The Whole Truth

A COSMOLOGIST'S REFLECTIONS ON THE SEARCH FOR OBJECTIVE REALITY

P. J. E. Peebles

WINNER OF THE NOBEL PRIZE IN PHYSICS

Published by Princeton University Press
41 William Street, Princeton, New Jersey 08540
99 Banbury Road, Oxford OX2 6JX

press.princeton.edu

Library of Congress Cataloging-in-Publication Data

Names: Peebles, P. J. E. (Phillip James Edwin), author.
Title: The whole truth: a cosmologist's reflections on the search for objective reality / P. J. E. Peebles.
Description: Princeton: Princeton University Press, [2022] | Includes bibliographical references and index.
Identifiers: LCCN 2021051638 (print) | LCCN 2021051639 (ebook) | ISBN 9780691231358 (hardback) | ISBN 9780691231365 (ebook)
Subjects: LCSH: Science—Philosophy. | Physics. | Cosmology. | Reality. | BISAC: SCIENCE / Space Science / Cosmology | SCIENCE / History
Classification: LCC Q175.P372 2022 (print) | LCC Q175 (ebook) | DDC 501—dc23/eng/20220128
LC record available at https://lccn.loc.gov/2021051638
LC ebook record available at https://lccn.loc.gov/2021051639

Version 1.0

British Library Cataloging-in-Publication Data is available

Editorial: Ingrid Gnerlich and Whitney Rauenhorst
Production Editorial: Mark Bellis
Jacket Design: Chris Ferrante
Production: Danielle Amatucci
Publicity: Matthew Taylor and Kate Farquhar-Thomson
Copyeditor: Bhisham Bherwani

Jacket Credit: Galaxy orbits flowing out of voids and impinging on regions of high density. Constructed by Edward Shaya (University of Maryland), Brent Tully (University of Hawaii), Daniel Pomarede (University of Paris-Saclay), and Yehuda Hoffman (Hebrew University, Jerusalem)

CONTENTS

PREFACE

What is at the core of what we do in natural science? I say that we are searching for the nature of reality, but what does that mean? You will not find ready answers from most working scientists; they would rather work on the problems arising in the research at hand. But there are lines of thought drawn from the philosophy, sociology, and history of science that are good descriptions of what I see is at the heart of what we are doing in science. I mean to explain, with illustrations from cosmology, the theory of the expansion of the universe from a hot dense state.

I began to work on cosmology a half century ago, when ideas were speculative and evidence scant, and I have seen it grow into one of the best-established of the branches of physical science. The experience has led me to reflect on what my colleagues and I have been up to, and what we have learned, apart from how to work out more or less well posed problems as they arise. The result is presented in this book.

I mean this book to be accessible to those who are not familiar with the language of physics but are interested in what scientists are doing and why. Colleagues in science may be surprised to find me discussing aspects of sociology and philosophy, and the evidence from natural science for objective physical reality. These are not familiar topics of conversation in my crowd, but I think they are essential for a more complete picture of what we are learning from research in natural science. I am not attempting an assessment of thinking about reality in the philosophy and sociology of science, but I hope authorities in those fields will find it interesting to see how some ideas drawn from their disciplines find resonance with the practice of natural science.

Scientists have different ways of thinking about our subject. I am on the empiricist side of the spectrum; I love to see debates settled by measurements. But despite my empiricist inclination to confine attention to the state of the experimental or observational tests of physical theories, and leave it at that, I have been led to think that curiosity-driven science has produced good evidence of an abstract notion: objective reality. The notion as it is applied in science can be falsified, by failure of the scientific method. It can never be proved, because the accuracy of empirical evidence always is limited. The theme of this book is that the empirical results from natural science, presented in the worked example of physical cosmology, add up to a persuasive case for observer-independent reality. This is the best we can do in science.

How did we arrive at the present state of natural science, and the case for objective reality? A turning point in my thinking about this grew out of recognition of a commonplace experience in physics: when an interesting idea turns up the odds are fairly good that someone else has already remarked on it, or will, independently, if word does not travel fast enough. An example from another branch of science that even I have known for a long time is the independent recognition of the concept of evolution by natural selection, by Charles Darwin and Alfred Wallace. I have observed other examples in physical science since I was a graduate student, and until recently gave no thought to them. I don't imagine my experience is special in this regard, and indeed I have never heard a physicist remark that the occurrence of multiple discoveries is a phenomenon that could teach us something. But it dawned on me that the fact that Darwin and Wallace both hit on the idea of natural selection reenforces the case that this idea is sensible. After all, two people with feet on the ground independently noticed the evidence. In a similar way, the remarkably large number of multiple discoveries in explorations of the large-scale nature and evolution of the universe suggests that the evidence was offering sensible motivation for the directions in which our thinking was taking us.

My search for who recognized these multiples as a phenomenon to be considered in an assessment of natural science led me to sociologists. They recognize the phenomenon, and I saw that I had something to learn about the practice of physical science from books on sociology, and from that to philosophy, books I had never before thought to consult.

One result was my belated recognition that the sociologists' concept of constructions, social and empirical, accounts for a curious situation in a book I treasure, *The Classical Theory of Fields*, by Landau and Lifschitz (1951; my copy is the English translation of the 1948 second Russian edition). The first two-thirds of the book is a careful analysis of the basics of the classical theory of electricity and magnetism. This is a well tested and broadly useful theory. The last third of the book presents Einstein's general theory of relativity. I knew from the time I joined Bob Dicke's Gravity Research Group as a graduate student in 1958 that this theory had scant empirical support, quite unlike electromagnetism. So why was general relativity given near equal time to electromagnetism in *The Classical Theory of Fields*? The sociology of constructions reviewed in Section 2.2 led me to understand the situation. It is one of the themes of this book.

I am still relatively unschooled in sociology and philosophy, but see now that there are in these disciplines quite relevant things to say about the practice of science. For those of us who are not sociologists or philosophers, the lessons are best appreciated by application to specific experience of research in physical science. I think of it as a worked example, similar to the problems we set for students with given steps to the solution. Physical cosmology, the study of the large-scale nature of the universe around us, is a good choice for a worked example of research in physical science. The subject in its modern form began with a reasonably clear set of starting ideas a century ago; the pursuit of these ideas involved interesting episodes of confusion and discovery which led to a satisfying conclusion. The tests of this theory are abundant enough, and well enough checked for reliability, to make what most physical scientists who have given it thought agree is a persuasively established case that we have a good approximation to what actually happened as our universe expanded and cooled.

I have to caution that this standard and accepted theory cannot be trusted in an extrapolation forward in time to the remote future, or back in time to arbitrarily large densities. There are interesting ideas about what is going to happen—you will hear talk of the big crunch, the big freeze, and the big rip—and ideas about what was happening before the big bang, whatever that means. But such speculations are beyond the scope of this study of lessons to be drawn from how an example of physical science got to be generally accepted. For the purpose of this book let us be content with the theory of what left the fossils we have been able to identify and interpret.

All natural science is fascinating for those of us with a taste for it. A lot more is added to the case for objective reality from the broad range of well-tested predictions of quantum physics. I recommend the discussion in Steven Weinberg's (1992) book, *Dreams of a Final Theory: The Search for the Fundamental Laws of Nature.* But what is learned from relativity physics and the establishment of the relativistic theory of the evolving universe serves as well to illustrate the argument that we are finding operationally useful approximations to the abstract idea of objective reality. The history and physics of cosmology are simpler, but I see no other reason to favor this example over topics in quantum physics.

The properties of products of living matter play a part in the present story, as examples of the issues that attend the study of complex physical systems. The usual presumption is that living systems are manifestations of the operation of the same objective reality that the evidence from tests of simpler systems indicates is usefully approximated by relativity and quantum physics. But we must live with the fact that a clear demonstration that this is so is beyond present methods of analysis.

There cannot be a universal guide to the nature of science, because operating conditions determine what can be done, and conditions are vastly different in different branches of science. But we can draw useful lessons from a specific worked example, here physical cosmology. It helps that this is a relatively simple branch of natural science. It helps too that I know the subject well; I have been working on it and seen it grow for over half a century.

The story of the development of physical cosmology, presented here in Chapters 3 to 7, is meant to illustrate three things. First, it is an example how physical science is done. Second, it is a guide to thinking drawn from what sociologists and philosophers have been saying we are doing that is particularly relevant from scientists' points of view. Other ideas from these disciplines do not seem so directly relevant, of course. Third, it shows the nature of the argument for objective physical reality that is drawn from the practice of natural science.

I start this story with considerations of the history of thoughts about the nature of the physical sciences tracing back over the past century, to the time when Einstein was musing about the possible nature of a satisfactory theory of gravity and a logically constructed universe. More is to be learned from the still earlier history of science, but coherence is aided by confining attention to later, more directly relevant, developments. Even this limited span of history is rich; I can only offer samples that illustrate ideas that I believe have influenced the way we now think of physical theories within the cultures of science and society. I venture to draw from these examples, and my own experience, a succinct statement of the working assumptions of physics: the ideas that are the fundamental basis for what we do in this subject.

These working assumptions are introduced in Chapter 1 and listed and discussed at the end of the chapter. They are centered on the idea, which is an assumption, that our better theories are useful approximations to objective reality. To some the notion of objective reality is obvious, to others doubtful. My examples in support of the first opinion are drawn from developments in the relativistic theory of the large-scale nature of the universe. As I said, there are many more supporting examples from quantum physics, but the examples from relativity serve.

The social nature of physical science is an important part of scientists' culture. This is recognized more clearly by historians, philosophers, and sociologists than by physical scientists, which should be remedied. I offer the example of how the sociology of physical science and our implicit working assumptions for research in this subject played out in the development and reception of Einstein's general theory of relativity, its eventual establishment by demanding tests, and the development of ideas about its application to the theory of the large-scale nature of the universe. For this purpose, research tends to be most informative in the opening moves. Developments after a particular line of research has been generally adopted as promising tend to be more direct and less instructive, unless the approach fails. I only briefly summarize closing moves on ideas that have proved to be successful so far.

Explanations of nomenclature are needed. I sometimes write about physics, physical science, natural science, or science. I mean by the first the exploration of phenomena that are simple enough to serve as tests of physical theories that are meant to be useful approximations to what we assume is some kind of objective and rationally operating reality. A physical science such as chemistry adds to physics the regularities and theories of phenomenology that are too complex for ready deduction from the approximations to objective reality that physics is claiming to have found. I assume the phenomena of chemistry rest on the same reality as physics, but testing that is challenging. The natural sciences go on from chemistry though geology to botany and biology and on to the nature of living things and the functioning of the human mind. This is an ordering by increasing degree of complexity. I emphasize that it is not meant to be an ordering by merit or interest, in either direction; all are worthy and fascinating examinations of the world around us. As I said, I take cosmology for a worked example of how natural science operates because it is relatively simple and the interaction of theory and observation is fairly easy to judge.

The subject of study in the natural sciences often is called "nature," or "Nature." I use "reality" so I can write "the nature of reality." The few exceptions I take to this rule seem to be demanded by the context. I often use "objective reality," which is redundant but adds emphasis. We have ready ideas about Nature, or reality; it's what we see it around us, it's in your face. I take it that the chair I am sitting on is real, since it seems silly to think I am dreaming. Natural science implicitly operates on the same assumption, that the nature of the world is independent of our ideas about it. Society influences our thinking, but we assume consultation of the empirical evidence corrects misleading thoughts.

I avoid the word "belief" because of the religious connotation, and the words "proof," "demonstration," and "verification," because they seem overly confident for natural science. I use the word "fact" because it appears in interesting discussions to be reviewed, but I mean it with the understanding that in this subject a fact is an approximation, only as good as the supporting data. The same applies to "true," though the word appears uncomfortably often in this text because it is so convenient. I favor "indication" for a reasonable interpretation of an observation, and the awkward but I think accurate terms, "persuasively established" and "a persuasive case," to apply to theories that pass enough tests of predictions to persuade the community to accept the theory, always pending the recognition of new evidence that might force the search for an even better theory. The more abundant the successful predictions, the more persuasive the case for establishment, of course. I use the phrase "community assessment" for ideas that the research community by and large has decided to accept as reasonable, and "standard and accepted" for community ideas that are so well fixed in our minds as to be canonical. The word "canonical" connotes a permanently established case, but of course in science this is not necessarily so.

A "theory" usually is taken to be more completely prescribed and tested than a "model," which tends to be more schematic and speculative. But ideas in cosmology have tended to be intermediate situations. I follow usual practice by taking the words "theory" and "model" to be interchangeable, unless the context suggests particularly schematic ideas that are best characterized as "models." I use the term "physical cosmology" for the theory and observation of the large-scale nature of the universe, the modifier "physical" applied because there are many other kinds of cosmologies.

Space is filled with a near uniform distribution of microwave radiation that is convincingly established to be a remnant, a fossil, from the early universe. Its standard name is the cosmic microwave background, or CMB. I am uneasy about this because the wavelengths of this radiation were much shorter in the past, and while the radiation is a background for us on Earth, coming to us from the sky, it is more properly termed a sea that near uniformly fills space. I call it the "sea of fossil radiation."

The spectra of galaxies are shifted toward the red by the motions of the galaxies away from us. This motion used to be known as the general recession of the galaxies. An observer in another galaxy would see the same general recession; there is no special position in space in the standard cosmology. On average, apart from the motions of galaxies relative to each other produced by local variations of the pull of gravity, the distance between two galaxies, measured by standard rulers and clocks, is increasing at a rate that is proportional to the distance between the two. This is Hubble's law, and the constant of proportionality is Hubble's constant, H_o. (That is, the velocity v of recession of a galaxy at distance r is $v = H_o r$.) The International Astronomical Union has determined that this relation shall be known as the

Hubble-Lemaître law, because Hermann Weyl anticipated it, and Georges Lemaître found a direct prediction, before Edwin Hubble recognized astronomical evidence for the relation. But for brevity I use the old name.

It is sometimes said that space is expanding, but this can be confusing. You and I are not expanding, beyond the usual biological effects, and the galaxies are not expanding, apart from effects of gain of mass by accretion and loss by winds. Best leave it that the spaces between galaxies are increasing, that is, on average the galaxies are moving apart.

Newtonian gravity physics is the nonrelativistic limit of Einstein's general theory of relativity. I use Einstein's term for his theory where it seems appropriate, but more often fall back on the shorter version, general relativity.

The familiar name for the relativistic theory of the expanding universe is the big bang. Simon Mitton (2005) attributes the name, big bang, to Fred Hoyle. The name is not appropriate, because a "bang" connotes an event at a particular place and time. The well-tested cosmology describes a near homogeneous universe, one with no observable edge and no particular place or preferred center, quite unlike a "bang." And cosmology does not deal with an event: it describes the evolution of the universe to its present state from an exceedingly dense, and we now say hot, rapidly expanding early condition. But the name "big bang" is so fixed in people's minds that I use it.

I conclude explanations in this punctilious mode with the difference between precise and accurate measurements. It is relevant for the discussion of the tests of the present standard cosmology, including those reviewed in Section 6.10. Suppose I measure the length of an object many times and find that the average value of the measurements is $L = 1.11 \pm 0.01$ cm. The uncertainty in each measurement is reduced by averaging over many trials. In this example the uncertainty that remains after averaging is give or take about 0.01 cm. But measurements inevitably are affected by systematic errors. Maybe the measuring instrument was slightly bent when I dropped it. Maybe its calibration is a little off. Let us say that my best judgment of the effects of all the systematic errors brings the result to $L = 1.2 \pm 0.1$ cm. The first result, $L = 1.11 \pm 0.01$ cm, is more precise. The second, $L = 1.2 \pm 0.1$ cm, is more accurate. I have been disconcerted to encounter accomplished theorists who fail to grasp the difference. The difference is important.

It is important too to be as accurate as possible about what people have been thinking and doing in this subject. I offer many quotations from authors; better their words than mine. I have been schooled since graduate school days to respect data, and to me quotations are data. Some quotes may be misleading, and other forms of data may be misleading too, but on average data are informative and to be treated with respect. That makes me uneasy about presentations of translations of quotations from German or French. I use published translations when available, and otherwise my translations aided by Google to assist my rudimentary memory of these languages. Something is lost in translation, but I suppose we must live with it.

I include references to the academic literature for those who might like to look into the evidence that supports my claims, or maybe just to be reassured that there is evidence. References to *The Collected Papers of Albert Einstein* (Stachel, Klein, Schulman et al. 1987) in footnotes give the volume and document numbers, available at https://einsteinpapers.press.princeton.edu

Cheryl Misak's (2016) *Cambridge Pragmatism: From Peirce and James to Ramsey and Wittgenstein*, and Karl Sigmund's (2017) *Exact Thinking in Demented Times: The Vienna Circle and the Epic Quest for the Foundations of Science*, introduced me to the varieties of thinking about physics a century ago. Bruno Latour and Steve Woolgar's (1979, 1986) book, *Laboratory Life*, introduced me to the sociologists' arguments for the social aspects of research in natural science. Ernst Mach's *The Science of Mechanics*, in the 1902 and later translations to English, has

wonderfully informative discussions of what we now term classical mechanics. They still are well worth reading. Mach's critiques of what he termed the "disproportionate formal development of physics" present us with a fascinating puzzle: what was Mach thinking? I have learned from many other authorities as well, including those listed in the bibliography.

I am on unfamiliar ground in writing about the philosophy of science, always dangerous, and I am particularly grateful to Paul Hoyningen-Huene, David Kaiser, Krystyna Koczanski, Peter Koczanski, and Cheryl Misak for their reality checks of my thinking about it, though I certainly own the errors that remain. Peter Saulson is a physicist and member of the LIGO Scientific Collaboration that detected gravitational waves from merging astronomical objects. Peter observed a sociologist, Harry Collins, who was observing the physicists in this collaboration. I value Peter's advice from that experience and his thinking about sociology. The sociology of science is a subtle business, and I am grateful to Angela Creager, Regina Kennan, Janet Vertesi, and Harriet Zuckerman for enlightening discussions of their experiences. Michel Janssen, Jürgen Renn, and Cliff Will gave me authoritative advice about the origins of Einstein's general theory of relativity. I am grateful to Charles Robert O'Dell for recollections of research into the astronomy of helium abundances at a critical time for cosmology; Virginia Trimble for recollections of the measurements of gravitational redshifts of white dwarf stars that were important for early tests of general relativity; and Stanislas Leibler for discussion of the curious role of biophysical molecules in this story. I am grateful to Florian Beutler for making for me the critical bottom panel in the figure on page 173. Ingrid Gnerlich, Publisher for the Sciences at Princeton University Press, has given me productive advice on how to make this book more presentable, and she is the source of two reviews by competent readers. Their comments have improved the book, and in particular made me see the need for a firmer explanation of what I was trying to do. Alison Peebles and Lesley Peebles were valuable guides to a preface that explains the purpose of this book. One of my pleasures in writing it has been the considerations of thinking by these people and many others during my long career. In many different ways they were laying out ideas for me to follow.

I had the great good fortune to have landed at a university, Princeton, where I was able to teach physics to interested students and work with inspiring colleagues. Most important to me, and to quite a few others, was Professor of Physics Robert Henry Dicke, Bob to everyone who knew him. I arrived in Princeton from the University of Manitoba as a fresh graduate student in physics in 1958, aiming for research in particle physics. To my great good fortune a fellow Manitoban, Bob Moore, invited me to join him in attending Dicke's research group meetings on how to improve the empirical basis for gravity physics and relativity. Quite a few Nobel Prizes, including mine, grew out of Bob's group. They are a testimony to Bob's vision.

The central administration at Princeton University has not expressed concern about my spending so much time and effort on research that is not at all likely to be monetizable. I count it as an illustration of the value society places on curiosity-driven explorations of what is going on in the world around us, in places large and small.

An active scientist might consider spending a few evenings with this book, reading about what I think is at the core of what they have been doing, and comparing it to their own opinions, which I expect they will have formed even if they never paused to think about it. People who are interested in what is happening in physical science but are not schooled in the mathematical language of the subject may well have spent more time wondering about what we are doing than those of us in the trenches. I have aimed to make the discussion accessible and informative for this camp. There are very few equations in the text, a few more are sequestered in parentheses and footnotes, and I use footnotes to explain the minimal technical jargon that is difficult to avoid. Professional philosophers and sociologists of science will be familiar with the lessons I draw from the wealth of ideas in their subjects, but I hope they will be interested in seeing how I apply my selections to the experience of those of us with feet on the ground.

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

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 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.

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.