Pragmatism in Physics

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1. Introduction

A primary focus in the history of philosophy has been on the foundations of physics. The philosophers who have engaged themselves in examining the foundations are too numerous to mention. Great historical figures, from Plato and Aristotle to Descartes and Kant, come immediately to mind. It is also evident that the foundational examination of physics does not play a very significant role in contemporary physics. In fact, the situation is even more subtle than this remark would indicate. Specialized topics in the foundations of physics, such as the measurement problem in quantum mechanics, or the nature of quantum logic, have in themselves become highly specialized subdisciplines lying at the interface between philosophy, physics and mathematics. The purely formal or mathematical aspects are particularly evident in the detailed developments of quantum logic, more so than is the case for the measurement problem. Although I myself have been involved over many years, in fact from my earliest publications in the 1950s, in foundational work in physics, in this paper I want to step back from that tradition. It will continue, I am sure, to be important in philosophy, and will have some currency among physicists, for its focus on the foundations of physics. But the point of my analysis today is what are the central tendencies of physics, as opposed to the rather specialized foundational tendencies emphasized mainly by philosophers. My main thesis is that pragmatism reigns in physics in a way that is completely different from anything to be found in pure mathematics, and that pragmatism is, surprisingly, more evident in pure physics than in more recent but increasingly mathematically oriented disciplines such as economics.

There are three principal ways in which I think of physics as being dominated by pragmatism. The first is that both theoretical and experimental physics are not at all obeisant to any form of foundationalism, either of a formal or axiomatic kind, or of an epistemological emphasis on epistemic certainty. Theoretical physicists do not really operate in an axiomatic mold and actual theoretical work in physics doesn't primarily proceed in an axiomatic way, contrary to pure mathematics. By the way, this is not a conceptual claim about theoretical physics, but an empirical claim. There are good practical reasons, but not strong reasons of principle, why theoretical physics could not always be done in an axiomatic way. The rough and tumble of the actual work in theoretical physics, a complicated interplay of experimental facts and theoretical assumptions of a variety of kinds, explain perhaps why formal or axiomatic approaches are very much in the background. I make this, as I say, as an empirical claim. Readers who don't be-

P. Weingartner, G. Schurz and G. Dorn (eds.), *The Role of Pragmatics in Contemporary Philosophy*, 236-53.

lieve me are challenged to look at the last 300 theoretical articles published in *Physical Review Letters*, the most important journal for publication of new results, either experimental or theoretical, in physics.

In similar fashion, experimentalists are not in any sense searching for epistemic certainty in designing their experiments or reporting their observations. God help them if they were. Almost all modern physics is working at the edge of what is technologically possible. The results are tremulous and often just barely significant. Repeated experiments are required to actually confirm that what is claimed as a result is indeed one. Nothing could be further from the philosopher's pursuit of epistemological perceptual certainty than the status of the actual results most usually obtained in experimental physics. It's not that these results are not sound or of the right sort, it is just that the fantasies of philosophers about certainty of observations is not at all the right model of how to think about what experimental physicists are doing, what observations they are making and how they are reporting those observations.

My second pragmatic point is one to some extent shared with modern mathematics, namely the focus on practice as opposed to foundations. As the saying goes these days about the foundations of mathematics, with the great divergence of opinions about what a proper foundation for mathematics is, working mathematicians recognize the practice of mathematics as more secure than the foundations. In fact, one of the great anti-foundationalist moves, in both mathematics and physics, in the last several decades, is away from building scientific structures on unimpeachable foundations. It has come to be seen as a dream that is a mistake to try to realize, and that in fact can get in the way of real progress in mathematics or physics. The focus is on practice in physics even more than in mathematics. The way in which results, as I was just emphasizing, are reported, the analysis that is back of those reports, etc., in physics, are much more rough and ready, and further from any kind of foundational analysis, than is to be found in mathematics. When we turn from theoretical physics to experimental physics, this is even more the case. Any hope to put experimental physics on a formal axiomatic basis with an emphasis on foundations is certainly mistaken in too many ways to detail here. It is just that in the case of experimental physics, with the extensive dealing with the physical apparatus and the actual physical performance of experiments, the hopes of giving any kind of complete foundational account is quite mistaken. Mistaken in the way it would be to give a set of axioms for playing tennis that were meant to be in any way adequate to lead to descriptions of actual tennis games. There has been a faint hope of providing such foundational analyses as opposed to pure analysis or practice in theoretical physics, but it is certainly out of the question once one turns to experimental work.

The third general point about pragmatism in physics is that, on the other hand, there is much broad agreement by both theoretical and experimental physicists on the truth or falsity of many kinds of observations made with or without refined instrumentation. There is a commonsense background of physics about which there is no disagreement among physicists. There isn't some kind of clubby sociology of knowledge that makes physics seem similar to astrology or necromancy. It will not be my task here to make the case for this robust commonsense concept of truth and agreement about observations that dominates much of physics, with care taken to lay out the cases about which there is disagreement. I just want to assert this as a property of physics which, if disputed, needs to be disputed in powerful and detailed ways in order to be in the least convincing, which I don't think the current fashionable sociology of science does in any serious way at all.

The three features of pragmatism in physics I have just mentioned seem to me evident aspects of current work in physics. I also want to mention a fourth and rather different aspect – related but different – that can be seen by looking at longer historical slices of physics. This is the pragmatic way in which physicists, and here I include as part of physics, astronomy and astrophysics, can use observations, computations and fragmentary theoretical models from many different viewpoints over many different centuries and take from the past work just that which is relevant and relatively sound, ignoring various and sundry theoretical and ideological views that accompany the hard core of scientific work. This aspect of the long history of astronomy within physics, and of other parts of physics as well, is something that very much separates the way in which the history of astronomy has developed as opposed, let us say, to the history of theology.

Keeping in mind these general observations, what I do in this paper is to present three case studies of pragmatism in physics. The first concerns the twists and turns of mathematical and observational astronomy from Sumerian or Babylonian times to Copernicus and Newton. Here I paint with a broad brush, but with the definite intent of showing how only the pragmatic remains of each successive wave carried over from one epoch to another of theoretical and observational astronomy. By the time I have finished saying what I want to say about ancient astronomy, it will be necessary to treat the other two cases in a much more abbreviated form. The second case has something more detailed to say, however, about physical experiments thought of in modern terms with lots of delicate instrumentation. The third and final example is back to theory. It concerns the vexing problem of the variety of interpretations of quantum mechanics and how this relates to a pragmatic theme about physics.

2. First Case Study: Ancient Astronomy

2.1 Old Babylonian

Not at the very beginning, but at a very good place to begin, is with the old Babylonian astronomy, roughly from the dynasty of Hammurabi, which is around 1500 BC. Of course my intention here is not to survey the technical details, but to emphasize what some of the motivating features of this astronomy were and how those motivating features were completely dropped out pragmatically in later epochs of astronomy when other concerns dominated the picture. In this old Babylonian astronomy, primary interest of one kind or another was in astrology and concerns with the well being of the country. Predictions about the weather, seasons, harvest time, famines, war, peace and also the fate of kings were important. As far as I know, there is not much serious data criticizing in a sensitive way what could be predicted and what couldn't.

From the standpoint of records, the best records were the observations of Venus used extensively by the court astrologers. The trinity of great gods was that of the Sun, Moon and Venus. Why is Venus (*Ishtar* in a rough translation) important? The moon (Sin) and sun (Shamash) were responsible for the regular progression of months, days and years, important obviously for harvest times and other essentially periodic events. The goddess Ishtar, on the other hand, that is, Venus, was presumed to communicate other important events and possibly control them by her appearances and disappearances.

We certainly don't have a good detailed feeling for the mix of astronomy and astrology in those ancient days. There were certainly people worried about good observations with the rough tools available. How those that made the observations thought about the predictions made by court astrologers, etc., we scarcely really know. But what we do get is something we even get at the end of the twentieth century, a wonderful mix of practical needs and fanciful hopes. From early times it was important to know about the seasons and planting times, which were mainly to be put on a systematic basis by astronomical observations. Predictions about the fate of kings are of quite another character, for example. (For details, see van der Waerden, 1974.)

2.2 Neo-Babylonian

Now we move on to the neo-Babylonian and Persian period, roughly around 700 BC. There was a new and important use of astronomy – data for individual horoscopes. The methods developed in those early days are still in use today (see your local newspaper). The earliest known individual horoscopes are dated 409 BC, from Babylon. Undoubtedly, this does not mark the beginning, but the epoch in which they begin to occur. Note that good astronomical methods of calculation were needed to give plausible detailed horoscopes. No doubt the desire for horoscopes encouraged better astronomical observations and calculations.

In fact, the Babylonian astronomers of the sixth and fifth centuries BC had a developed theory of the motion of the moon and the planets. Due to O. Neugebauer, A. Sachs, B.L. van der Waerden and others we now have a good understanding of this Babylonian astronomy (Neugebauer, 1957, 1975). What is perhaps most remarkable about it from our own perspective is that it is highly developed, precedes the Greek models, but seems to be completely lacking in the kind of intuitive geometrical ideas characteristic of ancient Greek astronomy. One of the great accomplishments over this long Babylonian period was careful study of the periods of the planets in the sense of what was the period in years of their return to a given position in the sky.

2 3 Aristotle

As we all know, the Greeks introduced new and powerful methods of analysis in astronomy, based upon geometrical models. What is just as important is the way in which they moved away from the gods and goddesses of earlier times to a more austere conception of what the heavens, that is, the stars and planets, were. Most famous of all, perhaps, is Aristotle's strong defense of their eternal and unchanging character. Here are two quotations from *On the Heavens*.

> "The truth of it is also clear from the evidence of the senses, enough at least to warrant the assent of human faith; for throughout all the past time, according to the records handed down from generation to generation, we find no trace of change either in the whole of the outermost heaven or in any one of its proper parts. It seems too that the name of this first body has been passed down to the present time by the ancients, who thought of it in the same way as we do, for we cannot help believing that the same ideas recur to men not once nor twice but over and over again. Thus they, believing that the primary body was something different from earth and fire and air and water, gave the name *aither* to the uppermost region, choosing its title from the fact that it "runs always" and eternally."
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> Aristotle, *On the Heavens*, Book I, Ch. III (270b10)

> "Trusting, then, to the foregoing arguments, we may take it that the world as a whole was not generated and cannot be destroyed, as some allege, but is unique and eternal, having no beginning or end of its whole life, containing infinite time and embracing it in itself."
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> Aristotle, *On the Heavens*, Book II, Ch. 1 (283b)

2 4 Ptolemy

I shall return to Aristotle's doctrine of the eternity of the heavens in a moment but, first, let us note that nearly half a millenium later, Ptolemy holds on firmly to Aristotle's basic doctrines about the nature of the world, but strongly affirms the importance of mathematical methods, especially as referred to Greek astronomy in the following passage from the *Almagest*.

> "For Aristotle divides theoretical philosophy too, very fittingly, into three primary categories, physics, mathematics and theology. For everything that exists is composed of matter, form and motion; none of these [three] can be observed in its substratum by itself, without the others: they can only be imagined. Now the first cause of the first motion of the universe, if one considers it simply, can be thought of as an invisible and motionless deity; the division [of theoretical philosophy] concerned with investigating this [can be called] 'theology', since

> this kind of activity, somewhere up in the highest reaches of the universe, can only be imagined, and is completely separated from perceptible reality. The division [of theoretical philosophy] which investigates material and ever-moving nature, and which concerns itself with 'white', 'hot', 'sweet', 'soft' and suchlike qualities one may call 'physics'; such an order of being is situated (for the most part) amongst corruptible bodies and below the lunar sphere. That division [of theoretical philosophy] which determines the nature involved in forms and motion from place to place, and which serves to investigate shape, number, size, and place, time and suchlike, one may define as 'mathematics'. Its subject-matter falls as it were in the middle between the other two, since, firstly, it can be conceived of both with and without the aid of the senses, and, secondly, it is an attribute of all existing things without exception, both mortal and immortal: for those things which are perpetually changing in their inseparable form, it changes with them, while for eternal things which have an aethereal nature, it keeps their unchanging form unchanged.
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> From all this we concluded: that the first two divisions of theoretical philosophy should rather be called guesswork than knowledge, theology because of its completely invisible and ungraspable nature, physics because of the unstable and unclear nature of matter; hence there is no hope that philosophers will ever be agreed about them; and that only mathematics can provide sure and unshakeable knowledge to its devotees, provided one approaches it rigorously. For its kind of proof proceeds by indisputable methods, namely arithmetic and geometry."
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> G.J. Toomer, *Ptolemy's Almagest*, pp. 35-36.

Descartes is reminiscent of Ptolemy in this passage. I cannot help but juxtapose the famous final proposition of Part II of Descartes' *Principles of Philosophy*. Descartes asserts in ringing terms in Proposition 64, the last one of Part II, that he has not received into the principles of physics any principle not already received into mathematics, nothing further is needed in reasoning about the world. In another passage nearly as categorical in character towards the end of the *Principles* (Part IV, Article 203), Descartes summarizes his theory in these terms (my translation):

> "Having considered in general all the clear and distinct notions that can be in our understanding concerning material things, and not having found any of these other than those of figure, size, and motion, and the rules according to which these things can be diversified by one another, which rules are the principles of geometry and mechanics, I judged that all the knowledge that men could have of nature had necessarily to be derived from this only; because all the other notions that we have of sensible things, being confused and obscure, cannot serve to give us knowledge of anything outside us."

These wonderful rhetorical passages by Ptolemy and, more than a thousand years later, by Descartes are just that – rhetorical in character. Ptolemy, above all, used in an intricate and compelling way not just mathematical arguments but astronomical observations extending back at least a thousand years. Moreover, even from a mathematical standpoint, he mixed his own geometrical methods with the linear calculational methods of the Babylonians, using these Babylonian methods more than a thousand years after they were first discovered by Babylonian astronomers. So what we have going from old Babylonian times to Ptolemy is a wonderful continuous use of data and method and complete pragmatic willingness to shed, when necessary, unnecessary baggage of theological, ideological or other kinds of beliefs intertwined with what we would think of today as astronomy proper.

2 5 Philiponus

But the story doesn't end there. We can continue this sequence of change of beliefs about the nature of the heavens. It was a problem for Christians to deal with Aristotle's view that the heavens are eternal, because of the necessity to follow the Bible and hold that God created the heavens and the earth and, therefore, the heavens must have a date of creation. Aristotle was attacked exactly on this point by Philiponus nearly a thousand years after he wrote *On the Heavens*. Here is a salient passage from Philiponus, written in the spirit and style of Aristotle, but very much in opposition. It is a passage cited by Simplicius in his commentary on Aristotle, for we no longer have the original text of Philiponus.

> "For even if it is agreed", he says, "that no movement is contrary to the movement of the heavens, it is at least not impossible that there is a privation of this movement. For there is some opposing privation of any natural thing that exists in a substrate. But motion is a natural thing. For the immobility (*akinêsia*) before the movement and after the cessation (*paula*) of the movement is in fact the privation of this movement. Therefore, if it is not impossible that there is a privation opposite to the movement of the heavens, it follows that <Aristotle> did not use <the word 'contraries'> in the sense of form and privation, but in the sense of contraries <in the proper sense>."

Philoponus (690-574 AD), *Against Aristotle on the Eternity of the World*
(Cited by Simplicius 122.1)

2.6 Aquinas

We can pass on now for another half a millenium, really more than that, to Aquinas, who gave a very sensible answer to Christians about this problem of the eternity of the heavens. I quote the recognition that it was not a matter that could be demonstrated but must be accepted on faith from *Summa Theologica, Part I, Question 46, Article 2*:

> "I answer that, That the world did not always exist we hold by faith alone: it

> cannot be proved demonstratively; which is what was said above of the mystery of the Trinity. The reason for this is that the newness of the world cannot be demonstrated from the world itself. For the principle of demonstration is the essence of a thing. Now everything, considered in its species, abstracts from here and now; which is why it is said that *universals are everywhere and always*. Hence it cannot be demonstrated that man, or the heavens, or a stone did not always exist."

So, the flip flops from contradiction of Aristotle to recognition that there needed to be a separation between what can be demonstrated and what must be taken on faith continues for well over a thousand years after Aristotle, and continues in spite of the great authority of Aristotle in most matters. What is important is that this controversy about the eternity of the heavens really had no serious influence on or bearing upon the continued tradition of astronomical observation and calculation, pragmatic to the core, with no serious relevant ontological commitment to the nature of the heavens.

2.7 Copernicus

With Copernicus, still another change took place. Copernicus was not the first, but certainly the most persistent, in arguing for the earth not being the center of the universe. From a metaphysical or, indeed, from a physical standpoint, this is a radical change from the Aristotelian and Ptolomaic view, but it did not affect the fact that Copernicus used, continually and ceaselessly, the data and computations of Ptolemy and his followers. Pragmatism, in this case, held sway by not being disturbed by a drastic physical change or a metaphysical change, but continuing without interruption the quantitative observation and analysis of data.

That the Copernican "revolution" was not as great as is popularly portrayed is well argued in the following reflective passage from Neugebauer:

> "...gravitational theory made heliocentric procedures the only reasonable ones (lunar theory always excepted). One generally has this fact in mind when one speaks about the 'simplification' introduced by the Copernican system in comparison to the Ptolemaic. In the actual sequence of historical events such a simplification never existed, considering equivalent problems. Neither Ptolemy nor Copernicus had the faintest concept of dynamical conditions prevailing in the solar system. For their purely cinematic purposes, however, it is irrelevant at what step one introduces the transformation to geocentric coordinates which are eventually needed as long as one wishes to describe what an observer is expected to see. And in fact it is by far simpler to analyze planetary phenomena like stations and retrogradations by means of a geocentric model than for a moving earth; and the same holds for the basic relations between sidereal and synodic periods, phenomena which are significant only for a stationary observer. Hence here, as always, the choice of a coordinate system depends on the problem one wishes to solve and since ancient astronomy is mainly concerned

> with the cinematics of planetary motion and with the related phases the use of geocentric coordinates is usually preferable. And geocentric coordinates are unavoidable for the theory of sun dials and related problems."
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> O. Neugebauer, *A History of Ancient Mathematical Astronomy, Part Three* p.1088.

How tangled and intricate the tale of comparison between Ptolomaic and Copernican astronomy is is documented in the greatest detail in Swerdlow and Neugebauer (1984). A point of emphasis, documented painstakingly, is that Copernicus assumed that Ptolemy's geocentric models were mathematically and empirically correct, so that his task was mainly to find physically reasonable, but mathematically equivalent, heliocentric models for all the main phenomena.

2 8 Early Chinese Astronomy

The same pragmatic tendency to use observations from the past, but to ignore increasingly the variety of cosmological ideas that accompanied these observations is well documented for early Chinese astronomy in Sivin (1969). As Sivin describes in detail, even the austere cosmological models of constant time cycles (first century AD) for major astronomical events had to give way to the demands for more reliable predictions in the later years of the Han dynasty. Here is Sivin's summary:

> "The demand for precision had to win out, once it had been maneuvered into conflict with the goal of metaphysical consistency and unity. With the aid of hindsight, we might propose that the Chinese had formulated their classic conception of the universe as a congeries of cyclical time relationships on the basis of too primitive a model. The assumption of simple cyclical behavior could not have survived for long. In the Han it was maintained because it made mathematical astronomy possible, but at the cost of compromising the integrity of the system. When this cost became intolerable, the assumption was discarded. It was never replaced by new assumptions more conformable to the complexity of the celestial motions, for by the time of its rejection the technical tasks of astronomy could be carried out without such assumptions. Later Chinese calendrical science was marked by an indifference toward cosmology – but this was the indifference of the disenchanted, not that of the inexperienced."
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> N. Sivin, *Cosmos and Computation in Early Chinese Mathematical Astronomy*, p. 67

For those who may think that such large swings of opinion about the fundamental natures of things are no longer to be found in modern scientific astronomy, I end this case study with a postscript on modern views concerning the Big Bang. The popular view is that there is no disagreement of a fundamental nature these days about the universe having begun some 15 billion years or so ago with an awesome event. It is important to note that respected scientific figures, no doubt ones very much in a minority, don't agree with this at all. I mention in particular the views of Fred Hoyle and his colleagues Burbridge and Narlikan, who in several articles in the 1990s have advanced the following radical thesis (Arp, Burbidge, Hoyle and Narlikar, 1992; Hoyle, Burbidge and Narlikar, 1994; and Hoyle, Burbidge and Narlikar, 1995). There was no Big Bang, but oscillations occur with approximate periods of 40 billion years. The most recent oscillatory minimum was about 14 billion years ago. Galaxies from earlier cycles should be present and should be subject to confirmation. The microwave background radiation field arose from earlier cycles of matter creation, and, most astonishingly of all, the bulk of photons in the background radiation field must be at least 800 billion years old, coming from at least 20 cycles. Now these large claims are not accepted, really, by perhaps anyone but the authors and a few of their disciples, but they are serious astronomers and it is important to recognize that the tradition of ever better observations and ever more detailed quantitative theories continues. There can continue to be large swings about what is fundamentally back of it all. The pragmatism of physics is such, however, that these large swings in fundamental views do not necessarily disturb in any major way continued programs of experiment, observation and the building of particular mathematical models.

3. Pragmatic Aspects of Physical Experiments

There are a number of observations about physical experiments that I think are correct and that bring out the pragmatic aspects of such experimentation.

1. Modern experimentation is often like driving an automobile. The details and theory of the instruments being used in the experiment are not known to the experimenter except in a very general way. There is a standing joke among physicists which reflects a serious truth. The experimental physicists claim that the theoretical physicists don't really understand the experiments that they are doing, but only their own theories of the experiment, and the technicians or engineers responsible for the instrumentation claim that the experimentalists don't really understand well enough the performance characteristics and the underlying theory of the instrumentation being used in the experiments. Of course, in turn, the theoretical physicists will often hold that only they really understand the relevant physical theory. The point is that modern experimentation is extremely complicated, using subtle and elaborate instruments of manipulation and observation of physical phenomena. The analogy to the driver of an automobile is not at all far fetched.

2. There is in the literature almost no foundational analysis of the skills required of an experimenter. In one sense we have a larger systematic literature on the skills required of an automobile driver than of an experimental physicist.

3. Experimenters are taught in an explicit way, often, how to write up reports of their experiments. But the tradition here is like sports reporting. Only the results of the experiment are reported in any serious detail. The procedures are not.

4. What is really missing is not what propositions are true or claimed to be true, in connection with an experiment, but *how* were instruments used and *how* were the experiments performed.

5. The main point is that this is not simply a deficiency of physicists, but that it is impossible to describe in detail the skills of an experimenter just as in the case of an athlete. Our description of the performance of a good tennis player is enormously sketchy in character. In the same way, our description of the performance of a good quantum optics experimenter is a bare outline at best.

6. Imagine learning to play the piano or tennis just from listening to lectures. The same is true for conducting physical experiments. It is no more feasible to lecture experimentalists about how to perform experiments than it is to lecture in any sufficient way how to play a sport. It doesn't mean that some lectures don't convey some information, but it is just that they are not in any sense adequate to preparing the sports player or the experimenter for the tasks ahead.

7. Learning is by apprenticeship, in the great and ancient tradition of learning to practice a craft. There is no adequate verbal description of what is learned. A codification of the practice is at best sketchy and far from sufficient. But this verbal inadequacy is an inadequacy in principle, because experimentation is a pragmatic matter of doing and of use, not a matter of propositional knowledge.

8. The physical use of instruments – from astrolabes to lasers – has no necessary and sufficient, or complete, verbal description.

9. Detailed instrumental knowledge or skills for use is difficult to convey. A good example of this is Neugebauer's admission that even he was not competent to provide a history of ancient astronomical instruments. It is generally conceded that one of the great problems of detailed interpretation of ancient astronomy in any culture is understanding in exact detail the use of the instruments constructed for making observations.

10. So we conclude that an adequate propositional and axiomatic foundation of experimentation is impossible in principle. There can not be an adequate verbal account of how experimentation is conducted. Learning is by doing – the most important and most radical form of pragmatism.

11. Fashions change in physics and the use of computers is now to be found everywhere, but as long as real experiments are to be conducted and new instrumentation is to be brought on line, learning by doing, not learning from propositional accounts of experimentation, will hold sway, and this will apply just as well to computers as to astrolabes and telescopes of the past.

4. Interpretations of Quantum Mechanics

The same pragmatic attitude holds sway, I claim, in the diverse interpretations of quantum mechanics that all rely on the same experimental data. I have in mind here, of course, non-relativistic quantum mechanics, and the large body of experimental evidence that does not require relativistic considerations and that supports the "classical" formulation of the theory. This is not to say there are not disagreements about the fine points of the theory and, in fact, that is one of the aspects of interpretation to be stressed. A good example would be many of the interpretations to be mentioned differ on the way they give an account of quantum mechanical measurement, what in the foundational literature is called "the measurement problem". What is important about these measurement procedures is that they are not really empirical disputes in the sense that they propose different empirical procedures for making measurements in quantum mechanics. If they did, then those differences could not be brought together by the conception of pragmatism in physics I am advocating. What is critical for the working physicist is that these different accounts of the measurement problem do not have experimental consequences that can be the basis of further experiments and disputes as to the correct interpretation.

I do want to emphasize that my remarks here are slightly at variance with the latest rounds of experimentation regarding matters such as decoherence. What happens when quantum mechanical experiments are analyzed in terms of the final mixing, so to speak, of the quantum mechanical particles, or other micro phenomena, that are observed with macroscopic equipment. Leaving these delicate matters aside, there is remarkable agreement about the experimental results, and yet remarkable disagreement on the correct interpretation of quantum mechanics. There are a number of variants, which I shall not go into, but I mention especially, first, the classical von Neumann Hilbert-space interpretation set forth in elegant detail in his early book, "*Mathematical Foundations of Quantum Mechanics*" (1932, 1955), and this view is amplified in a number of respects by later papers of Wigner and others. The remarkable aspect of this interpretation is the highly subjective account that is given of measurement and the way in which, especially at the hands of Wigner (1961), even consciousness is brought into the interpretation of quantum mechanics. What is important from a pragmatic standpoint is that the experimental articles from one end of standard quantum mechanics to another will scarcely mention any aspect of this part of the interpretation. What they will use will be the standard theory of bounded operators in Hilbert space, but even here, in the papers close to experiment, the mathematical apparatus set forth explicitly by von Neumann will scarcely be invoked.

Without being able to back up the following empirical statement with actual data, I think it probably is the case that most physicists operate more or less within this framework. This framework is also given a closely related but variant interpretation as the Copenhagen interpretation, advocated by Niels Bohr (1934). The von Neumann and Copenhagen interpretations are closely related, in that they make no search for hidden variables, that is, causes that cannot be observed but that satisfy more classical assumptions in generating quantum mechanical phenomena. In fact, it is probably fair to say that in conversation experimental physicists would probably be more inclined to refer to the "Copenhagen" interpretation rather than the "von Neumann" interpretation of quantum mechanics. Closely associated with the Copenhagen interpretation is the positivistically driven "matrix mechanics" of Heisenberg (1930). Bohr and Heisenberg in the 1930s were close to each other in their views on quantum mechanics.

I have not at all tried to go into the niceties of any of these interpretations mentioned so far, because what is really more important here has been the thrust for hidden variables that explain, if you will, the strangeness of quantum mechanics and bring quantum mechanics back more closely to something like classical physics. Perhaps the earliest and in many ways most discussed interpretation within this framework is that of David Bohm, first set forth in his classical article of 1952. The remarkable aspect of Bohm's theory is that it is not only a hidden-variable theory, but also a deterministic one, which takes us in some sense all the way back to classical physics, but it would have to be regarded as a strange version of classical physics. In brief, Bohm introduces something like a gravitational potential or gravitational field of classical physics, but in this case a quantum potential. And it is this quantum potential, unobservable and therefore hidden, that is used to give a classical, deterministic account of quantum mechanics. A recent and very up to date version of Bohm is to be found in the recent large book by Holland (1993), which shows that Bohm's ideas are still alive and well. It is also worth noting that many aspects are closely connected with the earlier work of the French physicist Louis de Broglie and sometimes the theory is referred to as the de Broglie-Bohm version of non-relativistic quantum mechanics.

The physicist John Bell, famous for his insightful work on hidden-variable theories of quantum mechanics, summarizes the de Broglie-Bohm version in the following positive way.

> "I will try to interest you in the de Broglie-Bohm version of non-relativistic quantum mechanics. It is, in my opinion, very instructive. It is experimentally equivalent to the usual version insofar as the latter is unambiguous. But it does not require, in its very formulation, a vague division of the world into 'system' and 'apparatus,' nor of history into 'measurement' and nonmeasurement.' So it applies to the world at large, and not just to idealized laboratory procedures. Indeed the de Broglie-Bohm theory is sharp where the usual one is fuzzy, and general where the usual one is special."
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> *Bell (1987)*, p. 111.

An example of the way in which the interpretation of the measurement process can lead to a new view of quantum mechanics is to be found in the influential work of Ghirardi, Rimini and Weber (1986), who overcome many of the standard paradoxes about measurement by providing a unified dynamics for microscopic and macroscopic systems. An essential step in their work is to suppress linear superposition of states of a macroscopic object localized in distant spatial regions. This step is needed in order to give a description of macroscopic objects in terms of trajectories. This implied restriction does not have known experimentally observable consequences.

Another very different interpretation, usually called stochastic quantum mechanics, or sometimes just stochastic mechanics, has been developed especially by Edward Nelson in two influential monographs (1967, 1985). Nelson's basic idea is a wonderful one, one that connects quantum mechanics to classical physics in a much more direct and mathematically and conceptually elegant way than Bohm's. This is by using the theory of Brownian motion, especially as developed in the 1920s by Norbert Wiener. Moreover, this is a theory that very much retains the stochastic or probabilistic element of quantum mechanics rather than being deterministic, as is the case with the Bohmian theory.

From a physical standpoint, still earlier than Wiener, Albert Einstein made substantive contributions to Brownian motion in the same year in which he discovered the special theory of relativity (1905). But, paradoxically enough, he seems to have been quite unaware of the earlier study of Brownian motion. Brownian motion is named after the Reverend Robert Brown, who published his first paper on it in 1828. From observing the familiar phenomena that dust particles can be seen moving around in air, in what seems to be a random way, and that when any small sample of water is looked at through a microscope it seems to have "particles" moving around in random fashion, he formulated the basic concept. One of the early difficulties was of course to decide whether the particles were themselves living organisms or not. That is an issue that stands apart from our present story. There was continued and occasional investigation, both conceptually and experimentally, into Brownian motion during the rest of the nineteenth century, but Einstein's ideas were among the first to put it into the mainstream of physics. The mathematical theory, brought to maturity by Wiener (1920, 1921a, 1921b, 1923, 1924), is used by Nelson as the basis of stochastic mechanics. (Wiener's work of this period is, more generally, a seminal contribution to the modern measure-theoretic theory of stochastic processes for continuous space and time.) But, and this is an important *but*, the experimental results are just the same as those dealt with in the Copenhagen interpretation. The remarkable thing about these many interpretations of quantum mechanics is that no new experimental results come out as predictions and, consequently, an empirical choice of the sole correct view is not possible. It is also the case that most experimental physicists will be essentially completely unaware of the development of stochastic mechanics by Nelson and others.

My words about stochastic mechanics and experiments are a little too simple. A feature of stochastic mechanics, not a necessary one, but a natural one, is to adopt the usual Markov assumption of classical mechanics, that is, a complete knowledge of the current state of the physical system is a sufficient condition for applying the dynamical equations to determine the future states. Earlier knowledge of past conditions is not needed. In his 1985 book, "*Quantum Fluctuations*," Nelson analyzes the locality phenomena, as familiarly embodied in Bell's inequalities, and notes that the Markov assumption of stochastic mechanics lies on the side of classical mechanics, not quantum mechanics, and it can be regarded as a refutation of stochastic mechanics that the Markov assumption of classical Brownian motion prevents stochastic mechanics from having the necessary quantum nonlocality features.

Nelson concludes this discussion of locality at the end of quantum fluctuations with the following personal note, unusual in any physical treatise. "I have loved and nurtured Markovian stochastic mechanics for 17 years, and it is painful to abandon it. But its whole point was to construct a physically realistic picture of microprocesses, and a theory that violates locality is untenable" (1985, p. 127). In the following pages Nelson does discuss the possibilities of introducing a non-Markovian Brownian motion, that is, a Brownian theory in which the motion of particles depends not only upon the current state of the particles but also upon their past history. But this is a sharp and conceptually radical departure from the classical theory, as Nelson emphasizes. In spite of these problems of locality, however, it is remarkable how much of the standard quantum mechanics Nelson was able to rederive within the framework of Markovian Brownian motion.

Still another interpretation of quantum mechanics is the famous Everett (1957) interpretation, the many-worlds view, well characterized in this explanatory footnote, added in press, to clarify his ideas:

> "In reply to a preprint of this article some correspondents have raised the question of the 'transition from possible to actual,' arguing that in 'reality' there is – as our experience testifies – no such splitting of observer states, so that only one branch can ever actually exist. Since this point may occur to other readers the following is offered in explanation. The whole issue of the transition from 'possible' to 'actual' is taken care of in the theory in a very simple way – there is no such transition, nor is such a transition necessary for the theory to be in accord with our experience. From the viewpoint of the theory *all* elements of a superposition (all 'branches') are 'actual,' none any more 'real' than the rest. It is unnecessary to suppose that all but one are somehow destroyed, since all the separate elements of a superposition individually obey the wave equation with complete indifference to the presence or absence ('actuality' or not) of any other elements. This total lack of effect of one branch on another also implies that no observer will ever be aware of any 'splitting' process.
>
> Arguments that the world picture presented by this theory is contradicted by experience, because we are unaware of any branching process, are like the criticism of the Copernican theory that the mobility of the earth as a real physical fact is incompatible with the common sense interpretation of nature because we feel no such motion. In both cases the argument fails when it is shown that the theory itself predicts that our experience will be what it in fact is. (In the Copernican case the addition of Newtonian physics was required to be able to show that the earth's inhabitants would be unaware of any motion of the earth.)"

Although we can find some empirical issues around the edges, for example, locality, the pragmatic point remains essentially untouched, that all of these interpretations are working off of the same empirical data, that is, the same experimental facts, and all give essentially the same operational interpretation of those facts. What is different is the conceptual or metaphysical extension beyond the experimental data of these widely varying views from de Broglie-Bohm to stochastic mechanics (non-Markovian) and the many-worlds of Everett.

And what is the real pragmatic story in physics? Namely that these different interpretations have little, if any, impact on the main business of experimental physics. Just as in the case of Greek, Chinese or Renaissance astronomy, the past views of cosmology and the ontology of the skies had little, if any, effect on the continued accumulation of new data or mathematical models. The same is true in quantum mechanics. The many interpretations of quantum mechanics have little impact on the continuing wide variety of new nonrelativistic quantum experiments, ranging from superconductors to isolated atoms. The present turmoil in physics is outside this framework. It is to be found in the relativistic phenomena of quantum electrodynamics and quantum field theory. It will be surprising indeed if any one of the many interpretations mentioned here is found to be the correct one as an extension of the standard nonrelativistic theory. Moreover, it is part of the irony of this tale that most of the interpretations just described do a rather poor job of handling relativistic phenomena. Certainly this is true of the de Broglie-Bohm theory or of stochastic mechanics. It is what makes these interpretations seem like fables from the past, intriguing tales, often of great intellectual interest, but pragmatically not very relevant.

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