scattering theory, he should join him there with a post-doctoral appointment.

The title of Kimball Milton's talk was "A Remembrance of Julian Schwinger". He was a PhD student of Schwinger at Harvard beginning in 1968. He received his doctorate in 1971, the year that Schwinger left Harvard



Michael Kiefer, Kimball Milton, and Howard Georgi

for UCLA. Milton subsequently joined him at UCLA for the rest of the 1970's as his post-doctoral assistant. He has co-authored two textbooks based on Schwinger's lectures. In addition he cowrote, with Jagdish Mehra, Climbing the Mountain. A Scientific Biography of Julian Schwinger. The authors interviewed several of Schwinger's friends and associates, and Milton recounted many of their recollections in his remarks. Particularly striking to me was a tale I had heard from my own teacher and friend at Syracuse University, Joe Weinberg. While a student at the City College of New York the fifteen year old Weinberg had found himself competing in the library with another youngster of the same age as they retrieved a mathematics book on real variables. Schwinger was the faster reader, but Weinberg was no slouch. They ended up as lifelong friends, actually overlapping for a time with Oppenheimer at Berkeley before the war. It was soon after that Schwinger came to the attention of Rabi who arranged a transfer as an undergraduate to Columbia University. The stories of the gentle prodigy at both institutions are legion and Milton could not begin to cover them. But he did include the episode at CCNY when Schwinger stopped abruptly before the hurdle in the compulsory gym exam, lamenting that there was "not enough time to solve the equations of motion",

and there was also the failed chemistry course at Columbia.

Perhaps less known of Schwinger's myriad accomplishments is the paper he wrote at age 16 at City College in which he effectively introduced the field theoretic interaction representation. (This unpublished paper appears in a collection edited by Milton.). The central role of nuclear physics in his early career has also tended to be unappreciated. Seven papers written under Rabi's guidance constituted his dissertation at Columbia. Most noteworthy was his correct prediction that the deuteron possessed an electric dipole moment arising through a broader isospin interaction. Already during his prewar stay with Oppenheimer and his wartime appointment to the MIT Radiation Laboratory he developed insights into the renormalization program in quantum electrodynamics that would earn him a share of the 1965 Nobel prize in physics. His work with synchrotron radiation not only taught him the importance of Green functions, but also suggested to him that electromagnetic interactions could be interpreted as altering both the mass and the charge of accelerating particles.

Howard Georgi spoke on "Julian Schwinger at Harvard before June, `67 – remembered by a star-struck

Continues on page 9

APRIL SESSION REPORTS: "Centennial of the Eddington Eclipse Expedition" (DGRAV co-sponsor)

By Donald Salisbury

ver a hundred conference participants gathered almost exactly one hundred years after the date in March 1919 when two English expeditions embarked from Liverpool to test Einstein's theory during the solar eclipse of May 29. The positive results reported by Arthur Stanley Eddington and Sir Frank Watson Dyson at a combined London meeting of the Royal Society and the Royal Astronomical Society on November 6, 1919, as reported in the New York Times with the headline LIGHTS ALL ASKEW IN THE HEAVENS, brought Einstein instant worldwide fame. As the first presenter, Daniel Kennefick's title was "No Shadow of a doubt: Eddington, Einstein and the 1919 Eclipse". He had recently thoroughly researched the expeditions of Dyson to Sobral, Brazil and Eddington to the island of Principe and recently published his studies in the book No Shadow of a Doubt. The 1919 Eclipse That Confirmed Einstein's Theory of Relativity. Looking at correspondence between Dyson and Eddington he was able to expose as a myth the claim that since Eddington believed in the correctness of Einstein's theory he had made the data conform with general relativity. It is indeed true that the data from the Greenwich astrographic lens taken in Brazil was excluded, but it was the skeptic Dyson who made this decision without any input from Eddington. The data, which seemed to indicate a deflection of starlight passing the rim of the sun of roughly twice the value predicted by Einstein, was discounted because of probable errors in analyzing the expansion of the photographic plates. The four-inch telescopic data yielded 1.98" compared to Einstein's prediction of 1.75". Because of the partial cloud cover in Principe only five stars were imaged as opposed to the twelve in Sobral, and Kennefick argues that Eddington was justified in assigning a larger uncertainty of 0.30" to the quoted result of 1.61".

Donald Bruns' spoke on "Repeating the Experiment that Made Einstein



Daniel Kennfick [Photo by Donald Salisbury]

Famous". He reported on a remarkably successful optical measurement of starlight deflection that he performed in Wyoming during the August 21, 2017, solar eclipse. The experiment was carefully planned over many months - with the use of optimal amateur astronomical equipment and analysis software. The site selection itself was not accidentally fortuitous. He chose an altitude that reduced atmospheric turbulence and offered high confidence in good seeing conditions. The chief innovation was the use of a CCD camera that permitted much shorter exposure time and reliance on the 2016 Gaia data release both to calibrate the plate scale and to determine the stellar image displacements during the eclipse. His reported value was precisely 1.75" with an extraordinary uncertainty in the deflection of only 0.05". This is by far the best earth-based optical measurement. Radio telescope quasar measurements are of course now able to confirm Einstein's theory with a precision of 0.01%. This work was considered so important from an historical perspective that it was published in Classical and Quantum Gravity.

Jeffrey Crelinsten's title was "Einstein's Jury: Trial by Telescope." He is a science writer who has analyzed the early resistance of US astronomers to Einstein's theory in his text

Einstein's Jury. The Race to Test Relativity. He offered a complementary perspective on historical eclipse observations. A central player in his saga was the astronomer William Wallace Campbell who was based at the Lick Observatory on Mount Hamilton in California. He had planned to observe the August 21, 1914, full eclipse in Russia, but although his equipment did reach his intended site he was forced to abandon his trip because of the outbreak of World War I. This was doubly disappointing since he was then forced to employ inferior equipment in observations he actually undertook in Goldendale Washington in June, 1916. He and his assistant were further hampered by their inability to obtain timely comparison plates that would enable them to compute the stellar deflections. They never did successfully analyze and publish their results from 1916 though rumors occasionally surfaced, even after 1919, that their observations conflicted with Einstein's prediction. Finally in 1922 seven different attempts were made to measure light deflection during the 1922 total eclipse in Australia. Campbell's group made observations on the west coast that were finally announced in 1924 as being in accord with Einstein.

BOOK REVIEW: The Oxford Handbook of the History of Modern Cosmology by Helge Kragh and Malcolm Longair

By Cormac O'Raifeartaigh

A valuable addition to the history of cosmology

It is sometimes claimed that cosmology, the study of the universe, is the oldest of the sciences.

However, there is little question that cosmology only became a quantitative science during the 20th century. Following the advent of Einstein's general theory of relativity and the observation of the first evidence of a universe in expansion, the stage was set for cosmology to emerge as a mature science based on mathematical models combined with astronomical observations and deductions from physical theory.

However, the path from static to expanding cosmologies, and from the discovery of the cosmic microwave background to today's concordance model of the universe was a route with many twists and turns, false trails and cul de sacs. Thus, the publication by Oxford University Press of a handbook on the history of modern cosmology is a most welcome development. As editors Helge Kragh and Malcolm Longair point out in the preface, there are hundreds of books on the emergence of the Copernican model of the solar system, yet few scholarly accounts of the emergence of today's model of the universe.

The book comprises thirteen distinct chapters written by an impressive array of astronomers, astrophysicists, cosmologists and historians of science. The chapters can be read as standalone articles but are arranged in rough chronological order, from Kragh's opening article on pre-relativistic models of the universe in the period 1850-1910 to Longair's paper on the observational and theoretical foundations of today's 'Lambda-CDM' model towards the end of the volume. Along the way, one finds articles on the development of extragalactic astronomy and the discovery of Hubble's law (by Robert Smith), on the rise of dynamic relativistic models of the universe in the 1920s (by Matteo Realdi), and on the long debate between expanding and steady-state

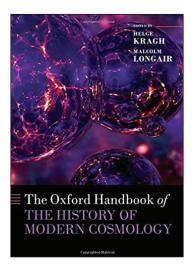
cosmologies (by Kragh). Other chapters include an article on observational and astrophysical cosmology in the period 1940-1980 (by Longair) and a paper on the discovery of the cosmic microwave background (by Bruce Partridge). Of particular interest to many students, practising cosmologists and historians will be the chapter (by Longair and Chris Smeenk) on inflation, dark matter, dark energy and other little-understood components of modern cosmology.

Some of the above may sound a little familiar to readers and this is perhaps one slight problem with the handbook. Much of the material in the first half of the volume has already been presented in classic books such as Kragh's Cosmology and Controversy (Princeton University Press 1996), Longair's The Cosmic Century (Cambridge University Press 2006) and the anthology Finding the Big Bang (Cambridge University Press 2009) edited by Jim Peebles, Lyman Page and Bruce Partridge.

However, it's hard to see how a handbook that seeks to provide a reasonably comprehensive history of modern cosmology in one place can avoid some re-working of this material.

With this in mind, I thought it was a pity that the book did not feature a chapter on the sociological history of cosmology, i.e., on the emergence of modern cosmology as a distinct scientific discipline with its own journals, associations, degree programs and reward system. As the editors point out in the preface, the social history of modern cosmology has received almost no scholarly attention and this would have been a valuable original addition to the literature.

A related but perhaps more serious criticism concerns the description of the development of modern theoretical cosmology. While the story of the emergence of dynamic cosmologies in the first decades of the 20 th century is well covered in early chapters, this narrative is quite well known. By contrast, there is surprisingly little in later chapters on the chronological development of



The Oxford Handbook of the History of Modern Cosmology, Helge Kragh and Malcolm Longair (Oxford University Press 2019)

theoretical cosmology from the 1960s onwards, with only passing descriptions of the key contributions of figures such as Roger Penrose, George Ellis, Robert Dicke, Yakov Zeldovitch and the late Stephen Hawking. It is probably fair to say that the book does not convey a strong sense of the historical development of theoretical cosmology in the second half of the 20 th century, or of the manner in which advances in general relativity impacted on cosmology. Indeed, it could be argued that the second half of the book presents a history of cosmology that is quite phenomenonological; this may be due to a surprising lack of theoretical cosmologists amongst the contributors.

However, these are minor quibbles. All in all, this new handbook provides a comprehensive and highly useful history of modern cosmology that many physicists and science historians alike will want on their shelf for reference.

Cormac O'Raifeartaigh lectures in physics at Waterford Institute of Technology and University College Dublin in Ireland. He is a Fellow of the Royal Astronomical Society and a Fellow of the Institute of Physics.

"Remembering Julian Schwinger"

Continued from page 6

and frequently confused undergraduate". As an undergraduate Georgi took Schwinger's course in source theory at Harvard in 1966. After graduation in 1967 he went on to obtain his Ph. D. from Yale in 1971 where his advisor was Charles Sommerfeld, another of Schwinger's 73 dissertation students. He returned to Harvard in 1971 after Schwinger had left for UCLA.

Schwinger can rightly be identified as a grandfather of electroweak unification. In 1956 he introduced the notion of hypercharge, and in 1957 he alluded to a notion of electroweak unification with a V-A interaction, two neutrinos, and a charged intermediate boson. These are building blocks of the eventual Glashow, Weinberg and Salam theory. Georgi modestly omitted mention of his

own contributions, in particular with the Schwinger student Glashow. But he did stress Schwinger's early mastery of group theory and his suggestion, concomitant with Gell-Mann in 1957, that there exists a larger global symmetry group for which the known baryons formed a multiplet. The suggestion was that this symmetry be broken through interaction with the known mesons.



Forum on History of Physics | American Physical Society, One Physics Ellipse, College Park, MD 20740

Forum Officers

Chair: Paul Cadden-Zimansky Chair-Elect: Joseph D. Martin Vice Chair: Michel H. P. Janssen Past Chair: Daniel Kennefick

Secretary-Treasurer:

Dwight E. Neuenschwander

Forum Councilor

Virginia Trimble

Other Executive Board Members

Melinda Baldwin Robert P. Crease (newsletter) Chanda Prescod-Weinstein Katemeri D. Rosa Alfred Douglas Stone Rebecca Ullrich Audra Wolfe

Program Committee

Chair: Joseph Martin Paul Cadden-Zimansky Michel Janssen

Nominating Committee

Chair: Daniel Kennefick Bruce Hunt Allan Needell Katemari Rosa Rebecca Ullrich

Fellowship Committee

Chair: Michel Janssen Peggy Kidwell A. Douglas Stone

Pais Prize Committee

Chair: Peter Zimmerman Peter Galison Gregory Good Helge Kragh Jinyan Liu