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1

The Physicists' Philosophy

THE PHYSICISTS' working philosophy as I see it practiced is defined by the four brief statements in section 1.1. I mean these statements to be complete but confined to the essential points, and like any pronouncement, they require explanations illustrated by worked examples. This is a lot to think about; it takes up the rest of this book. I learned some of it from real philosophers but certainly do not intend to instruct them on their subject. This book is about a particular craft, physics: why we are confident we have learned a lot about what we take to be objective physical reality, and why there still are so many things we would like know.

1.1 The Working Philosophy

The starting idea in all the natural sciences is that the world operates by rules and regularities that can be discovered in useful approximations from descriptions, observations, and experiments. In physics the idea is narrowed to four working statements or assumptions.

- A: The world operates by rules or laws or regularities and the logic of their applications, and the rules and logic can be discovered in successive approximations by consultation of Nature.

- B: A useful approximation to the rules, a useful concept or theory, yields predictions that are reliably computed or described and found to agree with reliable measurements or observations, within the uncertainties of the predictions and measurements.
- C: Physics is growing more complete by discoveries of theories that we take to be fundamental improvements because their predictions better agree with a broader range of empirical evidence, and by occasional unifications that demote well-tested physical theories to good approximations to still more broadly applicable and testable fundamental theories.
- D: Research in fundamental physical science is advancing toward a unique, mind-independent, physical reality that never will be completely known. Nature is too complicated for that.

These assumptions define what we do in physics. The historian of science Helge Kragh (2011, p. 361) offers a useful summary statement:

Although there is no invariant definition of science, there are certain standards of evaluation that are relatively stable and shared by almost all physical scientists. These standards are not, of course, divinely given but the result of a long historical and social process with roots stretching back to the scientific revolution or even further. They have changed over time and are likely to continue to change. On the other hand, the stabilized scientific standards are neither arbitrary nor merely social constructs.

Like all rules of science and society, the four assumptions that define physics require judgments of intent and policies for ambiguities and omissions, all informed by the experience of which interpretations work best. I draw lessons about this from the history of physics since Isaac Newton gave us a systematic theory

of mechanics and gravity and James Clerk Maxwell unified the classical theories of electricity and magnetism. More is to be learned from still earlier history, but I am going to offer only occasional thoughts about it. This still leaves a lot of considerations to be reviewed. I begin in this section with clarifications of some questions that immediately arise and then go on to details in the following sections. This can get lengthy, but I think interesting if you would like to see what physics is all about and what distinguishes it from the other natural sciences.

To begin I have to explain what I mean by “physics.” We have two fundamental physics—quantum theory and general relativity—with many branches, from thermodynamics to galaxy formation. But all these branches follow from the fundamental theories, and by “physics” I mean the fundamental physics and all that we can demonstrate follows from it. Chemistry is the closest relative; the difference is in Assumption C. The theory of quantum physics has been established by demonstrations that it passes demanding tests and applications to a broad variety of phenomena, though of course these demonstrations had to have been simple enough for analyses from first principles that produced predictions that can be reliably checked by experimental or observational tests. Chemistry is thought to follow from quantum physics, but chemistry deals with phenomena that are too complicated for analyses from first principles. This distinguishes chemistry from physics. Neither is superior to the other; you do what you can do in physics, or chemistry, or whatever subject you choose. And bear in mind that if physics were our only science, then science and society would be in a sorry state. This is why we have chemistry, which carries on from the physicists’ analyses of the fundamental properties of matter to how chemicals actually behave in real situations, and it is why we have botany and biology, geology and astronomy. You might wonder about the place of biophysics in this picture. Biophysics is good science, but it is biology, not physics, because it is too complicated to be reduced to physics. Categories that could be termed “hyphenated physics”—physical

chemistry, astrophysics, geophysics, biophysics—are discussed further in section 1.14. Apart from this I mean to reduce the possibility of confusion by not mentioning these terms anywhere else in this book.

The physical world that Nature offers us does lead us to put physics in a special place. We expect that what is known about chemistry could be derived from physics if only chemistry did not deal with such complicated situations. As one says, we expect that chemistry is emergent from physics. And we expect biology also is emergent from physics, a thought reenforced by the fact that living matter is composed of atoms and molecules that behave as expected from physics but in ways that are far too complicated for analysis of the behavior of living matter from the first principles of physics. We must rely on biologists to work that out as best can be done. In contrast to this we have no evidence that physics is emergent from some deeper science. Atoms are evidence that biology is emergent from physics, and one might argue that the quarks and gluons of particle physics could be taken as analogs of atoms, traces of the deeper theory from which physics emerged. The difference is that quarks and gluons act as true elementary entities whose behavior is sharply defined by the theory. Our physical theories are works in progress, and we expect ongoing research will lead to still better theories, a better fundamental basis for science. But as far as we can tell, it still will be the theory of physics on which we trace the emergence of chemistry and on up to biology.

Comments I have received about drafts of this book include concern about my stating that physical reality is an assumption. I have been asked: Isn't the meaning of reality obvious? But bear in mind that we have not been issued a guarantee that Nature operates by objective rules we can discover; this has to be an assumption. It certainly seems natural. I suppose any scientist celebrates a discovery, something to add to our knowledge, as a contribution to what we know about the nature of the world around us. That of course assumes there is a nature of the world to be discovered. I do not mean to express doubt about it; experience

certainly argues for it. A related concern is that by stating that reality is an assumption I might be taken to admit the possibility of a different assumption, maybe that we are only the subjects of a dream. I see no basis for this counter-assumption and lots of evidence for the physical reality that natural scientists aim to discover, with great success that supports the assumption. But we have to remember that this concept of physical reality is an assumption, entered as Assumption D.

I have been advised that my four statements/assumptions of the philosophy of physics can be termed “axioms,” or “beliefs,” or “faiths.” All make sense, but the connotations of confidence feel too strong. The choice of words must balance the compelling evidence that our established laws of physics are excellent approximations to many aspects of what is observed, while indicating that the physics nevertheless is incomplete because the theories we have cannot account for all questions one can pose. I prefer the phrases “working assumptions” or “working hypotheses” because the statements are subject to adjustment if the empirical evidence requires it.

Correspondents also have wondered why the four assumptions do not indicate much about how physical theories are discovered. It is because my topic is the framework, or guidelines, or philosophy of curiosity-driven scientific research in the physics that is defined by the above four statements. I argue that this is the philosophy that is implicit in what we find ourselves doing and that works in research in our two fundamental physical theories: quantum physics for small scales and the general relativity theory of the evolving universe for large scales. How we arrive at these theories is a capricious process that cannot be so tightly defined.

I have been asked to explain what is meant by “approximations” and “successive approximations” in physics. A theory that passes tests of predictions is a useful approximation. A theory that passes some tests of predictions and fails others can be a useful approximation too; a good example is the classical theory of electricity and magnetism. It is a really useful approximation, as you experience every time you turn on an electric light, but it fails

when you apply it to atoms. Quantum theory does better; it includes the classical theory as a limiting case, and it produces many new predictions including the behavior of atoms, all of which pass tests. It is an approximation that is better than, successive to, the classical theory. (I might add that this usage has nothing to do with successive approximations in mathematics.)

I also have been asked why I insist that established physics cannot be complete. Recall the concept of “just so stories” in physics, named after Rudyard Kipling and the “theory” of how the leopard got its spots. A clever theorist might be able to invent a theory that accounts for given things, but how do you judge whether the theory is only a just so story cleverly invented to fit the facts? We can’t blame the theorists who are only doing their jobs, but we must check them. We need empirical evidence, which can never be complete. It means physics can never be complete, only the best that can be done.

1.2 Rules

A good deal more about the practice of physics is to be considered, and a good place to begin is with the starting thought in Assumption A, that the world operates by rules we can discover. This is implicit in natural scientists’ expectation that the results of their research are adding to what is known about the behavior of our world. It seems perfectly natural, for how could life form and how could the fittest survive if Nature did not have reliable rules for what is required to be fit? In short, natural scientists expect that there are real things to discover and try to interpret in terms of rules and regularities that can be found. And it must be done in successive approximations as we continue to learn how to find still better theories.

It is often said that “mathematics is the language of science” to be used for the expression of these rules. We have not been issued a guarantee that this is so, that there is a language of science, and if so that it is mathematics, but experience certainly encourages

the thought. In Eugene Wigner's (1960) essay "The Unreasonable Effectiveness of Mathematics in the Natural Sciences," Wigner pointed to Galileo Galilei's thinking in the 1623 book, *The Assayer* that (I quote from the English translation by Stillman Drake, 1957, pp. 237–238):

Philosophy [natural philosophy, or science] is written in this grand book, the universe, which stands continually open to our gaze. But the book cannot be understood unless one first learns to comprehend the language and read the letters in which it is composed. It is written in the language of mathematics, and its characters are triangles, circles, and other geometric figures without which it is humanly impossible to understand a single word of it; without these, one wanders about in a dark labyrinth.

The statement is accurate but incomplete for the science of the time, and it remains so. Galileo made important descriptive discoveries: the presence of mountains on our Moon and the existence of moons moving around the planet Jupiter. Michael Faraday made great discoveries about the properties of electricity and magnetism without the use of mathematics much beyond arithmetic. Edwin Hubble convinced many of the case that the universe is expanding by his analysis of observations that indicate the galaxies are moving away from us in the way expected in an expanding universe. The evidence in the figure in Hubble (1929) was qualitative, but it seemed persuasive at the time.

But mathematics allowed great discoveries. Maxwell added the hypothetical displacement current to arrive at a mathematical theory that unifies two subjects, electricity and magnetism, in the theory of electromagnetism, which has given us so many practical applications. I suppose mathematics will continue to be the language of physics, but we don't know it, and if so we cannot anticipate what kind of mathematics, what dialect, will be applied in the future. It seems best to use the more generic term "rule" rather than "theory" in Assumption A.

The Ptolemaic system for keeping track of the motions of the planets across the sky can be termed a mathematical theory, one that produces quite accurate predictions of the angular positions of the planets. The same is true of Copernicus's improvement that placed the Sun near the center of the world. But both theories are limited; they say nothing about how an apple falls to the ground. The Newtonian theory takes care of falling apples, the motions of the moons of Earth and Jupiter, the precession of Foucault's pendulum, the flow of water in tides and air in tornados, and so much more. This mathematical theory is a far better approximation to reality than the Ptolemaic system. Maxwell wrote down the equations of electromagnetism in the mid-1800s. The notation has been improved, but the theoretical predictions remain perfectly clear and consistent with everyday experience. But Maxwell's electromagnetism is an approximation to quantum electrodynamics, which is a limiting case of the electroweak theory that physicists are hoping will be unified with the rest of quantum physics. This is what might be expected in a universe that operates by rules we can discover, but in steps, successive approximations, as we learn to do better.

To avoid possible confusion I should repeat that the "logic" in Assumption A is to be defined empirically, by the way Nature is observed to behave, under the assumption that there is a logic of some sort to be discovered in Nature's behavior. So how do we go about checking ideas about Nature's laws and logic? This is discussed next.

1.3 Inductive and Deductive Modes of Research

Earman and Roberts (1999) made the cautionary point that

the concept of a law of nature seems to us to be an important one for understanding what physics is up to, but it is a misguided egalitarianism that insists that what goes for physics goes for all the sciences.

Their point is illustrated by the difference between the mode of research dominant in testing and establishing physical theories and the mode commonly employed in the other natural sciences. The two are defined in a way suitable for the practices of the natural sciences as

1. bottom-up \simeq deductive: compute predictions of a theory that seems to be a useful approximation to reality and compare the predictions to what is observed;
2. top-down \simeq inductive: seek clues to a theory that might be a useful approximation by inference from what is observed.

The approximately equal sign is meant to indicate that the name I adopt, the first in each definition, has meaning similar but not identical to the commonly used name.

The philosopher John Norton (2023) presents a careful study of the philosophers' use of induction and the contrast to deduction. These names are used in many disciplines, however, and the result has been that definitions to be found on the Web are more finely argued. An example is the possibility of confusion with mathematical induction. Another is Smith's (2024) report that Isaac Newton stated in the *Principia* that

“in this experimental philosophy, propositions are deduced from the phenomena and made general by induction.”

I do not know the context and possible issues of translation of this statement; I offer it only to illustrate the possibility of confusion. I take it that Newton's “propositions” are theories, and the word “deduced” is meant to be the top-down mode of consultation of phenomena that serve as guides to the discovery of physical theories that promise to be useful approximations to reality. It is more usual now to consider a deduction to be an inference from a theory, a bottom-up prediction we might hope to be able to test. And Newton's use of the word “induction” seems to mean the bottom-up mode of assessment of a proposition by the success or failure of its predictions, while “induction” now is more

commonly considered to be the search of evidence from the observations. And maybe the word “general” means that the theory might prove to account for many phenomena, which is our best signature of a very good approximation. This interpretation of what Newton meant is not guaranteed of course; it is meant to illustrate why specially defined terms such as those adopted here might help reduce confusion.

In my terminology the word “bottom” is meant to connote the best approximation we have to the fundamental physics of reality, the theory that serves as the basis for the interpretation of phenomena because it passes tests of predictions in a broad variety of applications. That is subject to change if new empirical evidence requires it, of course. The word “up” is meant to refer to the phenomena, the empirical evidence that natural science is all about, and “top-down” has about the same connotation as Newton’s phrase, “propositions are deduced from phenomena.” In practice the creation of a physical proposition, or theory, tends to be a much more capricious mix of shrewd interpretations of evidence and lucky guesses about what is good and bad about theories to be replaced, a dynamical back and forth between considerations of evidence and ideas.

My definitions of the two modes are meant to align with the practice of natural science in general and physics in particular. My choice of names, which might seem inelegant, is a matter of taste. The key point is that physics is defined by its reliance on establishing theories by the practice I have labeled “bottom-up,” while biology usually operates in the “top-down” mode because the objects of enquiry in this subject tend to be far too complicated for predictions from first principles.

Physical theories are created in the top-down, we might say inductive, mode of consideration of empirical evidence, maybe along with attempts to remedy challenges to earlier theories, along with scientists’ experience of what worked in analogous situations, coupled with intuition and creative and maybe lucky guesses. You might say that anything goes. The judgment call required in the

top-down mode is whether an inference from evidence is likely to be on the right track or maybe is only a clever invention of a story that fits the given facts. As one says, an inference from evidence might only be a just so story.

Physical theories are tested in the bottom-up mode defined as the derivation of reasonably unambiguous predictions that can be compared to reasonably reliable observations or measurements. The test is ambiguous because the agreement of a measurement with a prediction might only be accidental. We see in the examples to be presented in this section and those discussed at greater length in chapters 2 and 3 the usual ambiguity of judgment of uncertainties in predictions and measurements and the argument that the ambiguity is seriously reduced by establishing many confrontations of theory and practice, particularly when referring to physically different situations.

In more formal terms the argument that a single successful prediction establishes a proposition is termed the “fallacy of affirming the consequent,” a mouthful. In natural science the analog is that you usually can't trust a single agreement of prediction and observation because the agreement might only be accidental. That problem is increased by the fact that empirical evidence cannot be arbitrarily accurate; you have to judge whether theory and observation are consistent within reasonable estimates of uncertainties in the measurement and the prediction. But there can be critical measurements that make a convincing scientific case. An example is the detection of the antineutrino discussed in section 2.5. A successful theory of nuclear reactions involving the creation and annihilation of electrons required the postulate of the existence of neutrinos and antineutrinos, and the theory predicted that the antineutrinos coming from a nuclear reactor could be detected. The critical question became: Would antineutrinos be detected? The actual detection settled this test of the theory.

More usually we assert that we have a convincing case for a theory that passes the tests of many predictions that agree with the evidence. Why else would the theory pass so many tests? If

the number of accidental agreements this would require is not at all plausible, then physicists take it that the case for the theory is seriously plausible.

Physical theories tend to have parameters that can be adjusted to fit the data. If an adjustment of a parameter improves the fit to the data, the result is encouraging but not necessarily a serious check; maybe it's only an improvement of a just so story. But if the value of the parameter that best fits the data agrees with independent ways to measure the parameter, it is serious positive evidence for the theory. Again, why else would independent means of measuring the parameter yield consistent results? Maybe only accidental? Try to find more tests. And one must resort to judgment calls; it is required in natural science as in all human activity.

There are rules in chemistry and biology, geology and astronomy, and the other natural sciences that successfully predict results of observations or experiments in the bottom-up mode. An important example is genetics. The difference from physics is that the standard and accepted physical theories successfully predict definite properties of broad varieties of phenomena. It is difficult to imagine finding such broad predictability in biology because biological phenomena are complicated, the results of the effects of many actors that formed by the eventualities of chemistry and happened to be adaptable to use in environments that support living matter. It does not seem at all likely that all this can be summarized in a theory similar to Newtonian mechanics. Physical theories account for broad varieties of phenomena because the phenomena are not determined by the vagaries of behavior of actors in the way that the behavior of atoms confuses predictability in biology.

An example that illustrates the roles of the two modes of treatment of theory and data is Niels Bohr's (1925) fascinating list of empirical hints to the quantum nature of atomic physics. They were influential in the creation of the two versions of quantum mechanics that appeared a few years later, the matrix and wave

theories that were soon seen to be equivalent. When quantum mechanics was created, it was seen to account for so many already known hints that the theory was accepted with little question. The short summary is that although quantum mechanics is not intuitively evident from our experience of the world the abundant top-down evidence led to the creation of the quantum theory that continues to pass enormous varieties of bottom-up tests.

A second example of the top-down and bottom-up modes is seen in Einstein's discovery of the general theory of relativity, which is carefully examined by Gutfreund, Renn, and Stachel (2015). A brief summary is that Einstein began with recognition of the evidence that the gravitational accelerations of small test particles are very nearly the same independent of the composition of the particle. Einstein's great creative thought was that this is what would be expected if the test particles were moving freely through curved spacetime. In this picture I am accelerated by the force of the chair I am sitting on, a force produced by our Earth. General relativity was in part a top-down discovery, this empirical evidence that the motions of free test particles are independent of composition, which offered an interesting and possibly useful hint to a theory. But in larger part it was a result of Einstein's remarkably creative intuition and hard work. It took him ten years from the starting ideas to completion of the theory, and it took experimentalists a half century after that for advances in technology to make possible the serious bottom-up checks of predictions of the theory summarized in section 3.1 that at last established this theory as a well-tested part of fundamental physics.

It might be appropriate to have a category for singularly important creativity such as Einstein's discovery of general relativity. Charles Sanders Peirce (1934, p. 106) recommended a third mode:

Abduction is the process of forming an explanatory hypothesis. It is the only logical operation which introduces any new idea; for induction does nothing but determine a value, and

deduction merely evolves the necessary consequences of a pure hypothesis.

The words “abduct” and “abduction” have been taken for other purposes, however, and since many years might pass before the significance of an idea can be determined, simplicity suggests it is best to leave it as the two categories in the ways data and theories are used.

Research in chemistry is largely top-down. Baird, Scerri, and McIntyre (2006) put it that

chemistry does sit right next to physics, with all its lovely unifying and foundational theory. Squinting our eyes up tight, it is possible to see chemistry as complicated applied physics. Even in denial, we say we are materialists, but the material world of our denial is the foundational world of physical theory, and so chemistry—in principle anyway—must be reducible to physics. But this has never been much more than an article of faith.

Physicists tend to take it without question that chemistry could be derived from standard quantum theory, if only chemistry were not so complicated. There is some justification. Chemists deal with molecular hydrogen, a molecule that is simple enough for a close to first principles quantum computation of its properties starting with the binding energy, the work required to pull apart a hydrogen molecule into two hydrogen atoms at rest. The computations are in reasonable agreement with the measurements; physics passes a bottom-up test.

If quantum physics accounts for molecular hydrogen, then surely it accounts for more complicated molecules; and there is something to this. The behavior of the ammonia molecule is important to physics and chemistry. The ground level is a quantum state in which the nitrogen atom is on both sides of the triangle of three hydrogen atoms. This is contrary to usual logic, a consequence of the quantum physics to be discussed in section 1.5. In

the first excited level of the molecule, the nitrogen atom is in effect oscillating, quantum tunneling, between positions on either side of the hydrogen triangle. The energy released by the transition from this inversion oscillation to the ground level was the source of microwave radiation in early generations of atomic clocks.

The chemist Sharon Hammes-Schiffer (2009) describes the role of the analog of this nitrogen inversion oscillation for the transport of electrons, protons, and hydrogen atoms in large molecules. She writes that these “proton-coupled electron transfer reactions . . . play an essential role in photosynthesis and respiration, as well as in many types of solar fuel cells and electrochemical devices.” In a private communication Hammes-Schiffer adds that “this is a good example of how fundamental principles of physics, such as tunneling, can play a critical role in a wide range of chemical and biological systems.” This illustrates how physics can be an important aide to the necessarily top-down approach to the complexity of chemistry and on to biology.

In the paper “Ambitions for Theory in the Physics of Life,” the physicist Bill Bialek (2024) shows how the methods of theoretical and experimental physics are usefully applied to biology. Bialek argues that

the view of biological systems as intrinsically messy would mean that there simply is no path to build an understanding of life that parallels the theoretical physicists' understanding of the inanimate world. It is not just that we would need new principles, which would be welcome, but that we would have to retreat from what we mean by “understand.” This all has changed dramatically. There has been an explosion of opportunities for physics experiments on the functional behavior of living systems, across all scales from molecules to ecosystems. I hope we can banish forever the prejudice that life is a mess. [Bialek includes a footnote here, saying, “Certain aspects of life, of course, will remain messy, and delightfully so.”] As data improve, we should ask more from our theories.

The curiosity-driven research in complicated systems exemplified by what Hammes-Schiffer and Bialek are doing is as intellectually demanding as bottom-up physics and more important to the immediate needs of society. The big difference from physics is that the science of biological systems must be top-down because biology is far too complicated for the physicists' bottom-up approach from first principles. Bialek remarks that a fruit fly egg "takes just twenty-four hours to go from one cell to a larva (maggot) that emerges from the egg shell and walks away, ready to navigate the world." Imagine trying to construct the mixed quantum state of a fruit fly larva, taking proper account of the classical and quantum entangled interactions of the larva with the local environment and the outside world. Surely it cannot be done.

Hammes-Schiffer and Bialek show how the methods of physics can be applied to complicated situations in chemistry and biology. As Bialek says, we should ask more of our theories. So shall we consider these lines of research to be physics? The answer matters only for the sake of consistent terminology, but for this purpose I take the enterprise of physics to be the establishment of theories in situations simple enough to allow reliable bottom-up tests of reliable theoretical predictions; and I take this to be a reasonably clear operational definition of physics. It is natural and important to seek to apply the successful methods of physics to complicated situations, but in the philosophy expressed in this book these applications are the province of research in chemistry, or geology, or on up in complexity to Bialek's discussion of the physics of life. As advances in technology and theory aid evolution of research in the natural sciences, the effective boundaries of its branches can evolve, and with it the appropriate terminology. But for the purpose of this book it is best to keep to the definition of physics presented in section 1.1 and to let the term "the methods of physics" signify the applications of this technology and ways of thinking to the study of the many complicated situations to be examined in the necessarily top-down way of exploring our complicated world.

1.4 Physics Will Remain Incomplete

Why do I insist that the physicists' best fundamental theories are incomplete, approximations? Why do the succinct statements of the physicists' working philosophy presented at the beginning of this chapter require room for a concept that is so important to science: that we must deal with theories that are at best incomplete? It follows if we accept, as I argue we must, that assessments of physical theories depend on empirical evidence—what is observed in one manner or another—and empirical evidence never can be complete and exact. What is more, physical theories are discovered top-down from empirical evidence and the inspired lucky guesses that are intended to fill in the gaps in the evidence. The freedom to design these theories, from the grand plan down to the finest adjustments of the most minor details, is effectively limitless, and the considerable difference between what empirical evidence can provide and what imaginative theorists can devise means that physical theories cannot be unambiguously derived from empirical knowledge. The argument against reductionism of the kind that is envisioned in these remarks is continued in section 1.10. John Norton (2018) presents a more formal demonstration of the impossibility of deriving a complete physical theory from experience.

This line of argument against reductionist thinking was not so clear in the 1920s and early 1930s when physicists and philosophers sought to bring physics to a rigorously logical system, maybe something close to pure thought, just as mathematics was thought to follow from pure thought. Philosophers and scientists gathered at the Vienna and Berlin Circles for the purpose of seeking how to reduce knowledge of Nature to this sort of firm and well-defined system. The philosopher Rudolf Carnap is remembered for his important contributions to philosophy and for his role in the search for a satisfactory logical philosophy of natural science. In his book *The Logical Syntax of Language* (in the English translation in Carnap 1937, p. 258) Carnap argued that

in order to determine whether or not one sentence is a consequence of another, no reference need be made to the meaning of the sentences. The mere statement of the truth-values is certainly too little; but the statement of the meaning is, on the other hand, too much. It is sufficient that the syntactical design of the sentences be given.

According to the *Stanford Encyclopedia of Philosophy* (Leitgeb and Carius 2024, in the supplement to Carnap on *The Logical Syntax of Language*),

The methods by which he [Carnap] aimed to carry out this program are mainly taken from the metamathematics of mathematical theories, such as Kurt Gödel's arithmetization of the syntax of mathematical theories.

Carnap argued in the final section of the *The Logical Syntax of Language* that

perhaps we may say that the researches of non-metaphysical philosophy, and especially those of the logic of science of the last decades, have all, at bottom, been syntactical researches, although unconsciously. . . . The use of this instrument [syntactical terms] for dealing with the numerous and urgent contemporary problems of the logic of science, and the improvement of it which will follow from its use, demands the cooperation of many minds.

I take the thinking to be that, just as mathematics was supposed to be reducible to a fully complete and logical system, it should be possible to reduce the physical character of Nature to a system of rigorous logical statements. It is an intuitive dream that is not possible if you accept that empirical evidence is necessary to avoid just so stories.

In the early 1930s Kurt Friedrich Gödel introduced methods of mathematical logic that showed that any system of mathematics that is complicated enough to be interesting contains theorems that cannot be proved. Kennedy (2020) put it that

the Second Incompleteness Theorem shows that the consistency of arithmetic cannot be proved in arithmetic itself.

This was disconcerting to mathematicians, and I expect it was equally disconcerting to the philosophers and physicists who took part in the discussions in the Vienna and Berlin Circles that sought to find an internally consistent and self-evident rational physical science. Gödel's result is not directly relevant to natural science as it is now understood, of course, because physics is grounded in the necessarily even greater limitation of empirical evidence, but Gödel's result was a helpful cautionary example of a mistaken way of thinking about what can be accomplished in mathematics and in the natural sciences. I put the lesson to be that, just as mathematics is too rich ever to be complete, the nature of physical reality is too rich in another sense ever to be reduced to theorems. Physical theories can be persuasively established as useful approximations to reality by the successful bottom-up tests of predictions, and theories can be rejected for failure to produce sufficient numbers of redeeming successful predictions; but theories cannot be proved to be unambiguous interpretations of reality if we accept that theories must be judged by the evidence from Nature.

By the 1930s we see evidence of the recognition that natural science cannot be reduced to a logically complete closed system. Hans Reichenbach (1938), an important participant in the Berlin Circle, wrote:

The idea that knowledge is an approximative system which will never become "true" has been acknowledged by almost all writers of the empiricist group; but never have the logical consequences of this idea been sufficiently realized. The approximative character of science has been considered as a necessary evil, unavoidable for all practical knowledge, but not to be counted among the essential features of knowledge.

Reichenbach's statement agrees with the physicists' position, provided that the word "true" is meant to mean exact, as in a mathematical theorem, and "the essential features of knowledge"

are not taken to be something we will ever know for sure. As long as physical theories are established by bottom-up tests, there cannot be a final theory, only the best we can do. In my experience this is no longer considered a necessary evil, only a part of the working conditions for all the natural sciences.

In more recent literature of philosophy the point is expressed by the Duhem–Quine thesis, which for the present purpose can be taken to be that it is not possible to prove that our canonical physical theories are the only way to fit the empirical evidence we have as well as the evidence we have not yet thought to seek. The American philosopher of science Norwood Russell Hanson (1961, p. 36) pointed out a consequence that is conceivable, at least in principle:

Given the *same* world, it might have been construed differently. We might have spoken of it, thought of it, perceived it differently. Perhaps facts are somehow moulded by the logical forms of the fact-stating language. Perhaps these provide a “mould” in terms of which the world coagulates for us in definite ways.

Thomas Kuhn (1970b, p. 20) put it that

scientific theories, it must be remembered, attach to nature only here and there. Are the interstices between those points of attachment perhaps now larger and more numerous than ever before?

One might imagine that Kuhn’s interstices are broad enough to allow Hanson’s proposal.

This way of thinking invites the thought, as Norton (2008) put it, that

The undermining thesis is all available empirical evidence cannot unambiguously determine the scientific theory that accounts for the evidence. Thus it is often argued that our agreement on the content of mature scientific theories must be due to social and other factors. In this chapter, I will draw on

a long-standing tradition of criticism to argue that the under-termination thesis is little more than speculation based on an impoverished account of induction.

Norton presents a clearly argued justification of this statement from a philosopher's point of view. It agrees with the physicists' philosophy.

Physicists should not too strongly condemn the social factors that Hanson and Kuhn mention, however, because from 1915 to 1965 the physics community accepted Einstein's general theory of relativity for its elegance without community recognition of its scant empirical support. (The situation is reviewed in sec. 3.1.) This aberration has been remedied by advances in technology since then that enabled checks of general relativity by bottom-up tests of predictions that make a compelling case for this theory. It is to be noted, however, that we have two physics, general relativity for large scales and quantum field theory for small scales, both of which pass demanding tests, but the two theories are not compatible. The situation is discussed in chapter 4. Might some more modest version of Hanson's thought of a sociological block in our thinking be exemplified by the need for a more creative way to reconcile our two physics? Maybe a scenario based on strings will lead to a theory that unifies the two physics, or maybe unification will be found by recognition of some block in our thinking, or perhaps physicists will have to learn to live with two theories. It is good practice to keep an open mind about such things while bearing in mind that both of our fundamental physical theories pass many tests that seriously restrict options for any considerable adjustment of standard and accepted theories and our ways of thinking about them.

This is a good place to pause to consider the limitation to the bottom-up tests of physics that are so valued by the physics community. Precise bottom-up tests require situations that allow accurate computations of predictions that can be checked by reliable experiments or observations. Almost all observations of

Nature do not allow this because the situations are too complicated for reliable first principles computations and reliable tests. An aspect of this situation is pointed out by the philosopher Nancy Cartwright (1983) in her book *How the Laws of Physics Lie*:

To explain a phenomenon is to find a model that fits it into the basic framework of the theory and that thus allows us to derive analogues for the messy and complicated phenomenological laws which are true of it. The models serve a variety of purposes, and individual models are to be judged according to how well they serve the purpose at hand. In each case we aim to “see” the phenomenon through the mathematical framework of the theory, but for different problems there are different emphases. . . . Is a helium-neon laser really a van der Pol oscillator? Well, it is really a mix of helium and neon atoms, in about the ratio nine to one, enclosed in a cavity with smooth walls and reflecting mirrors at both ends, and hooked up to a device to pump the neon atoms into their excited state.

A gentler way to put what Cartwright might have been thinking is that theoretical physics cannot describe the whole truth. An appropriate aphorism for this situation (I don’t remember who said it) is that you can learn theoretical physics from books, but you cannot learn experimental physics from books.

A physical theory almost always is specific enough that it can be communicated by explanations in a book, but experimental physics has aspects of a craft that must be learned by hands-on experience of what does and does not work in an experiment or observation. What is more, experimentalists must deal with physical theories that are incomplete, approximations meant to describe idealized situations. Great care might be taken to isolate an atom whose properties are to be measured for fair tests of predictions of quantum physics, but the isolation cannot be complete because after the atom is totally isolated from any significant physical contact it remains quantum entangled with the rest of

the world. The effect is tiny to nonexistent but physically real according to well-checked quantum physics. Physicists take it that an atom in a really complicated situation such as living matter also is well described by quantum physics after due correction for the environment, and indeed nuclear magnetic resonance imaging measurements can detect atoms in biological matter and check that they have the properties that are expected given the precision of the predictions for atoms *in vivo*.

Physicists expect the structure of a biological molecule could be computed from quantum physics, in principle, but in practice the molecule contains far too many atoms for an accurate computation starting from quantum physics. Organic chemists can give a good account of how the atoms are connected, but the far more difficult if not impossible task is to check that quantum physics accounts for the properties of the molecule. This is a loose end of physics that cannot be eliminated but is being made less serious by advances in technology and techniques for more accurate though of course still approximate computations of properties of atoms and molecules.

We see in these considerations opposing thoughts, both of which we must reject, and a middle way that seems to work. One thought is that physics can be reduced to a rigorously logical, mind-independent system. The opposing idea is that physics is a social construction with at best modest contact with objective reality, if there is such a thing as reality. The former was a beautiful dream that had to be abandoned as impossible. One still encounters arguments for the latter, but to my mind it is convincingly rejected by the philosophy that evidence from observations of Nature can reveal which theories are useful approximations and which are not. This process of examination of the many tests of small-scale physical theory is sampled in chapter 2 and the tests of large-scale physics is sampled in chapter 3. And we must remember everyday experiences of applications of physical science. Consider for example the conversion of the mechanical energy drawn from a waterfall or steam pressure to the electromagnetic

energy that travels along transmission lines and can be directed to your residence where it can make the doorbell ring and so much more, all drawn from Maxwell's theory of electromagnetism. You really do not want to do without the technology that has grown out of physics and the rest of the natural sciences. In short, research in science takes the middle way between the opposing thoughts of absolute truth and social construction. It means the physicists' philosophy must accommodate the dichotomy that aspects of our standard physical theories are well and persuasively checked and established as useful approximations to reality while others are at best understood by crude and maybe only hopeful approximations.

These limitations of the practice of physics can be difficult to explain to a nonscientist. A skeptic might ask, in the vernacular, "If you're so smart why can't you tell me what your cosmological dark matter is?" We can only answer that the evidence of its existence seems compelling while acknowledging that serious efforts to detect this hypothetical matter have failed so far. If this answer seems superficial, bear in mind that the issue is to be settled by the consultation of Nature. If dark matter interacts with the rest of the world only by its gravity, which means we cannot check the hypothesis by the direct detection of dark matter particles, assuming it is particles, then this is the way it is. We still have the indirect evidence from the success of the cosmological tests that require this dark matter and in time might yield clues to a better approximation to its properties.

I have not found it to easy to explain in lectures to the general public why puzzles in the natural sciences, including the simple questions we cannot answer, do not contradict the claim of firmly tested and established scientific results. It is an important task for natural scientists and philosophers and sociologists who understand what is happening in physics. Thoughts along these lines are expressed in the book *Doubt and Certainty*, by the physicists Tony Rothman and George Sudarshan (1998).

1.5 Quantum Physics

Quantum physics taught us a cautionary lesson about the logic mentioned in Assumption A in chapter 1. Our intuition about the way things behave has been formed by personal experience and what we inherited from times past that gave us a confident feeling of what is logical and sensible. We do not directly experience quantum physics, or the curved spacetime of relativity, and in retrospect it should not be considered surprising that logic on these two scales differs from what we experience. Nature must teach us the rules by which it operates, and maybe there still are lessons we must learn. It is why Assumption A in section 1.1 is cautiously phrased.

How do we reconcile the logic we inherited with the logic of quantum physics? I offer examples of the evolution of thinking about this question in the century since quantum mechanics was discovered and then present what I take to be the reasonably satisfactory present thinking.

An electron acts like an ordinary particle in many ways, but in standard and well-tested quantum physics an electron can be placed in a pure quantum state of two spatially separated wave packets—an entangled state—as in a double-slit experiment. It seems logical to insist that the electron, being a particle, really is in just one of the two wave packets, but that is not what quantum mechanics indicates. An early example of ways to think about this is presented by Heisenberg (1958) in the book *Physics and Philosophy*:

Any experiment in physics, whether it refers to the phenomena of daily life or to atomic events, is to be described in the terms of classical physics. The concepts of classical physics form the language by which we describe the arrangement of our experiments and state the results. We cannot and should not replace these concepts by any others. Still the application of these concepts is limited by the relations of uncertainty. We must keep in mind this limited range of applicability of the classical concepts

while using them, but we cannot and should not try to improve them.

In the book *Quantum Mechanics*, Landau and Lifshitz (1958) put it that

quantum mechanics occupies a very unusual place among physical theories: it contains classical mechanics as a limiting case, yet at the same time it requires this limiting case for its own formulation.

This is considered the standard Copenhagen interpretation. But we can do better.

The physicist John Stewart Bell (1976, 1981) was troubled by the Copenhagen line of thinking, as are many others. Bell stated that this interpretation does not have a

sharply defined boundary between what is to be treated as microscopic and what as macroscopic, and this introduces a basic vagueness into fundamental physical theory. But this vagueness, because of the immense difference of scale between the atomic level where quantum concepts are essential and the macroscopic level where classical concepts are adequate, is quantitatively insignificant in any situation hitherto envisaged. So, it is quite acceptable to many people.

The distinction between microscopic and macroscopic phenomena is clear but not so very pronounced. The electrons in the coherent state of a superconductor with macroscopic dimensions, maybe in a superconducting circuit a meter around, do not have definite positions or identities; they are in a stable coherent quantum state. Photons separated by a kilometer can remain in a quantum entangled state (e.g., Neumann et al. 2022). But Bell was right; it is troubling that in the Copenhagen interpretation we do not have a rule for the distinction between the two physical theories: classical for the observer, quantum for the object being observed. Who decides who or what the “observer” is?

Bell argued for the merits of a strictly classical world constructed on the postulate most fully developed by David Bohm (1952), that particles in quantum physics are particles in our usual sense, always at a definite position. The quantum wave function for the particle provides the construction of a pilot wave that determines where the particle is likely to be found, and physics hidden from us determines how the particle moves around in the pilot wave. But in a foundational essay, “Bertlmann’s Socks and the Nature of Reality,” Bell (1981) explained how to test between the nature of what standard quantum physics predicts and what would be predicted by a pilot wave theory that preserves our classical notions of logic and causality.

Bell’s argument, along with papers such as “The Emergence of Classical Properties Through Interaction with the Environment” by Joos and Zeh (1985) and *Decoherence and the Transition from Quantum to Classical* by Wojciech Zurek (1991) helped inspire Sidney Coleman’s (2020) memorable lecture, “Quantum Mechanics in Your Face,” on how to see that what Einstein is said to have termed “spooky action at a distance,” and the “collapse of the wave function,” are instead consequences of standard well-tested quantum physics.

For a simplified version of Coleman’s version of Bell’s argument, consider two electrons, one in each of two wave packets. When the wave packets were close together, the two electrons were placed in a pure quantum state with zero total spin. This means that if the spin of one electron is later found to be up in a chosen direction, then the spin of the other electron measured in the same direction will be found to be down. The two electrons are said to be entangled. Suppose that, before these measurements are made, the wave packets move apart, and at two spacetime events with spacelike separation the observers A and B measure the electron spins, up or down in a direction each chooses. Each observer remembers the chosen direction and the result of the measurement of the spin along that direction, up or down. The spacelike separation at the two measurement events

means that, if information cannot travel faster than light, the act of measurement by each observer cannot have any influence on what the other observer finds. The observers then move together again where they can communicate, compare notes. Given the direction A chose and the result, spin up or spin down, quantum physics predicts the probabilities for the measurement of spin components, up or down, along the direction B chose. To complete the experiment the procedure is repeated many times and the predicted probabilities checked against the average results of the measurements. Physicists Alain Aspect, John Clauser, and others conducted real experiments along this line that prove to be consistent with quantum physics and to disagree with the hidden-variables picture that is built on classical ideas about a reality of things.

The two measurements in this experiment are events in space-time with spacelike separation, meaning light cannot travel from one event to the other. And in quantum physics information does not travel faster than light. So how can the results of the two measurements conducted at spacelike separations be correlated? Our intuition cannot answer; we did not evolve to take account of quantum physics. The prediction of correlation of these spin measurements at spacelike separated events is not causal according to classical physics, but it is what quantum physics gives us, despite the fact that quantum physics does not allow transfer of information faster than the speed of light.

A related puzzle that troubled Bell and many others is that early authorities argued that we must work with two physical theories, one to apply to large beings like us, the other to apply to the quantum theory of small scales. When a state of a quantum system is measured, the state was said to discontinuously change to take account of the information gained about the state of the system. The measurement was thought to cause the collapse of the quantum state, and this collapse of the quantum state and the apparent acausality of the spin measurement experiment is what Einstein was said to have termed “spooky action at a distance.”

Coleman (2000) argued that the conceptual problem is not the interpretation of quantum mechanics. That's getting things just backwards. The problem is the interpretation of classical mechanics [in the terms of quantum physics]. . . . [P]eople say the reduction of the wave packet occurs because it looks like the reduction of the wave packet occurs, and that is indeed true. What I'm asking you in the second main part of this lecture is to consider seriously what it would look like if it were the other way around—if all that ever happened was causal evolution according to quantum mechanics. What I have tried to convince you is that what it looks like is ordinary everyday life.

Coleman's explanation of his thinking is well worth reading, but a short summary will do here. Suppose I set up an experiment to measure the spin of an electron, whether up or down in a chosen direction. The several systems involved in this measurement are (1) the electron, (2) the measuring device, (3) the observer, and (4) the environment: the rest of the universe. Coleman advises us to accept the quantum prediction that all four systems, electron to universe, are entangled in what we can take to be a pure quantum state. This state evolves in the usual deterministic way of quantum physics during the course of the measurement without the traditional notion of the collapse of the wave function. After the measurement the electron remains entangled with the measuring device and the observer. The observer, who could be you, is in a mixed quantum state that is so abundantly entangled with the enormous number and variety of things in our immense environment that the observer is in an excellent approximation to a classical state. The simplest argument for this last assertion follows from the assumption that quantum physics is accurate enough to account for biology, and the observation that you and I are biological systems that behave as if we were acting according classical physics. How could this be? In the language of quantum density matrices, the observer's off-diagonal components that represent

quantum entanglement have so many contributions that they average out very close to zero, producing a close approximation to the diagonal density matrix characteristic of a classical state. When this is done in the laboratory it is termed “engineered decoherence”; when it happens to us it is just the decoherence we experience. The remaining diagonal elements after decoherence give the probabilities of the outcomes of the measurement. So what is the result of the measurement? Ask the observer.

There is more to consider about this thinking. Erwin Schrödinger’s (1935 p. 812) celebrated thought experiment is that (in the translation to English by John Trimmer 1980)

one can even set up quite ridiculous cases. A cat is penned up in a steel chamber, along with the following device (which must be secured against direct interference by the cat): in a Geiger counter there is a tiny bit of radioactive substance, so small, that perhaps in the course of the hour one of the atoms decays, but also, with equal probability, perhaps none; if it happens, the counter tube discharges and through a relay releases a hammer which shatters a small flask of hydrocyanic acid. If one has left this entire system to itself for an hour, one would say that the cat still lives if meanwhile no atom has decayed. The psi-function [the quantum state vector, or wave function] of the entire system would express this by having in it the living and dead cat (pardon the expression) mixed or smeared out in equal parts.

This argument depends on the implicit and natural assumption that the cat in the chamber before it was opened is in a close-to-pure quantum state, a mixture of alive and dead. The argument that this cannot be so follows from two points. First is the standard assumption that cats and people are emergent from quantum physics. Second is the familiar observation that cats and people never are seen to be in more than one place; they always are seen to act like classical systems. Coleman points out that in quantum physics the cat, the observer, and the rest of the universe

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