Physics and Philosophy — Issn: 1863-7388 — 2007 — Id: 008

Article

The Controversial Universe: A Historical Perspective on the Scientific Status of Cosmology

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PHYSICS & PHILOSOPHY

Abstract: The domain of cosmology is the universe, a singular concept, and basically for this reason cosmology is a science that differs from other sciences. For a long time there have been critical voices which argue that cosmology cannot be a proper science on par with, say, nuclear physics or hydrodynamics. This kind of critique goes a long way back in time, and I review it here in a historical perspective, focusing on the century from 1870 to 1970. I suggest that there are no good reasons to deny cosmology the status of a proper science. On the other hand, I also consider it natural, and a sign of health, that such foundational questions continue to be part of the cosmological discourse.

Keywords: Cosmology, history of modern cosmology, science, universe, Kant

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During the last fifty years or so, cosmology has developed greatly and is today recognized to be a mature and fundamental part of the physical sciences. With the Nobel prizes in physics awarded in 1978 and 2006 to work related to big-bang cosmology it may seem pointless to argue that cosmology is not a proper science. After all, it is hard to imagine that Nobel laureates in physics should earn the prize for doing work that doesn't even qualify as science. It is generally recognized, often with a considerable measure of self-congratulation, that "The discovery of the cosmic microwave background in the 1960s established the big bang theory and made cosmology into an empirical science," such as stated in an article in *Scientific American* from 1992 (Brush 1992, p. 62). Even more impressively, cosmology does not any longer rely on observations exclusively but can boast of having developed into an experimental science, at least as far as early-universe cosmology is concerned. As two leading particle cosmologists expressed it: "Cosmology has become a true science in the sense that ideas not only are developed but also are being tested in the laboratory.... This is a far cry from earlier eras in which cosmological theories proliferated and there was little way to confirm or refute any of them other than on their aesthetic appeal" (Schramm/Steigman 1992, p. 66). I am not suggesting that such evaluations are not reasonable or that cosmology is not a field of science; I would rather like to take a look at the problem from a deeper, and in this case historical, perspective. The basic question I want to discuss is this: Is it possible at all to establish a science *of* the universe, on par with other sciences such as physics, chemistry and biology? – or is it only possible to deal scientifically with what is *in* the universe? These are clearly big and complicated questions, and I have no ambition to discuss them fully or come up with answers to them. Being a historian of science, I shall adopt an empirical or historical approach, rather than an abstract philosophical approach; and I will do so by examining a few episodes in the history of cosmology in which foundational issues were discussed and arguments of a philosophical or extrascientific kind naturally entered the cosmological arena. It is my belief that the value of such an approach is not limited to the history of the past, but that it may also illuminate the current situation in cosmology and perhaps provide modern cosmologists with a broader perspective as to the nature of their science. After a brief view at Kant's remarkable but somewhat neglected role in the history of cosmological thought, I turn to aspects of how thermodynamics inspired philosophical (and theological) debates of cosmology in the late nineteenth century. I shall then proceed with an equally brief account of the philosophical and meta-physical discussion during the formative period of modern cosmology, meaning the 1950s. Following some rather scattered remarks concerning cosmology's scientific status, I end with a look at the present uneasiness about whether cosmology is truly a science. Needless to say, the present account is brief and sketchy. For details and further literature, I refer to two of my books on the history of cosmology (Kragh 1996 and Kragh 2006).

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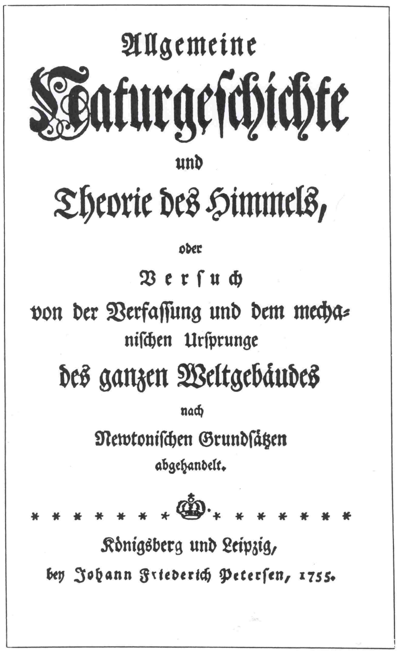
Cosmology was for a long time closely linked with philosophy, rather than astronomy and physics, and we should therefore not be surprised that one of the first and most acute critiques of cosmology as a science came from a philosopher. I am thinking of the famous philosopher of Königsberg, Immanuel Kant, who in one of his earliest works, the *Allgemeine Naturgeschichte und Theorie des Himmels* from 1755, established a grand and evolutionary cosmology purportedly on the basis of Newtonian mechanics (Kant 1981 and Falkenburg 2000). His qualitative theory or scenario was at first ignored, but in the nineteenth century – and especially after Helmholtz had called attention to it in a lecture of 1854 – it became widely adopted as a reasonable picture of how the universe had evolved. However, in the present context it is of more interest that Kant changed his view with regard to cosmology, and in his famous and highly influential *Kritik der reinen Vernunft* of 1781 concluded that the notions of age and extent are meaningless when applied to the universe as a whole. The concept of the universe, he argued,


Allgemeine
Naturgeschichte
und
Theorie des Himmels,
oder
Versuch
von der Verfassung und dem mecha-
nischen Ursprunge
des ganzen Weltgebäudes
nach
Newtonischen Grundsätzen
abgehandelt.

Königsberg und Leipzig,
bey Johann Friederich Petersen, 1755.

Figure 1: The original cover of Kant (1981).

is contradictory, and therefore it cannot be an object of possible experience; it is not something that exists objectively or cover a physical reality, but it is a regulative principle of merely heuristic value.

Kant's *Kritik* provided a penetrating analysis of foundational cosmological problems, but since it was purely philosophical it made little impact on physicists and astronomers engaged in studying the content and laws of the universe. Astronomers were anyway reluctant to take up cosmological research. The general view in the nineteenth century was that astronomy was strictly an empirical science assisted by mathematical calculations; its domain was restricted to what could be observed, a view which effectively implied an exclusion of cosmology in the proper sense of the word. In a letter of 1847, Carl Friedrich Gauss referred scornfully to cosmological hypotheses such as Kant's as *Phantasiespiele* or "plays of the imagination" that should be kept apart from astronomy proper (Peters 1860–65, vol. 5, p. 394). The attitude didn't change much over the subsequent decades. In an age of positivism, the general attitude was that theories must be confined by empirical data, which usually was taken to imply that astronomy was limited by the Milky Way system and that the possible existence of even more distant worlds was considered a question of metaphysics rather than physics. As the great American astronomer Simon Newcomb phrased it in 1907: "When there are no facts to be explained, no theory is required. As there are no observed facts as to what exists beyond the farthest stars, the mind of the astronomer is a complete blank on the subject. Popular imagination can fill up the blank as it pleases" (Kragh 2006, p. 111).

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This is not to say that cosmology was non-existent in the nineteenth century, only that it was not taken very seriously by the majority of physicists and astronomers. As a little known example of early physical cosmology I would like to draw attention to an interesting discussion about the physics of the universe that took place in the period from about 1870 to 1910 and which was the direct outcome of the global formulation of the laws of thermodynamics. As stated by Rudolf Clausius in 1865, the two laws were not only completely general, they also applied to the world or universe as a whole, that is, they were claimed to be valid in a cosmological sense. Clausius' canonical formulation of the laws was as follows: "1. The energy of the universe is constant. 2. The entropy of the universe tends toward a maximum." William Thomson in Great Britain came independently to the same conclusion, but preferred to speak of dissipation of energy rather than entropy.

From an early date it was argued that the second law of thermodynamics leads to the conclusion that in the course of time the universe will irreversibly approach a state of equilibrium, corresponding to a maximum entropy; when this state has been reached, it will be condemned to eternal rest. The message seemed to be that the world is in a state of decay or degeneration, and there is nothing that can prevent it from proceeding its suicidal course until its final consequence of a global *Götterdämmerung*. In addition to this dire prediction of a *Wärmetod*, a heat death, it was argued that the law of entropy increase also leads to the conclusion that the universe must be of finite age, have had a beginning in time and thus perhaps have been created. Because, so the argument ran, if the universe had existed in an eternity, and the second law had always been in operation, the entropy would by now be at its maximum. This is clearly not the case – after all, we exist (don't we?) – so the universe cannot have existed in an infinity of time. Q.e.d.!

These predictions are of dubious validity from a scientific and logical point of view, but they are nonetheless interesting because they were truly cosmological predictions based on an unrestricted extrapolation of the laws of thermodynamics. Moreover, they became highly controversial elements in the *Kulturkampf* of the late nineteenth century that raged between materialists and people of a Christian orientation. Issues such as the infinity of the universe and the possibility of counter-entropic processes were intensely discussed by scientists, philosophers, theologians and social critics, not least in the new Germany where Catholic scholars took them very seriously (Neswald 2006 and Kragh 2004).

What is of relevance in the present context is that the debate also provoked a critical discussion of the foundation of physical cosmology, and even of the possibility of speaking scientifically about the universe. For instance, the famous Austrian physicist and positivist philosopher Ernst Mach argued in the 1870s that it was illegitimate to apply the laws of physics to the entire universe, because the universe is a concept to which no meaningful statements can be attached. His point was that the universe does not have the status of an object, it is not a thing,

Figure 2: The "dying earth" (from Flammarion 1880, p. 5).

but the collection of all things. The whole, he maintained, cannot be treated in the same way as the parts of which it consists (Mach 1923, pp. 209 f.):

> "Energy and entropy are concepts of measure. What meaning can it have to apply these concepts to a case where they are *not even applicable*, in which their values are indeterminate? *If* the entropy of the world could be determined, it would be an absolute measure of time and it would be, at best, nothing but a tautology to say that the entropy of *the world* increases with *time*. Time, and the fact that certain changes take place in a definite sense, are one and the same thing."

Many other writers of the late nineteenth century agreed with Mach and asserted that the universe as a whole cannot possibly be the subject of the laws of physics. For example, the German-American philosopher John Stallo charged in 1882 that "all cosmogonies which purport to be theories of the universe as an absolute whole, in the light of physical and dynamical laws, are fundamentally absurd" (Stallo 1882, p. 276). Of course, this is precisely what modern cosmologists aim at, to provide theories of the universe as an absolute whole.

To mention but one more example, also Pierre Duhem in France denied that physics justifies long-term predictions of a cosmological kind. In an essay of 1905 he argued that, "physical theory can never demonstrate or contradict an assertion of cosmology, for the propositions constituting one of these doctrines can never bear on the same terms which the propositions forming the other do, and between two propositions not bearing on the same terms there can be neither agreement nor contradiction" (Duhem 1974, p. 301). The objections of Mach and Duhem with respect to the scientific nature of cosmology were in their spirit quite close to the criticism that Kant had raised in 1781. However, it is uncertain if their views were influenced in any direct way by Kant's *Kritik*. As far as Duhem is concerned, he mostly used the term "cosmology" in its scholastic or Thomistic sense, which includes many more aspects than those relating to the physics and astronomy of the universe.

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I shall now proceed to the situation in cosmology in the first decades after the relativistic revolution and the discovery of the expansion of the universe. In the 1930s and 1940s it was generally agreed that cosmology, whether based on general relativity or not, was in an unsatisfactory state because of the great gulf between the bewildering wealth of mathematical theories and the poverty of relevant observations. It seemed to some critics that cosmology was dominated by *a priori* principles and rationalistic thinking, and that cosmologists arrogantly erected ambitious and mathematically arcane theories without caring the least for observational support. The kind of cosmophysical theories proposed in the 1930s by Arthur Eddington, Edward A. Milne and Paul Dirac in England caused the astrophysicist and philosopher Herbert Dingle to launch a sharp attack against what he saw as a revival of speculative Aristotelianism, only dressed in a nearly incomprehensible mathematical language. "Instead of the induction of principles from phenomena we are given a pseudo-science of invertebrate cosmythology," he thundered (Dingle 1937, p. 385).

Dingle's criticism was particularly aimed against Milne's so-called kinematic cosmology and its foundation in the cosmological principle, which is the general claim that the universe is homogeneous and isotropic on a large scale. Although Dingle's criticism was rather extreme, he was not the only one who sensed the danger of rationalism in cosmology. Thus, the highly respected British-American astronomer George McVittie warned in 1940 that some cosmologists – he was thinking of Milne – "are not trying to understand Nature but rather are telling Nature what she ought to be. If Nature is recalcitrant and refuses to fall in with their pattern, so much the worse for her" (McVittie 1940. p. 280).

Foundational questions often come up in connection with major controversies, such as they did in the 1950s when relativistic evolution models of the universe were challenged by an entirely new kind of cosmology, the steady-state theory proposed by Fred Hoyle, Hermann Bondi and Thomas Gold. Among the interesting features of this famous episode in the history of cosmology is that philosophically oriented objections and arguments entered prominently in the controversy. While some of the arguments were specifically aimed at either the steady-state theory or the rival class of big-bang theories, others were of a more general kind and questioned the scientific legitimacy of cosmology *per se*, irrespective of the particular form or model. It was often assumed that the choice between the rival cosmological models could not be solved by ordinary scientific methods, but that there was wide room for appeal to aesthetical and philosophical preferences. According to the physicist Martin Johnson, cosmology had "more in common with the poetic and artistic attitude towards experience than with the solely logical" (Johnson 1951, p. 420). If the choice necessarily involved such extra-scientific considerations and was inevitably influenced by ”personal taste,” it might seem doubtful if cosmology was a proper science. After all, *de gustibus non est disputandum*.

I can only deal with a very few aspects of this interesting and illuminating debate, and want first to mention the exchange of views that took place between Hermann Bondi and Gerald Whitrow in 1954, in the pages of *The British Journal for the Philosophy of Science* (Whitrow/Bondi 1954). The subject under discussion was whether modern cosmology is a science or not. The very fact that this question could be seriously discussed by two leading cosmologists is a clear indication that cosmology at the time lacked professional maturity. According to Whitrow, contemporary cosmology was not truly scientific, and, moreover, it was unlikely that it would ever become a science in the ordinary sense. Because, so he argued, the subject of cosmology is an absolutely unique object, the universe, and this uniqueness makes cosmology a borderline subject between the special sciences and philosophy. Whitrow believed it would remain such a borderline semi-science. He added that cosmology's lack of scientific character was socially reflected in the lack of consensus among the cosmologists, for agreement about fundamental standards and methods is a hallmark of any mature science, and there evidently was no such agreement at the time. To use a Kuhnian terminology, one may say that cosmology was in a pre-paradigmatic phase, and Whitrow doubted that it would ever develop into a science with a shared paradigm.

Bondi, on the other hand, took a more optimistic view and argued that cosmology was quickly on its way to become a truly scientific branch of science and that the situation in cosmology did not differ fundamentally from that in other new sciences. Certainly, cosmologists disagreed radically about fundamental issues, but Bondi considered this to be natural for a young science that had not yet entered a mature phase, and he tended to see it as a sign of health rather than a weakness. Both Bondi and Gold mentioned philosophical and aesthetical considerations in cosmology and agreed that the importance of such considerations was a valid indicator of the lack of scientific character in the sense that whereas the role of philosophy was reduced to the heuristic level in mature sciences such as physics and chemistry, it was still of great importance in cosmology. But whereas Bondi believed that observation and physical theory had now largely replaced philosophy also in cosmology, Whitrow argued that philosophical arguments would always remain an essential part of the study of the universe.

One reason for Bondi's optimistic belief in the scientific level of cosmology was methodological, namely, that in spite of many disagreements there was, so he claimed, broad agreement with regard to what criteria should be used to settle controversies. Personal taste and metaphysical commitments such as preference for simplicity and logical beauty would always play a role, but a firm basis for demarcating science from non-science could only be found in the possibility of experimental disproof, such as highlighted by Karl Popper. Bondi, who was a faithful Popperian, claimed that there was universal agreement about falsification as a demarcation criterion, and so, ”by this test, the cardinal test of any science, modern cosmology must be regarded a science” (Whitrow/Bondi 1954, p. 279).

Discussions about a field's scientific status is not the usual area of scientists, but rather the business of philosophers, and in the present case there was no shortage of philosophers willing to enter the debate and tell the cosmologists whether the universe could be the object of scientific study. Is cosmology a science like physics and chemistry? What are the criteria of truth and how do they differ from those adopted by other sciences? Which of the competing theories is the more scientific, and on which grounds? – these were some of the questions eagerly discussed by both scientists and philosophers in connection with the cosmological controversy in the 1950s.

Many philosophers were critical in their judgment of cosmology, which they tended to deny scientific status. For example, the reputed Argentine physicist-philosopher Mario Bunge claimed that the steady-state theory was unscientific because it involved the notion of continual creation of matter *ex nihilo*, something he considered to be "characteristically theological or magical" (Bunge 1962, p. 117). The only justification for introducing continual creation was (so he claimed) to save the so-called *perfect cosmological principle*, that is, the large-scale uniformity of the universe in time as well as space. This principle, which formed the conceptual basis of the steady-state theory, Bunge saw as an unjustified dogma of an *a priori* nature. His rejection of the steady-state theory as plainly unscientific did not imply any sympathy for relativistic big-bang models, for he argued that any theory that operated with a finite age of the world was necessarily non-scientific because the beginning of the universe is beyond scientific explanation and can only be accounted for supernaturally. His rejection of both of the rival world models effectively meant that he denied cosmology the status of a science.

One of the subjects that (understandably) fascinated philosophers and philosophically minded scientists in the 1950s was the concept of the age of the universe, which was of course a central question in the cosmological controversy: according to the big-bang theory, the age is finite, while according to the steady-state theory it is infinite. It is symptomatic of the state and reputation of cosmology at the time, that when the *British Journal for the Philosophy of Science* announced a prize essay on this topic in 1953, the first prize was awarded to the American philosopher Michael Scriven, who in his essay denied the scientific validity of the question. According to Scriven, the notion of a beginning of the universe is unprovable in principle and therefore outside the power of science to determine. This was, more or less, the same conclusion that Kant had drawn back in 1781.

The reluctance of philosophers to accept cosmology as a genuine science may be further illustrated by the views of the two reputed philosophers Rom Harré and Stephen Toulmin. Harré concluded in 1962 that cosmogony, in the sense of the study of the origin and evolution of the universe, is not and cannot be a science because it relies on a first event that cannot be identified by any law of nature (Harré 1962). A few years later Toulmin/Goodfield (1968) concluded that "Cosmological theory is still basically philosophical" (p. 258) and that the field still faced the same objections that were raised by Kant nearly two hundred years earlier. Toulmin had earlier expressed his doubts about the scientific nature of cosmology by pointing out that when we say that a law is universally valid it does not mean that it is valid for the universe but for all subsystems of the universe (Toulmin/Goodfield 1968 and Toulmin 1982). By itself, the fact that a law is universal implies nothing about the universe as a whole. A statement which "holds universally" is one thing, a statement about "the universe" is another, and a step from one level to the other will always require justification.

It is interesting, given the history of the subject, that Toulmin applied his critique to the cosmological use of the second law of thermodynamics much in the same way as Mach and others had done nearly a century earlier. Toulmin shared Mach's belief that there are no laws of the universe, and that there is no intelligible sense in which one can apply the law of entropy increase to the universe as a whole. Even if it is established that the second law can be applied to all physical systems thermally isolated from the rest of the universe, it follows in no way that it applies also to the universe – contrary to what Clausius had stated back in the 1860s. He therefore concluded, as Mach and other late-nineteenth-century philosophers had done, that the notion of the heat death was scientifically meaningless. Or, as he phrased it: "The running-down universe is a myth, and we shall discover about the Apocalypse from physics only what we read into the subject. The pitfalls surrounding the notion of an Apocalypse surround equally the idea of a Beginning of All Things" (Toulmin 1982, p. 49).

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I will now like to consider from a more general perspective a few of the problems that have turned up in this brief and fragmented historical survey. One of the objections raised against cosmology as a science, both in the 1950s and at later occasions, is that it rests on certain presuppositions of a metaphysical or *a priori* nature, assumptions that cannot possibly be verified because they cover the universe in its entirety. I mentioned that some critics, such as Dingle and Bunge, objected to the cosmological principle either in its restricted spatial form or its wider spatio-temporal form known as the perfect cosmological principle. But it is really misleading to characterize these principles as *a priori* in the standard sense that their validity is necessary and independent of any experience.

The cosmological principles cannot be verified, but lack of verifiability is hardly a serious problem as long as they can be falsified. And they can indeed be falsified, such as shown most directly by the history of the steady-state theory which effectively led to the abandonment of the perfect cosmological principle. This principle is no longer defended by cosmologists, not even by the few who continue to develop cosmological models inspired by the steady-state theory (such as the quasi-steady-state model advocated by Jayant Narlikar). The reason why the steady-state theory was rejected, and hence that the perfect cosmological principle had to be given up, was basically that the theory was unable to account for the microwave background and other observations. Far from being an *a priori* assumption, the cosmological principle should be looked upon as merely an idealization or a simplifying assumption of the same type that we know from other areas of physics. The lack of verifiability is in no way problematic, as it is a feature the cosmological principle shares with the universal laws of physics with which the principle is closely connected: the cosmological principle is a hypothesis of global uniformity that implicitly presupposes the same kind of physics throughout the universe, and hence that the laws of physics are the same in any distant corner of the universe as we know them from terrestrial experiments.

The universe is in many ways a strange and frightening concept because it includes everything physical, and this may seem to distinguish cosmology from other sciences that are characterized by a limited domain. But the distinction should not be overrated, for in some sense it is not really a problem which is specific to cosmological research. Similar problems appear, if typically in a hidden form, in the ordinary laws of nature in so far these are claimed to be general and universally valid.

Consider, for example, the law of energy conservation or the law that states that all electrons have the same electrical charge. These laws are supposed to be valid throughout the universe, for *all* processes and for *all* electrons, even though there may be an infinity of them. A referent such as "all electrons" is unique and implicitly cosmological since it means "all electrons in the universe." Similarly, the law of energy conservation refers to all processes in the universe, anywhere and at any time. The implicit cosmological nature of much standard physics may be further exemplified by the existential statements that often appear in physics. When a particle physicist claims that a certain entity exists, and claims no more than that, it means that at least one instance of the predicted entity exists somewhere in the universe. Such a statement may evidently be verified, but it cannot be falsified (as Popper was well aware of).

From a realist point of view it is tempting to argue from the scientific success of modern cosmology that its domain – the universe – must exist independently of cosmological theory. On the other hand, the anti-realist will tend to conceive the universe as merely an idea or a mental reconstruction that only exists as a reflection of current cosmological theory. The universality of some laws of nature, mentioned above, is not by itself a convincing argument in favor of cosmological realism, cp. Toulmin's critical comments. Still, the universe is not just an idea. As Ernan McMullin has pointed out, ideas do not possess physical properties such as mass, volume and space curvature (McMullin 1981). Moreover, modern cosmology operates with measurable quantities that are not only in the universe but are truly cosmological in nature. The ubiquitous microwave background radiation is the best known example, while another example is furnished by the cosmological constant and the dark energy assumedly associated with it. These quantities do not relate to particular points of space-time, but are characteristics for the universe at large.

The physical sciences are *nomological* in the sense that phenomena are typically explained by subsuming them under covering laws. Such nomological or law-based explanations presuppose that the objects whose properties are to be explained are members of a class. For instance, if we want to know why Jupiter moves around the Sun in an elliptic orbit we may refer to Kepler's first law and the fact that Jupiter is a planet – or, on a deeper level we may wish to refer to Newton's law of gravitation or to Einstein's general theory of relativity. But in this respect the universe is different, since it is unique and therefore the only member of its class. It may seem to follow that whereas one can distinguish between law-bound and contingent properties in local physics, this is not possible in cosmology. Bondi, who considered cosmology to be necessarily phenomenological, expressed the difference as follows: "We have got to take the motion of the universe, and not its law of motion. It is boring to describe separately the motion of the apple and of the moon and so on. But if there is nothing but one apple falling, then you would be silly if you did anything but describe that motion" (Bondi 1967, p. 82).

However, even if we accept that nomological explanations have no place in cosmology, it does not mean that cosmology is bound to be descriptive rather than explanatory. After all, there are other types of explanation than the deductive-nomological one based on a covering law of the Hempel-Popper type. For example, consider the question, "why is space filled with a cold and uniform microwave radiation?" Such a question cannot be explained nomologically, but it can be explained genetically, in a way similar to which some facts of biology and geology are explained. A genetical explanation involves a scenario of how the phenomenon came into existence as a result of earlier phenomena. The favored answer to the question is "because the universe started in a big bang some 13 billion years ago," which is an explanation that of course involves laws of physics but not laws of the universe.

Altogether I find the epistemic pessimism that many philosophers and scientists expressed half a century ago to be exaggerated and misleading. Although one may question the scientific nature of certain cosmological theories, there is no good reason to conclude that cosmology as such is non-scientific or entirely different from other sciences with a more limited domain. The very history of modern cosmology seems to confirm this, for the way cosmological theories have been evaluated by means of theoretical arguments and observational testing is essentially the same that we find in other branches of the physical sciences.

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One might believe that the kind of problems I have alluded to, and presented in a historical context, are no longer of relevance after cosmology has become a mature and progressive science and the hot big-bang paradigm accepted by almost all cosmologists. But this is not quite the case, and fortunately so. There are still scientists and philosophers who doubt if cosmology is a proper science or, more commonly, object to the scientific status of particular cosmological theories, especially those that relate to the very early universe or other esoteric parts of cosmology. Examples may be models with varying constants of nature or, more typically, quantum cosmologies such as many-world models, pre-big-bang theories and models of the Hartle-Hawking type. The skeptics typically argue that it is nearly impossible to distinguish between mathematical models and theories of physical reality, and they complain that some cosmological theories are practically beyond empirical testing and therefore not truly scientific. I shall not enter this more recent discussion except that I find it relevant to point out the historical continuity, the similarity in spirit between the more recent criticism and the one that was aired in the period before big-bang standard cosmology.

To illustrate my point, let me end by quoting a couple of examples of fairly recent criticism, due to scientists rather than philosophers. Not so many years after the inflation scenario had become fashionable, Georges Ellis and Tony Rothman wrote an article entitled "Has Cosmology Become Metaphysical?" and in this article they stated (Rothman/Ellis 1987, p. 22):

> "A peculiar situation has arisen in cosmology. ... This [inflation] theory has no evidence to support it, and the one prediction it does make appears to be incorrect. It is too early to make a conclusive judgment on inflation, which is, without argument, aesthetically pleasing. But there also can be no argument that cosmology is approaching the frontier where science is no longer based on experimental evidence and makes no testable predictions. Once this border is crossed, we have left the world of physics behind and have entered the realm of metaphysics."

Other critics have objected that the inflation theory "has spawned a research program that has the potential to insulate itself almost completely from empirical falsification" (Rhook/Zangari 1994, p. 228).

The journal *General Relativity and Gravitation* carried in the year 2000 an article by M. J. Disney, a British astronomer, which repeated many of the accusations of earlier critics, not only in substance but also in rhetoric (Disney 2000 with reply in Ćirković 2002). Disney's basic concern was the gulf between observation and theory, and also the cosmologists' unrestrained willingness to extrapolate known physics over huge ranges in space and time. Cold dark matter, a notion accepted by the majority of cosmologists, sounded to Disney "like a religious liturgy which its adherents chant like a mantra in the mindless hope that it will spring into existence."

The comparison between cosmology and religion was far from new, and it was not accidental: "The most unhealthy aspect of cosmology is its unspoken parallel with religion. Both deal with big but probably unanswerable questions. The rapt audience, the media exposure, the big book-sale, tempt priests and rogues, as well as the gullible, like no other subject in science." Indeed, the claimed association between cosmology and religion is as old as, or even older than, the accusation that cosmology does not live up to the normal standards of science. And there are those who believe that the two problems go hand in hand, that a science cannot be truly scientific if it invites religious feelings or otherwise approaches domains that traditionally belong to religion and spiritual life. But this is, as so much else, another story.

Acknowledgments

A version of this paper was read as a plenary lecture at the spring 2007 meeting of the German Physical Society in Heidelberg. I am grateful to the organizers of the conference and to Brigitte Falkenburg for have invited me to deliver the lecture.

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