What have we learned?

In this lecture, we are going to look back over the content of the course and try to draw, at the end, a few general conclusions about the relationship of physics to philosophy. It may be helpful to cast the discussion in terms of the concept, popularized by the historian of science T.S. Kuhn, of a "paradigm". Very crudely and briefly, a "paradigm" may be regarded as the overarching conceptual framework that is generally accepted at a given period in the history of science, and that governs the kinds of question one is allowed to ask and the sorts of answer to them which are regarded as acceptable; in other words, it, as it were, sets the rules for the research that may legitimately be carried out. In Kuhn's thinking, the idea of a paradigm is associated with an important distinction between periods of so-called "normal science" when a given paradigm reigns supreme, and so-called "revolutionary" periods when an old paradigm is challenged and eventually overthrown by a new one. There has been much discussion in the literature as to how rigid this "revolutionary/ normal" distinction is, as well as of other aspects of Kuhn's thesis, but it is enough for our general purposes that almost everyone would agree that, for example, the introduction and eventual acceptance of special relativity and of quantum mechanics indeed correspond to a Kuhnian "revolution" in a recognizable sense of the word, and most people would probably also allow that the GRWP theory,* if it is eventually accepted, would constitute a "new paradigm". Of course, there are certainly border-line cases: does the inflationary scenario in cosmology constitute a new paradigm, or is it merely a particularly interesting variant of the existing one? (I would personally opt for the latter view.) But it is enough for our purposes that a fair number of clear-cut cases exist.

Given this definition, then, it makes sense to look back over the history of physics as we have traced it in this course and ask: How did the various paradigms we have met answer various rather generic questions, and in what ways did the answers (and perhaps the questions) change in the various revolutions we have reviewed? We will look at five questions: What is the nature of space and time? What kinds of things are "real"? Which sorts of behavior are "natural", and which need explaining? What constitutes an explanation? What exactly is it that makes a theory "correct"?

Space and time

As we saw, in the Aristotelian picture of the world, the idea of space in general is in some sense very much modeled on "domestic" space: all points are very far from equivalent, and in fact everything has its "proper" place – earth at the bottom (center), fire at the top (and ether even further out). Particularly interesting for the Aristotelian idea of space is the assertion that if the earth were somehow displaced from its present position, things would nevertheless gravitate to *that* position and not to the displaced position of the earth – an absolute reference point par excellence! As to the Aristotelian idea of time, it seems to have been not that different from the Newtonian one, except less

*The Ghirardi-Rimini-Weber-Pearle attempt to modify QM; see lecture 22.

quantitative (though the Greeks did have devices such as sand-glasses to assure uniform measurement of time).[†] Needless to say, there was no concept of any intrinsic connection between time and space, and in particular the only "valid" reference frame was that tied to the earth itself.

In the physics of the 17th-century scientific revolution, the notion of space that eventually evolved is that formulated explicitly by Newton: an absolute, infinite continuum with no special reference point (hence the "bite" of the Leibnizian question: if the Universe is finite, what determines the particular place in space at which it is to be formed?). The idea that there is an absolute measure of space (for any one Galilean observer), though not formulated explicitly, seems essential to Newtonian physics (otherwise we cannot unambiguously define velocities, etc.) – but the concept of a standard measuring rod ("ruler"), etc., really comes in much later, with Einstein's analysis. It is interesting that although from a twentieth-century point of view we recognize that Newtonian physics is not only compatible with but implies the concept of Galilean invariance, the idea that all uniformly moving observers are of equal validity would almost certainly have been anathema to Newton himself and to most of his followers – although his "absolute space" has no preferred point of reference, it does very definitely have a preferred rest frame (so that it was an important question for Newton what defines this frame and makes it special). In the same way, Newtonian physics incorporates the idea of "absolute" time, which presumably implies that one has a standard measure such as a pendulum clock. However, just as in the case of space, no point in time is "special". Recall that these ideas, although strongly emphasized by Newton and his followers, were not universally accepted in his day – in particular, there was strong competition from the Leibnizian view that neither space nor time had an absolute existence, but both were relations between objects or events.

Special relativity, needless to say, drastically modifies the Newtonian picture by rejecting the idea both of absolute space and of absolute time. Of course, the notion that neither space nor time has a special reference point ("space- and time-translation invariance") is preserved in SR; but now not only is the actual measure of the spatial distance between two events dependent on the observer (as was of course already the case in Newtonian classical mechanics, though not always recognized or emphasized), but the time interval is also observer-dependent. Indeed, space and time become almost different aspects of the same thing – the space-time interval. With its emphasis on the concept of "events" and the space and time intervals between them, SR, at least in Einstein's original formulation, might seem perhaps closer to the Leibnizian than to the Newtonian philosophy.

General relativity in some sense knocks the props out even further from the idea of absolute space and time, by legitimizing in principle the use of any coordinate system whatever (including systems in which, for example, the unit of time is not uniformly related to that measured by a standard pendulum clock). However, there is still a class of frames in which the equations of motions look particularly simple, namely the inertial

[†]Other "pre-scientific" cultures seem to have had a rather more anthropocentric concept of time – e.g., in Japan, the length of an "hour" was different during the day and during the night.

frames: were this all, (if for example we could forget altogether about gravity), GR would be only a formal generalization of SR allowing us, if preferred, to work in an arbitrary (noninertial) reference frame. The real revolution, of course, consists in the recognition that the inertial frames are not necessarily those that appear to be so in Newtonian physics, but rather are the freely falling ones.

When GR is applied to the Universe as a whole, the picture is further modified: the whole idea of space-time in the absence of matter becomes in some sense dubious, since the very geometry of spacetime is itself determined by matter. This is particularly striking in the case of a closed Friedmann universe: the whole idea of space "outside" such a universe, or for that matter "time" before the Big Bang or after the Big Crunch, seems prima facie dubious (but compare the discussion of "other universes" in lecture 27). Has history essentially decided in favor of Leibniz?

What kinds of things are "real"?

For Aristotle and his followers, what was real, at least in a physical context, was "stuff" (ὕλη). It came, of course, in several varieties (earth, air, fire, water, ether) and could be transformed into different forms, but still it seems to be essential that it "occupied" space (note the Aristotelian-Cartesian abhorrence of a vacuum!) and had finite weight (though in the case of the ether, very little). Space as such does not seem to have been "real" to an Aristotelian: it was only, as it were, a place to put stuff. Of course, not all the Greeks took this point of view: for Democritus and Leucippus, what was "real" were the atoms – to some extent an anticipation of a possible late 19th-century viewpoint.

The concept of "matter" becomes a bit better defined with Galileo (note that the first of his "two new sciences" is what we would nowadays call "rigid-body mechanics") and Newton: it is interesting that the latter defines "mass" as density × volume, indicating that he took density rather than mass as logically primary. However, in distinction from the Aristotelians, Newton certainly regarded space and time as such as possessing a reality independent of the matter occupying (or not occupying?) them.

A very great addition to the list of what could qualify as "real" came with the work of Maxwell and Hertz in the mid- and late-nineteenth century. Recall that the experiments of Coulomb, Ampere and others had shown that electric and magnetic *forces* are certainly "real" (i.e., could produce physical accelerations, etc.). The idea of an electric or magnetic field, however, was, at least logically, originally only a sort of mathematical shorthand permitting simplification of the calculations; if asked to define exactly what was meant by saying that the electric field at a particular point is of a certain strength, the physicist could only reply that it means that if a test charged body *were* put at that position, it *would* experience a certain force (note explicit use of counterfactual!). Certainly, as long as one were considering only the static phenomena investigated by Coulomb, the "field" seemed at least a secondary attribute of the source that produced it. The turning point came with Maxwell's realization that it is quite possible for fields to exist in the *absence* of sources (or at least nearby sources!), as happens in the propagation of EM waves through vacuum. Thus, a quantity that was originally introduced in an auxiliary and "counterfactual" way turns out to have very real physical consequences!

(As a digression, it is interesting to note a somewhat similar phenomenon that occurs in the context of QM. In classical electromagnetism, it is often convenient to introduce the so-called "EM vector potential", in order to derive the magnetic field from it by differentiation. Within classical physics, this is simply a mathematical trick and no effects arise from the vector potential other than those that we already know arise from the magnetic field itself. In quantum mechanics, however, there are effects associated with the vector potential that prima facie *cannot* be explained in terms of the magnetic field alone.)

How did special relativity affect concepts of what is "real"? Apart from abolishing the idea of "absolute time", it also led to the famous prediction that mass as such is not conserved – for example, when a heavy particle decays radioactively, part of its rest mass (rest energy) is converted into the kinetic energy of the decay products, which in general is subsequently dissipated as heat. (Cf. the working of a nuclear power station!) Since it is now total energy rather than total mass that is conserved, it is tempting to regard energy as the "ultimately real" property of parts of the physical world and mass as somewhat derivative from it.

The most devastating challenge to traditional notions of "reality", however, undoubtedly comes from quantum mechanics.[‡] We already discussed this in some detail in lecture 23, and I just summarize the results of that discussion. According to the *standard* (essentially Copenhagen) interpretation of quantum mechanics, there seem to be two obvious candidates for the title of "really existing": the outcomes of measurement on individual systems, assuming that such actually occur, and the state vector (which, however, in this interpretation is a description only of the ensemble, not of the individual system). Alternative (non-standard) interpretations allow different candidates for "reality"; for example, in the many worlds interpretation, the state vector is a real property of each individual system of the ensemble, while in most versions of hidden-variable interpretation, properties like position or polarization are real characteristics of each microsystem even in the absence of measurement. Naturally, this feature comes at a price – Bell's theorem forces us, if we wish to attribute real properties to physically isolated systems in their own right, to renounce either local causality or induction.

Which kinds of behavior are "natural" and which need explanation?

In some sense, the different answers given to this question at different periods in the history of physics define or illustrate most sharply the Kuhnian notion of "paradigm"; by definition, two paradigms are "different" if they give different answers! For the Aristotelian physicist contemplating the terrestrial region, the "natural" behavior of a body is either to be at rest (if it is in its "proper" place) or to move towards its proper place; since the latter is essentially defined by its vertical coordinates (only), vertical motion, but no other, is "natural". Thus, in particular, motion in a horizontal direction,

[‡]It should be noted that the question changes somewhat at this point since, crudely speaking, we are no longer discussing what kinds of *thing* are "real", but what kinds of property or event are.

even at constant velocity, *needs an explanation* (which is given, in Aristotelian physics, by a force that in the last resort derives from the [quite different] motion of the heavenly bodies). In the celestial realm, by contrast, the "natural" form of motion is circular, and any apparent departure from this needs an explanation (hence the elaborate Ptolemaic system of epicycles). One might comment that the preference for circular motion (and uniform circular motion at that) could be regarded as just an application (though we would now of course regard it as an inappropriate one) of precisely the same kind of symmetry principle that guides the modern search for "simplicity" in particle physics and cosmology; after all, uniform circular motion certainly is, intuitively speaking, the most "symmetric" form of motion possible!

To Aristotle, the persistence of matter (ὕλη) as such does not seem to have posed any particular problem or to have required explanation, though of course the ways in which it changed its form ("generation and corruption") did. It is interesting that not all subsequent thinkers agreed. In fact, for Descartes and many of his contemporaries, the continued existence of matter posed a severe problem:

> It is as a matter of fact perfectly clear and evident to all who consider with attention the nature of time, that, in order to be conserved in each moment in which it endures, a substance has need of the same power and action as would be necessary to produce and create it anew, supposing it did not yet exist, so that the light of nature shows us clearly that the distinction between creation and conservation is solely a [conceptual] distinction of the reason [rather than of ontological causation]. (In Leslie, *Physical Cosmology and Philosophy.*)

This is one question that subsequent history seems to have decided rather firmly is a pseudo-problem: with two obvious exceptions (the Hoyle-Gold-Bondi steady-state cosmology and the GRWP theory of reduction in quantum mechanics) just about all subsequent physics has taken the conservation of mass, or at least (following special relativity) of energy, as something that does not need explanation, in fact as so basic a principle that it can be used as a benchmark in constructing new theories.[§] Descartes' position on this is a little reminiscent of that of Berkeley, who regarded the existence of unobserved objects as a problem that needed a definite explanation; in the latter case the problem has mutated and re-emerged in the context of quantum mechanics.

Contrary to Aristotle (and Descartes), Newton took the point of view that the free uniform motion of a body in any direction is "natural", and that it is nonuniform (i.e., accelerated) motion that requires an explanation, in the form of an external force acting on the body. In particular, in the case of the planets, the problem is not why they do not move in circles but why they do not move in straight lines! And, of course, he identified the *cause* of this "unnatural" accelerated motion as the gravitational force.

In his general relativity, Einstein in some sense took Newton's point of view further, by generalizing the concept of rectilinear motion to the more general one, appropriate

[§]In Newtonian physics, the conservation of *energy* follows from the hypothesis of conservative forces, but the conservation of *mass* has to be put in by hand.

to curved space-time, of *geodesic* motion. Thus, for Einstein, it is geodesic motion that is "natural", and in particular free fall; thus, no "force" need be involved to account for it. It is only *deviations* from geodesic motion that need explanation, in the form of nongravitational forces (electrical, pressure, etc.).

An obvious question, which occupied Einstein as well as many other people in the twenties and thirties, is whether one could carry this point of view even further, so that all motions that appear to us as "accelerations" could in the end be regarded as a consequence of the spacetime geometry. This idea is part of the motivation behind so-called Kaluza-Klein theories, which introduce one or more extra "hidden" dimensions of space and/ or time; so far, such theories have not had the degree of success that would make them generally accepted.

In a different direction, an interesting paradigm shift occurred in the middle of the nineteenth century in the context of theories of complex matter. In the earlier half of the century, thermodynamics was well developed as a phenomenological description of such matter but had no microscopic underpinning. In these circumstances it made sense to tabulate the thermodynamic properties (such as specific heat) of various substances (and this was widely done by practical engineers for important substances such as H_2O), but there was little sense to the question "*Why* is the specific heat (e.g.) of water at 30^oC what it is?" Once statistical mechanics was developed, on the other hand, it became clear that the thermodynamic behavior of materials was not accidental or random but was determined by the configurations and motions of the atoms and molecules composing it; and the question as to why, e.g., water behaves thermodynamically very differently from copper immediately acquired significance – what exactly is it about the behavior of the component atoms, etc., that makes the difference? This is perhaps a particularly clear case of the *meaning* of a question being intimately tied to, indeed perhaps defined by, the emergence of a class of possible "legitimate" answers to it. Note by the way that not everyone was happy at this development: Mach, in particular, felt that it in some way defiled the "purity" of thermodynamics as a phenomenological discipline. (Compare the somewhat uneasy relationship, in modern times, between the social sciences and "sociobiology".)

If the emergence of microscopic "explanations" based on statistical mechanics is an example of a question that had previously been meaningless acquiring a meaning, a dramatic example of the opposite process is afforded by the acceptance of the Copenhagen interpretation of quantum mechanics. Although in pre-quantum days there had been a few excursions into indeterminism, very few people doubted, e.g., that the question "Why did this particular molecule arrive at this particular point on the photographic plate?" had in principle a definite answer in terms of the initial preparation of the molecular beam and the forces (including intermolecular forces) acting on it. But in its standard (Copenhagen) interpretation, quantum mechanics simply refuses to allow such questions: while it regards as meaningful, and in principle answerable, questions about the statistical distribution of outcomes obtained on a given ensemble, questions about the reasons for the particular behavior of a particular member of the ensemble (system) are ruled out of court as meaningless. Of course, not all interpretations of quantum mechanics share this feature: in particular, in most versions of hidden-variable interpretation, the question is prima facie as meaningful, conceptually speaking, as in classical physics (and this, needless to say, is one of the arguments often given in favor of such interpretations). It is an interesting philosophical question whether the fact that in most types of hidden-variable "interpretation" (as distinct from "theory") it is a priori guaranteed that we shall never in fact be able to find the answer to the question (since otherwise we could in principle prepare ensembles whose behavior would disagree with the quantum mechanical predictions) makes the question itself meaningless!

When we come to modern particle physics and cosmology, the issue of which aspects of physical behavior demand an explanation and which "just are" becomes even more obscure. We already discussed (lecture 27) the question of whether it makes sense to ask for a reason for the Big Bang, or whether it "just happened". What of the dimensionless ratios of particle physics? Does it just "happen" that the fine structure constant (the quantity $e^2/\hbar c$ where e is the electronic charge) is about 1/137, or is there a "reason" for it to take this precise value? And if it should eventually turn out (say) that the Universe is closed rather than open or flat, would it make sense to ask *why* this is so? We already discussed this question to some extent in connection with the anthropic principle.

These last examples raise in peculiarly acute form the question: suppose that one is working within a given paradigm, how does one know whether a particular question represents a real problem or a "pseudo-problem"? For example, in the essay cited in the last lecture, Grünbaum is very insistent that the "problem" of creation in cosmology, be it in the Hot Big Bag or steady-state scenarios, is indeed a "pseudo-problem"; but how in the last resort does he know this – even assuming, as he implicitly takes for granted, that we are working within the standard twentieth-century paradigm rather than (say) an Aristotelian one? Is a "pseudo-problem" by definition simply a question to which the reigning paradigm does not specify a class of possible answers? There are some difficulties with this view, not the least of them being that in areas such as cosmology, where we are at the very edge of our understanding, it is not always clear ahead of time which classes of answer are permitted (another way of saying this might be simply that some of the paradigms currently on offer are not terribly well-defined!). This naturally leads to the next topic.

What constitutes an "explanation"?

Presumably, in everyday usage an "explanation" of an event or of a feature of the world is an account such that the questioner is expected either to stop asking further questions or, at least, to base these further questions on the account given. Taken in this sense, the "explanations" of various physical phenomena given over the history of the subject have been very diverse in nature. One type of explanation that has been quite common right up to comparatively recent times has been teleological – "because it is for the best" or "because God willed it so". One can take this kind of explanation in one of two ways: as a conversation-stopper or as an invitation to further questions. For example, suppose that someone claims that the constants of elementary particle physics are what they are because God willed it exactly that way and no other. One might then ask him the question "*Why* did God will it exactly that way?" and at this point he has two kinds of reply. One is "He just *did*", and then of course this is apt to close the discussion. But if our discussant were familiar with the background of the anthropic principle, he might alternatively reply "Because God wished human life and consciousness to evolve", and this then leads to a host of interesting questions about, e.g., how precisely the fine structure constant has to be equal to its observed value for this to happen.

A quite different kind of "explanation" of *particular* states of affairs would have been given by late 19th-century physicists in the tradition of Du Bois-Reymond: this particular state of affairs now occurs because at some initial time the configurations of the molecules involved were such-and-such, and the deterministic laws of Newtonian mechanics then caused the system to evolve to its present state. But, you ask, *why* were the initial configurations such-and-such? To which the answer is: within the paradigm of Newtonian mechanics, that is a pseudo-problem!

An interesting case where the very legitimacy of a particular kind of answer has been a matter of continuing debate is that of action at a distance. As we have seen, Newton formally postulated such an action in order to construct his theory of universal gravitation, but he never really believed it, indeed was quite scathing about it. In some sense we could say that this skepticism has been justified by the modern theory of fields, which obtains the static gravitational attraction as the result of propagation, at a finite velocity (c), of a field whose equations of motion are entirely *local*.

A new type of "explanation" was introduced with the development of statistical mechanics: the behavior we actually see in a macroscopic body is not explained as uniquely determined, but as the overwhelmingly most probable behavior of the system in the light of the information we have about it. Sklar (pp. 100-108) discusses some interesting aspects of the more general phenomenon of probabilistic explanation, within and outside physics.

As with so many other concepts familiar from classical physics, quantum mechanics adds a new twist. Most (though not all) quantum-mechanical explanations are statistical in nature and refer to the *distribution* of results obtained from members of a large ensemble; occasionally, however, they refer to individual members, for example when the "reason" why no particles at all arrive at a particular point on the screen is given as the fact that the probability amplitudes to reach this point by different paths interfere destructively. What distinguishes quantum mechanics from classical statistical theories, as we have seen, is that *in principle* no further explanation other than the statistical (or other) one based on the state vector of the ensemble in question is supposed to be possible (at least in the standard interpretation).

Finally, we have already noted the problems connected with "explanations" in cosmology that involve the anthropic principle at one level or another. This example alone should perhaps make us skeptical about the claim that a given paradigm automatically defines, by itself, what kinds of answer are to count as legitimate!

One generic feature that all "explanations" in cosmology, or at least all explanations of particular states of affairs (for example, the structure of the galaxies we see around us) appear to have in common is that they refer to the past rather than to the future:

the Universe is the way it is now "because" it started off in a particular state in the past. Is it obvious that explanation in cosmology must have this character? One can think of two obvious reasons why it should. First, just as in everyday life we "remember" the past but not the further, so in investigating the Universe we can get information, for example from the light reaching us from distant quasars, about its past but not, presumably, about its future (this is related to the electromagnetic arrow of time). So unless we had some independent way of knowing what the state of the Universe will be in the future, a "future-oriented" explanation would seem unfeasible. A second reason, not unconnected with the first, is that given the usual thermodynamic arrow of time, many different initial (macroscopic) states can lead to the same final macrostate, but the converse is not true. Thus, it makes sense to ask *which* of the various initial states that could have led to the final state we see was in fact realized, but the converse question ("which state, of the various physically possible subsequent states, is actually going to be realized?") has only a trivial answer because in fact there is only one physically possible subsequent macrostate.

What is a "correct" theory?

What, exactly, are we saying when we say that we believe that Copernicus was right and Ptolemy wrong, or that the hot big bang cosmology is more likely to be "correct" than the steady-state theory?

One obvious suggestion is that a theory is "correct" if its predictions are verified, and that one theory is more "correct", or closer to the truth, then another if it gets more things right. But there are a number of well-known difficulties with this view. First, a theory that by this criterion would be "wrong", in that its predictions are not verified, can usually be saved by making a sufficiently complicated set of additional assumptions – e.g., in the case of Ptolemaic astronomy, by adding a few more epicycles, or in the case of the steady-state cosmology, perhaps by assuming we live in a very "untypical" neighborhood. Second, the concept of getting "more things right" is clearly a very subjective one unless one has a definite prescription for "weighting" different pieces of evidence; the Ptolemaic system would almost certainly do as well if not better than the Copernican one in predicting eclipses, at least for the next few hundred years! Third, and perhaps most importantly in some critical cases, it may not be entirely unambiguous what a theory does in fact predict. For example, we can agree that *given* the usual interpretative postulates, the predictions made by quantum mechanics for the relative probability of different outcomes in some type of standard experiment may agree with the experimentally observed distribution; but, in the absence of a unique specification of what exactly constitutes a "measurement" and hence where to apply the so-called measurement axioms, is it clear that quantum mechanics *as such* predicts definite outcomes at all?

Let's try to deal at least with the first couple of objections. Could we meet them by saying that a theory (A) is "correct", or should be preferred over its rival (B), if in order to explain the same experimental facts B has to be made much more complicated than A? In other words, other things being equal, the simpler theory should be judged correct?

For example, we judge special relativity to be "right" and classical mechanics "wrong" because in order to account for the results of the Michelson-Morley experiment, the latter has to be modified by the hypotheses of physical length contraction, time dilation, etc. Similarly, hidden-variable interpretations of quantum mechanics tend to be rejected because they require violations of local causality. And so on. As Poincaré pointed out very clearly long ago, it is possible to regard such choices as no more than a matter of convention. Is it at least clear which convention is enforced on us by the criterion of "simplicity"? It seems not necessarily: recall that the scheme that Copernicus eventually put together for a heliocentric solar system actually had *more* epicycles than the then-current Ptolemaic version; and the inflationary scenario, whatever its virtues, is by any reasonable criterion a good deal more complicated than an old-style uniformly expanding Friedmann universe!

An advantage of the conventionalist approach is that it may perhaps make us somewhat more skeptical about the validity of drawing great metaphysical conclusions from the data of physics. As an example, consider the case of the hot big bang. We have seen that there is a school of thought that has used the occurrence of the hot big bang in the standard cosmology to draw conclusions about the necessity of divine creation or something similar. As noted in the last lecture, the arguments used to support this conclusion are, even within the standard scenario, not necessarily immune to question. However, an equally important point in the present context is that, as we saw, the very occurrence of the hot big bang at any finite time in the past can be avoided by an appropriate redefinition of the time scale. The price of this redefinition is that various fundamental quantities, and in particular the speed of light, then depend explicitly on (redefined) cosmic time, and this no doubt would make actual calculations in this scheme a great deal more complicated; but it seems rather impertinent to demand of Nature that she behave in such a way as to make theoreticians' life easier!

One further criterion for preferring theory A to theory B might be that the former opens up new *kinds* of question, with the possibility of answering them. An example we have already seen is statistical mechanics versus phenomenological thermodynamics: in the case of the latter it is meaningless (or at least useless) to ask *why* the thermodynamic properties are what they are, a question to which statistical mechanics provides a set of answers in terms of the atomic structure and dynamics. Fairly clearly, a theory alternative to standard quantum field theory that could provide a *reason* why the masses and coupling constants of the known elementary particles (quarks, leptons, gauge bosons, etc.) are what they are would be likely to emerge victorious. Similarly, a cosmology alternative to the standard Friedmann one that could provide a reason why spacetime is $3+1$-dimensional would, provided it preserved the agreement with experiment as regards cosmic nucleosynthesis rates, etc., very likely be preferred to the standard model. Note that viewed from this angle, quantum mechanics is rather regressive: as we have seen, in its standard formulation it not only suggests (per se) no new questions but actually forbids us to ask certain old ones! In this case its other advantages (simplicity, etc.) seem in most peoples' minds to have outweighed this prima facie disadvantage.

At the end of the day the average physicist's response to the question "what is a good theory?" is probably similar to that of a well-known judge to "what is pornography?" – "I can't define it, but I know it when I see it!"

Philosophy and physics

What can philosophy do for physics, and vice versa? One view common among physicists is the one reflected in quotations from Weinberg and Davies (in Leslie, *Physical Cosmology and Philosophy*); Weinberg:

> I have difficulty in understanding the philosophical content that many people seem to find in discoveries in physics. It is true, of course, that many of the subjects of physics – space and time, causality, ultimate particles – have been the concern of philosophers since the earliest times. But in my view, when physicists make discoveries in these areas, they do not so much confirm or refute the speculations of philosophers as show that philosophers were out of their jurisdiction in speculating about these phenomena.

And Davies:

> It is a striking thought that ten years of radio astronomy have taught humanity more about the creation and organization of the universe than thousands of years of religion and philosophy.

Certainly, there is some truth in this, although a more charitable way of putting what is essentially the same point is that perhaps by definition, "philosophy" is the study of those questions that have not yet become the subject-matter of particular recognized scientific disciplines such as physics, biology, psychology, etc. (and in some cases may never become so). Most certainly, philosophers cannot afford to be ignorant of the experimental basis for our current notions about the physical world, and history has shown that it is extremely dangerous for them to use metaphysical preconceptions to make assertions about the world that are subject to *experimental* refutation (Kant on Euclidean geometry, Popper on nonlocality, etc.). Moreover, in the history of physics it is only comparatively rarely that a major advance has sprung directly from what could be called philosophical or quasi-philosophical analysis – the prime exhibit in this context would probably be special relativity, but this is the exception rather than the rule. Much more commonly, the historical and sociological origins of a major revolution in physics are to be traced, in roughly the way outlined by Kuhn, to the anomalies and surprises provided by *experiment* – and even in the case of special relativity, while it is a matter of debate to what extent, if at all, Einstein himself was influenced by a knowledge of the result of the Michelson-Morley experiment, it seems difficult to doubt that the special theory of relativity would have had a very much harder time getting accepted in the absence of that result. So it would be easy to relegate philosophy to a secondary role – the role, as it were, of mopping up after the great revolutions of physics.

On the other hand, if it is all too easy for philosophers to wander outside the boundaries of their legitimate expertise, the same can certainly be said for physicists. Many physicists, even those who self-consciously write about their subject for a lay readership, are all too obviously trapped in the assumptions of the current paradigm and unused to questioning them. In particular, there is a strong tendency to attribute to certain aspects of the current theory that are, as it were, technologically convenient, a much more fundamental role than they need in fact possess (an example, as we have seen, was the choice of a scale of cosmic time). This is the kind of point at which the sort of analysis that philosophers are used to giving can be very helpful; two typical examples, in the context of cosmology, are Grünbaum's analysis of the question of the "cause" of the big bang and Price's discussion of the arguments used by Hawking, Penrose and others on the question of time asymmetry in the Universe.

Once one realizes this in specific contexts, one becomes a lot more conscious of the obvious fact that just about *all* debate concerning the foundations of physics, be it by physicists, philosophers or whomever, takes place within a framework of certain very general assumptions about the physical world and our relationship to it that transcend the boundaries of specific paradigms, and indeed in many cases have been quite impervious to the major revolutions in the history of the subject. In the last lecture, I shall try, inter alia, to make explicit what some of these assumptions are, and ask whether they are indeed as unquestionable as we usually take them to be.