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Cosmology at a Crossroad: The 1958 Solvay Congress*

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Abstract: During the 1950s physical cosmology was in a state of transition characterized by the rivalry between relativistic evolution theories and the new, radically different steady-state theory. Remarkably, big-bang theories played almost no role at all until they were revived in 1964-1965. The Solvay physics congress in June 1958 on "The Structure and the Evolution of the Universe" was the first major international conference ever devoted to cosmology, a field which was still widely considered to be semi-philosophical rather than genuinely scientific. The question of whether the universe could be ascribed a definite age was typically evaded or denied scientific legitimacy. The congress in Brussels offers an interesting perspective of the state of art of cosmology at the time and of how mainstream physicists and astronomers looked upon the possibility of establishing a theory of the universe as a whole. The invited participants in Brussels included the leading steady-state theorists Fred Hoyle, Thomas Gold, Hermann Bondi, and William Hunter McCrea, whereas George Gamow was not invited and his nuclear-physical explosion theory of the early universe not even mentioned. With the Solvay conference as the pivotal point, this paper offers an account of how cosmology changed in the pre-big-bang era from about 1950 to the early 1960s, before the cosmic microwave background radiation entered the scene.

Keywords: Cosmology, Universe, Solvay Conferences, Steady-State Theory, Radio Astronomy.

1. A brief summary of cosmology 1948-1958[1]

By the late 1940s it was generally agreed that the universe expands and that its age could be expressed by a Hubble time T of approximately 1.8 billion years. Unfortunately, this value was much shorter than the age of the stars and even the Earth. This so-called timescale difficulty only eased in 1952, when Walter Baade announced a revised timescale twice as long. Six years later, Allan Sandage (1958, p. 525) concluded that the age of the universe t^*, based on the flat Einstein-de Sitter model where $t^* = 2T/3$, was between 6.5 and 13 billion years. "There is no reason to discard exploding world models on the evidence of inadequate time scale alone," he wrote. With the term "exploding world models" he may have had in mind the big-bang theory of the early universe developed by Gamow and his collaborators Ralph Alpher, Robert Herman, and James Follin in a series of papers between 1948 and 1953.

Gamow's ambitious aim was to account for the formation and abundance of elements in terms of thermonuclear processes taking place in the very early and very hot universe. In the course of this work Alpher and Herman recognized in 1948 that, as a result of the transition from a radiation-dominated universe to one dominated by matter, there would be produced a still existing cosmic background radiation. They estimated its present wavelength to lie in the microwave region and its intensity, as given by the temperature, to be about 5 K. Strangely from a later perspective, this prediction of a cosmic microwave background aroused no interest from physicists and astronomers outside the Gamow group. As far as element formation is concerned, it followed from the theory that the abundance of primordial helium was somewhere between 15 and 36 per cent by mass, but all attempts to build up heavier elements

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[1] For references and more details, see, for example: Bertotti *et al.* (1990) and Kragh (1996).

failed. To make a long story short, after 1953 the Gamow theory (as I shall call it for the sake of brevity) came to a halt. It was further developed only a decade later and then by physicists who came to the hot big bang independently of Gamow's earlier theory.

In 1948, three British physicists – Hoyle, Bondi, and Gold – introduced a new cosmological theory that differed radically not only from Gamow's but also from other models based upon the field equations of general relativity. The steady-state theory, on the other hand, did not admit general relativity to be cosmologically valid and it assumed as a fundamental postulate that the large-scale features of the universe are independent of time. Since the universe expands and the average density of matter remains the same, new matter must be created continually through space if at an exceedingly low rate, namely 10^{-43}g/cm^3/s. The matter creation was *ex nihilo*, meaning that the theory violated the fundamental law of energy conservation.

It further followed from the steady-state theory that space must be Euclidean (hence infinite) and expand exponentially, corresponding to a deceleration parameter $q_0 = -1$.[2] Moreover, galaxies were formed at all times and thus could have widely different ages contrary to the situation in the relativistic evolution models. Finally, it goes without saying that the steady-state universe was eternal in both the past and the future, and also that element synthesis was limited to the interior of stars of different kinds (Section 4).

To discriminate observationally between relativistic expansion models and the steady-state model, astronomers made use of the redshift-magnitude method with roots in the 1930s. By means of this method the space curvature as given by q_0 could in principle be found by measuring the redshifts and apparent magnitudes of a large number of galaxies or clusters of galaxies. In 1956 Sandage believed to have established a value of q_0 much larger than the one predicted by the steady-state theory, but his data were questioned by other astronomers. Despite much work the method did not yield results that unambiguously ruled out the theory of Hoyle and his allies. Although redshift-magnitude observations provided evidence that decreased its credibility, they did not disprove the steady-state universe.

The new science of radio astronomy promised to do better, such as suggested by Martin Ryle and other specialists. With his research group in Cambridge, Ryle studied the correlation of the flux density of radio sources and the number of the sources. The data by this method would – again in principle, but possibly also in practice – show if the universe was in a steady state or not. Ryle had little respect for cosmological theories and disliked the steady-state theory in particular. By the mid-1950s he thought that the Cambridge data were irreconcilable with this theory, but – not unlike the case of Sandage – his conclusion was premature and contradicted by results obtained by other radio astronomers (Section 5).

It needs to be emphasized that still in the 1950s cosmology was widely regarded as a low-status branch of research and not as a respected scientific discipline. Physicists and astronomers engaged in the study of the universe at large were not "cosmologists" and they disagreed to some extent on the fundamental aims, criteria, and methods of cosmology. The lack of professional and disciplinary maturity was, for example, reflected in a public discussion of 1954 between Bondi and the British physicist and astronomer Gerald Whitrow on whether or not physical cosmology qualified to be called a science (Whitrow & Bondi 1954; Kragh 2022). According to some prominent scientists in the period, the choice between cosmological models was as much based on philosophical and aesthetic criteria as on scientific and empirical reasoning. To mention but one example, in 1953 the Swedish theorist Oskar Klein stated that cosmology was a field in which "personal taste will greatly influence the choice of basic hypotheses" (Kragh 1996, p. 223). According to Walter Baade, another participant at the 1958 Solvay conference, theoretical cosmology was "a waste of time" (Osterbrock 2001, p. 205).

[2] With H the Hubble constant, R the scale factor, and $\dot{R} = dR/dT$, the dimensionless deceleration parameter is defined as $q_0 = -\left(\ddot{R}/RH^2\right)_0$.

The immature state of cosmology in the 1950s can be further illustrated from a more sociological perspective. First of all, with an average output of about thirty scientific papers per year, it was a very small field. It lacked social institutionalization in the sense that there were neither university departments nor academic chairs in the subject. To the limited extent that physics and astronomy students were taught cosmology, it was typically as an appendix to courses in astrophysics or general relativity. Textbooks were not missing, but they were few, rarely used in courses, and in some cases not up to date. Worth of mention is Bondi's *Cosmology* published in 1952 with a revised edition four years later. While this book gave much space to steady-state theory, other books more or less defined cosmology as a branch of applied general relativity, such as was the case with George McVittie's *Cosmological Theory* from 1949. In 1950, the French astrophysicist Paul Couderc published a book which two years later appeared in translation as *The Expansion of the Universe*. Couderc (1952, p. 220) was in favour of evolutionary cosmology based on relativity theory and only dealt briefly and critically with the "risky and over-imaginative" steady-state theory. Incidentally, Gamow's name appeared once in Couderc's book and not at all in Bondi's from the same year.

2. Some earlier conferences

Although the 1958 Solvay congress was the first international meeting exclusively devoted to cosmology, it was not the first meeting in which physicists and astronomers discussed topics of a cosmological nature.[3] A session of the British Association for the Advancement of Science in October 1931 arguably qualifies as the first conference in this category (De Baerdemacker & Schneider 2022). Organized by Herbert Dingle, a young British astrophysicist, the session included contributions by some of the leading cosmologists of the period such as James Jeans, Arthur Eddington, E. Arthur Milne, and Willem de Sitter, who all discussed various aspects of the new expanding universe. Another participant was Lemaître, who used the occasion to introduce and promote his daring hypothesis of a primeval atom out of which the present universe had been formed.

Of interest is also a symposium held at the University of Notre Dame, Illinois, in 1938 on "The Physics of the Universe and the Nature of Primordial Particles." It has, perhaps somewhat questionably, been called "the first conference entirely dedicated to the question of cosmology" (Wiescher 2017, p. 45). Arranged by the Austrian-American physicist Arthur Haas, it focused on the physical aspects of cosmology and the relation of this science to the new fields of particle and cosmic-ray physics. Among the speakers were Lemaître, Harlow Shapley, and Carl Anderson, the discoverer of the positron and the mysterious meson or "mesotron" later recognized to be a muon.

The subject of the eighth Solvay congress in 1948 was the physics of elementary particles (Mehra 1975, pp. 239-262). Nonetheless, this meeting also included a contribution to cosmology, a weighty report on the formation of elements in the universe prepared by the Chicago physicists Edward Teller and Maria Goeppert Mayer. While Teller was invited to Brussels, Mayer was not. The report is of interest not least because it discussed, critically and in some detail, the latest version of the big-bang neutron capture theory proposed by Gamow, Alpher, and Herman. This theory was still largely unknown or at least unappreciated in Europe, and so Teller's address effectively introduced Gamow's cosmology to the audience of European physicists gathered in Brussels. Among those who commented on the address was Klein, who at the time had begun working on cosmological problems. He suggested a new "close understanding of the cosmological problem connected with the redshift of the spiral nebulae" (Stoops 1950, p. 87).

The problem of element formation was also discussed at a conference on nuclear astrophysics taking place in Liège, Belgium, in September 1953. This was one of the very few occasions at which steady-

[3] References to these early conferences can be found in: Kragh (1996).

state physicists (Bondi and Gold) met with big-bang physicists (Alpher and Herman). Without endorsing the theory of Hoyle and his allies, most participants ignored or rejected the big-bang theory and its view of element formation. As the French astrophysicist Evry Schatzman commented: “The problem is to study under which conditions the actually observed abundance of the elements have been produced, and not to invent a state of the universe completely different from the one of its actual state” (Ledoux 1954).

Two years later, a large number of physicists convened in Berne, Switzerland, to celebrate the fiftieth anniversary of Einstein’s theory of relativity (Kiefer 2020). Several of the many speakers, among them Howard P. Robertson, Hoyle, Bondi, Otto Heckmann, and Pascual Jordan, addressed questions of cosmology. None of them referred to Gamow’s theory of an explosive universe or, for that matter, to Lemaître’s earlier version of it. In his detailed review of relativistic cosmology, Robertson noted that the accepted value of the Hubble parameter, which he took to be H_0 = 180 km/s/Mpc corresponding to a Hubble time $T_0 = 1/H_0 = 5.4 \times 10^9$ years, was still in conflict with the age of the oldest stars. This made him to reconsider a positive cosmological constant, whereas generally this constant was assumed to be zero. Contrary to the role that cosmology played in the Berne conference, the subject only appeared peripherally in the 1957 Chapel Hill conference, another of the important events in the renaissance of general relativity. As Peter Bergmann (1957) argued, “Cosmology is a field of its own and, at least at present, not intimately connected with the other aspects of general relativity to which this conference has been devoted”.

3. The 1958 Solvay congress

The theme of the tenth Solvay conference on physics, which took place 13-17 September 1954, was “The Electrons in Metals.” When the subject of the following conference had to be decided, Lawrence Bragg, president of the scientific committee, consulted another committee member, the Danish theoretical physicist Christian Møller: “A Solvay Conference on ‘The Structure of the Universe’ would bring together Cosmologists, Physicists and Astronomers, including Radio Astronomers, and the idea of such a Conference appeals to me strongly”, he wrote in a letter of 25 January 1957 (Niels Bohr Archive, University of Copenhagen). Møller wholeheartedly supported the idea and so did other members of the scientific committee, including Wolfgang Pauli, J. Robert Oppenheimer, and Francis Perrin.

It is worth observing that none of the committee members (which also included Nevill Mott, Cornelis Gorter, and Frans van den Dungen) were experts in either cosmology or astrophysics. The eleventh congress was originally scheduled for September 1957, but on the suggestion of Bragg it was postponed to June 1958 so that the participants could visit the large international exhibition in Brussels known as Expo 58 and its famous landmark, the Atomium tower.

As usual, the Solvay congress included, apart from the committee members, a number of invited speakers, discussants, and so-called reporters, among which were Klein from Sweden, John Wheeler from the United States, and the four British steady-state advocates Hoyle, Bondi, Gold, and McCrea. Lemaître from Belgium gave the opening talk. About half of the participants were astronomers or astrophysicists, including Heckmann and Engelbert Schücking from West Germany, Bernard Lovell from England, Schatzman from France, Jan Oort from The Netherlands, Sandage and Harlow Shapley from the United States, and Viktor Ambartsumian from Soviet Russia (Fig. 1). Yet another of the prominent astronomers attending the meeting was Baade, who the following year would return to Germany after nearly three decades in the United States. Several of the participants in Brussels – among them Pauli, Møller, Hoyle, Bondi, Heckmann, and Klein – had also attended and given talks at the Berne relativity conference three years earlier.

Fig. 1. The 1958 Solvay congress. Sitting at the table from the left: W. McCrea, J. Oort, G. Lemaître, C. Gorter, W. Pauli, W.L. Bragg, J.R. Oppenheimer, C. Møller, H. Shapley, and O. Heckmann. Standing from the left: O. Klein, W. Morgan, F. Hoyle, B.V. Kukarkin, H.C. van de Hulst, M. Fierz, A. Sandage, W. Baade, J. Wheeler, H. Bondi, T. Gold, H. Zanstra, L. Rosenfeld, L. Ledoux, A.C.B. Lovell, J. Géhéniau.
(Source: https://commons.wikimedia.org/wiki/File:Solvay_conference_1958_g.jpg)

Twenty-seven years after having introduced the primeval atom, in his 1958 Solvay lecture, Lemaître elaborated on this subject essentially as he had originally conceived it. There was almost no sign in his talk of the advances in cosmology which had taken place during the last couple of decades. Unusually for a Solvay lecture, Lemaître reflected on how science – in this case his cosmological theory – related to Christian faith. He, a Catholic priest, stressed that the big-bang theory of the primeval atom "remains entirely outside any metaphysical or religious question. It leaves the materialist free to deny any transcendental Being [God]". Lemaître also considered the idea of an eternally cyclic universe or what he called a "Phenix universe". However, although he found it to be "quite conceivable", he concluded that "a useful cosmology can[not] be built by starting from a Phenix nucleon gas".

On the more scientific side, Lemaître repeated from earlier writings that the cosmological constant – or what he called the "cosmical constant" – was an indispensable parameter in Einstein's field equations, a view that Wheeler contradicted in his report. Siding with the deceased Einstein, he declared the cosmological constant to be artificial and unreasonable. Unaffected by his critics, by making use of a cosmological constant and assuming a Hubble time of 4 billion years, Lemaître came up with an age of the universe much larger than even the oldest stars and galaxies. "One may confidently put the age of the Universe somewhere between 20 and 60 times 10^9 years", he stated. As indicated by the discussions following Lemaître's talk, those listening to it chose to ignore his philosophical reflections and to disregard his speculations of a primeval atom, which at the time were scarcely taken seriously.

Given the state of affairs in physical cosmology at the time, one scientist was conspicuously missing in Brussels, namely George Gamow. In fact, not only was Gamow not invited, the nuclear-physical cosmology pioneered by him and his assistants was completely absent from the talks and discussions in Brussels. His name does not even appear in the 310-page proceedings volume. As there was no mention of Gamow in Brussels, so there was no mention of the cosmic background radiation predicted from his theory as early as 1948. Of course, given the later development and Gamow's status as the father (or one of the fathers) of modern big-bang cosmology, it is most remarkable that the Solvay meeting proceeded as if he and his theory did not exist. Lemaître was alone in defending a finite-age universe with an explosive beginning, but he did so in his own primeval-atom version and without referring to Gamow's more advanced theory.

Gamow wanted very much to participate in the Brussels conference and told Pauli so. However, Bragg apparently refused to invite Gamow because there were no more vacancies, an excuse that Gamow (1970, p. 124) found hard to accept and interpreted as a result of his uncompromising opposition to the European steady-state cosmology. "I was not surprised (though somewhat disappointed) about the outcome", he wrote in his autobiography, "since I was an opponent of the steady-state theory" (Gamow 1970, p. 126). Although this theory was very much a British one (Kragh 1996, p. 378), there is no reason to suspect that Bragg should have favoured it for chauvinistic reasons and therefore kept Gamow away from Brussels. As Jane Gregory (2005, p. 106) points out in her biography of Hoyle, Bragg was a great supporter of Martin Ryle, who thoroughly disliked the steady-state theory and whose work in radio astronomy did much to discredit this theory.

At the time Gamow's theory of the early universe was widely considered to be wrong or at least inadequate, which may have been the chief reason why he was not invited. But there seems to have been other reasons as well, such as euphemistically suggested in a letter from Pauli (2005, p. 1208) to the Swiss physicist Jean Weigle:

> You remember, that we talked about the fact, that he [Gamow] was *not* invited to the Solvay-meeting in Brussels. Now I just returned from there and heard the true reason for it: there is some trouble with the general conditions of his health, about the details I would prefer to talk rather than to write. I am very sorry for him.

The problem that Pauli did not want to write about was Gamow's excessive consumption of alcohol, which on occasions led to embarrassing scenes at meetings and conferences (Kragh 1996, p. 139; Harper 2001, p. 367).

Hoyle, who had come to Brussels after having attended a conference on radio astronomy in Paris, gave as the only one of the invited scientists two talks. One of them was on the steady-state theory and the other a review of nucleosynthesis in stellar bodies (Section 4). His fellow-cosmologist Gold spoke on "The Arrow of Time" and its significance in cosmology, whereas the contributions of Bondi and McCrea were limited to the discussion sessions.

The fact that no one except Lemaître supported theories of the big-bang type did not mean that most of the attendees in Brussels sympathized with the alternative steady-state theory. On the contrary, Lovell, Oort, and Sandage raised serious objections against this theory, and so did Heckmann, Møller, and Oppenheimer. The latter dismissed it as "quite wrong" (p. 296). Heckmann and his young collaborator Schücking criticized the steady-state theory on methodological grounds. Like many other critics, they objected to the element of continuous creation of matter (pp. 149-150):

> A theory constructed on a sound foundation of empirical data ought not to be discarded unless there [*sic*] new facts turn up that cannot be fitted into the framework of this theory. […] Ten years ago Bondi, Gold, and Hoyle launched their steady-state theory. They denied the validity of the laws of local conservation of energy and momentum […] [but] it is sound policy to refrain from theorizing along the lines of Bondi, Gold, and Hoyle until there is strong empirical evidence for continuous creation of energy and momentum.

According to steady-state cosmology, but contrary to evolution theories based on general relativity, new galaxies were formed at all times. Thus, in any large volume of space there should be very young as well as very old galaxies, a prediction that Oort found to be contrary to observations. This was a main reason why he, at the end of the conference, concluded that "The observational data are in favour of the evolutionary picture and not in favour of the steady state picture" (p. 304).

The discussions in Brussels concerning the rival theories of the universe were on the whole technical and non-confrontational. Those in favour of an evolving universe governed by general relativity listened to and understood, and in some cases even appreciated, the arguments in favour of the steady-state universe, and vice versa. The two parties disagreed, but they spoke the same language. For example, even though Oppenheimer found the steady-state theory to be "quite wrong", he recognized its force when it came to the area of stellar nucleosynthesis. "By providing an incentive for understanding the present state of the cosmos in terms of processes that can now be in progress, this theory has led to the beautiful work reported yesterday by Hoyle on element synthesis", he said.

On the last day of the conference, Shapley expressed his thanks to Bragg for the way he had presided over the sessions. Alluding to what might have turned into an unpleasant clash between protagonists of two very different world views, he said (p. ix): "You have maintained a neutral – I might say neutron – pose during the turbulence, during negative and positive charges and countercharges, the explosions and implosions of gas and argument".

4. Nucleosynthesis

As to the scientific content of the Solvay meeting, I shall deal only with two topics that both were of central importance to the cosmological controversy. One concerns the formation of elements in either the interior of stars or in a primordial state of the universe, and the other is about the significance of radio astronomy as a test of steady-state cosmology in particular.

The problem of nucleosynthesis – to account for the formation and distribution of the chemical elements on a cosmic scale – was part and parcel of Gamow's theory while it had a very different status in the steady-state theory (Kragh 1996, pp. 295-305). The problem was extraneous to the latter theory except that the origin of the elements could not possibly be ascribed to thermonuclear reactions in a hypothetical past state of the universe. The elements had to be formed at all times in existing sources such as stars and novae. In other words, it was an astrophysical problem and not, strictly speaking, a cosmological problem. And yet it was closely if indirectly related to cosmology. It followed from the Hoyle-Bondi-Gold theory that *all* the elements (except hydrogen) were formed in local astrophysical processes and that none of them required the extraordinary circumstances of a hot and very dense big bang. As to Gamow's theory, it had at its disposal two nuclear furnaces, one cosmological and the other stellar, and thus cosmological nucleosynthesis, although essential, was not necessarily the source of the heavier elements. These could well have a stellar origin.

When Hoyle gave his Solvay lecture on "Origin of the Elements in Stars", he had recently completed a comprehensive and impressively detailed theory of how the elements had come into being. What is known as the B^2HF theory was the result of a collaboration which apart from Hoyle involved the Caltech nuclear physicist and later Nobel laureate William Fowler and also Margaret and Geoffrey Burbidge, a married couple of British astrophysicists. In a landmark paper published in 1957 in *Reviews of Modern Physics*, the four physicists were able to account for the origin and abundance of almost all elements and their isotopes, and to do so solely in terms of processes taking place in stars and supernovae. What Hoyle presented in Brussels was essentially a summary version of the B^2HF theory based on an earlier article in the journal *Science*. The talk was strictly technical and limited to nuclear astrophysics with no explicit mention of cosmological models. In response to a question from the Dutch astronomer Hendrik van de Hulst concerning the universal amount of deuterium, Hoyle admitted that it might not be possible to produce enough of this isotope in stellar processes. While this was a potential problem for the B^2HF theory, there was no deuterium problem in Gamow's big-bang theory.

Most of the discussion following Hoyle's report was concerned with astrophysical details, whereas the cosmological implications were only touched upon by two critics of the steady-state theory, namely Heckmann and Oppenheimer. For the latter's comment, see Section 3. Heckmann's question was this (p. 293): "As the discrepancy between the steady-state theory and the nonstationary models has always been in our minds during the discussions, I would be glad if Hoyle could tell us which of the processes he outlined are necessarily connected with the steady-state theory and which are not". Hoyle answered diplomatically that the B^2HF theory was consistent with "both types of cosmology, provided any superdense state of matter that may occur in non-stationary cosmology satisfies the requirement that matter emerges from the superdense state essentially as hydrogen". The "superdense state" was of course a reference to the big bang, a term Hoyle had coined in 1948 but which neither he nor others used at the Solvay conference. A similar passage appeared in the B^2HF paper, but of course Hoyle and his co-authors knew very well that the conditions of the early Gamow universe differed greatly from those in the stars, and so the admission was vacuous. At least indirectly, the success of the B^2HF theory weakened the appeal of the big-bang theory and strengthened that of the steady-state alternative.

As Bondi (1966, p. 400) later expressed it, the stellar theory of element synthesis was a "tremendous triumph" for the cosmological theory of which he had himself been one of the fathers. By that time the classical steady-state theory was barely alive, but Bondi still praised it for having led to great progress in nuclear astrophysics, much like Oppenheimer had done at the Solvay conference eight years earlier. Bondi found it fascinating "that a theory as uncertain as the steady-state theory should have inspired and directly caused one of the most important advances in physics during the last decade, an advance far more firmly grounded than the steady-state theory itself".

In 1958 there still were no reliable measurements of the cosmic abundance of either helium or deuterium. This changed less than a decade later and then the amount of helium in the universe became crucial evidence for the revived big-bang cosmology constructed by James Peebles and others. It took another decade before the abundance of primordial deuterium became known with sufficient accuracy, with the result that the hydrogen-deuterium ratio of approximately one million could be used in refined models of the big-bang universe.

5. Radio cosmology

After it was recognized about 1954 that most radio sources were extragalactic, a few radio astronomers considered how their science might contribute to cosmology and perhaps even decide between rival cosmological models. In Brussels, this was discussed by Lovell, the director of the Jodrell Bank Observatory, in a report titled "Radio-Astronomical Observations Which May Give Information on the Structure of the Universe". The favoured method used by the pioneers of what may be called radio cosmology was to count the number N of radio sources with a flux density larger than a certain value S and then plot in a diagram log N against log S. In a nutshell, while the steady-state model predicted that the sources would lie beneath a straight line with slope −1.5, no corresponding prediction followed from the class of relativistic evolution theories.

Ryle and his Cambridge group concluded in 1955 from the so-called 2C survey that the main part of the sources approximated a line of slope −3 and that the steady-state prediction was therefore proved wrong. As Ryle (1955) put it in his Halley Lecture, "there seems no way in which the observations can be explained in terms of a Steady-State theory". The conclusion agreed with, and may have been coloured by, Ryle's expectation that the steady-state theory could not possibly be correct. However, Bernard Mills and his team of radio astronomers in Sydney, Australia, got quite different results from the southern hemisphere, namely that the slope was −1.8, a value which in 1958 had come down to

−1.65. Might the steady-state theory be viable after all? Was it possible at all to test cosmological models by means of radio-astronomical data? These and other questions related to the confusing situation was what Lovell reviewed in his Solvay address.

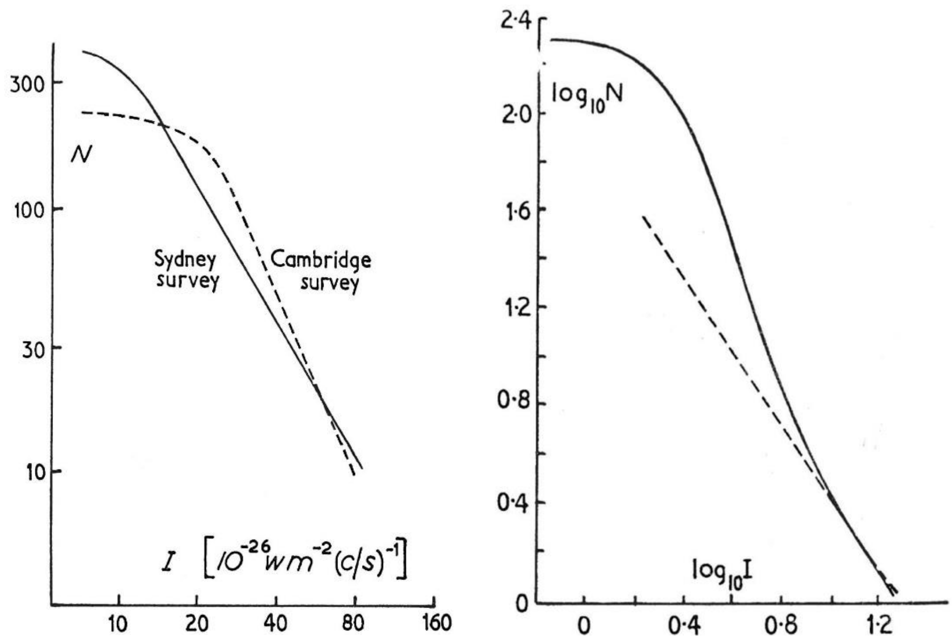

Fig. 2. To the left, Lovell's comparison at the Solvay congress of the log N – log S plots for the Cambridge and Sydney surveys. To the right, the C2 survey compared to the steady-state prediction with a slope of −1.5. Reproduced from: Stoops (1958).

As Lovell noted, the Cambridge and Sydney surveys revealed "a disturbing state of affairs in which two carefully executed series of measurements give results which are quite discordant" (p. 195; Fig. 2). Instead of arguing in favour of one or the other of the rival surveys and their implications for cosmology, he suggested that so far there were no observations based on radio astronomy "which can influence significantly the existing views on the large scale structure of the universe" (p. 201). With respect to the data from Jodrell Bank, nor did Lovell believe that they could be taken as evidence for any particular cosmological model. This somewhat pessimistic or agnostic view may have been shared by most astronomers in the summer of 1958. Following Lovell's talk, Oort was the only one who disagreed. "I am not quite as pessimistic […] with regard to the possibility that counts of radio sources may eventually give information on the large-scale structure of the universe", he stated.

Oort mentioned as another possibility the use of "diameter measures", which was a reference to the new so-called diameter-redshift method proposed by Hoyle and others. In brief, it consisted in correlating the apparent angular diameters of radio (or optical) sources with their redshifts. When plotted in a double-logarithmic diagram, for large redshifts the theoretical curves differed according to the chosen cosmological model (Kragh 1996, pp. 286-287). Although this method could in principle serve as a test for the steady-state model, or for the Einstein-de Sitter model, in practice it turned out to be ineffective as the data were not precise enough. Hoyle too referred to the diameter-redshift method in Brussels, but he only developed it into a cosmological test in a paper published 1959. According to a research project conducted by Jodrell Bank astronomers two years later, there was no obvious relationship between the data and particular cosmological models.

Although the situation in radio cosmology seemed to be a stalemate in 1958, over the next few years Oort's optimism was vindicated. New results from radio observations in both England and Australia turned out to be in approximate agreement and thus gave hope of deciding whether or not the slope in the log $N - \log S$ diagram ruled out the steady-state theory. By 1963 Ryle's group in Cambridge had narrowed down the slope to -1.8 ± 0.1, which agreed convincingly with the slope found by the Sydney group, namely -1.85 ± 0.1. Although the consensus did not kill the steady-state theory, it left it seriously wounded and with practically no support from the astronomical community. It was the beginning of the end for the now 15-year-old cosmological theory. But of course, all this was not known to the physicists and astronomers gathered in Brussels.

6. Conclusion

The 1958 Solvay conference on the structure of the universe marked an important change in the scientific reputation of physical cosmology. By admitting studies of the universe at large as a field worthy of one of the prestigious Solvay congresses, cosmology was placed on the same level as, for example, elementary particle physics and solid-state physics, the subjects of the previous Solvay meetings. Cosmology received a semi-official stamp not only as a proper science but also as a fundamental and most exciting one.[4] The meeting in Brussels took place at a time when the steady-state theory of the universe was still much alive and when relativistic models with an explosive beginning in time were not highly regarded. One indication of the low regard was that Gamow was not invited to the conference, and another was the conspicuous absence of big-bang theory from the conference proceedings. On the other hand, Lemaître was present, but his opening talk was effectively the swan song of his old primeval atom theory which at the time was half forgotten and no longer appreciated by the majority of cosmologists.

The meeting in Brussels was also of importance because it set a precedence for further Solvay conferences on cosmology and related sciences. Indeed, in 1964 – shortly before the big-bang revolution – the thirteenth conference was on "The Structure and Evolution of Galaxies" and in 1973 the theme of the sixteenth conference was "Astrophysics and Gravitation" (Mehra 1975, pp. 388-404). Among those present at the 1958 congress, several also attended the 1964 congress. Oppenheimer, Bragg, and Møller served as members of the scientific committee, and invited speakers and participants included Hoyle, Oort, Lovell, Sandage, and Schatzmann. A closer, comparative and contextual investigation of the early cosmology-related Solvay meetings would be of great historical interest and especially so if it took advantage of the unpublished material kept at the Solvay archives and elsewhere.

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[4] It took considerably longer until cosmology was accepted by the Nobel institution. Hubble and Lemaître were both nominated but without being considered serious candidates. The first Nobel Prize awarded for a work in cosmology was the one of 1978 to Arno Penzias and Robert Wilson for their discovery of the cosmic background radiation. See: Kragh (2017).

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