Some trends in the philosophy of physics*

Henrik Zinkernagel**

Abstract

A short review of some recent developments in the philosophy of physics is presented. I focus on themes which illustrate relations and points of common interest between philosophy of physics and three of its 'neighboring' fields: Physics, metaphysics and general philosophy of science. The main examples discussed in these three 'border areas' are (i) decoherence and the interpretation of quantum mechanics; (ii) time in physics and metaphysics; and (iii) methodological issues surrounding the multiverse idea in modern cosmology.

1 Introducing the philosophy of physics

Philosophy of physics is about the interpretation and critical examination of physical theories and concepts. The interpretation part is typically concerned with the question of what understanding of Nature is provided by our best physical theories, e.g. is space-time absolute or relational, is matter particle or field-like, or what is the role of probability in explanations of the fact that physical systems generally tend towards equilibrium? The critical examination part is a related, though more heterogeneous, activity which may be, for instance, about whether a given theory or hypothesis provides an adequate explanation for a specific phenomena or set of data; the consistency between claims made in different physical theories; the examination of the underlying (philosophical) assumptions in a theory, etc. Both historically and presently, such tasks are pursued by philosophers and physicists alike – and in this sense philosophy of physics may be said to be continuous with physics itself.

The philosophy of physics has traditionally been concerned with philosophical problems arising in, and in connection with, the main physics disciplines as developed in the (late nineteenth and) twentieth century: Space-time physics (mostly the theories of relativity), quantum physics and statistical physics.[1] Thus, a 'classic' textbook in the field, Sklar's *Philosophy of Physics* from 1992, deals precisely with philosophical issues related to these core fields of physics. Classifying philosophy of physics according to the main areas of physics continues to be a widely used strategy as can be seen from the recent *Handbook in the Philosophy of Physics* (Butterfield and Earman 2007) – an impressive, almost 1500 pages, and detailed compendium

*Published in *Theoria – An International Journal for Theory, History and Foundations of Science* (San Sebastián, Spain), Vol. 26/2, 2011, pp. 215-241.

**Department of Philosophy I, Granada University, 18071 Granada, Spain (*e-mail: zink@ugr.es*).

[1]Whereas the type of questions posed in the philosophy of physics are as old as physics and philosophy themselves, the institutionalized discipline is – just like general philosophy of science – predominantly a child of the twentieth century, and in particular of the relativistic and quantum revolutions in physics in the first part of that century.

of foundational and philosophical matters in most areas of the physics landscape. The same structure is used in *The Ashgate Companion to Contemporary Philosophy of Physics* (Rickles 2008) though the focus here is not so much on completeness in themes but rather on bringing the reader to the forefront of issues in contemporary philosophy of physics.

This manner of dividing the subject matter of philosophy of physics may be seen to reflect a preoccupation with the philosophy within specific physical theories – such as the nature of space and time in Einstein's theories or the notorious measurement problem in quantum mechanics – as opposed to more 'transversal' issues affecting a range or all of physics. Such issues could be fundamental concepts in physics like mass, fields and energy; Lange's (2002) book introduces the philosophy of physics by focusing on such concepts. Or they could be more overarching questions more closely related to traditional concerns in the philosophy of science (such as the realism debate). An example of this approach to the field is Kosso's (1998) book. Another way of approaching the philosophy of physics is through history. This strategy is reflected in the books by Cushing (1998), which emphasizes the relations between philosophy and physical theories from antiquity to the twentieth century, and Torretti (1999), which focus on the conceptual development of physics from Galileo onwards. The different approaches in the above mentioned books – which I think include every modern general textbook in philosophy of physics that is not focused on a specific theory or topic – testify the diversity of a thriving field.

What follows is a personal and interested view of some recent developments in philosophy of physics.[2] I will report on recent work which illustrates the relations between philosophy of physics and i) physics; ii) metaphysics; and iii) general philosophy of science (more methodological issues). An underlying theme will be that philosophy of physics has interesting points to offer to all, and cannot really be neglected by any, of these neighboring disciplines. Whereas this conclusion can probably be endorsed by most general philosophers of science, some metaphysicians will be skeptical, while many physicists are likely to disagree. So let me start in the reverse order and first try to explain why the physicist averse to philosophy ought to get on-board.

2 Physics and the philosophy of physics

A common belief among physicists is that philosophy starts where empirically based physics ends. Now, it is certainly true that when physics disciplines, such as cosmology or particle physics, are stretched to deal with phenomena outside observationally or experimentally accessible regions, the physics becomes more speculative and thus more dependent on *a priori* assumptions which are often of a philosophical nature. It is also true that, historically, philosophical beliefs have sometimes been replaced, or at least been challenged, by advances in science – think e.g. of Plato's insistence of the necessity of circular celestial motion or Kant's inclusion of Newtonian laws and Euclidean geometry among his synthetic a priori's. But it would be a mistake to believe that philosophy is only to be found at the borders of physics, and even

[2]Since philosophy of physics is a vast field, any state of the art survey is likely to be both selective and biased.

more mistaken to think that philosophical belief in general are to be substituted with well-established science when the time is ripe.

As will be illustrated in this section, philosophical considerations are necessarily part of the physics enterprise. A simple argument can be given for this point. No matter how elaborate the mathematical *formalism* of a physical theory is, it should be distinguished from the *interpretation* of that theory. Such an interpretation will involve a minimal part which is needed to hook up the formalism with the empirical evidence. A strictly instrumentalist view of physics will stop here but, of course, instrumentalism is itself a philosophical position susceptible to criticism. Moving beyond the minimal interpretation of a physical theory will involve questions about what the theory *mean* – in particular what the theory means for our understanding of the world. And, as stressed by many (e.g. Rickles 2008a, 8), the full interpretation of a theory is often underdetermined by its formalism, and so philosophical criteria are bound to be relevant for the interpretation chosen.[3]

The impact of philosophy is not limited to interpretative questions about what a physical theory is supposed to say about the world. Arguably, philosophical considerations, e.g. those contained in the famous discussions between Bohr and Einstein, may influence the development of the physical theories themselves.[4] Something along these lines may currently be happening with respect to physicist's attempts to construct a quantum theory of gravity. A recent development in this regard is that (at least some) physicists think that philosophers could be helpful in the quest (see e.g. the quotes by Rovelli and Baez in Rickles 2008b, 263). One reason that philosophers might here contribute to the actual course of science is that quantum gravity research is seriously engaged with what are (difficult technical but also) deep conceptual problems. Indeed, what is apparently needed is some sort of unification between a *quantum* theory of matter which is usually formulated in a *fixed* space-time background, and the general theory of relativity in which a *classical* description of matter is associated with a *dynamical* space-time.[5]

Though quantum gravity is currently a hot topic in the philosophy of physics (and physics proper), it is probably fair to say that the most discussed issue on the interface between physics and philosophy has been, and still is, the measurement problem and the associated attempts of providing a satisfactory interpretation of quantum mechanics.[6] For this reason I dedicate the rest of this section to a brief

[3]The problem of underdetermination can be raised at several levels. Not only may an interpretation be underdetermined by a theory (the interpretation is not unique) but also the theory (formalism) may be underdetermined by the data (the theory is not unique). In some cases, this non-uniqueness even arises at the formal level within what is considered the same theory as illustrated e.g. by the Hamiltonian vs. the Lagrangian formulation of classical mechanics, the Schrödinger vs. the Heisenberg formulation of quantum mechanics, or the geometrical vs. the field theoretical formulation of general relativity (for discussion of the latter, see e.g. Pitts 2011).

[4]Lange (2002, 200) makes the point thus (after discussing Einstein's criticism of the interpretation of classical electrodynamics, ultimately leading to a new mechanics): "The interpretation of physical theories is thus capable of leading to new scientific theories, making novel predictions regarding our observations".

[5]However, see Zinkernagel (2006) for a critical examination of the necessity for a quantum theory of gravity.

[6]Thus, in a recent survey article on the philosophy of physics, Saunders (2008) focuses exclusively on the measurement problem. Also, in the programmes of the last two editions (2007 and 2010) of the only regular joint meeting series for physicists and philosophers of physics – the "UK

introduction to the measurement problem and a recent development concerning this problem.

2.1 Quantum mechanics and the measurement problem

The (fairly) recent development concerning the measurement problem in quantum mechanics goes by the name of decoherence. Though to some extent originally motivated by conceptual issues (see Camilleri 2009), it is essentially a development within physics proper which has shed new light on the old debate surrounding the interpretation of quantum mechanics. To briefly describe this development, let me first sketch why quantum mechanics is so difficult to interpret.

Quantum mechanics is an immensely successful theory. Not only have all its predictions been experimentally confirmed to an unprecedented level of accuracy, allowing for a detailed understanding of the atomic and subatomic aspects of matter; the theory also lies at the heart of many of the technological advances shaping modern society – not least the transistor and therefore all of the electronic equipment which surrounds us. Yet for all its impressive predictive and practical successes, quantum mechanics cannot account for the most basic fact of experiments – namely that they have definite outcomes. To see this, consider a simplified description of this problem of definite outcomes, or the *measurement problem* as it has come to be called.[7]

Quantum systems, such as electrons, are described by so-called state vectors or wave functions. A basic idea in quantum theory is the superposition principle which holds that a linear combination of state vectors is also a state vector. That is, if a quantum system can be in two (or more) different states then it can also be in a superposition of these states. Thus, for instance, if we suppose that an electron can be in two position states (corresponding to two different positions, call them A and B), then it can also be in a superposition of these states (be in *both* places at the same time).

Since an electron is a microscopic entity such a superposition is not an immediate cause for concern for we never actually see electrons – let alone see them in different places simultaneously. Moreover, extensive empirical evidence shows that the electron in many circumstances is correctly described by such superpositions.[8] Trouble begins, however, when we assume that quantum theory is a fundamental and universal theory – applicable to each and every object (and interaction) in the universe – for then the measurement apparatus (and its interaction with the electron) should also be describable by this theory. Under this assumption, quantum theory (the Schrödinger equation) implies that microscopic superpositions are "amplified" to, and thus also show up at, the macroscopic level. This means that, say, a pointer on an electron-position measurement apparatus should be able to point in

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and European meeting on the foundations of physics" – the conceptual issues surrounding quantum mechanics are by far the most popular topics.

[7]For a recent detailed introduction to the measurement problem and other conceptual issues in quantum mechanics, see Dickson (2007). A shorter and more accessible introduction to the measurement problem, decoherence and the Everett interpretation (see below) can be found e.g. in Butterfield (2001).

[8]For instance, superpositions of the electron's position (of it's trajectories) are necessary to account for the characteristic interference pattern in the so-called double slit experiment.

two directions at the same time!

A simplified formalization of the electron-position example will be useful below. Consider a set-up in which the pointer on our measurement apparatus can point in only two directions, say, left ($\nwarrow$) and right ($\nearrow$), which correspond to measuring the electron in position A and B respectively. We assume that we start with the pointer on the measurement apparatus in the left ($\nwarrow$) position, and that the apparatus works according to the following rule: If the electron is found in position A then the pointer points (stays) left whereas if the electron is found in position B, then the pointer points right. Suppose now that we also start with the electron in the above mentioned superposition. The evolution of this composite system (electron + pointer) can then, according to the Schrödinger equation, be represented as follows:

$$(|A\rangle + |B\rangle)|\nwarrow\rangle \longrightarrow |A\rangle|\nwarrow\rangle + |B\rangle)|\nearrow\rangle$$

where $|\cdot\rangle$ represents a quantum state and is, in this case, mathematically shorthand for a two-dimensional column vector like $\left(\begin{smallmatrix}1\\0\end{smallmatrix}\right)$.[9]

The superposition on the right hand side is an expression of the measurement problem as it suggests that the macroscopic measurement apparatus (as well as the electron) really is in two distinct positions at once.[10] Of course, such superpositions of measurement apparatuses (and indeed any familiar macroscopic object) are clearly at odds with what we observe: familiar objects, such as a the tip of a pointer, are always found in only one place at a time. The measurement problem, then, is to explain how to get from the 'many positions' implied by the quantum formalism to the one position observed.

The measurement problem has been an important motivation for a number of different interpretations of quantum mechanics – none of which are widely believed to be problem-free. The first and most famous response to the measurement problem was von Neumann's 'projection postulate' from 1932 which consists in asserting that the superposition above "collapses" into one of the component states upon measurement. This idea, however, immediately raises difficult questions such as whether a supposed fundamental theory really can rely on an unanalyzed notion of measurement, and who is to judge when a measurement has been performed (e.g. would Schrödinger's cat be sufficient?).

More recent attempts to solve the measurement problem may roughly be classified in two categories.[11] Either one adds structure to quantum mechanics which is done, for instance, in Bohm's hidden variables theory or Ghiradi, Rimini and Weber's dynamical collapse theory.[12] Alternatively, the superposition above is taken at

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[9]Note again that this is in several ways a highly simplified description. Most obviously, there are of course more than two (indeed a continuum of) possible positions of objects. Also, normalization factors have been suppressed, and we ignore the problem of whether macroscopic quantities (e.g. pointer directions) can straightforwardly be represented as (pure) quantum states, see e.g. Dickson (2007, 298).

[10]Such a macroscopic superposition is also what underlies the famous Schrödinger's cat paradox. It should be noted that we have tacitly assumed the standard interpretative rule in quantum mechanics known as the eigenstate-eigenvalue link (see e.g. Dickson 2007, 285). For objects in superpositions such as the above, this rule implies that instead of saying that the object is in two places simultaneously it is more correct to say that the object has no definite position at all.

[11]See Butterfield (2001, 14-15) for a more fine-grained classification of proposed solutions to the measurement problem.

[12]The added structure in Bohm's theory is particle trajectories, and a stochastic dynamics for

face value but one somehow denies that what we see corresponds to how the world actually is, as in Everett's 'many worlds' interpretation of quantum mechanics. On one version of this idea, the world 'splits' whenever something is measured or observed. This means that all possible outcomes are realized – although in different worlds, so that an observer only perceives one of the outcomes (with copies of the observer in other worlds who perceive the other outcomes). Apart from these two categories, a third option – which amounts to an attempt to dissolve the problem from scratch – can be associated with Bohr's interpretation of quantum mechanics. The idea is that the apparatus should somehow be described by classical, not quantum, physics, and therefore the apparatus and its pointer should not be thought of as being in superpositions.[13] I shall come back to Bohr and Everett below.

2.1.1 Decoherence

As already noted, none of the approaches to quantum mechanics, and to the measurement problem, enjoys widespread consensus. But, from the early 1990s onwards, there has been a fairly broad agreement among physicists and philosophers that the idea of *decoherence* may have an important role to play concerning the measurement problem, the different interpretational stances and, more generally, the relation between the classical and the quantum (see e.g Bacciagaluppi 2008 and Schlosshauer 2007).

Schematically, the idea is as follows. A quantum system and, in particular, a quantum macroscopic measurement apparatus is not easily isolated from the surroundings. Thus, there will almost always be an environment, like air-molecules or light (photons), which should be taken into account. This implies that just as the electron and the measurement apparatus get "entangled" upon interaction, as represented by the superposition above, we get an additional entanglement between this joint system and the environment.

At first glance, this does not improve the situation as we now have a superposition involving the electron, the apparatus *and* the environment. But a crucial point in accounts of decoherence is that we have no control over – and no interest in – the environmental degrees of freedom (e.g. we are not measuring on the individual photons or air-molecules which have scattered off the joint system). Thus we may (again) restrict attention only to the electron + apparatus. At the formal level, this procedure leads to what is known as a reduced density matrix in which the interference terms (corresponding to the quantum 'strangeness' of the superposition) practically vanish – one talks about the *de*localization or disappearance into the environment of the *coherence* between the two components of the original superposition.[14] The density matrix is defined as $|\Psi\rangle\langle\Psi|$ where, in our case,

the quantum state in Ghiradi, Rimini and Weber's theory. A common open problem for both of these strategies is whether the theory with the added structure can be made compatible with the special theory of relativity. For a detailed discussion of conceptual issues and problems surrounding these (and other) approaches to quantum theory, see Wallace (2008).

[13] I say 'somehow' because it is subject to controversy exactly what Bohr's position was. What is agreed by most commentators is that Bohr did not endorse the collapse postulate and that his interpretation should not be understood as an expression of subjectivism or positivism, see e.g. Faye (2008).

[14] For this to happen, the environment must fulfil certain conditions (in quantum mechanical terms, its eigenstates must become approximately orthogonal) and models of decoherence demon-

$|\Psi\rangle = |A\rangle|\nwarrow\rangle + |B\rangle)|\nearrow\rangle$ is the superposition from above, and $\langle\Psi|$ is the corresponding row vector $(\langle A|\langle\nwarrow| + \langle B|\langle\nearrow|)$. The transition via decoherence from the density matrix to the reduced density matrix can be written as follows:

$$
\begin{aligned}
|\Psi\rangle\langle\Psi| \quad &= \quad |A\rangle|\nwarrow\rangle\langle A|\langle\nwarrow| + |B\rangle|\nearrow\rangle\langle B|\langle\nearrow| + |A\rangle|\nwarrow\rangle\langle B|\langle\nearrow| + |B\rangle|\nearrow\rangle\langle A|\langle\nwarrow| \\
&\overset{deco.}{\longrightarrow} \quad |A\rangle|\nwarrow\rangle\langle A|\langle\nwarrow| + |B\rangle|\nearrow\rangle\langle B|\langle\nearrow|
\end{aligned}
$$

The (in this example 2×2-) density matrix describes the probability distribution for the alternative outcomes of the experiment. In particular, when (after decoherence) the last two 'interference' terms can be neglected, the expression closely resembles – in fact formally equals – a classical probability distribution specifying the probabilities for getting either of the outcomes left (and electron in position A) or right (and electron in position B). The reduced density matrix (the classical probability distribution) is *mathematically* equivalent to the one which would be obtained from applying von Neumann's collapse postulate to an ensemble (a set) of systems in the original superposition (see e.g. Zurek 1991), and either one gives the right statistics – i.e. on many runs of the experiment the reduced density matrix correctly predicts, say, the expected number of electrons found in position A. In this way, the effect of decoherence looks similar to a collapse.

But the similarity is deceptive and it does not hold at the level of interpretation for, as many authors have emphasized, while a collapse gets rid of the interference terms, the absence (or approximate absence) of interference terms does not amount to a collapse.[15] Basically, the reason is that the total system (electron + apparatus + environment) is still described by a superposition, and this means, according to quantum mechanics, that one cannot attribute a definite state to any of the component systems (see e.g. Schlosshauer 2007, 333). In fact, the use of the reduced density matrix to reproduce the right experimental statistics presupposes – and thus cannot be taken to show – that a definite measurement outcome occurs on each run of the experiment (see also Zeh 2003, 36).

Though decoherence is thus not itself a solution to the measurement problem, it is, as mentioned above, widely believed to cast new light on the various interpretations of quantum mechanics. I shall here just briefly mention the possible roles of decoherence within two of the most well-known interpretations associated, respectively, with the names of Bohr and Everett. From the outset it seems clear that since decoherence is a (by now) well-established physical *phenomenon* – indeed, a consequence of quantum mechanics – there can be little question of letting decoherence decide between *interpretations* of quantum mechanics. Nevertheless, it has recently been claimed, for instance, that Bohr's interpretation is difficult to square with the insights of decoherence (e.g. Bacciagaluppi 2008 and Saunders 2008), whereas it makes more tractable a thorny issue, known as the preferred basis problem, in the Everett interpretation (e.g. Butterfield 2001 and Schlosshauer 2007).

strate that these conditions are met (and very rapidly so for interactions between the environment and macroscopic systems).

[15] In spite of occasional suggestions to the contrary, see e.g. Zurek (1991, 39).

2.1.2 Bohr

As noted above, Bohr insisted on a classical (not quantum) description of the measurement apparatus and this ensures that the pointer is always in a definite position.[16] Bohr's idea seems to be problematic as it suggests – and has often been taken to imply – that there is a separate classical realm of reality. This appears to clash with decoherence for, as Schlosshauer (2007, 336) remarks:

> Based on the progress already achieved by the decoherence program, it is reasonable to anticipate that decoherence embedded in some additional interpretive structure could lead to a complete and consistent derivation of the (appearance of the) classical world from quantum-mechanical principles. This would make the assumption of intrinsically classical apparatuses (which has to be treated outside of the realm of quantum mechanics), appear as neither a necessary nor a viable postulate.

For this to be taken as a criticism of Bohr, however, two assumptions must be made.[17] First, as we have seen above and as the quote hints, decoherence alone is insufficient as a solution of the measurement problem and hence for an explanation of the 'classical world', so one must assume that some other interpretation of quantum mechanics is (or can be made) satisfactory. Second, and more importantly, the criticism rests on the idea that Bohr conceived the classical/quantum distinction to be an ontological one. But this is a controversial matter since Bohr almost always stressed epistemological aspects of quantum mechanics and largely remained silent on ontological issues.[18] In fact, Bohr insisted that the border can be shifted so that what is considered the (classical) apparatus in one experimental context may be taken to be part of the quantum system in another different such context (see e.g. Bohr 1939, 104). A possible way to understand this might be that there is no context-independent answer to the question of what an object is (classical or quantum) – in analogy with the more standard idea in quantum mechanics that there is no context-independent answer to the question of whether an electron, say, is a particle or a wave.

Of course, even if Bohr's interpretation is compatible with the insights of decoherence, work still remains on accounting for what his interpretation precisely amounts to and whether it is ultimately satisfactory.

2.1.3 Everett

According to Everett, quantum mechanics alone (without any collapse postulate) should give a correct description of every physical system – including observers and

[16] Presumably, Bohr took the classical description of the apparatus to imply not only that it has a definite position but also, simultaneously, a definite momentum (and hence is not subject to Heisenberg's uncertainty relations). This reading of classicality has been disputed in Howard's influential reading of Bohr (Howard 1994), but see also Landsman (2007, 438).

[17] In personal communication, Schlosshauer points out that the above quote was not really intended as a criticism of Bohr (as Bohr may not have endorsed the idea of a separate classical realm of reality) and that, in any case, his view on the relation between Bohr and decoherence has since been refined, cf. Schlosshauer and Camilleri (2011).

[18] See Schlosshauer and Camilleri (2011) for a recent discussion on how this eases the tension between Bohr and decoherence.

measurement apparatuses.[19] While there are many different approaches to Everett's idea (see Barrett 2011), all of them share the core intuition that components of superpositions, such as the one we saw above, are all real in some sense. For instance, each one of these components may be taken to be correspond to experiences of observers in different worlds (many worlds interpretation); or an observer may be associated (simultaneously) with different mental states, each one having a definite experience corresponding to one of the components (many minds interpretation). In any case, on these interpretations, things really are not how they appear to be (e.g. in a definite position) and so, as even advocates admit, Everett's idea seems ontologically extravagant or fantastic (see e.g. Saunders 2008). However, given that none of the other interpretations of (or approaches to) quantum mechanics are widely believed to be satisfactory, this extravagance is generally not seen as a definitive drawback.

An important problem with the Everettian idea of taking quantum mechanics as a fundamental theory which, at least in principle, describes everything is the 'preferred basis problem': A quantum state may in general be written in many different bases – corresponding to different choices of basis vectors in the (abstract Hilbert) vector space in which all quantum states "live" – and the choice of basis is not given from the quantum formalism alone. Thus, in terms of our simple example above, instead of using the basis vectors $|A\rangle$ and $|B\rangle$, it is equally legitimate to use, for instance, $|A+B\rangle = |A\rangle + |B\rangle$ and $|A-B\rangle = |A\rangle - |B\rangle$. Correspondingly, instead of the basis states $|\nwarrow\rangle$ and $|\nearrow\rangle$ for the apparatus, we can use:

$$|\updownarrow\rangle = |\nwarrow\rangle + |\nearrow\rangle \quad \text{and} \quad |\leftrightarrow\rangle = |\nwarrow\rangle - |\nearrow\rangle$$

This means that our original superposition can be written (in terms of these states):

$$|\Psi\rangle = |A+B\rangle|\updownarrow\rangle + |A-B\rangle|\leftrightarrow\rangle$$

where, again, we have suppressed normalization factors. The problem is that in this rewriting of the original superposition, each component does not represent a well-defined macroscopic state (position) of the apparatus, and so these components cannot correspond to what an observer experiences in any of the worlds (see Zurek 1991, 39, for a simple but more realistic example of this situation).

Decoherence is generally supposed to alleviate this problem since it provides a physical argument for a preferred basis.[20] Thus, it can be shown from the form of the interaction between the apparatus and the environment that only the component states of the preferred basis – in which the states correspond to macroscopically definite positions of the pointer – are robust or stable enough over time to lead to perceivable records of measurement outcomes for observers (see e.g. Schlosshauer 2007, 85). Nevertheless, it is still a matter of debate whether decoherence fully resolves the preferred basis problem within the Everett interpretation. The reason is that the problem ultimately involves questions of how physical and mental states (of

[19] "[The interpretation] postulates that a wave function that obeys a linear wave equation [the Schrödinger equation] everywhere and at all times supplies a complete mathematical model for every isolated physical system without exception" (Everett 1957, 455).

[20] Decoherence may also be relevant for another important problem in Everett-type interpretations – namely the question of how the probabilistic nature of quantum mechanics can be reconciled with a deterministic (e.g. Everett's) theory, see e.g. Wallace (2008, 48).

the observer) relate, and how quantum mechanics precisely is supposed to account for our experiences. For instance, Vaidman (2008) notes:

> ... one cannot rely on the decoherence argument alone in order to single out the proper basis.... The fact that we can perceive only well localized objects in definite macroscopic states might not be just a physics issue: chemistry, biology, and even psychology might be needed to account for our evolution.

3 Metaphysics and philosophy of physics

Since the demise of logical positivism and the subsequent rise of scientific realism from the 1960s onwards, philosophers of physics have unhesitatingly embarked on metaphysical projects with the aim of analyzing the traditional philosophical notions of e.g. determinism, causality, time, and matter in the light of modern physics. But what is precisely the relation between (the philosophy of) physics and metaphysics? For instance, need metaphysicians take note of advances in physics? Unsurprisingly, a common view among philosophers of physics is that metaphysical analysis will be idle unless this is done. Thus, for instance, Maudlin (2007, 1) asserts that "... when choosing the fundamental posits of one's ontology, one must look to scientific practice rather than to philosophical prejudice".[21] Equally unsurprising, not all metaphysicians are convinced by this imperative. Sider (2007, 6) phrases the issue in these terms:

> The question leads some to extremes. At one end, we find those who think that all metaphysics can do is report science. At the other end we find those who think that metaphysics should ignore science and listen only to ordinary beliefs. Each extreme is questionable.

However, as Sider himself hints (2007, 7), it is not obvious that science vs. ordinary beliefs is the right dichotomy in the first place: "Perhaps the mere fact that a belief is an ordinary one counts for nothing at all; perhaps we should instead trust reason, a faculty capable of guiding both philosophically sophisticated scientists and scientifically informed philosophers". The characterization of the professionals in this quote suggests a more even-handed, and mutual beneficial, relationship between the two disciplines – an idea which seems to be endorsed also by philosopher of physics C. Callender (2010a, 16):[22]

> In slogan form, my claim is that metaphysics is best when informed by good science and science is best when informed by good metaphysics.

[21]Even if, as Suárez (2009) points out, Maudlin's own choice of ontology is not as unique as he seems to think (yet another case of the "notorious underdetermination of metaphysics by physics", Suárez 2009, 274).

[22]Callender's article gives, in my opinion, a well balanced survey of the present state of metaphysics from a philosopher's of science point of view, as well as an interesting historical analysis of the relations between philosophy of science (and physics) and metaphysics.

3.1 Time in physics and metaphysics

Whatever the exact relation between the two disciplines, a controversial issue on the borderline is the idea that physics may somehow rule out, or at least put pressure on, traditional metaphysical positions. This idea may be put under the heading of 'revisionary metaphysics' (Rickles 2008a, 10), or – less dramatically – of how physical theories affect our daily life notions which are usually taken as given. An obvious example here is the revisions concerning our notion of time brought about by Einstein's special theory of relativity. It is well known that relativistic time's dependence on the state of motion of the observer (or, more precisely, on his or her's path through space-time) leads to counter-intuitive, but experimentally confirmed, effects illustrated e.g. by the so-called twin paradox.

But the issue of time in the special theory of relativity goes deeper than just being counter-intuitive. Indeed, a much studied example of an apparent conflict between physics and metaphysics is that special relativity seems to be incompatible with a venerable position in the metaphysics of time, namely presentism. According to presentism, only present – as opposed to past and future – objects and events exist, and it, arguably, forms part of the common sense view of time as there seems to be something special about the present: The past is already gone and the future is yet to come.[23] The conflict with special relativity arises because, according to this theory, there is no frame-independent (absolute) notion of simultaneity and thus no privileged present. Consequently, two observers in relative motion will in general disagree about which objects and events are present (and may even disagree, for events which are not causally related, about which events are future and which are past). So, unless existence is relativized to a frame of reference – a move which few metaphysicians are prepared to make (see e.g. Hawley 2006, 466) – presentism does seem to be ruled out by special relativity. That this is not just a potential problem for metaphysicians is evidenced by Carnap's report on his discussions with Einstein (quoted from Savitt 2008):

> Once Einstein said that the problem of the Now worried him seriously. He explained that the experience of the Now means something special for man, something essentially different from the past and the future, but that this important difference does not and cannot occur within physics. That this experience cannot be grasped by science seemed to him a matter of painful but inevitable resignation.

Some metaphysicians speculate that the tension between presentism and special relativity might be resolved by masking up an ontological difference by epistemological inaccessibility, e.g. Markosian (2010):

> Perhaps it can be plausibly argued that while relativity entails that it is physically impossible to observe whether two events are absolutely simultaneous, the theory nevertheless has no bearing on whether there is such a phenomenon as absolute simultaneity.

[23] Another metaphysical position, sometimes known as 'possibilism', according to which past and present but not future objects and events are real, is equally (seemingly) incompatible with special relativity. Both presentism and possibilism are opposed to eternalism (or the 'block universe' view) according to which past, present and future events are equally real.

Another type of response (see e.g. Zimmerman 2011) insists that special relativity is strictly speaking false; that it is only an approximation to a supposedly more true theory – general relativity – and that this theory, in turn, is or may be (i) in conflict with quantum mechanics; and/or (ii) only an approximation to a future quantum theory of gravity.[24] Given that special relativity is not a complete theory of the universe, there is something right in this response. For, surely, if special relativity is relevant for the metaphysics of time, it cannot be irrelevant what general relativity (and perhaps a future theory of quantum gravity) has to say about the issue.

Recent work in the philosophy of (time and) physics indeed testifies that physics has more than special relativity to offer when it comes to surprising aspects of time.[25] At first sight, a theory shift does not help much for what generates the conflict with presentism is the relativity of simultaneity, and this relativity is maintained in the general theory of relativity. Yet, in some cases, e.g. in the standard cosmological model, a preferred frame (and hence a notion of absolute simultaneity) can be singled out in general relativity. Philosophers of physics, however, have recently discussed attempts to revive an argument, originally due to K. Gödel in 1949, according to which presentism not only has no future in the context of general relativity but even that general relativity compels us to reject the reality of time itself.

3.1.1 Gödel, general relativity and the (un-)reality of time

In various books, the latest from 2005, P. Yourgrau exposes and defends Gödel's argument for the ideality or unreality of time. In simplified form, the argument runs roughly as follows (cf. Yourgrau 2005, 132 ff.):

1. Given the relativity of simultaneity, there is no intuitive time (including an objective and universal notion of past, present and future) in the special theory of relativity.

2. In the context of general relativity, an intuitive time (and thus a preferred frame of reference) reappears in some models of the theory in the form of a cosmic time (as, for instance, in the standard cosmological – or Big Bang – model).

3. But in other models of general relativity, the so-called Gödel universes, no such intuitive notion of time can be found due to the existence of "closed time-like curves".[26]

[24]The possible conflict between relativity and quantum mechanics arises most obviously in those interpretations which add structure to quantum mechanics, e.g. Bohm's theory and Ghiradi, Weber and Rimini-type theories, as these added structures may demand a preferred frame of reference, see e.g. Zimmerman (2011) for an accessible discussion and references.

[25]Of course, metaphysicians are aware of this and have sometimes proposed to take the debate about presentism and physics from special relativity into general relativity and even quantum gravity, see e.g. Zimmerman (2011).

[26]On such curves, there can neither be a clear distinction between past, present and future nor between before and after. Thus neither what, after McTaggert, is called the A-series (dividing times and events into past, present and future) nor the B-series (ordering times and events via the relations before, simultaneous, and after) are viable accounts of time on these curves.

4. Although our world appears *not* to be well described by a Gödel model without intuitive time, the fact that such models are physically possible implies that there is no intuitive time even in our actual world.

For this to be an argument against the reality of time, one needs to add – as Gödel did – a zeroth premise saying that without an "intuitive time", and in particular without an objective and universal distinction of past, present and future, there is no time at all. Gödel effectively argued that time is real only if change is real, and that change is real only if there is an objective and universal lapse (or flow) of time. Moreover, Gödel took such an objective lapse to be equivalent to the fact "that reality consists of an infinity of layers of 'now' which come into existence successively" (Gödel 1949, 558); and this picture of reality is only possible, according to Gödel, if a distinguished global time can be found.

Though Gödel's argument has convinced few commentators of the unreality of time, the recent debate surrounding it has revealed the argument's importance both for a deep issue about time – namely its apparent flow (as reflected in common metaphors such as time as a river) – and for the interrelation between metaphysics and physics. This is indicated by the fact that the main criticisms of Gödel's argument have been directed against the zeroth (and first) and fourth premise.

Concerning the zeroth (and first) premise we have already touched upon the issue of presentism vs. relativity above. Among philosophers of physics, a common reaction to this conflict has been to reject not the reality of time – as Gödel's 0th and 1st premises amount to – but rather to reject presentism (and possibilism). This still leaves open, however, whether the flow or passage of time is an essential characteristic of time. For opinions are divided concerning both what the flow of time could possibly mean, and whether such a notion is compatible with the eternalist or block universe view in which the objectivity and universality of the distinction between past, present and future are denied.

3.1.2 The flow of time

According e.g. to Dorato (2002), Dieks (2006), and Arthur (2008), one need not conceive of the flow or passage of time in terms of an infinity of global now's which come into existence successively. Rather, these authors maintain, the flow of time may be understood in a minimalist and local sense as the successive happening of events along a world line. As noted e.g. by Arthur (2008, 208), local (or proper) time – the time according to which something ages or the lapse of time for somebody travelling in space-time – is independent of a frame of reference, and so it is independent of there being a global now.

Maudlin (2007, 116) also rejects Gödel's equivalence between objective time lapse and the successive coming into being of global now's. But he argues that time's passage is something over and above the mentioned minimalist sense. For Maudlin, the passage of time primarily amounts to "a fundamental objective distinction between two temporal *directions* [between past and future] in time" (2007, 116).[27] On the other hand, Price (2009, 10) argues against Maudlin that while it may be possible

[27] Maudlin does not clearly say what time's passage is beyond such a temporal direction although he notes (p. 108) that the "passage of time is an intrinsic asymmetry in the temporal structure of the world, an asymmetry that has no spatial counterpart".

to consistently assign a temporal direction at every place and time (so that the direction of time is the same everywhere), this is not sufficient for having an *objective* direction of time since only a conventional choice will establish if this direction should be taken as towards the past or the future.

Furthermore, Price contends, there is nothing flux-like about time, and so no coherent and informative notion of passage can be given. The reason is that the notion of passage, for Price, implies that it must be possible to say at what *rate* time passes. Price gives an, in my view, convincing argument against Maudlin's contention that the answer "one second per second" is reasonable (and informative). Since Price also rejects the notion of an objectively distinguished present moment – and holds that this notion together with that of an objective temporal direction and a flux-like character of time are the only available options for cashing out the notion of objective passage – he insists that the passage of time is "not so much false as doubtfully coherent" and that it is "a theoretical dead end" (2009, 2).

This conclusion, however, might be too fast. For one could argue, as e.g. Dorato (2002, 263) does, that we should resist the temptation to identify the flow or passage of time with a "moving now" or indeed with any kind of motion. Rather, one might insist that the flow of time is not further analyzable (beyond the minimalist notion of flow discussed above). On such a view, Price's demand for a sensible rate of passage might be resisted. In a different kind of defense of the notion of time's passage, Norton (2010a) argues that though passage may not be defined non-circularly, it shows no sign of being just an illusion (as Price seems to have it). Nevertheless, Norton (2010a, 30) appears more skeptical about the minimal (and local) notion of passage mentioned above, for he suggests that we "don't find passage in our present theories" and that when we dismiss passage as an illusion we simply (wrongly) attempt to "preserve the vanity that our physical theories of time have captured all the important facts of time".

3.1.3 From the possible to the actual

Let me now come back to the fourth premise in Gödel's argument. If we were to accept the first three (and the zeroth) premises we would have the result that time is ideal in the possible world described by the Gödel model universe. The step from here to the ideality of time in our world (which does not appear to be of the Gödel type, basically because it is not a rotating world) is considered by Yourgrau to be the "most subtle and elusive step, the one from the possible to the real" (2005, 130). As Yourgrau explains, this step fits well with Gödel's mathematical Platonism "which committed him to the existence of a realm of objects that are not accidental but ...necessary" and which implies that "...what necessarily exists cannot exist at all unless it exists in all possible worlds" (2005, 130). Given that the Gödel model and the standard Big Bang model of cosmology are but two different solutions to the same general relativistic equations, which only differ as regards the distribution and motion of matter at large scales, "it cannot be that whereas time fails to exist in that possible world [Gödel's model], it is present in our own" (Yourgrau 2005, 131).[28]

[28]Note that Yourgrau's conclusion follows when it is also assumed that if time exists then it exists necessarily (in all possible worlds). Gödel was apparently less sure about this premise (see

Both Dieks (2006, 163) and Arthur (2008, 223) seem to be willing to accept that if the Gödel model is physically possible, then one can argue that time does not exist in any model of general relativity (both, however, question the antecedent). By contrast, Dorato (2002, 265) accepts Gödel's universe as a physical possibility but insists that the metaphysical-modal step from the possible to the actual is "extremely difficult to justify". In any case – and no matter which part of the fourth premise is the weaker one – Gödel (1949, 562) admitted that, though he did not consider it philosophically satisfying, it would not be contradictory to assert that (at least some) aspects of time could after all be contingent and not necessary.

3.1.4 Time and quantum gravity

Before closing this section, let me very briefly comment on the question of time in connection with a possible future quantum theory of gravity. The so far unsuccessful attempt to construct such a theory is an attempt to unify Einstein's general theory of relativity with quantum theory (or quantum field theory). A quantum theory of gravity is supposed to fundamentally change our usual notion of time but it is still wide open what this change will amount to. Indeed, a key issue in the field is known as the *problem of time* which has to do with the fact that the central equation in (so-called canonical) quantum gravity, the Wheeler-DeWitt equation, does not depend on time at all. This has led to the suggestion that perhaps reality is timeless at some fundamental level. In a very readable article on the challenges to the reality of time coming from quantum gravity, Callender (2010b, 59) notes

> Some physicists argue that there is no such thing as time. Others think time ought to be promoted rather than demoted. In between these two positions is the fascinating idea that time exists but is not fundamental. A static world somehow gives rise to the time we perceive.

In relation to the first and last option, Callender appears to motivate the idea of a world without time (independently of quantum gravity) by means of an analogy (2010b, 65):[29]

> Physicists are able to compactly summarize the workings of the universe in terms of physical laws that play out in time. But this convenient fact should not trick us into thinking that time is a fundamental part of the world's furniture. Money, too, makes life much easier than negotiating a barter transaction every time you want to buy coffee. But it is an invented placeholder for the things we value, not something we value in and of itself. Similarly, time allows us to relate physical systems to one another without trying to figure out exactly how a glacier relates to a baseball. But it, too, is a convenient fiction that no more exists fundamentally in the natural world than money does.

below).

[29]For a critical philosophical examination of an attempt to argue from quantum gravity to the idea that time is not just emergent but rather an illusion (the first option in the former quote), see Butterfield (2002).

Does this analogy between time and money hold? Though it would be impractical, we could dispose of money since both goods and their relative value can be defined independently of them. It is far from obvious, however, that an analogous move is available for time. For while we can say that a year is (approximately) equal to 365 days, both days and years are intrinsically temporal notions as they refer to (the duration of) physical processes.[30] In any case, reflections on the possible consequences of quantum theories of gravity continue to be a fascinating avenue into the mysteries of time.

4 Philosophy of science and (philosophy of) physics

As pointed out in Butterfield and Earman's introduction to their *Handbook* (2007), analytical philosophy in general and philosophy of science in particular were highly influenced by physics in its formative years in the beginning of the twentieth century. For instance, the quantum and relativity revolutions highlighted the role of conceptual analysis in physics and the need for a critical re-evaluation of the Kantian ideas concerning the possibility of justifying scientific knowledge in a purely *a priori* manner. Together with the fact that influential philosophers of science often had formal training in physics, this makes it unsurprising that many of the traditional problems in general philosophy of science were originally formulated with a view to the situation in physics. This is true both for more epistemological and methodological issues such as the theory-ladenness of observation, the degree of rationality in theory choice, or the realism/instrumentalism debate; and for more ontological issues such as the nature of causality, the status of natural laws, or the prospects for reductionism.

Apart from the history of the subject mentioned above, one can point to several reasons why the questions posed by general philosophy of science are often particularly pressing in connection with physics. First, physics is sometimes seen as a quest for all encompassing theories which, at least in principle, describe everything. Such an ambition obviously puts the debate about reductionism and unity in science into an extreme form. Second, a characteristic feature of physics is that it often concerns objects quite different from those we deal with in daily life. In part because some physics objects, such as electrons, are hard to envisage in a pictorial manner; and in part because some of the objects from the zoo of physics are on the verge of what is testable (e.g. strings or black holes). No wonder then, that the realism debate is often framed most sharply with examples from physics in mind. In relation to this, one can thirdly point to the fact that physics theories are often in conflict with our pre-scientific experiences and intuitions. An obvious example is the idea that a body in motion will keep moving unless it is forced to stop – and the distance between our immediate experiences and the claims of physics only becomes larger in modern physical theories.

[30] Rugh and Zinkernagel (2009) argue for a version of what is known as (non-reductive) relationalism about time according to which time is necessarily related to physical processes in such a way that we can neither conceive of time without physical processes nor of physical processes without time.

4.1 The multiverse in modern cosmology

For a contemporary example in philosophy of physics where general philosophy of science issues come to the fore, consider the case of the multiverse idea in modern cosmology. As we shall see below, this is a case in which critical examinations of physics may also have an impact on discussions which take place within general philosophy of science proper. These discussions concern three issues, namely the question of a demarcation criterion for science, the nature of scientific explanation, and Bayesianism. One reason for choosing the multiverse example is that modern cosmology is still somewhat underexposed philosophically speaking even though the philosophy of space and time ranks as the second most popular (after the foundations of quantum mechanics) topic in the philosophy of physics. It therefore provides a fresh example to bear on the aforementioned (and classic) themes in the philosophy of science.

In general terms, the idea under discussion is that our universe is just a small portion of a much larger and/or older structure – the multiverse. A widely cited classification for multiverse scenarios is Tegmark's four levels of parallel universes (suggested in 1999 – for an update and references, see Tegmark 2003). Tegmark sorts multiverse models into those featuring spatial regions lying outside our cosmic horizon (class I), those containing many regions each with their own "big bang" – e.g. different bubbles in the inflationary scenario (class II), those corresponding to the worlds of Everett (class III), and those involving universes with different physical laws and even different mathematical structures (class IV).

Why a multiverse? Several motivations have been mentioned in the literature. For instance, Rees has employed what he calls the 'slippery slope argument': if you are willing to believe in the existence of galaxies outside our cosmic (visual) horizon, then why reject the existence of regions outside our universe (which you likewise cannot see even in principle)? Others, such as Vilenkin, take the multiverse hypothesis to be an inevitable consequence of the physical processes (e.g. inflation) which shaped our (part of the) universe. Yet others, like Carr, take the multiverse to be the most natural explanation for an alleged fine-tuning of our universe for life and consciousness (Carr and Ellis 2008, 2.31).

As pointed out by Ellis (Carr and Ellis 2008), each of the motivations for the multiverse (and there are more than mentioned above) can be resisted. Nevertheless, it appears that, for Ellis, the strongest (or least weak) motivation for the multiverse is that it offers "a reasonable theoretical explanation of the fine-tunings" – though he insists that "this does not help in observationally confirming the hypothesis [of a multiverse]" and that "the main problem with this proposal is that it can explain anything at all, because in a multiverse with an infinite or extremely large variety of universe properties – for example, the 10^{500} possibilities allowed by the landscape of string theory – virtually anything can happen" (Carr and Ellis, 2008, 2.35). The perceived fine-tunings are often discussed in connection with the so-called anthropic principle, and their possible explanation via the multiverse is envisaged in probabilistic terms. Below we take a closer look on recent commentaries on both the anthropic principle and the role of probabilities in the multiverse setting. But let us start with what has been and still is a central question in the debate surrounding the multiverse: Is it science?

4.1.1 The multiverse and demarcation criteria

From the logical positivists onwards, the question of a demarcation criterion for science has been central to the philosophy of science. As is well known, the positivists themselves first suggested verification, then confirmation (Carnap), before Popper came up with the idea of falsification. The Popperian notion still plays a dominant role in science which is evidenced in the debates on the multiverse, even if philosophers of science have modified it (Lakatos), or argued for an altogether different criterion. For instance, Kuhn took the distinctive feature of science to be periods of ‘normal science’ in which scientists are engaged in puzzle-solving within the framework of the paradigm in question (see e.g. Hanson 2008 for discussion and references).

The discussion about demarcation criteria is occasionally taken up outside philosophical circles, for instance in connection with attempts to discredit ideas (such as creationism) which do not fall within established science. The multiverse, however, is a case in which the involved scientists themselves (coming from the established sciences of theoretical physics and cosmology) discuss demarcation. In a recent article on the controversy over the multiverse, Kragh points to the interest of this case from a philosophical point of view (2009, 530):

> It is a widespread feeling in the community of theoretical physics that fundamental physics, in particular as related to cosmology and theories of quantum gravity, may be on its way to undergo a major epistemic shift . . . . There are clear indications that traditional standards of physics are increasingly being questioned and sought replaced by alternative non-empirical criteria of evaluation. The claims of an epistemic shift leading to a ‘new paradigm’ are of great philosophical and historical interest as they are truly foundational: they offer nothing less than a new answer to the question of what constitutes legitimate science, or at least legitimate physical science.

In connection with the multiverse, the feeling that Kragh refers to stems from the fact that the multiverse is in no obvious sense susceptible to be judged according to the standard criteria of testability and predictive power. Mainly for this reason, not all scientists involved are prepared to argue from the multiverse towards a new conception of what constitutes science. In a recent discussion with Carr, Ellis – a leading critic of the multiverse idea – opens his part of the article in this way (Carr and Ellis 2008, 2.33):

> The very nature of the scientific enterprise is at stake in the multiverse debate. Its advocates propose weakening the nature of scientific proof in order to claim that the multiverse hypothesis provides a scientific explanation. This is a dangerous tactic. . . . [C]an one maintain one has a genuine scientific theory when direct and indeed indirect tests of the theory are impossible? If one claims this, one is altering the meaning of science. One should be very careful before so doing. There are many other theories waiting in the wings, hoping for a weakening of what is meant by “science”. Those proposing this weakening in the case of

> cosmology should be aware of the flood of alternative scientific theories whose advocates will then state that they too can claim the mantle of scientific respectability.

While Ellis is worried, others have tried to turn a potential vice into a virtue. Thus, Weinberg, in the opening paragraph in his (2007) entitled “Living in the Multiverse” remarks: “Most advances in the history of science have been marked by discoveries about nature, but at certain turning points we have made discoveries about science itself. These discoveries lead to changes in how we score our work, in what we consider to be an acceptable theory”.[31]

Testability and predictive power are surely virtues of physical theories which should not be dismissed lightly. Nevertheless, as the above hints and as Kragh (2009) shows in detail, physicists and cosmologists are divided on the question of how (and at what stage in theory building) these criteria should be applied. Moreover, there is a tendency among physicists discussing these issues to focus on a simplistic version of Popper’s views. Kragh (2009, 547) quotes Susskind, another multiverse advocate, for this statement: “As for rigid philosophical rules, it would be the height of stupidity to dismiss a possibility just because it breaks with some philosopher’s dictum about falsifiability”.

But few, if any, philosophers of science today will defend a simplistic falsification method as a demarcation criterion. So what could or should philosophers of science say regarding the scientific status of the multiverse? In line with Kuhn (1977), one obvious response is that testability and prediction are certainly not the only virtues we may ask from a scientific theory. For instance, in a recent commentary on string theory, Cartwright and Frigg (2007) highlight such virtues as consistency, simplicity, explanatory power and unifying power. From the point of view of Lakatos’ methodology of scientific research, Cartwright and Frigg recommend a tolerant philosophical attitude to string theory as it has arguably made progress with respect to unifying and explanatory power even if not (yet) in terms of successful predictions. This view can plausibly be extended to the multiverse in general.[32] However, as Cartwright and Frigg (2007, 15) hint, this does not settle important questions about the priorities regarding allocation of funds and human resources which may well form part of the background context for the demarcation discussion among physicists: “In practice, however, the questions of how much to invest in this effort [string theory or other multiverse theorizing] and what should be sacrificed for that investment still remain”.

4.1.2 Anthropic explanations and a hypothetical infinite universe

Though the idea has a long history, anthropic explanations have recently been quite popular among physicists and cosmologists – a popularity which is naturally linked with that of the multiverse. Thus, Carter introduced the modern formulation of

[31] On the other hand, it is interesting to note, as Kragh (2009, 549) does, that Weinberg formed part of a 1986 anti-creationism coalition supporting the view that for something to be a legitimate scientific hypothesis, it “. . . must be consistent with prior and present observations and must remain subject to continued testing against future observations”.

[32] One version of the multiverse is the so-called string landscape, see e.g. Kragh 2009 for references.

the anthropic principle in 1974 in connection with the suggestion of "an ensemble of universes characterized by all conceivable combinations of initial conditions and fundamental constants" (quoted from Kragh 2009, 536). This modern formulation of the anthropic principle holds that the universe is somehow conditioned by the existence of human observers. The principle comes in (at least) two versions – weak and strong. Roughly, the weak principle states that the universe is compatible with life as we know it, whereas the strong one states that the universe must (necessarily) be compatible with life and/or conscious observers.

In a recent paper, Mosterín points out that whereas the weak anthropic principle is at most a tautological inference rule (e.g. from the fact that we exist, we infer that the physical conditions for our existence are satisfied in the universe), the strong version is a substantial metaphysical thesis without any foundation in physics (2005, 20-21). Thus, neither version can constitute a scientific explanation of anything. As Mosterín is well aware, this kind of critique of anthropic reasoning is not novel (see e.g. Earman 1987). But there is an important aspect of his discussion which has not received much attention in the philosophical literature surrounding the multiverse and anthropic reasoning – namely the implications drawn from alleged infinities in cosmology.

According to Stoeger (2007, 445) many cosmologists now interpret the anthropic principle to imply an ensemble of universes (a multiverse), and this has led to a moderate version of the principle which has been thought to play an explanatory role regarding the perceived fine-tunings in our universe. The point is that an infinity, or at least a very large number, of universes seem to automatically boost the variety of universes and hence make less mysterious the conditions in our own universe. The argument is summarized by the astrophysicist and multiverse advocate Martin Rees: "In an infinite ensemble, the existence of some universes that are seemingly fine-tuned to harbour life would occasion no surprise" (quoted from Mosterín 2005, 33). However, as Mosterín points out (2005, 34):

> A frequent confusion in the anthropic literature is the notion that an infinity of objects characterized by certain numbers or properties implies the existence among them of objects with any combination of those numbers or characteristics.

Take an infinite universe to be any hypothetical universe in which the number of worlds (or regions with certain properties) is infinite.[33] In that sense it is indeed easy to conceive of infinite universes in which far from all combinations of properties exist (think e.g. of a universe consisting of our actual observable region surrounded by an infinity of identical planet-less stars). This point should be borne in mind when one is faced with statements, such as the following, concerning the implications of the infinite:

[33] There are at least two relevant ways in which an infinite universe could be realized: i) a spatially infinite universe (as in some versions of the standard big bang cosmological model); or ii) an infinite number of bubble universes (each of which could be spatially finite or infinite) coming out of some inflation models. As pointed out by Ellis (2007, section 4.3) we cannot actually know whether our universe (or multiverse) is infinite as it depends on the, for us empirically inaccessible, global topology of the universe.

> Is there another copy of you reading this article, deciding to put it aside without finishing this sentence while you are reading on? A person living on a planet called Earth, with misty mountains, fertile fields and sprawling cities, in a solar system with eight other planets. . . . You probably find this idea strange and implausible . . . . Yet it looks like we will just have to live with it, since the simplest and most popular cosmological model today predicts that this person actually exists in a Galaxy about $10^{10^{29}}$ meters from here. This does not even assume speculative modern physics, merely that space is infinite and rather uniformly filled with matter as indicated by recent astronomical observations. (Tegmark 2003)

Contrary to what this claim suggests when taken at face value, Mosterín's point makes clear that more than infinity (e.g. an infinite space) must be assumed in order to assert the existence of copies of ourselves somewhere out there. Careful authors on the multiverse explicitly point out that the extra element is some sort of randomness (see also next subsection). For instance, Barrow and Tipler wrote in 1986 concerning the idea that all possibilities are realized in an infinite universe: "The infinity is not a sufficient condition for this to occur; it must be an exhaustively random infinity in order to include all possibilities" (quoted from Mosterín 2005, 35). The question then becomes: on what grounds can such an 'exhaustively random' infinity be justified or even postulated?[34]

As one might have guessed such grounds do indeed draw on speculative modern physics or at least on disputable philosophy. For instance, Knobe, Olum and Vilenkin (2006, 51) argue that "every possible history [including your entire life story] occurs an infinite number of times". For this argument to go through, however, they need to make at least two assumptions which may be questioned. First, to secure the relevant infinity, they presuppose the correctness not only of inflationary cosmology (in its particular 'eternal' version), but also that eternal inflation indeed predicts a multiverse with an infinite number of (spatially infinite) island universes.[35] More importantly, they assume a particular interpretation of quantum mechanics – namely that of "decoherent histories". Knobe *et* al asserts (p. 49) that this interpretation, which may be seen as a version of the many worlds interpretation mentioned earlier, implies that every history (e.g. mine or yours) is reducible to a quantum history, and that there are only a finite number of such quantum histories (which are each assigned a non-zero probability). In particular, this means that they, like Tegmark, assume what one could call quantum fundamentalism – the idea (already in Everett's original 1957 article) that everything in the universe is fundamentally quantum and ultimately describable in quantum mechanical terms. This idea, however, may be resisted, for instance via Bohr's approach to quantum mechanics that we discussed earlier.[36]

[34]This randomness should be such as to guarantee, for instance, that in an infinite universe (e.g. with an infinite number of stars) there is a non-zero probability for occurrences like: a star having a planet in an life-permitting distance; such a planet hosting life; life developing into humans like us; and these humans having a genetic set-up and history identical to ours.

[35]Ellis and Stoeger (2009) dispute the claim that eternal inflation leads to an infinite number of island universes.

[36]Note that once quantum fundamentalism is questioned, it becomes much less obvious that we

Thus, again, there is more than infinity involved in the argument for the realization of every possibility, and any or all of the 'extras' may of course be questioned. This also means that the explanatory virtues of the anthropic principle in an infinite universe are less straightforward than some authors seem to suggest.

4.1.3 The multiverse and Bayesianism

As already mentioned, one motivation for the multiverse is its alleged capacity to provide explanations in cosmology via probabilistic considerations. Indeed, the role of probabilities in connection with the multiverse forms part of the discussion among scientists for and against the idea. Thus, Carr remarks (against Ellis) that: "...a core difference between the Bayesian and frequentist views is the former's willingness to make inferences from single, and possibly unrepeatable, pieces of data [e.g. the actual conditions in our universe]" (Carr and Ellis 2008, 2.37). However, it transpires from a recent paper by Norton (2010b), that the problem is not so much about making inferences from single pieces of data but rather that Bayesianism has intrinsic limitations and that there are cases in which a probabilistic logic is simply inadequate.[37]

Norton's main point is that in cases where the evidence is completely neutral with respect to some hypothesis H, a Bayesian analysis leads to spurious results as it conflates this neutrality of the evidence with disfavouring evidence (support to non-H). Moreover, Norton contends, this conflation is not uncommon in cosmology which "often deals with problems of universal scope for which evidence is meager" (2010b, 502). The essential and formal point is easily stated (2010b, 501):

> The [Bayesian] system tries to represent this complete neutrality with a broadly spread probability measure that ends up assigning very low probability to each possibility. The trouble is that this same very low value of probability is correctly used when that same possibility is strongly disfavored by the evidence or, equivalently, its negation is strongly favored.

The problem is that the probabilistic Bayesian analysis has no natural way to represent neutral evidence. A common device for representing such neutrality is the principle of indifference which roughly states that, in the absence of further information, all outcomes are equally likely (i.e. assigning a uniform prior probability distribution to the outcomes). Of course, the problems with this principle are well known both in the literature on Bayesianism (see e.g. Howson 2008, 108 ff.) and on the multiverse – where it appears in connection with the so-called measure problem (see e.g. Aguirre 2007). Nevertheless, Norton seems to go further than these criticisms by arguing that in cases of completely neutral support (where we have no information or assumptions about some randomizer generating the outcomes), we cannot assign a prior probability distribution – uniform or otherwise – for the outcomes at all, as probabilities are simply the wrong tool for the occasion.

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can sensibly assign a probability – even a vanishingly small one – to an event like "a human having a genetic set-up and history identical to yours".

[37]Norton's paper was first presented at a joint philosophy and cosmology conference in Oxford in 2009 where the main theme was the multiverse, see http://astroweb1.physics.ox.ac.uk/~philcosmo2009/home.

The reason is, according to Norton, that in such cases the principle of indifference should be understood (for a continuous parameter) as assigning equal support to *any* interval of outcomes, and this idea is incompatible with the additivity of probabilities. Suppose, for instance, that there is a multiverse with an ensemble of totally disconnected universes each of which has some value of a constant h. In the case of completely neutral evidence (we know nothing about the distribution of h over the universes), for any universe each of the intervals $0 < h \leq 1$, $1 < h \leq 2$, $2 < h \leq 3$, etc. would, by the principle of indifference, have equal support. But given the neutrality of the evidence we might as well rescale and consider the quantity $1/h$ and so, analogously, we should assign equal support to the intervals $0 < 1/h \leq 1$, $1 < 1/h \leq 2$, etc. This is equivalent, however, to assigning equal support to the intervals $\infty > h \geq 1$ and $1 > h \geq 0$. From this rescaling line of argument, it is easy to see that "all nonempty, proper subintervals of $0 < h < \infty$ must be assigned the same support" (Norton 2010b, 506). Thus, the requirement of additivity of degrees of support inscribed in the probability calculus (e.g. that $P(0 < h \leq 1) + P(1 < h \leq 2) = P(0 < h \leq 2)$) cannot in general be met.[38]

To get a feeling for how probabilistic reasoning is alluded to in cases where it might be inappropriate, consider the analogy used in this quote of Carr and Ellis (2008, 231):

> [A]lthough multiverse models have not generally been motivated by an attempt to explain the anthropic fine-tuning, it now seems clear that the two concepts are interlinked. For if there are many universes, the question arises as to why we inhabit this particular one and (at the very least) one would have to concede that our own existence is a relevant selection effect. Many physicists therefore regard the multiverse as providing the most natural explanation of the anthropic fine-tunings. If one wins the lottery, it is natural to infer that one is not the only person to have bought a ticket.

From the perspective of Norton's critique, it is not hard to see what is wrong with the analogy. When you win the lottery ticket it may be reasonable to infer that other people bought a ticket but, in any case, the very idea of winning a lottery presupposes that other tickets exist and that the winning ticket has been drawn more or less randomly from the collection of tickets. By contrast, our universe being the way it is ("winning the lottery") does not presuppose that other universes (with different properties) exist – our evidence is simply neutral in this respect. Furthermore, we have no *a priori* right to presuppose that the values of the parameters characterizing our universe are bestowed on it by some random process – and so no right to presuppose a probability distribution (uniform or otherwise) of the outcomes. Therefore, a judgment of what is natural to infer from our universe being as it is (with us in it) hangs in the air.

A more specific and much discussed example of probabilistic reasoning in the multiverse has to do with the value of the so-called cosmological constant Λ. Here is Carr (Carr and Ellis, 2.37) on the example:

[38]For a very instructive case where the principle of indifference is used to generate an unreasonable conclusion, see Norton's discussion of the "Doomsday argument" (p. 513). As Norton shows, the conclusion – that our world is soon to end – is an artifact of the additivity of probability measures used.

George [Ellis] rejects the Λ argument but there is no doubt that this has been very influential in attracting many physicists to the multiverse cause. It used to be thought that Λ was exactly zero and it was then plausible that there might be some physical (non-anthropic) explanation for this. However, the fact that Λ is non-zero but very tiny is a profound mystery that completely changes the situation. Critics say that we cannot know what distribution for Λ is predicted across the multiverse and that is correct. It may be simplistic to assume that the distribution is uniform, but postulating that there is a spike in precisely the observed region is just as improbable as what we are trying to explain.

This however, seems to be a poor response to Norton's point on probabilities. As already noted, only *if* some randomizer is assumed – e.g. if we assume that universes with different values of the cosmological constant are produced according to some stochastic process – can the probabilistic analysis be used (but, obviously, the conclusion will then be only as strong as our belief in the stochastic process). This is not to say, of course, that physicists ought not to look for possible explanations for the values of important constants such as Λ. It is just that probabilistic explanations may not always be available.

Acknowledgements

I would like to thank Carl Hoefer, Svend E. Rugh, Maximilian Schlosshauer and Mauricio Suárez for helpful comments on a draft version of this paper. I also thank the Spanish Ministry of Science and Innovation (Project FFI2008-06418-C03-02) for financial support.

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