CONTENTS

CHAPTER ONE

On Science and Reality

The physical sciences that have grown out of curiosity-driven research have given us the enormous range of technology that so broadly affects our everyday lives, from electric power to the dubious benefit of cell phones. But are the theories that inform all this technology to be considered handy summaries, ways to remember useful experimental results? Or might we accept the assumption that most of us working on research in natural science take for granted, that our well-tested theories are good approximations to a reality that is objective, independent of our attempts to look into what this reality might be?

What is the meaning of reality? It is easy to say that it is what I experience when I wake up. But maybe I am still dreaming, or maybe, as the philosopher Gilbert Harman (1973, page 5) put it, "a playful brain surgeon might be giving [me] these experiences by stimulating [my] cortex in a special way." The thought is playful but the lesson is serious: natural scientists cannot prove they are discovering the nature of objective reality. The argument I am presenting in this book is that physical scientists are in a position to make a persuasive case about what they feel they have learned about a postulate: reality.

You will not often hear the case for reality made by scientists; they would rather go on with research conducted by the precepts they learned from what others are doing and by what they find works for them. This inattention has aided misunderstandings. Scientists point to the demonstrated power of theories that bring to order large ranges of phenomena, and successfully predict a lot more, down to cell phones. But philosophers and sociologists can point out that the best of our scientific theories are incomplete and rest on evidence that is limited by inevitable measurement uncertainties. How then can scientists claim to be discovering absolute truths? When scientists make such claims they should not, cannot, really mean it. The argument instead is that the predictive power of science, demonstrated by all the technology surrounding us, is what one would expect if objective physical reality operated by rules, and if we were discovering useful approximations to these rules.

We should pause for a little closer consideration of the thinking about the predictive power of theories. Suppose a theory is devised to fit a given set of observations. If the theory is a good approximation to what we are assuming is the reality underlying the observations, then we expect that applications of the theory to other, different, situations successfully predicts results of observations of the new situations. The greater the variety of successful predictions, that is, the greater the predictive power, the better the case that the theory is a good approximation to reality. It cannot be a proof; scientists will never be able to claim that the predictive power of their theories demonstrates that they are exact representations of reality. We can only assert that the impressive success of physical science, the broad predictive power of our theories, makes a case that our science is a good approximation to reality that is difficult to ignore.

You may say that these successful predictions are easy to ignore; just do it. But if you do, I urge you to pause to consider the technology you see operating around you. Scientists and engineers can make electrons do their bidding in your cell phone, by the operation of electric and magnetic fields that manipulate electrons and solid and liquid crystals. Does this look like the application of a myth peculiar to our culture? I put it that the many examples of technology of this sort that you see in operation around you make a case for culture-free physics that is very hard to ignore.

If it is accepted that the results of natural science are useful approximations to objective facts, then are our physical theories simply ways to remember these facts? The position taken in natural science is that the predictive power of the well-established theories demonstrates that they are more than that; they are useful approximations to the way reality operates. This is best explained by an example. The one to be considered in the following chapters is physical cosmology, the study of the large-scale nature of the observable universe.

Thinking about the power and limitations of natural science, about empirical facts and their unification and theoretical predictions, is not new. A century ago the American philosopher and scientist Charles Sanders Peirce emphasized the impressive predictive power of the physical theories of the time, what we now term the classical theories of electromagnetism, mechanics, and gravity. But there were expressions of doubt. Another excellent physicist, the Austrian Ernst Mach, asked whether these theories of mechanics and electromagnetism, and of heat and light, are overelaborate, maybe a little artificial. He preferred to think of theories as means of remembering facts. At that time the German-British philosopher F. C. S. Schiller went further, asking whether these facts are only constructions peculiar to eventualities of choices made by our particular society. The predictive power of our theories is even greater now but some things have not changed; we still hear such doubts. This is at least in part because scientists do not usually acknowledge that they work with some constructions that owe more to society than empirical evidence, to say nothing about even less well grounded speculations that physicists occasionally take more seriously than calm consideration would recommend. I will be discussing examples, and will argue that we find common ground by considering the many results from applications of theory and practice in natural science that certainly look like good approximations to reality, while bearing in mind lessons scientists can draw from what sociologists and philosophers observe scientists doing.

My worked example of all this is the growth of physical cosmology, the study of the nature of the universe on the largest scales we can observe. I start with the situation a century ago, when Einstein was thinking about this and he and others were contemplating the broader question of the nature of physical science: how is it done, and what do the results mean? My account begins in Chapter 3 with considerations of Einstein's theory of gravity, general relativity. The evidence we have now is that this theory gives a good description of the expansion of our universe. The idea was discussed in the 1930s, but until the 1960s the evidence for the expanding universe was meagre, the idea of cosmic expansion largely speculative. We can term it a social construction, to take a term from sociology. This with other assessments of science from the perspective of sociology is the subject of Chapter 2. The rest of the present chapter reviews thoughts about the nature of science, now and a century ago. These first two chapters are meant to introduce the considerations that are illustrated by the examples from general relativity and cosmology presented in Chapter 3 and on.

1.1 *Thinking a Century Ago*

A century ago Albert Einstein was thinking about about the nature of a satisfactory theory of gravity, and that led him to wonder how a philosophically sensible universe might be arranged. What were others thinking about physical science then? We see one line of thought in an essay published in *The Popular Science Monthly*,[1] by the American philosopher and scientist Charles Sanders Peirce[2] (1878a pages 299–300). Peirce wrote that[3]

> 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 apply it. 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; a fourth by that

1. The magazine continued publication, at least until recently, under the title *Popular Science*.

2. Brent (1993) describes Peirce's creativity and intelligence and not so edifying private life. Peirce's thinking is impressively sensible from my conditioned point of view, but of course there are exceptions. For example, I argue on page 12 that Peirce's discussion of free will is remarkably perceptive, but I do not know what to make of Peirce's thoughts that follow on how to "insert mind into our scheme" that Brent describes on page 208 of his book. But Peirce's ideas are of far more lasting value than most.

3. Because I admire so many aspects of Peirce's thinking, I can't let Peirce's use of gendered pronouns in this quotation and others to follow pass without comment. If challenged to consider the evidence, would Peirce agree that non-gendered pronouns are appropriate in this context? His creativity leads me to hope he could be persuaded to accept this thought that many of us have come to only fairly recently.

> of Foucault; a fifth by the motions of the curves of Lissajoux; a sixth, a seventh, an eighth, and a ninth, 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. So with all scientific research. Different minds may set out with the most antagonistic views, but the progress of investigation carries them by a force outside of themselves to one and the same conclusion. This activity of thought by which we are carried, not where we wish, but to a foreordained goal, is like the operation of destiny. No modification of the point of view taken, no selection of other facts for study, no natural bent of mind even, can enable a man to escape the predestinate opinion. This great law is embodied in the conception of truth and reality. The opinion which is fated[1] to be ultimately agreed to by all who investigate, is what we mean by the truth, and the object represented in this opinion is the real. That is the way I would explain reality.

The footnote in Peirce's second last sentence explains that

> Fate means merely that which is sure to come true, and can nohow be avoided. It is a superstition to suppose that a certain sort of events are ever fated, and it is another to suppose that the word fate can never be freed from its superstitious taint. We are all fated to die.

We see Peirce's pragmatic endorsement of the idea of objective facts. It is in line with common experience: hit a wine glass and it will break. The idea of facts is taken seriously if usually implicitly in the normal practice of natural science. Also usually implicit in natural science, though Peirce makes it explicit, is the assertion or, better, the postulate that there is "truth and reality" that would be "ultimately agreed to by all who investigate."

In other versions of this essay, which I think appeared later, the first sentence reads "all the followers of science are animated by a cheerful hope . . . ," and the third from the last begins "This great hope is embodied . . ."[4] Misak (2013, pages 50–52) makes it

4. This version is in the Collected Papers of Charles Sanders Peirce, edited by Hartshorne and Weiss (1934), Vol. 5, Book 2, Paper 5, §4, par. 407.

clear that Peirce took the word "hope" seriously. An example is his comment that[5]

> the only assumption upon which he can act rationally is the hope of success . . . it is always a hypothesis uncontradicted by facts and justified by its indispensibleness for making any action rational.

This is a good way to characterize the pragmatic study of physical science and the search for reality. We assume reality operates by rules so we can hope to discover them. It has worked well so far.

Peirce points out that values of the speed of light derived from quite different methods of observation, and using different theories for the reduction of the data from the observations, agree within reasonable allowance for measurement uncertainties. That is, given the result of one method, you could successfully predict what the results from the other methods would be. This demonstration of successful predictions is at the core of the meaning of results of research in physical science, so important that we should take the time to review the experiments and observations Peirce had in mind.

A transit of Venus is observed as a small black dot that moves across the sun along a chord of the face of the sun. Observers at different latitudes on Earth see the transit at chords of different length, meaning they find different times of transit as Venus enters and then leaves the face of the sun. Surveyors had measurements of the radius of the earth, so the distances between observers at different latitudes were known. Newton's theory of the motions of the planets gave the ratio of distances to Venus and the sun. With these data, trigonometry gives the earth-sun distance and the speed of the earth around the sun.[6] The former is known in the jargon as the solar parallax: the angular size of the earth at the sun. The latter is checked by the time it takes for the earth to complete one

5. These remarks are from *Writings of Charles S. Peirce: A Chronological Edition*, Volume 2: 1867-1871, page 272. Bloomington: Indiana University Press

6. In more detail, surveyors could establish the physical distance d between two observers normal to the line of sight to the sun. Newtonian mechanics gave the ratio r_v/r_s of the distances to Venus and to the sun. Then the physical distance between the two chords normal to the line of sight is $d(r_s - r_v)/r_v$. That with a little geometry gives the radius of the sun. The observed angular size of the sun gives r_s.

orbit around the sun, one year, given the solar parallax. Finally, the time of passage of Venus across the face of the sun gives the speed of Earth relative to the sun. The speed of Earth relative to the speed of light gives the angle, or aberration, by which the apparent positions of stars move as the earth swings around the sun. (Motion at speed v perpendicular to the direction to the star causes its angular position to shift in the direction of motion by v/c radians, where c is the speed of light.) Since our speed around the sun was measured, this ratio gave a measure of the speed of light.

Peirce mentioned a second measure of the solar parallax, derived from observations when Mars, Earth, and the sun are nearly in a line and Mars is on the opposite side of Earth from the sun. Mars is said to be in opposition to the sun. Since Mars is closest to us then, this is a good time to measure the distance to Mars by measuring angular positions of Mars relative to distant stars from different places on Earth, or from one place in the morning and evening when Mars and stars close to it in the sky are just visible. Then again trigonometry with the radius of the earth translates these angles to the Mars-Earth distance. Newtonian physics gives ratios of distances in the solar system, so the Mars-Earth distance gives another measure of the solar parallax, and from that aberration gives the speed of light. The consistency of measurements of the speed of light from observations of transits of Venus and oppositions of Mars checks that Newtonian gravity physics has the ratios of distances of the planets about right, and it checks the always present chance of systematic errors in these subtle measurements.

Peirce also mentioned observations of the eclipses of Jupiter's satellites as they pass behind Jupiter. The timing of the observed eclipses of the moons depends on the Jupiter-Earth distance, because of the light travel time. Given the solar parallax and Newtonian physics to get the Jupiter-Earth distance, the difference of timing of the moon passages as Jupiter moves closer and further from Earth yields the speed of light.

The laboratory measurements by Fizeau, Foucault, and Lissajoux also found the time that light takes to travel a known distance, but in experiments on Earth. The idea is the same as for the timing of the orbits of Jupiter's satellites, but the distances are so very different that I count this as an independent approach.

Peirce mentioned "statical and dynamical electricity." The theories of the electric field of a static distribution of electric charge, and the magnetic field of a steady current of electric charge, are similar apart from a multiplicative factor. That factor was known to be consistent with the speed of light. James Clerk Maxwell found that his theory predicts that electromagnetic waves propagate at this speed of light. As Peirce was writing this essay, experiments by the physicist Heinrich Rudolf Hertz were confirming the existence of these waves. Peirce was referring to some or all of this.[7]

Peirce later added that[8]

> All astronomers, for example, will agree that the earth is ninety-two or ninety-three millions of miles from the sun. And yet, one would base his conclusion on observations of the passage of Venus across the sun's disk; another upon observations of the planet Mars; another upon experiments upon light combined with observations of the satellites of Jupiter.

We see here and in the earlier quotations from Peirce two points that are of prime importance to understanding the nature of research in physical science.

First, Peirce stated that repeated measurements made under repeatable conditions, though they may be by different people, produce the same result. This seems obvious, it is our common experience, but we have not been issued a guarantee, so the evidence of repeatability from experience is essential. There is the complication that experimental and observational scientists, and scientists like me who live by their results, are conditioned to worry about systematic errors in subtle measurements. We pay close attention to what happens, in Peirce's terms, "as each perfects his method and his processes." Peirce was confident that "the results are found to move steadily together." Maybe he had in mind the reduction of corruption by suppression of systematic errors that always are present but in some cases can be made really small. Maybe he also had in

7. The electrostatic unit defined by the expression for the force between two elements of charge, and the magnetostatic unit by the expression for the force between two elements of current. The ratio c of the two has units of velocity. This was an experimental result before the connection with the speed of propagation of electromagnetic waves was recognized.

8. Hartshorne and Weiss 1934, Vol. 7, Book 2, Ch. 5, §3, par. 333.

mind that the result is independent of who made the measurement, within the uncertainty. Both are what would be expected under the assumption that reality operates in a lawful and regular way.

The second point Peirce was making is that measurements obtained by different kinds of observations, and reduced by application of different theories, can produce consistent results. By my count Peirce mentioned four independent ways to measure the speed of light: (1) transits of Venus and oppositions of Mars; (2) time delays in orbits of Jupiter's moons; (3) laboratory measurements of light travel times, the same effect as for Jupiter's moons but on length scales so very different as to count as an independent situation; and (4) laboratory experiments with electric and magnetic fields. If we had only one of these measurements then we could say that the speed of light need be nothing more than an artifice, designed to make a story that fits the evidence. But the speed of light determined by one of these methods successfully predicts the results of each of the other three. Peirce also pointed out that the solar parallax derived from one of these methods successfully predicts the solar parallax obtained by a second one. This is an impressive variety of successful predictions, based on applications of different physical theories to different kinds of observations. The consistency of results, within measurement uncertainties, supports the case that the speed of light and the solar parallax are meaningful physical concepts. This is what would be expected if our physical theories were useful working approximations to the behavior of an objective reality that operates by laws we are discovering.

Peirce put it that[9]

> 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. . . . Experience of the method has not led me to doubt it, but, on the contrary, scientific investigation has had the most wonderful triumphs in the way of settling opinion.

I feel the same way.

9. These remarks are from *Writings of Charles S. Peirce: A Chronological Edition*, Volume 3: 1872-1878, page 254

Gerald Holton (1988) gave the same argument a century after Peirce, in *Thematic Origins of Scientific Thought*:

> The meaning behind the statement that we "believe in the reality" of electrons is that the concept is at present needed so often and in so many ways—not only to explain cathode rays, the phenomenon that leads to the original formulation of the concept, but also for an understanding of thermionic and photoelectric phenomena, currents in solids and liquids, radioactivity, light emission, chemical bond and so on.

The progress of science allowed this even broader range of successful predictions, and Holton could have chosen many other examples.

Might there be another theory that would equally well fit these observations, perhaps when considered in the context of some other culture? It cannot be disproved, but for the examples Peirce and Holton mention it seems so unlikely as to be uninteresting. Herbert Dingle's (1931) succinct statement is that "Nature appears to be intelligible."

To repeat the important point: the consistency from diverse measurements and observations in the examples Peirce and Holton gave is what one would expect if objective physical reality operated by rules we can discover, and if the theories employed in interpreting these measurements were good enough approximations to these rules for successful predictions of new phenomena. Examples drawn from research in relativity and physical cosmology are discussed beginning in Chapter 3. But let us consider here the thinking of some of Peirce's contemporaries.

Peirce (1907) recalled that

> It was in the earliest seventies that a knot of us young men in Old Cambridge, calling ourselves, half-ironically, half-defiantly, "The Metaphysical Club"—for agnosticism was then riding its high horse, and was frowning superbly on all metaphysics—used to meet . . .

Misak (2013) shows that Peirce was particularly impressed by Chauncey Wright's contributions to discussions at the Metaphysical Club. Misak (page 17) presents an example of Wright's thinking:

> But whatever be the origin of the theories of science, whether from a systematic examination of empirical facts by conscious induction, or from the natural biases of the mind, the so-called intuitions of reason, what seems probable without a distinct survey of our experiences—whatever the origin, real or ideal, the *value* of these theories can only be tested . . . by deductions from them of consequences which we can confirm by the undoubted testimony of the senses.

These comments about the importance of what Wright termed "verification" in natural science agree with recent thinking; we need only add that the "testimony of the senses" is now received from quite sophisticated detectors. But I have not found evidence that Wright recognized Peirce's deeper point, the significance of the impressive predictive power of the physical theories of electromagnetism and Newtonian mechanics and gravitation.

William James (1907), another member of The Metaphysical Club and Peirce's close associate for many years, offered a different opinion of physical science. James wrote that

> as the sciences have developed farther, the notion has gained ground that most, perhaps all, of our laws are only approximations. The laws themselves, moreover, have grown so numerous that there is no counting them; and so many rival formulations are proposed in all the branches of science that investigators have become accustomed to the notion that no theory is absolutely a transcript of reality, but that any one of them may from some point of view be useful. Their great use is to summarize old facts and to lead to new ones. They are only a man-made language, a conceptual shorthand, as some one calls them, in which we write our reports of nature; and languages, as is well known, tolerate much choice of expression and many dialects.. . . Such is the large loose way in which the pragmatist interprets the word agreement. He treats it altogether practically. He lets it cover any process of conduction from a present idea to a future terminus, provided only it run prosperously. It is only thus that 'scientific' ideas, flying as they do beyond common sense, can be said to agree with their realities. It is, as I have already said, as if reality were made of ether, atoms or electrons, but we mustn't think so literally. The term 'energy' doesn't even pretend to stand for anything 'objective.' It is only a way of measuring the surface of phenomena so as to string their changes on a simple formula. . . . Clerk

> Maxwell somewhere says it would be "poor scientific taste" to choose the more complicated of two equally well-evidenced conceptions; and you will all agree with him. Truth in science is what gives us the maximum possible sum of satisfactions, taste included, but consistency both with previous truth and with novel fact is always the most imperious claimant.

Maybe the last sentence in these quotations, and earlier in James's comment that the "great use [of theories] is to summarize old facts and to lead to new ones," is a recognition of Peirce's point that tests of predictions can make a persuasive case for a physical theory. The rest of this commentary does not encourage the idea, however, and nor does James's (1907, page 153) remark that

> 'The true,' to put it very briefly, is only the expedient in the way of our thinking, just as 'the right' is only the expedient in the way of our behaving.

Misak (2013) reviews Peirce's objection to James's use of the term "pragmatism" to characterize this "expedient" reading of physical science. Peirce's "pragmatism" signifies the search for useful approximations to the way things are, as in electromagnetism and Newtonian physics.

In his book, *The Principles of Psychology*, James (1890, page 454) wrote that

> the whole feeling of reality, the whole sting and excitement of our voluntary life, depends on our sense that in it things are *really being decided* from one moment to another, and that it is not the dull rattling off of a chain that was forged innumerable ages ago. This appearance, which makes life and history tingle with such a tragic zest, *may* not be an illusion. As we grant to the advocate of the mechanical theory that it may be one, so he must grant to us that it may *not*. And the result is two conceptions of possibility face to face with no facts definitely enough known to stand as arbiter between them.

The metaphor of a chain of instructions is delightful, as is Peirce's (1892) way of putting it:

> The proposition in question is that . . . given the state of the universe in the original nebula, and given the laws of mechanics, a sufficiently

> powerful mind could deduce from these data the precise form of every curlicue of every letter I am now writing.

It seems typical that Peirce (1892) was willing to decide between the ideas of free will and the mechanical theory:

> the conclusions of science make no pretense to being more than probable, and considering that a probable inference can at most only suppose something to be most frequently, or otherwise approximately, true, but never that anything is precisely true without exception throughout the universe, we see how far this proposition [the mechanical theory] in truth is from being so postulated.

We must bear in mind that James was more interested in the ways people behave than in basic physics. Peirce was more on the side of objective reality, to be approached in successive approximations, as we see from the comparison of their thinking about free will.

Despite the hazard of over-interpreting what Peirce might have been thinking, I consider Peirce's comment to be a significant step to the recognition that in classical physics a system can lose memory of initial conditions. That surely would include memory of any programmed instruction about what I am supposed to do next. James's "tragic zest" of life remains a deep puzzle, but I avoid further discussion by confining attention to the far simpler issue of what we learn from tests of basic physics in controlled situations, along the lines of Peirce's discussion of measurements of the speed of light.

The philosopher Ferdinand Canning Scott Schiller (1910, page 89) was even less enthusiastic than James about Peirce's position. Schiller wrote that

> The 'independence' claimed for truth loses all meaning when its ambiguities are analysed. If 'independent' means 'wholly unaffected by,' it stands to reason that truth cannot be independent of us. Two strictly independent things could not co-exist in the same universe. Nor again can truth be 'independent' in the sense of 'unrelated'; for how in that case could we know it? Truth is meaningless if it does not imply a twofold relation, to a person *to whom* it is true, and to an object *about which* it is true. Any 'independence' which ignores either relation is impossible; any which is less than this, is not independence at all.

This is logical, and we have a new aspect of the situation. When I observe a system in a pure quantum state I might change it to another state. How does that affect the state vector of the universe, if there is such a thing? I place the question, with the meaning of free will, outside the bounds of this book.

Peirce's (1903) reaction to Schiller's remark was that

> Mr. Schilller does not believe there are any hard facts which remain true independently of what we may think about them. He admits it requires a hard struggle to make *all* facts suit our fancy, but he holds that facts change with every phase of experience, and that there are none which have been "all along" what history decides they shall have been. This doctrine he imagines is what Professor James means by the "will to believe." He is resolved that it shall have been so.

The problem in this exchange of positions seems to be the failure to recognize the two aspects of a physical theory. William James wrote that theories are "only approximations." Peirce agreed; we see it in the remark about free will. Peirce and James were right; our theories were then and are now approximations. But the best of them are excellent approximations with great predictive power. Peirce presented examples of this aspect of physics, examples that are hard to dismiss. We must consider the other aspect, however, that there are situations, then as now, in which our best theories fail or otherwise are incomplete. Maybe Schiller had in mind something like the incompleteness of theories, and maybe we can put a similar gloss on James's remark that it is "as if reality were made of ether, atoms or electrons, but we mustn't think so literally." Or maybe Schiller and James simply failed to grasp the significance of Peirce's point, that our better theories demonstrate great predictive power even though they are not exact.

Wright, Peirce, and James are considered to be founders of the philosophy of pragmatism.[10] Peirce (1878a, page 293) put it that

10. Peirce was one of the founders of pragmatism. He recalled (Hartshorne and Weiss 1934, Vol. 6, Book 2, Ch. 3, §5, Par. 483) using the term in private discussions, and expressing his thinking along this line in his discussion of the independent ways to measure the speed of light, though he used the term in his writings only later. Menand (2001, page 350) credits William James with bringing attention to Peirce's pragmatism, in a lecture at the University of California in Berkeley in 1898.

we should consider

> what effects, which might conceivably have practical bearings, we conceive the object of our conception to have. Then, our conception of these effects is the whole of our conception of the object.

William James (1907, page 45) asked:

> Is the world one or many?—fated or free?—material or spiritual?—here are notions either of which may or may not hold good of the world; and disputes over such notions are unending. The pragmatic method in such cases is to try to interpret each notion by tracing its respective practical consequences. What difference would it practically make to anyone if this notion rather than that notion were true? If no practical difference whatever can be traced, then the alternatives mean practically the same thing, and all dispute is idle. Whenever a dispute is serious, we ought to be able to show some practical difference that must follow from one side or the other's being right.

In his book, *The Metaphysical Club*, Menand (2001, page 351) wrote that

> Pragmatism is an account of the way people think—the way they come up with ideas, form beliefs, and reach decisions. What makes us decide to do one thing when we might do another thing instead? The question seems unanswerable, since life presents us with so many choices. . . . Thinking is a free-form and boundless activity that leads us to outcomes we feel justified in calling true, or just, or moral.

In her book, *The American Pragmatists*, Misak (2013) put it that

> Pragmatists are empiricists in that they require beliefs to be linked to experience. They want their explanations and ontology down-to-earth (natural as opposed to supernatural) and they require philosophical theories to arise out of our practices. . . . The nature of the requisite connections between beliefs and experience is a complex matter for the pragmatists . . . [among other reasons because] we need to assume things if we are to go on with our practices.

And among these complex matters for pragmatists was the division between Peirce, who expressed the hope that we can find good approximations to objective facts, and James, who does not seem to have been so sure about that.

My summary impression is that the pragmatism philosophy amounts to the pragmatic acceptance of the world as we observe it. By this definition I find that I have been a pragmatist.[11]

A contribution to divisions of thinking about reality was the advances in physics in the second half of the nineteenth century and continuing through the twentieth. Some found them exciting, but others at the time were disconcerted, naturally enough. Maxwell's replacement of the mechanical picture of the ether by the electromagnetic field was a distinct departure from the familiar laws of mechanics. Boltzmann's account of the second law of thermodynamics, the increase of entropy, applied familiar notions of mechanics, but to a hypothetical particle, the atom, in what may have seemed to be an abstract statistical version of mechanics. Thomson's demonstration of the deflections of electrons by electric and magnetic fields introduced evidence of an unexpected particle to replace the electric fluid, and it established the ratio of the electron charge to its mass. Millikan's 1909 experiment was soon to give evidence of the unique value of the charge of the electron. And consider Max Planck's (1900) introduction of quantum physics.

The Peirce side of the spectrum in the pragmatism camp would find all these developments exciting, but perhaps they added to the less than secure feelings about the ether, atoms, and electrons expressed by James on the other side of the pragmatism spectrum. James may have been wondering whether those physicists really had a solid basis for what they were claiming.

11. In the literature of philosophy you will find the argument that Peirce was an anti-realist because, as Hacking (1983) put it, Peirce was content to allow "the real and the true" to be whatever "information and reasoning would finally result in." The Stanford Encyclopedia of Philosophy is a valuable guide to such considerations, freely available if you have access to the internet. The entry on Scientific Realism (Chakravartty 2017) explains that this philosophy recommends acceptance of "both observable and unobservable aspects of the world described by the sciences." Anti-realists would argue for a more cautious, pragmatic, approach to our theories. This subtle distinction is interesting but the term "anti-realism" is likely to confuse a pragmatic scientist who would take a theory that passes an abundance of tests to be reasonably close to realistic. I avoid use of the terms "realist" and "anti-realist" in the philosophers' technical sense.

While physical science has grown far richer in the past century some things remain the same. In the concluding chapter of *The American Pragmatists*, on current pragmatist thinking, Misak (2013) remarks that

> two kinds of pragmatism emerge. One kind tries to retain a place for objectivity and for our aspiration to get things right while the other is not nearly so committed to that.

Peirce, a century ago, fits in the first of these present-day camps; James, the second.

The Austrian physicist and philosopher Ernst Mach occupied a special place in the spectrum of empiricism, because he had a clear and sharp understanding of physical science, and made important contributions to the study of transonic and supersonic flows. But Mach wanted nothing to do with the "disproportionate formal development of physics" (Mach 1902, page 505) in the more speculative theories that Peirce was willing to consider.

Mach's thinking about this is expressed in his book, *The Science of Mechanics*. (I refer to the Mach 1902 revised and enlarged English translation of the German edition, Mach 1883.) This book presents clear and instructive discussions of what we now term classical mechanics. When I taught introductory physics to university undergraduate students I showed them working models that demonstrate physical effects. I had a ready supply accumulated from many years of these demonstrations, but I added several good ones from Mach's descriptions in my 1960 copy of the 6th edition of the English translation. My favorite is the demonstration of the conservation of angular momentum pictured on page 302 in Mach (1902; page 391 in Mach 1960).

Though Mach clearly understood the physics of the time, he had little patience for the abstract side that might be considered to border on the metaphysics he considered idle speculation. To Mach the idea that motion has an absolute meaning is a metaphysical obscurity; velocity and acceleration surely are meaningful only relative to the rest of matter. The thought became known as Mach's principle. It inspired Einstein's Cosmological Principle, the assumption that the universe is the same everywhere, apart from local irregularities such as stars and planets and people. The story of how Mach's arguments led Einstein to this idea, and how the idea eventually was found to pass the test of a considerable network of evidence, is the subject of Chapter 4.

Mach was willing to consider speculative ideas. On page 493 he wrote that

> A thinking being is supposed to live in the surface of a sphere, with no other kind of space to institute comparisons with. His space will appear to him similarly constituted throughout. He might regard it as infinite, and could only be convinced of the contrary by experience. Starting from any two points of a great circle of the sphere and proceeding at right angles thereto on other great circles, he could hardly expect that the circles last mentioned would intersect. So, also, with respect to the space in which we live, only experience can decide whether it is finite, whether parallel lines intersect in it, or the like. The significance of this elucidation can scarcely be overrated. An enlightenment similar to that which Riemann inaugurated in science was produced in the mind of humanity at large, as regards the surface of the earth, by the discoveries of the first circumnavigators.

Mach also was quite aware of the importance of research that might call for revisions of facts. He wrote (on page 79) of an observer who has the

> opportunity to take note of some *new* aspects of the facts before him—of some aspect which former observers had not considered. A rule, reached by the observation of facts, cannot possibly embrace the *entire* fact, in all its infinite wealth, in all its inexhaustible manifoldness; on the contrary, it can furnish only a rough *outline* of the fact, one-sidedly emphasising the feature that is of importance for the given technical (or scientific) aim in view. *What* aspects of a fact are taken notice of, will consequently depend upon circumstances, or even on the caprice of the observer. Hence there always is opportunity for the discovery of new aspects of the fact, which will lead to the establishment of new rules of equal validity with, or superior to, the old.

It is an empiricist's refrain: ideas are only as good as the evidence.

Mach understood the power of theories but saw a restricted role for them. He wrote (on page 481 in Mach 1902) 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.

Thus, while Ludwig Boltzmann and others were making good use of the concept of atoms, Mach wrote (on page 492) 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 was referring to the observation that the distance d a freely moving body falls in time t starting from rest is proportional to the square of the time (that is, $d = gt^2/2$, where g is the local acceleration of gravity). The "circular functions" (sines and cosines, as in $\cos(\omega t)$) describe the displacement of a simple oscillator as a function of time. This still is a widely used model in physics. As Mach wrote, a fall by the square of the time, a simple oscillator, and an atom are useful constructs in theories. The first two are are not meant to be considered real, they are only helpful models. So Mach was asking a good question: why take atoms to be real, as opposed to yet another helpful model?

In another way to put it, Mach protested against

> the disproportionate formal development of physics [that has led] the majority of natural inquirers [to] ascribe to the intellectual implements of physics, to the concepts mass, force, atom, and so forth, whose sole office is to revive economically arranged experiences, a reality beyond and independent of thought. . . . A person who knew the world only through the theatre, if brought behind the scenes and permitted to view

> the mechanism of the stage's action, might possibly believe that the real world also was in need of a machine-room, and that if this were once thoroughly explored, we should know all. Similarly, we, too, should beware lest the *intellectual* machinery, employed in the representation of the world *on the stage of thought*, be regarded as the basis of the real world.

I admire Mach's analogy, but remain puzzled by his refusal to be impressed, or maybe it was his refusal to admit to being impressed, by the predictive power of what he termed the "economically arranged experiences." Mach's economy of science, also known as our suite of physical theories, predicts new phenomena that are found to pass empirical tests. In Mach's time this was clear enough to Peirce, and now we can cite many more examples such as the one Holton (1988) mentioned.

My empiricist tendencies lead me to sympathize with Mach's dislike of "disproportionate formal development of physics," but Mach's thinking was not prescient. Formal developments gave us the wonderfully productive principles of quantum and relativity physics.

Others shared Mach's thinking. On the subject of electricity and magnetism, Mach's (1902 page 494) opinion was that

> Our conceptions of electricity fit in at once with the electrical phenomena, and take almost spontaneously the familiar course, the moment we note that things take place as if attracting and repelling fluids moved on the surface of the conductors. But these mental expedients have nothing whatever to do with the phenomenon *itself*.

Auguste Comte, who was of the generation before Peirce and Mach, argued for "positivisme scientifique," along lines quite similar to Mach's later thinking. Comte asked (in Comte 1896, the later translation to English)

> What scientific use can there be in fantastic notions about fluids and imaginary ethers, which are to account for phenomena of heat, light, electricity and magnetism? . . . These fluids are supposed to be invisible, intangible, even imponderable, and to be inseparable from the substances which they actuate. Their very definition shows them to have no place in real science; for the question of their existence is not a subject

> for judgment: it can no more be denied then affirmed: our reason has no grasp of them at all. Those who in our day, believe in caloric, in a luminous ether, or electric fluids, have no right to despise the elementary spirits of Paracelsus, or to refuse to admit angels and genii.

The American philosopher John Dewey (1903) put it that

> not every hypothesis can be actually experienced. For example, one employs in physics the hypothesis of electric fluid, but does not expect actually to meet with it.

We have seen that James expressed a similar sentiment. These authors put an important question: is the existence of electric fluids a subject for judgment?

The scientific community has a standard way to answer the question; the case for or against the existence of hypothetical objects such as atoms and electric fluids is made by checking predictions against measurements. Mach understood the physical science involved, but he refused to recognize the idea of tests by predictions. Consider Mach's (1902 page 599) argument that

> Even if an hypothesis were fully competent to reproduce a given department of natural phenomena, say, the phenomena of heat, we should, by accepting it, only substitute for the actual relations between the mechanical and thermal processes, the hypothesis. The real fundamental facts are replaced by an equally large number of hypotheses, which is certainly no gain.

As this is stated, the theory of heat—thermodynamics—would be a circular construction: the theory fits what is observed because it is constructed to fit what is observed. But there is more to the situation than this; Maxwell's relations, the predictions of relations among useful thermodynamic quantities, are a real and useful gain. But let us consider an example that is more direct and counter to Mach's thinking on this point.

Mach (1902 pages 157, 231, 302) explained how the Newtonian physics of mechanics and gravity account for the motions of the planets, the motions of the moons around their planets, the flattened shape of the rotating earth, ocean tides and trade winds, the rotation of the plane of Foucault's pendulum, and the operation of a pendulum clock. This is a wonderfully diverse variety of phenomena, operating on distance scales that range from roughly 10^{13} cm for the paths of the planets down to a few centimeters for a pendulum. All are fit by the brief statements of Newtonian physics. Mach termed this an example of the economy of science. The more adventurous way to put it is that Newtonian physics seems to be a good approximation to how objective reality operates.

Peirce presented another example of this economy, without naming it, in the multiple ways to find the distance to the sun and the speed of light from astronomy and the study of electricity and magnetism. Mach knew all this physics. In the collection of *Popular Science Lectures* Mach (1898) explained measurements of the speed of light in the laboratory and from the timing of the orbits of the moons of Jupiter. Mach also remarked that the

> current which by the magnetic C. G. S. [centimeter, gram, second] standard represents the unit, would require a flow of some 30,000,000,000 electrostatic units per second through its cross-section. Accordingly, different units must be adopted here. The development of this point, however, lies beyond my present task.

This is the speed of light, in units of centimeters per second, that relates laboratory electrostatic and magnetostatic units. It also determines the speed of propagation of Herz's waves. Mach understood the physics, and in these lectures was making the same point as Peirce, but putting a less enthusiastic gloss on it.

Mach's economy of science was then and remains so commonplace that we might fail to notice that it is a remarkable phenomenon. The world around us is observed to be operating by rules we have discovered in approximations that are predictive and unify broad ranges of phenomena. It is remarkable too that although Mach certainly understood this property of experience he made no attempt to understand what it might signify, whether there might be something to learn from it. This economy, also known as the predictive power of our better theories, allows the judgments that Comte felt are missing. The luminous ether Comte distrusted has been discarded in favor of the electromagnetic field that has proved to be far more predictive. The hypothetical electric fluid has become the electrons and ions of the predictive and well-tested theory of electromagnetism. Of course, it is far easier now to say that Mach's economy of physics would suggest that the physical theories in Mach's days were useful approximations to the way reality operates. Mach certainly would not venture this far; I expect he would have dismissed the thought as uninteresting metaphysics.

Peirce was willing to venture further into speculation, willing to entertain the abstract idea of atoms, though with particular attention to ideas that might be tested. This thinking is summarized in his remark that[12]

> I am a physicist and a chemist, and as such eager to push investigation in the direction of a better acquaintance with the minute anatomy and physiology of matter. What led me into these metaphysical speculations, to which I had not before been inclined, I being up to that time mainly a student of the methods of science, was my asking myself, how are we ever going to find out anything more than we now [know] about molecules and atoms? How shall we lay out a broad plan for any further grand advance?

We still ask ourselves, how shall we plan for the next grand advance?

A century ago, recognition of the considerable range of tests and practical applications of mechanics, gravity theory, electromagnetism, and thermodynamics encouraged some to take a more adventurous line of thought, that physical science might be advanced enough to be formulated in a precise logical way. The sixth of David Hilbert's (1900) famous mathematical problems presented at the 1900 *International Congress of Mathematicians* is the "Mathematical treatment of the axioms of physics." Hilbert's explanation begins

> The investigations on the foundations of geometry suggest the problem: To treat in the same manner, by means of axioms, those physical sciences in which mathematics plays an important part; in the first rank are the theories of probabilities and mechanics.

The goal of the Vienna Circle of people who were inspired by Mach's thinking was even broader. They sought a unified system of mathematics, philosophy, physics, and the social sciences,

12. Hartshorne and Weiss 1934, Vol. 7, Book 3, Ch. 3, §7, Par. 506

organized in a strictly logical way on the basis of direct empirical facts. Philipp Frank (1949), who became a member of the Vienna Circle, recalls his early thinking as he was moving toward this vision. In the years around 1907

> I used to associate with a group of students who assembled every Thursday night in one of the old Viennese coffee houses. We stayed until midnight and even later, discussing problems of science and philosophy. . . . At the turn of the century the decline of mechanistic physics was accompanied by a belief that the scientific method itself had failed to give us the "truth about the universe"

Frank recalls that they were fascinated by the assessment of the evolving situation in natural science by the philosopher of science Abel Rey, in the book *La théorie de la physique chez les physiciens contemporains* (Rey 1907). In Frank's translation, Rey argued that

> Science became nothing but a symbolic pattern, a frame of reference. Moreover, since this frame of reference varied according to the school of thought, it was soon discovered that actually nothing was referred that had not previously been fashioned in such a way that it could be so referred.

How to remedy this circular construction? Frank recalls that

> We agreed with Abel Rey's characterization of Poincaré's contribution as a "new positivism" which was a definite improvement over the positivism of Comte and Mill.

Poincaré (1902, page 127) wrote that[13]

> Experience is the sole source of truth: it alone can teach us something new; it alone can give us certainty.

The emphasis on experience rather than a priori knowledge was central to the thinking of Wright and Peirce, and that of Mach and the Vienna Circle. But Poincaré was cautious about how far experience can take us. He remarked that Mariotte's law (also known as Boyle's law) is quite accurate for some gases, but the gas examined in sufficient detail breaks up into the chaotic motions of particles.

13. I use Halsted's (1913) translation.

Poincaré (1902, page 132) suggested that gravity examined in fine enough detail similarly might depart from the simplicity of Newton's law.

> No doubt, if our means of investigation should become more and more penetrating, we should discover the simple under the complex, then the complex under the simple, then again the simple under the complex, and so on, without our being able to foresee what will be the last term.

Poincaré's vision, which I characterize as successive approximations all the way down, seems as plausible as the talk of a final theory. Poincaré (1905, page 14) also asked,

> Does the harmony the human intelligence thinks it discovers in nature exist outside of this intelligence? No, beyond doubt a reality completely independent of the mind which conceives it, sees or feels it, is an impossibility. A world as exterior as that, even if it existed, would for us be forever inaccessible.

We have seen similar thoughts by Schiller (1910). The logic is impeccable, the meaning still open to debate.

Rudolf Carnap (1963), who became another member of the Vienna Circle, recalls his earlier thinking.

> I imagined the ideal system of physics as consisting of three volumes: The first was to contain the basic physical laws, represented as a formal axiom system; the second to contain the phenomenal-physical dictionary, that is to say, the rules of correspondence between observable qualities and physical magnitudes; the third to contain descriptions of the physical state of the universe

This is Hilbert's style, but I think not Poincaré's.

The manifesto, *Wissenschaftliche Weltauffassung Der Wiener Kreis*, or *The Scientific World View of the Vienna Circle* (Hahn, Neurath, and Carnap 1929), gives us a further impression of their approach. It mentions inspiring thinkers, including Bertrand Russell and Ludwig Wittgenstein on the side of mathematics and philosophy, and Ernst Mach and Albert Einstein on the side of physics. Poincaré and Mach were more directly influential in how members of the Vienna Circle thought about physical science.

The concluding paragraph in the manifesto in the section on the Fundamentals of Physics is particularly relevant for our considerations.

> Through the application of the *axiomatic method* to the problems mentioned, the empirical components of science differ from the merely conventional, the meaningfulness from the definition. There is no room left for a synthetic judgment *a priori*. The fact that knowledge of the world is possible is not based on the fact that human reason imprints its form on the material, but on the fact that the material is ordered in a certain way. Nothing can be known from the start about the type and degree of this order. The world could be organized much more than it is; it could, however, also be much less ordered but still understandable. Only the pressing step-by-step research of empirical science can teach us the lawful nature of the world. The method of *Induction*, the conclusion from yesterday to tomorrow, from here to there, is of course only valid if there is a law. But this method is not based on an *a priori* premise of this law. It may be used wherever it leads to fruitful results, whether sufficient or insufficiently justified; it never grants security. But epistemological reflection demands that an induction conclusion is only given meaning insofar as it can be verified empirically. The scientific view of the world will not reject the success of a research work because it has been achieved with inadequate, logically insufficiently clarified or empirically inadequately justified methods. However, it will always strive for and demand verification through thoroughly firm aids, namely the indirect or direct tracing back to experiences.

In the foreword to this document the authors are named as Hans Hahn, Otto Neurath, and Rudolf Carnap. Sigmund (2017) mentions more contributors, so it is not surprising to see expressions of a variety of thoughts in this paragraph.

I take the paragraph to be a statement of what is termed logical positivism, or logical empiricism. The philosophy is no longer considered interesting by some authorities, but I see things to like about these statements. The Vienna Circle seeks the “lawful nature of the world,” as we still do. They recognize that it is an assumption that “there is a law.” With Poincaré they emphasize that a law requires “verification through . . . the indirect or direct tracing back to experiences." Perhaps they were more confident than Poincaré that the law can be discovered, rather than approached in successive approximations. With Peirce, and the Vienna Circle, we continue to assume there is a lawfully acting reality so we can hope to discover its laws. In short, this paragraph is a reasonable statement of a good part of current thinking about research in physical science. I see two missing points.

First, Carnap and others in the Vienna Circle proposed a "creative reconstruction" that would reduce the language of natural science to a strictly logical and manifestly meaningful system. Research in natural science cannot operate this way because we are attempting to discover the logic by which our assumed reality operates. Informal trial and error is inefficient, but it has served well in our learning how to frame the language that gave you cell phones, despite the incomplete nature of the language.

The second missing point is the importance of empirical tests of theoretical predictions, tests of extrapolations beyond the range of evidence used to construct a theory. We have seen Perice's examples of the impressive predictive power of electromagnetic and Newtonian physics. Why this predictive power? It is what you would expect if these theories were useful approximations to the way reality operates, useful enough to allow extrapolation to different situations. I have not found arguments in the document for or against this thought; perhaps it would have been considered an example of the metaphysical thinking Mach so disliked and members of the Vienna Circle sought to replace with a logical arrangement of the empirical facts, and only the empirical facts.

An approach similar in spirit to the sixth of Hilbert's mathematical problems, and the thinking taken up by the Vienna Circle, is the search for a unified physical theory, maybe beginning with the unified field theory of gravitation and electromagnetism. Einstein and Hilbert looked for it; Kaluza (1921) and Klein (1926) found it in a space of five dimensions. But it adds nothing to either theory if we accept Klein's prescription for a tiny fixed length that closes the fifth dimension. Some argue that we might be on the threshold of finding the "theory of everything," or as Steven Weinberg (1992) puts it, "Dreams of a Final Theory," maybe along the lines of the Kaluza-Klein approach. Weinberg presents a careful and well-informed case; the final theory might serve as the axioms that Hilbert and others dreamed of a century ago. But, as Poincaré argued, we cannot empirically determine that we have the final theory, as opposed to the best approximation the world economy can afford to establish.

The philosopher Karl Popper was not a member of the Vienna Circle, but there are accounts of their mutual influence. Popper's philosophy is commonly remembered for the emphasis on falsifiability. In his autobiography Popper recalls being fascinated by Einstein's remark that, if specific predictions of relativity theory disagree with the measurements, then the theory is untenable. Popper (1974, page 29) recalls that

> I arrived, by the end of 1919, at the conclusion that the scientific attitude was the critical attitude, which did not look for verifications but for crucial tests; tests which could *refute* the theory tested, though they could never establish it.

Popper (1965, page 287) also considered confirmability:

> The severity of possible tests of a statement or a theory depends (among other factors) on the precision of its assertions and upon its predictive power. . . . The better a statement can be tested, the better it can be confirmed, i.e. attested by its tests.

An illustration of Popper's (1965, page 117) thinking about what tests might tell us about reality is his comment that

> Testable conjectures or guesses, at any rate, are thus conjectures or guesses about reality; from their uncertain or conjectural character it only follows that our knowledge concerning the reality they describe is uncertain or conjectural. And although only that is certainly real which can be known with certainty, it is a mistake to think that only that is real which is known to be certainly real. We are not omniscient and, no doubt, much is real that is unknown to us all.

And let us take note of Popper's (1945, page 208) opinion of the influence of society on science:

> It was one of the greatest achievements of our time when Einstein showed that, in the light of experience, we may question and revise our presuppositions regarding even space and time, ideas which had been held to be necessary presuppositions of all science and to belong to its 'categorial apparatus.' Thus the sceptical attack upon science launched by the sociology of knowledge breaks down in the light of scientific method. The empirical method has proved to be quite capable of taking care of itself.

Let us turn to the considerations of the sociology of scientific knowledge.

1.2 On Social, Empirical, and Circular Constructions

Thomas Kuhn's thinking about constructions in natural science is presented in the book *The Structure of Scientific Revolutions*. Kuhn wrote, in the postscript in the second edition (Kuhn 1970, page 206), that

> A scientific theory is usually felt to be better than its predecessors not only in the sense that it is a better instrument for discovering and solving puzzles but also because it is somehow a better representation of what nature is really like. One often hears that successive theories grow ever closer to, or approximate more and more closely to, the truth. Apparently generalizations like that refer not to the puzzle-solutions and the concrete predictions derived from a theory but rather to its ontology, to the match, that is, between the entities with which the theory populates nature and what is "really there." Perhaps there is some other way of salvaging the notion of 'truth' for application to whole theories, but this one will not do. 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.

Physicists have a regrettable tendency to declare that we are at last approaching the final theory, the discovery of the ultimate nature of reality. But physics cannot do that, because all empirical tests have limited accuracy. This means our world picture, our ontology, must evolve with the advance of science. Kuhn was right; we can only claim to establish persuasive cases for approximations to an assumed reality. In the following chapters I argue for some quite persuasive examples.[14]

Consider also the thought in an extract Galison (2016) drew from Kuhn's correspondence:

> 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.

This is a good description of the circular constructions of theories that fit our perceptions of the evidence because they were designed to fit our perceptions. It is a standard and important part of research in the natural sciences; a more charitable way to put it is that we attempt to follow the evidence. The follow-up stage is the search for checks of a theory thus constructed by what follows from the theory that was not part of the perceptions that went into its construction. The tests of predictions have been an obsession among physical scientists for a long time.

General relativity and relativistic cosmology pass the broad range of demanding tests of predictions to be discussed in following chapters. So what would this signify in Kuhn's philosophy? He had a PhD in physics, a doctoral dissertation with van Vleck (Kuhn and van Vleck 1950), but later Kuhn displayed little interest in the experimental side of physics. It is illustrated by what Kuhn wrote (on page 61 in *The Structure of Scientific Revolutions*) about

14. David I. Kaiser, in a private communication, writes that "Kuhn was particularly concerned about apparent mismatches between the sets of entities that various theories described as being part of reality. For example, in Maxwell's day there was no empirical evidence for fundamental, microscopic charge-carriers with fixed electric charge—what would later be identified as electrons [and ions]. Yet calculations in quantum electrodynamics begin from an assumption that electrons exist and are among the fundamental entities responsible for electromagnetic phenomena. Given the very different assumptions about what the world is ultimately made of—and the equally striking fact that theoretical predictions from Maxwell's theory and from quantum electrodynamics are compatible in various limits—some philosophers of science have developed arguments for "structural realism": perhaps natural science reveals relationships that really hold in nature, even if theories make incompatible claims about specific theoretical entities. See the entry on "Structural Realism" in the Stanford Encyclopedia of Philosophy (Ladyman 2020)"

(continued...)

INDEX