Cosmic Discovery

The Search, Scope, and Heritage of Astronomy

Martin Harwit's influential book, *Cosmic Discovery*, is rereleased after more than 35 years, with a new Preface written by the author. The work chronicles the astronomical discoveries up to the late twentieth century and draws conclusions that major discoveries have often been unexpected, unrelated to prevailing astronomical theories and made by outsiders from other fields. One trend alone seems to prevail: major discoveries follow major technological innovations in observational instruments. The author also examines discovery in terms of its political, financial, and sociological contexts including the role of industry and the military in enabling new technologies, and methods of funding. The challenges encountered by astronomy in the 1980s are remarkably similar to those astronomers face today. Difficulties persist in controlling recurrent cost overruns on planned missions, and in confronting mounting costs in developing observatories for detecting gravitational waves, high-energy cosmic rays, and particles that might explain dark matter.

Martin Harwit is Professor Emeritus of Astronomy at Cornell University. For many years he also served as Director of the National Air and Space Museum in Washington, D.C. For much of his astrophysical career he built instruments and made pioneering observations in infrared astronomy. His advanced textbook, *Astrophysical Concepts*, has taught several generations of astronomers through its four editions. Harwit has had an abiding interest in questions first raised in *Cosmic Discovery* on how science advances or is constrained by factors beyond the control of scientists. His subsequent book, *In Search of the True Universe*, explores how philosophical outlook, historical precedents, industrial progress, economic factors, and national priorities have affected our understanding of the Cosmos. Harwit is a recipient of the Astronomical Society of the Pacific's highest honor, the Bruce Medal, which commends "his original ideas, scholarship, and thoughtful advocacy".

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The Search, Scope, and Heritage of Astronomy

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CAMBRIDGE
UNIVERSITY PRESS

CAMBRIDGE
UNIVERSITY PRESS

University Printing House, Cambridge CB2 8BS, United Kingdom

One Liberty Plaza, 20th Floor, New York, NY 10006, USA

477 Williamstown Road, Port Melbourne, VIC 3207, Australia

314–321, 3rd Floor, Plot 3, Splendor Forum, Jasola District Centre,
New Delhi – 110025, India

79 Anson Road, #06–04/06, Singapore 079906

Cambridge University Press is part of the University of Cambridge.

It furthers the University's mission by disseminating knowledge in the pursuit of education, learning, and research at the highest international levels of excellence.

www.cambridge.org
Information on this title: www.cambridge.org/9781108722049
DOI: 10.1017/9781108655088

First published by Basic Books, Inc. 1981

Reissued by Cambridge University Press 2019

Printed in the United Kingdom by TJ International Ltd. Padstow Cornwall

A catalog record for this publication is available from the British Library.

Library of Congress Cataloging-in-Publication Data
Names: Harwit, Martin, 1931– author.
Title: Cosmic discovery : the search, scope, and heritage of astronomy / Martin Harwit (Cornell University, New York).
Description: [2019 edition]. | Cambridge ; New York, NY : Cambridge University Press, 2019. | Originally published: New York : Basic Books, 1981. | Includes bibliographical references and index.
Identifiers: LCCN 2018034174 | ISBN 9781108722049
Subjects: LCSH: Astronomy. | Astronomy – History.
Classification: LCC QB43.2 .H37 2019 | DDC 520–dc23
LC record available at https://lccn.loc.gov/2018034174

ISBN 978-1-108-72204-9 Paperback

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Cambridge University Press
978-1-108-72204-9 — Cosmic Discovery
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About the Book

When I was a student in college, just after World War II, my friends and I naturally wanted to know what fields of science held the greatest excitement, the largest number of potential discoveries. We wanted to participate in the scientific enterprise and needed to know where to start. Yet no one could guide us, except in the vaguest of ways.

At roughly the same time, I imagine, planners in Washington similarly were wondering what fields of science might promise immediate advances and striking returns—where incentives might be offered to young graduate students in the form of fellowships and where new facilities might be established for novel ventures.

Such questions still remain largely unanswered. Planning commissions and advisory boards still grapple with them and seem no closer to their goal than they were three decades ago.

Cosmic Discovery is a first attempt to collect the kind of information that might be needed to answer questions on the promise of a particular science. It restricts itself to one part of one discipline and asks, How was it that we first came to discover the major phenomena we now observe in the universe? Who were the individuals responsible for the discoveries? How had they prepared for their careers? What methods led to their successes? In a different vein, the book also asks, What is the scope of future astronomical discovery? How many major cosmic phenomena remain to be found? How much remains to be done? Finally we can take all the information we can gather and ask, Are there lessons we can learn from earlier searches? Can we plan future enterprises to make them more effective than our efforts of the past? Is an imposed national plan likely to be more successful than the striving of individual, motivated scientists?

I have attempted to answer these questions by collecting the information needed to arrive at well-informed conclusions. And though these efforts must be regarded as no more than a start along a very long trek, there are new findings that clearly stand out even at this early stage of the search. I hope this approach will prove useful to others.

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Preface to the Revised Issue

The past four decades have witnessed astonishing advances in astronomy, astrophysics, and cosmology. So, why reissue a book published nearly four decades ago?[i]

Astrophysicists tend to be most interested in new results, novel discoveries, theoretical approaches that explain observations – in brief, scientific advances. What can they still learn from a book as old as *Cosmic Discovery* that isn't covered in the astronomical journals they read every day to keep up with the flood of research results and new insights they provide?

These are fair questions and they deserve frank answers.

A main reason for making *Cosmic Discovery* available once again is that it dwells on fundamental questions astrophysicists were asking in the late 1970s. Many of these remain unanswered. More important, the book focuses less on what we know now, and concentrates instead on what we will need to do in the years ahead to assure further progress.

It asks, "Where have the tools our community uses every day come from? Who invented and perfected them? Who paid to make them available at affordable cost? How long should an older space observatory remain active when more powerful capabilities could provide a totally distinct view of the Universe never accessible before?"

We know that the public cannot forever support the construction and staffing of new observatories unless older facilities are set aside to make room for newer ventures. Otherwise, the costs of astronomy would escalate unaffordably. But, what is the best timing for initiating divestments we need to face, so that expertise developed at high cost, both to the public and to the investigators who may have spent decades perfecting their craft, will not be lost?

[i] *Cosmic Discovery* was originally published by Basic Books, New York, in 1981. With the acquisition of the publisher by Perseus Books, the book gradually went out of print, and its copyright was reverted.

Throughout the 1950–90 Cold War era, dramatic advances in military surveillance technologies in the United States, largely aimed at avoiding miscalculations in a conflict that might drive either side to seek unilateral dominance through a first strike, had led to a sequence of surprising astronomical discoveries. As the military's capabilities rapidly escalated, ever more-powerful instruments replaced outdated earlier models that could then be made available to an astronomical community eager to adopt and adapt any new observational techniques.

This was how the hand-me-down new technologies inherited from the military created entirely new research fields: first radio astronomy, and then in rapid succession, infrared, X-ray, and gamma-ray astronomy. The new instruments enabled astronomers to discover quasars and pulsars, interstellar magnetic fields, stars that were more luminous in the infrared than in any other spectral range, galaxies that emitted most of their energy in the mid- or far-infrared, X-ray stars and galaxies, and gamma-ray bursts. The last of these was not only enabled by military equipment but actually discovered as part of a classified military program and kept secret for some years.

This beneficial flood of equipment, and the astronomical discoveries the instruments enabled in the US, came to a partial halt in the early 1970s. The eponymous Mansfield amendment, introduced by Senator Michael Joseph (Mike) Mansfield, the Democratic Majority Leader in the U.S. Senate, forbade the Defense Department the use of appropriated funds "to carry out any research project or study unless [it had] a direct and apparent relationship to a specific military function."

This was not an unreasonable directive. The U.S. National Science Foundation and NASA were expected to take over and fund the vacated research areas. But, with the huge cost of the Vietnam War at the time, neither agency could be adequately funded to take on the new research that they, rather than the military, were now expected to manage. It took roughly a decade to reestablish balanced funding for both military and civilian research.

Such operational problems are not the kinds of questions most astronomers worry about daily. But somebody clearly did. At NASA in the late 1970s and early 1980s it was Frank Martin.

Throughout the late 1970s, Franklin D. Martin, a Ph.D. physicist in charge of advanced astrophysics projects at NASA, was under constant pressure from gamma-ray, X-ray, optical, and infrared astronomers demanding an early launch of their field's most significant mission. In 1978 he issued a compendium of 24 of the most sought-after astronomical space missions recommended by the Space Science Board of the National Research Council or by an *Outlook for Space Study Group* reporting to the NASA Administrator. The two dozen proposals showed the scope of the problem but did not solve it.

In 1979, Frank was appointed Director of NASA's Astrophysics Division. This led to even greater pressures, not only from mutually competing astronomers, but also from the U.S. Congress, which could not understand why NASA was asking for so many different missions.

At about that time, Frank became aware of an article I had written.[ii] It documented the need for a paced program of observations, by all potential means and in all available wavelength or energy ranges, to show how assembly of all these findings might ultimately lead to a coherent understanding of the Universe. In an interview with the historian of astronomy Renee Rottner, three decades later, Frank Martin recalled, "Somebody walked down the hall and handed it to me . . ., I read it and I knew exactly what to do."[iii]

Not long thereafter, Frank and I first met during a coffee break at a NASA meeting. We didn't know each other, but he came up to mention that he had read the article, and thought it had probably been intended specifically for the NASA Director of Astrophysics. In our brief exchange, I didn't quite get what he meant by this. But a year or two later it became clear he had concluded he would need to initiate a program of missions directed at carrying out most, if not all, of the missions astronomers had been requesting. As Director of Astrophysics, he would need to persuade the U.S. Congress, which until then had been unwilling to consider more than one astronomical mission at a time, to take a longer view and at least agree in principle to a multiyear census of the Universe by a wide range of observational means.

By 1982, Frank Martin was leaving NASA. He was replaced by his former deputy and long-time friend, Dr. Charles (Charlie) J. Pellerin, who inherited not only the position but also the pressures Frank had faced – particularly the fierce competition among astronomers to get their most important, most expensive missions launched, the *Compton Gamma-ray Observatory*, the *Hubble Space Telescope*, the *Advanced X-ray Facility AXAF*, later renamed *Chandra*, and the *Space Infrared Telescope Facility SIRTF*, subsequently renamed *Spitzer.*

In a similar interview with Renee Rottner, the same historian of astronomy with whom Frank had also spoken, Charlie recalled his frustrations at the time, and his realization that he needed to "come up with a story that [would get] everyone to support the whole program . . . " When a member of his staff in 1982 called Charlie's attention to *Cosmic Discovery*, which had just been published a year earlier, Charlie felt it would provide him "the ammunition for making the

[ii] "The Number of Class A Phenomena Characterizing the Universe," Martin Harwit, *Quarterly Journal of the Royal Astronomical Society*, 16, 378–409, 1975.

[iii] Frank Martin interview recorded in Making the Invisible Visible, *A History of the Spitzer Infrared Telescope Facility (1971–2003)*, Renee M. Rottner, NASA *Monograph in Aerospace History* No. 47, p. 57, 2017.

discovery argument for all these missions . . . "[iv] Such a coherent program would bring the astronomical community together and permit him to forge a cohesive program. He asked his deputy, George Newton, to invite a group of leading astrophysicists to come to Washington on January 3, 1985, and to ask me to chair the meeting.

I had never met Charlie, knew nothing about the political background of the invitation, but was glad to help in whatever way might be useful.

That one-day January meeting, in which all the participants pitched in with useful ideas for furthering the NASA Astrophysics program, was such a success that Charlie decided on making these meetings a regular feature, and asked me to continue chairing the sessions. In addition to 15 to 20 regular members, Charlie often also invited experts on topics under discussion to attend and provide advice.

By April 1985, the group had come up with an attractive brochure that explained the capabilities of the different space observatories we hoped Congress would agree to provide. In colorful doodles and the simplest possible accompanying text, this booklet titled "*The Great Observatories for Space Astrophysics*" explained to Congressional staff what each observatory would contribute, and how it could be implemented. Charlie ordered a print run of 15,000 copies so we'd be sure not to run short.[v]

In the fall of 1985, we arranged late afternoon talks for Congressional staff, in which we showed what we hoped to achieve, and how the four missions we needed to launch, the Compton Gamma Ray Observatory, the Chandra X-Ray Observatory, the Hubble Space Telescope, and the Spitzer Infrared Observatory would become four mutually complementing components of "The Great Observatories," the name by which we hoped the set of observatories would become known. At these sessions we could also answer questions, a move which later paid off in securing the consent of the actual members of Congress to approve the launch of the Great Observatories, at a combined cost to US tax payers of $8 billion over the next 15 years.

Throughout the next years Charlie provided dynamic inspired leadership, and concrete plans for the Great Observatories began to fall into place.

By the time I left the advisory group in the fall of 1987, we had established good relations not only within NASA, and with the Congress, but also with President Reagan's Science Advisor, Bill Graham. By February 1988, the budget the President submitted to Congress contained a new entry for the X-ray portion

[iv] Charles Pellerin interview recorded in the same volume by Renee M. Rottner, pp. 72–74, 2017.

[v] "*The Great Observatories for Space Astrophysics*," Charles Pellerin, Jr., Martin Harwit, & Valerie Neal, NASA Publication, 1985.

of the Great Observatories. Over the years, Frank Martin, Charlie Pellerin, and I became fast friends. Three decades later we still remain in touch.

The problems being faced by astronomy today are remarkably similar to those astronomy faced in the 1980s. Difficulties persist in controlling recurrent cost overruns on planned missions. We also are confronting mounting costs in developing observatories for detecting gravitational waves, high-energy cosmic rays, and particles that may, or may not, be responsible for the apparent existence of dark matter. Neither the military nor industry share our interests in advancing these esoteric technologies. We may thus need to find new sponsors, sources of funding, and possibly new international partners to pursue *Cosmic Discovery – the Search, Scope and Heritage of Astronomy*.

Acknowledgments

I thank Vince Higgs editor of this revised issue of Cosmic Discovery at Cambridge University Press for his good advice, and Esther Miguéliz Obanos for her care and attention to the many details involved. I am grateful also to Lori Allen, director of the National Optical Astronomy Observatory, Thomas A. Fleming, archivist at the University of Arizona, and Jean Mueller, archivist at the Mount Palomar Observatory, for providing new scans of images from the original 1981 edition that had meanwhile been lost.

Most of all I thank my wife Marianne for her constant support over the many years the original volume and now its reissue took to complete.

Preface to the 1981 Publication

Cosmic Discovery is an investigation into the complexity of the universe. It is addressed to a wide range of readers interested in astronomical discovery from the astronomer's, the historian's, and the policymaker's points of view. I have tried to avoid the specialized jargon of each of these three fields and have appended a glossary[i] to explain those technical and lesser-known terms and abbreviations that had to be included.

The book contains five chapters. The first summarizes the most important findings and conclusions of the study. Readers largely interested in ideas and results, rather than substantiation and evidence, may find themselves satisfied by this chapter-length essay. Others, particularly professionals more interested in a thorough examination of the subject, will find full documentation in the remaining four chapters—and may, in fact, prefer to read chapter 1 only after reading these later chapters. Extensive bibliographic notes facilitate access to original sources. Two technical appendices containing tables and background material complete the text.

The universe contains stars that shine steadily like the sun, variable stars that pulsate regularly, and eruptive variables that periodically flare. There are supernovae, pulsars, and X-ray stars. Clouds of luminous gas permeate the spaces around bright blue stars, while dark clouds of dust linger just beyond. Faint red stars in the hundreds of thousands aggregate in globular clusters. Galaxies that rival or exceed the Milky Way in size populate the universe out to all distances our telescopes can reach. Here and there galaxies emit their energy, not in visible light but predominantly as X rays or infrared radiation. Clusters of galaxies abound. Quasars are interspersed, some seemingly ejecting mass at velocities exceeding the speed of light.

[i] See the Glossary/Index at the end of the book for these explanations.

These phenomena and the circumstances of their discovery are described in chapter 2, which is meant to provide not only factual information but also a sense for the flavor of cosmic discovery: We encounter immense variations in scale—size, luminosity, variability, energy of emitted particles and waves—that differentiate some forty-three phenomena. We see the role that theory and ideas play in the discovery of each new phenomenon. We become aware that many of the discoverers come to astronomy from other disciplines, bringing with them new tools with which they stumble on the unexpected.

Many cosmic phenomena have only come to be recognized in the past thirty-five years, largely through the introduction into astronomy of radio, X-ray, infrared, and gamma-ray techniques. None of the new phenomena had been anticipated before World War II, and it is natural to wonder how many more remain unrecognized even today, how rich and complex the universe might be. Further, if technological advances already have helped us uncover so many new cosmic features, how many more innovations of similar kinds could we put to use in future cosmic searches?

These are some of the questions I will seek to answer in chapter 3. And while this attempt may appear brash or even presumptuous, there are good reasons why success may be expected in astronomy, though it eludes us in other sciences.

Astronomy is largely an observational science, and for at least the next century our technology will be insufficiently advanced to permit exploration of the universe beyond the solar system. The distances simply are too great. So enormous is the distance to the nearest stars, so overwhelming our separation from the nearest galaxies, that these journeys might never be tried even in remote future aeons.

Because astronomy is so dependent on observations, it is relatively simple to assess the impact that further technological advances are likely to make. In the experimental sciences such an assessment would be far more complex. The experimentalist studies a system by imposing constraints and observing the system's response to a controlled stimulus. The variety of these constraints and of stimuli may be extended at will, and experiments can become arbitrarily complex (figure P.1).

Astronomy is different. The observer has only two choices. He can seek to detect and analyze signals incident from the sky, or he may choose to ignore them. But he has no way of stimulating a cosmic source to alter its emission. He can only observe what is offered. He is entirely dependent on the carriers of information that transmit to him all he may learn about the universe.

Information carriers, however, are not infinite in their variety. All the information we currently have about the universe beyond the solar system

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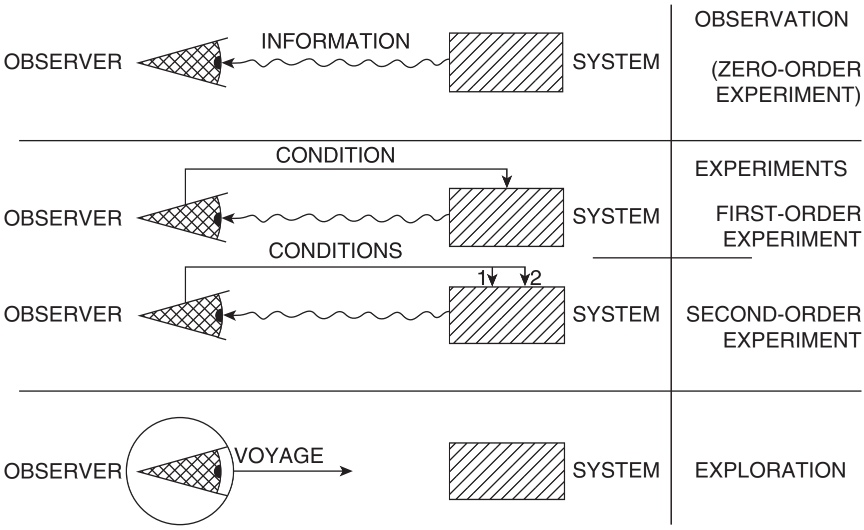

Figure P.1 Observation, Experiments, and Exploration

Observation is the most passive means for gathering data. The observer receives and analyzes information transmitted naturally by the system he is studying. The experimenter, in contrast, stimulates the system under controlled conditions to evoke responses in some observable fashion.

Exploration is an attempt to gather increasing amounts of information by means of a voyage which brings the experimenter or observer closer to the system to be studied.

When an experimenter is permitted to change no more than a single imposed condition—such as temperature—the system's range of responses is relatively confined. We may call such an experiment a first order experiment. When two experimental parameters—perhaps temperature and ambient magnetic field—are varied, the system's potential responses become more complex. Such an experiment can be considered a second order experiment. Correspondingly, an experiment in which some ten parameters are varied at will becomes a tenth order experiment. And a set of observations, in which no conditions at all are imposed can be considered a zero order experiment.

Observation is the simplest form of experimentation. Because of this simplicity the scope of a purely observational discipline, such as the study of the universe beyond the solar system, should be simpler to analyze than the potential wealth and complexity of an experimental science. That is the premise of this book.

has been transmitted to us by means of electromagnetic radiation (light, radio waves, X rays, infrared radiation) or by cosmic-ray particles (electrons and atomic nuclei). Two other carriers are known in physics, neutrinos and gravitational waves. Both are difficult to detect; both have eluded the scrutiny of astronomers.

Even among electromagnetic waves and cosmic-ray particles there are many classes that can never reach us; and others arrive so transmuted and jumbled that all traces of origin are lost: Powerful cosmic radio beacons emitting their energy at wavelengths of 100 kilometers could never be directly observed. We might infer their existence from other observations, but the emitted waves would be totally lost—absorbed by traces of gas between Earth and even the nearest neighboring stars.

No magic of technology, no inventiveness of man could help us detect these or other waves that never reach Earth. Technology can only help the astronomer reach the natural boundaries imposed by the universe itself.

When we discover a new cosmic phenomenon, how do we know that we have found something new, rather than just a variant of an already recognized species? Chapter 4 examines this question in order to arrive at a quantitative estimate of the number of phenomena we actually have discovered to date.

If we know the ultimate limits of astronomical observation, we can also attempt to estimate the number of phenomena that remain undiscovered. Such estimates usually encounter incredulity and arouse controversy if not outright hostility. Nevertheless, I cannot see how a study of cosmic discoveries can be complete or how such a study can help us to improve methods or policies for future searches unless we provide at least a tentative sense of scale; and that scale is determined by the number of phenomena we have discovered and the number remaining unrecognized.

The number of cosmic phenomena I estimate to exist can be verified long before our knowledge of astronomy becomes complete. The estimate itself takes the form of a procedure that can be applied by anyone at any future stage in the development of astronomy to obtain either the same number or perhaps a quite different one. If the two numbers differ, my assessment may prove to have been wrong; or the recipe—the formula telling us what to do—may need to be revised in view of developments that I had not foreseen.

What is important here is not so much whether my appraisal of cosmic complexity is correct; rather, it is that I provide a prescription so anyone can make that judgment himself—perhaps with astronomical data better than those available to me—in order to arrive at a result which he himself can trust. It is a way of making the best informed estimate of the scope of astronomy, though that estimate may still have shortcomings.

That is the novelty of the approach.

Chapter 5 discusses ways in which we might best continue cosmic searches in the future, the directions we will need to follow for rapid progress, the main technological gaps that will have to be spanned, and the manpower needed to accomplish all this.

Most human enterprises involve planning. The larger the venture, the more detailed are our plans. The more expensive the project, the more scientific are the analyses. Only in planning science itself do we often lack scientific insight. I know of no systematic studies that attempt to predict the rate of progress or the ultimate scope of even one of the many branches of science. In fact, there have been few attempts at systematic examination of how the scope of a scientific discipline might be correctly assessed. The only astronomer to have recognized the need for such an examination appears to have been Fritz Zwicky, who twenty years ago discussed his ideas in his book *Morphological Astronomy.*[1]

Yet there is a pressing need for clear analysis. In a thoughtful editorial written for the interdisciplinary journal *Science*, Jurgen Schmandt of the Lyndon B. Johnson School of Public Affairs at the University of Texas in Austin has summarized this need.

> Difficult and controversial policy decisions often need a factual base that can only be provided by careful scientific investigation. . . . Without extensive research, embodied in numerous individual studies, such policy decisions would be blind. However, the results of scientific research do not enter the decision-making process in an automatic fashion, nor should they be allowed to be used in a haphazard way. To be used responsibly, scientific data must first be summarized, evaluated, and interpreted. What does the evidence add up to? How solid is it? Are the results tentative or final? Is there consensus or disagreement among the experts about the significance and meaning of the data? What is suggested by contradictory evidence? What is needed to fill gaps in available knowledge? . . .
> Policy analysis is in heavy demand in government. . . . While the level of activity is increasing, little is known about the quality and impact of its results . . . [2]

In a subsequent editorial M. Granger Morgan of the Carnegie-Mellon University elaborates.

> Good policy analysis recognizes that physical truth may be poorly or incompletely known. Its objective is to evaluate, order, and structure incomplete knowledge so as to allow decisions to be made with as complete an understanding as possible of the current state of knowledge, its limitations, and its implications. Like good science, good policy analysis does not draw hard conclusions unless they are warranted by unambiguous data or well-founded theoretical insight. Unlike good science, good policy analysis must deal with opinions, preferences, and

> values, but it does so in ways that are open and explicit and that allow different people, with different opinions and values, to use the same analysis as an aid in making their own decisions. . . . Scientists who find policy analysis alien must strive to understand its value and importance, even if they cannot bring themselves to engage in its practice.[3]

Even though most scientists would agree that systematic studies of scientific planning could yield an improvement on the intuitive approaches we normally take, two attitudes have seemed to prevail. First, scientists tend to be skeptical about the value of any predictions concerning the future of science; and second, they worry about the potential abuses of centralized planning, no matter how accurate the predictions on which the plans are based.

An uneasy feeling persists that long-term predictions on the progress of science are doomed to fail. Among the many anecdotes concerning great scientists of the nineteenth and early twentieth centuries who bungled their predictions on the future of physics, here are two frequently recalled stories.

In 1902, only five years before he was to become the first American scientist to win the Nobel Prize, Albert A. Michelson was able to write:

> The more important fundamental laws and facts of physical science have all been discovered, and these are now so firmly established that the possibility of their ever being supplanted in consequence of new discoveries is exceedingly remote.[4]

Three years later Albert Einstein announced his startling new principle of relativity which found convincing support in measurements Michelson himself had carried out some two decades earlier.

In a similar vein, Walter Meissner, a colleague of Max Planck for thirty years, recalls the young Planck's choice of a career after matriculating in preparation for entry to the University of Munich at the age of seventeen.

> At first he was uncertain whether to select classical philology, music, or physics, but he finally decided on physics in spite of the fact that [Philipp von] Jolly, then professor of physics at the University of Munich, advised him against it, since in the field of physics there was nothing new to be discovered.[5]

A quarter of century later Max Planck was to lay the foundations for the quantum theory of physics, an approach to prove vital for progress in the investigation of atomic, nuclear, and subnuclear structures.

Whether stories concerning men like Michelson or Jolly are representative of nineteenth-century thinking is not clear. Stephen Brush and Lawrence Badash have debated this question.[6] Certainly men like James Clerk Maxwell and William Thomson (Lord Kelvin) were constantly finding imaginative ways to probe the wonders of Nature and were corresponding with each other about their latest discoveries.[7] Theirs was nothing like an attitude of complacency.

Nevertheless, the anecdotes most frequently recalled today portray the late nineteenth-century scientist as confident in his own understanding of Nature, unwilling to grant the possibility of further revolutionary discoveries.

I believe it is this caricature that has left most of us reluctant to venture serious predictions about the future course of science. What if we should fail just as dismally as Michelson or Jolly? Worse yet, what if the predictions were to be taken seriously? Many scientists are concerned that their disciplines might be threatened through centralized management if detailed scientific or social scientific studies were to err in prescribing just how best to proceed in our scientific ventures.

David Edge, writing on the sociology of innovation in British astronomy, has summarized this attitude:

> In my experience . . . scientists tend to think that sociologists are trying to discover the "one true theory" of how science should organize itself and proceed, if it is to advance more efficiently and effectively. Once that theory is established, our lords and masters . . . will then attempt to beat scientists into appropriate conformity. In other words, the first sense of threat stems from the idea that the sociology of science is normative.[8]

Edge's article is based on a talk delivered in Edinburgh at the April, 1977, meeting of the Royal Astronomical Society, and he tries to reassure his audience that sociological analyses cannot be *normative*—cannot set up new procedural standards. He writes, "The aim of sociology is to explain and understand, not to evaluate or judge."[9] This last statement, while true, need not really lessen the potential normative impact of sociological and other analytical studies on how a science like astronomy should progress.

Scientists are quick to pick up and put to good use any successful new research tool. Any social or procedural strategy shown to be effective would quickly become assimilated into plans for the future. Were this not so, whole classes of potentially effective approaches would be permitted to go unused—a waste quite uncharacteristic of most scientific efforts.

We should therefore acknowledge that any reliable study concerned with progress in science—with procedures, attitudes, or working conditions under which great advances are made—may be useful in bringing about further advances and might ultimately influence how we actually conduct science.

Changes in the conduct of science, however, tend to involve centralized planning. We see that most clearly whenever and wherever large expenditures are required for steps likely to lead to particularly useful advances.

Our dilemma, then, is this: Communally reached decisions seldom provide the flexibility that appears to have been an essential ingredient of the most startling astronomical discoveries of recent decades. And yet centrally imposed decisions seem unavoidable, especially where costly, highly promising investigations are to be initiated. We must, therefore, worry about regulating our major plans so they will not inadvertently choke scientific progress.

There are clear grounds for concern about the ways in which a grand scientific strategy might be implemented. The scientific spirit firmly believes in challenging dogma through confrontation with new facts. How can this confrontation continue to succeed if a bureaucracy is to prescribe specific areas a scientist should investigate and others that are to be left untouched?

This is a reasonable and important fear. The scientific method has led to great discoveries, primarily when freedom to investigate new paths has not been curtailed. If centralized planning is to play an important role, as it now does in most fields that require massive funding, then ways must be found to assure freedom of objective investigation no matter where it leads. How this freedom is to be made compatible with the security of society is a difficult question. Most recently it has been raised in discussions on studies of recombinant DNA.

Complex issues concerning science and its service to society will no doubt continue to require complex solutions. Responsible government can, nevertheless, encourage daring science: Steps to implement innovative research in astronomy are not difficult to find once we have thoroughly understood measures that have succeeded in the past and attempts that have led nowhere. An analysis of these successes and failures leads directly to a set of specific recommendations that occupy the final portions of the book. Some of these recommendations involve communal endeavor; others depend on the imagination and enterprise of the individual. Together they are meant to provide incisive approaches to astronomical ventures and promise a rich and exciting era of cosmic discovery.

Acknowledgments

I thank many colleagues, friends, and members of my family for their help while I was writing this book. An early draft of the manuscript was critically read by Bart J. Bok, F. Westy Dain, and Thomas A. Pauls. Later versions were read by Kenneth Brecher, Thomas F. Gieryn, Eric Harwit, Gina and Felix Haurowitz, William E. Howard III, Franklin D. Martin, Heinrich Pfeiffer, Johannes Schmid-Burgk, Alice H. Sievert, Woodruff T. Sullivan III, and Ira M. Wasserman. I thank them all for their patience, for suggestions they made, and for strongly arguing their points of view, which often differed from mine. Together they have left a strong imprint on my presentation of the subject.

Much of the history of modern astronomy has not yet been written. I thank countless colleagues in astronomy who talked to me about their personal recollections, wrote me letters, sent me material that I have quoted, and permitted extensive interviews. Without their willing collaboration the book would have lacked basic authenticity.

Successive drafts of the book were typed by Gabriele Breuer, Judith A. Marcus, Sylvia Corbin, and Barbara Davidson. Drawings were prepared by Barbara Boettcher and Walter Fusshöller. It is a pleasure to thank them for their help.

Early support for the studies that led to this book was received from the Section on the History and Philosophy of Science at the National Science Foundation. The first draft of the book was completed during the fall of 1976 while I was working at the Max-Planck Institute for Radioastronomy in Bonn, with a Senior U.S. Scientist Award from the Alexander von Humboldt Foundation of the Federal Republic of Germany. I thank Dr. Peter G. Mezger, director of the Institute, for his hospitality during that year.

The manuscript was brought to completion at Cornell University. I am grateful to students in my class, Astronomy 215, who critically read the manuscript and provided me with some of the most uninhibited commentary I received.

A book like this gives the author an opportunity to see what a wonderful institution a university can be: I thank my colleagues at Cornell, Richard N. Boyd, Terrence L. Fine, Robert McGinnis, L. Pearce Williams, and the late Raymond Bowers for elucidating conversations, respectively, on science and government, philosophy of science, viable theories of probability, the sociology of science and the history of science. I also acknowledge a most enjoyable discussion with Robert K. Merton of Columbia University and thank him for his encouraging remarks.

Finally, it is a pleasure to acknowledge the enjoyable collaboration with Martin Kessler of Basic Books and with his staff, particularly Maureen Bischoff and Ruth Gales, who brought the book into print.

Cambridge University Press
978-1-108-72204-9 — Cosmic Discovery
Martin Harwit
Frontmatter
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