The physicists philosophy of physics

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Abstract

I argue that research in physics operates under an implicit community philosophy, and I offer a definition I think physicists would accept, by and large. I compare this definition to what philosophers, sociologists, historians of science, and physicists say physicists are doing.

The appearance of an earlier version of this essay on arXiv produced feedback that aided clarifications of what I have in mind. I place the result, this version, on the PhilSci archive see if it produces more advice that will help improve statements of what I think I am thinking.

1 Introduction

There are many paths of experience to consider and perhaps bring into order, or maybe conclude are best left in disorder. Fundamental physics operates on the far more limited task of empirically establishing well-tested approximations to the rules that we assume are at the foundational basis for how the world operates. In Section 2 I offer a statement of what I take to be the physicists' philosophy in this enterprise: the starting assumptions for research. In Section 3 these assumptions are compared to ways of thinking about physical science by philosophers, sociologists, and physicists. Section 4 is a review the evidence from comparisons of predictions and measurements that physics has arrived good approximations to what we assume is the fundamental nature of mind-independent reality. Physics has a sociology, though again physicists seldom think about it; it is considered along with what sociologists think in Section 5. Concluding remarks are in Section 6.

2 Starting Assumptions

The starting idea of the natural sciences is that the world operates by rules that can be discovered by observations on scales large or small, depending on what is

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considered interesting. In fundamental physics, the subject of this essay, the idea is narrowed to four starting assumptions.

A: The world operates by rules and the logic of their application that can be discovered, in successive approximations.

B: A useful approximation to the rules and logic, a theory, yields reliably computed quantitative predictions that agree with reliable and repeatable measurements, within the uncertainties of the predictions and measurements.

C: Fundamental physical science is growing more complete by advances in the quantity, variety, and precision of empirical fits to predictions, and by occasional unifications that demote well-tested fundamental physical theories to useful approximations to still better theories.

D: Research in fundamental physical science is advancing toward a unique mind-independent reality, though not necessarily a reality that ever will be completely known.

Einstein's elegant way to put it is that (in the English translation by Sonja Bargmann 1954)

> The supreme task of the physicist is to arrive at those universal elementary laws from which the cosmos can be built up by pure deduction.

This is beautifully put but assumes a lot. My four statements are meant to spell this out in a more cautious way. Other physicists would frame the situation in other ways that I expect would express similar sentiments.

These four statements are not axioms; they are assumptions. They might be termed beliefs, or faiths, though these words are too strong. They are more accurately described as working hypotheses that are subject to adjustment if the empirical evidence requires it. That has not happened so far.

The statements A to D serve as generally accepted guidelines for research aimed at improving our understanding of the world, of nature. I do not discuss applications such as in the philosophies of spacetime or of quantum physics. The histories of scientific discoveries I do consider are meant only as examples that offer support for the four assumptions. But my central purpose is to seek to clarify the meaning of the fundamental starting assumptions that are employed in scientific research by considering thoughts from philosophers, sociologists, historians, and the occasional physicist that are in agreement with or in useful disagreement with thinking bounded by the assumptions. I intend to draw lessons about the assumptions from the history of physics since Maxwell unified the theories of electricity and magnetism. More is to be learned from the history of development of natural science up to Newton, but this is not discussed here.

Physicists do not tend to see much use for philosophy. The feeling is expressed in the chapter *Against Philosophy* in Steven Weinberg's (1992) book, *Dreams of a Final Theory*:

> Physicists do of course carry around with them a working philosophy. For most of us, it is a rough-and-ready realism [but] we should not expect [philosophy] to provide today's scientists with any useful guidance about how to go about their work or what they are likely to find.

Weinberg makes sense; just let physicists get to work. But I intend to explore how learned nonscientists can help us better understand what we are doing even when disagreements about that force us to think about why we disagree.

3 Interpreting the Assumptions

The assumptions in Section 2 call for judgements of ambiguities and sensible exceptions. This is best explored by reviewing what knowledgeable people say about the practice of natural science.

3.1 Reality

Physicists replace the rich philosophical issues of reality with the simple hypothesis in Section 2 that our world is real, mind-independent, and worthy of study. This is seldom stated in the practice of research natural science, but the hypothesis is there as a goal, usually implicit. The evidence from the success of science is that the world does operate by rules and logic we can discover.

In the years around 1900 Charles Sanders Peirce made perceptive comments about physics, much of it published in the magazine *Popular Science Monthly*. (The magazine was renamed *Popular Science*; I and other youths read it and some became physicists in part because of that magazine.) The Canadian philosopher Cheryl Misak (2016), in *Cambridge Pragmatism From Peirce and James to Ramsey and Wittgenstein*, gives an informative account of Peirce and like thinkers. Peirce (1877, p. 11, 12) wrote in *Popular Science Monthly* that

> Such is the method of science. Its fundamental hypothesis, restated in more familiar language, is this: There are real things, whose characters

> are entirely independent of our opinions about them; those realities affect our senses according to regular laws, and, though our sensations are as different as are our relations to the objects, yet, by taking advantage of the laws of perception, we can ascertain by reasoning how things really are . . . It may be asked how I know that there are any realities . . . If investigation cannot be regarded as proving that there are real things, it at least does not lead to a contrary conclusion.

This is a notable though seldom celebrated point.

Percy Bridgman (1927), the 1949 Nobel Laureate honored for his research on the behavior of matter at high pressure, put it that

> It is of course the merest truism that all our experimental knowledge and our understanding of nature is impossible and non-existent apart from our own mental processes, so that strictly speaking no aspect of psychology or epistemology is without pertinence . . . We shall accept as significant our common sense judgment that there is a world external to us, and shall limit as far as possible our inquiry to the behaviour and interpretation of this "external" world.

Natural science has advanced a lot since these remarks but they remain a good expression of thinking in the natural science community. We have not found evidence against Assumption A, and we have considerable evidence for it from the success of science.

The rules and logic mentioned in Assumption A are supposed to apply whether or not we are present to attempt to discover them. The galaxies of stars certainly look real and we imagine they were real before people existed. A useful term for this in the philosophy of science is mind-independent reality. A roughly equivalent term is objective reality, but I avoid the word "objective" because Lorraine Daston and Peter Galison (2007) describe in *Objectivity* the complicated history and broad variety of present usages of the term, and on the advice of David Hogg that I use "mind-independent."

3.2 Predictions and Falsifications

Karl Popper's contribution to the philosophy of science is informally honored in physics by the use of Popper's word, "falsifiable," as in a list of advantages of a proposed theory that ends with " . . . and it is falsifiable." Popper's (1959, p. 10) more nuanced argument includes the point that

> singular statements — which we may call 'predictions' — are deduced from the theory . . . those are selected which are not derivable from the current theory, and more especially those which the current theory contradicts. Next we seek a decision as regards these (and other) derived statements by comparing them with the results of practical applications and experiments . . . if the conclusions [predictions] have been falsified, then their falsification also falsifies the theory from which they were logically deduced.

Popper's tests of empirical predictions are essential to science. This is because people can be wonderfully imaginative in constructing stories, or theories, that account for what is thought to be appropriate. It is encouraging if a theory can be devised to fit many known observations, but that might be only a clever contrivance. The more secure checks are the tests of predictions that were not considered in constructing the theory. The greater their number, variety, and reliability that fit reliable measurements the more compelling the case that the theory is a useful approximation to reality. Why else would the predictions agree with the observations? We must consider that some predictions might be only apparent, actually known ahead of time, and others might be successful because many theories were examined and one at last found that accidentally fits the measurements. But these thoughts are difficult to accept when the successful predictions are numerous. The point is put a different way by the philosophers' "miracle argument" (Putnam 1975, p. 73):

> The positive argument for realism is that it is the only philosophy that doesn't make the success of science a miracle.

This agrees with scientists' intuitive feeling.

Peirce (1878, p. 299) wrote that

> all the followers of science are fully persuaded that the processes of investigation, if only pushed far enough, will give one certain solution to every question to which they can be applied. One man may investigate the velocity of light by studying the transits of Venus and the aberration of the stars; another by the oppositions of Mars and the eclipses of Jupiter's satellites; a third by the method of Fizeau . . . [and another] may follow the different methods of comparing the measures of statical and dynamical electricity. They may at first obtain different results, but, as each perfects his method and his processes, the results will move steadily together toward a destined centre.

The first two of these measurements are from astronomical observations, one is from laboratory measurements of the speed of light, and the last is from the theories of electricity and magnetism (which scientists were realizing predicts the speed of electromagnetic radiation, as in light). We can put it that one of these measurements produced a prediction of what the other quite different methods would find. It is impressive that the results are pretty close. Peirce was confident that all would agree as the measurements improved; this was his understanding of reality. It could have occurred to him that the known consistent results from very different ways to probe the speed of light argues for his understanding of reality. The same can be said of a considerable variety of tests of consistency of other probes of the world; some are reviewed in Section 4.

To see why the assumptions in Section 2 do not include Popper's falsifiability consider the classical theory of electromagnetism that Maxwell put together in the mid-1800s. In the 1930s Maxwell's theory passed many tests by its applications in the laboratory and to a broad range of technology, things like transoceanic telegraph cables and electric street cars. The theory nevertheless was known to fail when applied to an atom. That falsified Maxwell's equations. But a more accurate way to put it is that the classical theory is a demonstrably useful approximation, but one that requires improvement. This proves to be quantum electrodynamics, QED. It contains the classical theory as a limiting case, as a viable theory must, and it yields new predictions that pass experimental tests, some to considerable accuracy, as an established theory must. (This is reviewed in Sec. 4.) But QED also is an approximation, a limiting situation in the electroweak theory, which is part of the standard model of particle physics that the community agrees calls for discovery of another step toward unification.

The judgement of a physical theory must take account of its parameters. Some might have been tightly constrained by measurements while others are freely adjustable to fit other measurements. If the count of different reliable and successful predictions of a theory exceeds the number of parameters that can be adjusted to fit the theory to the data then it offers a plausible case that the theory is a useful approximation to reality.

The American philosopher of science Norwood Russell Hanson (1961, p. 36) offered another thought, that

> 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.

We must live with this thought. No matter how tight and accurate the agreement of theory and practice it remains conceivable that a different theory would do as well or even better. We can only aim to add predictions that if successful make this unlikely thought even more unlikely.

3.3 Fundamental Physics

Many lines of enquiry in natural science seek an accurate and useful account of nature by research shaped by the particular operating conditions: some quantitative, some informative descriptions. For the purpose of this essay fundamental physics is the reductionist search for mind-independent reality defined and constrained by the assumptions in Section 2. This research aims for theories that yield reliable quantitative predictions that can be compared to reliable, reproducible, measurements. This limits the reach of physics but, it is hoped, aids the approach to mind-independent reality.

Here is how Baird, Scerri, and McIntyre (2006) put the relation of this kind of physics to chemistry.

> 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 concrete justification. Molecular hydrogen is simple enough for a clean close to first principles computation of its binding energy. In the ground level of an ammonia molecule the nitrogen atom is literally on both sides of the triangle of the three hydrogen atoms. And Pauling's resonating valence bonds were an excellent start to chemistry. These steps in the entry of physics to chemistry have gone further with the help advances in computation and data storage, but Earman and Roberts (1999) put the broader situation correctly:

> 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.

Philip Anderson's (2011, pp. 144, 201) thinking about this point is illustrated by his statements that

> The important lessons to be drawn are two: 1) totally new physics can *emerge* when systems get large enough to break the symmetries of the underlying laws; 2) by construction, if you like, those *emergent properties* can be completely unexpected and intellectually independent of the underlying laws, and have no referent in them . . . I think almost all the things worth studying are irreducibly complex [and that] requires research which I think is as fundamental in its nature as any other.

Anderson was a good physicist with a good point to make. His subject, condensed matter, is as intellectually interesting and challenging as elementary particle physics; research in condensed matter physics costs far less and contributes far more to the world economy; and there are far more irreducibly complex things, from condensed matter to botany to people, than phenomena that are useful probes of foundational reality. But people are curious, and the concept of mind-independent reality fascinates many because it promises insights into the basic nature of reality, surely a fascinating topic for research.

I do not know what Anderson meant by the word "referent." The actors in condensed matter physics are electrons and ions with electromagnetic fields, all of which are reasonably well understood actors in fundamental physics. And the behavior of these actors in condensed matter can be reliably computed starting from good approximations to quantum physics in some interesting cases. That is, fundamental physics is an important aspect of research into the properties of condensed matter as well as chemistry.

I suppose most physical scientists expect that the properties of a botanical specimen also are determined by quantum physics, but of course it would be absurd to attempt a first-principles analysis of the specimen, an analysis starting from quantum physics. A sensible research program in this field includes close examinations of botanical phenomena that suggest effective theories: theories in the sense that they unify and are predictive of botanical phenomena, effective in the sense that they might be derived from a more fundamental theory, we suppose quantum physics, but which, as Anderson puts it, has been obscured by renormalization. Checking whether this last point is so is one of many loose ends in physics.

Another loose end is the thought that our established quantum and general relativity physics might be effective theories emergent from something even more fundamental. The important difference is that botanists know about the effects of atoms and ions, elements of our best approximation to fundamental theory, but physicists have no empirical reason to suspect that our theories, including atoms and ions and even spacetime, might be emergent from some still deeper kind of physics. It is another loose end, one that fascinates physicists. Some of these loose ends might be resolved by better methods of observation and computation with more capable computers. Some might be clues to a better physical theory. Maybe people never will weave some loose ends into fundamental theory even though more capable beings could do it.

I became interested in the topic of this essay too late to discuss it with Phil Anderson, who was a colleague in physics at Princeton. I have tried to make this discussion as close to Phil's opinions as I could consistent with my own thinking, but I know he would insist that I have not given proper credit to the study of complex systems. We agree on the science but differ on priorities. The philosophy of studies of complex systems is in no way inferior to that of fundamental physics, but it belongs in a different essay.

3.4 A Final Theory

Popper (1959, p. 452) wrote that

> I see no reason to believe that the doctrine of the existence of ultimate explanations is true, and many reasons to believe that it is false. The more we learn about theories, or laws of nature, the less do they remind us of Cartesian self-explanatory truisms or of essentialist definitions. It is not truisms which science unveils. Rather, it is part of the greatness and the beauty of science that we can learn, through our own critical investigations, that the world is utterly different from what we ever imagined—until our imagination was fired by the refutation of our earlier theories. There does not seem any reason to think that this process will come to an end.

It might be successive approximations all the way down, as Popper suggested, or maybe fundamental physics ends in a final theory that passes all tests of elegance, logic, and feasible observations. Or maybe there is more than one point of convergence, a thought noted in Section 3.2 that seems wildly unlikely but motivates Assumption D in Section 2.

In the book *Dreams of a Final Theory* the physicist Steven Weinberg (1992) pointed out that the establishment of a physicists' final theory would not improve weather forecasts: a computation from first principles would be far too complicated. And as Popper suggested, and Weinberg knew quite well, a final theory is not going be a self-explanatory truism. Generations of theories and experiments have led to the present-day fundamental classical and quantum theories that are wonderfully elegant and empirically successful. This encourages the thought that physics will arrive at a theory so elegant and empirically well supported that it compels community acceptance. The situation would be awkward, however, because theory would have outpaced observation. Without empirical checks what would be the evidence that scientists are not fooling themselves? For all we know quantum and general relativity physics might never be reconciled. That is why the starting assumptions listed in Section 2 mention a final theory, a fully adequate picture of foundational reality, only as an asymptotic goal.

3.5 The Anthropic Principle

Brandon Carter (1974) introduced his thinking about the anthropic principle with the remark that

> Copernicus taught us the very sound lesson that we must not assume gratuitously that we occupy a privileged *central* position in the Universe. Unfortunately there has been a strong (not always subconscious) tendency to extend this to a most questionable dogma to the effect that our situation cannot be privileged in any sense. This dogma (which in its most extreme form led to the 'perfect cosmological principle' on which the steady state theory was based) is clearly untenable, as was pointed out by Dicke . . .

Dicke (1961) drew attention to the consistency condition that the time elapsed since the early stages of expansion of the universe had to have been long enough to have allowed stars to produce the chemical elements we need, but not so long that stars suitable for supporting our existence would have exhausted their supplies of nuclear fuel and faded away. The evolution ages of the oldest stars and the radioactive decay age of the solar system are consistent with these conditions, as is the time elapsed to a reasonable present mean mass density in an expanding relativistic universe. It would have been a serious problem otherwise.

Weinberg (1989) discussed another consistency condition. The cosmic mean mass density expected from quantum physics, if represented by Einstein's cosmological constant Λ, is far larger than what is allowed by the standard relativistic cosmology. Nima Arkani-Hamed (2012) put it that

> This is the largest disagreement between a "back of the envelope" estimate and reality in the history of physics—all the more disturbing in a subject accustomed to twelve-decimal place agreements between theory and experiment.

What is more, we have good evidence that we flourish just as the rate of expansion of the universe is making the transition from slowing due to the attraction of gravity to increasing due to the effect of a positive value of Λ. Why should that be? Weinberg offered an anthropic explanation. If Λ had been positive and much larger than the effect of the observed mean mass density, then the universe would have been expanding too rapidly to have allowed the formation of the clusters of galaxies within which the chemical elements we need were produced. If Λ had been negative and large the universe would have collapsed before natural evolution could have produced observers such as us. So imagine a statistical ensemble of universes, a multiverse, with Λ different in different universes and typically large, whether positive or negative, as might be expected from the large value suggested by quantum physics. We might expect to have flourished in a universe in the multiverse with the largest absolute value of Λ allowed by our existence. This is roughly what is observed.

A multiverse of universes is expected in some versions of the cosmological inflation picture of what our universe was doing in the very early stages of expansion. It would be interesting to compare the value of Λ derived from cosmology to the range of values expected in those universes in a multiverse that have physics capable of supporting life of a kind that would take an interest in the value of Λ. But we do not have an adequate theory of the properties of universes in a multiverse, or of the kinds of physical theories that allow the formation of entities that take an interest in their surroundings.

If research continues to fail to reconcile the value of Λ from quantum physics with the value from cosmology we can anticipate two camps of physicists. One would insist on working even harder to avoid the anthropic argument. The other would accept a multiverse. The latter risks missing discovery of a perfectly good resolution by a sensible improvement of standard physics. The former risks spending a lot of time looking for that improvement with no guarantee of success. You choose.

My former colleague John Archibald Wheeler liked new ideas. He encouraged Brandon Carter's thinking about the anthropic principle and offered his own adventurous "participatory anthropic principle," that we are entangled with the rest of the universe. This is standard quantum physics, but I cannot see how it could be relevant to Schrödinger's cat in the alive and dead states, because the entanglement of the two states with all that is around us is so mixed up that I suppose entanglement averages out to zero. Entanglement across the universe seems likely to be even more suppressed. But progress demands adventurous ideas.

3.6 Philosophies of Physics

I cannot offer a fair sample of what physicists can learn from philosophers; the variety is too broad. But here are samples that seem instructive.

I admire Ernst Mach's (1902) book, *The Science of Mechanics*, for its informative discussions and elegant demonstrations of classical mechanics. Mach's demonstrations still serve as valuable teaching tools; I used them in my introductory physics lecture demonstrations.

To Mach the science of mechanics is an economical way to state empirical results. For example, Mach wrote that the

> atomic theory plays a part in physics similar to that of certain auxiliary concepts in mathematics; it is a mathematical *model* for facilitating the mental reproduction of facts. Although we represent vibrations by the harmonic formula, the phenomena of cooling by exponentials, falls by squares of times, etc., no one will fancy that vibrations *in themselves* have anything to do with the circular functions, or the motion of falling bodies with squares. It has simply been observed that the relations between the quantities investigated were similar to certain relations obtaining between familiar mathematical functions, and these *more familiar* ideas are employed as an easy means of supplementing experience.

Mach's negative thinking about atoms could not last. Rutherford, Boltzmann, Einstein and others were using the atom model to arrive at predictions that were encouragingly similar to observations, but Mach's auxiliary concepts still are part of physics. Quantum operators on state vectors in an abstract space are used to compute wonderfully precise and successful predictions. I expect these auxiliary concepts will last as long as quantum physics, but know of no concept that would guide ideas about their presence in a final theory.

At a more tangible level the dark matter of physical cosmology is not directly detected at the time of writing, and for all we know will prove to be observable only by the effect of its gravity. If so it will remain another of Mach's auxiliary concepts. The web of indirect evidence from the effects of its gravity is tight enough that dark matter has a place in standard and accepted physics, however, and it would be a serious shock if dark matter were falsified by a failure of its predicted effects.

Mach (1902) understood what we would term the predictive power of Newtonian mechanics, as we see in these remarks:

> The riddle of the tides, the connection of which with the moon had long before been guessed, was suddenly explained [by Newton's theory] as due to the acceleration of the mobile masses of terrestrial water by the moon . . . The trade-winds, the deviation of the oceanic currents and of rivers, Foucault's pendulum experiment, and the like, may also be treated as examples of the laws of areas [conservation of angular momentum in Newtonian physics]

In present-day thinking it is remarkable that the compact formulation of Newtonian physics accommodates this broad list of phenomena and more: Mach could have mentioned the motions of the planets and their moons. The broad success of Newtonian physics offers an excellent case for the assumptions in Section 2: nature does seem to operate by rules we can discover. But Mach saw the situation differently; he wrote that

> It is the object of science to replace, or *save*, experiences, by the reproduction and anticipation of facts in thought. Memory is handier than experience, and often answers the same purpose. This economical office of science, which fills its whole life, is apparent at first glance; and with its full recognition all mysticism in science disappears.

Mach's "anticipation of facts" can be read to mean "successful predictions," but Mach does not seem to have been interested in this thought. Mach's thinking is a mystery that has given positivism a bad name.

The physicists' common philosophy is a good approximation to the philosophers' realism. Anjan Chakravartty (2017) wrote that

> Scientific realism is a positive epistemic attitude toward the content of our best theories and models, recommending belief in both observable and unobservable aspects of the world described by the sciences. ... It is perhaps only a slight exaggeration to say that scientific realism is characterized differently by every author who discusses it.

This is a reasonable description of physicists' ways of thinking. Cheryl Misak (2016) considers Charles Sanders Peirce to be a pragmatist who starts with this realism. Since Peirce demonstrated an admirable understanding of physics I take it that physicists have a pragmatist philosophy. That agrees with the fact that empirical tests of physical theories cannot check all eventualities to all accuracy, meaning physical theories cannot be empirically established as mathematical theorems. We must instead rely on pragmatic judgements of how well predictions fit evidence and what that signifies. David Hogg (2009) puts it that the acceptance of an advance in physics is a plausibility argument. This is accurate, but as Hogg points out many aspects of physics have become plausible enough to inspire confidence, though never absolute belief.

The discovery of the uncertainty principle in quantum physics was a surprise that still exercises philosophers and physicists (e.g. Mermin 2019), for good reason. But although quantum measurement theory makes the search for the nature of reality far more interesting it fits the fundamental starting assumptions in Section 2. That is, the physicists' working philosophy as I understand it was not seriously disturbed by quantum physics.

Niels Bohr (1925) presented a review of the many phenomena that are suggestive of quantum physics, an excellent illustration of the rich empirically-driven side of the invention of this theory. Bohr's early statement of the correspondence principle is that the

> demonstration of the asymptotic agreement between spectrum and motion gave rise to the formulation of the "correspondence principle," ... [which] expresses the tendency to utilise in the systematic development of the quantum theory every feature of the classical theories in a rational transcription appropriate to the fundamental contrast between the postulates and the classical theories.

This outlines a prescription for the algebra of quantum observables and the always serious condition that the predictions of the quantum theory agree with the classical theory in conditions where the classical version is known to be accurate. (Later thoughts about the complementary roles of observables that do not commute are not needed here.)

3.7 Thomas Kuhn

Why do scientists expect that what is observed by eye and instruments that probe the world on scales large and small operates by rules we can discover? Thomas Kuhn had interesting things to say about this. Kaiser (2016) reported that Kuhn's (1962, 1970a) book, *The Structure of Scientific Revolutions*, has "Cumulative sales [that] exceed one million copies, and at least sixteen foreign-language translations have been published." The sociologist Andrew Abbott (2016, p. 168) found in the Web of Science some 17,000 citations of the book in the humanities and social sciences in publications up to 2012, a mean rate of about one citation a day. Paul Hoyningen-Huene (1993) presents a close analysis of *The Structure of Scientific Revolutions* in the book *Reconstructing Scientific Revolutions*. In the forward Kuhn wrote that "I recommend it warmly."

Kuhn's influential book, hereinafter SSR (the second edition), is particularly interesting to physicists because Kuhn had first-hand experience. He studied physics when he was an undergraduate at Harvard, graduating in 1943, then spent several years on war research (tracking German use of metallic chaff to avoid radar detections), returned to Harvard, and completed a doctoral dissertation (Kuhn 1949) on *The Cohesive Energy of Monovalent Metals as a Function of Their Atomic Quantum Defects*. His advisor was Nobel laureate John Van Vleck. It was followed by a paper with Van Vleck as co author, and Kuhn's (1950) single-author paper on *An Application of the W.K.B. Method to the Cohesive Energy of Monovalent Metals*, published in the *Physical Review*. But Kuhn came away from this experience with a concept of physics that differs from the community opinion in interesting ways.

Kuhn asked (in SSR, pp. 166, 171)

> Why should progress also be the apparently universal concomitant of scientific revolutions? . . . Does it really help to imagine that there is some one full, objective, true account of nature and that the proper measure of scientific achievement is the extent to which it brings us closer to that ultimate goal?

These are good questions. Natural scientists have not been issued a guarantee that the world acts on the basis of mind-independent rules that can be discovered in approximations that are converging toward ever better theory. These are assumptions. Natural scientists rarely question them, in part because our instructors and their instructors seldom questioned them, but certainly also because we have been encouraged by generations of advances that continue to yield predictions that agree with improving observations. This is what would be expected if we were approaching an ultimate goal, our Assumption D.

How did Kuhn's experience in physics affect his thinking about his two questions? Galison (2016) and Kaiser (2016) report that inspections of Kuhn's notebooks do not offer much evidence of what Kuhn thought of his experience as a physics undergraduate and graduate student. Aristides Baltas, Kostas Gavroglu, and Vassiliki Kindi (2000), who combine expertise in physics, history, and philosophy, gave us "an edited transcript of a tape-recorded three-day discussion—essentially an extended interview" in Athens on October 19-21, 1995. They asked Kuhn about recollections ranging from his childhood to experiences at Harvard to life after SSR.

Kuhn (2000, pp. 272, 273) recalled that, after completion of a reduced three-year undergraduate degree in physics at Harvard, employment in war research, and returning to Harvard,

> I was finding it [physics] fairly dull, the work was not interesting . . . I couldn't go back and sit still for that undergraduate chicken-shit and go on from there. So, I decided I'm going to take my degree in physics. But it also was clear, and becoming increasingly clear, that I was not being very much fulfilled by my graduate physics teaching.

This is to be contrasted with Kuhn's (2000, p. 276) recollection of what was leading up to SSR:

> I used to think . . . I could read texts, get into the heads of the people who wrote them, better than anybody else in the world. I loved doing that.

Kuhn expressed similar thoughts in an interview by Skúli Sigurdsson (1990):

> I wondered whether a physics career was what I really wanted. I was very conscious of the narrowing, the specialization required, and . . . I was beginning to look for alternatives. No one of those seemed more attractive than the rest, until all of a sudden I was asked to assist President [James B.] Conant in teaching an experimental General Education course on the history of science, through readings of case histories. It sounded like a pretty good idea; it would be a good experience, a chance to work with the President of Harvard, and also my first exposure to history of science. So I grabbed the opportunity and found it fascinating.

It might be relevant to Kuhn's thinking that his PhD dissertation on the physics of condensed matter required clever methods of approximation of quantum many-body physics. Anderson (2011, p. 38) put it that

> This process of "model building," essentially that of discarding all but the essentials and focusing on a model simple enough to do the job but not too hard to see all the way through, is possibly the least understood—and often the most dangerous—of all the functions of a theoretical physicist.

Anderson was discussing the models used in the first successful theory of superconductivity. Kuhn's doctoral dissertation employed quantum model-building that Galison (2016 p. 48) described this way:

> Central to his [Kuhn's] effort was Eugene Wigner and Fred Seitz's work from 1933-34, where they developed a quantum mechanical method for calculating the properties of metallic lattices [in solids].

This prewar approach was less abstract than the postwar methods that account for superconductivity, but still they were adventurous approximations. Kuhn might have had his experience in mind in the statement in SSR (p. 179) that

> normal puzzle-solving research [is] possible . . . as consequences of the acquisition of the sort of paradigm that identifies challenging puzzles, supplies clues to their solution, and guarantees that the truly clever practitioner will succeed.

This is a reasonable description of condensed matter physics if the paradigm Kuhn mentioned is a clever way to approximate what is expected from quantum mechanics and arrive at a reliable and useful approximation to the data. But the approximations are bold and Kuhn could have wondered whether they were contrived to get the wanted answers. Kuhn's thoughts of "challenging puzzles" and the "clever practitioner" could have been drawn from his experience in physics, though I have not found corroborating evidence.

Kuhn (1992, pp. 18-20) offered an assessment of the state of the natural sciences:

> what replaces the one big mind-independent world about which scientists were once said to discover the truth is the variety of niches within which the practitioners of these various specialties practice their trade.

> Those niches, which both create and are created by the conceptual and instrumental tools with which their inhabitants practice upon them, are as solid, real, resistant to arbitrary change as the external world was once said to be. But, unlike the so-called external world, they are not independent of mind and culture, and they do not sum to a single coherent whole of which we and the practitioners of all the individual scientific specialties are inhabitants . . . [Natural sciences] should be seen as a complex but unsystematic structure of distinct specialties or species, each responsible for a different domain of phenomena, and each dedicated to changing current beliefs about their domain in ways that increase accuracy and the other standard criteria I've mentioned. For that enterprise, I suggest, the sciences, which must then be viewed as plural, can be seen to retain a very considerable authority.

This is an interesting thought. Niches are unavoidable; the broad variety of research in the natural sciences requires specialization. And Kuhn's picture combines his interest in the influence of culture on science with the acknowledgement of the authority gained by the many useful applications of the sciences. As Kuhn wrote, astronomers and botanists have been building their own theories that are bringing each science "closer and closer to nature" as it is revealed by what they have been studying, each in a culture set by what their research can and cannot accomplish. Astronomers depend on physics for the structures of stars and galaxies of stars, and botanists depend on chemistry to trace the ways by which plants grow, but the two niches are quite different. This need not violate the physicists' vision of the unity of the natural sciences, though I expect the natural sciences never will "sum to a single coherent whole" because many parts of natural science are far too complicated.

Kuhn expressed more interest in the thinking of people who were doing the physics than the physics itself, which leads me to offer a personal thought. Among my early memories is of looking into an older sister's schoolbook and finding an illustration of a compound pulley (technically a block and tackle). I thought that was neat and still do. Kuhn was born to write SSR. I was born to write a close examination of SSR from a physicist's point of view.

4 Empirical Tests of Physical Theories

Physicists tend to be proud of their subject, maybe even arrogant about it. "Don't bother even thinking about making a machine that violates local energy conservation." But there is a reason, the remarkable consistency of predictions and measurements when both can be reliably established. Here are examples of what a useful philosophy cannot ignore.

4.1 Precision Tests of Quantum Physics

When Kuhn was writing SSR tests of the quantum theory of electrodynamics, QED, were celebrated for their precision, as they still are. The magnetic dipole moments of the electron and muon are written as $g = 2(1 + a)$, where the term $g = 2$ follows from Dirac's equation and a is the effect of the quantum interaction of the electron or muon with the quantum electromagnetic field and the other field operators of standard particle theory. The following results are taken from the Brodsky and Drell (1970) review of *The Present Status of Quantum Electrodynamics*. The quantum correction for the electron, e^-, in 1970, is

$$\begin{aligned} \text{predicted } a^- &= 0.001\,159\,663, \\ \text{measured } a^- &= 0.001\,159\,646. \end{aligned} \tag{1}$$

For the positron Broadsky and Drell give

$$\text{measured } a^+ = 0.001\,160. \tag{2}$$

The theoretical values of the electron and positron are the same. For positive and negative muons, the more massive relatives of the electron, Broadsky and Drell give

$$\begin{aligned} \text{predicted } a_\mu &= 0.001\,165\,87, \\ \text{measured } a_\mu &= 0.001\,166. \end{aligned} \tag{3}$$

I have slightly spoiled the precision of the theory of a by simplifying to the fixed value of the fine-structure constant in Broadsky and Drell. The predicted value of a for muons is a little different from the electron because the greater muon mass increases the effect of the quantum interaction with the other quantum fields.

There were other precision tests of QED, but this illustrates a situation that impressed physicists then as it does now. I have not seen any evidence that Kuhn knew the QED situation, and if so what he made of it.

Quantum Chromodynamics, QCD, the theory of the strong interaction, grew as a parallel to QED; it now passes the rich variety of tests reviewed by Campbell, Huston, and Krauss (2018) in *The Black Book of Quantum Chromodynamics: A*

Primer for the LHC Era. These tests make a compelling case that QCD also is a very useful approximation to reality.

The application of quantum theory to the structure of the helium atom with its two electrons bound to the more massive atomic nucleus required serious numerical computation, but it was accomplished using 1950s computers. Kuhn could have known a celebrated paper, Pekeris (1959), which reported a precision computation of the ionization energy of atomic helium, the energy required to pull an electron from a helium atom in its ground level. The result compared to the measurement is

$$\begin{aligned}&\text{predicted } 198\,310.687 \text{ cm}^{-1},\\ &\text{measured } 198\,310.82 \pm 0.15 \text{ cm}^{-1}.\end{aligned} \tag{4}$$

The three electrons in the next heavier element, lithium, make a precision computation of its structure much more difficult, but it has been done more recently. King (1997) reported

$$\begin{aligned}&\text{predicted } 43\,487.163 \text{ cm}^{-1},\\ &\text{measured } 43\,487.150 \text{ cm}^{-1}.\end{aligned} \tag{5}$$

Heavier atoms are far too complicated for similarly precise computations, but approximation methods put electron configurations in the elements in proper order, as presented in Herzberg (1944).

These precision tests of energy levels finesse the challenge of unifying classical and quantum physics by focusing on good approximations to energy eigenstates, where the meaning of energy agrees well enough with classical physics. This standard physics yields the remarkably precise tests of consistency of theory and measurement in equations (1) to (5). It is what would be expected if quantum physics is a useful approximation to reality.

4.2 The Fundamental Physical Constants

Peirce (1878), as discussed in Section 3.2, took the reasonably close to consistent values of the speed of light, c, obtained from measurements of quite different phenomena to be an illustration of the nature of the world, reality. Others seeking values of the fundamental constants might simply have sought to make the measurements as accurate as possible, a natural impulse. But maybe some also meant to check this aspect of reality.

The physicist Raymond Thayer Birge (1929) reported three different measurements of c (in units of 10^{10} cm s^{-1}):

$$\begin{aligned}
&c = 2.99796 \text{ from laboratory timing of reflected light pulses,}\\
&c = 2.9970 \text{ from wavelengths and frequencies of standing waves,}\\
&c = 2.9971 \text{ from the ratio of electrostatic to magnetostatic units.}
\end{aligned} \tag{6}$$

The treatment of measurement uncertainties was not well organized in 1929; uncertainties in these three quantities likely are in the last digit quoted. Peirce (1878) had mentioned results from the first and last of these methods along with several astronomical measurements that could not be made accurate enough for Birge's purpose. But they remain important historical evidence that it was known that c in the solar system agrees with terrestrial measurements.

Birge (1929) wrote that

> The decision as to the most probable value, at a particular time, of any given constant, necessarily demands a certain amount of judgment . . .

It is sociology again: what are the criteria by which Birge judged which of the measurements in equation (6) is most trustworthy? My experience is that the reputations of the authors are influential, though who knows how fair or unfair that is in each particular case? But let us bear in mind the key point: the close agreement of the three numbers in equation (6) that were obtained in quite different ways.

Birge (1929) reanalyzed data from Millikan's (1917) report of measurements of the electric charges on water and oil drops, and concluded that the charge is in units of what we term the electron charge,

$$e = -(4.768 \pm 0.005) \times 10^{-10}) \text{ absolute electrostatics units.} \tag{7}$$

with the statement that

> This, the writer believes, is the most reliable value that can be deduced from Millikan's oil-drop work.

Millikan (1917) had reported $e = -4.774 \pm 0.005$, quite similar to Birge's value in equation (7).

Barnes, Bloor, and Henry (1996, pp. 33 - 45) presented a detailed assessment of the evidence they found in Millikan's surviving notebooks of charge measurements that Millikan discarded as faulty, perhaps because of questionable measurements or problems with experimental setups, and maybe because the indicated charges were less than the theoretical minimum, the charge e of a single electron. It is notoriously difficult to control unconscious bias against unexpected results such as this despite serious attempts to control it. Barnes et al. meant to illustrate this sociological effect (which is discussed further in Sec. 5.3). We are interested in whether Millikan, despite great care, got e seriously wrong (or maybe overlooked a fractionally charged particle, maybe a quark, but the evidence now is that this is exceedingly unlikely). For our purpose it is important that Birge had a direct check (though I know of no evidence Birge had in mind challenges to Millikan's measurements), the comparison to measurements of the electron charge by a quite different method, electrolysis.

Electrolysis gives the mass transported by the transport of charge that could be accurately measured: current times time. That with molecular weights from lattice spacings in crystals from X-ray diffraction gives a measure of e. Birge quoted results from this approach by two groups:

$$e = -4.776 \text{ and } e = -4.794, \tag{8}$$

in the units in equation (7). Birge placed greater trust in the first value. Both differ from Millikan's result by more than Millikan's stated uncertainty, it is thought due to an error in Millikan's viscosity of air. But the point for the present purpose is that the values of c (in eq. [6]) and the values of e (in eqs. [7]) and [8]) are consistent within sensible allowance for systematic errors. Recall Peirce's (1878) point: the values of c and e derived from quite different ways to probe the world are quite similar, an indication of the way the world operates.

Richard Cohen and Jesse DuMond led a celebrated a series of papers in the mid-1940s to the mid-1960s on values of the fundamental constants. DuMond and Cohen (1953) reported improved measurements

> chiefly made possible by the intense development since World War II of microwave and atomic beam techniques for the study of proton resonance in magnetic fields, the fine structure of energy levels in hydrogen and deuterium, the magnetic moments, spin gyromagnetic ratios, nuclear magnetic resonance frequencies, and cyclotron frequencies of fundamental particles such as protons and electrons, etc.

Precision measurements of this variety of phenomena and others placed constraints on combinations of values of the fundamental parameters, including c, always provided standard physics is accurate enough. DuMond and Cohen found

$$\begin{aligned} c &= 299792.9 \pm 0.8 \text{ km sec}^{-1}, \\ e &= -(4.80288 \pm 0.00021) \times 10^{-10} \text{ esu.} \end{aligned} \tag{9}$$

The nominally most precise value for c from Birge's 1929 list in equation (6) differs from DuMond and Cohen by one part in 10^5. The new value of e differs from Birge's estimate (eq. 7) from the oil drop experiment by 0.7‰, and 0.4‰ from the electrolysis measurement. This is several times Birge's stated uncertainty. The subtleties of systematic errors can make this happen. The best check is from even more accurate measurements. We have the reasonable agreement of the speed of light Peirce reported, and the speed of light and electron charge Birge (1929) reported, then by DuMond and Cohen (1953), and the recent CODATA (Committee on data of the international science council) recommended values of the fundamental constants of physics and chemistry presented in Table 31 in Tiesinga, Mohr, Newell, and Taylor (2021). There are a few anomalies, notably the 3.5 standard deviations difference between the prediction and measurement of the magnetic dipole moment of the muon. It is judged to be significant and likely to be a hint to an improvement of the particle physics model, always an interesting possibility. But the greater point is the consistency of results from the long-standing tradition of precision measurements of c, e, and the other physical constants. I dislike being a scold, but I have to repeat that a useful philosophy must take account of this.

Entangled motions of electrons in condensed matter, and the entangled photon experiments for which Alain Aspect, John F. Clauser, and Anton Zeilinger were awarded the Nobel Prize, are fascinating examples of quantum physics that are observed and important tests of quantum physics but are not discussed here. A related phenomenon, the creation and annihilation of fundamental particles, is considered next.

4.3 Detection of the Neutrino

The direct detection of the neutrino in the 1950s was an important addition to the case for quantum physics. Instead of a precise measurement this was the check of existence of a hypothetical particle.

Wolfgang Pauli introduced the idea of neutrinos to save local conservation of energy and momentum in nuclear reactions involving creation and annihilation of electrons. Enrico Fermi introduced the four-fermion theory of the interaction of a neutrino with an electron, proton, and neutron, as in the reactions

$$n \rightarrow p + \bar{\nu} + e^-, \tag{10}$$

$$p + \bar{\nu} \rightarrow n + e^+. \tag{11}$$

The proton and neutron are n and p, e^+ is a positron, e^- an electron, and $\bar{\nu}$ an antineutrino. The first line describes the decay of a neutron with the creation of a proton, an electron that conserves electric charge, and an antineutrino that conserves lepton number. In the second line the annihilation of an antineutrino by a proton with the creation of a positron and the conversion of the proton to a neutron follows by a rotation of the first line. It is the same theory in quantum physics. Fermi's theory with refinements had served well in experiments in nuclear physics, but before the 1950s a neutrino could be considered a postulate, in the same sense that dark matter in the standard cosmology is a postulate at the time of writing.

The first direct detections in the 1950s (Cowan, Reines, Harrison, Kruse, and McGuire 1956 and references therein), were of antineutrinos from a nuclear reactor, where fissions of neutron-rich heavy atomic nuclei released neutrons that decayed by equation (10). A tiny fraction of the antineutrinos were annihilated in a detector near the nuclear reactor by the reaction in equation (11). The $\bar{\nu}$ annihilation would be accompanied by the creation of a positron that could be annihilated with an electron to produce gamma rays that could be detected. Some neutrons would be captured by an atomic nucleus, producing more gamma rays. The expected rate of $\bar{\nu}$ annihilations in the detector was computed from laboratory measurements of the neutron half-life in the reaction (10).

Reines and Cowan designed their experiments to detect the expected flux of antineutrinos. This was a test of a prediction: if the antineutrinos had not been detected at the expected rate, within the uncertainties of the neutron half-life and what was happening in the reactor, the falsification would have been a serious challenge to standard physics. Why would the reactions in equations (10) and (11) have successfully fit nuclear reaction measurements involving electrons and positrons but failed direct neutrino detection? This is Popper's point: an absence of detection at the predicted rate would have been a falsification of what otherwise looked like a good theory.

The physics community welcomed the Reines and Cowan detection, though not with the enthusiasm it merited because it was expected. The detection ranks in importance with the precision measurements reviewed in Sections 4.1 and 4.2 because it was a confirmation of a prediction that added to the weight of evidence of the existence of neutrinos, the reliability of quantum theory, and what looks like a good approximation to reality. The Nobel Committee recognized this some 40 years later.

Kuhn mentioned the $\bar{\nu}$ experiment (in SSR, p. 26):

> the gigantic scintillation counter designed to demonstrate the existence of the neutrino—these pieces of special apparatus and many others like them illustrate the immense effort and ingenuity that have been required to bring nature and theory into closer and closer agreement. That attempt to demonstrate agreement is a second type of normal experimental work, and it is even more obviously dependent than the first upon a paradigm. The existence of the paradigm sets the problem to be solved; often the paradigm theory is implicated directly in the design of apparatus able to solve the problem.

As Kuhn wrote, the design of the $\bar{\nu}$ experiment was determined by the theory. That is because the experiment was designed to check a prediction of the theory. The successful detection meant the theory passed a serious test. That is of prime importance to physicists.

4.4 The General Theory of Relativity

Einstein's general theory of relativity that Kuhn encountered in the 1950s was a good example of a social construction. (Details are reviewed in Sec. 5.1) Now the theory passes demanding checks of predictions on a broad range of scales, from tests of the inverse square law of gravity in the laboratory down to lengths of about 0.1 mm (Lee, Adelberger, Cook, et al. 2020); timing tests on the scales of the earth, $\sim 10^7$ cm, and the solar system, $\sim 10^{13}$ cm, reviewed by Clifford Will (2014, 2018); the tests of general relativity and the search for gravitational waves by precision measurements of periods of binary pulsars in the Milky Way galaxy, at distances $\sim 10^{23}$ cm (Will (2014, 2018; Agazie, Anumarlapudi, Archibald., et al., 2023); and detection of gravitational waves from merging black holes at distances $\sim 10^{27}$ cm (the LIGO Scientific Collaboration and Virgo Collaboration 2016). Added to this are the results of the cosmological tests reviewed in Peebles (2022) that probe the universe at close to the largest detectible scale according to standard theory. These tests include precision measurements of the anisotropy spectra of the thermal cosmic background radiation temperature and polarization and the power spectrum of the space distribution of large galaxies. It is important that the fits of the predictions of the standard cosmology agree with the measured departures from homogeneity of both the background radiation and the galaxies, because they are based on quite different probes of the universe. The values of the parameters in the standard cosmology are constrained by these measurements and other tests, typically to a few tens of percent. This is not very precise, but the consistencies of constraints are important because they are derived from what was happening at quite different stages of expansion of the universe, ranging from the present back to the time of formation of the isotopes of hydrogen and helium when the mean distance between nucleons was nine orders of magnitude smaller than it is now and the temperature was nine orders of magnitude larger. There are hints of discrepancies in parameter values. That does not seem very surprising because in the standard theory the dark matter and what behaves like Einstein's cosmological constant are modelled in a simple way, leaving ample room for adjustments. And maybe we are seeing indications that Einstein's field equation will have to be adjusted. But general relativity remains a remarkably useful approximation over a broad range of scales, from the laboratory to the observable universe.

4.5 Commonplace Evidence

We tend to consider it obvious, not worthy of notice, that the equations Maxwell wrote down in the mid-1800s still prove to be excellent approximations, tested every day by ordinary experience. But that is not what you might expect in a universe that does not operate by fixed rules. For that matter how could life form, how could the fittest survive, if nature kept changing the rules that determine what is required to be fit? All the considerations of this sort are commonplace evidence that nature is operating by rules we can rely on, evidence that is not to be ignored.

This commonplace evidence is a prime motivation for Assumption A in Section 2. Also commonplace enough is the manifestly real progress of the natural sciences, evidence that useful approximations to the rules have been discovered, and additional support for Assumption A.

4.6 Seeking a Unified Approximation to Reality

We have two well-tested approximations to what looks like reality: Einstein's general theory of relativity and quantum physics with the standard model for particle physics. The two are not compatible. That need not be surprising because both theories are incomplete. The conditions under which they have been tested so far are different enough that the incompatibility has caused no problem with precision tests so far, except for the puzzle of the quantum vacuum energy density.

These are some of the loose ends of fundamental physics. Some will be resolved, not necessarily all.

5 Sociology

Kuhn wrote in SSR (p. 8) that "many of my generalizations are about the sociology or social psychology of scientists," including physicists' subjective judgements. Consistent with this is Kaiser's (2016) report that in the abundant correspondence Kuhn received about SSR 28% were about about psychology, sociology, or philosophy, and only 11% were about physics or chemistry. Physicists tended to be a little offended by Kuhn's suggestion that psychology and sociology played a role in the establishment of our physical principles, though we are people and it must be so. The issue is the degree to which nominally well-established physics has been influenced by society.

5.1 Elegance

Kuhn's thinking about physics could have been influenced by the community acceptance of Einstein's general theory of relativity. The scant empirical support of this theory prior to 1960 is detailed in Peebles (2022, chapter 3). In SSR (p. 155) Kuhn wrote that Einstein

> seems not to have anticipated that general relativity would account with precision for the well-known anomaly in the motion of Mercury's perihelion, and he experienced a corresponding triumph when it did so.

The triumph was limited. In a letter to Conrad Habicht in 1907 Einstein wrote that[2]

> At the moment I am working on a relativistic analysis of the law of gravitation by means of which I hope to explain the still unexplained secular changes in the perihelion of Mercury [and added at the bottom of the page] so far, however, it does not seem to be going anywhere.

The explanation of the anomaly was not a blind prediction: Einstein found what he was seeking. There is nothing wrong with this, of course, apart from the reduced significance of the fit of the theory to the observed orbit of Mercury.

[2]The English translation is in the Collected Papers of Albert Einstein, Vol. 5, Doc. 69.

Einstein stopped looking for a still better theory because of the compelling elegance of his 1915 field equation. In more recent terms, general relativity is the covariant classical tensor field theory that preserves local conservations of energy and momentum with the simplest acceptable Lagrangian density, just as classical electromagnetism is the covariant classical scalar field theory that preserves charge with the simplest acceptable Lagrangian density. This is why general relativity is presented with classical electromagnetism in the book, *The classical theory of fields*, by Landau and Lifshitz (1951), the English translation of the Russian 1948 edition. It is the second volume in the celebrated series, the *Course of Theoretical Physics*, a standard and respected compendium of theoretical physics. The presence of general relativity in this series is recognition that it is a canonical part of theoretical physics, along with classical electromagnetism. But in the 1950s classical electromagnetism had been thoroughly tested while the empirical evidence for general relativity was meagre. The significance of the check from the motion of Mercury could be debated. The measurement of the gravitational redshift was in a confused state. That left the gravitational deflection of light by the mass of the sun that was detected, measured to a few tens of a percent. It disagreed with the estimate from Newtonian gravity theory and agreed with the general relativity prediction. This was important but slender empirical support. General relativity before 1960 was standard and accepted by the authority of respected theoretical physicists.

Kuhn (in SSR p. 155) made the point about elegance another way, by remarking on the influence of

> arguments, rarely made entirely explicit, that appeal to the individual's sense of the appropriate or the aesthetic—the new theory is said to be "neater," "more suitable," or "simpler" than the old.

Nobel laureate Frank Wilczek (2015) describes this feeling of elegance in fundamental physics in the book, *A Beautiful Question: Finding Nature's Deep Design*. Nobel laureate Steven Weinberg (1992) wrote in his book, *Dreams of a Final Theory*, that

> in this century, as we have seen in the cases of general relativity and the electroweak theory, the consensus in favor of physical theories has often been reached on the basis of aesthetic judgments before the experimental evidence for these theories became really compelling. I see in this the remarkable power of the physicist's sense of beauty acting in conjunction with and sometimes even in opposition to the weight of experimental evidence.

Kuhn, Wilczek, and Weinberg, I expect along with the general community, agree that elegance is influential in science. We cannot rely on it, of course, because people are adaptable: we learn to like what is successful. What is more, elegance can be misleading. The steady-state cosmology is elegant but wrong. Most agree that general relativity would be more elegant without Einstein's cosmological constant Λ, though the evidence requires it. (The convincing evidence for Λ was established after Weinberg wrote his Dreams book.) This is why Assumptions A to D do not mention elegance.

Despite these cautions we should not ignore the elegance of physics; it must have something to teach us, though I do not know what. Elegance led the community to general relativity well before it was empirically established. Brian Greene (2003) and Richard Dawid (2013) argue that some version of superstring theory, which is an elegant extension of the standard model for particle physics, might similarly be elevated to established theoretical physics, and some feel that Dawid's "non-empirical confirmation" deserves a place in the starting assumptions for physics. The failures of other arguments from elegance rule that out for now but there will come a time, if our present society lasts long enough, when theory can no longer be empirically tested and must be judged by elegance. It might work, but if so how could we know for sure?

5.2 The Bandwagon Effect

Scientists, being people, are subjects to fads, what Lakatos (1978, p. 91) termed the bandwagon effect. The distinguished astrophysicist Geoffrey Burbidge was fond of the term. Burbidge (2007) wrote

> the bandwagon effect, which has reached enormous proportions in some areas of astrophysics, prevails. Minority views are not given fair weight. Public relations departments in universities, and government agencies (particularly NASA), where decisions are made not on the basis of science but on the basis of who you are, and where you are, are particularly guilty. And of course, we are all individuals who often find it difficult to control our own feelings of superiority or jealousy when someone else makes a great discovery, particularly when it doesn't fit in with our own beliefs

In my experience Burbidge had a particular complaint: the community acceptance of the general relativity theory of the expansion of the universe from a hot dense early state. He was right; at the time this theory was more popular than merited by the evidence. But since then advances in observations have made a persuasive case that this theory is a good approximation to reality (Peebles 2022, chapter 6).

Andrew Pickering (1984, p. 7) had thoughts similar to Burbidge's about the bandwagon for QCD:

> by interpreting quarks and so on as real entities the choice of quark models and gauge theories is made to seem unproblematic: if quarks really are the fundamental building blocks of the world, why should anyone want to explore alternative theories?

Pickering was observing the confusion of creation of QCD, the analog of QED for strongly interacting particles, when there still was reason to question the idea of quarks. But QCD has since become a predictive theory that passes the abundant tests reviewed by Campbell, Huston, and Krauss (2018). In these two cases, QCD and the relativistic cosmology, the bandwagon pointed in productive directions. These are examples of the power of elegance, suitably defined.

Bandwagons, like elegance, can be misleading. In the 1990s the community opinion was that the universe is expanding at escape speed, in effect. It would mean the kinetic energy of expansion is exactly the same as the magnitude of the negative gravitational potential energy. If so the universe will expand into the indefinite future at a rate that is ever slowing by the ever diminishing attraction of gravity. In general relativity this would mean that Einstein's cosmological constant, Λ, vanishes or is negligibly small. This has the appeal of a neat conjecture, that a physical principle to be discovered forces the quantum vacuum energy density to vanish, and with it Λ. But I, Neta Bahcall at Princeton, and a few others were complaining that the evidence is that the mass density is too small for that. It was a challenge to the bandwagon favoring a negligible value of Λ. By the end of the century improved tests forced an abrupt change in thinking: the community had to learn to live with the inelegant Λ (by the evidence reviewed in Peebles 2022).

An aspect of the bandwagon effect, expectation bias, is illustrated by the variation with time of published values of the measured speed of light. Reports tended to be consistent with other recent previous reports, while the similar values at similar times could be inconsistent with later more accurate measurements. Henrion and Fischhoff (1986) show data illustrating the effect. It is seen also in measurements of Hubble's constant in cosmology, here largely the results of discoveries of systematic errors but also, inevitably, the peer pressure of the sociological bandwagon.

5.3 The Sociology of Physics

One camp in the sociology of science draws lessons from observations of what scientists are doing. The sociologist Robert Merton (1961) discussed an important example in the article *Singletons and Multiples in Scientific Discovery: A Chapter in the Sociology of Science*. It refers to a common experience: when an interesting idea starts to circulate there is a good chance the idea had already been proposed, independently, and escaped wide attention, or else will be independently proposed again if news does not travel fast enough. I have in mind advances in science, but expect it is a more general effect, as in gossip.

Merton referred to sociologists William F. Ogburn and Dorothy S. Thomas (1922), who published a list of 148 examples "collected from histories of astronomy, mathematics, chemistry, physics, electricity, physiology, biology, psychology and practical mechanical invention." The phrase, "the times were right," has been dismissed as trite but it fits this phenomenon. Multiples can result from developments of technology that enable scientific or practical advances that more than one person or group might independently recognize and use. But also important is the sociological effect that half-formed ideas, like gossip, tend to move through the community by causal remarks and behavior that can be evocative until they reach someone, or maybe more than one person, who is prepared to act on whatever form of the idea came through.

A second camp, the sociology of scientific knowledge, or SSK, gives more attention to the inevitable social influences on what scientists are thinking and doing. Bloor and MacKenzie (1997) expressed a soft version of this thinking:

> the goal of the sociology of knowledge, in our view, is the explanation of belief, not its evaluation . . . For the historian or sociologist studying nineteenth-century evolutionism, for example, both Darwinism and anti-Darwinism stand equally in need of explanation.

Bloor (1991) added

> But doesn't the strong programme [of SSK] say that knowledge is purely social? Isn't that what the epithet 'strong' means? No. The strong programme says that the social component always is present and always constitutive of knowledge. It does not say that it is the *only* component, or that it is the component that must necessarily be located as the trigger of any and every change: it can be a background condition.

This makes sense; I can add an example. I worked on cosmology in the 1960s even though I was dismayed by the scant empirical evidence along with the scant tests of the fundamental theory, general relativity. It helped that I was encouraged by the tolerant attitudes of colleagues, and the encouragement of my thesis adviser, Bob Dicke, who I considered my professor of continuing education. This was sociology. Also important was the realization in 1965 that we are in a near uniform sea of microwave radiation, something new and interesting to study, and maybe another aspect of sociology.

Though Kuhn rejected the strong programme of SSK I place on the more debatable side of this philosophy Kuhn's (1992, p. 7) argument that scientific research seeks

> the facts from which scientific conclusions should be drawn, together with the conclusions—the new laws or theories—which should be based upon them. These two aspects of the negotiation—the factual and the interpretive—are carried on concurrently, the conclusions shaping the description of facts just as the facts shape the conclusions drawn from them. . . Such a process is clearly circular, and it becomes very difficult to see what role experiment can have in its outcome.

Kuhn's circular reasoning is real, part of the healthy search for hints from experiments and observations that guide considerations of options for revising or devising theories to get better fits to the data. More senior physicists usually have greater influence in assessing these considerations: sociology at work. But when the community has settled on a theory what follows usually is comparisons of predictions that ideally are non-negotiable to reliable empirical tests that were not part of the circularity of search and discovery. They are the tests of predictions discussed in Section 3.2. (Bear in mind that physical theories tend to have parameters that can be adjusted to fit the data. An adjustment of a parameter within the bounds allowed by other tests that improves the fit is encouraging but not a demanding check. The tests of non-negotiable predictions that remain after adjustments of such free parameters are crucial.)

Each discovery story is different: Einstein's general relativity was accepted as canonical theoretical physics before it was seriously tested; quantum mechanics was discovered twice, in the wave and matrix forms, to fit the very suggestive list of phenomena Bohr (1925) reviewed. But the test of whether we are fooling ourselves remains the empirical checks of non-negotiable predictions, in the laboratory and practical experience.

The book on SSK, *Scientific Knowledge: A Sociological Analysis,* by Barnes, Bloor, and Henry (1996) drew a frank and thorough exchange of opinions by the physicist David Mermin (1998) and the sociologist David Bloor (1998). Mermin discussed aspects of physics: crystals and quasicrystals illustrate subtleties of classifications in science. This was part of well-reasoned critiques of arguments in *Scientific Knowledge.* Bloor responded with well-reasoned critiques of Mermin's critiques. But in this exchange of richly argued considerations Mermin did not take the opportunity to say how his experience in condensed matter physics informed his thoughts about the philosophy of physics. Physicists do not often publish such thoughts.

I venture to offer an example of what is to physicists distinctly odd thinking associated with SSK. It begins with the statement on page 107 of *Scientific Knowledge* that

> the different physical constants which appear in different problem solutions serve to connect them together and make the data emerging from the use of one relevant to the use and appraisal of others. Thus, the velocity of light [c] appears as a universal constant in many exemplars [paradigms, or theories] in physics . . . [c must] have a single value, and this requirement makes the development of the different exemplars in which they appear mutually interdependent and mutually constraining.[13]

The superscript refers to the note on page 207 in *Scientific Knowledge* where the authors ask whether in these considerations[3]

> the Duhem-Quine thesis becomes in practice irrelevant? And what is the nature of the need to keep these constants [including c] fixed: what kind of a transgression is it to refuse to keep them fixed?
>
> A strictly finitist answer to the first question (about the Duhem-Quine thesis) is that even after specific constants and values are accepted as fixed and non-negotiable, so many degrees of freedom will remain elsewhere in an exemplar that its further development will remain a matter of scientists' discretion. The task of keeping given constants and values fixed will merely increase the technical complexity of the task of extending an exemplar in one way rather than another.

[3]These considerations led me to write the review of measurements of the fundamental physical constants, including c, in Sec. 4.2.

> No fundamental change to the way we understand modelling will be entailed, no elimination of Duhem-Quine type problems.
>
> A strictly sociological answer to the second question is that to accept the need to keep certain constants and values fixed is to align one's practice with the practice of other scientists, and nothing more. To share physical constants is to share conventions. To propose alternative values for the constants is to challenge the practice of other scientists.

The "strictly finitist" answer refers to the Duhem-Quine thesis that so many free parameters, real and hidden, can be chosen to accompany the computation and experimental test of a prediction that unambiguous tests are not possible. But that misses two points. When a theory is fixed there is no freedom to adjust it. And all the hidden parameters are not adjustable unless they changed from experiment to experiment, but if that were so experimental results would not be reproducible. But they are reproducible. It makes the possibility of the discovery of a different physical theory that passes all available tests ever more exceedingly unlikely with the success of each well-checked prediction.

I trust the authors of *Scientific Knowledge* do not realize how insulting their "strictly sociological answer" is to the tradition of generations of physicists who spent so much time and effort making precision measurements of c, e, and the other fundamental constants, and carefully compiling the results with due attention to the inevitable measurement uncertainties. If there were a way to "align one's practice" to differ from "the practice of other scientists" these generations of careful and capable of people surely would have run across it. It would be big news.

The evidence Barnes et al. (1996) offered for their argument that this scientific tradition readily yields adjustable results is drawn from the measurements by Robert Andrews Millikan (1917 and earlier references) of the electric charges on drops of water or oil, and the analyses of the measurements by Millikan and his critics. This was discussed in Section 4.2. Let us note here that the practice of rejecting bad measurements must be done, but it is dangerous because it could bias results, maybe allowing artificial consistency or inconsistency with other measurements. Experimentalists take great care to control this problem. In blind medical trials the subjects and the scientists do not know who received the placebo until completion of the trial. The Higgs particle was detected by two groups that used the same particle accelerator beam line but different technologies applied to different methods of detection and analyses. Both found significant detections with consistent Higgs masses, important support for a celebrated result. Peirce (1878) discussed another important check, the comparison of values of fundamental constants derived from measurements of different phenomena by different people, all of whom had to take great care with the rejection of faulty measurements.

The authors of *Scientific Knowledge* could have known the celebrated series of papers by Richard Cohen and Jesse DuMond in the late-1940s through to the mid-1960s on the values of the fundamental physical constants. Section 4.2 presents a short review of this tradition. The speed of light Birge (1929) found (eq. [6]) differs by a part in 10^5 from the DuMond and Cohen (1953) value (eq. [9]) from a considerably larger variety of measurable phenomena that improve the constraints on the values of the constants. Birge's (1929) reanalysis of Millikan's (1917) report of the electron charge from the oil drop experiment differs from Cohen and DuMond by the fractional amount 7×10^{-3}. Birge's (1929) mean value of e from two electrolysis experiments differs from Cohen and DuMond by 4×10^{-3}.

What did the authors of *Scientific Knowledge* mean by their "strictly sociological" answer? Surely they did not intend to offer a sociological answer that is manifestly inconsistent with thoroughly explored and established physics. I do not imagine the authors intended to offer the absurd proposal that Cohen and DuMond manipulated the data from the many experiments made possible by the far more capable postwar technology to secure agreement with the values of c and e that Birge compiled in the late 1920s. The latter certainly no longer could be manipulated. Try to imagine the many postwar experimental groups contributing results to the Cohen and DuMond compilations keeping what they report consistent with prewar measurements, thus concealing an anomaly that would be a celebrated discovery. The authors of *Scientific Knowledge* were aware of challenges to Millikan's measurements of the charge of the electron, but they cannot have looked further into a tradition that is a deeply important part of the physics that is a subject of their sociological considerations.

5.3.1 Kuhn's Position

Norton Wise (2016) recalls Kuhn's emphatic disapproval of SSK in the early 1970s. Kuhn (1992, pp. 8,9) later wrote that

> the most extreme form of the movement, "the strong program," has been widely understood as claiming that power and interest are all there are. Nature itself, whatever that may be, has seemed to have no part in the development of beliefs about it. . . . I am among those who have found the claims of the strong program absurd: an example of deconstruction gone mad.

It is easy to imagine why Kuhn objected to this version of SSK; he knew examples of solid progress of physical science, as in the detection of neutrinos. But Kuhn held to the thought that physicists underestimate the influence of our social conditioning on the lessons we draw from nature. It is illustrated by what Peter Galison (2016, p. 58) found in Kuhn's notebooks:

> objective observation is, in an important sense, a contradiction in terms. Any particular set of observations . . . presupposes a predisposition toward a conceptual scheme of a corresponding sort: the 'facts' of science already contain (in a psychological, not a metaphysical, sense) a portion of the theory from which they will ultimately be deduced.

Kuhn was right to argue for the influence of society on the natural sciences, including fundamental physics. It is part of how we operate. And Kuhn asked another good question: why are physicists so enthusiastic about unification? This is discussed in Section 3.7; my paraphrase of Kunh's is, why suppose astronomy and botany can be unified? I have discussed this a some length elsewhere in this essay. The short answer is that unification is part of our sociology.

5.4 Knowledge Gained and Lost

Physicists tend to celebrate the advances in our subject without taking care to explain, or rationalize, the limitations. It invites the question of how well physics really is doing.

Skúli Sigurdsson (1990) reported Kuhn's recollection that

> What Aristotle could be saying baffled me at first, until—and I remember the point vividly—I suddenly broke in and found a way to understand it, a way which made Aristotle's philosophy make sense. [It] first got me onto the idea of gestalt switches and changes in conceptual frameworks, which was to show up in the *Structure of Scientific Revolutions* in 1962.

Kuhn (SSR pp. 4, 77, 102, 120) indicated that this line of thinking led him to conclude that

> No process yet disclosed by the historical study of scientific development at all resembles the methodological stereotype of falsification by direct comparison with nature. [Thus] Aristotle and Galileo both

> saw pendulums, but they differed [in] their incommensurable ways of seeing the world and of practicing science in it . . . [Newton's Laws are not] a limiting case of Einstein's. For in the passage to the limit it is not only the forms of the laws that have changed. Simultaneously we have had to alter the fundamental structural elements of which the universe to which they apply is composed.

Kuhn's thought is that since Aristotle had to have been intelligent his thinking about pendulums must seem naïve because Aristotle had an incommensurate way to think about reality, different from what became standard. This would make the difference of thinking by Aristotle and Galileo a pronounced paradigm shift, a fascinating sociological effect. Aristotle's way of thinking is lost, and maybe with it a valuable way to think about reality. Or maybe not. It is not at all likely that the tests of standard physics reviewed in Section 4 allow room for another kind of physics, though it is hard to check.

The philosopher Paul Feyerabend recalled that he and Kuhn independently found the term "incommensurable," taken from mathematics, to be useful in their interpretations of the natures of the natural sciences. (Oberheim and Hoyningen-Huene 2018 review the rich varieties of thinking about *The Incommensurability of Scientific Theories*.) Feyerabend and his mentor Karl Popper shared an interest in quantum measurement theory, but I have not found evidence that Feyerabend was exposed to the application of quantum theory to real physical systems, as in Kuhn's experience. Feyerabend (1970) mentioned vigorous debates with Kuhn in 1960-61 when both were at the University of California in Berkeley, but there also are examples of common thinking. Kuhn (1970b, p. 20) asked why physicists think they have been accumulating ever better approximations to reality, which is our Assumption C.

> Is it not possible, or perhaps even likely, that contemporary scientists know less of what there is to know about their world than the scientists of the eighteenth century knew of theirs? 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?

Feyerabend (1970, p. 219) recalled that Kuhn and he

> agreed that new theories, while often better and more detailed than their predecessors were not always rich enough to deal with *all* the

> problems to which the predecessor had given a definite and precise answer. The growth of knowledge or, more specifically, the replacement of one comprehensive theory by another involves losses as well as gains.

Kuhn (in SSR p. 6) argued that

> the major turning points in scientific development associated with the names of Copernicus, Newton, Lavoisier, and Einstein . . . Each of them necessitated the community's rejection of one time-honored scientific theory in favor of another incompatible with it.

These are interesting arguments, but we must consider also the essential condition that an improved physical theory must take account of knowledge that had been substantiated by empirical tests of the older theory. General relativity would not be interesting if its predictions did not agree with Newtonian gravity when speeds are much less than the speed of light and gravitational potential differences are small, conditions under which the Newtonian theory is thoroughly tested and found to be accurate. Einstein realized this. The consistency with Newtonian physics was a first check of his general theory of relativity. Useful knowledge was not lost; it was enriched.

I draw attention to another of Kuhn's always interesting thoughts. Kuhn (SSR p. 206) wrote that

> There is, I think, no theory-independent way to reconstruct phrases like 'really there'; the notion of a match between the ontology of a theory and its "real" counterpart in nature now seems to me illusive in principle.

WikipediA[4] informs us that, in

> metaphysics, ontology is the philosophical study of being. It investigates what types of entities exist, how they are grouped into categories, and how they are related to one another on the most fundamental level (and whether there even is a fundamental level).

The fundamental physics considered in this essay (and defined in sec. 3.3) does not have a secure ontology, for two reasons. First, the quantum field operators in a multidimensional space are not likely candidates for an ontology; they are

[4] `https://en.wikipedia.org/wiki/Ontology` (Accessed 28 December 2023)

better described as examples of Mach's auxiliary concepts, though they enable wonderfully precise agreements of predictions and measurements. Second, our best physical theories are at best useful approximations to reality. Consider what happened to electromagnetism when Maxwell put aside the search for a mechanical model of the ether and put together two laboratory-based theories, electricity and magnetism, with adjustments for consistency. Nothing was lost but the mechanical ether, which was not regretted because it never produced useful predictions, and of course the older ontologies of electricity and of magnetism. Another revolution reduced classical electromagnetism to a limiting case of the quantum theory, QED, which does not have a recognizable ontology. QED is the subject of Volume 4 in the Landau and Lifshitz *Course of Theoretical Physics*. But Maxwell's classical theory is not forgotten; it is reviewed in beautiful detail in the second volume of the series and it still is put to productive everyday use.

Consider an engineer designing a transmission line for electromagnetic energy who thinks about electric and magnetic fields with definite values as functions of position and time, a practical ontology for that purpose. A scientist designing an experiment to test QED thinks about the quantum electromagnetic field operator, another practical ontology. But the scientist must also think about the classical magnetic and electric fields that direct the motions of particles and communicate what is detected. The engineer might not know about QED, and the QED experimentalist might not be aware of continental drift, regrettable consequences of the breadth of research in the natural sciences. These are losses to individuals but not to the natural science community. The demotion of the Earth from the center of the world to an ordinary place in a universe that is evolving might be a cultural loss to some, but the Vatican is comfortable with this change of ontology.

Research in a niche, a branch of natural science, probes nature in the manner found to be practical for the establishment of useful categories, regularities, and effective theories. If science is converging toward the unique mind-independent reality envisioned in Assumption D in Section 2 then each niche that probes nature in its own way will find useful approximations to this unique reality. But apparent inconsistencies among niche theories must be expected because the probes reach different approximations to what is observed under different conditions. Sorting that out, and checking whether the effects of mind and the limitations of theories and experiments can be corrected to allow demonstration of convergence of niche theories to a unique reality is a good long-term goal that cannot be reached—too complex.

Science and society have lost useful knowledge. One is handwriting; others range from how to avoid tsunamis to the proper care and use of resources from fields and forests, the ground and the air. But I cannot think of evidence that physical science has forgotten useful knowledge in the past century and a half.

5.5 Existence

Bruno Latour spent two years embedded in the *Salk Institute for Biological Studies*, which left him with mixed feelings about the reality of molecules. Latour knew nothing about natural science; he was in effect an anthropologist observing a previously unknown culture. In the book about this remarkable experience Latour and Woolgar (1986) reported that (p. 183)

> We are not arguing that [the biophysical molecule] somatostatin does not exist, nor that it does not work, but that it cannot jump out of the very network of social practice which makes possible its existence.

Feyerabend (1989 p. 402) put it that

> Molecules, for example, the basic entities of chemistry and molecular biology, do not simply *exist*—period—they *appear* only under well-defined and rather complex conditions.

So is the biophysical molecule somatostatin real?

To keep it simple let us consider first atomic helium. We have an excellent case for the reality of this simple atom. Helium emission lines are observed in plasma around the sun and in the plasma around massive stars in galaxies near and far. Its effect as a low mass density noble gas is required to account for the mass density as a function of radius in the theory of the structure of the sun, with predictions that are well tested by heliosismology (Aerts and Kurtz 2010 Chapter 1). The same low mass density is tangible in a helium-filled balloon you hold by a string. The phase diagrams of condensed helium at low temperature in its two isotopes illustrate remarkable aspects of quantum physics. Fundamental theory yields precise and accurate predictions of the energy levels of an isolated helium atom (as illustrated in eq. [4]), which adds to the evidence that helium atoms exist and quantum physics is an excellent approximation to reality. Similar remarks apply to an isolated lithium atom (eq. [5]). All of this argues for the case that the physical pictures of atomic helium and lithium are good approximations to reality. But quantum physics is not a useful tool for the study of the therapeutic effect of lithium. From the point of view of physicists lithium in a living body is real, along with all the other atoms that are detected by nuclear magnetic resonance measurements. But analysis of the effect lithium has on how people feel is far too complex for computation from first principles. Physicists must leave this emergent phenomenon in more capable hands.

Latour observed laboratory studies of the biological molecule somatostatin. In its less massive form (fourteen amino acids) it consists of 219 atoms with 870 electrons. A computation from first principles of the properties of this molecule in isolation (if it exists in isolation as a close to pure state), as has been done for atomic lithium, is unthinkable. But quantum mechanics yields a reasonably good prediction of the energy levels of molecular hydrogen, Pauling's resonating valence bonds, and band theory, all products of quantum physics. In physicists' thinking molecular hydrogen and somatostatin existed before they were discovered, as surely as there were galaxies of stars before it was possible to observe galaxies. And it seems equally sensible to accept the evidence from the spectra of radiation from distant galaxies to mean that the chemical elements existed before there was life on Earth. But of course we cannot hope to send a robot to a planet in a distant galaxy to conduct some chemistry experiments and report back. Fascinating things are happening on the surfaces of planets around the stars in galaxies, things that are real in the mind of a scientist but we will never observe. They are examples of loose ends in science.

6 Concluding Thoughts

The bland statements of the basic starting assumptions in Section 2 that I take to define the physicists' philosophy of fundamental physics are intellectually demanding only in the attempt at comparisons to thinking by philosophers, sociologists, and historians. Maybe the physicists' philosophy would be more interesting if physicists spent more time thinking about it. Physicists have had a century to think about the inconsistency between the ontology of quantum physics, if it can be said to have one, and the ontology of the classical physics of general relativity. These two fundamental theories are well tested, and I cannot think of any empirical evidence made ambiguous by their inconsistency. (The usual approximations for dealing with situations such as the quantum formation of classical spacetime curvature fluctuations during cosmological inflation seem adequate for predictions of feasible measurements.) We have a philosophy that fits the state of the science so far.

We need a better way to explain to colleagues in other disciplines and to the interested public that the claims of advances in fundamental physics are solid, by and large, even though limited by the requirement of reliably computed predictions to compare to reliable tests. Botanists and astronomers examine reality by methods that seriously differ because the conditions of research demand different methodologies that inspire different philosophies. Physicists would like to think that botany and astronomy, along with the other lines of research in the natural sciences, share the foundational basis that our present fundamental physics approximates, but we do not know it and may never. This is not a crisis; research in the natural sciences is productive and advancing toward what look like still better approximations to aspects of reality. But the demonstrations of unity in science are limited, and a unified theory of botany and astronomy could be a step too far. We need a way to clarify this situation without leaving the impression that fundamental physics is in trouble. It is just limited, like everything else.

Feyerabend's (2010, p. viii) opinion—he had many—was that

> science should be taught as one view among many and not as the one and only road to truth and reality.

But students should have the opportunity to learn about the evidence we have of the foundational nature of the world. Maybe Feyerabend meant truth and beauty to include such things as the pleasure of attending a concert that is to your taste. This is a real part of our life, but it belongs in another essay. And I hope we keep clear to students and ourselves that these are quite different kinds of reality, one a matter of opinion, the other a result of painstaking observations and experiments that test and establish theories.

It is good to question authority in physics but it can go too far. I admire Mach's expositions that demonstrate the broad predictive power of classical physics, but cannot understand Mach's lack of appreciation of predictions that he understood quite well and are such an essential part of physics as it is understood now. I admire Thomas Kuhn's independence of mind and imagination in exploring the undoubtedly real influence of cultural norms on the development of physics. Physicists are quite capable of developing fixed opinions that can be taken up by enough people to be termed a bandwagon (Sec. 5.2). An example was the fixed opinion in the 1990s that Einstein's cosmological constant Λ surely is negligibly small. But I am perplexed by Kuhn's lack of appreciation of the broad range of successful predictions of physics that are established within the tight assumptions listed in Section 2. The puzzle of Mach and Kuhn is a meet topic for sociology.

What is the future of the fundamental physics defined in Section 3.3 in the centuries of research we hope are to come? The great weight of empirical support for present-day physics motivates Assumption C in Section 2: research will continue to produce ever better approximations to reality. Chemistry deals with more complicated situations but I expect advances in the technology of measurement and analysis will enable the reduction of much of chemistry to first principles, making it fundamental physics. (I do not mean this as a promotion, just a definition.) Maybe the result will show the way to an even better fundamental physical theory that of course preserves the empirical successes of what we have now. The wording of Assumption D is chosen to avoid predicting whether future advances in physics will be incremental, successive approximations all the way down, or maybe a brilliant conceptual advance that compels acceptance.

Weinberg's (1992) vision was that

> The final theory may be centuries away and may be totally different from anything we can now imagine. But suppose for a moment that it was just around the corner. What can we guess about this theory on the basis of what we already know?
>
> The one part of today's physics that seems to me likely to survive unchanged in a final theory is quantum mechanics. This is not only because quantum mechanics is the basis of all of our present understanding of matter and force and has passed extraordinarily stringent experimental tests; more important is the fact that no one has been able to think of any way to change quantum mechanics in any way that would preserve its successes without leading to logical absurdities.

Arkani-Hamed (2012) puts it that

> while we may not have experimental data to tell us about physics near the Planck scale [length scale $\sim 10^{-33}$ cm], we do have an ocean of "theoretical data" in the wonderful mathematical structures hidden in quantum field theory and string theory. These structures beg for a deeper explanation. The standard formulation of field theory hides these amazing features as a direct consequence of its deference to space-time locality. There must be a new way of thinking about quantum field theories, in which space-time locality is not the star of the show and these remarkable hidden structures are made manifest . . . by removing spacetime from its primary place in our description of standard physics, we may be in a better position to make the leap to the next theory, where space-time finally ceases to exist.

It is not unreasonable to imagine that a final theory will be expressed in the terms of quantum physics but, following Mach, I have to wonder whether quantum field operators are appropriate in a final theory of reality. Nature does not seem to need them. Although physicists speak of a final theory we cannot know for sure how it would be framed, if it exists.

I have emphasized the essential role of predictions, including the practical results of applications that also test theories. Some might say I have overemphasized it, but I cannot think of another way to check whether we are fooling ourselves. And since human ingenuity in devising physical theories will outstrip the ability to test them empirically we must anticipate that essential parts of what looks like a final theory will not be empirically supported. The theory might fit all available empirical tests, and it might be compellingly elegant, but the scientific community will not have tangible reason to expect they are correctly reading nature's opinion.

The end of physics has been announced on occasion. I prefer Peirce's later version of his remark quoted in Section 3.2, that

> all the followers of science are animated by a cheerful hope that the processes of investigation, if only pushed far enough, will give one certain solution to each question to which they apply it.

Peirce's cheerful hope continues to animate research in the natural sciences, including the physics that has so persuasively established empirically real things: the speed of light in vacuum and so much more that we have now and will continue to discover for many years to come if society allows it.

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