"The most philosophically of all the sciences": Karl Popper and physical cosmology

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Abstract Problems of scientific cosmology only rarely occur in the works of Karl Popper, the famous Austrian-British philosopher. Nevertheless, it was a subject that interested him and which he occasionally commented on. What is more important, his general claim of falsifiability as a criterion that demarcates science from non-science has played a significant role in periods of the development of modern physical cosmology. The paper examines the historical contexts of the interaction between cosmology and Popperian philosophy of science. Apart from covering Popper's inspiration from Einstein and his views on questions of cosmology, it focuses on the impact of his thoughts in two periods of controversy of modern cosmology, the one related to the steady state theory and the other to the recent multiverse proposal. It turns out that the impact has been considerable, and continues to be so, but also that the versions of Popperian methodology discussed by cosmologists are sometimes far from what Popper actually thought and wrote.

1 Introduction

While Karl Popper's philosophy of science has only few followers among modern philosophers, it is easily the view of science with the biggest impact on practicing scientists. According to Peter Medawar, Nobel laureate and eminent physiologist, Popper was the greatest authority ever on the scientific method. He praised the "great strength of Karl Popper's conception of the scientific process," a main reason for the praise being "that is realistic – it gives a pretty fair picture of what goes on in real life laboratories."[1] Either explicitly or (more often) implicitly, many scientists subscribe to some version of simplified Popperianism, usually by adopting the demarcation criterion that a scientific theory must be falsifiable. This criterion has played an important role in science ever since the publication of *The Logic of Scientific Discovery* in 1959. One of the sciences where this continues to be the case is cosmology, which Popper once called "the most philosophically of all the sciences."[2] Yet his own comments on cosmological research were few and scattered, in marked contrast to the positive reception of his ideas in the community of physicists, astronomers, and cosmologists.

In this paper I deal in some detail with the interaction between Popper's philosophy of science and developments in physical cosmology after World War II. This is a topic that has received little attention in either the historical or philosophical literature but deserves a closer examination.[3] Apart from examining what little can be found in Popper's writings, I include in Section 5 some new information about his late opinion of cosmological models, as he described it in a hitherto unknown letter shortly before his death in 1994. The way in which Popperian methodology has influenced the development of cosmology is analyzed by looking in particular at two important controversies, the one (the steady state debate) dating from the 1950s and the other (the multiverse debate) from the first decade of the twenty-first century. It is hardly surprising that the version of Popperianism appearing in scientific cosmology, as in science in general,

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[1] Medawar (1990, p. 100).
[2] Popper (1994, p. 59).
[3] For the case of the steady state controversy, see Kragh (1996, pp. 244-246), and for the later impact of Popper's ideas, Sovacool (2005).

differs quite substantially from what Popper himself believed and wrote. This study has two purposes: it aims at adding to the Popper scholarship by looking at Popper's ideas of cosmology; and it aims at adding to the history of modern cosmology by investigating how Popper's philosophical ideas influenced the development. Thus, it is placed at the crossroad between the histories of science and philosophy.

2 Popper and Einstein

In January 1921 eighteen-year old Popper listened to a public lecture Einstein gave in Vienna's crowded Concert Hall, but the relativity theory that Einstein talked about "was quite beyond my understanding," as he later recalled. Yet, shortly thereafter he was introduced to Einstein's theory and what he called his "marvellous idea of a new cosmology – a finite but unbounded universe."[4] Although he did not develop an interest in this particular topic, he was greatly inspired by the general theory of relativity and Einstein's attitude to observational tests. Einstein insisted that if there were *conclusive* evidence against a theory, it had to be abandoned. Referring to the prediction of a gravitational redshift, he wrote: "If the displacement of spectral lines towards the red by the gravitational potential does not exist, then the general theory of relativity will be untenable."[5] This was an attitude that impressed Popper and which he approvingly quoted in his autobiography.[6]

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[4] Popper (1974a, pp. 28-29). The friend who introduced him to Einstein's general theory of relativity was a student of mathematics by the name Max Elstein.

[5] Einstein (1920, p. 132). Einstein (2004, p. 304) said the same in a letter to Eddington of 15 December 1919. See also Hentschel (1992), where Einstein's attitude to empirical testing is analyzed and provided with documentary evidence. As pointed out by Holton (1998, p. 8), the earlier editions and printings of Einstein's *Über die*

Popper first published his later so famous demarcation criterion in a letter of 1933 to *Erkenntnis*, the journal for analytic philosophy founded a few years earlier by Hans Reichenbach and Rudolf Carnap. It reads: "Statements, or systems of statements, convey information about the empirical world only if they are capable of clashing with experience; or more precisely, only if they can be subjected … to tests which *might* result in their refutation."[7] A few lines later he reformulated the demarcation criterion by paraphrasing a formulation by Einstein in an address of 1933: "In so far as a scientific statement speaks about reality, it must be falsifiable; and in so far as it is not falsifiable, it does not speak about reality."[8] A very similar formulation appeared in Popper's extensive manuscript, *Die beiden Grundprobleme der Erkenntnistheorie*, which was only published in 1979.[9]

At several occasions Popper described key elements in his philosophy of science as crucially relying on, or even derived from, Einsteinian physics and methodology. In a BBC radio programme he expressed his indebtedness to Einstein as follows: "Einstein's influence on my thinking has been immense. I might even say that what I have done is mainly to make explicit certain points which are implicit in the work of Einstein. … The Einsteinian revolution has influenced my own views deeply:

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spezielle und allgemeine Relativitätstheorie (1917-1919) did not include either this sentence or something to the same effect.

6 Popper (1974a, p. 29).

7 Popper (1932-1933), as translated in Popper (1959, pp. 312-314). Quotation on p. 313.

8 Popper (1959, p. 313). In his address of 1921 on "Geometry and Experience," Einstein (1962, p. 119) said: "In so far as the statements of mathematics refer to reality, they are uncertain; and in so far that they are certain, they do not refer to reality."

9 An English translation has only recently appeared: Popper (2008).

I feel I would never had arrived at them without him."[10] This is also what he said in his autobiography:

> What impressed me most was Einstein's own clear statement that he would regard his theory as untenable if it should fail in certain tests. … Thus I arrived, by the end of 1919, at the conclusion that the scientific attitude was the critical attitude, which did not look for verifications but for crucial tests; tests which could *refute* the theory tested, though they could never establish it.[11]

However, his retrospective accounts of the genesis of his ideas on induction and demarcation should not be accepted uncritically. These ideas may not have had their roots in Einsteinian physics versus psychoanalysis and Marxism, but rather in Popper's occupation with pedagogy and psychology. Moreover, he most likely arrived at the ideas only in 1929 or 1930, about a decade after he learned about Einstein's attitude to critical tests.[12] On the other hand, there is no reason to doubt Einstein's crucial influence on Popper's development of his philosophy. This influence is illustrated by the first edition of *The Logic of Scientific Discovery*, where Einstein is the second-most cited author, whether scientist or philosopher. Only the German

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[10] Whitrow (1967, pp. 23-25).

[11] Popper (1974a, p. 29). Also in Popper (1963, p. 39) did he date the falsifiability criterion to the winter of 1919-1920. At the time it seemed "almost trivial" to him, and so it took thirteen years before he published it.

[12] See ter Hark (2002) and also Eggers Hansen (2006) from which it appears that still in 1927 Popper considered laws of nature to be verifiable and based on inductive reasoning. Only in *Die beiden Grundprobleme* did he reach the conclusion that laws of nature must be falsifiable and not verifiable.

logician and philosopher Rudolf Carnap, the leading figure in the Vienna positivist circle, received more references.

As to Einstein, he recognized in Popper a kindred spirit. Having read *Logik der Forschung* shortly after it appeared in the autumn of 1934, he wrote to Popper: "Your book has pleased me very much in many ways: rejection of the 'inductive method' from an epistemological standpoint. Also falsifiability as the crucial element of a theory (of reality) … . You have further defended your positions really well and astutely."[13] He even offered to bring Popper's book to the attention of his colleagues in physics and asked Popper of how he could best help him.

The close affinity between the views of Einstein and Popper, and therefore the reason why Einstein greeted *Logik der Forschung* with such sympathy, is perhaps best illustrated by an essay Einstein wrote in late 1919 in the *Berliner Tageblatt*. Here he spelled out his conviction that the creative scientist must always start with a hypothesis, which may be nothing but a preconceived view of intuition. The scientist then deduces certain consequences from his hypothesis, and these can be confronted with nature:

> A theory can thus be recognized as erroneous [*unrichtig*] if there is a logical error in its deductions, or as incorrect [*unzutreffend*] if a fact is not in agreement with its consequences. But the truth of a theory can never be proven. For one never knows that even in the future no experience will be encountered which contradicts its consequences;

[13] Einstein to Popper, 15 June 1935, as quoted in Van Dongen (2010, p. 43).

> and still other systems of thought are always conceivable which are capable of joining together the same given facts.[14]

Also Einstein's mature philosophy of science, as he formulated it in a letter of 1952 to his friend Maurice Solovine, was in broad agreement with Popper's views.[15]

From 1937 to 1945 Popper stayed in Christchurch, New Zealand, where he taught philosophy at the University of Canterbury. He subsequently moved to England to take up a position at the London School of Economics and Political Science, and in 1949 he was appointed professor of logic and scientific method at the University of London. Einstein had since 1933 been at the Institute for Advanced Study in Princeton. It was only in 1950, when invited to give a paper in Princeton on indeterminism in quantum physics, that Popper came to meet the physicist he admired so much. On this occasion he discussed, both with Einstein and Kurt Gödel, a cosmological model that Gödel had recently proposed on the basis of Einstein's field equations and in which there was no cosmic time.[16] Gödel's strange universe was rotating and stationary – hence with no redshifts – and allowed for time travels into both the past and the future. Understandably, Einstein rejected the model as nothing but the brainchild of a clever mathematician. What matters here is that Popper seems to have been well

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[14] Einstein, "Induktion und Deduktion in der Physik," *Berliner Tageblatt*, 25 December 1919, reprinted in Einstein (2002, p. 219). English translation in Adam (2000), who discusses the significance of Einstein's essay in relation to Popper's views. However, Popper was unaware of the essay and only came to know about it in 1983, when the physicist and Einstein scholar John Stachel approached him on the matter.

[15] Einstein to Solovine, 7 May 1952, as analyzed in Holton (1998, pp. 28-56).

[16] For Gödel's model universe and sources relevant to it, see Rindler (2009).

acquainted with Gödel's model universe and its deficiencies, indicating that he was able to follow the technical literature in mathematical cosmology.[17]

It is debatable whether Einstein, in his scientific practice, can be characterized as a Popperian. Yet the affinity between his views and those of Popper was not limited to the rhetorical level, as may be illustrated by his response to the time-scale difficulty that plagued cosmology for more than twenty years. In most relativistic models of the expanding universe the age T is of the order of the Hubble time, or the inverse of the Hubble constant H. The problem was that the age, as derived in this way, was not only shorter than the age of the stars but also of the reliably determined age of the Earth. For example, according to the widely favoured Einstein-de Sitter model, the age is given by $T = 2/3H$, which resulted in $T \cong 1.2$ billion years. There were ways to avoid the paradox, but Einstein insisted that the anomaly should be taken seriously and not be explained away. "The cosmologic theory here presented," he wrote in 1945, referring to the relativistic big bang theory, "would be *disproved* if it were found to contradict any such results. In this case I can see no reasonable solution."[18]

Einstein's attitude was in the spirit of Popperian falsificationism, such as was his attitude in 1919 with respect to the gravitational bending of starlight. The "if" should be noticed, however, for it indicates that Einstein was not, after all, convinced that the cosmologically derived age of the universe was an incontrovertible fact. But this qualification, too, agrees with

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[17] See Popper (1974a, p. 172).

[18] Einstein (1956, p. 132), emphasis added. On the time-scale difficulty, see Kragh (1996, pp. 73-79, 271-275) and Brush (2001). The anomaly only disappeared in the 1950s, when it was realized that the Hubble constant is much smaller than originally assumed. By 1960 the age of the Einstein-de Sitter universe was taken to be at least 7 billion years.

the view of Popper according to whom a scientist should not instantly abandon a theory that leads to disagreements with data (see Section 6).

3 Physical cosmology in Popper's work

As we shall see in the following sections, Popper's philosophy of science has played, and continues to play, an important role in methodological debates concerning cosmology. On the other hand, one looks in vain in his main works for discussions of the science of the universe. Quantum mechanics, special relativity theory, thermodynamics, and statistical physics appear prominently, and within these branches of foundational physics Popper's contributions are still recognized as important.[19] But he was nearly silent about cosmology in the meaning of space-time theories of the universe at large. The silence is not complete, though, for if one looks closely at his books and papers, questions of cosmology do turn up, if only briefly and scattered.

Before we look at these, it needs to be pointed out that while the term "cosmology" appears repeatedly in Popper's writings, in far most of the cases it does not refer to scientific cosmology, whether of the physical, observational, or mathematical kind. "All science is cosmology," he wrote in the preface to the English edition of *Logik der Forschung* that appeared in 1959 as *The Logic of Scientific Discovery*.[20] However, he used the term in a very broad sense, stating that the problem of cosmology was "the problem of understanding the world – including ourselves, and our knowledge, as part of the world." This was a sense not very different from, for example, the one adopted by Alfred North Whitehead, whose main work, *Process and Reality*

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[19] See the survey in Jammer (1991).

[20] Popper (1959, p. 15). The sentence also appeared in some of his later works, e.g., Popper (1982, p. 1).

from 1929, carried the subtitle *An Essay in Cosmology*. When Popper dealt with the history of cosmological thought, it was not with developments in the twentieth century, but with the Presocratics, Plato, Copernicus, and Kant.[21] Parmenides and Kant were his favourite cosmologists. In 1982 he said that his ideas about the quantum world had been inspired by "the problems of physical cosmology," yet he used the term in a meaning entirely different from the one adopted by astronomers and physicists. Tellingly, his interest in what he called physical cosmology – but what others might perhaps call metaphysics or natural philosophy – led him "to biology, to the human mind, and to the products of the human mind."[22]

In spite of his silence about scientific cosmology in *Logik der Forschung*, Popper was aware of the developments initiated by Einstein's seminal paper of 1917 in which Einstein had applied his general theory of relativity to the entire universe. He seems to have been broadly aware of what happened in cosmology, but not to have followed the development closely. Shortly before his death in 1994, he described himself as "an ardent admirer of Friedmann's suggestion," a reference to the Russian physicist Alexander Friedmann who in 1922 had shown that there are expanding and other dynamical solutions to Einstein's cosmological field equations. According to Popper, Friedmann's cosmology "made the explosion (now called the 'Big Bang') a simple explanation of all that was then 'known empirically' about the Universe."[23] Moreover, he considered it a

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[21] See, for example, the essay "Kant's critique and cosmology," reprinted in Popper (1963, pp. 175-183). The essay was first published in *The Listener* 51 (1954), 291-292, 303.

[22] Popper (1982, p. 31).

[23] K. Popper to H. Kragh, 10 June 1994. The letter, presumably one of Popper's last (he died on 17 September 1994), was a reply to some questions I had asked him in preparation of a book on the history of modern cosmology (Kragh 1996). It is now

simplification of Einstein's original theory because it made the cosmological constant Λ an unnecessary assumption.

Only at one occasion, a little known paper of 1940 that has never been reprinted, did Popper enter the discussion in cosmology. At that time the Einstein-Friedmann theory was challenged by a very different system of cosmological physics proposed by E. Arthur Milne, a brilliant and unorthodox British astrophysicist. It was part of Milne's system that physical events could be described in different time scales, leading to different but physically equivalent descriptions. For example, according to one time scale t the universe would expand, while according to another time scale τ, logarithmically related to the first ($t \sim \log \tau$), it would remain in a static state.[24] Popper's purpose with his letter in *Nature* was to discuss the generally accepted expansion of the universe in relation to theories that explained Edwin Hubble's redshift data on the assumption of special mechanisms operating in a static universe. According to one alternative the speed of light decreased with time, while another alternative (the class of "tired light" hypotheses) assumed that light gradually lost energy during its journey through empty space from the nebulae to the Earth.

Clearly inspired by Milne, Popper examined two alternatives to the relativistic theory of cosmic expansion, arguing that they agreed in regard to observable effects, that is, they led to galactic redshifts of the kind predicted

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deposited at the Karl Popper Library, Klagenfurt University, Austria. The main content of the letter is quoted in a note forthcoming in the *Journal for the History of Astronomy*. Friedmann's ideas of a universe evolving in time (which included both $\Lambda = 0$ and $\Lambda \neq 0$) became generally known and accepted only after 1930, so Popper's admiration presumably refers to the 1930s. For the early cosmologies of Einstein, de Sitter, Friedmann and others, see, for example, North (1965).

[24] On Milne's system, known as the theory of "kinematic relativity," see Lepeltier (2006) and Kragh (2011, pp. 101-108). For contemporary assessments from a philosophical point of view, see Cohen (1950) and Grünbaum (1952).

by the expansion theory. "The three theories are logically equivalent," he wrote, "and therefore do not describe alternative *facts*, but the same facts in alternative *languages*." But does the universe *really* expand? Or does the speed of light instead decrease? Or is it rather the case that the frequency of photons changes with the distance they travel? According to Popper, the question was "not more legitimate than, when prices of goods fall throughout the economic system, to ask whether 'in reality' the value of money has increased or the value of the goods has decreased."[25] Nevertheless, he considered Milne's alternative a simpler and therefore more attractive explanation of the observed redshifts. Popper's intervention in the debate concerning the expanding universe did not arouse much attention. It seems to have received only one response, from the English astrophysicist and philosopher of science Herbert Dingle.[26]

At two other occasions did Popper take up topics of cosmological relevance, although in none of the cases in ways that directly related to research going on in scientific cosmology. In 1953 the newly established *British Journal for the Philosophy of Science* announced a prize essay on "the logical and scientific status of the concept of the temporal origin and age of

[25] Popper (1940, p. 70). According to Popper's letter to the author (note 23), he was at the time interested in non-Doppler explanations of the redshifts and later published a remark in favour of redshifts increasing with distance because of collisions with particles in intergalactic space. I have been unable to trace this remark, which is not mentioned in any of the Popper bibliographies, and suspect that Popper's memory failed him.

[26] Dingle (1940), who criticized Popper's argument, accusing it of being an example of "a very dangerous tendency in modern physics, … , namely, a retreat from experience into the world of pure logic." Dingle was an outspoken critic of Milne's theory. He later became friendly with Popper, with whom he corresponded with regard to the validity of the special theory of relativity. See Hayes (2010).

the universe."[27] This was of course a central question in the controversy between the steady state theory and the big bang theory that was raging at the time. The twenty-six essays were judged by a committee including Popper, the other members being Harold Jeffreys (a mathematician and geophysicist), Fritz Paneth (a chemist), and Lancelot Whyte (a physicist and science writer). Half of the first prize in the competition was awarded the American philosopher Michael Scriven, who in his essay denied that it could be decided on a scientific basis whether the age of the universe is finite or infinite.[28] Popper may have agreed.

The essay of the other prize winner, the physical chemist J. T. Davies of King's College, London, is of interest because it demonstrates the influence of Popper's ideas. In his discussion of how to choose between rival theories of the universe, Davies stressed that the ultimate test was to compare deductions with new observations. He referred twice to Popper's *Logik der Forschung*, which he evidently had studied carefully: "Unless we can conceive of a critical observation which could distinguish between rival theories, the latter are either 'unscientific' or are stating the same thing in different guises. As Popper has emphasised, the criterion of a scientific theory is that it must be possible for an observational check to be devised (however impractical with existing techniques), by which it might be disproved."[29]

If science were powerless to decide between an eternal and a finite-age universe, perhaps there could be given philosophical and logical arguments that ruled out an infinite past? This is an old question, much

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[27] See *British Journal for the Philosophy of Science* 4 (1953): 92; 5 (1954): 179, and *Nature* 175 (1955): 68-69.

[28] Scriven (1954). See also Brush (2001, p. 168).

[29] Davies (1954, p. 199).

discussed already in the middle ages, and in 1978 the astronomer Gerald Whitrow argued from reasons of logic that an infinitely old universe is indeed impossible.[30] Popper replied that such a priori reasoning was invalid and that the question might well be scientifically meaningless. Referring to Milne's two time scales, by means of which a universe without a beginning could be transformed into one with a beginning, he wondered "whether there is an ontological difference corresponding to the difference between a time co-ordinate reaching into an infinite past and a time co-ordinate with a beginning."[31] His reasoning was thus of the same kind as in his paper of 1940. Of course, by the late 1970s the big bang model had obtained a paradigmatic status and the large majority of cosmologists were convinced that the universe had existed for only a finite number of years. In his reply to Whitrow, Popper did not refer to the physical arguments for a finite-age universe, but he implicitly denied that these rigorously proved a big bang universe of finite age. As far as he was concerned, the universe might have existed for an infinity of time.

Much interested in questions of the direction of time and the irreversibility of physical processes, in the period 1956-1967 Popper wrote several papers on these and related subjects in *Nature* and the *British Journal for the Philosophy of Science*. The problem of the so-called arrow of time is a classical one in natural philosophy, and it is no less important in foundational physics. Does the second law of thermodynamics provide an arrow of time? Or do the laws of electrodynamics perhaps provide one? The

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[30] Whitrow (1978).

[31] Popper (1978). See also Craig (1979), who responded to the arguments of Whitrow and Popper and related them to contemporary developments in physical cosmology. Craig suggested that Popper's arguments were unconvincing and that modern cosmology and astrophysics confirmed that the age of the universe was of the order ten billion years.

question entered cosmology in the 1960s, when several physicists and cosmologists discussed the expansion of the universe as a cosmological arrow of time and how it related to the one based on electromagnetic signals. Some advocates of the steady state programme, including Fred Hoyle, Jayant Narlikar, and Dennis Sciama, argued that only the steady state theory was able to explain why we live in a world with a sharp difference between signals coming from the past and from the future.[32]

At about the same time that the arrow of time problem was discussed among cosmologists, Popper examined it independently and from a more general perspective. In a paper of 1965 he argued that the entropic theory of time, according to which the growth of entropy as given by thermodynamics defines the direction of time, was unfounded and in any case of no cosmological significance.[33] Radiation processes, he suggested, were the only irreversible processes that were relevant on a cosmic scale. At a later occasion he explained that when he insisted that time has an arrow, it was to emphasize that the universe has a history: "'The present age of the universe' is a perfectly good term in cosmology. … In other words, the past, present, and future are perfectly good terms in cosmology and astronomy."[34] Popper's approach was primarily philosophical, and he did not relate his work on the arrow of time to the contemporary debate in the small community of cosmologists.

In a rejoinder of 1967 to critical objections raised by a German philosopher, W. Büchel, Popper expanded his discussion of irreversibility to

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[32] See Kragh (1996, pp. 368-372) and Sánchez-Ron (1990).

[33] Popper (1965). For a critical discussion of Popper's view, see Esfeld (2006).

[34] Popper (1974b, p. 1143). The complex of problems Popper dealt with continues to attract scientific and philosophical interest. See, for example, the contributions in *Studies in History and Philosophy of Modern Physics* 37: 3 (2006), a special issue on the arrows of time.

cover some of the relativistic models of the universe, including the cyclic model that Einstein had proposed in 1931.[35] For this particular case, which assumes a positive curvature of space, he admitted that the entropy would increase continually. The 1967 note demonstrates that Popper had a clear grasp of the essence of relativistic cosmology and also indicates that he did not believe in a universe of the cyclic type. In the case of $\Lambda = 0$, a positive space curvature necessitates an average mass density ρ greater than a certain value known as the critical density and given by $\rho_{crit} = 3H^2/\kappa$, where H is Hubble's expansion parameter and κ Einstein's gravitational constant related to Newton's constant G by $\kappa = 8\pi G/c^2$. Popper apparently thought that the condition $\rho > \rho_{crit}$ was an untestable hypothesis, or rather that Einstein thought so. Relying on Einstein's *The Meaning of Relativity*, Popper wrote: "He [Einstein] regarded the conjecture $\rho > 3H^2/\kappa$ as untestable, and the opposite conjecture as testable or refutable, which might make it preferable to some."[36]

As Einstein pointed out, the gravitating mass density ρ is likely to consist of two components, one due to nebulae and other radiating matter ρ_r and another to non-radiating or dark matter ρ_d. The total mass density and therefore the curvature of space will depend on $\rho = \rho_r + \rho_d$ or ρ_d/ρ_r. If $\rho_r < \rho_{crit}$, we may still have $\rho > \rho_{crit}$, a positive curvature. Einstein showed that the lower bound for ρ will depend on ρ_d/ρ_r, a quantity which can be estimated from the spectroscopically determined velocities of the galaxies in a globular cluster. "On the other hand," he wrote, "I cannot think of any reasonably reliable determination of an upper bound for ρ." Popper seems to have read Einstein as saying that a closed universe cannot be falsified, for the only way

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[35] Popper (1967). Büchel (1967).

[36] Popper (1967). Einstein (1956, pp. 131-132). Notice Popper's identification of "testable" and "refutable."

will be to show that $\rho \leq \rho_{crit}$; and even if it is shown that $\rho_r \leq \rho_{crit}$ it can always be argued that $\rho = \rho_r + \rho_d > \rho_{crit}$.

At about the time Popper wrote his 1967 note to *Nature*, there had occurred something like a paradigm shift in cosmology, with the emergence of the standard hot big bang theory. Popper did not comment explicitly on the new physical cosmology, but in a few cases he indicated his view. In a plenary lecture on "The Theory of the Objective Mind" delivered in 1968 to the 14th International Congress of Philosophy in Vienna, he briefly dealt with cosmological models – "the most interesting kind of all hypotheses," he called them. Some of these, he said, "can of course be tested, and some have been even sufficiently precise for refutation. But others, and very interesting ones, seem to be untestable, and may remain so."[37] He may have thought of the steady state theory and the new big bang theory, respectively, but did not elaborate. In a more general way Popper referred to "the infinitely improbable" success of modern cosmology in the 1974 festschrift issued in the *Library of Living Philosophers* series. He said:

> Our theories tell us that the world is almost completely empty, and that empty space is filled with chaotic radiation. And almost all places which are not empty are occupied either by chaotic dust, or by gases, or by very hot stars – all in conditions which seem to make the application of any physical method of acquiring knowledge impossible. … Modern cosmology teaches us that to generalize from

[37] Popper (1972, p. 186). First published 1968 in German, in the proceedings of the Vienna international congress.

> observations taken, for the most part, in our incredibly idiosyncratic region of the universe would almost always be quite invalid.[38]

From what little Popper wrote about cosmology one gets the feeling that he did not like the standard big bang theory. At a conference in 1972 he admitted that the theory was "at present the most widely accepted theory of the origin of the universe," but in his view it was nonetheless highly precarious. The cosmological origin of the content of helium in the universe he called a "speculation." Popper indicated some sympathy for a recent proposal that did not presuppose either a big bang or an expanding universe.[39] More generally he considered cosmology and cosmogony – "though immensely fascinating parts of physics" – to be somewhat immature sciences, which, "though they are becoming better testable, are still almost borderline cases of physical science."[40]

Popper's most explicit public comments on modern cosmology appeared in a lecture given in 1982 at the European Alpbach Forum in Alpbach, Austria. Cosmology – "the most philosophically important of all the sciences," he repeated – had undergone a revolutionary development during the last few decades. He praised the by then defunct Bondi-Gold-Hoyle theory as "a very fine and promising theory," not because it was true but because it was testable and had in fact been falsified. As a result of measurements based on methods of radio astronomy, "it seems to have been refuted in favour of the (older) big bang theory of expansion." Popper did

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[38] Popper (1974b, p. 1027). Since Popper wrote this, the universe has turned even more exotic and the success of cosmology even more improbable. The universe is no longer almost completely empty, but filled with dark energy and dark matter.

[39] He referred to Pecker, Roberts and Vigier (1972), who proposed inelastic photon-photon interaction as an explanation of galactic redshifts.

[40] Popper (1974c, pp. 267-268), based on a lecture given at a conference on reductionism in biology in Bellagio, Italy, 9-16 September 1972.

not mention the cosmic microwave background radiation or other evidence (such as the measured amount of helium in the universe) that had laid the steady state theory in the grave. Without speaking out against the big bang theory, he remarked that "we seem to be almost as helpless in the field of cosmology in the face of some of these revolutionary results as we are in politics when faced with the task of making peace."[41]

Given that radio astronomy represented "a most exciting and revolutionary episode in the history of cosmology," Popper found it pertinent to suggest that the discovery of the method did *not* originate from the technical invention of an instrument, the radio telescope. No, it was the *idea* of using radio waves emitted by stellar bodies that was important, because "I believe that the history of science is essentially a history of ideas."[42] The remark underlines how little appreciation Popper had for discoveries based on advances in experimental and observational techniques. He was interested in the "logic of scientific discovery," but discoveries themselves were foreign to his system of philosophy. This system portrayed science as a series of critical discussions, not of real discoveries made in the laboratory or in the astronomical observatory. As the Australian philosopher David Store quipped, in Popper any "actual discovery would be as out of place as a hippopotamus in a philosophy class."[43]

Moreover, according to Popper experiments and observations are wholly subordinated ideas and theories. Science starts with problems rather

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[41] Popper (1994, pp. 58-60). Even before the discovery of the cosmic microwave background in 1965, the steady state theory was contradicted by surveys of radio sources made by Martin Ryle and others, which evidently impressed Popper. For this method and its role in the refutation of the steady state theory, see Kragh (1996, pp. 305-317, 323-331).

[42] Popper (1994, p. 59).

[43] Stove (1982, p. 13).

than with observations. What he called his epistemological theory of experiment was this: "The theoretician puts certain definite questions to the experimenter, and the latter, by his experiments, tries to elicit a decisive answer to these questions, and to no others. … Thus it is he [the theoretician] who shows the experimenter the way."[44] Perhaps it was this attitude that caused him to ignore the discovery of the cosmic microwave background and its crucial role in the revival of the big bang universe. The Nobel Prize-rewarded discovery of Arno Penzias and Robert Wilson was serendipitous and not motivated by theory at all.[45] It was not a discovery that fitted into Popper's epistemological theory of experiment.

4 The controversy over the steady state model

The steady state theory of the universe aroused a great deal of philosophical interest, in part because of the theory's controversial claim of continual creation of matter and more generally because of its appeal to philosophy and methods of science. For example, in their paper of 1948 Bondi and Gold argued that the new steady state theory was preferable from a methodological point of view, as it was simpler, more direct, and more predictive than the cosmological theories based on general relativity. The latter class of theories, they said, was "utterly unsatisfactory" since it covered a whole spectrum of theories that could only be confronted with the observed universe if supplied with more or less arbitrary assumptions and parameters: "In general relativity a very wide range of models is available and the comparisons [between theory and observation] merely attempt to find which of these models fits the facts best. The number of free parameters

[44] Popper (1959, p. 107).
[45] On this discovery and its consequences, see Kragh (1996, pp. 343-355).

is so much larger than the number of observational points that a fit certainly exists and not even all the parameters can be fixed."[46] Relativistic cosmology sorely lacked the deductive character of the steady state theory, which uniquely led to a number of predictions, such as the mean density of matter, the curvature of space, and the average age of galaxies. According to Bondi and Gold (but not Hoyle), the predictions were essentially based on the so-called "perfect cosmological principle," the postulate that there is neither a privileged place nor a privileged time in the universe.

Whether in the Bondi-Gold or the Hoyle version, the steady state theory was critically discussed by many philosophers and philosophically minded astronomers and physicists. To the first category belonged Adolf Grünbaum, Mario Bunge, Milton Munitz, Norwood Russell Hanson, and Rom Harré, and to the latter Dingle, Whitrow, William McCrea, and William Davidson. Much of the methodological discussion in the 1950s focused on the criteria on which to judge the scientific nature of the steady state theory, or of cosmology in general.[47] It was in this context that Popper's demarcation criterion came to play a role in the cosmological debate. The leading Popperian cosmologist in the period was undoubtedly Hermann Bondi, at the time professor of applied mathematics at King's College, London. While Bondi took Popper very seriously indeed, the more unphilosophical Hoyle did not.[48] His post-1960 modifications of the steady state theory, developed

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[46] Bondi and Gold (1948, p. 269 and p. 262). On the philosophical foundation of the steady state theory, see also Balashov (1994).

[47] For this discussion and relevant references to the literature, see Kragh (1996, pp. 224-256).

[48] To my knowledge, Hoyle only referred once to Popper, and then in connection with the anthropic principle and not the steady state cosmology. At a conference of 1989, he defended the anthropic principle "if our existence leads to a potentially falsifiable prediction in the sense of Popper." See Hoyle (1993, p. 85).

in collaboration with Narlikar, were most un-Popperian, which was a major reason why Bondi would have nothing to do with them.

Although Popper's philosophical views only became generally known with the publication in 1959 of *Logic of Scientific Discovery*, among philosophers and some scientists they had attracted attention at an earlier date. The "surprisingly successful" *Logik der Forschung* was favourably received and reviewed also in English journals, and as a consequence Popper was invited to England in 1935 to give lectures.[49] Bondi may have become acquainted with Popper's views through the German original. Himself an Austrian immigrant – he came to England in 1937 and became a British subject in 1946 – he may have read *Logik der Forschung* in the late 1940s.[50] At any rate, he had adopted Popperian standards of science and used them in the cosmological debate even before 1959. Thus, in the *British Journal for the Philosophy of Science* of February 1954 Bondi and Whitrow engaged in a public discussion about the scientific status of physical cosmology. In this memorable exchange of ideas, Bondi defended the optimistic view that cosmology had already become a science on par with other sciences, while Whitrow, stressing the unique domain of cosmology, argued that it was not truly scientific and probably never would be so. It would remain, he thought, a borderland subject between science and philosophy. Bondi, on the other hand, suggested that the hallmark of science was falsifiability of theories and

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[49] Popper (1974a, pp. 86-88). Although widely read, the book only sold in a few hundred copies. On the impact of the book and the reactions it occasioned, see Hacohen (2002).

[50] According to a letter from H. Bondi to H. Kragh, January 1995. In his obituary of Popper, Bondi (1994) wrote that "His seminal work in the philosophy of science … became available in English only in 1959, but its principal ideas were already well appreciated in the English-speaking countries by that time." At an earlier occasion, Popper's eightieth birthday, Bondi (1982) recalled that it was "thirty years ago, when I was first so deeply and lastingly influenced by Popper's analysis."

that on this criterion cosmology was indeed a science. "Every advocate of any [cosmological] theory will always be found to stress especially the supposedly excellent agreement between the forecasts of his theory and the sparse observational results," he admitted. And yet,

> The acceptance of the possibility of experimental and observational disproof of any theory is as universal and undisputed in cosmology as in any other science, and, though the possibility of logical disproof is not denied in cosmology, it is not denied in any other science either. By this test, the cardinal test of any science, modern cosmology must be regarded as a science. … I consider universal acceptance of the possibility of experimental disproof of any claim an absolute test of what constitutes a science.[51]

Although not mentioning Popper by name, Bondi was clearly defending a main methodological point in Popperian philosophy. Whitrow, who was also well acquainted with Popper's views, did not disagree, although he warned that falsifiability should not be considered a final and absolute criterion: "The important role of disproof in science, which has been so cogently argued by K. R. Popper, is intimately related to the self-correcting tendency of science and this, in my view, is another aspect of the pursuit of unanimity."[52] A few years later, in an article on philosophical problems of cosmology, Bondi repeated that the scientific status of a theory was given by its ability to produce predictions that could be empirically disproved. This, he argued,

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[51] Whitrow and Bondi (1954, p. 279 and p. 282). For the Bondi-Whitrow discussion, see also Kragh (1996, pp. 233-237).

[52] Whitrow and Popper (1954, p. 280).

established the scientific status of the perfect cosmological principle and hence of the steady state theory building on it.[53]

Both in the debate with Whitrow and at some later occasions, Bondi seems to have conceived Popper's methods as prescriptions to be implemented in concrete scientific work, which may not be what Popper had in mind. Whitrow put more emphasis on the asymmetry between proof and disproof being purely logical, whereas in scientific practice a conclusive disproof or falsification can never be produced. Indeed, this was clearly recognized by Popper.[54] In practice it was also recognized by Bondi and other steady state protagonists, who in concrete cases, such as when confronted with Ryle's data from radio astronomy, denied that falsifying evidence amounted to actual disproof of the theory. They did not always behave in accordance with what they perceived to be Popperian standards.[55]

Latest by 1960, Bondi had become a convinced and enthusiastic advocate of Popper's philosophy, such as is evident from a glowing review essay he wrote of *The Logic of Scientific Discovery* together with his colleague at King's College, the mathematical physicist Clive Kilmister. The two physicists were full of praise of Popper's "splendid book," which they found was so well written that it was "almost desirable bed-time reading." More importantly, its main appeal to scientists was that it "rings true." According to the two reviewers, "Popper speaks as a working scientist to the working scientist in a language that time and again comes straight out of one's

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[53] Bondi (1957). In a postscript of 1963 to the second edition published in 1966, Bondi referred to the cosmological significance of some recent developments in nuclear astrophysics. Expressing himself in Popperian language, he said that "the steady-state theory has effectively passed a severe test."
[54] Popper (1959, p. 50). See also Section 5.
[55] Kragh (1996, pp. 327-328).

heart."[56] Bondi and Kilmister did not miss the opportunity to point out the relevance of the Popperian falsifiability criterion to the contemporary situation in cosmology. According to Popper, "Once a hypothesis has been proposed and tested, and has proved its mettle, it may not be allowed to drop out without 'good reason'," which quotation Bondi and Kilmister used as an argument for the steady state theory.[57] A "good reason" might be the replacement of the hypothesis by another, better testable one, and no such alternative existed. Speaking of the steady state theory: "For here the correct argument has always been that the steady state model was the one that could be disproved most easily by observation. Therefore, it should take precedence over other less disprovable ones until it has been disproved. Twenty years before the introduction of this hypothesis, Popper [in 1934] formulated the conditions with the utmost accuracy."

At about the same time as Bondi studied *The Logic of Scientific Discovery*, he participated in two BBC broadcasts on modern cosmology together with William Bonnor, Raymond Lyttleton, and Whitrow. In the published version, he once again paid tribute to Popper as the preeminent philosopher of science, summarizing his view as follows:

> By far the most successful analysis of scientific method is due to Professor Karl Popper. … The purpose of a theory is to make forecasts that can be checked against observation and experiment. A scientific theory is one that it is in principle possible to disprove by empirical means. It is this supremacy of empirical disproof that distinguishes science from other human activities. We can never regard a theory as

[56] Bondi and Kilmister (1959-1960, p. 55).
[57] Ibid. p. 56. Popper (1959, p. 53). North (1965, pp. 294-295), discusses the meaning of Popper's quote and the use made of it by Bondi and Kilmister.

> proved, because all we can say is that, so far, there have been no experiments contradicting it. A scientific theory, to be useful, must be testable and vulnerable.[58]

From the mid-1960s, after the steady state theory had fallen in disfavour, Bondi increasingly turned to studies of gravitational radiation and other problems of general relativity. But he was not yet quite ready to abandon his preferred cosmological model, which he continued to defend for some time from a methodological point of view rather than as a viable scientific model of the universe. Without embracing the victorious big bang theory, he admitted that the battle was lost. The steady state model might be wrong, but it was nevertheless more scientific than the relativistic big bang theories: "The steady-state theory is far more testable than any other. According to Popper and other philosophers of science, this makes it clearly preferable to alternative theories. The only open question is whether it has indeed already been disproved."[59] Bondi's high appreciation of Popper's philosophy was strengthened by his work on steady state cosmology, but it did not depend on it. Even after having abandoned cosmology as a research field, he continued to praise Popper's system in the strongest possible words, such as: "There is no more to science than its method, and there is no more to its method than Popper has said."[60] On the occasion of Popper's 90th birthday,

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[58] Bondi (1960, p. 12). While Bonnor, a theoretical physicist at Queen Elizabeth College, London, opposed the steady state model, the Cambridge astronomer Lyttleton was in favour of it. At the time he collaborated with Bondi and Gold on a new "electrical" version of the steady state theory. On this cosmological hypothesis, vulnerable to experimental disproof and indeed quickly disproved, see Kragh (1997).

[59] Bondi (1966, p. 32).

[60] As reported by the philosopher Bryan Magee in his biography of Popper. See Magee (1973, p. 2), which gives no source for the quotation.

Bondi spoke in a similar laudatory language, stating that Popper's view of empirical disproof "has profoundly influenced me and many others."[61] Finally, in an obituary in *Nature*: "Although many scientists have little interest in the philosophy of science, … to me his thoughts came as a flash of brilliant light."[62]

Although Popperian criteria of science played a considerable role during the cosmological controversy, and were highlighted by the steady state proponents in particular, they were rarely an issue of dispute. By and large, criteria of a Popperian kind were accepted also by many cosmologists favouring an evolving universe governed by the laws of general relativity. One of them was George McVittie, a British-American astronomer strongly opposed to the steady state theory and other theories he suspected were based on a priori principles. He described the philosophical foundation of the Bondi-Gold theory as "Karl Popper's dictum that a scientific theory can never be proved to be true but, instead, that certain theories can be proved to be false by an appeal to observation." While he considered the dictum to be a "probably unimpeachable doctrine," he parodied Bondi's use of it. If one followed Bondi's vulgar version of Popper's philosophy, "we should be justified in inventing a theory of gravitation which would prove that the orbit of every planet was necessarily a circle. The theory would be most vulnerable to observation and could, indeed, be immediately shot down."[63]

Also Bonnor referred to Popperian standards in his defense of relativistic cosmology, maintaining that it was in fact empirically refutable, if admittedly not as easily or crucially as the steady state alternative. "Indeed,

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[61] Bondi (1992).
[62] Bondi (1994).
[63] McVittie (1961, p. 1231). On McVittie and his empiricist attitude to cosmology, see Sánchez-Ron (2005).

if the steady-state theory is right, then relativistic cosmology is wrong, and, in my opinion, we have to scrap general relativity as well," he wrote. But he did not see it as a question bearing on Popper's criterion: "I make this point because every scientific theory must be capable of *disproof*; otherwise it says nothing."[64]

5 Popper's late views of cosmological models

As mentioned in Section 3, Popper was to some extent sympathetic to the steady state theory, of which he was well informed. Concerning his attitude to this theory, in his letter to me of 1994 he wrote as follows:[65]

> I discussed the controversial position with Bondi in considerable detail, and so I was quite well informed; but I did not read those papers of his. Incidentally, I liked his theory, but not the so-called "cosmological principle" and even less its (temporal) extension. (Because I dislike making of our *lack* of knowledge a principle of *knowing something*.) I therefore was *not* shocked when Ryle "killed" the theory, for he only killed this temporal extension of a "principle" that had nothing to do with the *basic idea* of the Bondi-Gold theory: that matter might be created out of some field and so keep the universe going – more or less "steady". For since mass-creation was a random process anyway, there was no need to assume that it was "steady" *over* comparatively "small" parts of the universe (even if we assume that it was "steady" over "large" parts – whatever "large" may mean): the propensity of creating

[64] Bonnor (1964, p. 158).

[65] See note 23. Popper divided his letter in five parts, denoted (1) to (5). I have not included this numbering in the transcription. The letter is now deposited at the Karl Popper Collection at the University Library in Klagenfurt, Austria.

a proton (or electron, or some other particle) may depend on (the gravitational? Or the electromagnetic?) fields linked with the nearest galaxies explaining galactic (?) evolution and offering an answer to Ryle's arguments.

While Popper thus was sympathetic to a version of steady state theory, if not to the original Bondi-Gold theory based on the perfect cosmological principle, he came to dislike and reject the victorious big bang theory. About this he wrote:

> I not only liked the idea of a *more or less* "steady" mass creation *much better* than the big bang – for obvious reasons: I mean the inexplicability of a beginning of time. But also because big bang theory became rapidly more and more complicated. And my present view is that the number of auxiliary hypotheses is simply intolerable: according to my theory of science, *this is not science*. It is (1) introducing a new auxiliary hypothesis every time the theory is refuted; and (2), it is *mutual support* of cosmological *theory* and particle *theory* – but criticism, and critical experiments (= attempted refutations) are ignored out of hand. And not only is it *not* stressed by the upholders of the theory that it is all speculation without tests, but it is presented as if the theory were a proven *fact*. This is horrid; impermissible; against scientific ethics.
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> By contrast, Einstein's General Relativity is a marvelous theory. But it may be superseded, as all theories may; and I actually suspect that it may be superseded already. But these are big problems, and I cannot now go i[n]to discussing them.

Popper summed up his attitude to cosmology as follows:

> I once *was* an enthusiastic admirer of (Friedmann's) Big Bang. I am *now* a disgusted opponent. As to the "steady state" theory, it is insufficiently developed, and Ryle's criticism insufficiently discussed. And the "cosmological principles" were, I fear, dogmas that should not have been proposed.

It is noteworthy that Popper's main objection to the classical steady state theory was the perfect cosmological principle, which he considered unscientific and dogmatic. He wanted to replace it with matter creation. Bondi and Gold, on the other hand, stressed the scientific nature of the falsifiable perfect cosmological principle and regarded it as primary relative to the continual creation of matter. According to them, matter creation was a direct consequence of the perfect cosmological principle when combined with the observationally established expansion of the universe. Without this principle, cosmology could not be truly scientific.[66] It is also worth to observe that much of Popper's criticism of the big bang theory agreed with the objections raised by Hoyle and others at about the same time. This criticism led to the so-called quasi steady state cosmology (QSSC), which Popper, had he known about it, might have found a more satisfactory theory of the universe.[67]

6 The influence of Popperian philosophy

Popper's falsificationist philosophy has been influential in a broad range of sciences, from botany to theoretical cosmology. According to David Stove, a

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[66] "If it does not hold, … cosmology is no longer a science." Bondi and Gold 1948, p. 255.

[67] A full description of the QSSC alternative is given in Hoyle, Burbidge, and Narlikar 2000. See also Kragh (2011, pp. 133-137).

critic of Popper, "if you scratch a scientist of middle age or older, you are almost certain to meet with a philosophy of science which consists of half-remembered scraps of Popperism."[68] The influence of falsificationist philosophy *à la* Popper varies of course from one science to another, and it may be particularly strong in the astronomical sciences. According to a study by Benjamin Sovacool, astronomers and cosmologists often invoke Popper's ideas as a guide for constructing and evaluating theories, although they rarely reveal a deeper familiarity with these ideas.[69] In addition to what has already been mentioned, a few more examples will indicate the enduring influence of Popperian methodology at least on the rhetorical level. In a review article on the comparative merits of standard big bang cosmology and QSSC theory, Narlikar and his coauthor T. Padmanabhan emphasize the crucial role of tests in distinguishing between cosmological models:

> What test can be performed that could in principle disprove this [standard or QSSC] cosmology? This question is in the spirit of Karl Popper's view of a scientific theory, that it should be disprovable. Thus if such a test is performed and its results disagree with the prediction of the theory, the theory is considered disproved. If the theory seeks survival by adding an extra parametric dimension, that is against the spirit of this question. On the other hand, if the prediction is borne out, our confidence in the theory may be enhanced, but the theory still cannot be considered proven.[70]

By these standards Narlikar and Padmanabhan consider standard relativistic cosmology to be a scientific theory because there are tests that are "decisive

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[68] Stove (1999, p. 8).
[69] Sovacool (2005).
[70] Narlikar and Padmanabhan (2001, p. 241).

in disproving or strongly discrediting" the theory. As another example, in a review of the state of cosmology at the millennium, the American astrophysicist Michael Turner advocated the new standard model of the universe, including the cosmological constant and dark matter, by appealing to its methodological virtues: "However, with its unidentified dark matter and mysterious dark energy, it is currently very much out on a limb. According to Karl Popper that's what strong theories do! … Inflation + cold dark matter is bold and testable."[71]

Stephen Hawking, the celebrated cosmologist and theoretical physicist, rates highly the power of mathematical elegance and logical consistency in theory construction. While these are the crucial sources in the context of discovery, in the context of justification the theory needs to be confronted with nature. In his best-selling *A Brief History of Time*, Hawking wrote:

> Any physical theory is always provisional, in the sense that it is only a hypothesis: you can never prove it. ... On the other hand, you can disprove a theory by finding even a single observation that disagrees with the predictions of the theory. As philosopher of science Karl Popper has emphasized, a good theory is characterized by the fact that it makes a number of predictions that could in principle be disproved or falsified by observation. Each time new experiments are observed to agree with the predictions the theory survives, and our confidence in it is increased; but if ever a new observation is found to disagree, we have to abandon or modify the theory.[72]

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[71] Turner (2001, p. 656).

[72] Hawking (1989, p. 11).

Hawking realized that this is a normative prescription rather than a description of real research practices, and that consequently it has to be qualified: "Or rather, that is what is supposed to happen. In practice, people are very reluctant to give up a theory in which they have invested a lot of time and effort. They usually start by questioning the accuracy of the observations. If that fails, they try to modify the theory in an ad hoc manner."[73] While the first of Hawking's quotations is a paraphrase of Popper's philosophy, the second one may appear to deviate from Popperian standards.

But it does not, for Popper did not claim that these standards reflected the actual practices of scientists. First of all, he never held that falsifiability is a sufficient condition for a theory being scientific, but only that it is a necessary condition. He knew well that the demarcation criterion cannot be very sharp, but must be assigned degrees, a topic to which he devoted a whole chapter in *The Logic of Scientific Discovery*.[74] Although somewhat ambiguous with regard to the relationship between his methodological rules and scientific practice, he admitted that strict falsifiability does not belong to the real world of science:

> In point of fact, no conclusive disproof of a theory can ever be produced; for it is always possible to say that the experimental results are not reliable, or that the discrepancies which are asserted to exist between the experimental results and the theory are only apparent and that they will disappear with the advance of our understanding. … If you insist on strict proof (or strict disproof) in

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[73] Hawking (1994, p. 36). A sentence to the same effect followed the previous quotation: "At least, that is what is supposed to happen, but you can always question the competence of the person who carried out the observation."
[74] Popper (1959, pp. 112-145).

> the empirical sciences, you will never benefit from experience, and never learn from it how wrong you are.[75]

Again, in a section in the 1974 festschrift on "Difficulties of the Demarcation Proposal" Popper made it clear that he did not assign any absolute value to the criterion of falsifiability and did not consider it a *definition* of science. He recognized that the distinction between metaphysics and science is often blurred. "What was a metaphysical idea yesterday can become a testable theory tomorrow," such as happened with speculative atomism at the time of Dalton.[76] Far from elevating falsificationism to an inviolable principle, he suggested that it is itself fallible and that it may be rational to keep even an admittedly wrong theory alive for some time:

> There is a legitimate place for dogmatism, though a very limited place. He who gives up his theory too easily in the face of apparent refutations will never discover the possibilities inherent in his theory. *There is room in science for debate*: for attack and therefore also for defence. Only if we try to defend them can we learn all the different possibilities inherent in our theories. As always, science is conjecture. You have to conjecture when to stop defending a favourite theory, and when to try a new one.[77]

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[75] Ibid., p. 50. In a note appended to the English edition, Popper remarked that "I have been constantly misinterpreted as upholding a criterion (and moreover one of *meaning* rather than of *demarcation*) based upon a doctrine of 'complete' or 'conclusive' falsifiability." On the ambiguity between the prescriptive and descriptive in Popper's writings, see Mulkay and Gilbert (1981).

[76] Popper (1974b, p. 981). In Popper (1959, p. 38), he referred to atomism as an example illustrating that "scientific discovery is impossible without faith in ideas which are of a purely speculative kind."

[77] Popper (1974b, p. 984).

The same theme appeared in Popper's autobiography, where he, referring to his early studies, said: "I also realized that we must not exclude all immunizations, not even all which introduce *ad hoc* auxiliary hypotheses. … All this shows not only that some degree of dogmatism is fruitful, even in science, but also that logically speaking falsifiability, or testability, cannot be regarded as a very sharp criterion."[78] The view indicated by these quotes is indeed far from the strict or naïve falsificationism often discussed by scientists either for or against Popper.

The allegiance to Popperian standards among cosmologists may be further illustrated by a new cyclic model of the infinite universe that was proposed in 2002 by Paul Steinhardt and Neil Turok as an alternative to the popular inflationary theory. In their efforts to promote the cyclic cosmology Steinhardt and Turok emphasized its falsifiability in the form of predictions of details in the cosmic microwave background. These details differ from those predicted by the rival inflationary theory and are of such a kind that, if they are not found, "then this would support the inflationary picture and definitively rule out the cyclic model."[79] An example of a different kind is the Israeli physicist Benjamin Gal-Or's *Cosmology, Physics, and Philosophy*, a textbook that ambitiously aims at integrating the three subjects. Although heavily influenced by Popper's philosophy, which it quotes extensively, in

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[78] Popper (1974a, p. 32). The reference to "immunizations" is to attempts to protect a theory against refutation.
[79] Steinhardt (2004, p. 469). While inflation predicts a spectrum of primordial gravitational waves, according to the cyclic model there should be no gravitational signature. For a survey of the new cyclic model, see Kragh (2011, pp. 202-208).

this case it is not the demarcation criterion which is in focus but rather Popper's critical rationalism and general attitude to science and philosophy.[80]

It is not difficult to find more examples of the influence of Popperianism on the views of modern cosmologists than those already mentioned, and a few more will be cited in the following section. Yet it is clear from the literature, both in cosmology and in other sciences, that Popper and the methodological rules associated with his name mostly play a role in general discussions and only very rarely in research papers. This was the case during the earlier cosmological controversy, and it is still the case. When cosmologists refer to Popperian criteria of science, it is typically in popular books, in review articles of a broad scope, in public lectures, or sometimes in conference proceedings. A series of interviews with biochemists conducted by Michael Mulkay and Nigel Gilbert indicated that although Popper's philosophical message was well known among them, and many of the scientists subscribed to it, his philosophy of science had only very limited influence on their actual scientific practice.[81] The cosmological research literature provides evidence that the situation in cosmology is about the same.

Few of the scientists commenting on the merits or faults of Popper's philosophy of science have actually read him, but rely on what they have been told or happen to know. This results in discussions that are often simplistic and sometimes based on misunderstandings. What cosmologists (and other scientists) discuss is most often naïve falsificationism rather than

[80] Gal-Or (1981), with a foreword by Popper. Using "the criterion of scientific testability, refutability and falsifiability," Gal-Or concludes that the Planck length, as given by $\sqrt{Gh/c^3} \cong 10^{-33}$ cm, is a "myth" (p. 242).

[81] Mulkay and Gilbert (1981).

the sophisticated versions of authentic Popperianism.[82] In a lecture in Cracow in 1973, the anthropic cosmological principle was introduced by 31-year old astrophysicist Brandon Carter. He soon realized that the principle disagreed with generally accepted standards of physics, such as Popper's emphasis on predictivity and falsifiability. Carter was at the time aware of Popper's ideas, but without having studied them.[83] In a paper of 1983 he criticized "the doctrine that scientific theories are never verifiable but only falsifiable," arguing that the Popperian doctrine implied that "all existing theories are not only falsifiable, but may safely be assumed in advance to be false."[84]

Six years later Carter amplified his attack on "the widespread misunderstanding that has led to undiscriminating insistence on the requirement that a theory should satisfy the requirement of 'refutability'." If a consequence of a theory is confirmed, he argued, it is turned from a hypothesis into a fact, and then it ceases to be refutable; and yet the strength of the theory increases. However, this misrepresents the Popperian view, for according to Popper confirmation merely corroborates a theory, it does not change its degree of falsifiability. In any case, Carter believed that, "far from being indispensable, such 'refutability' is definitely less satisfactory (scientifically) than an equal amount of (irrefutable) 'verified postdictive output' provided the latter is deduced logically from the same amount of independently hypothesised input information."[85] While Carter and a few

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[82] Heller (2009, pp. 88-89).

[83] In an e-mail to the author of 6 February 2010, Carter wrote: "Simplified folklore versions of Popper's falsifiability criterion (and Ockham's razor criterion) were already familiar to me – from coffee table discussions – when I was a student, but I did not get to know about Kuhn's very apposite ideas until much later." Reproduced with the permission of B. Carter.

[84] Carter (1983, p. 352).

[85] Carter (1989, p. 194). For Carter's ideas and the historical context of the controversial anthropic principle, see Kragh (2011, pp. 217-254).

other proponents of the anthropic principle objected to the norms of falsificationism (or what they took to be the norms), antagonists considered the conflict between the norms and anthropic reasoning as an indication that the latter did not belong to science. One of the antagonists, the American physicist Heinz Pagels, claimed in 1985 that the anthropic principle was untestable and "immune to experimental falsification – a sure sign that it is not a scientific principle."[86]

7 Multiverse and Popperianism

Together with string theory and the inflationary scenario, the anthropic hypothesis is a key element in the modern hypothesis of the "multiverse" that for a decade or so has been hotly debated in the cosmology community. According to this hypothesis or proposal, our universe is only one instance among a huge ensemble of universes and perhaps the only one privileged by the existence of intelligent and cognizable beings. The numerous other universes are causally disconnected from ours, hence unobservable in principle.[87] They may have their own laws of physics, own content of elementary particles, and perhaps even their own number of space-time dimensions. While some physicists and cosmologists are enthusiastic advocates of the multiverse, others tend to dismiss it as an interesting but unscientific speculation.

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[86] Pagels (1990, p. 177). Neither Pagels in 1985 nor Carter in his publications of 1983 and 1989 mentioned Popper by name.

[87] There are different kinds of multiverse models, and in some of them the constants of nature and basic laws of physics are the same in all universes. On the multiverse idea and the controversy it has aroused, see Carr (2007) and Kragh (2011, pp. 255-290).

One problem with the multiverse hypothesis is that the excessive amount of universes seems to allow almost any physical state of affairs – if not in our universe, then in some other. This, together with the unobservability of the other universes, makes the multiverse problematic, not to say unacceptable, from a Popperian point of view. According to Popper's philosophy, a scientific theory must be falsifiable and therefore set constraints to the results of possible observations: "Every 'good' scientific theory is a prohibition: it forbids certain things to happen," as he said in a lecture of 1953. "The more a theory forbids, the better it is."[88] At least in some versions, multiverse cosmology suffers from an extreme lack of prohibitiveness.

In the ongoing controversy over the multiverse Popper's views play a significant role, sometimes explicitly and at others times implicitly. Do multiverse theories qualify as scientific according to Popperian standards? Consider Lee Smolin, a leading theoretical cosmologist and critic of the multiverse and anthropic reasoning in physics:

> According to Popper, a theory is falsifiable if one can derive from it unambiguous predictions for practical experiments, such that – were contrary results seen – at least one premise of the theory would have been proven not true. … Confirmation of a prediction of a theory does not show that the theory is true, but falsification of a prediction can show it is false.[89]

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88 Popper (1963, p. 36).

89 Smolin (2007, pp. 323-324). Emphasis added. Smolin's Popperian view of science is not unlike the one earlier espoused by Bondi. See also Smolin (2008, p. 369), where he proudly declares himself a "Popperazo."

Smolin even goes as far as arguing that "scientists have an *ethical imperative* to consider only falsifiable theories as possible explanations of natural phenomena." Also Burton Richter, a Nobel laureate in physics, appeals to "Karl Popper's definition of science" in his attack on the anthropic multiverse. This supposed definition – that science is limited to "models with testable and falsifiable consequences" – rules out multiverse cosmology and relegates it to the domain of metaphysics.[90]

While Smolin and some other critics dismiss the multiverse on the ground that it violates Popper's demarcation criterion for science, it speaks to the authority of this criterion that it is invoked also by some cosmologists favourably inclined to the multiverse hypothesis. For example, Mario Livio (who is one such cosmologist) emphasizes that a theory that cannot be tested even in principle can hardly be counted as scientific. It "goes against the principles of the scientific method, and in particular it violates the basic concept that every scientific theory should be falsifiable," he says.[91] Another sympathizer, the French cosmologist Aurélien Barrau, maintains that "the multiverse remains within the realm of Popperian science," although he adds that "falsifiability is just one criterion among many possible ones."[92] Again, Max Tegmark, proposing a multiverse theory of the TOE (theory of everything) kind, discusses its relation to Popper's "hardly controversial" falsifiability criterion. Rather than questioning the criterion, he argues that

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[90] Richter (2006). The misunderstanding that Popper's demarcation criterion is a *definition* of science is widespread among scientists and science writers. Thus, an article on multiverse cosmology carrying the subtitle "Do we need to change the definition of science?" speaks of falsifiability as the "defining characteristic of real science," whereas Popper considered it a proposal scientists can decide to follow (Matthews 2008).

[91] Livio (2000, p. 187).

[92] Barrau (2007).

"the TOE we have proposed makes a large number of statistical predictions, and therefore *can* eventually be ruled out."[93]

Other multiverse physicists admit that the multiverse is hardly reconcilable with Popperian standards of science, but they tend to consider the tension a virtue rather than a problem. Leonard Susskind, one of the fathers of the modern multiverse hypothesis and an ardent advocate of it, has no respect at all for Popper and the "Popperazi" who follow him. Here is Susskind's opinion of the falsifiability criterion and what he calls "overzealous Popperism":

> Throughout my long experience as a scientist I have heard unfalsifiability hurled at so many important ideas that I am inclined to think that no idea can have great merit unless it has drawn this criticism. … Good scientific methodology is not an abstract set of rules dictated by philosophers. ... Falsification, in my opinion, is a red herring, but confirmation is another story.[94]

What Susskind refers to as confirmation includes mathematical consistency, which he and other string physicists rate highly as a method of testing. In this context it should be pointed out that Popper (probably unknown to Susskind) also included internal consistency as a way of testing a theory. In *Logic of Scientific Discovery* he distinguished between four different lines along which a theory can be tested, and only one of them was empirical

[93] Tegmark (1998, p. 1 and p. 42). This is one of the few research papers with specific references to Popper's demarcation criterion.
[94] Susskind (2006, pp. 193-195).

testing or falsification. He considered self-consistency to be a criterion formally analogous to falsification.[95]

More could be said about the role of Popperian standards in the modern debate of whether the universe is unique or a multiverse consisting of different universes. I have merely intended to illustrate that the legacy of Popper is still very much alive in twenty-first century cosmology.

8 Conclusion

Whereas Popper dealt extensively and critically with problems in statistical physics and quantum mechanics, he had disappointingly little to say about the new kind of physical cosmology that emerged and matured during his own career as a philosopher.[96] He followed the development from the sideline, but without examining any of the rival cosmologies from the perspective of his philosophy of science. In regard of the role that his views played in the steady state controversy, and the appeal to Popperian standards that Bondi highlighted in particular, this is somewhat surprising. From Popper's scattered remarks, supplemented with his testimony of 1994, it seems safe to conclude that he was sceptical with regard to the scientific status of cosmology. He had a certain preference for the falsifiable steady state theory, whereas he came to see the successful big bang theory as essentially ad hoc and unscientific. At the end of his life he expressed in

[95] Popper (1959, pp. 32, 92, and 314). Although distinguishing sharply between falsification and confirmation, Popper did not necessarily give priority to empirical falsification.

[96] The absence in Popper's books and articles of discussions of modern cosmology is only surprising in the light of the role that his ideas played among cosmologists during his own lifetime. Most other prominent philosophers of science – including Thomas Kuhn, Imre Lakatos, Paul Feyerabend, and Larry Laudan – also ignored the new developments in physical cosmology.

strong words his dislike of the big bang theory, especially for methodological reasons (Section 5). He characterized the methods on which the theory built as "intolerable," "impermissible," and "against scientific ethics."

From about 1955 and until the present Popper's ideas have been highly visible in the development of cosmology – much more so than the ideas of other philosophers, including Thomas Kuhn. It appears that the significance of his views has been largely limited to periods of conceptual uncertainty or rivalry between competing models. References to Popper, sometimes explicit and at other times implicit, appear frequently in the popular literature and general discussions, but only very rarely in the research papers. Another feature worth noticing, and one which is not peculiar to cosmology, is that the version of Popperianism that is discussed in scientific and semi-scientific contexts is almost always a simplified folklore version. This was the case in the 1950s, and it is still the case. Popper's views of what characterizes science and how it progresses are often distorted and frequently boiled down to the seductively simple formula that a non-falsifiable theory is unscientific per definition. This is a formula that has little to do with authentic Popperian philosophy.

How unique is the case here described? The views of philosophers have played a role in many other sciences in the modern period, but they have usually been detached from the actual research practices. Most often they have been interpretations of what has happened, retrospective evaluations of recent developments. In some fields of the biological sciences Popperian standards have been influential, and it has been discussed what these standards really are and if biological theories should live up to them.[97] Another case of possible relevance to the one here discussed is the plate

[97] Helfenbein and DeSalle (2005). Rieppel (2008).

tectonics "revolution" of the 1960s, where it was Kuhn's philosophy rather than Popper's that entered the discussion.[98] However, this mostly occurred *post factum*, in attempts to establish whether or not the development that led to the new picture of the mobile Earth followed a pattern of the kind argued by Kuhn. The case is nevertheless relevant because it shares with the one of cosmology the feature that philosophies of science were discussed by the scientists themselves and, to some extent, became part of the scientific discourse.[99]

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[98] Hallam (1973). Laudan (1980).

[99] The Canadian geophysicist John Tuzo Wilson, one of the pioneers of plate tectonics, was influenced by Kuhn's *Structure of Scientific Revolution*. As early as 1968 he referred specifically to Kuhn's "brilliant analysis of scientific methods," arguing that what had recently taken place in the earth sciences constituted a "Wegenerian revolution" in Kuhn's sense. See Wilson (1968, p. 317) and also Cohen (1985, pp. 564-565). It is not hard to see an analogy between Wilson and Bondi, the first advocating Kuhn in the case of the earth sciences and the latter advocating Popper in the case of cosmology and the physical sciences generally.

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