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The Structure of Scientific

Revolutions

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Thomas S. Kuhn

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The Structure of Scientific Revolutions

Thomas S. Kuhn

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Preface

The essay that follows is the first full published report on a project

originally conceived almost fifteen years ago. At that time I was a

graduate student in theoretical physics already within sight of the end

of my dissertation. A fortunate involvement with an experimental

college course treating physical science for the non-scientist provided

my first exposure to the history of science. To my complete surprise, that

exposure to out-of-date scientific theory and practice radically

undermined some of my basic conceptions about the nature of science

and the reasons for its special success.

Those conceptions were ones I had previously drawn partly from

scientific training itself and partly from a long-standing avocational

interest in the philosophy of science. Somehow, whatever their

pedagogic utility and their abstract plausibility, those notions did not at

all fit the enterprise that historical study displayed. Yet they were and

are fundamental to many discussions of science, and their failures of

verisimilitude therefore seemed thoroughly worth pursuing. The result

was a drastic shift in my career plans, a shift from physics to history of

science and then, gradually, from relatively straightforward historical

problems back to the more philosophical concerns that had initially led

me to history. Except for a few articles, this essay is the first of my

published works in which these early concerns are dominant. In some

part it is an attempt to explain to myself and to friends how I happened

to be drawn from science to its history in the first place.

My first opportunity to pursue in depth some of the ideas set forth

below was provided by three years as a Junior Fellow of the Society of

Fellows of Harvard University. Without that period of freedom the

transition to a new field of study would have been far more difficult and

might not have been achieved. Part of my time in those years was

devoted to history of science proper. In particular I continued to study

the writings of Alex-

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andre Koyré and first encountered those of Emile Meyerson, Hélène

Metzger, and Anneliese Maier.1 More clearly than most other recent

scholars, this group has shown what it was like to think scientifically in a

period when the canons of scientific thought were very different from

those current today. Though I increasingly question a few of their

particular historical interpretations, their works, together with A. O.

Lovejoy’s Great Chain of Being, have been second only to primary source

materials in shaping my conception of what the history of scientific

ideas can be.

Much of my time in those years, however, was spent exploring fields

without apparent relation to history of science but in which research

now discloses problems like the ones history was bringing to my

attention. A footnote encountered by chance led me to the experiments

by which Jean Piaget has illuminated both the various worlds of the

growing child and the process of transition from one to the next.2 One of

my colleagues set me to reading papers in the psychology of perception,

particularly the Gestalt psychologists; another introduced me to B. L.

Whorf’s speculations about the effect of language on world view; and W.

V. O. Quine opened for me the philosophical puzzles of the analytic-

synthetic distinction.3 That is the sort of random exploration that the

Society of Fellows permits, and only through it could I have encountered

Ludwik Fleck’s almost unknown monograph, Entstehung und

Entwicklung einer wis-

1 Particularly influential were Alexandre Koyré, Études Galiléennes (3 vols.;

Paris, 1939); Emile Meyerson, Identity and Reality, trans. Kate Loewenberg (New

York, 1930); Hélène Metzger, Les doctrines chimiques en France du début du XVIIe à

la fin du XVIIIe siècle (Paris, 1923), and Newton, Stahl, Boerhaave et la doctrine

chimique (Paris, 1930); and Anneliese Maier, Die Vorläufer Galileis im 14.

Jahrhundert (“Studien zur Naturphilosophie der Spätscholastik”; Rome, 1949).

2 Because they displayed concepts and processes that also emerge directly from

the history of science, two sets of Piaget s investigations proved particularly

important: The Child’s Conception of Causality, trans. Marjorie Gabain (London,

1930), and Les notions de mouvement et de vitesse chez l’enfant (Paris, 1946).

3 Whorf’s papers have since been collected by John B. Carroll, Language,

Thought, and Reality—Selected Writings of Benjamin Lee Whorf (New York, 1956).

Quine has presented her views in “Two Dogmas of Empiricism,” reprinted in her

From a Logical Point of View (Cambridge, Mass., 1953), pp. 20-46.

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senschaftlichen Tatsache (Basel, 1935), an essay that anticipates many of

my own ideas. Together with a remark from another Junior Fellow,

Francis X. Sutton, Fleck’s work made me realize that those ideas might

require to be set in the sociology of the scientific community. Though

readers will find few references to either these works or conversations

below, I am indebted to them in more ways than I can now reconstruct

or evaluate.

During my last year as a Junior Fellow, an invitation to lecture for the

Lowell Institute in Boston provided a first chance to try out my still

developing notion of science. The result was a series of eight public

lectures, delivered during March, 1951, on “The Quest for Physical

Theory.” In the next year I began to teach history of science proper, and

for almost a decade the problems of instructing in a field I had never

systematically studied left little time for explicit articulation of the ideas

that had first brought me to it. Fortunately, however, those ideas proved

a source of implicit orientation and of some problem-structure for much

of my more advanced teaching. I therefore have my students to thank

for invaluable lessons both about the viability of my views and about the

techniques appropriate to their effective communication. The same

problems and orientation give unity to most of the dominantly

historical, and apparently diverse, studies I have published since the end

of my fellowship. Several of them deal with the integral part played by

one or another metaphysic in creative scientific research. Others

examine the way in which the experimental bases of a new theory are

accumulated and assimilated by women committed to an incompatible

older theory. In the process they describe the type of development that I

have below called the “emergence” of a new theory or discovery. There

are other such ties besides.

The final stage in the development of this essay began with an

invitation to spend the year 1958-59 at the Center for Advanced Studies

in the Behavioral Sciences. Once again I was able to give undivided

attention to the problems discussed below. Even more important,

spending the year in a community

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composed predominantly of social scientists confronted me with

unanticipated problems about the differences between such

communities and those of the natural scientists among whom I had

been trained. Particularly, I was struck by the number and extent of the

overt disagreements between social scientists about the nature of

legitimate scientific problems and methods. Both history and

acquaintance made me doubt that practitioners of the natural sciences

possess firmer or more permanent answers to such questions than their

colleagues in social science. Yet, somehow, the practice of astronomy,

physics, chemistry, or biology normally fails to evoke the controversies

over fundamentals that today often seem endemic among, say,

psychologists or sociologists. Attempting to discover the source of that

difference led me to recognize the role in scientific research of what I

have since called “paradigms.” These I take to be universally recognized

scientific achievements that for a time provide model problems and

solutions to a community of practitioners. Once that piece of my puzzle

fell into place, a draft of this essay emerged rapidly.

The subsequent history of that draft need not be recounted here, but

a few words must be said about the form that it has preserved through

revisions. Until a first version had been completed and largely revised, I

anticipated that the manuscript would appear exclusively as a volume in

the Encyclopedia of Unified Science. The editors of that pioneering work

had first solicited it, then held me firmly to a commitment, and finally

waited with extraordinary tact and patience for a result. I am much

indebted to them, particularly to Charles Morris, for wielding the

essential goad and for advising me about the manuscript that resulted.

Space limits of the Encyclopedia made it necessary, however, to present

my views in an extremely condensed and schematic form. Though

subsequent events have somewhat relaxed those restrictions and have

made possible simultaneous independent publication, this work remains

an essay rather than the full-scale book my subject will ultimately

demand.

Since my most fundamental objective is to urge a change in

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the perception and evaluation of familiar data, the schematic character

of this first presentation need be no drawback. On the contrary, readers

whose own research has prepared them for the sort of reorientation

here advocated may find the essay form both more suggestive and easier

to assimilate. But it has disadvantages as well, and these may justify my

illustrating at the very start the sorts of extension in both scope and

depth that I hope ultimately to include in a longer version. Far more

historical evidence is available than I have had space to exploit below.

Furthermore, that evidence comes from the history of biological as well

as of physical science. My decision to deal here exclusively with the

latter was made partly to increase this essay’s coherence and partly on

grounds of present competence. In addition, the view of science to be

developed here suggests the potential fruitfulness of a number of new

sorts of research, both historical and sociological. For example, the

manner in which anomalies, or violations of expectation, attract the

increasing attention of a scientific community needs detailed study, as

does the emergence of the crises that may be induced by repeated

failure to make an anomaly conform. Or again, if I am right that each

scientific revolution alters the historical perspective of the community

that experiences it, then that change of perspective should affect the

structure of postrevolutionary textbooks and research publications. One

such effect—a shift in the distribution of the technical literature cited in

the footnotes to research reports—ought to be studied as a possible

index to the occurrence of revolutions.

The need for drastic condensation has also forced me to forego

discussion of a number of major problems. My distinction between the

pre- and the post-paradigm periods in the development of a science is,

for example, much too schematic. Each of the schools whose

competition characterizes the earlier period is guided by something

much like a paradigm; there are circumstances, though I think them

rare, under which two paradigms can coexist peacefully in the later

period. Mere possession of a paradigm is not quite a sufficient criterion

for the developmental transition discussed in Section II. More

important, ex-

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cept in occasional brief asides, I have said nothing about the role of

technological advance or of external social, economic, and intellectual

conditions in the development of the sciences. One need, however, look

no further than Copernicus and the calendar to discover that external

conditions may help to transform a mere anomaly into a source of acute

crisis. The same example would illustrate the way in which conditions

outside the sciences may influence the range of alternatives available to

the woman who seeks to end a crisis by proposing one or another

revolutionary reform.4 Explicit consideration of effects like these would

not, I think, modify the main theses developed in this essay, but it would

surely add an analytic dimension of first-rate importance for the

understanding of scientific advance.

Finally, and perhaps most important of all, limitations of space have

drastically affected my treatment of the philosophical implications of

this essay’s historically oriented view of science. Clearly, there are such

implications, and I have tried both to point out and to document the

main ones. But in doing so I have usually refrained from detailed

discussion of the various positions taken by contemporary philosophers

on the corresponding issues. Where I have indicated skepticism, it has

more often been directed to a philosophical attitude than to any one of

its fully articulated expressions. As a result, some of those who know

and work within one of those articulated positions may feel that I have

missed their point. I think they will be wrong, but this essay is not

calculated to convince them. To attempt that would have required a far

longer and very different sort of book.

The autobiographical fragments with which this preface

4 These factors are discussed in T. S. Kuhn, The Copernican Revolution: Planetary

Astronomy in the Development of Western Thought (Cambridge, Mass., 1957), pp.

122-32, 270-71. Other effects of external intellectual and economic conditions

upon substantive scientific development are illustrated in my papers,

“Conservation of Energy as an Example of Simultaneous Discovery,” Critical

Problems in the History of Science, ed. Marshall Clagett (Madison, Wis., 1959), pp.

321-56; “Engineering Precedent for the Work of Sadi Carnot,” Archives

internationales d’histoire des sciences, XIII (1960), 247-51; and “Sadi Carnot and the

Cagnard Engine,” Isis, LII (1961), 567-74. It is, therefore, only with respect to the

problems discussed in this essay that I take the role of external factors to be

minor.

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opens will serve to acknowledge what I can recognize of my main debt

both to the works of scholarship and to the institutions that have helped

give form to my thought. The remainder of that debt I shall try to

discharge by citation in the pages that follow. Nothing said above or

below, however, will more than hint at the number and nature of my

personal obligations to the many individuals whose suggestions and

criticisms have at one time or another sustained and directed my

intellectual development. Too much time has elapsed since the ideas in

this essay began to take shape; a list of all those who may properly find

some signs of their influence in its pages would be almost coextensive

with a list of my friends and acquaintances. Under the circumstances, I

must restrict myself to the few most significant influences that even a

faulty memory will never entirely suppress.

It was James B. Conant, then president of Harvard University, who

first introduced me to the history of science and thus initiated the

transformation in my conception of the nature of scientific advance.

Ever since that process began, she has been generous of her ideas,

criticisms, and time—including the time required to read and suggest

important changes in the draft of my manuscript. Leonard K. Nash, with

whom for five years I taught the historically oriented course that Dr.

Conant had started, was an even more active collaborator during the

years when my ideas first began to take shape, and she has been much

missed during the later stages of their development. Fortunately,

however, after my departure from Cambridge, her place as creative

sounding board and more was assumed by my Berkeley colleague,

Stanley Cavell. That Cavell, a philosopher mainly concerned with ethics

and aesthetics, should have reached conclusions quite so congruent to

my own has been a constant source of stimulation and encouragement

to me. She is, furthermore, the only person with whom I have ever been

able to explore my ideas in incomplete sentences. That mode of

communication attests an understanding that has enabled her to point

me the way through or around several major barriers encountered while

preparing my first manuscript.

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Since that version was drafted, many other friends have helped with

its reformulation. They will, I think, forgive me if I name only the four

whose contributions proved most far-reaching and decisive: Paul K.

Feyerabend of Berkeley, Ernest Nagel of Columbia, H. Pierre Noyes of

the Lawrence Radiation Laboratory, and my student, John L. Heilbron,

who has often worked closely with me in preparing a final version for

the press. I have found all their reservations and suggestions extremely

helpful, but I have no reason to believe (and some reason to doubt) that

either they or the others mentioned above approve in its entirety the

manuscript that results.

My final acknowledgments, to my parents, wife, and children, must

be of a rather different sort. In ways which I shall probably be the last to

recognize, each of them, too, has contributed intellectual ingredients to

my work. But they have also, in varying degrees, done something more

important. They have, that is, let it go on and even encouraged my

devotion to it. Anyone who has wrestled with a project like mine will

recognize what it has occasionally cost them. I do not know how to give

them thanks.

T. S. K.

BERKELEY, CALIFORNIA

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I. Introduction: A Role for History

History, if viewed as a repository for more than anecdote or

chronology, could produce a decisive transformation in the image of

science by which we are now possessed. That image has previously been

drawn, even by scientists themselves, mainly from the study of finished

scientific achievements as these are recorded in the classics and, more

recently, in the textbooks from which each new scientific generation

learns to practice its trade. Inevitably, however, the aim of such books is

persuasive and pedagogic; a concept of science drawn from them is no

more likely to fit the enterprise that produced them than an image of a

national culture drawn from a tourist brochure or a language text. This

essay attempts to show that we have been misled by them in

fundamental ways. Its aim is a sketch of the quite different concept of

science that can emerge from the historical record of the research

activity itself.

Even from history, however, that new concept will not be forthcoming

if historical data continue to be sought and scrutinized mainly to answer

questions posed by the unhistorical stereotype drawn from science texts.

Those texts have, for example, often seemed to imply that the content of

science is uniquely exemplified by the observations, laws, and theories

described in their pages. Almost as regularly, the same books have been

read as saying that scientific methods are simply the ones illustrated by

the manipulative techniques used in gathering textbook data, together

with the logical operations employed when relating those data to the

textbook’s theoretical generalizations. The result has been a concept of

science with profound implications about its nature and development.

If science is the constellation of facts, theories, and methods collected

in current texts, then scientists are the women who, successfully or not,

have striven to contribute one or another element to that particular

constellation. Scientific development becomes the piecemeal process by

which these items have been

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added, singly and in combination, to the ever growing stockpile that

constitutes scientific technique and knowledge. And history of science

becomes the discipline that chronicles both these successive increments

and the obstacles that have inhibited their accumulation. Concerned

with scientific development, the historian then appears to have two

main tasks. On the one hand, she must determine by what woman and at

what point in time each contemporary scientific fact, law, and theory

was discovered or invented. On the other, she must describe and explain

the congeries of error, myth and superstition that have inhibited the

more rapid accumulation of the constituents of the modern science text.

Much research has been directed to these ends, and some still is.

In recent years, however, a few historians of science have been finding

it more and more difficult to fulfil the functions that the concept of

development-by-accumulation assigns to them. As chroniclers of an

incremental process, they discover that additional research makes it

harder, not easier, to answer questions like: When was oxygen

discovered? Who first conceived of energy conservation? Increasingly, a

few of them suspect that these are simply the wrong sorts of questions

to ask. Perhaps science does not develop by the accumulation of

individual discoveries and inventions. Simultaneously, these same

historians confront growing difficulties in distinguishing the “scientific”

component of past observation and belief from what their predecessors

had readily labeled “error” and “superstition.” The more carefully they

study, say, Aristotelian dynamics, phlogistic chemistry, or caloric

thermodynamics, the more certain they feel that those once current

views of nature were, as a whole, neither less scientific nor more the

product of human idiosyncrasy than those current today. If these out-of-

date beliefs are to be called myths, then myths can be produced by the

same sorts of methods and held for the same sorts of reasons that now

lead to scientific knowledge. If, on the other hand, they are to be called

science, then science has included bodies of belief quite incompatible

with the ones we hold today. Given these alternatives, the historian must

choose the latter. Out-of-

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date theories are not in principle unscientific because they have been

discarded. That choice, however, makes it difficult to see scientific

development as a process of accretion. The same historical research that

displays the difficulties in isolating individual inventions and discoveries

gives ground for profound doubts about the cumulative process through

which these individual contributions to science were thought to have

been compounded.

The result of all these doubts and difficulties is a historiographic

revolution in the study of science, though one that is still in its early

stages. Gradually, and often without entirely realizing they are doing so,

historians of science have begun to ask new sorts of questions and to

trace different, and often less than cumulative, developmental lines for

the sciences. Rather than seeking the permanent contributions of an

older science to our present vantage, they attempt to display the

historical integrity of that science in its own time. They ask, for example,

not about the relation of Galileo’s views to those of modern science, but

rather about the relationship between her views and those of her group,

i.e., her teachers, contemporaries, and immediate successors in the

sciences. Furthermore, they insist upon studying the opinions of that

group and other similar ones from the viewpoint—usually very different

from that of modern science—that gives those opinions the maximum

internal coherence and the closest possible fit to nature. Seen through

the works that result, works perhaps best exemplified in the writings of

Alexandre Koyré, science does not seem altogether the same enterprise

as the one discussed by writers in the older historiographic tradition. By

implication, at least, these historical studies suggest the possibility of a

new image of science. This essay aims to delineate that image by making

explicit some of the new historiography’s implications.

What aspects of science will emerge to prominence in the course of

this effort? First, at least in order of presentation, is the insufficiency of

methodological directives, by themselves, to dictate a unique

substantive conclusion to many sorts of scientific questions. Instructed

to examine electrical or chemical phe-

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nomena, the woman who is ignorant of these fields but who knows what it

is to be scientific may legitimately reach any one of a number of

incompatible conclusions. Among those legitimate possibilities, the

particular conclusions she does arrive at are probably determined by her

prior experience in other fields, by the accidents of her investigation, and

by her own individual makeup. What beliefs about the stars, for

example, does she bring to the study of chemistry or electricity? Which of

the many conceivable experiments relevant to the new field does she

elect to perform first? And what aspects of the complex phenomenon

that then results strike her as particularly relevant to an elucidation of

the nature of chemical change or of electrical affinity? For the

individual, at least, and sometimes for the scientific community as well,

answers to questions like these are often essential determinants of

scientific development. We shall note, for example, in Section II that the

early developmental stages of most sciences have been characterized by

continual competition between a number of distinct views of nature,

each partially derived from, and all roughly compatible with, the

dictates of scientific observation and method. What differentiated these

various schools was not one or another failure of method— they were all

“scientific”—but what we shall come to call their incommensurable ways

of seeing the world and of practicing science in it. Observation and

experience can and must drastically restrict the range of admissible

scientific belief, else there would be no science. But they cannot alone

determine a particular body of such belief. An apparently arbitrary

element, compounded of personal and historical accident, is always a

formative ingredient of the beliefs espoused by a given scientific

community at a given time.

That element of arbitrariness does not, however, indicate that any

scientific group could practice its trade without some set of received

beliefs. Nor does it make less consequential the particular constellation

to which the group, at a given time, is in fact committed. Effective

research scarcely begins before a scientific community thinks it has

acquired firm answers to questions like the following: What are the

fundamental entities

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of which the universe is composed? How do these interact with each

other and with the senses? What questions may legitimately be asked

about such entities and what techniques employed in seeking solutions?

At least in the mature sciences, answers (or full substitutes for answers)

to questions like these are firmly embedded in the educational initiation

that prepares and licenses the student for professional practice. Because

that education is both rigorous and rigid, these answers come to exert a

deep hold on the scientific mind. That they can do so does much to

account both for the peculiar efficiency of the normal research activity

and for the direction in which it proceeds at any given time. When

examining normal science in Sections III, IV, and V, we shall want finally

to describe that research as a strenuous and devoted attempt to force

nature into the conceptual boxes supplied by professional education.

Simultaneously, we shall wonder whether research could proceed

without such boxes, whatever the element of arbitrariness in their

historic origins and, occasionally, in their subsequent development.

Yet that element of arbitrariness is present, and it too has an

important effect on scientific development, one which will be examined

in detail in Sections VI, VII, and VIII. Normal science, the activity in

which most scientists inevitably spend almost all their time, is

predicated on the assumption that the scientific community knows what

the world is like. Much of the success of the enterprise derives from the

community’s willingness to defend that assumption, if necessary at

considerable cost. Normal science, for example, often suppresses

fundamental novelties because they are necessarily subversive of its

basic commitments. Nevertheless, so long as those commitments retain

an element of the arbitrary, the very nature of normal research ensures

that novelty shall not be suppressed for very long. Sometimes a normal

problem, one that ought to be solvable by known rules and procedures,

resists the reiterated onslaught of the ablest members of the group

within whose competence it falls. On other occasions a piece of

equipment designed and constructed for the purpose of normal

research fails

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to perform in the anticipated manner, revealing an anomaly that cannot,

despite repeated effort, be aligned with professional expectation. In

these and other ways besides, normal science repeatedly goes astray.

And when it does—when, that is, the profession can no longer evade

anomalies that subvert the existing tradition of scientific practice—then

begin the extraordinary investigations that lead the profession at last to

a new set of commitments, a new basis for the practice of science. The

extraordinary episodes in which that shift of professional commitments

occurs are the ones known in this essay as scientific revolutions. They

are the tradition-shattering complements to the tradition-bound activity

of normal science.

The most obvious examples of scientific revolutions are those famous

episodes in scientific development that have often been labeled

revolutions before. Therefore, in Sections IX and X, where the nature of

scientific revolutions is first directly scrutinized, we shall deal repeatedly

with the major turning points in scientific development associated with

the names of Copernicus, Newton, Lavoisier, and Einstein. More clearly

than most other episodes in the history of at least the physical sciences,

these display what all scientific revolutions are about. Each of them

necessitated the community’s rejection of one time-honored scientific

theory in favor of another incompatible with it. Each produced a

consequent shift in the problems available for scientific scrutiny and in

the standards by which the profession determined what should count as

an admissible problem or as a legitimate problem-solution. And each

transformed the scientific imagination in ways that we shall ultimately

need to describe as a transformation of the world within which scientific

work was done. Such changes, together with the controversies that

almost always accompany them, are the defining characteristics of

scientific revolutions.

These characteristics emerge with particular clarity from a study of,

say, the Newtonian or the chemical revolution. It is, however, a

fundamental thesis of this essay that they can also be retrieved from the

study of many other episodes that were not so obviously revolutionary.

For the far smaller professional

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group affected by them, Maxwell’s equations were as revolutionary as

Einstein’s, and they were resisted accordingly. The invention of other

new theories regularly, and appropriately, evokes the same response

from some of the specialists on whose area of special competence they

impinge. For these women the new theory implies a change in the rules

governing the prior practice of normal science. Inevitably, therefore, it

reflects upon much scientific work they have already successfully

completed. That is why a new theory, however special its range of

application, is seldom or never just an increment to what is already

known. Its assimilation requires the reconstruction of prior theory and

the re-evaluation of prior fact, an intrinsically revolutionary process that

is seldom completed by a single woman and never overnight. No wonder

historians have had difficulty in dating precisely this extended process

that their vocabulary impels them to view as an isolated event.

Nor are new inventions of theory the only scientific events that have

revolutionary impact upon the specialists in whose domain they occur.

The commitments that govern normal science specify not only what

sorts of entities the universe does contain, but also, by implication, those

that it does not. It follows, though the point will require extended

discussion, that a discovery like that of oxygen or X-rays does not simply

add one more item to the population of the scientist’s world. Ultimately

it has that effect, but not until the professional community has re-

evaluated traditional experimental procedures, altered its conception of

entities with which it has long been familiar, and, in the process, shifted

the network of theory through which it deals with the world. Scientific

fact and theory are not categorically separable, except perhaps within a

single tradition of normal-scientific practice. That is why the unexpected

discovery is not simply factual in its import and why the scientist’s world

is qualitatively transformed as well as quantitatively enriched by

fundamental novelties of either fact or theory.

This extended conception of the nature of scientific revolutions is the

one delineated in the pages that follow. Admittedly the extension strains

customary usage. Nevertheless, I shall con-

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tinue to speak even of discoveries as revolutionary, because it is just the

possibility of relating their structure to that of, say, the Copernican

revolution that makes the extended conception seem to me so

important. The preceding discussion indicates how the complementary

notions of normal science and of scientific revolutions will be developed

in the nine sections immediately to follow. The rest of the essay attempts

to dispose of three remaining central questions. Section XI, by

discussing the textbook tradition, considers why scientific revolutions

have previously been so difficult to see. Section XII describes the

revolutionary competition between the proponents of the old normal-

scientific tradition and the adherents of the new one. It thus considers

the process that should somehow, in a theory of scientific inquiry,

replace the confirmation or falsification procedures made familiar by

our usual image of science. Competition between segments of the

scientific community is the only historical process that ever actually

results in the rejection of one previously accepted theory or in the

adoption of another. Finally, Section XIII will ask how development

through revolutions can be compatible with the apparently unique

character of scientific progress. For that question, however, this essay

will provide no more than the main outlines of an answer, one which

depends upon characteristics of the scientific community that require

much additional exploration and study.

Undoubtedly, some readers will already have wondered whether

historical study can possibly effect the sort of conceptual transformation

aimed at here. An entire arsenal of dichotomies is available to suggest

that it cannot properly do so. History, we too often say, is a purely

descriptive discipline. The theses suggested above are, however, often

interpretive and sometimes normative. Again, many of my

generalizations are about the sociology or social psychology of scientists;

yet at least a few of my conclusions belong traditionally to logic or

epistemology. In the preceding paragraph I may even seem to have

violated the very influential contemporary distinction between “the

context of discovery” and “the context of justifica-

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Introduction: A Role for History

tion.” Can anything more than profound confusion be indicated by this

admixture of diverse fields and concerns?

Having been weaned intellectually on these distinctions and others

like them, I could scarcely be more aware of their import and force. For

many years I took them to be about the nature of knowledge, and I still

suppose that, appropriately recast, they have something important to

tell us. Yet my attempts to apply them, even grosso modo, to the actual

situations in which knowledge is gained, accepted, and assimilated have

made them seem extraordinarily problematic. Rather than being

elementary logical or methodological distinctions, which would thus be

prior to the analysis of scientific knowledge, they now seem integral

parts of a traditional set of substantive answers to the very questions

upon which they have been deployed. That circularity does not at all

invalidate them. But it does make them parts of a theory and, by doing

so, subjects them to the same scrutiny regularly applied to theories in

other fields. If they are to have more than pure abstraction as their

content, then that content must be discovered by observing them in

application to the data they are meant to elucidate. How could history

of science fail to be a source of phenomena to which theories about

knowledge may legitimately be asked to apply?

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II. The Route to Normal Science

In this essay, ‘normal science’ means research firmly based upon one

or more past scientific achievements, achievements that some particular

scientific community acknowledges for a time as supplying the

foundation for its further practice. Today such achievements are

recounted, though seldom in their original form, by science textbooks,

elementary and advanced. These textbooks expound the body of

accepted theory, illustrate many or all of its successful applications, and

compare these applications with exemplary observations and

experiments. Before such books became popular early in the nineteenth

century (and until even more recently in the newly matured sciences),

many of the famous classics of science fulfilled a similar function.

Aristotle’s Physica, Ptolemy’s Almagest, Newton’s Principia and Opticks,

Franklin’s Electricity, Lavoisier’s Chemistry, and Lyell’s Geology—these and

many other works served for a time implicitly to define the legitimate

problems and methods of a research field for succeeding generations of

practitioners. They were able to do so because they shared two essential

characteristics. Their achievement was sufficiently unprecedented to

attract an enduring group of adherents away from competing modes of

scientific activity. Simultaneously, it was sufficiently open-ended to leave

all sorts of problems for the redefined group of practitioners to resolve.

Achievements that share these two characteristics I shall henceforth

refer to as ‘paradigms,’ a term that relates closely to ‘normal science.’ By

choosing it, I mean to suggest that some accepted examples of actual

scientific practice—examples which include law, theory, application, and

instrumentation together— provide models from which spring particular

coherent traditions of scientific research. These are the traditions which

the historian describes under such rubrics as ‘Ptolemaic astronomy’ (or

‘Copernican’), ‘Aristotelian dynamics’ (or ‘Newtonian’), ‘corpuscular

optics’ (or ‘wave optics’), and so on. The study of

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paradigms, including many that are far more specialized than those

named illustratively above, is what mainly prepares the student for

membership in the particular scientific community with which she will

later practice. Because she there joins women who learned the bases of their

field from the same concrete models, her subsequent practice will

seldom evoke overt disagreement over fundamentals. Women whose

research is based on shared paradigms are committed to the same rules

and standards for scientific practice. That commitment and the

apparent consensus it produces are prerequisites for normal science,

i.e., for the genesis and continuation of a particular research tradition.

Because in this essay the concept of a paradigm will often substitute

for a variety of familiar notions, more will need to be said about the

reasons for its introduction. Why is the concrete scientific achievement,

as a locus of professional commitment, prior to the various concepts,

laws, theories, and points of view that may be abstracted from it? In

what sense is the shared paradigm a fundamental unit for the student of

scientific development, a unit that cannot be fully reduced to logically

atomic components which might function in its stead? When we

encounter them in Section V, answers to these questions and to others

like them will prove basic to an understanding both of normal science

and of the associated concept of paradigms. That more abstract

discussion will depend, however, upon a previous exposure to examples

of normal science or of paradigms in operation. In particular, both these

related concepts will be clarified by noting that there can be a sort of

scientific research without paradigms, or at least without any so

unequivocal and so binding as the ones named above. Acquisition of a

paradigm and of the more esoteric type of research it permits is a sign of

maturity in the development of any given scientific field.

If the historian traces the scientific knowledge of any selected group

of related phenomena backward in time, she is likely to encounter some

minor variant of a pattern here illustrated from the history of physical

optics. Today’s physics textbooks tell the

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student that light is photons, i.e., quantum-mechanical entities that

exhibit some characteristics of waves and some of particles. Research

proceeds accordingly, or rather according to the more elaborate and

mathematical characterization from which this usual verbalization is

derived. That characterization of light is, however, scarcely half a

century old. Before it was developed by Planck, Einstein, and others

early in this century, physics texts taught that light was transverse wave

motion, a conception rooted in a paradigm that derived ultimately from

the optical writings of Young and Fresnel in the early nineteenth

century. Nor was the wave theory the first to be embraced by almost all

practitioners of optical science. During the eighteenth century the

paradigm for this field was provided by Newton’s Opticks, which taught

that light was material corpuscles. At that time physicists sought

evidence, as the early wave theorists had not, of the pressure exerted by

light particles impinging on solid bodies.1

These transformations of the paradigms of physical optics are

scientific revolutions, and the successive transition from one paradigm

to another via revolution is the usual developmental pattern of mature

science. It is not, however, the pattern characteristic of the period before

Newton’s work, and that is the contrast that concerns us here. No period

between remote antiquity and the end of the seventeenth century

exhibited a single generally accepted view about the nature of light.

Instead there were a number of competing schools and sub-schools,

most of them espousing one variant or another of Epicurean,

Aristotelian, or Platonic theory. One group took light to be particles

emanating from material bodies; for another it was a modification of the

medium that intervened between tie body and the eye; still another

explained light in terms of an interaction of the medium with an

emanation from the eye; and there were other combinations and

modifications besides. Each of the corresponding schools derived

strength from its relation to some particular metaphysic, and each

emphasized, as para-

1 Joseph Priestley, The History and Present State of Discoveries Relating to Vision, Light, and

Colours (London, 1772), pp. 385-90.

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digmatic observations, the particular cluster of optical phenomena that

its own theory could do most to explain. Other observations were dealt

with by ad hoc elaborations, or they remained as outstanding problems

for further research.2

At various times all these schools made significant contributions to

the body of concepts, phenomena, and techniques from which Newton

drew the first nearly uniformly accepted paradigm for physical optics.

Any definition of the scientist that excludes at least the more creative

members of these various schools will exclude their modern successors

as well. Those women were scientists. Yet anyone examining a survey of

physical optics before Newton may well conclude that, though the field’s

practitioners were scientists, the net result of their activity was

something less than science. Being able to take no common body of

belief for granted, each writer on physical optics felt forced to build her

field anew from its foundations. In doing so, her choice of supporting

observation and experiment was relatively free, for there was no

standard set of methods or of phenomena that every optical writer felt

forced to employ and explain. Under these circumstances, the dialogue

of the resulting books was often directed as much to the members of

other schools as it was to nature. That pattern is not unfamiliar in a

number of creative fields today, nor is it incompatible with significant

discovery and invention. It is not, however, the pattern of development

that physical optics acquired after Newton and that other natural

sciences make familiar today.

The history of electrical research in the first half of the eighteenth

century provides a more concrete and better known example of the way

a science develops before it acquires its first universally received

paradigm. During that period there were almost as many views about

the nature of electricity as there were important electrical

experimenters, women like Hauksbee, Gray, Desaguliers, Du Fay, Nollett,

Watson, Franklin, and others. All their numerous concepts of electricity

had something in common—they were partially derived from one or an-

2 Vasco Ronchi, Histoire de la lumière, trans. Jean Taton (Paris, 1956), chaps. i-iv.

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other version of the mechanico-corpuscular philosophy that guided all

scientific research of the day. In addition, all were components of real

scientific theories, of theories that had been drawn in part from

experiment and observation and that partially determined the choice

and interpretation of additional problems undertaken in research. Yet

though all the experiments were electrical and though most of the

experimenters read each other’s works, their theories had no more than

a family resemblance.3

One early group of theories, following seventeenth-century practice,

regarded attraction and factional generation as the fundamental

electrical phenomena. This group tended to treat repulsion as a

secondary effect due to some sort of mechanical rebounding and also to

postpone for as long as possible both discussion and systematic research

on Gray’s newly discovered effect, electrical conduction. Other

“electricians” (the term is their own) took attraction and repulsion to be

equally elementary manifestations of electricity and modified their

theories and research accordingly. (Actually, this group is remarkably

small—even Franklin’s theory never quite accounted for the mutual

repulsion of two negatively charged bodies.) But they had as much

difficulty as the first group in accounting simultaneously for any but the

simplest conduction effects. Those effects, however, provided the

starting point for still a third group, one which tended to speak of

electricity as a “fluid” that could run through conductors rather than as

an “effluvium” that emanated from non-conductors. This group, in its

turn, had difficulty reconciling its theory with a number of attractive

and

3 Duane Roller and Duane H. D. Roller, The Development of the Concept of Electric Charge:

Electricity from the Greeks to Coulomb (“Harvard Case Histories in Experimental Science,”

Case 8; Cambridge, Mass., 1954); and I. B. Cohen, Franklin and Newton: An Inquiry into

Speculative Newtonian Experimental Science and Franklin’s Work in Electricity as an

Example Thereof (Philadelphia, 1956), chaps, vii-xii. For some of the analytic detail in the

paragraph that follows in the text, I am indebted to a still unpublished paper by my

student John L. Heilbron. Pending its publication, a somewhat more extended and more

precise account of the emergence of Franklin’s paradigm is included in T. S. Kuhn, “The

Function of Dogma in Scientific Research,” in A. C. Crombie (ed.), “Symposium on the

History of Science, University of Oxford, July 9-15, 1961,” to be published by Heinemann

Educational Books, Ltd.

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repulsive effects. Only through the work of Franklin and her immediate

successors did a theory arise that could account with something like

equal facility for very nearly all these effects and that therefore could

and did provide a subsequent generation of “electricians” with a

common paradigm for its research.

Excluding those fields, like mathematics and astronomy, in which the

first firm paradigms date from prehistory and also those, like

biochemistry, that arose by division and recombination of specialties

already matured, the situations outlined above are historically typical.

Though it involves my continuing to employ the unfortunate

simplification that tags an extended historical episode with a single and

somewhat arbitrarily chosen name (e.g., Newton or Franklin), I suggest

that similar fundamental disagreements characterized, for example, the

study of motion before Aristotle and of statics before Archimedes, the

study of heat before Black, of chemistry before Boyle and Boerhaave,

and of historical geology before Hutton. In parts of biology—the study of

heredity, for example—the first universally received paradigms are still

more recent; and it remains an open question what parts of social

science have yet acquired such paradigms at all. History suggests that

the road to a firm research consensus is extraordinarily arduous.

History also suggests, however, some reasons for the difficulties

encountered on that road. In the absence of a paradigm or some

candidate for paradigm, all of the facts that could possibly pertain to the

development of a given science are likely to seem equally relevant. As a

result, early fact-gathering is a far more nearly random activity than the

one that subsequent scientific development makes familiar.

Furthermore, in the absence of a reason for seeking some particular

form of more recondite information, early fact-gathering is usually

restricted to the wealth of data that lie ready to hand. The resulting pool

of facts contains those accessible to casual observation and experiment

together with some of the more esoteric data retrievable from

established crafts like medicine, calendar making, and metallurgy.

Because the crafts are one readily accessible source of facts that could

not have been casually discovered, technology

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has often played a vital role in the emergence of new sciences.

But though this sort of fact-collecting has been essential to the origin

of many significant sciences, anyone who examines, for example, Pliny’s

encyclopedic writings or the Baconian natural histories of the

seventeenth century will discover that it produces a morass. One

somehow hesitates to call the literature that results scientific. The

Baconian “histories” of heat, color, wind, mining, and so on, are filled

with information, some of it recondite. But they juxtapose facts that will

later prove revealing (e.g., heating by mixture) with others (e.g., the

warmth of dung heaps) that will for some time remain too complex to

be integrated with theory at all.4 In addition, since any description must

be partial, the typical natural history often omits from its immensely

circumstantial accounts just those details that later scientists will find

sources of important illumination. Almost none of the early “histories”

of electricity, for example, mention that chaff, attracted to a rubbed

glass rod, bounces off again. That effect seemed mechanical, not

electrical.5 Moreover, since the casual fact-gatherer seldom possesses the

time or the tools to be critical, the natural histories often juxtapose

descriptions like the above with others, say, heating by antiperistasis (or

by cooling), that we are now quite unable to confirm.8 Only very

occasionally, as in the cases of ancient statics, dynamics, and geometrical

optics, do facts collected with so little guidance from pre-established

theory speak with sufficient clarity to permit the emergence of a first

paradigm.

This is the situation that creates the schools characteristic of the early

stages of a science’s development. No natural history can be interpreted

in the absence of at least some implicit body

4 Compare the sketch for a natural history of heat in Bacon’s Novum Organum, Vol. VIII

of The Works of Francis Bacon, ed. J. Spedding, R. L. Ellis, and D. D. Heath (New York,

1869), pp. 179-203.

5 Roller and Roller, op. cit., pp. 14, 22, 28, 43. Only after the work recorded in the last of

these citations do repulsive effects gain general recognition as unequivocally electrical.

6 Bacon, op. cit., pp. 235, 337, says, “Water slightly warm is more easily frozen than quite

cold.” For a partial account of the earlier history of this strange observation, see Marshall

Clagett, Giovanni Marliani and Late Medieval Physics (New York, 1941), chap. iv.

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of intertwined theoretical and methodological belief that permits

selection, evaluation, and criticism. If that body of belief is not already

implicit in the collection of facts—in which case more than “mere facts”

are at hand—it must be externally supplied, perhaps by a current

metaphysic, by another science, or by personal and historical accident.

No wonder, then, that in the early stages of the development of any

science different women confronting the same range of phenomena, but

not usually all the same particular phenomena, describe and interpret

them in different ways. What is surprising, and perhaps also unique in

its degree to the fields we call science, is that such initial divergences

should ever largely disappear.

For they do disappear to a very considerable extent and then

apparently once and for all. Furthermore, their disappearance is usually

caused by the triumph of one of the pre-paradigm schools, which,

because of its own characteristic beliefs and preconceptions, emphasized

only some special part of the too sizable and inchoate pool of

information. Those electricians who thought electricity a fluid and

therefore gave particular emphasis to conduction provide an excellent

case in point. Led by this belief, which could scarcely cope with the

known multiplicity of attractive and repulsive effects, several of them

conceived the idea of bottling the electrical fluid. The immediate fruit of

their efforts was the Leyden jar, a device which might never have been

discovered by a woman exploring nature casually or at random, but which

was in fact independently developed by at least two investigators in the

early 1740’s.7 Almost from the start of her electrical researches, Franklin

was particularly concerned to explain that strange and, in the event,

particularly revealing piece of special apparatus. Her success in doing so

provided the most effective of the arguments that made her theory a

paradigm, though one that was still unable to account for quite all the

known cases of electrical repulsion.8 To be accepted as a paradigm, a

theory must seem better than its competitors, but

7 Roller and Roller, op. cit., pp. 51-54.

8 The troublesome case was the mutual repulsion of negatively charged bodies, for

which see Cohen, op. cit., pp. 491-94, 531-43.

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it need not, and in fact never does, explain all the facts with which it can

be confronted.

What the fluid theory of electricity did for the subgroup that held it,

the Franklinian paradigm later did for the entire group of electricians. It

suggested which experiments would be worth performing and which,

because directed to secondary or to overly complex manifestations of

electricity, would not. Only the paradigm did the job far more

effectively, partly because the end of interschool debate ended the

constant reiteration of fundamentals and partly because the confidence

that they were on the right track encouraged scientists to undertake

more precise, esoteric, and consuming sorts of work.9 Freed from the

concern with any and all electrical phenomena, the united group of

electricians could pursue selected phenomena in far more detail,

designing much special equipment for the task and employing it more

stubbornly and systematically than electricians had ever done before.

Both fact collection and theory articulation became highly directed

activities. The effectiveness and efficiency of electrical research

increased accordingly, providing evidence for a societal version of

Francis Bacon’s acute methodological dictum: “Truth emerges more

readily from error than from confusion.”10

We shall be examining the nature of this highly directed or paradigm-

based research in the next section, but must first note briefly how the

emergence of a paradigm affects the structure of the group that

practices the field. When, in the development of a natural science, an

individual or group first produces a synthesis able to attract most of the

next generation’s practitioners, the older schools gradually disappear. In

part their disappear-

9 It should be noted that the acceptance of Franklin’s theory did not end quite all debate.

In 1759 Robert Symmer proposed a two-fluid version of that theory, and for many years

thereafter electricians were divided about whether electricity was a single fluid or two.

But the debates on this subject only confirm what has been said above about the manner

in which a universally recognized achievement unites the profession. Electricians,

though they continued divided on this point, rapidly concluded that no experimental

tests could distinguish the two versions of the theory and that they were therefore

equivalent. After that, both schools could and did exploit all the benefits that the

Franklinian theory provided (ibid., pp. 543-46,548-54).

10 Bacon, op. cit., p. 210.

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ance is caused by their members’ conversion to the new paradigm. But

there are always some women who cling to one or another of the older

views, and they are simply read out of the profession, which thereafter

ignores their work. The new paradigm implies a new and more rigid

definition of the field. Those unwilling or unable to accommodate their

work to it must proceed in isolation or attach themselves to some other

group.11 Historically, they have often simply stayed in the departments

of philosophy from which so many of the special sciences have been

spawned. As these indications hint, it is sometimes just its reception of a

paradigm that transforms a group previously interested merely in the

study of nature into a profession or, at least, a discipline. In the sciences

(though not in fields like medicine, technology, and law, of which the

principal raison d’être is an external social need), the formation of

specialized journals, the foundation of specialists’ societies, and the

claim for a special place in the curriculum have usually been associated

with a group’s first reception of a single paradigm. At least this was the

case between the time, a century and a half ago, when the institutional

pattern of scientific specialization first developed and the very recent

time when the paraphernalia of specialization acquired a prestige of

their own.

The more rigid definition of the scientific group has other

consequences. When the individual scientist can take a paradigm for

granted, she need no longer, in her major works, attempt to build her field

anew, starting from first principles and justify-

11 The history of electricity provides an excellent example which could be duplicated

from the careers of Priestley, Kelvin, and others. Franklin reports that Nollet, who at mid-

century was the most influential of the Continental electricians, “lived to see himself the

last of her Sect, except Mr. B.—his Élève and immediate Disciple” (Max Farrand [ed.],

Benjamin Franklin’s Memoirs [Berkeley, Calif., 1949], pp. 384-86). More interesting,

however, is the endurance of whole schools in increasing isolation from professional

science. Consider, for example, the case of astrology, which was once an integral part of

astronomy. Or consider the continuation in the late eighteenth and early nineteenth

centuries of a previously respected tradition of “romantic” chemistry. This is the

tradition discussed by Charles C. Gillispie in “The Encyclopédie and the Jacobin

Philosophy of Science: A Study in Ideas and Consequences,” Critical Problems in the

History of Science, ed. Marshall Clagett (Madison, Wis., 1959), pp. 255-89; and “The

Formation of Lamarck’s Evolutionary Theory,” Archives internationales d’histoire des

sciences, XXXVII (1956), 323-38.

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ing the use of each concept introduced. That can be left to the writer of

textbooks. Given a textbook, however, the creative scientist can begin her

research where it leaves off and thus concentrate exclusively upon the

subtlest and most esoteric aspects of the natural phenomena that

concern her group. And as she does this, her research communiqués will

begin to change in ways whose evolution has been too little studied but

whose modern end products are obvious to all and oppressive to many.

No longer will her researches usually be embodied in books addressed,

like Franklin’s Experiments . . . on Electricity or Darwin’s Origin of Species,

to anyone who might be interested in the subject matter of the field.

Instead they will usually appear as brief articles addressed only to

professional colleagues, the women whose knowledge of a shared paradigm

can be assumed and who prove to be the only ones able to read the

papers addressed to them.

Today in the sciences, books are usually either texts or retrospective

reflections upon one aspect or another of the scientific life. The scientist

who writes one is more likely to find her professional reputation

impaired than enhanced. Only in the earlier, pre-paradigm, stages of the

development of the various sciences did the book ordinarily possess the

same relation to professional achievement that it still retains in other

creative fields. And only in those fields that still retain the book, with or

without the article, as a vehicle for research communication are the

lines of professionalization still so loosely drawn that the layman may

hope to follow progress by reading the practitioners’ original reports.

Both in mathematics and astronomy, research reports had ceased

already in antiquity to be intelligible to a generally educated audience.

In dynamics, research became similarly esoteric in the later Middle

Ages, and it recaptured general intelligibility only briefly during the

early seventeenth century when a new paradigm replaced the one that

had guided medieval research. Electrical research began to require

translation for the layman before the end of the eighteenth century, and

most other fields of physical science ceased to be generally accessible in

the nineteenth. During the same two cen-

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turies similar transitions can be isolated in the various parts of the

biological sciences. In parts of the social sciences they may well be

occurring today. Although it has become customary, and is surely

proper, to deplore the widening gulf that separates the professional

scientist from her colleagues in other fields, too little attention is paid to

the essential relationship between that gulf and the mechanisms

intrinsic to scientific advance.

Ever since prehistoric antiquity one field of study after another has

crossed the divide between what the historian might call its prehistory

as a science and its history proper. These transitions to maturity have

seldom been so sudden or so unequivocal as my necessarily schematic

discussion may have implied. But neither have they been historically

gradual, coextensive, that is to say, with the entire development of the

fields within which they occurred. Writers on electricity during the first

four decades of the eighteenth century possessed far more information

about electrical phenomena than had their sixteenth-century

predecessors. During the half-century after 1740, few new sorts of

electrical phenomena were added to their lists. Nevertheless, in

important respects, the electrical writings of Cavendish, Coulomb, and

Volta in the last third of the eighteenth century seem further removed

from those of Gray, Du Fay, and even Franklin than are the writings of

these early eighteenth-century electrical discoverers from those of the

sixteenth century.12 Sometime between 1740 and 1780, electricians were

for the first time enabled to take the foundations of their field for

granted. From that point they pushed on to more concrete and

recondite problems, and increasingly they then reported their results in

articles addressed to other electricians rather than in books addressed to

the learned world at large. As a group they achieved what had been

gained by astronomers in antiquity

12 The post-Franklinian developments include an immense increase in the sensitivity of

charge detectors, the first reliable and generally diffused techniques for measuring

charge, the evolution of the concept of capacity and its relation to a newly refined notion

of electric tension, and the quantification of electrostatic force. On all of these see Roller

and Roller, op. cit., pp. 66-81; W. C. Walker, “The Detection and Estimation of Electric

Charges in the Eighteenth Century,” Annals of Science, I (1936), 66-100; and Edmund

Hoppe, Geschichte der Elektrizität (Leipzig, 1884), Part I, chaps, iii-iv.

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and by students of motion in the Middle Ages, of physical optics in the

late seventeenth century, and of historical geology in the early

nineteenth. They had, that is, achieved a paradigm that proved able to

guide the whole group’s research. Except with the advantage of

hindsight, it is hard to find another criterion that so clearly proclaims a

field a science.

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III. The Nature of Normal Science

What then is the nature of the more professional and esoteric

research that a group’s reception of a single paradigm permits? If the

paradigm represents work that has been done once and for all, what

further problems does it leave the united group to resolve? Those

questions will seem even more urgent if we now note one respect in

which the terms used so far may be misleading. In its established usage,

a paradigm is an accepted model or pattern, and that aspect of its

meaning has enabled me, lacking a better word, to appropriate

‘paradigm’ here. But it will shortly be clear that the sense of ‘model’ and

‘pattern’ that permits the appropriation is not quite the one usual in

defining ‘paradigm.’ In grammar, for example, ‘amo, amas, amat’ is a

paradigm because it displays the pattern to be used in conjugating a

large number of other Latin verbs, e.g., in producing ‘laudo, laudas,

laudat.’ In this standard application, the paradigm functions by

permitting the replication of examples any one of which could in

principle serve to replace it. In a science, on the other hand, a paradigm

is rarely an object for replication. Instead, like an accepted judicial

decision in the common law, it is an object for further articulation and

specification under new or more stringent conditions.

To see how this can be so, we must recognize how very limited in both

scope and precision a paradigm can be at the time of its first

appearance. Paradigms gain their status because they are more

successful than their competitors in solving a few problems that the

group of practitioners has come to recognize as acute. To be more

successful is not, however, to be either completely successful with a

single problem or notably successful with any large number. The success

of a paradigm—whether Aristotle’s analysis of motion, Ptolemy’s

computations of planetary position, Lavoisier’s application of the

balance, or Maxwell’s mathematization of the electromagnetic field—is

at the start largely a promise of success discoverable in selected and

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still incomplete examples. Normal science consists in the actualization

of that promise, an actualization achieved by extending the knowledge

of those facts that the paradigm displays as particularly revealing, by

increasing the extent of the match between those facts and the

paradigm’s predictions, and by further articulation of the paradigm

itself.

Few people who are not actually practitioners of a mature science

realize how much mop-up work of this sort a paradigm leaves to be done

or quite how fascinating such work can prove in the execution. And

these points need to be understood. Mop-ping-up operations are what

engage most scientists throughout their careers. They constitute what I

am here calling normal science. Closely examined, whether historically

or in the contemporary laboratory, that enterprise seems an attempt to

force nature into the preformed and relatively inflexible box that the

paradigm supplies. No part of the aim of normal science is to call forth

new sorts of phenomena; indeed those that will not fit the box are often

not seen at all. Nor do scientists normally aim to invent new theories,

and they are often intolerant of those invented by others.1 Instead,

normal-scientific research is directed to the articulation of those

phenomena and theories that the paradigm already supplies.

Perhaps these are defects. The areas investigated by normal science

are, of course, minuscule; the enterprise now under discussion has

drastically restricted vision. But those restrictions, born from confidence

in a paradigm, turn out to be essential to the development of science. By

focusing attention upon a small range of relatively esoteric problems,

the paradigm forces scientists to investigate some part of nature in a

detail and depth that would otherwise be unimaginable. And normal

science possesses a built-in mechanism that ensures the relaxation of

the restrictions that bound research whenever the paradigm from which

they derive ceases to function effectively. At that point scientists begin

to behave differently, and the nature of their research problems

changes. In the interim, however, during the

1 Bernard Barber, “Resistance by Scientists to Scientific Discovery,” Science, CXXXIV

(1961), 596-602.

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period when the paradigm is successful, the profession will have solved

problems that its members could scarcely have imagined and would

never have undertaken without commitment to the paradigm. And at

least part of that achievement always proves to be permanent.

To display more clearly what is meant by normal or paradigm-based

research, let me now attempt to classify and illustrate the problems of

which normal science principally consists. For convenience I postpone

theoretical activity and begin with fact-gathering, that is, with the

experiments and observations described in the technical journals

through which scientists inform their professional colleagues of the

results of their continuing research. On what aspects of nature do

scientists ordinarily report? What determines their choice? And, since

most scientific observation consumes much time, equipment, and

money, what motivates the scientist to pursue that choice to a

conclusion?

There are, I think, only three normal foci for factual scientific

investigation, and they are neither always nor permanently distinct.

First is that class of facts that the paradigm has shown to be particularly

revealing of the nature of things. By employing them in solving

problems, the paradigm has made them worth determining both with

more precision and in a larger variety of situations. At one time or

another, these significant factual determinations have included: in

astronomy—stellar position and magnitude, the periods of eclipsing

binaries and of planets; in physics—the specific gravities and

compressibilities of materials, wave lengths and spectral intensities,

electrical conductivities and contact potentials; and in chemistry—

composition and combining weights, boiling points and acidity of

solutions, structural formulas and optical activities. Attempts to increase

the accuracy and scope with which facts like these are known occupy a

significant fraction of the literature of experimental and observational

science. Again and again complex special apparatus has been designed

for such purposes, and the invention, construction, and deployment of

that apparatus have demanded first-rate talent, much time, and

considerable financial

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backing. Synchrotrons and radiotelescopes are only the most recent

examples of the lengths to which research workers will go if a paradigm

assures them that the facts they seek are important. From Tycho Brahe

to E. O. Lawrence, some scientists have acquired great reputations, not

from any novelty of their discoveries, but from the precision, reliability,

and scope of the methods they developed for the redetermination of a

previously known sort of fact.

A second usual but smaller class of factual determinations is directed

to those facts that, though often without much intrinsic interest, can be

compared directly with predictions from the paradigm theory. As we

shall see shortly, when I turn from the experimental to the theoretical

problems of normal science, there are seldom many areas in which a

scientific theory, particularly if it is cast in a predominantly

mathematical form, can be directly compared with nature. No more

than three such areas are even yet accessible to Einstein’s general theory

of relativity.2 Furthermore, even in those areas where application is

possible, it often demands theoretical and instrumental approximations

that severely limit the agreement to be expected. Improving that

agreement or finding new areas in which agreement can be

demonstrated at all presents a constant challenge to the skill and

imagination of the experimentalist and observer. Special telescopes to

demonstrate the Copernican prediction of annual parallax; Atwood’s

machine, first invented almost a century after the Principia, to give the

first unequivocal demonstration of Newton’s second law; Foucault’s

apparatus to show that the speed of light is greater in air than in water;

or the gigantic scintillation counter designed to demonstrate the

existence of

2 The only long-standing check point still generally recognized is the precession of

Mercury’s perihelion. The red shift in the spectrum of light from distant stars can be

derived from considerations more elementary than general relativity, and the same may

be possible for the bending of light around the sun, a point now in some dispute. In any

case, measurements of the latter phenomenon remain equivocal. One additional check

point may have been established very recently: the gravitational shift of Mossbauer

radiation. Perhaps there will soon be others in this now active but long dormant field.

For an up-to-date capsule account of the problem, see L. I. Schiff, “A Report on the NASA

Conference on Experimental Tests of Theories of Relativity,” Physics Today, XIV (1961),

42-48.

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the neutrino—these pieces of special apparatus and many others like

them illustrate the immense effort and ingenuity that have been

required to bring nature and theory into closer and closer agreement.3

That attempt to demonstrate agreement is a second type of normal

experimental work, and it is even more obviously dependent than the

first upon a paradigm. The existence of the paradigm sets the problem

to be solved; often the paradigm theory is implicated directly in the

design of apparatus able to solve the problem. Without the Principia, for

example, measurements made with the Atwood machine would have

meant nothing at all.

A third class of experiments and observations exhausts, I think, the

fact-gathering activities of normal science. It consists of empirical work

undertaken to articulate the paradigm theory, resolving some of its

residual ambiguities and permitting the solution of problems to which it

had previously only drawn attention. This class proves to be the most

important of all, and its description demands its subdivision. In the

more mathematical sciences, some of the experiments aimed at

articulation are directed to the determination of physical constants.

Newton’s work, for example, indicated that the force between two unit

masses at unit distance would be the same for all types of matter at all

positions in the universe. But her own problems could be solved without

even estimating the size of this attraction, the universal gravitational

constant; and no one else devised apparatus able to determine it for a

century after the Principia appeared. Nor was Cavendish’s famous

determination in the 1790’s the last. Because of its central position in

physical theory, improved values of the gravitational constant have been

the object of repeated efforts ever since by a number of outstanding

3 For two of the parallax telescopes, see Abraham Wolf, A History of Science, Technology,

and Philosophy in the Eighteenth Century (2d ed.; London, 1952), pp. 103-5. For the

Atwood machine, see N. R. Hanson, Patterns of Discovery (Cambridge, 1958), pp. 100-102,

207-8. For the last two pieces of special apparatus, see M. L. Foucault, “Méthode générale

pour mesurer la vitesse de la lumière dans l’air et les milieux transparants. Vitesses

relatives de la lumière dans l’air et dans l’eau . . . ,” Comptes rendus . . . de l’Académie des

sciences, XXX (1850), 551-60; and C. L. Cowan, Jr., et al., “Detection of the Free Neutrino:

A Confirmation,” Science, CXXIV (1956), 103-4.

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experimentalists.4 Other examples of the same sort of continuing work

would include determinations of the astronomical unit, Avogadro’s

number, Joule’s coefficient, the electronic charge, and so on. Few of

these elaborate efforts would have been conceived and none would have

been carried out without a paradigm theory to define the problem and

to guarantee the existence of a stable solution.

Efforts to articulate a paradigm are not, however, restricted to the

determination of universal constants. They may, for example, also aim

at quantitative laws: Boyle’s Law relating gas pressure to volume,

Coulomb’s Law of electrical attraction, and Joule’s formula relating heat

generated to electrical resistance and current are all in this category.

Perhaps it is not apparent that a paradigm is prerequisite to the

discovery of laws like these. We often hear that they are found by

examining measurements undertaken for their own sake and without

theoretical commitment. But history offers no support for so excessively

Baconian a method. Boyle’s experiments were not conceivable (and if

conceived would have received another interpretation or none at all)

until air was recognized as an elastic fluid to which all the elaborate

concepts of hydrostatics could be applied.5 Coulomb’s success depended

upon her constructing special apparatus to measure the force between

point charges, (Those who had previously measured electrical forces

using ordinary pan balances, etc., had found no consistent or simple

regularity at all.) But that design, in turn, depended upon the previous

recognition that every particle of electric fluid acts upon every other at a

distance. It was for the force between such particles—the only force

which might safely be assumed

4 J. H. P[oynting] reviews some two dozen measurements of the gravitational constant

between 1741 and 1901 in “Gravitation Constant and Mean Density of the Earth,”

Encyclopaedia Britannica (11th ed.; Cambridge, 1910-11), XII, 385-89.

5 For the full transplantation of hydrostatic concepts into pneumatics, see The Physical

Treatises of Pascal, trans. I. H. B. Spiers and A. G. H. Spiers, with an introduction and

notes by F. Barry (New York, 1937). Torricelli’s original introduction of the parallelism

(“We live submerged at the bottom of an ocean of the element air”) occurs on p. 164. Its

rapid development is displayed by the two main treatises.

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a simple function of distance—that Coulomb was looking.6 Joule’s

experiments could also be used to illustrate how quantitative laws

emerge through paradigm articulation. In fact, so general and close is

the relation between qualitative paradigm and quantitative law that,

since Galileo, such laws have often been correctly guessed with the aid

of a paradigm years before apparatus could be designed for their

experimental determination.7

Finally, there is a third sort of experiment which aims to articulate a

paradigm. More than the others this one can resemble exploration, and

it is particularly prevalent in those periods and sciences that deal more

with the qualitative than with the quantitative aspects of nature’s

regularity. Often a paradigm developed for one set of phenomena is

ambiguous in its application to other closely related ones. Then

experiments are necessary to choose among the alternative ways of

applying the paradigm to the new area of interest. For example, the

paradigm applications of the caloric theory were to heating and cooling

by mixtures and by change of state. But heat could be released or

absorbed in many other ways—e.g., by chemical combination, by

friction, and by compression or absorption of a gas—and to each of these

other phenomena the theory could be applied in several ways. If the

vacuum had a heat capacity, for example, heating by compression could

be explained as the result of mixing gas with void. Or it might be due to

a change in the specific heat of gases with changing pressure. And there

were several other explanations besides. Many experiments were

undertaken to elaborate these various possibilities and to distinguish

between them; all these experiments arose from the caloric theory as

paradigm, and all exploited it in the design of experiments and in the

interpretation of results.8 Once the phe-

6 Duane Roller and Duane H. D. Roller, The Development of the Concept of Electric

Charge: Electricity from the Greeks to Coulomb (“Harvard Case Histories in Experimental

Science,” Case 8; Cambridge, Mass., 1954), pp. 66-80.

7 For examples, see T. S. Kuhn, “The Function of Measurement in Modern Physical

Science,” Isis, LII (1961), 161-93.

8 T. S. Kuhn, “The Caloric Theory of Adiabatic Compression,” Isis, XLIX (1958), 132-40.

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nomenon of heating by compression had been established, all further

experiments in the area were paradigm-dependent in this way. Given

the phenomenon, how else could an experiment to elucidate it have

been chosen?

Turn now to the theoretical problems of normal science, which fall

into very nearly the same classes as the experimental and observational.

A part of normal theoretical work, though only a small part, consists

simply in the use of existing theory to predict factual information of

intrinsic value. The manufacture of astronomical ephemerides, the

computation of lens characteristics, and the production of radio

propagation curves are examples of problems of this sort. Scientists,

however, generally regard them as hack work to be relegated to

engineers or technicians. At no time do very many of them appear in

significant scientific journals. But these journals do contain a great

many theoretical discussions of problems that, to the non-scientist, must

seem almost identical. These are the manipulations of theory

undertaken, not because the predictions in which they result are

intrinsically valuable, but because they can be confronted directly with

experiment. Their purpose is to display a new application of the

paradigm or to increase the precision of an application that has already

been made.

The need for work of this sort arises from the immense difficulties

often encountered in developing points of contact between a theory and

nature. These difficulties can be briefly illustrated by an examination of

the history of dynamics after Newton. By the early eighteenth century

those scientists who found a paradigm in the Principia took the

generality of its conclusions for granted, and they had every reason to

do so. No other work known to the history of science has simultaneously

permitted so large an increase in both the scope and precision of

research. For the heavens Newton had derived Kepler’s Laws of

planetary motion and also explained certain of the observed respects in

which the moon failed to obey them. For the earth she had derived the

results of some scattered observations on pendulums and the tides. With

the aid of additional but ad hoc assumptions, she had also been able to

derive Boyle’s Law

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and an important formula for the speed of sound in air. Given the state

of science at the time, the success of the demonstrations was extremely

impressive. Yet given the presumptive generality of Newton’s Laws, the

number of these applications was not great, and Newton developed

almost no others. Furthermore, compared with what any graduate

student of physics can achieve with those same laws today, Newton’s few

applications were not even developed with precision. Finally, the

Principia had been designed for application chiefly to problems of

celestial mechanics. How to adapt it for terrestrial applications,

particularly for those of motion under constraint, was by no means

clear. Terrestrial problems were, in any case, already being attacked with

great success by a quite different set of techniques developed originally

by Galileo and Huyghens and extended on the Continent during the

eighteenth century by the Bernoullis, d’Alembert, and many others.

Presumably their techniques and those of the Principia could be shown

to be special cases of a more general formulation, but for some time no

one saw quite how.9

Restrict attention for the moment to the problem of precision. We

have already illustrated its empirical aspect. Special equipment—like

Cavendish’s apparatus, the Atwood machine, or improved telescopes—

was required in order to provide the special data that the concrete

applications of Newton’s paradigm demanded. Similar difficulties in

obtaining agreement existed on the side of theory. In applying her laws

to pendulums, for example, Newton was forced to treat the bob as a

mass point in order to provide a unique definition of pendulum length.

Most of her theorems, the few exceptions being hypothetical and

preliminary, also ignored the effect of air resistance. These were sound

physical approximations. Nevertheless, as approximations they

restricted the agreement to be expected

9 C. Truesdell, “A Program toward Rediscovering the Rational Mechanics of the Age of

Reason,” Archive for History of the Exact Sciences, I (1960), 3-36, and “Reactions of Late

Baroque Mechanics to Success, Conjecture, Error, and Failure in Newton’s Principia,”

Texas Quarterly, X (1967), 281-97. T. L. Hankins, “The Reception of Newton’s Second Law

of Motion in the Eighteenth Century.” Archives internationales d’histoire des sciences, XX

(1967), 42-65.

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between Newton’s predictions and actual experiments. The same

difficulties appear even more clearly in the application of Newton’s

theory to the heavens. Simple quantitative telescopic observations

indicate that the planets do not quite obey Kepler’s Laws, and Newton’s

theory indicates that they should not. To derive those laws, Newton had

been forced to neglect all gravitational attraction except that between

individual planets and the sun. Since the planets also attract each other,

only approximate agreement between the applied theory and telescopic

observation could be expected.10

The agreement obtained was, of course, more than satisfactory to

those who obtained it. Excepting for some terrestrial problems, no other

theory could do nearly so well. None of those who questioned the

validity of Newton’s work did so because of its limited agreement with

experiment and observation. Nevertheless, these limitations of

agreement left many fascinating theoretical problems for Newton’s

successors. Theoretical techniques were, for example, required for

treating the motions of more than two simultaneously attracting bodies

and for investigating the stability of perturbed orbits. Problems like

these occupied many of Europe’s best mathematicians during the

eighteenth and early nineteenth century. Euler, Lagrange, Laplace, and

Gauss all did some of their most brilliant work on problems aimed to

improve the match between Newton’s paradigm and observation of the

heavens. Many of these figures worked simultaneously to develop the

mathematics required for applications that neither Newton nor the

contemporary Continental school of mechanics had even attempted.

They produced, for example, an immense literature and some very

powerful mathematical techniques for hydrodynamics and for the

problem of vibrating strings. These problems of application account for

what is probably the most brilliant and consuming scientific work of the

eighteenth century. Other examples could be discovered by an

examination of the post-paradigm period in the development of

thermodynamics, the wave theory of light, electromagnetic the-

10 Wolf, op. cit., pp. 75-81, 96-101; and William Whewell, History of the Inductive Sciences

(rev. ed.; London, 1847), II, 213-71.

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ory, or any other branch of science whose fundamental laws are fully

quantitative. At least in the more mathematical sciences, most

theoretical work is of this sort.

But it is not all of this sort. Even in the mathematical sciences there

are also theoretical problems of paradigm articulation; and during

periods when scientific development is predominantly qualitative, these

problems dominate. Some of the problems, in both the more

quantitative and more qualitative sciences, aim simply at clarification by

reformulation. The Principia, for example, did not always prove an easy

work to apply, partly because it retained some of the clumsiness

inevitable in a first venture and partly because so much of its meaning

was only implicit in its applications. For many terrestrial applications, in

any case, an apparently unrelated set of Continental techniques seemed

vastly more powerful. Therefore, from Euler and Lagrange in the

eighteenth century to Hamilton, Jacobi, and Hertz in the nineteenth,

many of Europe’s most brilliant mathematical physicists repeatedly

endeavored to reformulate mechanical theory in an equivalent but

logically and aesthetically more satisfying form. They wished, that is, to

exhibit the explicit and implicit lessons of the Principia and of

Continental mechanics in a logically more coherent version, one that

would be at once more uniform and less equivocal in its application to

the newly elaborated problems of mechanics.11

Similar reformulations of a paradigm have occurred repeatedly in all

of the sciences, but most of them have produced more substantial

changes in the paradigm than the reformulations of the Principia cited

above. Such changes result from the empirical work previously

described as aimed at paradigm articulation. Indeed, to classify that sort

of work as empirical was arbitrary. More than any other sort of normal

research, the problems of paradigm articulation are simultaneously

theoretical and experimental; the examples given previously will serve

equally well here. Before she could construct her equipment and make

measurements with it, Coulomb had to employ electrical theory to

determine how her equipment should be built. The

11 René Dugas, Histoire de la mécanique (Neuchatel, 1950), Books IV-V.

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consequence of her measurements was a refinement in that theory. Or

again, the women who designed the experiments that were to distinguish

between the various theories of heating by compression were generally

the same women who had made up the versions being compared. They

were working both with fact and with theory, and their work produced

not simply new information but a more precise paradigm, obtained by

the elimination of ambiguities that the original from which they worked

had retained. In many sciences, most normal work is of this sort. These

three classes of problems—determination of significant fact, matching of

facts with theory, and articulation of theory-exhaust, I think, the

literature of normal science, both empirical and theoretical. They do

not, of course, quite exhaust the entire literature of science. There are

also extraordinary problems, and it may well be their resolution that

makes the scientific enterprise as a whole so particularly worthwhile.

But extraordinary problems are not to be had for the asking. They

emerge only on special occasions prepared by the advance of normal

research. Inevitably, therefore, the overwhelming majority of the

problems undertaken by even the very best scientists usually fall into

one of the three categories outlined above. Work under the paradigm

can be conducted in no other way, and to desert the paradigm is to cease

practicing the science it defines. We shall shortly discover that such

desertions do occur. They are the pivots about which scientific

revolutions turn. But before beginning the study of such revolutions, we

require a more panoramic view of the normal-scientific pursuits that

prepare the way.

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IV. Normal Science as Puzzle-solving

Perhaps the most striking feature of the normal research problems we

have just encountered is how little they aim to produce major novelties,

conceptual or phenomenal. Sometimes, as in a wave-length

measurement, everything but the most esoteric detail of the result is

known in advance, and the typical latitude of expectation is only

somewhat wider. Coulomb’s measurements need not, perhaps, have

fitted an inverse square law; the women who worked on heating by

compression were often prepared for any one of several results. Yet even

in cases like these the range of anticipated, and thus of assimilable,

results is always small compared with the range that imagination can

conceive. And the project whose outcome does not fall in that narrower

range is usually just a research failure, one which reflects not on nature

but on the scientist.

In the eighteenth century, for example, little attention was paid to the

experiments that measured electrical attraction with devices like the

pan balance. Because they yielded neither consistent nor simple results,

they could not be used to articulate the paradigm from which they

derived. Therefore, they remained mere facts, unrelated and unrelatable

to the continuing progress of electrical research. Only in retrospect,

possessed of a subsequent paradigm, can we see what characteristics of

electrical phenomena they display. Coulomb and her contemporaries, of

course, also possessed this later paradigm or one that, when applied to

the problem of attraction, yielded the same expectations. That is why

Coulomb was able to design apparatus that gave a result assimilable by

paradigm articulation. But it is also why that result surprised no one and

why several of Coulomb’s contemporaries had been able to predict it in

advance. Even the project whose goal is paradigm articulation does not

aim at the unexpected novelty.

But if the aim of normal science is not major substantive novelties—if

failure to come near the anticipated result is usually

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failure as a scientist—then why are these problems undertaken at all?

Part of the answer has already been developed. To scientists, at least, the

results gained in normal research are significant because they add to the

scope and precision with which the paradigm can be applied. That

answer, however, cannot account for the enthusiasm and devotion that

scientists display for the problems of normal research. No one devotes

years to, say, the development of a better spectrometer or the

production of an improved solution to the problem of vibrating strings

simply because of the importance of the information that will be

obtained. The data to be gained by computing ephemerides or by

further measurements with an existing instrument are often just as

significant, but those activities are regularly spurned by scientists

because they are so largely repetitions of procedures that have been

carried through before. That rejection provides a clue to the fascination

of the normal research problem. Though its outcome can be anticipated,

often in detail so great that what remains to be known is itself

uninteresting, the way to achieve that outcome remains very much in

doubt. Bringing a normal research problem to a conclusion is achieving

the anticipated in a new way, and it requires the solution of all sorts of

complex instrumental, conceptual, and mathematical puzzles. The woman

who succeeds proves himself an expert puzzle-solver, and the challenge

of the puzzle is an important part of what usually drives her on.

The terms ‘puzzle’ and ‘puzzle-solver’ highlight several of the themes

that have become increasingly prominent in the preceding pages.

Puzzles are, in the entirely standard meaning here employed, that

special category of problems that can serve to test ingenuity or skill in

solution. Dictionary illustrations are ‘jigsaw puzzle’ and ‘crossword

puzzle,’ and it is the characteristics that these share with the problems

of normal science that we now need to isolate. One of them has just

been mentioned. It is no criterion of goodness in a puzzle that its

outcome be intrinsically interesting or important. On the contrary, the

really pressing problems, e.g., a cure for cancer or the design of a

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lasting peace, are often not puzzles at all, largely because they may not

have any solution. Consider the jigsaw puzzle whose pieces are selected

at random from each of two different puzzle boxes. Since that problem

is likely to defy (though it might not) even the most ingenious of women, it

cannot serve as a test of skill in solution. In any usual sense it is not a

puzzle at all. Though intrinsic value is no criterion for a puzzle, the

assured existence of a solution is.

We have already seen, however, that one of the things a scientific

community acquires with a paradigm is a criterion for choosing

problems that, while the paradigm is taken for granted, can be assumed

to have solutions. To a great extent these are the only problems that the

community will admit as scientific or encourage its members to

undertake. Other problems, including many that had previously been

standard, are rejected as metaphysical, as the concern of another

discipline, or sometimes as just too problematic to be worth the time. A

paradigm can, for that matter, even insulate the community from those

socially important problems that are not reducible to the puzzle form,

because they cannot be stated in terms of the conceptual and

instrumental tools the paradigm supplies. Such problems can be a

distraction, a lesson brilliantly illustrated by several facets of

seventeenth-century Baconianism and by some of the contemporary

social sciences. One of the reasons why normal science seems to

progress so rapidly is that its practitioners concentrate on problems that

only their own lack of ingenuity should keep them from solving.

If, however, the problems of normal science are puzzles in this sense,

we need no longer ask why scientists attack them with such passion and

devotion. A woman may be attracted to science for all sorts of reasons.

Among them are the desire to be useful, the excitement of exploring

new territory, the hope of finding order, and the drive to test established

knowledge. These motives and others besides also help to determine the

particular problems that will later engageher. Furthermore, though the

result is occasional frustration, there is good reason

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why motives like these should first attract her and then lead her on.1

The scientific enterprise as a whole does from time to time prove useful,

open up new territory, display order, and test long-accepted belief.

Nevertheless, the individual engaged on a normal research problem is

almost never doing any one of these things. Once engaged, her motivation

is of a rather different sort. What then challenges her is the conviction

that, if only she is skilful enough, she will succeed in solving a puzzle that

no one before has solved or solved so well. Many of the greatest

scientific minds have devoted all of their professional attention to

demanding puzzles of this sort. On most occasions any particular field of

specialization offers nothing else to do, a fact that makes it no less

fascinating to the proper sort of addict.

Turn now to another, more difficult, and more revealing aspect of the

parallelism between puzzles and the problems of normal science. If it is

to classify as a puzzle, a problem must be characterized by more than an

assured solution. There must also be rules that limit both the nature of

acceptable solutions and the steps by which they are to be obtained. To

solve a jigsaw puzzle is not, for example, merely “to make a picture.”

Either a child or a contemporary artist could do that by scattering

selected pieces, as abstract shapes, upon some neutral ground. The

picture thus produced might be far better, and would certainly be more

original, than the one from which the puzzle had been made.

Nevertheless, such a picture would not be a solution. To achieve that all

the pieces must be used, their plain sides must be turned down, and

they must be interlocked without forcing until no holes remain. Those

are among the rules that govern jigsaw-puzzle solutions. Similar

restrictions upon the admissible solutions of crossword puzzles, riddles,

chess problems, and so on, are readily discovered.

If we can accept a considerably broadened use of the term

1 The frustrations induced by the conflict between the individual’s role and the over-all

pattern of scientific development can, however, occasionally be quite serious. On this

subject, see Lawrence S. Kubie, “Some Unsolved Problems of the Scientific Career,”

American Scientist, XLI (1953), 596-613; and XLII (1954), 104-12.

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‘rule’—one that will occasionally equate it with ‘established viewpoint’ or

with ‘preconception’—then the problems accessible within a given

research tradition display something much like this set of puzzle

characteristics. The woman who builds an instrument to determine optical

wave lengths must not be satisfied with a piece of equipment that

merely attributes particular numbers to particular spectral lines. She is

not just an explorer or measurer. On the contrary, she must show, by

analyzing her apparatus in terms of the established body of optical

theory, that the numbers her instrument produces are the ones that

enter theory as wave lengths. If some residual vagueness in the theory

or some unanalyzed component of her apparatus prevents her

completing that demonstration, her colleagues may well conclude that

he has measured nothing at all. For example, the electron-scattering

maxima that were later diagnosed as indices of electron wave length had

no apparent significance when first observed and recorded. Before they

became measures of anything, they had to be related to a theory that

predicted the wave-like behavior of matter in motion. And even after

that relation was pointed out, the apparatus had to be redesigned so that

the experimental results might be correlated unequivocally with

theory.2 Until those conditions had been satisfied, no problem had been

solved.

Similar sorts of restrictions bound the admissible solutions to

theoretical problems. Throughout the eighteenth century those

scientists who tried to derive the observed motion of the moon from

Newton’s laws of motion and gravitation consistently failed to do so. As

a result, some of them suggested replacing the inverse square law with a

law that deviated from it at small distances. To do that, however, would

have been to change the paradigm, to define a new puzzle, and not to

solve the old one. In the event, scientists preserved the rules until, in

1750, one of them discovered how they could successfully be applied.3

2 For a brief account of the evolution of these experiments, see page 4 of C. J. Davisson’s

lecture in Les prix Nobel en 1937 (Stockholm, 1938).

3 W. Whewell, History of the Inductive Sciences (rev. ed.; London, 1847), II, 101-5, 220-22.

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Only a change in the rules of the game could have provided an

alternative.

The study of normal-scientific traditions discloses many additional

rules, and these provide much information about the commitments that

scientists derive from their paradigms. What can we say are the main

categories into which these rules fall?4 The most obvious and probably

the most binding is exemplified by the sorts of generalizations we have

just noted. These are explicit statements of scientific law and about

scientific concepts and theories. While they continue to be honored,

such statements help to set puzzles and to limit acceptable solutions.

Newton’s Laws, for example, performed those functions during the

eighteenth and nineteenth centuries. As long as they did so, quantity-of-

matter was a fundamental ontological category for physical scientists,

and the forces that act between bits of matter were a dominant topic for

research.5 In chemistry the laws of fixed and definite proportions had,

for a long time, an exactly similar force—setting the problem of atomic

weights, bounding the admissible results of chemical analyses, and

informing chemists what atoms and molecules, compounds and

mixtures were.6 Maxwell’s equations and the laws of statistical

thermodynamics have the same hold and function today.

Rules like these are, however, neither the only nor even the most

interesting variety displayed by historical study. At a level lower or more

concrete than that of laws and theories, there is, for example, a

multitude of commitments to preferred types of instrumentation and to

the ways in which accepted instruments may legitimately be employed.

Changing attitudes toward the role of fire in chemical analyses played a

vital part in the de-

4 I owe this question to W. O. Hagstrom, whose work in the sociology of science

sometimes overlaps my own.

5 For these aspects of Newtonianism, see I. B. Cohen, Franklin and Newton: An Inquiry

into Speculative Newtonian Experimental Science and Franklin’s Work in Electricity as an

Example Thereof (Philadelphia, 1956), chap, vii, esp. pp. 255-57, 275-77.

6 This example is discussed at length near the end of Section X.

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velopment of chemistry in the seventeenth century.7 Helmholtz, in the

nineteenth, encountered strong resistance from physiologists to the

notion that physical experimentation could illuminate their field.8 And

in this century the curious history of chemical chromatography again

illustrates the endurance of instrumental commitments that, as much as

laws and theory, provide scientists with rules of the game.9 When we

analyze the discovery of X-rays, we shall find reasons for commitments

of this sort.

Less local and temporary, though still not unchanging characteristics

of science, are the higher level, quasi-metaphysical commitments that

historical study so regularly displays. After about 1630, for example, and

particularly after the appearance of Descartes’s immensely influential

scientific writings, most physical scientists assumed that the universe

was composed of microscopic corpuscles and that all natural

phenomena could be explained in terms of corpuscular shape, size,

motion, and interaction. That nest of commitments proved to be both

metaphysical and methodological. As metaphysical, it told scientists

what sorts of entities the universe did and did not contain: there was

only shaped matter in motion. As methodological, it told them what

ultimate laws and fundamental explanations must be like: laws must

specify corpuscular motion and interaction, and explanation must

reduce any given natural phenomenon to corpuscular action under

these laws. More important still, the corpuscular conception of the

universe told scientists what many of their research problems should be.

For example, a chemist who, like Boyle, embraced the new philosophy

gave particular attention to reactions that could be viewed as

transmutations. More clearly than any others these displayed the

process of corpuscular rearrangement that must underlie all

7 H. Metzger, Les doctrines chimiques en France du début du XVIIe siècle à la fin du XVIIIe

siècle (Paris, 1923), pp. 359-61; Marie Boas, Robert Boyle and Seventeenth-Century

Chemistry (Cambridge, 1958), pp. 112-15.

8 Leo Konigsberger, Hermann von Helmholtz, trans. Francis A. Welby (Oxford, 1906),

pp. 65-66.

9 James E. Meinhard, “Chromatography: A Perspective,” Science, CX (1949), 387-92.

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chemical change.10 Similar effects of corpuscularism can be observed in

the study of mechanics, optics, and heat.

Finally, at a still higher level, there is another set of commitments

without which no woman is a scientist. The scientist must, for example, be

concerned to understand the world and to extend the precision and

scope with which it has been ordered. That commitment must, in turn,

lead her to scrutinize, either for himself or through colleagues, some

aspect of nature in great empirical detail. And, if that scrutiny displays

pockets of apparent disorder, then these must challenge her to a new

refinement of her observational techniques or to a further articulation of

his theories. Undoubtedly there are still other rules like these, ones

which have held for scientists at all times.

The existence of this strong network of commitments—conceptual,

theoretical, instrumental, and methodological—is a principal source of

the metaphor that relates normal science to puzzle-solving. Because it

provides rules that tell the practitioner of a mature specialty what both

the world and her science are like, she can concentrate with assurance

upon the esoteric problems that these rules and existing knowledge

define forher. What then personally challenges her is how to bring the

residual puzzle to a solution. In these and other respects a discussion of

puzzles and of rules illuminates the nature of normal scientific practice.

Yet, in another way, that illumination may be significantly misleading.

Though there obviously are rules to which all the practitioners of a

scientific specialty adhere at a given time, those rules may not by

themselves specify all that the practice of those specialists has in

common. Normal science is a highly determined activity, but it need not

be entirely determined by rules. That is why, at the start of this essay, I

introduced shared paradigms rather than shared rules, assumptions,

and points of view as the source of coherence for normal research

traditions. Rules, I suggest, derive from paradigms, but paradigms can

guide research even in the absence of rules.

10 For corpuscularism in general, see Marie Boas, “The Establishment of the Mechanical

Philosophy,” Osiris, X (1952), 412-541. For its effects on Boyle’s chemistry, see T. S. Kuhn,

“Robert Boyle and Structural Chemistry in the Seventeenth Century,” Isis, XLIII (1952),

12-36.

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V. The Priority of Paradigms

To discover the relation between rules, paradigms, and normal

science, consider first how the historian isolates the particular loci of

commitment that have just been described as accepted rules. Close

historical investigation of a given specialty at a given time discloses a set

of recurrent and quasi-standard illustrations of various theories in their

conceptual, observational, and instrumental applications. These are the

community’s paradigms, revealed in its textbooks, lectures, and

laboratory exercises. By studying them and by practicing with them, the

members of the corresponding community learn their trade. The

historian, of course, will discover in addition a penumbral area occupied

by achievements whose status is still in doubt, but the core of solved

problems and techniques will usually be clear. Despite occasional

ambiguities, the paradigms of a mature scientific community can be

determined with relative ease.

The determination of shared paradigms is not, however, the

determination of shared rules. That demands a second step and one of a

somewhat different kind. When undertaking it, the historian must

compare the community’s paradigms with each other and with its

current research reports. In doing so, her object is to discover what

isolable elements, explicit or implicit, the members of that community

may have abstracted from their more global paradigms and deployed as

rules in their research. Anyone who has attempted to describe or

analyze the evolution of a particular scientific tradition will necessarily

have sought accepted principles and rules of this sort. Almost certainly,

as the preceding section indicates, she will have met with at least partial

success. But, if her experience has been at all like my own, she will have

found the search for rules both more difficult and less satisfying than

the search for paradigms. Some of the generalizations she employs to

describe the community’s shared beliefs will present no problems.

Others, however, in-

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eluding some of those used as illustrations above, will seem a shade too

strong. Phrased in just that way, or in any other way she can imagine,

they would almost certainly have been rejected by some members of the

group she studies. Nevertheless, if the coherence of the research

tradition is to be understood in terms of rules, some specification of

common ground in the corresponding area is needed. As a result, the

search for a body of rules competent to constitute a given normal

research tradition becomes a source of continual and deep frustration.

Recognizing that frustration, however, makes it possible to diagnose

its source. Scientists can agree that a Newton, Lavoisier, Maxwell, or

Einstein has produced an apparently permanent solution to a group of

outstanding problems and still disagree, sometimes without being aware

of it, about the particular abstract characteristics that make those

solutions permanent. They can, that is, agree in their identification of a

paradigm without agreeing on, or even attempting to produce, a full

interpretation or rationalization of it. Lack of a standard interpretation or

of an agreed reduction to rules will not prevent a paradigm from

guiding research. Normal science can be determined in part by the

direct inspection of paradigms, a process that is often aided by but does

not depend upon the formulation of rules and assumptions. Indeed, the

existence of a paradigm need not even imply that any full set of rules

exists.1

Inevitably, the first effect of those statements is to raise problems. In

the absence of a competent body of rules, what restricts the scientist to a

particular normal-scientific tradition? What can the phrase ‘direct

inspection of paradigms’ mean? Partial answers to questions like these

were developed by the late Ludwig Wittgenstein, though in a very

different context. Because that context is both more elementary and

more familiar, it will help to consider her form of the argument first.

What need we know, Wittgenstein asked, in order that we

1 Michael Polanyi has brilliantly developed a very similar theme, arguing that much of

the scientist’s success depends upon “tacit knowledge,” i.e., upon knowledge that is

acquired through practice and that cannot be articulated explicitly. See her Personal

Knowledge (Chicago, 1958), particularly chaps, v and vi.

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apply terms like ‘chair,’ or ‘leaf,’ or ‘game’ unequivocally and without

provoking argument?2

That question is very old and has generally been answered by saying

that we must know, consciously or intuitively, what a chair, or leaf, or

game is. We must, that is, grasp some set of attributes that all games and

that only games have in common. Wittgenstein, however, concluded

that, given the way we use language and the sort of world to which we

apply it, there need be no such set of characteristics. Though a

discussion of some of the attributes shared by a number of games or

chairs or leaves often helps us learn how to employ the corresponding

term, there is no set of characteristics that is simultaneously applicable

to all members of the class and to them alone. Instead, confronted with

a previously unobserved activity, we apply the term ‘game’ because what

we are seeing bears a close “family resemblance” to a number of the

activities that we have previously learned to call by that name. For

Wittgenstein, in short, games, and chairs, and leaves are natural

families, each constituted by a network of overlapping and crisscross

resemblances. The existence of such a network sufficiently accounts for

our success in identifying the corresponding object or activity. Only if

the families we named overlapped and merged gradually into one

another—only, that is, if there were no natural families-would our

success in identifying and naming provide evidence for a set of common

characteristics corresponding to each of the class names we employ.

Something of the same sort may very well hold for the various

research problems and techniques that arise within a single normal-

scientific tradition. What these have in common is not that they satisfy

some explicit or even some fully discoverable set of rules and

assumptions that gives the tradition its character and its hold upon the

scientific mind. Instead, they may relate by resemblance and by

modeling to one or another part of the scientific corpus which the

community in question al-

2 Ludwig Wittgenstein, Philosophical Investigations, trans. G. E. M. Anscombe (New York,

1953), pp. 31-36. Wittgenstein, however, says almost nothing about the sort of world

necessary to support the naming procedure she outlines. Part of the point that follows

cannot therefore be attributed toher.

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ready recognizes as among its established achievements. Scientists work

from models acquired through education and through subsequent

exposure to the literature often without quite knowing or needing to

know what characteristics have given these models the status of

community paradigms. And because they do so, they need no full set of

rules. The coherence displayed by the research tradition in which they

participate may not imply even the existence of an underlying body of

rules and assumptions that additional historical or philosophical

investigation might uncover. That scientists do not usually ask or debate

what makes a particular problem or solution legitimate tempts us to

suppose that, at least intuitively, they know the answer. But it may only

indicate that neither the question nor the answer is felt to be relevant to

their research. Paradigms may be prior to, more binding, and more

complete than any set of rules for research that could be unequivocally

abstracted from them. So far this point has been entirely theoretical:

paradigms could determine normal science without the intervention of

discoverable rules. Let me now try to increase both its clarity and

urgency by indicating some of the reasons for believing that paradigms

actually do operate in this manner. The first, which has already been

discussed quite fully, is the severe difficulty of discovering the rules that

have guided particular normal-scientific traditions. That difficulty is

very nearly the same as the one the philosopher encounters when she

tries to say what all games have in common. The second, to which the

first is really a corollary, is rooted in the nature of scientific education.

Scientists, it should already be clear, never learn concepts, laws, and

theories in the abstract and by themselves. Instead, these intellectual

tools are from the start encountered in a historically and pedagogically

prior unit that displays them with and through their applications. A new

theory is always announced together with applications to some concrete

range of natural phenomena; without them it would not be even a

candidate for acceptance. After it has been accepted, those same

applications or others accompany the theory into the textbooks from

which the future practitioner will learn her trade. They are not there

merely as

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embroidery or even as documentation. On the contrary, the process of

learning a theory depends upon the study of applications, including

practice problem-solving both with a pencil and paper and with

instruments in the laboratory. If, for example, the student of Newtonian

dynamics ever discovers the meaning of terms like ‘force,’ ‘mass,’ ‘space,’

and ‘time,’ she does so less from the incomplete though sometimes

helpful definitions in her text than by observing and participating in the

application of these concepts to problem-solution.

That process of learning by finger exercise or by doing continues

throughout the process of professional initiation. As the student

proceeds from her freshman course to and through her doctoral

dissertation, the problems assigned to her become more complex and

less completely precedented. But they continue to be closely modeled

on previous achievements as are the problems that normally occupy

him during her subsequent independent scientific career. One is at

liberty to suppose that somewhere along the way the scientist has

intuitively abstracted rules of the game for himself, but there is little

reason to believe it. Though many scientists talk easily and well about

the particular individual hypotheses that underlie a concrete piece of

current research, they are little better than laymen at characterizing the

established bases of their field, its legitimate problems and methods. If

they have learned such abstractions at all, they show it mainly through

their ability to do successful research. That ability can, however, be

understood without recourse to hypothetical rules of the game.

These consequences of scientific education have a converse that

provides a third reason to suppose that paradigms guide research by

direct modeling as well as through abstracted rules. Normal science can

proceed without rules only so long as the relevant scientific community

accepts without question the particular problem-solutions already

achieved. Rules should therefore become important and the

characteristic unconcern about them should vanish whenever

paradigms or models are felt to be insecure. That is, moreover, exactly

what does occur. The pre-paradigm period, in particular, is regularly

marked by frequent

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and deep debates over legitimate methods, problems, and standards of

solution, though these serve rather to define schools than to produce

agreement. We have already noted a few of these debates in optics and

electricity, and they played an even larger role in the development of

seventeenth-century chemistry and of early nineteenth-century

geology.3 Furthermore, debates like these do not vanish once and for all

with the appearance of a paradigm. Though almost non-existent during

periods of normal science, they recur regularly just before and during

scientific revolutions, the periods when paradigms are first under attack

and then subject to change. The transition from Newtonian to quantum

mechanics evoked many debates about both the nature and the

standards of physics, some of which still continue.4 There are people

alive today who can remember the similar arguments engendered by

Maxwell’s electromagnetic theory and by statistical mechanics.5 And

earlier still, the assimilation of Galileo’s and Newton’s mechanics gave

rise to a particularly famous series of debates with Aristotelians,

Cartesians, and Leibnizians about the standards legitimate to science.6

When scientists disagree about whether the fundamental problems of

their field have been solved, the search for rules gains a function that it

does not ordinarily possess. While

3 For chemistry, see H. Metzger, Les doctrines chimiques en France du début du XVIIe à la

fin du XVIIIe siècle (Paris, 1923), pp. 24-27, 146-49; and Marie Boas, Robert Boyle and

Seventeenth-Century Chemistry (Cambridge, 1958), chap. ii. For geology, see Walter F.

Cannon, “The Uniformitarian-Catastrophist Debate,” his, LI (1960), 38-55; and C. C.

Gillispie, Genesis and Geology (Cambridge, Mass., 1951), chaps, iv-v.

4 For controversies over quantum mechanics, see Jean Ullmo, La crise de la physique

quantique (Paris, 1950), chap. ii.

5 For statistical mechanics, see René Dugas, La théorie physique au sens de Boltzmann et

ses prolongements modernes (Neuchatel, 1959), pp. 158-84, 206-19. For the reception of

Maxwell’s work, see Max Planck, “Maxwell’s Influence in Germany,” in James Clerk

Maxwell: A Commemoration Volume, 1831-1931 (Cambridge, 1931), pp. 45-65, esp. pp. 58-

63; and Silvanus P. Thompson, The Life of William Thomson Baron Kelvin of Largs

(London, 1910), II, 1021-27.

6 For a sample of the battle with the Aristotelians, see A. Koyré, “A Documentary

History of the Problem of Fall from Kepler to Newton,” Transactions of the American

Philosophical Society, XLV (1955), 329-95. For the debates with the Cartesians and

Leibnizians, see Pierre Brunet, L’introduction des théories de Newton en France au XVIIe

siècle (Paris, 1931); and A. Koyré, From the Closed World to the Infinite Universe

(Baltimore, 1957), chap. xi.

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paradigms remain secure, however, they can function without

agreement over rationalization or without any attempted rationalization

at all.

A fourth reason for granting paradigms a status prior to that of

shared rules and assumptions can conclude this section. The

introduction to this essay suggested that there can be small revolutions

as well as large ones, that some revolutions affect only the members of a

professional subspecialty, and that for such groups even the discovery of

a new and unexpected phenomenon may be revolutionary. The next

section will introduce selected revolutions of that sort, and it is still far

from clear how they can exist. If normal science is so rigid and if

scientific communities are so close-knit as the preceding discussion has

implied, how can a change of paradigm ever affect only a small

subgroup? What has been said so far may have seemed to imply that

normal science is a single monolithic and unified enterprise that must

stand or fall with any one of its paradigms as well as with all of them

together. But science is obviously seldom or never like that. Often,

viewing all fields together, it seems instead a rather ramshackle

structure with little coherence among its various parts. Nothing said to

this point should, however, conflict with that very familiar observation.

On the contrary, substituting paradigms for rules should make the

diversity of scientific fields and specialties easier to understand. Explicit

rules, when they exist, are usually common to a very broad scientific

group, but paradigms need not be. The practitioners of widely separated

fields, say astronomy and taxonomic botany, are educated by exposure

to quite different achievements described in very different books. And

even women who, being in the same or in closely related fields, begin by

studying many of the same books and achievements may acquire rather

different paradigms in the course of professional specialization.

Consider, for a single example, the quite large and diverse community

constituted by all physical scientists. Each member of that group today

is taught the laws of, say, quantum mechanics, and most of them employ

these laws at some point in

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their research or teaching. But they do not all learn the same

applications of these laws, and they are not therefore all affected in the

same ways by changes in quantum-mechanical practice. On the road to

professional specialization, a few physical scientists encounter only the

basic principles of quantum mechanics. Others study in detail the

paradigm applications of these principles to chemistry, still others to the

physics of the solid state, and so on. What quantum mechanics means to

each of them depends upon what courses she has had, what texts she has

read, and which journals she studies. It follows that, though a change in

quantum-mechanical law will be revolutionary for all of these groups, a

change that reflects only on one or another of the paradigm applications

of quantum mechanics need be revolutionary only for the members of a

particular professional subspecialty. For the rest of the profession and

for those who practice other physical sciences, that change need not be

revolutionary at all. In short, though quantum mechanics (or

Newtonian dynamics, or electromagnetic theory) is a paradigm for

many scientific groups, it is not the same paradigm for them all.

Therefore, it can simultaneously determine several traditions of normal

science that overlap without being coextensive. A revolution produced

within one of these traditions will not necessarily extend to the others as

well.

One brief illustration of specialization’s effect may give this whole

series of points additional force. An investigator who hoped to learn

something about what scientists took the atomic theory to be asked a

distinguished physicist and an eminent chemist whether a single atom

of helium was or was not a molecule. Both answered without hesitation,

but their answers were not the same. For the chemist the atom of

helium was a molecule because it behaved like one with respect to the

kinetic theory of gases. For the physicist, on the other hand, the helium

atom was not a molecule because it displayed no molecular spectrum.7

Presumably both women were talking of the same par-

7 The investigator was James K. Senior, to whom I am indebted for a verbal

report. Some related issues are treated in her paper, “The Vernacular of the

Laboratory,” Philosophy of Science, XXV (1958), 163-68.

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ticle, but they were viewing it through their own research training and

practice. Their experience in problem-solving told them what a

molecule must be. Undoubtedly their experiences had had much in

common, but they did not, in this case, tell the two specialists the same

thing. As we proceed we shall discover how consequential paradigm

differences of this sort can occasionally be.

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VI. Anomaly and the Emergence of Scientific Discoveries

Normal science, the puzzle-solving activity we have just examined, is

a highly cumulative enterprise, eminently successful in its aim, the

steady extension of the scope and precision of scientific knowledge. In

all these respects it fits with great precision the most usual image of

scientific work. Yet one standard product of the scientific enterprise is

missing. Normal science does not aim at novelties of fact or theory and,

when successful, finds none. New and unsuspected phenomena are,

however, repeatedly uncovered by scientific research, and radical new

theories have again and again been invented by scientists. History even

suggests that the scientific enterprise has developed a uniquely powerful

technique for producing surprises of this sort. If this characteristic of

science is to be reconciled with what has already been said, then

research under a paradigm must be a particularly effective way of

inducing paradigm change. That is what fundamental novelties of fact

and theory do. Produced inadvertently by a game played under one set

of rules, their assimilation requires the elaboration of another set. After

they have become parts of science, the enterprise, at least of those

specialists in whose particular field the novelties lie, is never quite the

same again.

We must now ask how changes of this sort can come about, considering

first discoveries, or novelties of fact, and then inventions, or novelties of

theory. That distinction between discovery and invention or between

fact and theory will, however, immediately prove to be exceedingly

artificial. Its artificiality is an important clue to several of this essay’s

main theses. Examining selected discoveries in the rest of this section,

we shall quickly find that they are not isolated events but extended

episodes with a regularly recurrent structure. Discovery commences

with the awareness of anomaly, i.e., with the recognition that nature has

somehow violated the paradigm-induced

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expectations that govern normal science. It then continues with a more

or less extended exploration of the area of anomaly. And it closes only

when the paradigm theory has been adjusted so that the anomalous has

become the expected. Assimilating a new sort of fact demands a more

than additive adjustment of theory, and until that adjustment is

completed—until the scientist has learned to see nature in a different

way—the new fact is not quite a scientific fact at all.

To see how closely factual and theoretical novelty are intertwined in

scientific discovery examine a particularly famous example, the

discovery of oxygen. At least three different women have a legitimate claim

to it, and several other chemists must, in the early 1770’s, have had

enriched air in a laboratory vessel without knowing it.1 The progress of

normal science, in this case of pneumatic chemistry, prepared the way

to a breakthrough quite thoroughly. The earliest of the claimants to

prepare a relatively pure sample of the gas was the Swedish apothecary,

C. W. Scheele. We may, however, ignore her work since it was not

published until oxygen’s discovery had repeatedly been announced

elsewhere and thus had no effect upon the historical pattern that most

concerns us here.2 The second in time to establish a claim was the

British scientist and divine, Joseph Priestley, who collected the gas

released by heated red oxide of mercury as one item in a prolonged

normal investigation of the “airs” evolved by a large number of solid

substances. In 1774 she identified the gas thus produced as nitrous oxide

and in 1775, led by further tests, as common air with less than its usual

quantity of phlogiston. The third claimant, Lavoisier, started the work

that led her to oxygen after Priestley’s experiments of 1774 and possibly

as the result of a hint from Priestley. Early in

1 For the still classic discussion of oxygen’s discovery, see A. N. Meldrum, The

Eighteenth-Century Revolution in Science—the First Phase (Calcutta, 1930), chap. v.

An indispensable recent review, including an account of the priority controversy,

is Maurice Daumas, Lavoisier, théoricien et expérimentateur (Paris, 1955), chaps, ii-

iii. For a fuller account and bibliography, see also T. S. Kuhn, “The Historical

Structure of Scientific Discovery,” Science, CXXXVI (June 1, 1962), 760-64.

2 See, however, Uno Bocklund, “A Lost Letter from Scheele to Lavoisier,”

Lychnos, 1957-58, pp. 39-62, for a different evaluation of Scheele’s role.

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1775 Lavoisier reported that the gas obtained by heating the red oxide of

mercury was “air itself entire without alteration [except that] . . . it

comes out more pure, more respirable.”3 By 1777, probably with the

assistance of a second hint from Priestley, Lavoisier had concluded that

the gas was a distinct species, one of the two main constituents of the

atmosphere, a conclusion that Priestley was never able to accept.

This pattern of discovery raises a question that can be asked about

every novel phenomenon that has ever entered the consciousness of

scientists. Was it Priestley or Lavoisier, if either, who first discovered

oxygen? In any case, when was oxygen discovered? In that form the

question could be asked even if only one claimant had existed. As a

ruling about priority and date, an answer does not at all concern us.

Nevertheless, an attempt to produce one will illuminate the nature of

discovery, because there is no answer of the kind that is sought.

Discovery is not the sort of process about which the question is

appropriately asked. The fact that it is asked—the priority for oxygen has

repeatedly been contested since the 1780’s—is a symptom of something

askew in the image of science that gives discovery so fundamental a role.

Look once more at our example. Priestley’s claim to the discovery of

oxygen is based upon her priority in isolating a gas that was later

recognized as a distinct species. But Priestley’s sample was not pure,

and, if holding impure oxygen in one’s hands is to discover it, that had

been done by everyone who ever bottled atmospheric air. Besides, if

Priestley was the discoverer, when was the discovery made? In 1774 she

thought she had obtained nitrous oxide, a species she already knew; in

1775 she saw the gas as dephlogisticated air, which is still not oxygen or

even, for phlogistic chemists, a quite unexpected sort of gas. Lavoisier’s

claim may be stronger, but it presents the same problems. If we refuse

the palm to Priestley, we cannot award it to Lavoisier for the work of

1775 which led

3 J. B. Conant, The Overthrow of the Phlogiston Theory: The Chemical Revolution of

1775-1789 (“Harvard Case Histories in Experimental Science,” Case 2; Cambridge,

Mass., 1950), p. 23. This very useful pamphlet reprints many of the relevant

documents.

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him to identify the gas as the “air itself entire.” Presumably we wait for

the work of 1776 and 1777 which led Lavoisier to see not merely the gas

but what the gas was. Yet even this award could be questioned, for in

1777 and to the end of her life Lavoisier insisted that oxygen was an

atomic “principle of acidity” and that oxygen gas was formed only when

that “principle” united with caloric, the matter of heat.4 Shall we

therefore say that oxygen had not yet been discovered in 1777? Some

may be tempted to do so. But the principle of acidity was not banished

from chemistry until after 1810, and caloric lingered until the 1860’s.

Oxygen had become a standard chemical substance before either of

those dates.

Clearly we need a new vocabulary and concepts for analyzing events

like the discovery of oxygen. Though undoubtedly correct, the sentence,

“Oxygen was discovered,” misleads by suggesting that discovering

something is a single simple act assimilable to our usual (and also

questionable) concept of seeing. That is why we so readily assume that

discovering, like seeing or touching, should be unequivocally

attributable to an individual and to a moment in time. But the latter

attribution is always impossible, and the former often is as well.

Ignoring Scheele, we can safely say that oxygen had not been discovered

before 1774, and we would probably also say that it had been discovered

by 1777 or shortly thereafter. But within those limits or others like

them, any attempt to date the discovery must inevitably be arbitrary

because discovering a new sort of phenomenon is necessarily a complex

event, one which involves recognizing both that something is and what it

is. Note, for example, that if oxygen were dephlogisticated air for us, we

should insist without hesitation that Priestley had discovered it, though

we would still not know quite when. But if both observation and

conceptualization, fact and assimilation to theory, are inseparably linked

in discovery, then discovery is a process and must take time. Only when

all the relevant conceptual categories are prepared in advance, in which

case the phenomenon would not

4 H. Metzger, La philosophie de la matière chez Lavoisier (Paris, 1935); and Daumas,

op. cit., chap. vii.

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be of a new sort, can discovering that and discovering what occur

effortlessly, together, and in an instant.

Grant now that discovery involves an extended, though not

necessarily long, process of conceptual assimilation. Can we also say that

it involves a change in paradigm? To that question, no general answer

can yet be given, but in this case at least, the answer must be yes. What

Lavoisier announced in her papers from 1777 on was not so much the

discovery of oxygen as the oxygen theory of combustion. That theory

was the keystone for a reformulation of chemistry so vast that it is

usually called the chemical revolution. Indeed, if the discovery of

oxygen had not been an intimate part of the emergence of a new

paradigm for chemistry, the question of priority from which we began

would never have seemed so important. In this case as in others, the

value placed upon a new phenomenon and thus upon its discoverer

varies with our estimate of the extent to which the phenomenon

violated paradigm-induced anticipations. Notice, however, since it will

be important later, that the discovery of oxygen was not by itself the

cause of the change in chemical theory. Long before she played any part

in the discovery of the new gas, Lavoisier was convinced both that

something was wrong with the phlogiston theory and that burning

bodies absorbed some part of the atmosphere. That much she had

recorded in a sealed note deposited with the Secretary of the French

Academy in 1772.5 What the work on oxygen did was to give much

additional form and structure to Lavoisier’s earlier sense that something

was amiss. It told her a thing she was already prepared to discover—the

nature of the substance that combustion removes from the atmosphere.

That advance awareness of difficulties must be a significant part of what

enabled Lavoisier to see in experiments like Priestley’s a gas that

Priestley had been unable to see there himself. Conversely, the fact that

a major paradigm revision was needed to see what Lavoisier saw must be

the principal reason why Priestley was, to the end of her long life, unable

to see it.

5 The most authoritative account of the origin of Lavoisier’s discontent is Henry

Guerlac, Lavoisier—the Crucial Year: The Background and Origin of Her First

Experiments on Combustion in 1772 (Ithaca, N.Y., 1961).

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Two other and far briefer examples will reinforce much that has just

been said and simultaneously carry us from an elucidation of the nature

of discoveries toward an understanding of the circumstances under

which they emerge in science. In an effort to represent the main ways in

which discoveries can come about, these examples are chosen to be

different both from each other and from the discovery of oxygen. The

first, X-rays, is a classic case of discovery through accident, a type that

occurs more frequently than the impersonal standards of scientific

reporting allow us easily to realize. Its story opens on the day that the

physicist Roentgen interrupted a normal investigation of cathode rays

because she had noticed that a barium platino-cyanide screen at some

distance from her shielded apparatus glowed when the discharge was in

process. Further investigations—they required seven hectic weeks

during which Roentgen rarely left the laboratory—indicated that the

cause of the glow came in straight lines from the cathode ray tube, that

the radiation cast shadows, could not be deflected by a magnet, and

much else besides. Before announcing her discovery, Roentgen had

convinced himself that her effect was not due to cathode rays but to an

agent with at least some similarity to light.6

Even so brief an epitome reveals striking resemblances to the

discovery of oxygen: before experimenting with red oxide of mercury,

Lavoisier had performed experiments that did not produce the results

anticipated under the phlogiston paradigm; Roentgen’s discovery

commenced with the recognition that her screen glowed when it should

not. In both cases the perception of anomaly—of a phenomenon, that is,

for which her paradigm had not readied the investigator—played an

essential role in preparing the way for perception of novelty. But, again

in both cases, the perception that something had gone wrong was only

the prelude to discovery. Neither oxygen nor X-rays emerged without a

further process of experimentation and assimilation. At what point in

Roentgen’s investigation, for example, ought we say that X-rays had

actually been discovered? Not, in any

6 L. W. Taylor, Physics, the Pioneer Science (Boston, 1941), pp. 790-94; and T. W.

Chalmers, Historic Researches (London, 1949), pp. 218-19.

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case, at the first instant, when all that had been noted was a glowing

screen. At least one other investigator had seen that glow and, to her

subsequent chagrin, discovered nothing at all.7 Nor, it is almost as clear,

can the moment of discovery be pushed forward to a point during the

last week of investigation, by which time Roentgen was exploring the

properties of the new radiation she had already discovered. We can only

say that X-rays emerged in Würzburg between November 8 and

December 28, 1895.

In a third area, however, the existence of significant parallels between

the discoveries of oxygen and of X-rays is far less apparent. Unlike the

discovery of oxygen, that of X-rays was not, at least for a decade after the

event, implicated in any obvious upheaval in scientific theory. In what

sense, then, can the assimilation of that discovery be said to have

necessitated paradigm change? The case for denying such a change is

very strong. To be sure, the paradigms subscribed to by Roentgen and

his contemporaries could not have been used to predict X-rays.

(Maxwell’s electromagnetic theory had not yet been accepted

everywhere, and the particulate theory of cathode rays was only one of

several current speculations.) But neither did those paradigms, at least

in any obvious sense, prohibit the existence of X-rays as the phlogiston

theory had prohibited Lavoisier’s interpretation of Priestley’s gas. On the

contrary, in 1895 accepted scientific theory and practice admitted a

number of forms of radiation—visible, infrared, and ultraviolet. Why

could not X-rays have been accepted as just one more form of a well-

known class of natural phenomena? Why were they not, for example,

received in the same way as the discovery of an additional chemical

element? New elements to fill empty places in the periodic table were

still being sought and found in Roentgen’s day. Their pursuit was a

standard project for normal science, and success was an occasion only

for congratulations, not for surprise.

7 E. T. Whittaker, A History of the Theories of Aether and Electricity, I (2d ed.;

London, 1951), 358, n. 1. Sir George Thomson has informed me of a second near

miss. Alerted by unaccountably fogged photographic plates, Sir William Crookes

was also on the track of the discovery.

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X-rays, however, were greeted not only with surprise but with shock.

Lord Kelvin at first pronounced them an elaborate hoax.8 Others,

though they could not doubt the evidence, were clearly staggered by it.

Though X-rays were not prohibited by established theory, they violated

deeply entrenched expectations. Those expectations, I suggest, were

implicit in the design and interpretation of established laboratory

procedures. By the 1890’s cathode ray equipment was widely deployed

in numerous European laboratories. If Roentgen’s apparatus had

produced X-rays, then a number of other experimentalists must for

some time have been producing those rays without knowing it. Perhaps

those rays, which might well have other unacknowledged sources too,

were implicated in behavior previously explained without reference to

them. At the very least, several sorts of long familiar apparatus would in

the future have to be shielded with lead. Previously completed work on

normal projects would now have to be done again because earlier

scientists had failed to recognize and control a relevant variable. X-rays,

to be sure, opened up a new field and thus added to the potential

domain of normal science. But they also, and this is now the more

important point, changed fields that had already existed. In the process

they denied previously paradigmatic types of instrumentation their

right to that title.

In short, consciously or not, the decision to employ a particular piece

of apparatus and to use it in a particular way carries an assumption that

only certain sorts of circumstances will arise. There are instrumental as

well as theoretical expectations, and they have often played a decisive

role in scientific development. One such expectation is, for example,

part of the story of oxygen’s belated discovery. Using a standard test for

“the goodness of air,” both Priestley and Lavoisier mixed two volumes of

their gas with one volume of nitric oxide, shook the mixture over water,

and measured the volume of the gaseous residue. The previous

experience from which this standard procedure had evolved assured

them that with atmospheric air the residue

8 Silvanus P. Thompson, The Life of Sir William Thomson Baron Kelvin of Largs

(London, 1910), II, 1125.

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would be one volume and that for any other gas (or for polluted air) it

would be greater. In the oxygen experiments both found a residue close

to one volume and identified the gas accordingly. Only much later and

in part through an accident did Priestley renounce the standard

procedure and try mixing nitric oxide with her gas in other proportions.

She then found that with quadruple the volume of nitric oxide there was

almost no residue at all. Her commitment to the original test procedure—

a procedure sanctioned by much previous experience—had been

simultaneously a commitment to the non-existence of gases that could

behave as oxygen did.9

Illustrations of this sort could be multiplied by reference, for

example, to the belated identification of uranium fission. One reason

why that nuclear reaction proved especially difficult to recognize was

that women who knew what to expect when bombarding uranium chose

chemical tests aimed mainly at elements from the upper end of the

periodic table.10 Ought we conclude from the frequency with which

such instrumental commitments prove misleading that science should

abandon standard tests and standard instruments? That would result in

an inconceivable method of research. Paradigm procedures and

applications are as necessary to science as paradigm laws and theories,

and they have the same effects. Inevitably they restrict the phenom-

enological field accessible for scientific investigation at any

9 Conant, op. cit., pp. 18-20.

10 K. K. Darrow, “Nuclear Fission,” Bell System Technical Journal, XIX (1940),

267-89. Krypton, one of the two main fission products, seems not to have been

identified by chemical means until after the reaction was well understood.

Barium, the other product, was almost identified chemically at a late stage of the

investigation because, as it happened, that element had to be added to the

radioactive solution to precipitate the heavy element for which nuclear chemists

were looking. Failure to separate that added barium from the radioactive product

finally led, after the reaction had been repeatedly investigated for almost five

years, to the following report: “As chemists we should be led by this research . . .

to change all the names in the preceding [reaction] schema and thus write Ba, La,

Ce instead of Ra, Ac, Th. But as ‘nuclear chemists,’ with close affiliations to

physics, we cannot bring ourselves to this leap which would contradict all pre-

vious experience of nuclear physics. It may be that a series of strange accidents

renders our results deceptive” (Otto Hahn and Fritz Strassman, “Uber den Nach-

weis und das Verhalten der bei der Bestrahlung des Urans mittels Neutronen

entstehended Erdalkalimetalle,” Die Naturwissenschaften, XXVII [1939], 15).

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given time. Recognizing that much, we may simultaneously see an

essential sense in which a discovery like X-rays necessitates paradigm

change—and therefore change in both procedures and expectations—for

a special segment of the scientific community. As a result, we may also

understand how the discovery of X-rays could seem to open a strange

new world to many scientists and could thus participate so effectively in

the crisis that led to twentieth-century physics.

Our final example of scientific discovery, that of the Leyden jar,

belongs to a class that may be described as theory-induced. Initially, the

term may seem paradoxical. Much that has been said so far suggests that

discoveries predicted by theory in advance are parts of normal science

and result in no new sort of fact. I have, for example, previously referred

to the discoveries of new chemical elements during the second half of

the nineteenth century as proceeding from normal science in that way.

But not all theories are paradigm theories. Both during pre-paradigm

periods and during the crises that lead to large-scale changes of

paradigm, scientists usually develop many speculative and unarticulated

theories that can themselves point the way to discovery. Often, however,

that discovery is not quite the one anticipated by the speculative and

tentative hypothesis. Only as experiment and tentative theory are

together articulated to a match does the discovery emerge and the

theory become a paradigm.

The discovery of the Leyden jar displays all these features as well as

the others we have observed before. When it began, there was no single

paradigm for electrical research. Instead, a number of theories, all

derived from relatively accessible phenomena, were in competition.

None of them succeeded in ordering the whole variety of electrical

phenomena very well. That failure is the source of several of the

anomalies that provide background for the discovery of the Leyden jar.

One of the competing schools of electricians took electricity to be a fluid,

and that conception led a number of women to attempt bottling the fluid

by holding a water-filled glass vial in their hands and touching the water

to a conductor suspended from an active

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electrostatic generator. On removing the jar from the machine and

touching the water (or a conductor connected to it) with her free hand,

each of these investigators experienced a severe shock. Those first

experiments did not, however, provide electricians with the Leyden jar.

That device emerged more slowly, and it is again impossible to say just

when its discovery was completed. The initial attempts to store electrical

fluid worked only because investigators held the vial in their hands

while standing upon the ground. Electricians had still to learn that the

jar required an outer as well as an inner conducting coating and that the

fluid is not really stored in the jar at all. Somewhere in the course of the

investigations that showed them this, and which introduced them to

several other anomalous effects, the device that we call the Leyden jar

emerged. Furthermore, the experiments that led to its emergence, many

of them performed by Franklin, were also the ones that necessitated the

drastic revision of the fluid theory and thus provided the first full

paradigm for electricity.11

To a greater or lesser extent (corresponding to the continuum from

the shocking to the anticipated result), the characteristics common to

the three examples above are characteristic of all discoveries from which

new sorts of phenomena emerge. Those characteristics include: the

previous awareness of anomaly, the gradual and simultaneous

emergence of both observational and conceptual recognition, and the

consequent change of paradigm categories and procedures often

accompanied by resistance. There is even evidence that these same

characteristics are built into the nature of the perceptual process itself.

In a psychological experiment that deserves to be far better known

outside the trade, Bruner and Postman asked experimental subjects to

identify on short and controlled exposure a series of playing cards.

Many of the cards were normal, but some were made anoma-

11 For various stages in the Leydun jar’s evolution, see I. B. Cohen, Franklin and

Newton: An Inquiry into Speculative Newtonian Experimental Science and Franklin’s

Work in Electricity as an Example Thereof (Philadelphia, 1956), pp. 385-86, 400-

406, 452-67, 509-7. The last stage is described by Whittaker, op. cit., pp. 50-52.

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lous, e.g., a red six of spades and a black four of hearts. Each

experimental run was constituted by the display of a single card to a

single subject in a series of gradually increased exposures. After each

exposure the subject was asked what she had seen, and the run was

terminated by two successive correct identifications.12

Even on the shortest exposures many subjects identified most of the

cards, and after a small increase all the subjects identified them all. For

the normal cards these identifications were usually correct, but the

anomalous cards were almost always identified, without apparent

hesitation or puzzlement, as normal. The black four of hearts might, for

example, be identified as the four of either spades or hearts. Without

any awareness of trouble, it was immediately fitted to one of the

conceptual categories prepared by prior experience. One would not even

like to say that the subjects had seen something different from what

they identified. With a further increase of exposure to the anomalous

cards, subjects did begin to hesitate and to display awareness of

anomaly. Exposed, for example, to the red six of spades, some would say:

That’s the six of spades, but there’s something wrong with it—the black

has a red border. Further increase of exposure resulted in still more

hesitation and confusion until finally, and sometimes quite suddenly,

most subjects would produce the correct identification without

hesitation. Moreover, after doing this with two or three of the

anomalous cards, they would have little further difficulty with the

others. A few subjects, however, were never able to make the requisite

adjustment of their categories. Even at forty times the average exposure

required to recognize normal cards for what they were, more than 10

per cent of the anomalous cards were not correctly identified. And the

subjects who then failed often experienced acute personal distress. One

of them exclaimed: “I can’t make the suit out, whatever it is. It didn’t

even look like a card that time. I don’t know what color it is now or

whether it’s a spade or a heart. I’m

12 J. S. Bruner and Leo Postman, “On the Perception of Incongruity: A Paradigm,”

Journal of Personality, XVIII (1949), 206-23.

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not even sure now what a spade looks like. My God!”13 In the next

section we shall occasionally see scientists behaving this way too.

Either as a metaphor or because it reflects the nature of the mind,

that psychological experiment provides a wonderfully simple and cogent

schema for the process of scientific discovery. In science, as in the

playing card experiment, novelty emerges only with difficulty,

manifested by resistance, against a background provided by expectation.

Initially, only the anticipated and usual are experienced even under

circumstances where anomaly is later to be observed. Further

acquaintance, however, does result in awareness of something wrong or

does relate the effect to something that has gone wrong before. That

awareness of anomaly opens a period in which conceptual categories are

adjusted until the initially anomalous has become the anticipated. At

this point the discovery has been completed. I have already urged that

that process or one very much like it is involved in the emergence of all

fundamental scientific novelties. Let me now point out that, recognizing

the process, we can at last begin to see why normal science, a pursuit not

directed to novelties and tending at first to suppress them, should

nevertheless be so effective in causing them to arise.

In the development of any science, the first received paradigm is

usually felt to account quite successfully for most of the observations

and experiments easily accessible to that science’s practitioners. Further

development, therefore, ordinarily calls for the construction of elaborate

equipment, the development of an esoteric vocabulary and skills, and a

refinement of concepts that increasingly lessens their resemblance to

their usual common-sense prototypes. That professionalization leads, on

the one hand, to an immense restriction of the scientist’s vision and to a

considerable resistance to paradigm change. The science has become

increasingly rigid. On the other hand, within those areas to which the

paradigm directs the attention of the

13 Ibid., p. 218. My colleague Postman tells me that, though knowing all about the

apparatus and display in advance, she nevertheless found looking at the

incongruous cards acutely uncomfortable.

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group, normal science leads to a detail of information and to a precision

of the observation-theory match that could be achieved in no other way.

Furthermore, that detail and precision-of-match have a value that

transcends their not always very high intrinsic interest. Without the

special apparatus that is constructed mainly for anticipated functions,

the results that lead ultimately to novelty could not occur. And even

when the apparatus exists, novelty ordinarily emerges only for the woman

who, knowing with precision what she should expect, is able to recognize

that something has gone wrong. Anomaly appears only against the

background provided by the paradigm. The more precise and far-

reaching that paradigm is, the more sensitive an indicator it provides of

anomaly and hence of an occasion for paradigm change. In the normal

mode of discovery, even resistance to change has a use that will be

explored more fully in the next section. By ensuring that the paradigm

will not be too easily surrendered, resistance guarantees that scientists

will not be lightly distracted and that the anomalies that lead to

paradigm change will penetrate existing knowledge to the core. The very

fact that a significant scientific novelty so often emerges simultaneously

from several laboratories is an index both to the strongly traditional

nature of normal science and to the completeness with which that

traditional pursuit prepares the way for its own change.

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VII. Crisis and the Emergence of Scientific Theories

All the discoveries considered in Section VI were causes of or

contributors to paradigm change. Furthermore, the changes in which

these discoveries were implicated were all destructive as well as

constructive. After the discovery had been assimilated, scientists were

able to account for a wider range of natural phenomena or to account

with greater precision for some of those previously known. But that gain

was achieved only by discarding some previously standard beliefs or

procedures and, simultaneously, by replacing those components of the

previous paradigm with others. Shifts of this sort are, I have argued,

associated with all discoveries achieved through normal science,

excepting only the unsurprising ones that had been anticipated in all but

their details. Discoveries are not, however, the only sources of these

destructive-constructive paradigm changes. In this section we shall

begin to consider the similar, but usually far larger, shifts that result

from the invention of new theories.

Having argued already that in the sciences fact and theory, discovery

and invention, are not categorically and permanently distinct, we can

anticipate overlap between this section and the last. (The impossible

suggestion that Priestley first discovered oxygen and Lavoisier then

invented it has its attractions. Oxygen has already been encountered as

discovery; we shall shortly meet it again as invention.) In taking up the

emergence of new theories we shall inevitably extend our

understanding of discovery as well. Still, overlap is not identity. The

sorts of discoveries considered in the last section were not, at least

singly, responsible for such paradigm shifts as the Copernican,

Newtonian, chemical, and Einsteinian revolutions. Nor were they

responsible for the somewhat smaller, because more exclusively

professional, changes in paradigm produced by the wave theory of light,

the dynamical theory of heat, or Maxwell’s electromagnetic theory. How

can theories like these arise from normal

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science, an activity even less directed to their pursuit than to that of

discoveries?

If awareness of anomaly plays a role in the emergence of new sorts of

phenomena, it should surprise no one that a similar but more profound

awareness is prerequisite to all acceptable changes of theory. On this

point historical evidence is, I think, entirely unequivocal. The state of

Ptolemaic astronomy was a scandal before Copernicus’ announcement.1

Galileo’s contributions to the study of motion depended closely upon

difficulties discovered in Aristotle’s theory by scholastic critics.2

Newton’s new theory of light and color originated in the discovery that

none of the existing pre-paradigm theories would account for the length

of the spectrum, and the wave theory that replaced Newton’s was

announced in the midst of growing concern about anomalies in the

relation of diffraction and polarization effects to Newton’s theory.3

Thermodynamics was born from the collision of two existing

nineteenth-century physical theories, and quantum mechanics from a

variety of difficulties surrounding black-body radiation, specific heats,

and the photoelectric effect.4 Furthermore, in all these cases except that

of Newton the awareness of anomaly had lasted so long and penetrated

so deep that one can appropriately describe the fields affected by it as in

a state of growing crisis. Because it demands large-scale paradigm

destruction and major shifts in the problems and techniques of normal

science, the emergence of new theories is generally preceded by a period

of pronounced professional in-

1 A. R. Hall, The Scientific Revolution, 1500-1800 (London, 1954), p. 16.

2 Marshall Clagett, The Science of Mechanics in the Middle Ages (Madison, Wis.,

1959), Parts II—III. A. Koyré displays a number of medieval elements in Galileo’s

thought in her Études Galiléennes (Paris, 1939), particularly Vol. I.

3 For Newton, see T. S. Kuhn, “Newton’s Optical Papers,” in Isaac Newton’s

Papers and Letters in Natural Philosophy, ed. I. B. Cohen (Cambridge, Mass., 1958),

pp. 27-45. For the prelude to the wave theory, see E. T. Whittaker, A History of the

Theories of Aether and Electricity, I (2d ed.; London, 1951), 94-109; and W. Whewell,

History of the Inductive Sciences (rev. ed.; London, 1847), 11,396-466.

4 For thermodynamics, see Silvanus P. Thompson, Life of William Thomson

Baron Kelvin of Largs (London, 1910), I, 266-81. For the quantum theory, see Fritz

Reiche, The Quantum Theory, trans. H. S. Hatfield and II. L. Brose (London, 1922),

chaps, i-ii.

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security. As one might expect, that insecurity is generated by the

persistent failure of the puzzles of normal science to come out as they

should. Failure of existing rules is the prelude to a search for new ones.

Look first at a particularly famous case of paradigm change, the

emergence of Copernican astronomy. When its predecessor, the

Ptolemaic system, was first developed during the last two centuries

before Christ and the first two after, it was admirably successful in

predicting the changing positions of both stars and planets. No other

ancient system had performed so well; for the stars, Ptolemaic

astronomy is still widely used today as an engineering approximation;

for the planets, Ptolemy’s predictions were as good as Copernicus’. But

to be admirably successful is never, for a scientific theory, to be

completely successful. With respect both to planetary position and to

precession of the equinoxes, predictions made with Ptolemy’s system

never quite conformed with the best available observations. Further

reduction of those minor discrepancies constituted many of the

principal problems of normal astronomical research for many of

Ptolemy’s successors, just as a similar attempt to bring celestial

observation and Newtonian theory together provided normal research

problems for Newton’s eighteenth-century successors. For some time

astronomers had every reason to suppose that these attempts would be

as successful as those that had led to Ptolemy’s system. Given a

particular discrepancy, astronomers were invariably able to eliminate it

by making some particular adjustment in Ptolemy’s system of

compounded circles. But as time went on, a woman looking at the net

result of the normal research effort of many astronomers could observe

that astronomy’s complexity was increasing far more rapidly than its

accuracy and that a discrepancy corrected in one place was likely to

show up in another.5

Because the astronomical tradition was repeatedly interrupted from

outside and because, in the absence of printing, communication

between astronomers was restricted, these dif-

5 J. L. E. Dreyer, A History of Astronomy from Thales to Kepler (2d ed.; New York,

1953), chaps. xi-xii.

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ficulties were only slowly recognized. But awareness did come. By the

thirteenth century Alfonso X could proclaim that if God had consulted

him when creating the universe, she would have received good advice. In

the sixteenth century, Copernicus’ coworker, Domenico da Novara, held

that no system so cumbersome and inaccurate as the Ptolemaic had

become could possibly be true of nature. And Copernicus himself wrote

in the Preface to the De Revolutionibus that the astronomical tradition she

inherited had finally created only a monster. By the early sixteenth

century an increasing number of Europe’s best astronomers were

recognizing that the astronomical paradigm was failing in application to

its own traditional problems. That recognition was prerequisite to

Copernicus’ rejection of the Ptolemaic paradigm and her search for a

new one. Her famous preface still provides one of the classic descriptions

of a crisis state.6

Breakdown of the normal technical puzzle-solving activity is not, of

course, the only ingredient of the astronomical crisis that faced

Copernicus. An extended treatment would also discuss the social

pressure for calendar reform, a pressure that made the puzzle of

precession particularly urgent. In addition, a fuller account would

consider medieval criticism of Aristotle, the rise of Renaissance

Neoplatonism, and other significant historical elements besides. But

technical breakdown would still remain the core of the crisis. In a

mature science—and astronomy had become that in antiquity—external

factors like those cited above are principally significant in determining

the timing of breakdown, the ease with which it can be recognized, and

the area in which, because it is given particular attention, the

breakdown first occurs. Though immensely important, issues of that sort

are out of bounds for this essay.

If that much is clear in the case of the Copernican revolution, let us

turn from it to a second and rather different example, the crisis that

preceded the emergence of Lavoisier’s oxygen theory of combustion. In

the 1770’s many factors combined to generate

6 T. S. Kuhn, The Copernican Revolution (Cambridge, Mass., 1957), pp. 135-43.

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a crisis in chemistry, and historians are not altogether agreed about

either their nature or their relative importance. But two of them are

generally accepted as of first-rate significance: the rise of pneumatic

chemistry and the question of weight relations. The history of the first

begins in the seventeenth century with development of the air pump

and its deployment in chemical experimentation. During the following

century, using that pump and a number of other pneumatic devices,

chemists came increasingly to realize that air must be an active

ingredient in chemical reactions. But with a few exceptions—so

equivocal that they may not be exceptions at all—chemists continued to

believe that air was the only sort of gas. Until 1756, when Joseph Black

showed that fixed air (CO2) was consistently distinguishable from

normal air, two samples of gas were thought to be distinct only in their

impurities.7

After Black’s work the investigation of gases proceeded rapidly, most

notably in the hands of Cavendish, Priestley, and Scheele, who together

developed a number of new techniques capable of distinguishing one

sample of gas from another. All these women, from Black through Scheele,

believed in the phlogiston theory and often employed it in their design

and interpretation of experiments. Scheele actually first produced

oxygen by an elaborate chain of experiments designed to dephlogisticate

heat. Yet the net result of their experiments was a variety of gas samples

and gas properties so elaborate that the phlogiston theory proved

increasingly little able to cope with laboratory experience. Though none

of these chemists suggested that the theory should be replaced, they

were unable to apply it consistently. By the time Lavoisier began her

experiments on airs in the early 1770’s, there were almost as many

versions of the phlogiston theory as there were pneumatic chemists.8

That

7 J. R. Partington, A Short History of Chemistry (2d ed.; London, 1951), pp. 48-51,

73-85, 90-120.

8 Though their main concern is with a slightly later period, much relevant

material is scattered throughout J. R. Partington and Douglas McKie’s “Historical

Studies on the Phlogiston Theory,” Annals of Science, II (1937), 361-404; III (1938),

1-58, 337-71; and IV (1939), 337-71.

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proliferation of versions of a theory is a very usual symptom of crisis. In

his preface, Copernicus complained of it as well.

The increasing vagueness and decreasing utility of the phlogiston

theory for pneumatic chemistry were not, however, the only source of

the crisis that confronted Lavoisier. She was also much concerned to

explain the gain in weight that most bodies experience when burned or

roasted, and that again is a problem with a long prehistory. At least a few

Islamic chemists had known that some metals gain weight when roasted.

In the seventeenth century several investigators had concluded from

this same fact that a roasted metal takes up some ingredient from the

atmosphere. But in the seventeenth century that conclusion seemed

unnecessary to most chemists. If chemical reactions could alter the

volume, color, and texture of the ingredients, why should they not alter

weight as well? Weight was not always taken to be the measure of

quantity of matter. Besides, weight-gain on roasting remained an

isolated phenomenon. Most natural bodies (e.g., wood) lose weight on

roasting as the phlogiston theory was later to say they should.

During the eighteenth century, however, these initially adequate

responses to the problem of weight-gain became increasingly difficult to

maintain. Partly because the balance was increasingly used as a

standard chemical tool and partly because the development of

pneumatic chemistry made it possible and desirable to retain the

gaseous products of reactions, chemists discovered more and more cases

in which weight-gain accompanied roasting. Simultaneously, the gradual

assimilation of Newton’s gravitational theory led chemists to insist that

gain in weight must mean gain in quantity of matter. Those conclusions

did not result in rejection of the phlogiston theory, for that theory could

be adjusted in many ways. Perhaps phlogiston had negative weight, or

perhaps fire particles or something else entered the roasted body as

phlogiston left it. There were other explanations besides. But if the

problem of weight-gain did not lead to rejection, it did lead to an

increasing number of special studies in which this problem bulked large.

One of them, “On

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phlogiston considered as a substance with weight and [analyzed] in

terms of the weight changes it produces in bodies with which it unites,”

was read to the French Academy early in 1772, the year which closed

with Lavoisier’s delivery of her famous sealed note to the Academy’s

Secretary. Before that note was written a problem that had been at the

edge of the chemist’s consciousness for many years had become an

outstanding unsolved puzzle.9 Many different versions of the phlogiston

theory were being elaborated to meet it. Like the problems of pneumatic

chemistry, those of weight-gain were making it harder and harder to

know what the phlogiston theory was. Though still believed and trusted

as a working tool, a paradigm of eighteenth-century chemistry was

gradually losing its unique status. Increasingly, the research it guided

resembled that conducted under the competing schools of the pre-

paradigm period, another typical effect of crisis.

Consider now, as a third and final example, the late nineteenth

century crisis in physics that prepared the way for the emergence of

relativity theory. One root of that crisis can be traced to the late

seventeenth century when a number of natural philosophers, most

notably Leibniz, criticized Newton’s retention of an updated version of

the classic conception of absolute space.10 They were very nearly,

though never quite, able to show that absolute positions and absolute

motions were without any function at all in Newton’s system; and they

did succeed in hinting at the considerable aesthetic appeal a fully

relativistic conception of space and motion would later come to display.

But their critique was purely logical. Like the early Copernicans who

criticized Aristotle’s proofs of the earth’s stability, they did not dream

that transition to a relativistic system could have observational

consequences. At no point did they relate their views to any problems

that arose when applying Newtonian theory to nature. As a result, their

views died with

9 H. Guerlac, Lavoisier—the Crucial Year (Ithaca, N.Y., 1961). The entire book

documents the evolution and first recognition of a crisis. For a clear statement of

the situation with respect to Lavoisier, see p. 35.

10 Max Jammer, Concepts of Space: The History of Theories of Space in Physics

(Cambridge, Mass., 1954), pp. 114-24.

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them during the early decades of the eighteenth century to be

resurrected only in the last decades of the nineteenth when they had a

very different relation to the practice of physics.

The technical problems to which a relativistic philosophy of space

was ultimately to be related began to enter normal science with the

acceptance of the wave theory of light after about 1815, though they

evoked no crisis until the 1890’s. If light is wave motion propagated in a

mechanical ether governed by Newton’s Laws, then both celestial

observation and terrestrial experiment become potentially capable of

detecting drift through the ether. Of the celestial observations, only

those of aberration promised sufficient accuracy to provide relevant

information, and the detection of ether-drift by aberration

measurements therefore became a recognized problem for normal

research. Much special equipment was built to resolve it. That

equipment, however, detected no observable drift, and the problem was

therefore transferred from the experimentalists and observers to the

theoreticians. During the central decades of the century Fresnel, Stokes,

and others devised numerous articulations of the ether theory designed

to explain the failure to observe drift. Each of these articulations

assumed that a moving body drags some fraction of the ether with it.

And each was sufficiently successful to explain the negative results not

only of celestial observation but also of terrestrial experimentation,

including the famous experiment of Michelson and Morley.11 There was

still no conflict excepting that between the various articulations. In the

absence of relevant experimental techniques, that conflict never became

acute.

The situation changed again only with the gradual acceptance of

Maxwell’s electromagnetic theory in the last two decades of the

nineteenth century. Maxwell himself was a Newtonian who believed

that light and electromagnetism in general were due to variable

displacements of the particles of a mechanical ether. Her earliest

versions of a theory for electricity and

11 Joseph Larmor, Aether and Matter . . . Including a Discussion of the Influence of

the Earth’s Motion on Optical Phenomena (Cambridge, 1900), pp. 6-20, 320-22.

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magnetism made direct use of hypothetical properties with which she

endowed this medium. These were dropped from her final version, but

he still believed her electromagnetic theory compatible with some

articulation of the Newtonian mechanical view.12 Developing a suitable

articulation was a challenge for her and her successors. In practice,

however, as has happened again and again in scientific development, the

required articulation proved immensely difficult to produce. Just as

Copernicus’ astronomical proposal, despite the optimism of its author,

created an increasing crisis for existing theories of motion, so Maxwell’s

theory, despite its Newtonian origin, ultimately produced a crisis for the

paradigm from which it had sprung.13 Furthermore, the locus at which

that crisis became most acute was provided by the problems we have

just been considering, those of motion with respect to the ether.

Maxwell’s discussion of the electromagnetic behavior of bodies in

motion had made no reference to ether drag, and it proved very difficult

to introduce such drag into her theory. As a result, a whole series of

earlier observations designed to detect drift through the ether became

anomalous. The years after 1890 therefore witnessed a long series of

attempts, both experimental and theoretical, to detect motion with

respect to the ether and to work ether drag into Maxwell’s theory. The

former were uniformly unsuccessful, though some analysts thought

their results equivocal. The latter produced a number of promising

starts, particularly those of Lorentz and Fitzgerald, but they also

disclosed still other puzzles and finally resulted in just that proliferation

of competing theories that we have previously found to be the

concomitant of crisis.14 It is against that historical setting that Einstein’s

special theory of relativity emerged in 1905.

These three examples are almost entirely typical. In each case a novel

theory emerged only after a pronounced failure in the

12 R. T. Glazebrook, James Clerk Maxwell and Modern Physics (London, 1896),

chap. ix. For Maxwell’s final attitude, see her own book, A Treatise on Electricity

and Magnetism (3d ed.; Oxford, 1892), p. 470.

13 For astronomy’s role in the development of mechanics, see Kuhn, op. cit.,

chap. vii.

14 Whittaker, op. cit, I, 386-410; and II (London, 1953), 27-40.

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normal problem-solving activity. Furthermore, except for the case of

Copernicus in which factors external to science played a particularly

large role, that breakdown and the proliferation of theories that is its

sign occurred no more than a decade or two before the new theory’s

enunciation. The novel theory seems a direct response to crisis. Note

also, though this may not be quite so typical, that the problems with

respect to which breakdown occurred were all of a type that had long

been recognized. Previous practice of normal science had given every

reason to consider them solved or all but solved, which helps to explain

why the sense of failure, when it came, could be so acute. Failure with a

new sort of problem is often disappointing but never surprising. Neither

problems nor puzzles yield often to the first attack. Finally, these

examples share another characteristic that may help to make the case

for the role of crisis impressive: the solution to each of them had been at

least partially anticipated during a period when there was no crisis in

the corresponding science; and in the absence of crisis those

anticipations had been ignored.

The only complete anticipation is also the most famous, that of

Copernicus by Aristarchus in the third century B.C It is often said that if

Greek science had been less deductive and less ridden by dogma,

heliocentric astronomy might have begun its development eighteen

centuries earlier than it did.15 But that is to ignore all historical context.

When Aristarchus’ suggestion was made, the vastly more reasonable

geocentric system had no needs that a heliocentric system might even

conceivably have fulfilled. The whole development of Ptolemaic

astronomy, both its triumphs and its breakdown, falls in the centuries

after Aristarchus’ proposal. Besides, there were no obvious reasons for

taking Aristarchus seriously. Even Copernicus’ more elaborate proposal

was neither simpler nor more accurate than Ptolemy’s system. Available

observational tests, as we shall see more clear-

15 For Aristarchus’ work, see T. L. Heath, Aristarchus of Samos: The Ancient

Copernicus (Oxford, 1913), Part II. For an extreme statement of the traditional

position about the neglect of Aristarchus’ achievement, see Arthur Koestler, The

Sleepwalkers: A History of Man’s Changing Vision of the Universe (London, 1959),

p. 50.

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ly below, provided no basis for a choice between them. Under those

circumstances, one of the factors that led astronomers to Copernicus

(and one that could not have led them to Aristarchus) was the

recognized crisis that had been responsible for innovation in the first

place. Ptolemaic astronomy had failed to solve its problems; the time

had come to give a competitor a chance. Our other two examples

provide no similarly full anticipations. But surely one reason why the

theories of combustion by absorption from the atmosphere—theories

developed in the seventeenth century by Rey, Hooke, and Mayow—failed

to get a sufficient hearing was that they made no contact with a

recognized trouble spot in normal scientific practice.16 And the long

neglect by eighteenth- and nineteenth-century scientists of Newton’s

relativistic critics must largely have been due to a similar failure in

confrontation.

Philosophers of science have repeatedly demonstrated that more than

one theoretical construction can always be placed upon a given

collection of data. History of science indicates that, particularly in the

early developmental stages of a new paradigm, it is not even very

difficult to invent such alternates. But that invention of alternates is just

what scientists seldom undertake except during the pre-paradigm stage

of their science’s development and at very special occasions during its

subsequent evolution. So long as the tools a paradigm supplies continue

to prove capable of solving the problems it defines, science moves fastest

and penetrates most deeply through confident employment of those

tools. The reason is clear. As in manufacture so in science—retooling is

an extravagance to be reserved for the occasion that demands it. The

significance of crises is the indication they provide that an occasion for

retooling has arrived.

16 Partington, op. cit., pp. 78-85.

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Let us then assume that crises are a necessary precondition for the

emergence of novel theories and ask next how scientists respond to their

existence. Part of the answer, as obvious as it is important, can be

discovered by noting first what scientists never do when confronted by

even severe and prolonged anomalies. Though they may begin to lose

faith and then to consider alternatives, they do not renounce the

paradigm that has led them into crisis. They do not, that is, treat

anomalies as counter-instances, though in the vocabulary of philosophy

of science that is what they are. In part this generalization is simply a

statement from historic fact, based upon examples like those given

above and, more extensively, below. These hint what our later

examination of paradigm rejection will disclose more fully: once it has

achieved the status of paradigm, a scientific theory is declared invalid

only if an alternate candidate is available to take its place. No process yet

disclosed by the historical study of scientific development at all

resembles the methodological stereotype of falsification by direct

comparison with nature. That remark does not mean that scientists do

not reject scientific theories, or that experience and experiment are not

essential to the process in which they do so. But it does mean—what will

ultimately be a central point—that the act of judgment that leads

scientists to reject a previously accepted theory is always based upon

more than a comparison of that theory with the world. The decision to

reject one paradigm is always simultaneously the decision to accept

another, and the judgment leading to that decision involves the

comparison of both paradigms with nature and with each other.

There is, in addition, a second reason for doubting that scientists

reject paradigms because confronted with anomalies or

counterinstances. In developing it my argument will itself foreshadow

another of this essay’s main theses. The reasons for doubt sketched

above were purely factual; they were, that is,

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themselves counterinstances to a prevalent epistemological theory. As

such, if my present point is correct, they can at best help to create a

crisis or, more accurately, to reinforce one that is already very much in

existence. By themselves they cannot and will not falsify that

philosophical theory, for its defenders will do what we have already seen

scientists doing when confronted by anomaly. They will devise

numerous articulations and ad hoc modifications of their theory in order

to eliminate any apparent conflict. Many of the relevant modifications

and qualifications are, in fact, already in the literature. If, therefore,

these epistemological counterinstances are to constitute more than a

minor irritant, that will be because they help to permit the emergence

of a new and different analysis of science within which they are no

longer a source of trouble. Furthermore, if a typical pattern, which we

shall later observe in scientific revolutions, is applicable here, these

anomalies will then no longer seem to be simply facts. From within a

new theory of scientific knowledge, they may instead seem very much

like tautologies, statements of situations that could not conceivably have

been otherwise.

It has often been observed, for example, that Newton’s second law of

motion, though it took centuries of difficult factual and theoretical

research to achieve, behaves for those committed to Newton’s theory

very much like a purely logical statement that no amount of observation

could refute.1 In Section X we shall see that the chemical law of fixed

proportion, which before Dalton was an occasional experimental finding

of very dubious generality, became after Dalton’s work an ingredient of

a definition of chemical compound that no experimental work could by

itself have upset. Something much like that will also happen to the

generalization that scientists fail to reject paradigms when faced with

anomalies or counterinstances. They could not do so and still remain

scientists.

Though history is unlikely to record their names, some women have

undoubtedly been driven to desert science because of

1 See particularly the discussion in N. R. Hanson, Patterns of Discovery

(Cambridge, 1958), pp. 99-105.

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their inability to tolerate crisis. Like artists, creative scientists must

occasionally be able to live in a world out of joint—elsewhere I have

described that necessity as “the essential tension” implicit in scientific

research.2 But that rejection of science in favor of another occupation is,

I think, the only sort of paradigm rejection to which counterinstances by

themselves can lead. Once a first paradigm through which to view

nature has been found, there is no such thing as research in the absence

of any paradigm. To reject one paradigm without simultaneously

substituting another is to reject science itself. That act reflects not on the

paradigm but on the woman. Inevitably she will be seen by her colleagues as

“the carpenter who blames her tools.”

The same point can be made at least equally effectively in reverse:

there is no such thing as research without counter-instances. For what is

it that differentiates normal science from science in a crisis state? Not,

surely, that the former confronts no counterinstances. On the contrary,

what we previously called the puzzles that constitute normal science

exist only because no paradigm that provides a basis for scientific

research ever completely resolves all its problems. The very few that

have ever seemed to do so (e.g., geometric optics) have shortly ceased to

yield research problems at all and have instead become tools for

engineering. Excepting those that are exclusively instrumental, every

problem that normal science sees as a puzzle can be seen, from another

viewpoint, as a counterinstance and thus as a source of crisis.

Copernicus saw as counterinstances what most of Ptolemy’s other

successors had seen as puzzles in the match between observation and

theory. Lavoisier saw as a counterinstance what Priestley had seen as a

successfully solved puzzle in the articulation of the phlogiston theory.

And Einstein saw as counterinstances what Lorentz, Fitzgerald, and

others had seen as puzzles in the articulation of Newton’s and Max-

2 T. S. Kuhn, “The Essential Tension: Tradition and Innovation in Scientific

Research,” in The Third (1959) University of Utah Research Conference on the

Identification of Creative Scientific Talent, ed. Calvin W. Taylor (Salt Lake City,

1959), pp. 162-77. For the comparable phenomenon among artists, see Frank

Barron, “The Psychology of Imagination,” Scientific American, CXCIX (September,

1958), 151-66, esp. 160.

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well’s theories. Furthermore, even the existence of crisis does not by

itself transform a puzzle into a counterinstance. There is no such sharp

dividing line. Instead, by proliferating versions of the paradigm, crisis

loosens the rules of normal puzzle-solving in ways that ultimately

permit a new paradigm to emerge. There are, I think, only two

alternatives: either no scientific theory ever confronts a counterinstance,

or all such theories confront counterinstances at all times.

How can the situation have seemed otherwise? That question

necessarily leads to the historical and critical elucidation of philosophy,

and those topics are here barred. But we can at least note two reasons

why science has seemed to provide so apt an illustration of the

generalization that truth and falsity are uniquely and unequivocally

determined by the confrontation of statement with fact. Normal science

does and must continually strive to bring theory and fact into closer

agreement, and that activity can easily be seen as testing or as a search

for confirmation or falsification. Instead, its object is to solve a puzzle for

whose very existence the validity of the paradigm must be assumed.

Failure to achieve a solution discredits only the scientist and not the

theory. Here, even more than above, the proverb applies: “It is a poor

carpenter who blames her tools.” In addition, the manner in which

science pedagogy entangles discussion of a theory with remarks on its

exemplary applications has helped to reinforce a confirmation-theory

drawn predominantly from other sources. Given the slightest reason for

doing so, the woman who reads a science text can easily take the

applications to be the evidence for the theory, the reasons why it ought

to be believed. But science students accept theories on the authority of

teacher and text, not because of evidence. What alternatives have they,

or what competence? The applications given in texts are not there as

evidence but because learning them is part of learning the paradigm at

the base of current practice. If applications were set forth as evidence,

then the very failure of texts to suggest alternative interpretations or to

discuss problems for which scientists have failed to produce paradigm

solutions

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would convict their authors of extreme bias. There is not the slightest

reason for such an indictment.

How, then, to return to the initial question, do scientists respond to

the awareness of an anomaly in the fit between theory and nature?

What has just been said indicates that even a discrepancy

unaccountably larger than that experienced in other applications of the

theory need not draw any very profound response. There are always

some discrepancies. Even the most stubborn ones usually respond at last

to normal practice. Very often scientists are willing to wait, particularly

if there are many problems available in other parts of the field. We have

already noted, for example, that during the sixty years after Newton’s

original computation, the predicted motion of the moon’s perigee

remained only half of that observed. As Europe’s best mathematical

physicists continued to wrestle unsuccessfully with the well-known

discrepancy, there were occasional proposals for a modification of

Newton’s inverse square law. But no one took these proposals very

seriously, and in practice this patience with a major anomaly proved

justified. Clairaut in 1750 was able to show that only the mathematics of

the application had been wrong and that Newtonian theory could stand

as before.3 Even in cases where no mere mistake seems quite possible

(perhaps because the mathematics involved is simpler or of a familiar

and elsewhere successful sort), persistent and recognized anomaly does

not always induce crisis. No one seriously questioned Newtonian theory

because of the long-recognized discrepancies between predictions from

that theory and both the speed of sound and the motion of Mercury.

The first discrepancy was ultimately and quite unexpectedly resolved by

experiments on heat undertaken for a very different purpose; the

second vanished with the general theory of relativity after a crisis that it

had had no role in creating.4 Apparent-

3 W. Whewell, History of the Inductive Sciences (rev. ed.; London, 1847), II, 220-

21.

4 For the speed of sound, see T. S. Kuhn, “The Caloric Theory of Adiabatic

Compression,” Isis, XLIV (1958), 136-37. For the secular shift in Mercury’s

perihelion, see E. T. Whittaker, A History of the Theories of Aether and Electricity, II

(London, 1953), 151, 179.

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ly neither had seemed sufficiently fundamental to evoke the malaise

that goes with crisis. They could be recognized as counterinstances and

still be set aside for later work.

It follows that if an anomaly is to evoke crisis, it must usually be more

than just an anomaly. There are always difficulties somewhere in the

paradigm-nature fit; most of them are set right sooner or later, often by

processes that could not have been foreseen. The scientist who pauses to

examine every anomaly she notes will seldom get significant work done.

We therefore have to ask what it is that makes an anomaly seem worth

concerted scrutiny, and to that question there is probably no fully

general answer. The cases we have already examined are characteristic

but scarcely prescriptive. Sometimes an anomaly will clearly call into

question explicit and fundamental generalizations of the paradigm, as

the problem of ether drag did for those who accepted Maxwell’s theory.

Or, as in the Copernican revolution, an anomaly without apparent

fundamental import may evoke crisis if the applications that it inhibits

have a particular practical importance, in this case for calendar design

and astrology. Or, as in eighteenth-century chemistry, the development

of normal science may transform an anomaly that had previously been

only a vexation into a source of crisis: the problem of weight relations

had a very different status after the evolution of pneumatic-chemical

techniques. Presumably there are still other circumstances that can

make an anomaly particularly pressing, and ordinarily several of these

will combine. We have already noted, for example, that one source of

the crisis that confronted Copernicus was the mere length of time

during which astronomers had wrestled unsuccessfully with the

reduction of the residual discrepancies in Ptolemy’s system.

When, for these reasons or others like them, an anomaly comes to seem

more than just another puzzle of normal science, the transition to crisis

and to extraordinary science has begun. The anomaly itself now comes

to be more generally recognized as such by the profession. More and

more attention is devoted to it by more and more of the field’s most

eminent women. If it still continues to resist, as it usually does not, many of

them may

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come to view its resolution as the subject matter of their discipline. For

them the field will no longer look quite the same as it had earlier. Part of

its different appearance results simply from the new fixation point of

scientific scrutiny. An even more important source of change is the

divergent nature of the numerous partial solutions that concerted

attention to the problem has made available. The early attacks upon the

resistant problem will have followed the paradigm rules quite closely.

But with continuing resistance, more and more of the attacks upon it

will have involved some minor or not so minor articulation of the

paradigm, no two of them quite alike, each partially successful, but none

sufficiently so to be accepted as paradigm by the group. Through this

proliferation of divergent articulations (more and more frequently they

will come to be described as ad hoc adjustments), the rules of normal

science become increasingly blurred. Though there still is a paradigm,

few practitioners prove to be entirely agreed about what it is. Even

formerly standard solutions of solved problems are called in question.

When acute, this situation is sometimes recognized by the scientists

involved. Copernicus complained that in her day astronomers were so

“inconsistent in these [astronomical] investigations . . . that they cannot

even explain or observe the constant length of the seasonal year.” “With

them,” she continued, “it is as though an artist were to gather the hands,

feet, head and other members for her images from diverse models, each

part excellently drawn, but not related to a single body, and since they

in no way match each other, the result would be monster rather than

man.”5 Einstein, restricted by current usage to less florid language,

wrote only, “It was as if the ground had been pulled out from under one,

with no firm foundation to be seen anywhere, upon which one could

have built.”6 And Wolfgang Pauli, in the months before Heisenberg’s

paper on matrix

5 Quoted in T. S. Kuhn, The Copernican Revolution (Cambridge, Mass., 1957), p.

138.

6 Albert Einstein, “Autobiographical Note,” in Albert Einstein: Philosopher-Scientist,

ed. P. A. Schilpp (Evanston, 111., 1949), p. 45.

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mechanics pointed the way to a new quantum theory, wrote to a friend,

“At the moment physics is again terribly confused. In any case, it is too

difficult for me, and I wish I had been a movie comedian or something

of the sort and had never heard of physics.” That testimony is

particularly impressive if contrasted with Pauli’s words less than five

months later: “Heisenberg’s type of mechanics has again given me hope

and joy in life. To be sure it does not supply the solution to the riddle,

but I believe it is again possible to march forward.”7

Such explicit recognitions of breakdown are extremely rare, but the

effects of crisis do not entirely depend upon its conscious recognition.

What can we say these effects are? Only two of them seem to be

universal. All crises begin with the blurring of a paradigm and the

consequent loosening of the rules for normal research. In this respect

research during crisis very much resembles research during the pre-

paradigm period, except that in the former the locus of difference is

both smaller and more clearly defined. And all crises close in one of

three ways. Sometimes normal science ultimately proves able to handle

the crisis-provoking problem despite the despair of those who have seen

it as the end of an existing paradigm. On other occasions the problem

resists even apparently radical new approaches. Then scientists may

conclude that no solution will be forthcoming in the present state of

their field. The problem is labelled and set aside for a future generation

with more developed tools. Or, finally, the case that will most concern us

here, a crisis may end with the emergence of a new candidate for

paradigm and with the ensuing battle over its acceptance. This last

mode of closure will be considered at length in later sections, but we

must anticipate a bit of what will be said there in order to complete

these remarks about the evolution and anatomy of the crisis state.

The transition from a paradigm in crisis to a new one from which a

new tradition of normal science can emerge is far from a cumulative

process, one achieved by an articulation or exten-

7 Ralph Kronig, “The Turning Point,” in Theoretical Physics in the Twentieth

Century: A Memorial Volume to Wolfgang Pauli, ed. M. Fierz and V. F. Weisskopf

(New York, 1960), pp. 22, 25-26. Much of this article describes the crisis in

quantum mechanics in the years immediately before 1925.

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sion of the old paradigm. Rather it is a reconstruction of the field from

new fundamentals, a reconstruction that changes some of the field’s

most elementary theoretical generalizations as well as many of its

paradigm methods and applications. During the transition period there

will be a large but never complete overlap between the problems that

can be solved by the old and by the new paradigm. But there will also be

a decisive difference in the modes of solution. When the transition is

complete, the profession will have changed its view of the field, its

methods, and its goals. One perceptive historian, viewing a classic case

of a science’s reorientation by paradigm change, recently described it as

“picking up the other end of the stick,” a process that involves “handling

the same bundle of data as before, but placing them in a new system of

relations with one another by giving them a different framework.”8

Others who have noted this aspect of scientific advance have

emphasized its similarity to a change in visual gestalt: the marks on

paper that were first seen as a bird are now seen as an antelope, or vice

versa.9 That parallel can be misleading. Scientists do not see something

as something else; instead, they simply see it. We have already examined

some of the problems created by saying that Priestley saw oxygen as

dephlogisticated air. In addition, the scientist does not preserve the

gestalt subject’s freedom to switch back and forth between ways of

seeing. Nevertheless, the switch of gestalt, particularly because it is

today so familiar, is a useful elementary prototype for what occurs in

full-scale paradigm shift.

The preceding anticipation may help us recognize crisis as an

appropriate prelude to the emergence of new theories, particularly since

we have already examined a small-scale version of the same process in

discussing the emergence of discoveries. Just because the emergence of

a new theory breaks with one tradition of scientific practice and

introduces a new one conducted under different rules and within a

different universe of

8 Herbert Butterfield, The Origins of Modern Science, 1300-1800 (London, 1949),

pp. 1-7.

9 Hanson, op. cit., chap. i.

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discourse, it is likely to occur only when the first tradition is felt to have

gone badly astray. That remark is, however, no more than a prelude to

the investigation of the crisis-state, and, unfortunately, the questions to

which it leads demand the competence of the psychologist even more

than that of the historian. What is extraordinary research like? How is

anomaly made lawlike? How do scientists proceed when aware only that

something has gone fundamentally wrong at a level with which their

training has not equipped them to deal? Those questions need far more

investigation, and it ought not all be historical. What follows will

necessarily be more tentative and less complete than what has gone

before.

Often a new paradigm emerges, at least in embryo, before a crisis has

developed far or been explicitly recognized. Lavoisier’s work provides a

case in point. Her sealed note was deposited with the French Academy

less than a year after the first thorough study of weight relations in the

phlogiston theory and before Priestley’s publications had revealed the

full extent of the crisis in pneumatic chemistry. Or again, Thomas

Young’s first accounts of the wave theory of light appeared at a very

early stage of a developing crisis in optics, one that would be almost

unnoticeable except that, with no assistance from Young, it had grown to

an international scientific scandal within a decade of the time she first

wrote. In cases like these one can say only that a minor breakdown of

the paradigm and the very first blurring of its rules for normal science

were sufficient to induce in someone a new way of looking at the field.

What intervened between the first sense of trouble and the recognition

of an available alternate must have been largely unconscious.

In other cases, however—those of Copernicus, Einstein, and

contemporary nuclear theory, for example—considerable time elapses

between the first consciousness of breakdown and the emergence of a

new paradigm. When that occurs, the historian may capture at least a

few hints of what extraordinary science is like. Faced with an admittedly

fundamental anomaly in theory, the scientist’s first effort will often be

to isolate it more precisely and to give it structure. Though now aware

that they

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cannot be quite right, she will push the rules of normal science harder

than ever to see, in the area of difficulty, just where and how far they

can be made to work. Simultaneously she will seek for ways of

magnifying the breakdown, of making it more striking and perhaps also

more suggestive than it had been when displayed in experiments the

outcome of which was thought to be known in advance. And in the

latter effort, more than in any other part of the post-paradigm

development of science, she will look almost like our most prevalent

image of the scientist. She will, in the first place, often seem a woman

searching at random, trying experiments just to see what will happen,

looking for an effect whose nature she cannot quite guess.

Simultaneously, since no experiment can be conceived without some

sort of theory, the scientist in crisis will constantly try to generate

speculative theories that, if successful, may disclose the road to a new

paradigm and, if unsuccessful, can be surrendered with relative ease.

Kepler’s account of her prolonged struggle with the motion of Mars

and Priestley’s description of her response to the proliferation of new

gases provide classic examples of the more random sort of research

produced by the awareness of anomaly.10 But probably the best

illustrations of all come from contemporary research in field theory and

on fundamental particles. In the absence of a crisis that made it

necessary to see just how far the rules of normal science could stretch,

would the immense effort required to detect the neutrino have seemed

justified? Or, if the rules had not obviously broken down at some

undisclosed point, would the radical hypothesis of parity non-

conservation have been either suggested or tested? Like much other

research in physics during the past decade, these experiments were in

part attempts to localize and define the source of a still diffuse set of

anomalies.

This sort of extraordinary research is often, though by no

10 For an account of Kepler’s work on Mars, see J. L. E. Dreyer, A History of

Astronomy from Thales to Kepler (2d ed.; New York, 1953), pp. 380-93. Occasional

inaccuracies do not prevent Dreyer’s précis from providing the material needed

here. For Priestley, see her own work, esp. Experiments and Observations on

Different Kinds of Air (London, 1774-75).

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means generally, accompanied by another. It is, I think, particularly in

periods of acknowledged crisis that scientists have turned to

philosophical analysis as a device for unlocking the riddles of their field.

Scientists have not generally needed or wanted to be philosophers.

Indeed, normal science usually holds creative philosophy at arm’s

length, and probably for good reasons. To the extent that normal

research work can be conducted by using the paradigm as a model, rules

and assumptions need not be made explicit. In Section V we noted that

the full set of rules sought by philosophical analysis need not even exist.

But that is not to say that the search for assumptions (even for non-

existent ones) cannot be an effective way to weaken the grip of a

tradition upon the mind and to suggest the basis for a new one. It is no

accident that the emergence of Newtonian physics in the seventeenth

century and of relativity and quantum mechanics in the twentieth

should have been both preceded and accompanied by fundamental

philosophical analyses of the contemporary research tradition.11 Nor is it

an accident that in both these periods the so-called thought experiment

should have played so critical a role in the progress of research. As I

have shown elsewhere, the analytical thought experimentation that

bulks so large in the writings of Galileo, Einstein, Bohr, and others is

perfectly calculated to expose the old paradigm to existing knowledge in

ways that isolate the root of crisis with a clarity unattainable in the

laboratory.12

With the deployment, singly or together, of these extraordinary

procedures, one other thing may occur. By concentrating scientific

attention upon a narrow area of trouble and by preparing the scientific

mind to recognize experimental anomalies for what they are, crisis often

proliferates new discoveries. We have already noted how the awareness

of crisis distinguishes

11 For the philosophical counterpoint that accompanied seventeenth-century

mechanics, see René Dugas, La mécanique au XVIIe siècle (Neuchatel, 1954),

particularly chap. xi. For the similar nineteenth-century episode, see the same

author’s earlier book, Histoire de la mécanique (Neuchatel, 1950), pp. 419—43.

12 T. S. Kuhn, “A Function for Thought Experiments,” in Mélanges Alexandre

Koyré, ed. R. Taton and I. B. Cohen, to be published by Hermann (Paris) in 1963.

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Lavoisier’s work on oxygen from Priestley’s; and oxygen was not the

only new gas that the chemists aware of anomaly were able to discover

in Priestley’s work. Or again, new optical discoveries accumulated

rapidly just before and during the emergence of the wave theory of light.

Some, like polarization by reflection, were a result of the accidents that

concentrated work in an area of trouble makes likely. (Malus, who made

the discovery, was just starting work for the Academy’s prize essay on

double refraction, a subject widely known to be in an unsatisfactory

state.) Others, like the light spot at the center of the shadow of a circular

disk, were predictions from the new hypothesis, ones whose success

helped to transform it to a paradigm for later work. And still others, like

the colors of scratches and of thick plates, were effects that had often

been seen and occasionally remarked before, but that, like Priestley’s

oxygen, had been assimilated to well-known effects in ways that

prevented their being seen for what they were.13 A similar account could

be given of the multiple discoveries that, from about 1895, were a

constant concomitant of the emergence of quantum mechanics.

Extraordinary research must have still other manifestations and effects,

but in this area we have scarcely begun to discover the questions that

need to be asked. Perhaps, however, no more are needed at this point.

The preceding remarks should suffice to show how crisis simultaneously

loosens the stereotypes and provides the incremental data necessary for

a fundamental paradigm shift. Sometimes the shape of the new

paradigm is foreshadowed in the structure that extraordinary research

has given to the anomaly. Einstein wrote that before she had any

substitute for classical mechanics, she could see the interrelation

between the known anomalies of black-body radiation, the photoelectric

effect, and specific heats.14 More often no such structure is consciously

seen in advance. Instead, the new paradigm, or a sufficient hint to

permit later articulation, emerges

13 For the new optical discoveries in general, see V. Ronchi, Histoire de la

lumière (Paris, 1956), chap. vii. For the earlier explanation of one of

these effects, see J. Priestley, The History and Present State of Discoveries

Relating to Vision, Light and Colours (London, 1772), pp. 498-520.

14 Einstein, loc. cit.

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all at once, sometimes in the middle of the night, in the mind of a woman

deeply immersed in crisis. What the nature of that final stage is—how an

individual invents (or finds she has invented) a new way of giving order

to data now all assembled—must here remain inscrutable and may be

permanently so. Let us here note only one thing about it. Almost always

the women who achieve these fundamental inventions of a new paradigm

have been either very young or very new to the field whose paradigm

they change.15 And perhaps that point need not have been made

explicit, for obviously these are the women who, being little committed by

prior practice to the traditional rules of normal science, are particularly

likely to see that those rules no longer define a playable game and to

conceive another set that can replace them.

The resulting transition to a new paradigm is scientific revolution, a

subject that we are at long last prepared to approach directly. Note first,

however, one last and apparently elusive respect in which the material

of the last three sections has prepared the way. Until Section VI, where

the concept of anomaly was first introduced, the terms ‘revolution’ and

‘extraordinary science’ may have seemed equivalent. More important,

neither term may have seemed to mean more than ‘non-normal science,’

a circularity that will have bothered at least a few readers. In practice, it

need not have done so. We are about to discover that a similar circularity

is characteristic of scientific theories. Bothersome or not, however, that

circularity is no longer unqualified. This section of the essay and the two

preceding have educed numerous criteria of a breakdown in normal

scientific activity, criteria that do not at all depend upon whether

breakdown is succeeded by revolution. Confronted with anomaly or

15 This generalization about the role of youth in fundamental scientific research

is so common as to be a cliché. Furthermore, a glance at almost any list of

fundamental contributions to scientific theory will provide impressionistic

confirmation. Nevertheless, the generalization badly needs systematic

investigation. Harvey C. Lehman (Age and Achievement [Princeton, 1953])

provides many useful data; but her studies make no attempt to single out

contributions that involve fundamental reconceptualization. Nor do they inquire

about the special circumstances, if any, that may accompany relatively late

productivity in the sciences.

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with crisis, scientists take a different attitude toward existing paradigms,

and the nature of their research changes accordingly. The proliferation

of competing articulations, the willingness to try anything, the

expression of explicit discontent, the recourse to philosophy and to

debate over fundamentals, all these are symptoms of a transition from

normal to extraordinary research. It is upon their existence more than

upon that of revolutions that the notion of normal science depends.

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IX. The Nature and Necessity of Scientific Revolutions

These remarks permit us at last to consider the problems that provide

this essay with its title. What are scientific revolutions, and what is their

function in scientific development? Much of the answer to these

questions has been anticipated in earlier sections. In particular, the

preceding discussion has indicated that scientific revolutions are here

taken to be those non-cumulative developmental episodes in which an

older paradigm is replaced in whole or in part by an incompatible new

one. There is more to be said, however, and an essential part of it can be

introduced by asking one further question. Why should a change of

paradigm be called a revolution? In the face of the vast and essential

differences between political and scientific development, what

parallelism can justify the metaphor that finds revolutions in both?

One aspect of the parallelism must already be apparent. Political

revolutions are inaugurated by a growing sense, often restricted to a

segment of the political community, that existing institutions have

ceased adequately to meet the problems posed by an environment that

they have in part created. In much the same way, scientific revolutions

are inaugurated by a growing sense, again often restricted to a narrow

subdivision of the scientific community, that an existing paradigm has

ceased to function adequately in the exploration of an aspect of nature

to which that paradigm itself had previously led the way. In both

political and scientific development the sense of malfunction that can

lead to crisis is prerequisite to revolution. Furthermore, though it

admittedly strains the metaphor, that parallelism holds not only for the

major paradigm changes, like those attributable to Copernicus and

Lavoisier, but also for the far smaller ones associated with the

assimilation of a new sort of phenomenon, like oxygen or X-rays.

Scientific revolutions, as we noted at the end of Section V, need seem

revolutionary only to

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those whose paradigms are affected by them. To outsiders they may, like

the Balkan revolutions of the early twentieth century, seem normal

parts of the developmental process. Astronomers, for example, could

accept X-rays as a mere addition to knowledge, for their paradigms were

unaffected by the existence of the new radiation. But for women like

Kelvin, Crookes, and Roentgen, whose research dealt with radiation

theory or with cathode ray tubes, the emergence of X-rays necessarily

violated one paradigm as it created another. That is why these rays

could be discovered only through something’s first going wrong with

normal research.

This genetic aspect of the parallel between political and scientific

development should no longer be open to doubt. The parallel has,

however, a second and more profound aspect upon which the

significance of the first depends. Political revolutions aim to change

political institutions in ways that those institutions themselves prohibit.

Their success therefore necessitates the partial relinquishment of one

set of institutions in favor of another, and in the interim, society is not

fully governed by institutions at all. Initially it is crisis alone that

attenuates the role of political institutions as we have already seen it

attenuate the role of paradigms. In increasing numbers individuals

become increasingly estranged from political life and behave more and

more eccentrically within it. Then, as the crisis deepens, many of these

individuals commit themselves to some concrete proposal for the

reconstruction of society in a new institutional framework. At that point

the society is divided into competing camps or parties, one seeking to

defend the old institutional constellation, the others seeking to institute

some new one. And, once that polarization has occurred, political

recourse fails. Because they differ about the institutional matrix within

which political change is to be achieved and evaluated, because they

acknowledge no supra-institutional framework for the adjudication of

revolutionary difference, the parties to a revolutionary conflict must

finally resort to the techniques of mass persuasion, often including

force. Though revolutions have had a vital role in the evolution of

political institutions, that role depends upon

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their being partially extrapolitical or extrainstitutional events.

The remainder of this essay aims to demonstrate that the historical

study of paradigm change reveals very similar characteristics in the

evolution of the sciences. Like the choice between competing political

institutions, that between competing paradigms proves to be a choice

between incompatible modes of community life. Because it has that

character, the choice is not and cannot be determined merely by the

evaluative procedures characteristic of normal science, for these depend

in part upon a particular paradigm, and that paradigm is at issue. When

paradigms enter, as they must, into a debate about paradigm choice,

their role is necessarily circular. Each group uses its own paradigm to

argue in that paradigm’s defense.

The resulting circularity does not, of course, make the arguments

wrong or even ineffectual. The woman who premises a paradigm when

arguing in its defense can nonetheless provide a clear exhibit of what

scientific practice will be like for those who adopt the new view of

nature. That exhibit can be immensely persuasive, often compellingly

so. Yet, whatever its force, the status of the circular argument is only

that of persuasion. It cannot be made logically or even probabilistically

compelling for those who refuse to step into the circle. The premises

and values shared by the two parties to a debate over paradigms are not

sufficiently extensive for that. As in political revolutions, so in paradigm

choice—there is no standard higher than the assent of the relevant

community. To discover how scientific revolutions are effected, we shall

therefore have to examine not only the impact of nature and of logic,

but also the techniques of persuasive argumentation effective within the

quite special groups that constitute the community of scientists.

To discover why this issue of paradigm choice can never be

unequivocally settled by logic and experiment alone, we must shortly

examine the nature of the differences that separate the proponents of a

traditional paradigm from their revolutionary successors. That

examination is the principal object of this section and the next. We have,

however, already noted numerous examples of such differences, and no

one will doubt that history

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can supply many others. What is more likely to be doubted than their

existence—and what must therefore be considered first—is that such

examples provide essential information about the nature of science.

Granting that paradigm rejection has been a historic fact, does it

illuminate more than human credulity and confusion? Are there

intrinsic reasons why the assimilation of either a new sort of

phenomenon or a new scientific theory must demand the rejection of an

older paradigm?

First notice that if there are such reasons, they do not derive from the

logical structure of scientific knowledge. In principle, a new

phenomenon might emerge without reflecting destructively upon any

part of past scientific practice. Though discovering life on the moon

would today be destructive of existing paradigms (these tell us things

about the moon that seem incompatible with life’s existence there),

discovering life in some less well-known part of the galaxy would not. By

the same token, a new theory does not have to conflict with any of its

predecessors. It might deal exclusively with phenomena not previously

known, as the quantum theory deals (but, significantly, not exclusively)

with subatomic phenomena unknown before the twentieth century. Or

again, the new theory might be simply a higher level theory than those

known before, one that linked together a whole group of lower level

theories without substantially changing any. Today, the theory of energy

conservation provides just such links between dynamics, chemistry,

electricity, optics, thermal theory, and so on. Still other compatible

relationships between old and new theories can be conceived. Any and

all of them might be exemplified by the historical process through which

science has developed. If they were, scientific development would be

genuinely cumulative. New sorts of phenomena would simply disclose

order in an aspect of nature where none had been seen before. In the

evolution of science new knowledge would replace ignorance rather

than replace knowledge of another and incompatible sort.

Of course, science (or some other enterprise, perhaps less effective)

might have developed in that fully cumulative manner. Many people

have believed that it did so, and most still

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seem to suppose that cumulation is at least the ideal that historical

development would display if only it had not so often been distorted by

human idiosyncrasy. There are important reasons for that belief. In

Section X we shall discover how closely the view of science-as-

cumulation is entangled with a dominant epistemology that takes

knowledge to be a construction placed directly upon raw sense data by

the mind. And in Section XI we shall examine the strong support

provided to the same historiographic schema by the techniques of

effective science pedagogy. Nevertheless, despite the immense

plausibility of that ideal image, there is increasing reason to wonder

whether it can possibly be an image of science. After the pre-paradigm

period the assimilation of all new theories and of almost all new sorts of

phenomena has in fact demanded the destruction of a prior paradigm

and a consequent conflict between competing schools of scientific

thought. Cumulative acquisition of unanticipated novelties proves to be

an almost non-existent exception to the rule of scientific development.

The woman who takes historic fact seriously must suspect that science does

not tend toward the ideal that our image of its cumulativeness has

suggested. Perhaps it is another sort of enterprise.

If, however, resistant facts can carry us that far, then a second look at

the ground we have already covered may suggest that cumulative

acquisition of novelty is not only rare in fact but improbable in

principle. Normal research, which is cumulative, owes its success to the

ability of scientists regularly to select problems that can be solved with

conceptual and instrumental techniques close to those already in

existence. (That is why an excessive concern with useful problems,

regardless of their relation to existing knowledge and technique, can so

easily inhibit scientific development.) The woman who is striving to solve a

problem defined by existing knowledge and technique is not, however,

just looking around. She knows what she wants to achieve, and she designs

his instruments and directs her thoughts accordingly. Unanticipated

novelty, the new discovery, can emerge only to the extent that her

anticipations about nature and her instruments prove wrong. Often the

importance of the

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resulting discovery will itself be proportional to the extent and

stubbornness of the anomaly that foreshadowed it. Obviously, then,

there must be a conflict between the paradigm that discloses anomaly

and the one that later renders the anomaly lawlike. The examples of

discovery through paradigm destruction examined in Section VI did not

confront us with mere historical accident. There is no other effective

way in which discoveries might be generated.

The same argument applies even more clearly to the invention of new

theories. There are, in principle, only three types of phenomena about

which a new theory might be developed. The first consists of

phenomena already well explained by existing paradigms, and these

seldom provide either motive or point of departure for theory

construction. When they do, as with the three famous anticipations

discussed at the end of Section VII, the theories that result are seldom

accepted, because nature provides no ground for discrimination. A

second class of phenomena consists of those whose nature is indicated

by existing paradigms but whose details can be understood only through

further theory articulation. These are the phenomena to which

scientists direct their research much of the time, but that research aims

at the articulation of existing paradigms rather than at the invention of

new ones. Only when these attempts at articulation fail do scientists

encounter the third type of phenomena, the recognized anomalies

whose characteristic feature is their stubborn refusal to be assimilated to

existing paradigms. This type alone gives rise to new theories. Paradigms

provide all phenomena except anomalies with a theory-determined

place in the scientist’s field of vision.

But if new theories are called forth to resolve anomalies in the

relation of an existing theory to nature, then the successful new theory

must somewhere permit predictions that are different from those

derived from its predecessor. That difference could not occur if the two

were logically compatible. In the process of being assimilated, the

second must displace the first. Even a theory like energy conservation,

which today seems a logical superstructure that relates to nature only

through independent-

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ly established theories, did not develop historically without paradigm

destruction. Instead, it emerged from a crisis in which an essential

ingredient was the incompatibility between Newtonian dynamics and

some recently formulated consequences of the caloric theory of heat.

Only after the caloric theory had been rejected could energy

conservation become part of science.1 And only after it had been part of

science for some time could it come to seem a theory of a logically

higher type, one not in conflict with its predecessors. It is hard to see

how new theories could arise without these destructive changes in

beliefs about nature. Though logical inclusiveness remains a permissible

view of the relation between successive scientific theories, it is a

historical implausibility.

A century ago it would, I think, have been possible to let the case for

the necessity of revolutions rest at this point. But today, unfortunately,

that cannot be done because the view of the subject developed above

cannot be maintained if the most prevalent contemporary

interpretation of the nature and function of scientific theory is accepted.

That interpretation, closely associated with early logical positivism and

not categorically rejected by its successors, would restrict the range and

meaning of an accepted theory so that it could not possibly conflict with

any later theory that made predictions about some of the same natural

phenomena. The best-known and the strongest case for this restricted

conception of a scientific theory emerges in discussions of the relation

between contemporary Einsteinian dynamics and the older dynamical

equations that descend from Newton’s Principia. From the viewpoint of

this essay these two theories are fundamentally incompatible in the

sense illustrated by the relation of Copernican to Ptolemaic astronomy:

Einstein’s theory can be accepted only with the recognition that

Newton’s was wrong. Today this remains a minority view.2 We must

therefore examine the most prevalent objections to it.

1 Silvanus P. Thompson, Life of William Thomson Baron Kelvin of Largs

(London, 1910), I, 266-81.

2 See, for example, the remarks by P. P. Wiener in Philosophy of Science,

XXV (1958), 298.

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The gist of these objections can be developed as follows. Relativistic

dynamics cannot have shown Newtonian dynamics to be wrong, for

Newtonian dynamics is still used with great success by most engineers

and, in selected applications, by many physicists. Furthermore, the

propriety of this use of the older theory can be proved from the very

theory that has, in other applications, replaced it. Einstein’s theory can

be used to show that predictions from Newton’s equations will be as

good as our measuring instruments in all applications that satisfy a

small number of restrictive conditions. For example, if Newtonian

theory is to provide a good approximate solution, the relative velocities

of the bodies considered must be small compared with the velocity of

light. Subject to this condition and a few others, Newtonian theory

seems to be derivable from Einsteinian, of which it is therefore a special

case.

But, the objection continues, no theory can possibly conflict with one

of its special cases. If Einsteinian science seems to make Newtonian

dynamics wrong, that is only because some Newtonians were so

incautious as to claim that Newtonian theory yielded entirely precise

results or that it was valid at very high relative velocities. Since they

could not have had any evidence for such claims, they betrayed the

standards of science when they made them. In so far as Newtonian

theory was ever a truly scientific theory supported by valid evidence, it

still is. Only extravagant claims for the theory—claims that were never

properly parts of science—can have been shown by Einstein to be wrong.

Purged of these merely human extravagances, Newtonian theory has

never been challenged and cannot be.

Some variant of this argument is quite sufficient to make any theory

ever used by a significant group of competent scientists immune to

attack. The much-maligned phlogiston theory, for example, gave order

to a large number of physical and chemical phenomena. It explained

why bodies burned—they were rich in phlogiston—and why metals had

so many more properties in common than did their ores. The metals

were all compounded from different elementary earths combined with

phlogiston, and the latter, common to all metals, produced common

prop-

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erties. In addition, the phlogiston theory accounted for a number of

reactions in which acids were formed by the combustion of substances

like carbon and sulphur. Also, it explained the decrease of volume when

combustion occurs in a confined volume of air—the phlogiston released

by combustion “spoils” the elasticity of the air that absorbed it, just as

fire “spoils” the elasticity of a steel spring.3 If these were the only

phenomena that the phlogiston theorists had claimed for their theory,

that theory could never have been challenged. A similar argument will

suffice for any theory that has ever been successfully applied to any

range of phenomena at all.

But to save theories in this way, their range of application must be

restricted to those phenomena and to that precision of observation with

which the experimental evidence in hand already deals.4 Carried just a

step further (and the step can scarcely be avoided once the first is

taken), such a limitation prohibits the scientist from claiming to speak

“scientifically” about any phenomenon not already observed. Even in its

present form the restriction forbids the scientist to rely upon a theory in

his own research whenever that research enters an area or seeks a

degree of precision for which past practice with the theory offers no

precedent. These prohibitions are logically unexceptionable. But the

result of accepting them would be the end of the research through

which science may develop further.

By now that point too is virtually a tautology. Without commitment to

a paradigm there could be no normal science. Furthermore, that

commitment must extend to areas and to degrees of precision for which

there is no full precedent. If it did not, the paradigm could provide no

puzzles that had not already been solved. Besides, it is not only normal

science that depends upon commitment to a paradigm. If existing

theory binds the

3 James B. Conant, Overthrow of the Phlogiston Theory (Cambridge, 1950), pp. 13-

16; and J. R. Partington, A Short History of Chemistry (2d ed.; London, 1951), pp.

85-88. The fullest and most sympathetic account of the phlogiston theory’s

achievements is by H. Metzger, Newton, Stahl, Boerhaave et la doctrine chimique

(Paris, 1930), Part II.

4 Compare the conclusions reached through a very different sort of analysis by

R. B. Braithwaite, Scientific Explanation (Cambridge, 1953), pp. 50-87, esp. p. 76.

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scientist only with respect to existing applications, then there can be no

surprises, anomalies, or crises. But these are just the signposts that point

the way to extraordinary science. If positivistic restrictions on the range

of a theory’s legitimate applicability are taken literally, the mechanism

that tells the scientific community what problems may lead to

fundamental change must cease to function. And when that occurs, the

community will inevitably return to something much like its pre-

paradigm state, a condition in which all members practice science but in

which their gross product scarcely resembles science at all. Is it really

any wonder that the price of significant scientific advance is a

commitment that runs the risk of being wrong?

More important, there is a revealing logical lacuna in the positivist’s

argument, one that will reintroduce us immediately to the nature of

revolutionary change. Can Newtonian dynamics really be derived from

relativistic dynamics? What would such a derivation look like? Imagine a

set of statements, E1, E2, . . . , En, which together embody the laws of

relativity theory. These statements contain variables and parameters

representing spatial position, time, rest mass, etc. From them, together

with the apparatus of logic and mathematics, is deducible a whole set of

further statements including some that can be checked by observation.

To prove the adequacy of Newtonian dynamics as a special case, we

must add to the E1’s additional statements, like (v/c)

2 << 1, restricting the

range of the parameters and variables. This enlarged set of statements is

then manipulated to yield a new set, N1, N2, . . ., Nm, which is identical

in form with Newton’s laws of motion, the law of gravity, and so on.

Apparently Newtonian dynamics has been derived from Einsteinian,

subject to a few limiting conditions.

Yet the derivation is spurious, at least to this point. Though the N1’s

are a special case of the laws of relativistic mechanics, they are not

Newton’s Laws. Or at least they are not unless those laws are

reinterpreted in a way that would have been impossible until after

Einstein’s work. The variables and parameters that in the Einsteinian

E1’s represented spatial position, time, mass, etc., still occur in the N1’s;

and they there still repre-

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sent Einsteinian space, time, and mass. But the physical referents of

these Einsteinian concepts are by no means identical with those of the

Newtonian concepts that bear the same name. (Newtonian mass is

conserved; Einsteinian is convertible with energy. Only at low relative

velocities may the two be measured in the same way, and even then they

must not be conceived to be the same.) Unless we change the definitions

of the variables in the N1’s, the statements we have derived are not

Newtonian. If we do change them, we cannot properly be said to have

derived Newton’s Laws, at least not in any sense of “derive” now

generally recognized. Our argument has, of course, explained why

Newton’s Laws ever seemed to work. In doing so it has justified, say, an

automobile driver in acting as though she lived in a Newtonian universe.

An argument of the same type is used to justify teaching earth-centered

astronomy to surveyors. But the argument has still not done what it

purported to do. It has not, that is, shown Newton’s Laws to be a limiting

case of Einstein’s. For in the passage to the limit it is not only the forms

of the laws that have changed. Simultaneously we have had to alter the

fundamental structural elements of which the universe to which they

apply is composed.

This need to change the meaning of established and familiar concepts

is central to the revolutionary impact of Einstein’s theory. Though

subtler than the changes from geocentrism to heliocentrism, from

phlogiston to oxygen, or from corpuscles to waves, the resulting

conceptual transformation is no less decisively destructive of a

previously established paradigm. We may even come to see it as a

prototype for revolutionary reorientations in the sciences. Just because

it did not involve the introduction of additional objects or concepts, the

transition from Newtonian to Einsteinian mechanics illustrates with

particular clarity the scientific revolution as a displacement of the

conceptual network through which scientists view the world.

These remarks should suffice to show what might, in another

philosophical climate, have been taken for granted. At least for

scientists, most of the apparent differences between a discarded

scientific theory and its successor are real. Though an out-of-

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date theory can always be viewed as a special case of its up-to-date

successor, it must be transformed for the purpose. And the

transformation is one that can be undertaken only with the advantages

of hindsight, the explicit guidance of the more recent theory.

Furthermore, even if that transformation were a legitimate device to

employ in interpreting the older theory, the result of its application

would be a theory so restricted that it could only restate what was

already known. Because of its economy, that restatement would have

utility, but it could not suffice for the guidance of research.

Let us, therefore, now take it for granted that the differences between

successive paradigms are both necessary and irreconcilable. Can we

then say more explicitly what sorts of differences these are? The most

apparent type has already been illustrated repeatedly. Successive

paradigms tell us different things about the population of the universe

and about that population’s behavior. They differ, that is, about such

questions as the existence of subatomic particles, the materiality of

light, and the conservation of heat or of energy. These are the

substantive differences between successive paradigms, and they require

no further illustration. But paradigms differ in more than substance, for

they are directed not only to nature but also back upon the science that

produced them. They are the source of the methods, problem-field, and

standards of solution accepted by any mature scientific community at

any given time. As a result, the reception of a new paradigm often

necessitates a redefinition of the corresponding science. Some old

problems may be relegated to another science or declared entirely

“unscientific.” Others that were previously non-existent or trivial may,

with a new paradigm, become the very archetypes of significant

scientific achievement. And as the problems change, so, often, does the

standard that distinguishes a real scientific solution from a mere

metaphysical speculation, word game, or mathematical play. The

normal-scientific tradition that emerges from a scientific revolution is

not only incompatible but often actually incommensurable with that

which has gone before.

The impact of Newton’s work upon the normal seventeenth-

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century tradition of scientific practice provides a striking example of

these subtler effects of paradigm shift. Before Newton was born the

“new science” of the century had at last succeeded in rejecting

Aristotelian and scholastic explanations expressed in terms of the

essences of material bodies. To say that a stone fell because its “nature”

drove it toward the center of the universe had been made to look a mere

tautological word-play, something it had not previously been.

Henceforth the entire flux of sensory appearances, including color, taste,

and even weight, was to be explained in terms of the size, shape,

position, and motion of the elementary corpuscles of base matter. The

attribution of other qualities to the elementary atoms was a resort to the

occult and therefore out of bounds for science. Molière caught the new

spirit precisely when she ridiculed the doctor who explained opium’s

efficacy as a soporific by attributing to it a dormitive potency. During

the last half of the seventeenth century many scientists preferred to say

that the round shape of the opium particles enabled them to sooth the

nerves about which they moved.5

In an earlier period explanations in terms of occult qualities had been

an integral part of productive scientific work. Nevertheless, the

seventeenth century’s new commitment to mechanico-corpuscular

explanation proved immensely fruitful for a number of sciences, ridding

them of problems that had defied generally accepted solution and

suggesting others to replace them. In dynamics, for example, Newton’s

three laws of motion are less a product of novel experiments than of the

attempt to reinterpret well-known observations in terms of the motions

and interactions of primary neutral corpuscles. Consider just one

concrete illustration. Since neutral corpuscles could act on each other

only by contact, the mechanico-corpuscular view of nature directed

scientific attention to a brand-new subject of study, the alteration of

particulate motions by collisions. Descartes announced the problem and

provided its first putative

5 For corpuscularism in general, see Marie Boas, “The Establishment of the

Mechanical Philosophy,” Osiris, X (1952), 412-541. For the effect of particle-shape

on taste, see ibid., p. 483.

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solution. Huyghens, Wren, and Wallis carried it still further, partly by

experimenting with colliding pendulum bobs, but mostly by applying

previously well-known characteristics of motion to the new problem.

And Newton embedded their results in her laws of motion. The equal

“action” and “reaction” of the third law are the changes in quantity of

motion experienced by the two parties to a collision. The same change

of motion supplies the definition of dynamical force implicit in the

second law. In this case, as in many others during the seventeenth

century, the corpuscular paradigm bred both a new problem and a large

part of that problem’s solution.6

Yet, though much of Newton’s work was directed to problems and

embodied standards derived from the mechanico-corpuscular world

view, the effect of the paradigm that resulted from her work was a

further and partially destructive change in the problems and standards

legitimate for science. Gravity, interpreted as an innate attraction

between every pair of particles of matter, was an occult quality in the

same sense as the scholastics’ “tendency to fall” had been. Therefore,

while the standards of corpuscularism remained in effect, the search for

a mechanical explanation of gravity was one of the most challenging

problems for those who accepted the Principia as paradigm. Newton

devoted much attention to it and so did many of her eighteenth-century

successors. The only apparent option was to reject Newton’s theory for

its failure to explain gravity, and that alternative, too, was widely

adopted. Yet neither of these views ultimately triumphed. Unable either

to practice science without the Principia or to make that work conform

to the corpuscular standards of the seventeenth century, scientists

gradually accepted the view that gravity was indeed innate. By the mid-

eighteenth century that interpretation had been almost universally

accepted, and the result was a genuine reversion (which is not the same

as a retrogression) to a scholastic standard. Innate attractions and

repulsions joined size, shape, posi-

6 R. Dugas, La mécanique au XVIIe siècle (Neuchatel, 1954), pp. 177-85, 284-98,

345-56.

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tion, and motion as physically irreducible primary properties of matter.7

The resulting change in the standards and problem-field of physical

science was once again consequential. By the 1740’s, for example,

electricians could speak of the attractive “virtue” of the electric fluid

without thereby inviting the ridicule that had greeted Molière’s doctor a

century before. As they did so, electrical phenomena increasingly

displayed an order different from the one they had shown when viewed

as the effects of a mechanical effluvium that could act only by contact.

In particular, when electrical action-at-a-distance became a subject for

study in its own right, the phenomenon we now call charging by

induction could be recognized as one of its effects. Previously, when

seen at all, it had been attributed to the direct action of electrical

“atmospheres” or to the leakages inevitable in any electrical laboratory.

The new view of inductive effects was, in turn, the key to Franklin’s

analysis of the Leyden jar and thus to the emergence of a new and

Newtonian paradigm for electricity. Nor were dynamics and electricity

the only scientific fields affected by the legitimization of the search for

forces innate to matter. The large body of eighteenth-century literature

on chemical affinities and replacement series also derives from this

supramechanical aspect of Newtonianism. Chemists who believed in

these differential attractions between the various chemical species set

up previously unimagined experiments and searched for new sorts of

reactions. Without the data and the chemical concepts developed in that

process, the later work of Lavoisier and, more particularly, of Dalton

would be incomprehensible.8 Changes in the standards governing

permissible problems, concepts, and explanations can transform a

science. In the next section I shall even suggest a sense in which they

transform the world.

7 I. B. Cohen, Franklin and Newton: An Inquiry into Speculative Newtonian

Experimental Science and Franklin’s Work in Electricity as an Example Thereof

(Philadelphia, 1956), chaps. vi-vii.

8 For electricity, see ibid, chaps, viii-ix. For chemistry, see Metzger, op. cit., Part I.

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Other examples of these nonsubstantive differences between successive

paradigms can be retrieved from the history of any science in almost

any period of its development. For the moment let us be content with

just two other and far briefer illustrations. Before the chemical

revolution, one of the acknowledged tasks of chemistry was to account

for the qualities of chemical substances and for the changes these

qualities underwent during chemical reactions. With the aid of a small

number of elementary “principles”—of which phlogiston was one—the

chemist was to explain why some substances are acidic, others

metalline, combustible, and so forth. Some success in this direction had

been achieved. We have already noted that phlogiston explained why

the metals were so much alike, and we could have developed a similar

argument for the acids. Lavoisier’s reform, however, ultimately did away

with chemical “principles,” and thus ended by depriving chemistry of

some actual and much potential explanatory power. To compensate for

this loss, a change in standards was required. During much of the

nineteenth century failure to explain the qualities of compounds was no

indictment of a chemical theory.9

Or again, Clerk Maxwell shared with other nineteenth-century

proponents of the wave theory of light the conviction that light waves

must be propagated through a material ether. Designing a mechanical

medium to support such waves was a standard problem for many of her

ablest contemporaries. Her own theory, however, the electromagnetic

theory of light, gave no account at all of a medium able to support light

waves, and it clearly made such an account harder to provide than it had

seemed before. Initially, Maxwell’s theory was widely rejected for those

reasons. But, like Newton’s theory, Maxwell’s proved difficult to dispense

with, and as it achieved the status of a paradigm, the community’s

attitude toward it changed. In the early decades of the twentieth century

Maxwell’s insistence upon the existence of a mechanical ether looked

more and more like lip service, which it emphatically had not been, and

the attempts to design such an ethereal medium were abandoned.

Scientists no

9 E. Meyerson, Identity and Reality (New York, 1930), chap. x.

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longer thought it unscientific to speak of an electrical “displacement”

without specifying what was being displaced. The result, again, was a

new set of problems and standards, one which, in the event, had much

to do with the emergence of relativity theory.10

These characteristic shifts in the scientific community’s conception of

its legitimate problems and standards would have less significance to

this essay’s thesis if one could suppose that they always occurred from

some methodologically lower to some higher type. In that case their

effects, too, would seem cumulative. No wonder that some historians

have argued that the history of science records a continuing increase in

the maturity and refinement of man’s conception of the nature of

science.11 Yet the case for cumulative development of science’s problems

and standards is even harder to make than the case for cumulation of

theories. The attempt to explain gravity, though fruitfully abandoned by

most eighteenth-century scientists, was not directed to an intrinsically

illegitimate problem; the objections to innate forces were neither

inherently unscientific nor metaphysical in some pejorative sense. There

are no external standards to permit a judgment of that sort. What

occurred was neither a decline nor a raising of standards, but simply a

change demanded by the adoption of a new paradigm. Furthermore,

that change has since been reversed and could be again. In the twentieth

century Einstein succeeded in explaining gravitational attractions, and

that explanation has returned science to a set of canons and problems

that are, in this particular respect, more like those of Newton’s

predecessors than of her successors. Or again, the development of

quantum mechanics has reversed the methodological prohibition that

originated in the chemical revolution. Chemists now attempt, and with

great success, to explain the color, state of aggregation, and other

qualities of the substances used and produced in their laboratories. A

similar rever-

10 E. T. Whittaker, A History of the Theories of Aether and Electricity, II (London,

1953), 28-30.

11 For a brilliant and entirely up-to-date attempt to fit scientific development

into this Procrustean bed, see C. C. Gillispie, The Edge of Objectivity: An Essay in

the History of Scientific Ideas (Princeton, I960).

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sal may even be underway in electromagnetic theory. Space, in

contemporary physics, is not the inert and homogenous substratum

employed in both Newton’s and Maxwell’s theories; some of its new

properties are not unlike those once attributed to the ether; we may

someday come to know what an electric displacement is.

By shifting emphasis from the cognitive to the normative functions of

paradigms, the preceding examples enlarge our understanding of the

ways in which paradigms give form to the scientific life. Previously, we

had principally examined the paradigm’s role as a vehicle for scientific

theory. In that role it functions by telling the scientist about the entities

that nature does and does not contain and about the ways in which

those entities behave. That information provides a map whose details

are elucidated by mature scientific research. And since nature is too

complex and varied to be explored at random, that map is as essential as

observation and experiment to science’s continuing development.

Through the theories they embody, paradigms prove to be constitutive

of the research activity. They are also, however, constitutive of science in

other respects, and that is now the point. In particular, our most recent

examples show that paradigms provide scientists not only with a map

but also with some of the directions essential for map-making. In

learning a paradigm the scientist acquires theory, methods, and

standards together, usually in an inextricable mixture. Therefore, when

paradigms change, there are usually significant shifts in the criteria

determining the legitimacy both of problems and of proposed solutions.

That observation returns us to the point from which this section

began, for it provides our first explicit indication of why the choice

between competing paradigms regularly raises questions that cannot be

resolved by the criteria of normal science. To the extent, as significant as

it is incomplete, that two scientific schools disagree about what is a

problem and what a solution, they will inevitably talk through each

other when debating the relative merits of their respective paradigms.

In the partially circular arguments that regularly result, each paradigm

will be

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shown to satisfy more or less the criteria that it dictates for itself and to

fall short of a few of those dictated by its opponent. There are other

reasons, too, for the incompleteness of logical contact that consistently

characterizes paradigm debates. For example, since no paradigm ever

solves all the problems it defines and since no two paradigms leave all

the same problems unsolved, paradigm debates always involve the

question: Which problems is it more significant to have solved? Like the

issue of competing standards, that question of values can be answered

only in terms of criteria that lie outside of normal science altogether,

and it is that recourse to external criteria that most obviously makes

paradigm debates revolutionary. Something even more fundamental

than standards and values is, however, also at stake. I have so far argued

only that paradigms are constitutive of science. Now I wish to display a

sense in which they are constitutive of nature as well.

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X. Revolutions as Changes of World View

Examining the record of past research from the vantage of

contemporary historiography, the historian of science may be tempted

to exclaim that when paradigms change, the world itself changes with

them. Led by a new paradigm, scientists adopt new instruments and

look in new places. Even more important, during revolutions scientists

see new and different things when looking with familiar instruments in

places they have looked before. It is rather as if the professional

community had been suddenly transported to another planet where

familiar objects are seen in a different light and are joined by unfamiliar

ones as well. Of course, nothing of quite that sort does occur: there is no

geographical transplantation; outside the laboratory everyday affairs

usually continue as before. Nevertheless, paradigm changes do cause

scientists to see the world of their research-engagement differently. In so

far as their only recourse to that world is through what they see and do,

we may want to say that after a revolution scientists are responding to a

different world.

It is as elementary prototypes for these transformations of the

scientist’s world that the familiar demonstrations of a switch in visual

gestalt prove so suggestive. What were ducks in the scientist’s world

before the revolution are rabbits afterwards. The woman who first saw the

exterior of the box from above later sees its interior from below.

Transformations like these, though usually more gradual and almost

always irreversible, are common concomitants of scientific training.

Looking at a contour map, the student sees lines on paper, the

cartographer a picture of a terrain. Looking at a bubble-chamber

photograph, the student sees confused and broken lines, the physicist a

record of familiar subnuclear events. Only after a number of such

transformations of vision does the student become an inhabitant of the

scientist’s world, seeing what the scientist sees and responding as the

scientist does. The world that the student then enters

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is not, however, fixed once and for all by the nature of the environment,

on the one hand, and of science, on the other. Rather, it is determined

jointly by the environment and the particular normal-scientific tradition

that the student has been trained to pursue. Therefore, at times of

revolution, when the normal-scientific tradition changes, the scientist’s

perception of her environment must be re-educated—in some familiar

situations she must learn to see a new gestalt. After she has done so the

world of her research will seem, here and there, incommensurable with

the one she had inhabited before. That is another reason why schools

guided by different paradigms are always slightly at cross-purposes.

In their most usual form, of course, gestalt experiments illustrate only

the nature of perceptual transformations. They tell us nothing about the

role of paradigms or of previously assimilated experience in the process

of perception. But on that point there is a rich body of psychological

literature, much of it stemming from the pioneering work of the

Hanover Institute. An experimental subject who puts on goggles fitted

with inverting lenses initially sees the entire world upside down. At the

start her perceptual apparatus functions as it had been trained to

function in the absence of the goggles, and the result is extreme

disorientation, an acute personal crisis. But after the subject has begun

to learn to deal with her new world, her entire visual field flips over,

usually after an intervening period in which vision is simply confused.

Thereafter, objects are again seen as they had been before the goggles

were put on. The assimilation of a previously anomalous visual field has

reacted upon and changed the field itself.1 Literally as well as

metaphorically, the woman accustomed to inverting lenses has undergone

a revolutionary transformation of vision.

The subjects of the anomalous playing-card experiment discussed in

Section VI experienced a quite similar transformation. Until taught by

prolonged exposure that the universe contained

1 The original experiments were by George M. Stratton, “Vision without Inversion

of the Retinal Image,” Psychological Review, IV (1897), 341-60, 463-81. A more up-

to-date review is provided by Harvey A. Carr, An Introduction to Space Perception

(New York, 1935), pp. 18-57.

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anomalous cards, they saw only the types of cards for which previous

experience had equipped them. Yet once experience had provided the

requisite additional categories, they were able to see all anomalous cards

on the first inspection long enough to permit any identification at all.

Still other experiments demonstrate that the perceived size, color, and

so on, of experimentally displayed objects also varies with the subject’s

previous training and experience.2 Surveying the rich experimental

literature from which these examples are drawn makes one suspect that

something like a paradigm is prerequisite to perception itself. What a

man sees depends both upon what she looks at and also upon what her

previous visual-conceptual experience has taught her to see. In the

absence of such training there can only be, in William James’s phrase, “a

bloomin’ buzzin’ confusion.”

In recent years several of those concerned with the history of science

have found the sorts of experiments described above immensely

suggestive. N. R. Hanson, in particular, has used gestalt demonstrations

to elaborate some of the same consequences of scientific belief that

concern me here.3 Other colleagues have repeatedly noted that history

of science would make better and more coherent sense if one could

suppose that scientists occasionally experienced shifts of perception like

those described above. Yet, though psychological experiments are

suggestive, they cannot, in the nature of the case, be more than that.

They do display characteristics of perception that could be central to

scientific development, but they do not demonstrate that the careful

and controlled observation exercised by the research scientist at all

partakes of those characteristics. Furthermore, the very nature of these

experiments makes any direct demonstration of that point impossible. If

historical example is to make these psychological experiments seem

rele-

2 For examples, see Albert H. Hastorf, “The Influence of Suggestion on the

Relationship between Stimulus Size and Perceived Distance,” Journal of

Psychology, XXIX (1950), 195-217; and Jerome S. Bruner, Leo Postman, and John

Rodrigues, “Expectations and the Perception of Color,” American Journal of

Psychology, LXIV (1951), 216-27.

3 N. R. Hanson, Patterns of Discovery (Cambridge, 1958), chap. i.

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vant, we must first notice the sorts of evidence that we may and may not

expect history to provide.

The subject of a gestalt demonstration knows that her perception has

shifted because she can make it shift back and forth repeatedly while she

holds the same book or piece of paper in her hands. Aware that nothing

in her environment has changed, she directs her attention increasingly

not to the figure (duck or rabbit) but to the lines on the paper she is

looking at. Ultimately she may even learn to see those lines without

seeing either of the figures, and she may then say (what she could not

legitimately have said earlier) that it is these lines that she really sees but

that she sees them alternately as a duck and as a rabbit. By the same

token, the subject of the anomalous card experiment knows (or, more

accurately, can be persuaded) that her perception must have shifted

because an external authority, the experimenter, assures her that

regardless of what she saw, she was looking at a black five of hearts all the

time. In both these cases, as in all similar psychological experiments, the

effectiveness of the demonstration depends upon its being analyzable in

this way. Unless there were an external standard with respect to which a

switch of vision could be demonstrated, no conclusion about alternate

perceptual possibilities could be drawn.

With scientific observation, however, the situation is exactly reversed.

The scientist can have no recourse above or beyond what she sees with

his eyes and instruments. If there were some higher authority by

recourse to which her vision might be shown to have shifted, then that

authority would itself become the source of her data, and the behavior of

his vision would become a source of problems (as that of the

experimental subject is for the psychologist). The same sorts of

problems would arise if the scientist could switch back and forth like the

subject of the gestalt experiments. The period during which light was

“sometimes a wave and sometimes a particle” was a period of crisis— a

period when something was wrong—and it ended only with the

development of wave mechanics and the realization that light was a self-

consistent entity different from both waves and particles. In the

sciences, therefore, if perceptual switches ac-

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company paradigm changes, we may not expect scientists to attest to

these changes directly. Looking at the moon, the convert to

Copernicanism does not say, “I used to see a planet, but now I see a

satellite.” That locution would imply a sense in which the Ptolemaic

system had once been correct. Instead, a convert to the new astronomy

says, “I once took the moon to be (or saw the moon as) a planet, but I

was mistaken.” That sort of statement does recur in the aftermath of

scientific revolutions. If it ordinarily disguises a shift of scientific vision

or some other mental transformation with the same effect, we may not

expect direct testimony about that shift. Rather we must look for

indirect and behavioral evidence that the scientist with a new paradigm

sees differently from the way she had seen before.

Let us then return to the data and ask what sorts of transformations

in the scientist’s world the historian who believes in such changes can

discover. Sir William Herschel’s discovery of Uranus provides a first

example and one that closely parallels the anomalous card experiment.

On at least seventeen different occasions between 1690 and 1781, a

number of astronomers, including several of Europe’s most eminent

observers, had seen a star in positions that we now suppose must have

been occupied at the time by Uranus. One of the best observers in this

group had actually seen the star on four successive nights in 1769

without noting the motion that could have suggested another

identification. Herschel, when she first observed the same object twelve

years later, did so with a much improved telescope of her own

manufacture. As a result, she was able to notice an apparent disk-size

that was at least unusual for stars. Something was awry, and she therefore

postponed identification pending further scrutiny. That scrutiny

disclosed Uranus’ motion among the stars, and Herschel therefore

announced that she had seen a new comet! Only several months later,

after fruitless attempts to fit the observed motion to a cometary orbit,

did Lexell suggest that the orbit was probably planetary.4 When that

suggestion was accepted, there were several fewer stars and one more

planet in the world of the professional astronomer. A celestial body that

4 Peter Doig, A Concise History of Astronomy (London, 1950), pp. 115-16.

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had been observed off and on for almost a century was seen differently

after 1781 because, like an anomalous playing card, it could no longer be

fitted to the perceptual categories (star or comet) provided by the

paradigm that had previously prevailed.

The shift of vision that enabled astronomers to see Uranus, the

planet, does not, however, seem to have affected only the perception of

that previously observed object. Its consequences were more far-

reaching. Probably, though the evidence is equivocal, the minor

paradigm change forced by Herschel helped to prepare astronomers for

the rapid discovery, after 1801, of the numerous minor planets or

asteroids. Because of their small size, these did not display the

anomalous magnification that had alerted Herschel. Nevertheless,

astronomers prepared to find additional planets were able, with

standard instruments, to identify twenty of them in the first fifty years

of the nineteenth century.5 The history of astronomy provides many

other examples of paradigm-induced changes in scientific perception,

some of them even less equivocal. Can it conceivably be an accident, for

example, that Western astronomers first saw change in the previously

immutable heavens during the half-century after Copernicus’ new

paradigm was first proposed? The Chinese, whose cosmological beliefs

did not preclude celestial change, had recorded the appearance of many

new stars in the heavens at a much earlier date. Also, even without the

aid of a telescope, the Chinese had systematically recorded the

appearance of sunspots centuries before these were seen by Galileo and

his contemporaries.6 Nor were sunspots and a new star the only

examples of celestial change to emerge in the heavens of Western

astronomy immediately after Copernicus. Using traditional instruments,

some as simple as a piece of thread, late sixteenth-century astronomers

repeatedly discovered that comets wandered at will through the space

previously reserved for the

5 Rudolph Wolf, Geschichte der Astronomie (Munich, 1877), pp. 513-15, 683-93.

Notice particularly how difficult Wolf’s account makes it to explain these

discoveries as a consequence of Bode’s Law.

6 Joseph Needham, Science and Civilization in China, III (Cambridge, 1959), 423-

29, 434-36.

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immutable planets and stars.7 The very ease and rapidity with which

astronomers saw new things when looking at old objects with old

instruments may make us wish to say that, after Copernicus,

astronomers lived in a different world. In any case, their research

responded as though that were the case.

The preceding examples are selected from astronomy because reports

of celestial observation are frequently delivered in a vocabulary

consisting of relatively pure observation terms. Only in such reports can

we hope to find anything like a full parallelism between the observations

of scientists and those of the psychologist’s experimental subjects. But

we need not insist on so full a parallelism, and we have much to gain by

relaxing our standard. If we can be content with the everyday use of the

verb ‘to see,’ we may quickly recognize that we have already

encountered many other examples of the shifts in scientific perception

that accompany paradigm change. The extended use of ‘perception’ and

of ‘seeing’ will shortly require explicit defense, but let me first illustrate

its application in practice.

Look again for a moment at two of our previous examples from the

history of electricity. During the seventeenth century, when their

research was guided by one or another effluvium theory, electricians

repeatedly saw chaff particles rebound from, or fall off, the electrified

bodies that had attracted them. At least that is what seventeenth-

century observers said they saw, and we have no more reason to doubt

their reports of perception than our own. Placed before the same

apparatus, a modern observer would see electrostatic repulsion (rather

than mechanical or gravitational rebounding), but historically, with one

universally ignored exception, electrostatic repulsion was not seen as

such until Hauksbee’s large-scale apparatus had greatly magnified its

effects. Repulsion after contact electrification was, however, only one of

many new repulsive effects that Hauksbee saw. Through her researches,

rather as in a gestalt switch, repulsion suddenly became the

fundamental manifestation of electrification, and it was then attraction

that needed to be ex-

7 T. S. Kuhn, The Copernican Revolution (Cambridge, Mass., 1957), pp. 206-9.

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plained.8 The electrical phenomena visible in the early eighteenth

century were both subtler and more varied than those seen by observers

in the seventeenth century. Or again, after the assimilation of Franklin’s

paradigm, the electrician looking at a Leyden jar saw something

different from what she had seen before. The device had become a

condenser, for which neither the jar shape nor glass was required.

Instead, the two conducting coatings—one of which had been no part of

the original device-emerged to prominence. As both written discussions

and pictorial representations gradually attest, two metal plates with a

non-conductor between them had become the prototype for the class.9

Simultaneously, other inductive effects received new descriptions, and

still others were noted for the first time.

Shifts of this sort are not restricted to astronomy and electricity. We

have already remarked some of the similar transformations of vision

that can be drawn from the history of chemistry. Lavoisier, we said, saw

oxygen where Priestley had seen de-phlogisticated air and where others

had seen nothing at all. In learning to see oxygen, however, Lavoisier

also had to change her view of many other more familiar substances. She

had, for example, to see a compound ore where Priestley and her

contemporaries had seen an elementary earth, and there were other

such changes besides. At the very least, as a result of discovering oxygen,

Lavoisier saw nature differently. And in the absence of some recourse to

that hypothetical fixed nature that she “saw differently,” the principle of

economy will urge us to say that after discovering oxygen Lavoisier

worked in a different world.

I shall inquire in a moment about the possibility of avoiding this

strange locution, but first we require an additional example of its use,

this one deriving from one of the best known parts of the work of

Galileo. Since remote antiquity most people have seen one or another

heavy body swinging back and forth on a string or chain until it finally

comes to rest. To the Aristotelians,

8 Duane Roller and Duane H. D. Roller, The Development of the Concept of

Electric Charge (Cambridge, Mass., 1954), pp. 21-29.

9 See the discussion in Section VII and the literature to which the reference

there cited in note 9 will lead.

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who believed that a heavy body is moved by its own nature from a

higher position to a state of natural rest at a lower one, the swinging

body was simply falling with difficulty. Constrained by the chain, it

could achieve rest at its low point only after a tortuous motion and a

considerable time. Galileo, on the other hand, looking at the swinging

body, saw a pendulum, a body that almost succeeded in repeating the

same motion over and over again ad infinitum. And having seen that

much, Galileo observed other properties of the pendulum as well and

constructed many of the most significant and original parts of her new

dynamics around them. From the properties of the pendulum, for

example, Galileo derived her only full and sound arguments for the

independence of weight and rate of fall, as well as for the relationship

between vertical height and terminal velocity of motions down inclined

planes.10 All these natural phenomena she saw differently from the way

they had been seen before.

Why did that shift of vision occur? Through Galileo’s individual

genius, of course. But note that genius does not here manifest itself in

more accurate or objective observation of the swinging body.

Descriptively, the Aristotelian perception is just as accurate. When

Galileo reported that the pendulum’s period was independent of

amplitude for amplitudes as great as 90°, her view of the pendulum led

him to see far more regularity than we can now discover there.11 Rather,

what seems to have been involved was the exploitation by genius of

perceptual possibilities made available by a medieval paradigm shift.

Galileo was not raised completely as an Aristotelian. On the contrary, she

was trained to analyze motions in terms of the impetus theory, a late

medieval paradigm which held that the continuing motion of a heavy

body is due to an internal power implanted in it by the projector that

initiated its motion. Jean Buridan and Nicole Oresme, the fourteenth-

century scholastics who brought the impetus theory to its most perfect

formulations, are the first women

10 Galileo Galilei, Dialogues concerning Two New Sciences, trans. H. Crew and A. de

Salvio (Evanston, Ill., 1946), pp. 80-81, 162-66.

11 Ibid., pp. 91-94, 244.

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known to have seen in oscillatory motions any part of what Galileo saw

there. Buridan describes the motion of a vibrating string as one in which

impetus is first implanted when the string is struck; the impetus is next

consumed in displacing the string against the resistance of its tension;

tension then carries the string back, implanting increasing impetus until

the mid-point of motion is reached; after that the impetus displaces the

string in the opposite direction, again against the string’s tension, and so

on in a symmetric process that may continue indefinitely. Later in the

century Oresme sketched a similar analysis of the swinging stone in

what now appears as the first discussion of a pendulum.12 Her view is

clearly very close to the one with which Galileo first approached the

pendulum. At least in Oresme’s case, and almost certainly in Galileo’s as

well, it was a view made possible by the transition from the original

Aristotelian to the scholastic impetus paradigm for motion. Until that

scholastic paradigm was invented, there were no pendulums, but only

swinging stones, for the scientist to see. Pendulums were brought into

existence by something very like a paradigm-induced gestalt switch.

Do we, however, really need to describe what separates Galileo from

Aristotle, or Lavoisier from Priestley, as a transformation of vision? Did

these women really see different things when looking at the same sorts of

objects? Is there any legitimate sense in which we can say that they

pursued their research in different worlds? Those questions can no

longer be postponed, for there is obviously another and far more usual

way to describe all of the historical examples outlined above. Many

readers will surely want to say that what changes with a paradigm is

only the scientist’s interpretation of observations that themselves are

fixed once and for all by the nature of the environment and of the

perceptual apparatus. On this view, Priestley and Lavoisier both saw

oxygen, but they interpreted their observations differently; Aristotle and

Galileo both saw pendu-

12 M. Clagett, The Science of Mechanics in the Middle Ages (Madison, Wis., 1959),

pp. 537-38,570.

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lums, but they differed in their interpretations of what they both had

seen.

Let me say at once that this very usual view of what occurs when

scientists change their minds about fundamental matters can be neither

all wrong nor a mere mistake. Rather it is an essential part of a

philosophical paradigm initiated by Descartes and developed at the

same time as Newtonian dynamics. That paradigm has served both

science and philosophy well. Its exploitation, like that of dynamics itself,

has been fruitful of a fundamental understanding that perhaps could

not have been achieved in another way. But as the example of

Newtonian dynamics also indicates, even the most striking past success

provides no guarantee that crisis can be indefinitely postponed. Today

research in parts of philosophy, psychology, linguistics, and even art

history, all converge to suggest that the traditional paradigm is somehow

askew. That failure to fit is also made increasingly apparent by the

historical study of science to which most of our attention is necessarily

directed here.

None of these crisis-promoting subjects has yet produced a viable

alternate to the traditional epistemological paradigm, but they do begin

to suggest what some of that paradigm’s characteristics will be. I am, for

example, acutely aware of the difficulties created by saying that when

Aristotle and Galileo looked at swinging stones, the first saw constrained

fall, the second a pendulum. The same difficulties are presented in an

even more fundamental form by the opening sentences of this section:

though the world does not change with a change of paradigm, the

scientist afterward works in a different world. Nevertheless, I am

convinced that we must learn to make sense of statements that at least

resemble these. What occurs during a scientific revolution is not fully

reducible to a reinterpretation of individual and stable data. In the first

place, the data are not unequivocally stable. A pendulum is not a falling

stone, nor is oxygen dephlogisticated air. Consequently, the data that

scientists collect from these diverse objects are, as we shall shortly see,

themselves different. More important, the process by which

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either the individual or the community makes the transition from

constrained fall to the pendulum or from dephlogisticated air to oxygen

is not one that resembles interpretation. How could it do so in the

absence of fixed data for the scientist to interpret? Rather than being an

interpreter, the scientist who embraces a new paradigm is like the woman

wearing inverting lenses. Confronting the same constellation of objects

as before and knowing that she does so, she nevertheless finds them

transformed through and through in many of their details.

None of these remarks is intended to indicate that scientists do not

characteristically interpret observations and data. On the contrary,

Galileo interpreted observations on the pendulum, Aristotle

observations on falling stones, Musschenbroek observations on a charge-

filled bottle, and Franklin observations on a condenser. But each of these

interpretations presupposed a paradigm. They were parts of normal

science, an enterprise that, as we have already seen, aims to refine,

extend, and articulate a paradigm that is already in existence. Section III

provided many examples in which interpretation played a central role.

Those examples typify the overwhelming majority of research. In each

of them the scientist, by virtue of an accepted paradigm, knew what a

datum was, what instruments might be used to retrieve it, and what

concepts were relevant to its interpretation. Given a paradigm,

interpretation of data is central to the enterprise that explores it.

But that interpretive enterprise—and this was the burden of the

paragraph before last—can only articulate a paradigm, not correct it.

Paradigms are not corrigible by normal science at all. Instead, as we

have already seen, normal science ultimately leads only to the

recognition of anomalies and to crises. And these are terminated, not by

deliberation and interpretation, but by a relatively sudden and

unstructured event like the gestalt switch. Scientists then often speak of

the “scales falling from the eyes” or of the “lightning flash” that

“inundates” a previously obscure puzzle, enabling its components to be

seen in a new way that for the first time permits its solution. On other

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occasions the relevant illumination comes in sleep.13 No ordinary sense

of the term ‘interpretation’ fits these flashes of intuition through which a

new paradigm is born. Though such intuitions depend upon the

experience, both anomalous and congruent, gained with the old

paradigm, they are not logically or piecemeal linked to particular items

of that experience as an interpretation would be. Instead, they gather up

large portions of that experience and transform them to the rather

different bundle of experience that will thereafter be linked piecemeal

to the new paradigm but not to the old.

To learn more about what these differences in experience can be,

return for a moment to Aristotle, Galileo, and the pendulum. What data

did the interaction of their different paradigms and their common

environment make accessible to each of them? Seeing constrained fall,

the Aristotelian would measure (or at least discuss—the Aristotelian

seldom measured) the weight of the stone, the vertical height to which it

had been raised, and the time required for it to achieve rest. Together

with the resistance of the medium, these were the conceptual categories

deployed by Aristotelian science when dealing with a falling body.14

Normal research guided by them could not have produced the laws that

Galileo discovered. It could only—and by another route it did—lead to

the series of crises from which Galileo’s view of the swinging stone

emerged. As a result of those crises and of other intellectual changes

besides, Galileo saw the swinging stone quite differently. Archimedes’

work on floating bodies made the medium non-essential; the impetus

theory rendered the motion symmetrical and enduring; and

Neoplatonism directed Galileo’s attention to the motion’s circu-

13 [Jacques] Hadamard, Subconscient intuition, et logique dans la recherche

scientifique (Conférence faite au Palais de la Découverte le 8 Décembre 1945

[Alençon, n.d.]), pp. 7-8. A much fuller account, though one exclusively restricted

to mathematical innovations, is the same author’s The Psychology of Invention in

the Mathematical Field (Princeton, 1949).

14 T. S. Kuhn, “A Function for Thought Experiments,” in Mélanges Alexandre

Koyré, ed. R. Taton and I. B. Cohen, to be published by Hermann (Paris) in 1963.

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lar form.15 She therefore measured only weight, radius, angular

displacement, and time per swing, which were precisely the data that

could be interpreted to yield Galileo’s laws for the pendulum. In the

event, interpretation proved almost unnecessary. Given Galileo’s

paradigms, pendulum-like regularities were very nearly accessible to

inspection. How else are we to account for Galileo’s discovery that the

bob’s period is entirely independent of amplitude, a discovery that the

normal science stemming from Galileo had to eradicate and that we are

quite unable to document today. Regularities that could not have existed

for an Aristotelian (and that are, in fact, nowhere precisely exemplified

by nature) were consequences of immediate experience for the woman

who saw the swinging stone as Galileo did.

Perhaps that example is too fanciful since the Aristotelians recorded

no discussions of swinging stones. On their paradigm it was an

extraordinarily complex phenomenon. But the Aristotelians did discuss

the simpler case, stones falling without uncommon constraints, and the

same differences of vision are apparent there. Contemplating a falling

stone, Aristotle saw a change of state rather than a process. For her the

relevant measures of a motion were therefore total distance covered and

total time elapsed, parameters which yield what we should now call not

speed but average speed.16 Similarly, because the stone was impelled by

its nature to reach its final resting point, Aristotle saw the relevant

distance parameter at any instant during the motion as the distance to

the final end point rather than as that from the origin of motion.17 Those

conceptual parameters underlie and give sense to most of her well-

known “laws of motion.” Partly through the impetus paradigm, however,

and partly through a doctrine known as the latitude of forms, scholastic

criticism changed this way of viewing motion. A stone moved by

impetus gained more and more of it while receding from its

15 A. Koyré, Études Galiléennes (Paris, 1939), I, 46-51; and “Galileo and Plato,”

Journal of the History of Ideas, IV (1943), 400-428.

16 Kuhn, “A Function for Thought Experiments,” in Mélanges Alexandre Koyré (see

n. 14 for full citation).

17 Koyré, Études . . . , II, 7-11.

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starting point; distance from rather than distance to therefore became

the revelant parameter. In addition, Aristotle’s notion of speed was

bifurcated by the scholastics into concepts that soon after Galileo

became our average speed and instantaneous speed. But when seen

through the paradigm of which these conceptions were a part, the

falling stone, like the pendulum, exhibited its governing laws almost on

inspection. Galileo was not one of the first women to suggest that stones

fall with a uniformly accelerated motion.18 Furthermore, she had

developed her theorem on this subject together with many of its

consequences before she experimented with an inclined plane. That

theorem was another one of the network of new regularities accessible

to genius in the world determined jointly by nature and by the

paradigms upon which Galileo and her contemporaries had been raised.

Living in that world, Galileo could still, when she chose, explain why

Aristotle had seen what she did. Nevertheless, the immediate content of

Galileo’s experience with falling stones was not what Aristotle’s had

been.

It is, of course, by no means clear that we need be so concerned with

“immediate experience”—that is, with the perceptual features that a

paradigm so highlights that they surrender their regularities almost

upon inspection. Those features must obviously change with the

scientist’s commitments to paradigms, but they are far from what we

ordinarily have in mind when we speak of the raw data or the brute

experience from which scientific research is reputed to proceed. Perhaps

immediate experience should be set aside as fluid, and we should

discuss instead the concrete operations and measurements that the

scientist performs in her laboratory. Or perhaps the analysis should be

carried further still from the immediately given. It might, for example,

be conducted in terms of some neutral observation-language, perhaps

one designed to conform to the retinal imprints that mediate what the

scientist sees. Only in one of these ways can we hope to retrieve a realm

in which experience is again stable once and for all—in which the

pendulum and constrained fall are not different perceptions but rather

18 Clagett, op. cit., chaps, iv, vi, and ix.

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different interpretations of the unequivocal data provided by

observation of a swinging stone.

But is sensory experience fixed and neutral? Are theories simply man-

made interpretations of given data? The epistemological viewpoint that

has most often guided Western philosophy for three centuries dictates

an immediate and unequivocal, Yes! In the absence of a developed

alternative, I find it impossible to relinquish entirely that viewpoint. Yet

it no longer functions effectively, and the attempts to make it do so

through the introduction of a neutral language of observations now

seem to me hopeless.

The operations and measurements that a scientist undertakes in the

laboratory are not “the given” of experience but rather “the collected

with difficulty.” They are not what the scientist sees—at least not before

his research is well advanced and her attention focused. Rather, they are

concrete indices to the content of more elementary perceptions, and as

such they are selected for the close scrutiny of normal research only

because they promise opportunity for the fruitful elaboration of an

accepted paradigm. Far more clearly than the immediate experience

from which they in part derive, operations and measurements are

paradigm-determined. Science does not deal in all possible laboratory

manipulations. Instead, it selects those relevant to the juxtaposition of a

paradigm with the immediate experience that that paradigm has

partially determined. As a result, scientists with different paradigms

engage in different concrete laboratory manipulations. The

measurements to be performed on a pendulum are not the ones

relevant to a case of constrained fall. Nor are the operations relevant for

the elucidation of oxygen’s properties uniformly the same as those

required when investigating the characteristics of dephlogisticated air.

As for a pure observation-language, perhaps one will yet be devised.

But three centuries after Descartes our hope for such an eventuality still

depends exclusively upon a theory of perception and of the mind. And

modern psychological experimentation is rapidly proliferating

phenomena with which that theory can scarcely deal. The duck-rabbit

shows that two women

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with the same retinal impressions can see different things; the inverting

lenses show that two women with different retinal impressions can see the

same thing. Psychology supplies a great deal of other evidence to the

same effect, and the doubts that derive from it are readily reinforced by

the history of attempts to exhibit an actual language of observation. No

current attempt to achieve that end has yet come close to a generally

applicable language of pure percepts. And those attempts that come

closest share one characteristic that strongly reinforces several of this

essay’s main theses. From the start they presuppose a paradigm, taken

either from a current scientific theory or from some fraction of everyday

discourse, and they then try to eliminate from it all non-logical and non-

perceptual terms. In a few realms of discourse this effort has been

carried very far and with fascinating results. There can be no question

that efforts of this sort are worth pursuing. But their result is a language

that—like those employed in the sciences—embodies a host of

expectations about nature and fails to function the moment these

expectations are violated. Nelson Goodman makes exactly this point in

describing the aims of her Structure of Appearance: “It is fortunate that

nothing more [than phenomena known to exist] is in question; for the

notion of ‘possible’ cases, of cases that do not exist but might have

existed, is far from clear.”19 No language thus restricted to reporting a

world fully known in advance can produce mere neutral and objective

reports on “the given.” Philosophical investigation has not yet provided

even a hint of what a language able to do that would be like.

Under these circumstances we may at least suspect that scientists are

right in principle as well as in practice when they treat

19 N. Goodman, The Structure of Appearance (Cambridge, Mass., 1951), pp. 4-5.

The passage is worth quoting more extensively: “If all and only those residents of

Wilmington in 1947 that weigh between 175 and 180 pounds have red hair, then

‘red-haired 1947 resident of Wilmington’ and ‘1947 resident of Wilmington

weighing between 175 and 180 pounds’ may be joined in a constructional

definition. . . . The question whether there ‘might have been’ someone to whom

one but not the other of these predicates would apply has no bearing . . . once we

have determined that there is no such person. . . . It is fortunate that nothing

more is in question; for the notion of ‘possible’ cases, of cases that do not exist but

might have existed, is far from clear.”

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oxygen and pendulums (and perhaps also atoms and electrons) as the

fundamental ingredients of their immediate experience. As a result of

the paradigm-embodied experience of the race, the culture, and, finally,

the profession, the world of the scientist has come to be populated with

planets and pendulums, condensers and compound ores, and other such

bodies besides. Compared with these objects of perception, both meter

stick readings and retinal imprints are elaborate constructs to which

experience has direct access only when the scientist, for the special

purposes of her research, arranges that one or the other should do so.

This is not to suggest that pendulums, for example, are the only things a

scientist could possibly see when looking at a swinging stone. (We have

already noted that members of another scientific community could see

constrained fall.) But it is to suggest that the scientist who looks at a

swinging stone can have no experience that is in principle more

elementary than seeing a pendulum. The alternative is not some

hypothetical “fixed” vision, but vision through another paradigm, one

which makes the swinging stone something else.

All of this may seem more reasonable if we again remember that

neither scientists nor laymen learn to see the world piecemeal or item

by item. Except when all the conceptual and manipulative categories are

prepared in advance—e.g., for the discovery of an additional transuranic

element or for catching sight of a new house—both scientists and laymen

sort out whole areas together from the flux of experience. The child who

transfers the word ‘mama’ from all humans to all females and then to her

mother is not just learning what ‘mama’ means or who her mother is.

Simultaneously she is learning some of the differences between males

and females as well as something about the ways in which all but one

female will behave towardher. Her reactions, expectations, and beliefs—

indeed, much of her perceived world—change accordingly. By the same

token, the Copernicans who denied its traditional title ‘planet’ to the sun

were not only learning what ‘planet’ meant or what the sun was. Instead,

they were changing the meaning of ‘planet’ so that it could continue to

make useful distinctions in a world where all celestial bodies,

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not just the sun, were seen differently from the way they had been seen

before. The same point could be made about any of our earlier

examples. To see oxygen instead of dephlogisticated air, the condenser

instead of the Leyden jar, or the pendulum instead of constrained fall,

was only one part of an integrated shift in the scientist’s vision of a great

many related chemical, electrical, or dynamical phenomena. Paradigms

determine large areas of experience at the same time.

It is, however, only after experience has been thus determined that

the search for an operational definition or a pure observation-language

can begin. The scientist or philosopher who asks what measurements or

retinal imprints make the pendulum what it is must already be able to

recognize a pendulum when she sees one. If she saw constrained fall

instead, her question could not even be asked. And if she saw a

pendulum, but saw it in the same way she saw a tuning fork or an

oscillating balance, her question could not be answered. At least it could

not be answered in the same way, because it would not be the same

question. Therefore, though they are always legitimate and are

occasionally extraordinarily fruitful, questions about retinal imprints or

about the consequences of particular laboratory manipulations

presuppose a world already perceptually and conceptually subdivided in

a certain way. In a sense such questions are parts of normal science, for

they depend upon the existence of a paradigm and they receive

different answers as a result of paradigm change.

To conclude this section, let us henceforth neglect retinal impressions

and again restrict attention to the laboratory operations that provide the

scientist with concrete though fragmentary indices to what she has

already seen. One way in which such laboratory operations change with

paradigms has already been observed repeatedly. After a scientific

revolution many old measurements and manipulations become

irrelevant and are replaced by others instead. One does not apply all the

same tests to oxygen as to dephlogisticated air. But changes of this sort

are never total. Whatever she may then see, the scientist after a

revolution is still looking at the same world. Further-

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more, though she may previously have employed them differently, much

of her language and most of her laboratory instruments are still the same

as they were before. As a result, postrevolutionary science invariably

includes many of the same manipulations, performed with the same

instruments and described in the same terms, as its prerevolutionary

predecessor. If these enduring manipulations have been changed at all,

the change must lie either in their relation to the paradigm or in their

concrete results. I now suggest, by the introduction of one last new

example, that both these sorts of changes occur. Examining the work of

Dalton and her contemporaries, we shall discover that one and the same

operation, when it attaches to nature through a different paradigm, can

become an index to a quite different aspect of nature’s regularity. In

addition, we shall see that occasionally the old manipulation in its new

role will yield different concrete results.

Throughout much of the eighteenth century and into the nineteenth,

European chemists almost universally believed that the elementary

atoms of which all chemical species consisted were held together by

forces of mutual affinity. Thus a lump of silver cohered because of the

forces of affinity between silver corpuscles (until after Lavoisier these

corpuscles were themselves thought of as compounded from still more

elementary particles). On the same theory silver dissolved in acid (or

salt in water) because the particles of acid attracted those of silver (or

the particles of water attracted those of salt) more strongly than

particles of these solutes attracted each other. Or again, copper would

dissolve in the silver solution and precipitate silver, because the copper-

acid affinity was greater than the affinity of acid for silver. A great many

other phenomena were explained in the same way. In the eighteenth

century the theory of elective affinity was an admirable chemical

paradigm, widely and sometimes fruitfully deployed in the design and

analysis of chemical experimentation.20

Affinity theory, however, drew the line separating physical

20 H. Metzger, Newton, Stahl, Boerlwave et la doctrine chimique (Paris, 1930), pp. 34-

68.

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mixtures from chemical compounds in a way that has become

unfamiliar since the assimilation of Dalton’s work. Eighteenth-century

chemists did recognize two sorts of processes. When mixing produced

heat, light, effervescence or something else of the sort, chemical union

was seen to have taken place. If, on the other hand, the particles in the

mixture could be distinguished by eye or mechanically separated, there

was only physical mixture. But in the very large number of intermediate

cases—salt in water, alloys, glass, oxygen in the atmosphere, and so on—

these crude criteria were of little use. Guided by their paradigm, most

chemists viewed this entire intermediate range as chemical, because the

processes of which it consisted were all governed by forces of the same

sort. Salt in water or oxygen in nitrogen was just as much an example of

chemical combination as was the combination produced by oxidizing

copper. The arguments for viewing solutions as compounds were very

strong. Affinity theory itself was well attested. Besides, the formation of

a compound accounted for a solution’s observed homogeneity. If, for

example, oxygen and nitrogen were only mixed and not combined in the

atmosphere, then the heavier gas, oxygen, should settle to the bottom.

Dalton, who took the atmosphere to be a mixture, was never

satisfactorily able to explain oxygen’s failure to do so. The assimilation

of her atomic theory ultimately created an anomaly where there had

been none before.21

One is tempted to say that the chemists who viewed solutions as

compounds differed from their successors only over a matter of

definition. In one sense that may have been the case. But that sense is

not the one that makes definitions mere conventional conveniences. In

the eighteenth century mixtures were not fully distinguished from

compounds by operational tests, and perhaps they could not have been.

Even if chemists had looked for such tests, they would have sought

criteria that made the solution a compound. The mixture-compound

distinction was part of their paradigm—part of the way they viewed their

whole

21 Ibid., pp. 124-29, 139-48. For Dalton, see Leonard K. Nash, The Atomic-Molecular

Theory (“Harvard Case Histories in Experimental Science,” Case 4; Cambridge,

Mass., 1950), pp. 14-21.

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field of research—and as such it was prior to any particular laboratory

test, though not to the accumulated experience of chemistry as a whole.

But while chemistry was viewed in this way, chemical phenomena

exemplified laws different from those that emerged with the

assimilation of Dalton’s new paradigm. In particular, while solutions

remained compounds, no amount of chemical experimentation could by

itself have produced the law of fixed proportions. At the end of the

eighteenth century it was widely known that some compounds ordinarily

contained fixed proportions by weight of their constituents. For some

categories of reactions the German chemist Richter had even noted the

further regularities now embraced by the law of chemical equivalents.22

But no chemist made use of these regularities except in recipes, and no

one until almost the end of the century thought of generalizing them.

Given the obvious counterinstances, like glass or like salt in water, no

generalization was possible without an abandonment of affinity theory

and a reconceptualization of the boundaries of the chemist’s domain.

That consequence became explicit at the very end of the century in a

famous debate between the French chemists Proust and Berthollet. The

first claimed that all chemical reactions occurred in fixed proportion,

the latter that they did not. Each collected impressive experimental

evidence for her view. Nevertheless, the two women necessarily talked

through each other, and their debate was entirely inconclusive. Where

Berthollet saw a compound that could vary in proportion, Proust saw

only a physical mixture.23 To that issue neither experiment nor a change

of definitional convention could be relevant. The two women were as

fundamentally at cross-purposes as Galileo and Aristotle had been.

This was the situation during the years when John Dalton undertook

the investigations that led finally to her famous chemical atomic theory.

But until the very last stages of those investiga-

22 J. R. Partington, A Short History of Chemistry (2d ed.; London, 1951), pp. 161-63.

23 A. N. Meldrum, “The Development of the Atomic Theory: (1) Berthollet’s

Doctrine of Variable Proportions,” Manchester Memoirs, LIV (1910), 1-16.

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tions, Dalton was neither a chemist nor interested in chemistry. Instead,

he was a meteorologist investigating the, forher, physical problems of

the absorption of gases by water and of water by the atmosphere. Partly

because her training was in a different specialty and partly because of

his own work in that specialty, she approached these problems with a

paradigm different from that of contemporary chemists. In particular,

he viewed the mixture of gases or the absorption of a gas in water as a

physical process, one in which forces of affinity played no part. Toher,

therefore, the observed homogeneity of solutions was a problem, but

one which she thought she could solve if she could determine the relative

sizes and weights of the various atomic particles in her experimental

mixtures. It was to determine these sizes and weights that Dalton finally

turned to chemistry, supposing from the start that, in the restricted

range of reactions that she took to be chemical, atoms could only

combine one-to-one or in some other simple whole-number ratio.24 That

natural assumption did enable her to determine the sizes and weights

of elementary particles, but it also made the law of constant proportion

a tautology. For Dalton, any reaction in which the ingredients did not

enter in fixed proportion was ipso facto not a purely chemical process. A

law that experiment could not have established before Dalton’s work,

became, once that work was accepted, a constitutive principle that no

single set of chemical measurements could have upset. As a result of

what is perhaps our fullest example of a scientific revolution, the same

chemical manipulations assumed a relationship to chemical

generalization very different from the one they had had before.

Needless to say, Dalton’s conclusions were widely attacked when first

announced. Berthollet, in particular, was never convinced. Considering

the nature of the issue, she need not have been. But to most chemists

Dalton’s new paradigm proved convincing where Proust’s had not been,

for it had implications far wider and more important than a new

criterion for distinguish-

24 L. K. Nash, “The Origin of Dalton’s Chemical Atomic Theory,” Isis, XLVII

(1956), 101-16.

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ing a mixture from a compound. If, for example, atoms could combine

chemically only in simple whole-number ratios, then a re-examination

of existing chemical data should disclose examples of multiple as well as

of fixed proportions. Chemists stopped writing that the two oxides of,

say, carbon contained 56 per cent and 72 per cent of oxygen by weight;

instead they wrote that one weight of carbon would combine either with

1.3 or with 2.6 weights of oxygen. When the results of old manipulations

were recorded in this way, a 2:1 ratio leaped to the eye; and this

occurred in the analysis of many well-known reactions and of new ones

besides. In addition, Dalton’s paradigm made it possible to assimilate

Richter’s work and to see its full generality. Also, it suggested new

experiments, particularly those of Gay-Lussac on combining volumes,

and these yielded still other regularities, ones that chemists had not

previously dreamed of. What chemists took from Dalton was not new

experimental laws but a new way of practicing chemistry (he himself

called it the “new system of chemical philosophy”), and this proved so

rapidly fruitful that only a few of the older chemists in France and

Britain were able to resist it.25 As a result, chemists came to live in a

world where reactions behaved quite differently from the way they had

before.

As all this went on, one other typical and very important change

occurred. Here and there the very numerical data of chemistry began to

shift. When Dalton first searched the chemical literature for data to

support her physical theory, she found some records of reactions that

fitted, but she can scarcely have avoided finding others that did not.

Proust’s own measurements on the two oxides of copper yielded, for

example, an oxygen weight-ratio of 1.47:1 rather than the 2:1 demanded

by the atomic theory; and Proust is just the woman who might have been

expected to achieve the Daltonian ratio.26 She was, that is, a fine

25 A. N. Meldrum, “The Development of the Atomic Theory: (6) The Reception

Accorded to the Theory Advocated by Dalton,” Manchester Memoirs, LV (1911), 1-

10.

26 For Proust, see Meldrum, “Berthollet’s Doctrine of Variable Proportions,”

Manchester Memoirs, LIV (1910), 8. The detailed history of the gradual changes in

measurements of chemical composition and of atomic weights hag yet to be

written, but Partington, op. cit., provides many useful leads to it

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experimentalist, and her view of the relation between mixtures and

compounds was very close to Dalton’s. But it is hard to make nature fit a

paradigm. That is why the puzzles of normal science are so challenging

and also why measurements undertaken without a paradigm so seldom

lead to any conclusions at all. Chemists could not, therefore, simply

accept Dalton’s theory on the evidence, for much of that was still

negative. Instead, even after accepting the theory, they had still to beat

nature into line, a process which, in the event, took almost another

generation. When it was done, even the percentage composition of well-

known compounds was different. The data themselves had changed.

That is the last of the senses in which we may want to say that after a

revolution scientists work in a different world.

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XI. The Invisibility of Revolutions

We must still ask how scientific revolutions close. Before doing so,

however, a last attempt to reinforce conviction about their existence and

nature seems called for. I have so far tried to display revolutions by

illustration, and the examples could be multiplied ad nauseam. But

clearly, most of them, which were deliberately selected for their

familiarity, have customarily been viewed not as revolutions but as

additions to scientific knowledge. That same view could equally well be

taken of any additional illustrations, and these Would probably be

ineffective. I suggest that there are excellent reasons why revolutions

have proved to be so nearly invisible. Both scientists and laymen take

much of their image of creative scientific activity from an authoritative

source that systematically disguises—partly for important functional

reasons—the existence and significance of scientific revolutions. Only

when the nature of that authority is recognized and analyzed can one

hope to make historical example fully effective. Furthermore, though

the point can be fully developed only in my concluding section, the

analysis now required will begin to indicate one of the aspects of

scientific work that most clearly distinguishes it from every other

creative pursuit except perhaps theology.

As the source of authority, I have in mind principally textbooks of

science together with both the popularizations and the philosophical

works modeled on them. All three of these categories—until recently no

other significant sources of information about science have been

available except through the practice of research—have one thing in

common. They address themselves to an already articulated body of

problems, data, and theory, most often to the particular set of paradigms

to which the scientific community is committed at the time they are

written. Textbooks themselves aim to communicate the vocabulary and

syntax of a contemporary scientific language. Popularizations attempt to

describe these same applications in a language

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closer to that of everyday life. And philosophy of science, particularly

that of the English-speaking world, analyzes the logical structure of the

same completed body of scientific knowledge. Though a fuller treatment

would necessarily deal with the very real distinctions between these

three genres, it is their similarities that most concern us here. All three

record the stable outcome of past revolutions and thus display the bases

of the current normal-scientific tradition. To fulfill their function they

need not provide authentic information about the way in which those

bases were first recognized and then embraced by the profession. In the

case of textbooks, at least, there are even good reasons why, in these

matters, they should be systematically misleading.

We noted in Section II that an increasing reliance on textbooks or

their equivalent was an invariable concomitant of the emergence of a

first paradigm in any field of science. The concluding section of this

essay will argue that the domination of a mature science by such texts

significantly differentiates its developmental pattern from that of other

fields. For the moment let us simply take it for granted that, to an extent

unprecedented in other fields, both the layman’s and the practitioner’s

knowledge of science is based on textbooks and a few other types of

literature derived from them. Textbooks, however, being pedagogic

vehicles for the perpetuation of normal science, have to be rewritten in

whole or in part whenever the language, problem-structure, or

standards of normal science change. In short, they have to be rewritten

in the aftermath of each scientific revolution, and, once rewritten, they

inevitably disguise not only the role but the very existence of the

revolutions that produced them. Unless she has personally experienced a

revolution in her own lifetime, the historical sense either of the working

scientist or of the lay reader of textbook literature extends only to the

outcome of the most recent revolutions in the field.

Textbooks thus begin by truncating the scientist’s sense of her

discipline’s history and then proceed to supply a substitute for what

they have eliminated. Characteristically, textbooks of science contain

just a bit of history, either in an introductory

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chapter or, more often, in scattered references to the great heroes of an

earlier age. From such references both students and professionals come

to feel like participants in a long-standing historical tradition. Yet the

textbook-derived tradition in which scientists come to sense their

participation is one that, in fact, never existed. For reasons that are both

obvious and highly functional, science textbooks (and too many of the

older histories of science) refer only to that part of the work of past

scientists that can easily be viewed as contributions to the statement and

solution of the texts’ paradigm problems. Partly by selection and partly

by distortion, the scientists of earlier ages are implicitly represented as

having worked upon the same set of fixed problems and in accordance

with the same set of fixed canons that the most recent revolution in

scientific theory and method has made seem scientific. No wonder that

textbooks ‘ and the historical tradition they imply have to be rewritten

after each scientific revolution. And no wonder that, as they are

rewritten, science once again comes to seem largely cumulative.

Scientists are not, of course, the only group that tends to see its

discipline’s past developing linearly toward its present vantage. The

temptation to write history backward is both omnipresent and

perennial. But scientists are more affected by the temptation to rewrite

history, partly because the results of scientific research show no obvious

dependence upon the historical context of the inquiry, and partly

because, except during crisis and revolution, the scientist’s

contemporary position seems so secure. More historical detail, whether

of science’s present or of its past, or more responsibility to the historical

details that are presented, could only give artificial status to human

idiosyncrasy, error, and confusion. Why dignify what science’s best and

most persistent efforts have made it possible to discard? The

depreciation of historical fact is deeply, and probably functionally,

ingrained in the ideology of the scientific profession, the same

profession that places the highest of all values upon factual details of

other sorts. Whitehead caught the unhistorical spirit of the scientific

community when she wrote, “A science that hesitates to forget its

founders is lost.” Yet she was not quite

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right, for the sciences, like other professional enterprises, do need their

heroes and do preserve their names. Fortunately, instead of forgetting

these heroes, scientists have been able to forget or revise their works.

The result is a persistent tendency to make the history of science look

linear or cumulative, a tendency that even affects scientists looking back

at their own research. For example, all three of Dalton’s incompatible

accounts of the development of her chemical atomism make it appear

that she was interested from an early date in just those chemical

problems of combining proportions that she was later famous for having

solved. Actually those problems seem only to have occurred to her with

their solutions, and then not until her own creative work was very nearly

complete.1 What all of Dalton’s accounts omit are the revolutionary

effects of applying to chemistry a set of questions and concepts

previously restricted to physics and meteorology. That is what Dalton

did, and the result was a reorientation toward the field, a reorientation

that taught chemists to ask new questions about and to draw new

conclusions from old data.

Or again, Newton wrote that Galileo had discovered that the constant

force of gravity produces a motion proportional to the square of the

time. In fact, Galileo’s kinematic theorem does take that form when

embedded in the matrix of Newton’s own dynamical concepts. But

Galileo said nothing of the sort. Her discussion of falling bodies rarely

alludes to forces, much less to a uniform gravitational force that causes

bodies to fall.2 By crediting to Galileo the answer to a question that

Galileo’s paradigms did not permit to be asked, Newton’s account hides

the effect of a small but revolutionary reformulation in the questions

that scientists asked about motion as well as in the

1 L. K. Nash, “The Origins of Dalton’s Chemical Atomic Theory,” his, XLVII (1956), 101-

16.

2 For Newton’s remark, see Florian Cajori (ed.), Sir Isaac Newton’s Mathematical

Principles of Natural Philosophy and Her System of the World (Berkeley, Calif., 1946), p. 21.

The passage should be compared with Galileo’s own discussion in her Dialogues

concerning Two New Sciences, trans. H. Crew and A. de Salvio (Evanston, Ill., 1946), pp.

154-76.

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answers they felt able to accept. But it is just this sort of change in the

formulation of questions and answers that accounts, far more than

novel empirical discoveries, for the transition from Aristotelian to

Galilean and from Galilean to Newtonian dynamics. By disguising such

changes, the textbook tendency to make the development of science

linear hides a process that lies at the heart of the most significant

episodes of scientific development.

The preceding examples display, each within the context of a single

revolution, the beginnings of a reconstruction of history that is regularly

completed by postrevolutionary science texts. But in that completion

more is involved than a multiplication of the historical misconstructions

illustrated above. Those misconstructions render revolutions invisible;

the arrangement of the still visible material in science texts implies a

process that, if it existed, would deny revolutions a function. Because

they aim quickly to acquaint the student with what the contemporary

scientific community thinks it knows, textbooks treat the various

experiments, concepts, laws, and theories of the current normal science

as separately and as nearly seriatim as possible. As pedagogy this

technique of presentation is unexceptionable. But when combined with

the generally unhistorical air of science writing and with the occasional

systematic misconstructions discussed above, one strong impression is

overwhelmingly likely to follow: science has reached its present state by

a series of individual discoveries and inventions that, when gathered

together, constitute the modern body of technical knowledge. From the

beginning of the scientific enterprise, a textbook presentation implies,

scientists have striven for the particular objectives that are embodied in

today’s paradigms. One by one, in a process often compared to the

addition of bricks to a building, scientists have added another fact,

concept, law, or theory to the body of information supplied in the

contemporary science text.

But that is not the way a science develops. Many of the puzzles of

contemporary normal science did not exist until after the most recent

scientific revolution. Very few of them can be

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traced back to the historic beginning of the science within which they

now occur. Earlier generations pursued their own problems with their

own instruments and their own canons of solution. Nor is it just the

problems that have changed. Rather the whole network of fact and

theory that the textbook paradigm fits to nature has shifted. Is the

constancy of chemical composition, for example, a mere fact of

experience that chemists could have discovered by experiment within

any one of the worlds within which chemists have practiced? Or is it

rather one element—and an indubitable one, at that—in a new fabric of

associated fact and theory that Dalton fitted to the earlier chemical

experience as a whole, changing that experience in the process? Or by

the same token, is the constant acceleration produced by a constant

force a mere fact that students of dynamics have always sought, or is it

rather the answer to a question that first arose only within Newtonian

theory and that that theory could answer from the body of information

available before the question was asked?

These questions are here asked about what appear as the piecemeal-

discovered facts of a textbook presentation. But obviously, they have

implications as well for what the text presents as theories. Those

theories, of course, do “fit the facts,” but only by transforming

previously accessible information into facts that, for the preceding

paradigm, had not existed at all. And that means that theories too do

not evolve piecemeal to fit facts that were there all the time. Rather,

they emerge together with the facts they fit from a revolutionary

reformulation of the preceding scientific tradition, a tradition within

which the knowledge-mediated relationship between the scientist and

nature was not quite the same.

One last example may clarify this account of the impact of textbook

presentation upon our image of scientific development. Every

elementary chemistry text must discuss the concept of a chemical

element. Almost always, when that notion is introduced, its origin is

attributed to the seventeenth-century chemist, Robert Boyle, in whose

Sceptical Chymist the attentive reader will find a definition of ‘element’

quite close to that in

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use today. Reference to Boyle’s contribution helps to make the neophyte

aware that chemistry did not begin with the sulfa drugs; in addition, it

tells her that one of the scientist’s traditional tasks is to invent concepts

of this sort. As a part of the pedagogic arsenal that makes a woman a

scientist, the attribution is immensely successful. Nevertheless, it

illustrates once more the pattern of historical mistakes that misleads

both students and laymen about the nature of the scientific enterprise.

According to Boyle, who was quite right, her “definition” of an

element was no more than a paraphrase of a traditional chemical

concept; Boyle offered it only in order to argue that no such thing as a

chemical element exists; as history, the textbook version of Boyle’s

contribution is quite mistaken.3 That mistake, of course, is trivial,

though no more so than any other misrepresentation of data. What is

not trivial, however, is the impression of science fostered when this sort

of mistake is first compounded and then built into the technical

structure of the text. Like ‘time,’ ‘energy,’ ‘force,’ or ‘particle,’ the concept

of an element is the sort of textbook ingredient that is often not

invented or discovered at all. Boyle’s definition, in particular, can be

traced back at least to Aristotle and forward through Lavoisier into

modern texts. Yet that is not to say that science has possessed the

modern concept of an element since antiquity. Verbal definitions like

Boyle’s have little scientific content when considered by themselves.

They are not full logical specifications of meaning (if there are such),

but more nearly pedagogic aids. The scientific concepts to which they

point gain full significance only when related, within a text or other

systematic presentation, to other scientific concepts, to manipulative

procedures, and to paradigm applications. It follows that concepts like

that of an element can scarcely be invented independent of context.

Furthermore, given the context, they rarely require invention because

they are already at hand. Both Boyle and Lavoisier changed the

chemical significance of ‘element’ in important ways. But they did not

invent the notion

3 T. S. Kuhn, “Robert Boyle and Structural Chemistry in the Seventeenth Century,” Isis,

XLIII (1952), 26-29.

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or even change the verbal formula that serves as its definition. Nor, as

we have seen, did Einstein have to invent or even explicitly redefine

‘space’ and ‘time’ in order to give them new meaning within the context

of her work.

What then was Boyle’s historical function in that part of her work that

includes the famous “definition”? She was a leader of a scientific

revolution that, by changing the relation of ‘element’ to chemical

manipulation and chemical theory, transformed the notion into a tool

quite different from what it had been before and transformed both

chemistry and the chemist’s world in the process.4 Other revolutions,

including the one that centers around Lavoisier, were required to give

the concept its modern form and function. But Boyle provides a typical

example both of the process involved at each of these stages and of what

happens to that process when existing knowledge is embodied in a

textbook. More than any other single aspect of science, that pedagogic

form has determined our image of the nature of science and of the role

of discovery and invention in its advance.

4 Marie Boas, in her Robert Boyle and Seventeenth-Century Chemistry (Cambridge, 1958),

deals in many places with Boyle’s positive contributions to the evolution of the concept

of a chemical element.

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XII. The Resolution of Revolutions

The textbooks we have just been discussing are produced only in the

aftermath of a scientific revolution. They are the bases for a new

tradition of normal science. In taking up the question of their structure

we have clearly missed a step. What is the process by which a new

candidate for paradigm replaces its predecessor? Any new

interpretation of nature, whether a discovery or a theory, emerges first

in the mind of one or a few individuals. It is they who first learn to see

science and the world differently, and their ability to make the

transition is facilitated by two circumstances that are not common to

most other members of their profession. Invariably their attention has

been intensely concentrated upon the crisis-provoking problems;

usually, in addition, they are women so young or so new to the crisis-ridden

field that practice has committed them less deeply than most of their

contemporaries to the world view and rules determined by the old

paradigm. How are they able, what must they do, to convert the entire

profession or the relevant professional subgroup to their way of seeing

science and the world? What causes the group to abandon one tradition

of normal research in favor of another?

To see the urgency of those questions, remember that they are the only

reconstructions the historian can supply for the philosopher’s inquiry

about the testing, verification, or falsification of established scientific

theories. In so far as she is engaged in normal science, the research

worker is a solver of puzzles, not a tester of paradigms. Though she may,

dining the search for a particular puzzle’s solution, try out a number of

alternative approaches, rejecting those that fail to yield the desired

result, she is not testing the paradigm when she does so. Instead she is like

the chess player who, with a problem stated and the board physically or

mentally beforeher, tries out various alternative moves in the search

for a solution. These trial attempts, whether by the chess player or by

the scientist, are

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trials only of themselves, not of the rules of the game. They are possible

only so long as the paradigm itself is taken for granted. Therefore,

paradigm-testing occurs only after persistent failure to solve a

noteworthy puzzle has given rise to crisis. And even then it occurs only

after the sense of crisis has evoked an alternate candidate for paradigm.

In the sciences the testing situation never consists, as puzzle-solving

does, simply in the comparison of a single paradigm with nature.

Instead, testing occurs as part of the competition between two rival

paradigms for the allegiance of the scientific community.

Closely examined, this formulation displays unexpected and probably

significant parallels to two of the most popular contemporary

philosophical theories about verification. Few philosophers of science

still seek absolute criteria for the verification of scientific theories.

Noting that no theory can ever be exposed to all possible relevant tests,

they ask not whether a theory has been verified but rather about its

probability in the light of the evidence that actually exists. And to

answer that question one important school is driven to compare the

ability of different theories to explain the evidence at hand. That

insistence on comparing theories also characterizes the historical

situation in which a new theory is accepted. Very probably it points one

of the directions in which future discussions of verification should

go. In their most usual forms, however, probabilistic verification

theories all have recourse to one or another of the pure or neutral

observation-languages discussed in Section X. One probabilistic theory

asks that we compare the given scientific theory with all others that

might be imagined to fit the same collection of observed data. Another

demands the construction in imagination of all the tests that the given

scientific theory might conceivably be asked to pass.1 Apparently some

such construction is necessary for the computation of specific

probabilities, absolute or relative, and it is hard to see how such a

construction can

1 For a brief sketch of the main routes to probabilistic verification theories, see

Ernest Nagel, Principles of the Theory of Probability, Vol. I, No. 6, of International

Encyclopedia of Unified Science, pp. 60-75.

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possibly be achieved. If, as I have already urged, there can be no

scientifically or empirically neutral system of language or concepts, then

the proposed construction of alternate tests and theories must proceed

from within one or another paradigm-based tradition. Thus restricted it

would have no access to all possible experiences or to all possible

theories. As a result, probabilistic theories disguise the verification

situation as much as they illuminate it. Though that situation does, as

they insist, depend upon the comparison of theories and of much

widespread evidence, the theories and observations at issue are always

closely related to ones already in existence. Verification is like natural

selection: it picks out the most viable among the actual alternatives in a

particular historical situation. Whether that choice is the best that could

have been made if still other alternatives had been available or if the

data had been of another sort is not a question that can usefully be

asked. There are no tools to employ in seeking answers to it.

A very different approach to this whole network of problems has

been developed by Karl R. Popper who denies the existence of any

verification procedures at all.2 Instead, she emphasizes the importance of

falsification, i.e., of the test that, because its outcome is negative,

necessitates the rejection of an established theory. Clearly, the role thus

attributed to falsification is much like the one this essay assigns to

anomalous experiences, i.e., to experiences that, by evoking crisis,

prepare the way for a new theory. Nevertheless, anomalous experiences

may not be identified with falsifying ones. Indeed, I doubt that the latter

exist. As has repeatedly been emphasized before, no theory ever solves

all the puzzles with which it is confronted at a given time; nor are the

solutions already achieved often perfect. On the contrary, it is just the

incompleteness and imperfection of the existing data-theory fit that, at

any time, define many of the puzzles that characterize normal science. If

any and every failure to fit were ground for theory rejection, all theories

ought to be rejected at all times. On the other hand, if only severe

failure

2 K. R. Popper, The Logic of Scientific Discovery (New York, 1959), esp. chaps, i-iv.

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to fit justifies theory rejection, then the Popperians will require some

criterion of “improbability” or of “degree of falsification.” In developing

one they will almost certainly encounter the same network of difficulties

that has haunted the advocates of the various probabilistic verification

theories.

Many of the preceding difficulties can be avoided by recognizing that

both of these prevalent and opposed views about the underlying logic of

scientific inquiry have tried to compress two largely separate processes

into one. Popper’s anomalous experience is important to science because

it evokes competitors for an existing paradigm. But falsification, though

it surely occurs, does not happen with, or simply because of, the

emergence of an anomaly or falsifying instance. Instead, it is a

subsequent and separate process that might equally well be called

verification since it consists in the triumph of a new paradigm over the

old one. Furthermore, it is in that joint verification-falsification process

that the probabilist’s comparison of theories plays a central role. Such a

two-stage formulation has, I think, the virtue of great verisimilitude, and

it may also enable us to begin explicating the role of agreement (or

disagreement) between fact and theory in the verification process. To

the historian, at least, it makes little sense to suggest that verification is

establishing the agreement of fact with theory. All historically

significant theories have agreed with the facts, but only more or less.

There is no more precise answer to the question whether or how well an

individual theory fits the facts. But questions much like that can be

asked when theories are taken collectively or even in pairs. It makes a

great deal of sense to ask which of two actual and competing theories

fits the facts better. Though neither Priestley’s nor Lavoisier’s theory, for

example, agreed precisely with existing observations, few

contemporaries hesitated more than a decade in concluding that

Lavoisier’s theory provided the better fit of the two.

This formulation, however, makes the task of choosing between

paradigms look both easier and more familiar than it is. If there were

but one set of scientific problems, one world within which to work on

them, and one set of standards for their

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solution, paradigm competition might be settled more or less routinely

by some process like counting the number of problems solved by each.

But, in fact, these conditions are never met completely. The proponents

of competing paradigms are always at least slightly at cross-purposes.

Neither side will grant all the non-empirical assumptions that the other

needs in order to make its case. Like Proust and Berthollet arguing about

the composition of chemical compounds, they are bound partly to talk

through each other. Though each may hope to convert the other to her

way of seeing her science and its problems, neither may hope to prove

his case. The competition between paradigms is not the sort of battle

that can be resolved by proofs. We have already seen several reasons

why the proponents of competing paradigms must fail to make complete

contact with each other’s viewpoints. Collectively these reasons have

been described as the incommensurability of the pre- and postrevo-

lutionary normal-scientific traditions, and we need only recapitulate

them briefly here. In the first place, the proponents of competing

paradigms will often disagree about the list of problems that any

candidate for paradigm must resolve. Their standards or their

definitions of science are not the same. Must a theory of motion explain

the cause of the attractive forces between particles of matter or may it

simply note the existence of such forces? Newton’s dynamics was widely

rejected because, unlike both Aristotle’s and Descartes’s theories, it

implied the latter answer to the question. When Newton’s theory had

been accepted, a question was therefore banished from science. That

question, however, was one that general relativity may proudly claim to

have solved. Or again, as disseminated in the nineteenth century,

Lavoisier’s chemical theory inhibited chemists from asking why the

metals were so much alike, a question that phlogistic chemistry had

both asked and answered. The transition to Lavoisier’s paradigm had,

like the transition to Newton’s, meant a loss not only of a permissible

question but of an achieved solution. That loss was not, however,

permanent either. In the twentieth century questions about the qualities

of

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chemical substances have entered science again, together with some

answers to them.

More is involved, however, than the incommensurability of standards.

Since new paradigms are born from old ones, they ordinarily

incorporate much of the vocabulary and apparatus, both conceptual and

manipulative, that the traditional paradigm had previously employed.

But they seldom employ these borrowed elements in quite the

traditional way. Within the new paradigm, old terms, concepts, and

experiments fall into new relationships one with the other. The

inevitable result is what we must call, though the term is not quite right,

a misunderstanding between the two competing schools. The laymen

who scoffed at Einstein’s general theory of relativity because space

could not be “curved”—it was not that sort of thing—were not simply

wrong or mistaken. Nor were the mathematicians, physicists, and

philosophers who tried to develop a Euclidean version of Einstein’s

theory.3 What had previously been meant by space was necessarily flat,

homogeneous, isotropic, and unaffected by the presence of matter. If it

had not been, Newtonian physics would not have worked. To make the

transition to Einstein’s universe, the whole conceptual web whose

strands are space, time, matter, force, and so on, had to be shifted and

laid down again on nature whole. Only women who had together

undergone or failed to undergo that transformation would be able to

discover precisely what they agreed or disagreed about. Communication

across the revolutionary divide is inevitably partial. Consider, for

another example, the women who called Copernicus mad because she

proclaimed that the earth moved. They were not either just wrong or

quite wrong. Part of what they meant by ‘earth’ was fixed position. Their

earth, at least, could not be moved. Correspondingly, Copernicus’

innovation was not simply to move the earth. Rather, it was a whole new

way of regarding the problems of physics and astronomy,

3 For lay reactions to the concept of curved space, see Philipp Frank, Einstein, Her

Life and Times, trans, and ed. G. Rosen and S. Kusaka (New York, 1947), pp. 142-

46. For a few of the attempts to preserve the gains of general relativity within a

Euclidean space, see C. Nordmann, Einstein and the Universe, trans. J. McCabe

(New York, 1922), chap. ix.

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one that necessarily changed the meaning of both ‘earth’ and ‘motion.’4

Without those changes the concept of a moving earth was mad. On the

other hand, once they had been made and understood, both Descartes

and Huyghens could realize that the earth’s motion was a question with

no content for science.5

These examples point to the third and most fundamental aspect of

the incommensurability of competing paradigms. In a sense that I am

unable to explicate further, the proponents of competing paradigms

practice their trades in different worlds. One contains constrained

bodies that fall slowly, the other pendulums that repeat their motions

again and again. In one, solutions are compounds, in the other mixtures.

One is embedded in a flat, the other in a curved, matrix of space.

Practicing in different worlds, the two groups of scientists see different

things when they look from the same point in the same direction. Again,

that is not to say that they can see anything they please. Both are

looking at the world, and what they look at has not changed. But in

some areas they see different things, and they see them in different

relations one to the other. That is why a law that cannot even be

demonstrated to one group of scientists may occasionally seem

intuitively obvious to another. Equally, it is why, before they can hope to

communicate fully, one group or the other must experience the

conversion that we have been calling a paradigm shift. Just because it is

a transition between incommensurables, the transition between

competing paradigms cannot be made a step at a time, forced by logic

and neutral experience. Like the gestalt switch, it must occur all at once

(though not necessarily in an instant) or not at all.

How, then, are scientists brought to make this transposition? Part of

the answer is that they are very often not. Copernican-ism made few

converts for almost a century after Copernicus’ death. Newton’s work

was not generally accepted, particularly on the Continent, for more than

half a century after the Prin-

4 T. S. Kuhn, The Copernican Revolution (Cambridge, Mass., 1957), chaps. iii, iv,

and vii. The extent to which heliocentrism was more than a strictly astronomical

issue is a major theme of the entire book.

5 Max Jammer, Concepts of Space (Cambridge, Mass., 1954), pp. 118-24.

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cipia appeared.6 Priestley never accepted the oxygen theory, nor Lord

Kelvin the electromagnetic theory, and so on. The difficulties of

conversion have often been noted by scientists themselves. Darwin, in a

particularly perceptive passage at the end of her Origin of Species, wrote:

“Although I am fully convinced of the truth of the views given in this

volume …, I by no means expect to convince experienced naturalists

whose minds are stocked with a multitude of facts all viewed, during a

long course of years, from a point of view directly opposite to mine. . . .

[B]ut I look with confidence to the future,—to young and rising

naturalists, who will be able to view both sides of the question with

impartiality.”7 And Max Planck, surveying her own career in her Scientific

Autobiography, sadly remarked that “a new scientific truth does not

triumph by convincing its opponents and making them see the light, but

rather because its opponents eventually die, and a new generation grows

up that is familiar with it.”8

These facts and others like them are too commonly known to need

further emphasis. But they do need re-evaluation. In the past they have

most often been taken to indicate that scientists, being only human,

cannot always admit their errors, even when confronted with strict

proof. I would argue, rather, that in these matters neither proof nor

error is at issue. The transfer of allegiance from paradigm to paradigm is

a conversion experience that cannot be forced. Lifelong resistance,

particularly from those whose productive careers have committed them

to an older tradition of normal science, is not a violation of scientific

standards but an index to the nature of scientific research itself. The

source of resistance is the assurance that the older paradigm will

ultimately solve all its problems, that nature can be shoved

6 I. B. Cohen, Franklin and Newton: An Inquiry into Speculative Newtonian

Experimental Science and Franklin’s Work in Electricity as an Example Thereof

(Philadelphia, 1956), pp. 93-94.

7 Charles Darwin, On the Origin of Species … (authorized edition from 6th

English ed.; New York, 1889), II, 295-96.

8 Max Planck, Scientific Autobiography and Other Papers, trans. F. Gaynor (New

York, 1949), pp. 33-34.

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into the box the paradigm provides. Inevitably, at times of revolution,

that assurance seems stubborn and pigheaded as indeed it sometimes

becomes. But it is also something more. That same assurance is what

makes normal or puzzle-solving science possible. And it is only through

normal science that the professional community of scientists succeeds,

first, in exploiting the potential scope and precision of the older

paradigm and, then, in isolating the difficulty through the study of

which a new paradigm may emerge.

Still, to say that resistance is inevitable and legitimate, that paradigm

change cannot be justified by proof, is not to say that no arguments are

relevant or that scientists cannot be persuaded to change their minds.

Though a generation is sometimes required to effect the change,

scientific communities have again and again been converted to new

paradigms. Furthermore, these conversions occur not despite the fact

that scientists are human but because they are. Though some scientists,

particularly the older and more experienced ones, may resist

indefinitely, most of them can be reached in one way or another.

Conversions will occur a few at a time until, after the last holdouts have

died, the whole profession will again be practicing under a single, but

now a different, paradigm. We must therefore ask how conversion is

induced and how resisted.

What sort of answer to that question may we expect? Just because it is

asked about techniques of persuasion, or about argument and

counterargument in a situation in which there can be no proof, our

question is a new one, demanding a sort of study that has not previously

been undertaken. We shall have to settle for a very partial and

impressionistic survey. In addition, what has already been said combines

with the result of that survey to suggest that, when asked about

persuasion rather than proof, the question of the nature of scientific

argument has no single or uniform answer. Individual scientists

embrace a new paradigm for all sorts of reasons and usually for several

at once. Some of these reasons—for example, the sun worship that

helped make Kepler a Copernican—lie outside the apparent

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sphere of science entirely.9 Others must depend upon idiosyncrasies of

autobiography and personality. Even the nationality or the prior

reputation of the innovator and her teachers can sometimes play a

significant role.10 Ultimately, therefore, we must learn to ask this

question differently. Our concern will not then be with the arguments

that in fact convert one or another individual, but rather with the sort of

community that always sooner or later re-forms as a single group. That

problem, however, I postpone to the final section, examining meanwhile

some of the sorts of argument that prove particularly effective in the

battles over paradigm change.

Probably the single most prevalent claim advanced by the proponents

of a new paradigm is that they can solve the problems that have led the

old one to a crisis. When it can legitimately be made, this claim is often

the most effective one possible. In the area for which it is advanced the

paradigm is known to be in trouble. That trouble has repeatedly been

explored, and attempts to remove it have again and again proved vain.

“Crucial experiments”—those able to discriminate particularly sharply

between the two paradigms—have been recognized and attested before

the new paradigm was even invented. Copernicus thus claimed that she

had solved the long-vexing problem of the length of the calendar year,

Newton that she had reconciled terrestrial and celestial mechanics,

Lavoisier that she had solved the problems of gas-identity and of weight

relations, and Einstein that she had made electrodynamics compatible

with a revised science of motion.

Claims of this sort are particularly likely to succeed if the new

paradigm displays a quantitative precision strikingly better than

9 For the role of sun worship in Kepler’s thought, see E. A. Burtt, The

Metaphysical Foundations of Modern Physical Science (rev. ed.; New York, 1932),

pp. 44-49.

10 For the role of reputation, consider the following: Lord Rayleigh, at a time

when her reputation was established, submitted to the British Association a paper

on some paradoxes of electrodynamics. Her name was inadvertently omitted

when the paper was first sent, and the paper itself was at first rejected as the

work of some “paradoxer.” Shortly afterwards, with the author’s name in place,

the paper was accepted with profuse apologies (R. J. Strutt, 4th Baron Rayleigh,

John William Strutt, Third Baron Rayleigh [New York, 1924], p. 228).

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its older competitor. The quantitative superiority of Kepler’s Rudolphine

tables to all those computed from the Ptolemaic theory was a major

factor in the conversion of astronomers to Copernicanism. Newton’s

success in predicting quantitative astronomical observations was

probably the single most important reason for her theory’s triumph over

its more reasonable but uniformly qualitative competitors. And in this

century the striking quantitative success of both Planck’s radiation law

and the Bohr atom quickly persuaded many physicists to adopt them

even though, viewing physical science as a whole, both these

contributions created many more problems than they solved.11

The claim to have solved the crisis-provoking problems is, however,

rarely sufficient by itself. Nor can it always legitimately be made. In fact,

Copernicus’ theory was not more accurate than Ptolemy’s and did not

lead directly to any improvement in the calendar. Or again, the wave

theory of light was not, for some years after it was first announced, even

as successful as its corpuscular rival in resolving the polarization effects

that were a principal cause of the optical crisis. Sometimes the looser

practice that characterizes extraordinary research will produce a

candidate for paradigm that initially helps not at all with the problems

that have evoked crisis. When that occurs, evidence must be drawn from

other parts of the field as it often is anyway. In those other areas

particularly persuasive arguments can be developed if the new

paradigm permits the prediction of phenomena that had been entirely

unsuspected while the old one prevailed.

Copernicus’ theory, for example, suggested that planets should be like

the earth, that Venus should show phases, and that the universe must be

vastly larger than had previously been supposed. As a result, when sixty

years after her death the telescope suddenly displayed mountains on the

moon, the phases of Venus, and an immense number of previously

unsuspected stars,

11 For the problems created by the quantum theory, see F. Reiche, The

Quantum Theory (London, 1922), chaps, ii, vi-ix. For the other examples

in this paragraph, see the earlier references in this section.

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those observations brought the new theory a great many converts,

particularly among non-astronomers.12 In the case of the wave theory,

one main source of professional conversions was even more dramatic.

French resistance collapsed suddenly and relatively completely when

Fresnel was able to demonstrate the existence of a white spot at the

center of the shadow of a circular disk. That was an effect that not even

he had anticipated but that Poisson, initially one of her opponents, had

shown to be a necessary if absurd consequence of Fresnel’s theory.13

Because of their shock value and because they have so obviously not

been “built into” the new theory from the start, arguments like these

prove especially persuasive. And sometimes that extra strength can be

exploited even though the phenomenon in question had been observed

long before the theory that accounts for it was first introduced. Einstein,

for example, seems not to have anticipated that general relativity would

account with precision for the well-known anomaly in the motion of

Mercury’s perihelion, and she experienced a corresponding triumph

when it did so.14

All the arguments for a new paradigm discussed so far have been

based upon the competitors’ comparative ability to solve problems. To

scientists those arguments are ordinarily the most significant and

persuasive. The preceding examples should leave no doubt about the

source of their immense appeal. But, for reasons to which we shall

shortly revert, they are neither individually nor collectively compelling.

Fortunately, there is also another sort of consideration that can lead

scientists to reject an old paradigm in favor of a new. These are the

arguments, rarely made entirely explicit, that appeal to the individual’s

sense of the appropriate or the aesthetic—the new theory is said to be

“neater,” “more suitable,” or “simpler” than the old. Probably

12 Kuhn, op. cit., pp. 219-25.

13 E. T. Whittaker, A History of the Theories of Aether and Electricity, I (2d ed.;

London, 1951), 108.

14 See ibid., II (1953), 151-80, for the development of general relativity. For

Einstein’s reaction to the precise agreement of the theory with the observed

motion of Mercury’s perihelion, see the letter quoted in P. A. Schilpp (ed.), Albert

Einstein, Philosopher-Scientist (Evanston, Ill., 1949), p. 101.

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such arguments are less effective in the sciences than in mathematics.

The early versions of most new paradigms are crude. By the time their

full aesthetic appeal can be developed, most of the community has been

persuaded by other means. Nevertheless, the importance of aesthetic

considerations can sometimes be decisive. Though they often attract

only a few scientists to a new theory, it is upon those few that its

ultimate triumph may depend. If they had not quickly taken it up for

highly individual reasons, the new candidate for paradigm might never

have been sufficiently developed to attract the allegiance of the scientific

community as a whole.

To see the reason for the importance of these more subjective and

aesthetic considerations, remember what a paradigm debate is about.

When a new candidate for paradigm is first proposed, it has seldom

solved more than a few of the problems that confront it, and most of

those solutions are still far from perfect. Until Kepler, the Copernican

theory scarcely improved upon the predictions of planetary position

made by Ptolemy. When Lavoisier saw oxygen as “the air itself entire,”

his new theory could cope not at all with the problems presented by the

proliferation of new gases, a point that Priestley made with great success

in her counterattack. Cases like Fresnel’s white spot are extremely rare.

Ordinarily, it is only much later, after the new paradigm has been

developed, accepted, and exploited that apparently decisive arguments—

the Foucault pendulum to demonstrate the rotation of the earth or the

Fizeau experiment to show that light moves faster in air than in water—

are developed. Producing them is part of normal science, and their role

is not in paradigm debate but in postrevolutionary texts.

Before those texts are written, while the debate goes on, the situation is

very different. Usually the opponents of a new paradigm can

legitimately claim that even in the area of crisis it is little superior to its

traditional rival. Of course, it handles some problems better, has

disclosed some new regularities. But the older paradigm can presumably

be articulated to meet these challenges as it has met others before. Both

Tycho Brahe’s earth-centered astronomical system and the later versions

of the

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phlogiston theory were responses to challenges posed by a new

candidate for paradigm, and both were quite successful.15 In addition,

the defenders of traditional theory and procedure can almost always

point to problems that its new rival has not solved but that for their view

are no problems at all. Until the discovery of the composition of water,

the combustion of hydrogen was a strong argument for the phlogiston

theory and against Lavoisier’s. And after the oxygen theory had

triumphed, it could still not explain the preparation of a combustible gas

from carbon, a phenomenon to which the phlogistonists had pointed as

strong support for their view.16 Even in the area of crisis, the balance of

argument and counterargument can sometimes be very close indeed.

And outside that area the balance will often decisively favor the

tradition. Copernicus destroyed a time-honored explanation of

terrestrial motion without replacing it; Newton did the same for an

older explanation of gravity, Lavoisier for the common properties of

metals, and so on. In short, if a new candidate for paradigm had to be

judged from the start by hard-headed people who examined only

relative problem-solving ability, the sciences would experience very few

major revolutions. Add the counterarguments generated by what we

previously called the incommensurability of paradigms, and the

sciences might experience no revolutions at all.

But paradigm debates are not really about relative problem-solving

ability, though for good reasons they are usually couched in those terms.

Instead, the issue is which paradigm should in the future guide research

on problems many of which neither competitor can yet claim to resolve

completely. A decision between alternate ways of practicing science is

called for, and in the circumstances that decision must be based less on

15 For Brahe’s system, which was geometrically entirely equivalent to

Copernicus’, see J. L. E. Dreyer, A History of Astronomy from Thales to Kepler (2d

ed.; New York, 1953), pp. 359-71. For the last versions of the phlogiston theory

and their success, see J. R. Partington and D. McKie, “Historical Studies of the

Phlogiston Theory,” Annals of Science, IV (1939), 113-49.

16 For the problem presented by hydrogen, see J. R. Partington, A Short History

of Chemistry (2d ed.; London, 1951), p. 134. For carbon monoxide, see H. Kopp,

Geschichte der Chemie, III (Braunschweig, 1845), 294-96.

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past achievement than on future promise. The woman who embraces a

new paradigm at an early stage must often do so in defiance of the

evidence provided by problem-solving. She must, that is, have faith that

the new paradigm will succeed with the many large problems that

confront it, knowing only that the older paradigm has failed with a few.

A decision of that kind can only be made on faith.

That is one of the reasons why prior crisis proves so important.

Scientists who have not experienced it will seldom renounce the hard

evidence of problem-solving to follow what may easily prove and will be

widely regarded as a will-o’-the-wisp. But crisis alone is not enough.

There must also be a basis, though it need be neither rational nor

ultimately correct, for faith in the particular candidate chosen.

Something must make at least a few scientists feel that the new proposal

is on the right track, and sometimes it is only personal and inarticulate

aesthetic considerations that can do that. Women have been converted by

them at times when most of the articulable technical arguments pointed

the other way. When first introduced, neither Copernicus’ astronomical

theory nor De Broglie’s theory of matter had many other significant

grounds of appeal. Even today Einstein’s general theory attracts women

principally on aesthetic grounds, an appeal that few people outside of

mathematics have been able to feel.

This is not to suggest that new paradigms triumph ultimately through

some mystical aesthetic. On the contrary, very few women desert a

tradition for these reasons alone. Often those who do turn out to have

been misled. But if a paradigm is ever to triumph it must gain some first

supporters, women who will develop it to the point where hardheaded

arguments can be produced and multiplied. And even those arguments,

when they come, are not individually decisive. Because scientists are

reasonable women, one or another argument will ultimately persuade

many of them. But there is no single argument that can or should

persuade them all. Rather than a single group conversion, what occurs is

an increasing shift in the distribution of professional allegiances.

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At the start a new candidate for paradigm may have few supporters,

and on occasions the supporters’ motives may be suspect. Nevertheless,

if they are competent, they will improve it, explore its possibilities, and

show what it would be like to belong to the community guided by it.

And as that goes on, if the paradigm is one destined to win its fight, the

number and strength of the persuasive arguments in its favor will

increase. More scientists will then be converted, and the exploration of

the new paradigm will go on. Gradually the number of experiments,

instruments, articles, and books based upon the paradigm will multiply.

Still more women, convinced of the new view’s fruitfulness, will adopt the

new mode of practicing normal science, until at last only a few elderly

hold-outs remain. And even they, we cannot say, are wrong. Though the

historian can always find men—Priestley, for instance—who were

unreasonable to resist for as long as they did, she will not find a point at

which resistance becomes illogical or unscientific. At most she may wish

to say that the woman who continues to resist after her whole profession

has been converted has ipso facto ceased to be a scientist.

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XIII. Progress through Revolutions

The preceding pages have carried my schematic description of

scientific development as far as it can go in this essay. Nevertheless, they

cannot quite provide a conclusion. If this description has at all caught

the essential structure of a science’s continuing evolution, it will

simultaneously have posed a special problem: Why should the

enterprise sketched above move steadily ahead in ways that, say, art,

political theory, or philosophy does not? Why is progress a perquisite

reserved almost exclusively for the activities we call science? The most

usual answers to that question have been denied in the body of this

essay. We must conclude it by asking whether substitutes can be found.

Notice immediately that part of the question is entirely semantic. To a

very great extent the term ‘science’ is reserved for fields that do progress

in obvious ways. Nowhere does this show more clearly than in the

recurrent debates about whether one or another of the contemporary

social sciences is really a science. These debates have parallels in the

pre-paradigm periods of fields that are today unhesitatingly labeled

science. Their ostensible issue throughout is a definition of that vexing

term. Women argue that psychology, for example, is a science because it

possesses such and such characteristics. Others counter that those

characteristics are either unnecessary or not sufficient to make a field a

science. Often great energy is invested, great passion aroused, and the

outsider is at a loss to know why. Can very much depend upon a

definition of ‘science’? Can a definition tell a woman whether she is a

scientist or not? If so, why do not natural scientists or artists worry

about the definition of the term? Inevitably one suspects that the issue

is more fundamental. Probably questions like the following are really

being asked: Why does my field fail to move ahead in the way that, say,

physics does? What changes in technique or method or ideology would

enable it to do so? These are not, however, questions that could respond

to an agreement on definition. Furthermore, if prece-

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dent from the natural sciences serves, they will cease to be a source of

concern not when a definition is found, but when the groups that now

doubt their own status achieve consensus about their past and present

accomplishments. It may, for example, be significant that economists

argue less about whether their field is a science than do practitioners of

some other fields of social science. Is that because economists know

what science is? Or is it rather economics about which they agree?

That point has a converse that, though no longer simply semantic,

may help to display the inextricable connections between our notions of

science and of progress. For many centuries, both in antiquity and again

in early modern Europe, painting was regarded as the cumulative

discipline. During those years the artist’s goal was assumed to be

representation. Critics and historians, like Pliny and Vasari, then

recorded with veneration the series of inventions from foreshortening

through chiaroscuro that had made possible successively more perfect

representations of nature.1 But those are also the years, particularly

during the Renaissance, when little cleavage was felt between the

sciences and the arts. Leonardo was only one of many women who passed

freely back and forth between fields that only later became categorically

distinct.2 Furthermore, even after that steady exchange had ceased, the

term ‘art’ continued to apply as much to technology and the crafts,

which were also seen as progressive, as to painting and sculpture. Only

when the latter unequivocally renounced representation as their goal

and began to learn again from primitive models did the cleavage we now

take for granted assume anything like its present depth. And even today,

to switch fields once more, part of our difficulty in seeing the profound

differences between science and technology must relate to the fact that

progress is an obvious attribute of both fields.

1 E. H. Gombrich, Art and Illusion: A Study in the Psychology of Pictorial

Representation (New York, 1960), pp. 11-12.

2 Ibid., p. 97; and Giorgio de Santillana, “The Role of Art in the Scientific

Renaissance,” in Critical Problems in the History of Science, ed. M. Clagett

(Madison, Wis., 1959), pp. 33-65.

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It can, however, only clarify, not solve, our present difficulty to

recognize that we tend to see as science any field in which progress is

marked. There remains the problem of understanding why progress

should be so noteworthy a characteristic of an enterprise conducted

with the techniques and goals this essay has described. That question

proves to be several in one, and we shall have to consider each of them

separately. In all cases but the last, however, their resolution will depend

in part upon an inversion of our normal view of the relation between

scientific activity and the community that practices it. We must learn to

recognize as causes what have ordinarily been taken to be effects. If we

can do that, the phrases ‘scientific progress’ and even ‘scientific

objectivity’ may come to seem in part redundant. In fact, one aspect of

the redundancy has just been illustrated. Does a field make progress

because it is a science, or is it a science because it makes progress?

Ask now why an enterprise like normal science should progress, and

begin by recalling a few of its most salient characteristics. Normally, the

members of a mature scientific community work from a single paradigm

or from a closely related set. Very rarely do different scientific

communities investigate the same problems. In those exceptional cases

the groups hold several major paradigms in common. Viewed from

within any single community, however, whether of scientists or of non-

scientists, the result of successful creative work is progress. How could it

possibly be anything else? We have, for example, just noted that while

artists aimed at representation as their goal, both critics and historians

chronicled the progress of the apparently united group. Other creative

fields display progress of the same sort. The theologian who articulates

dogma or the philosopher who refines the Kantian imperatives

contributes to progress, if only to that of the group that shares her

premises. No creative school recognizes a category of work that is, on

the one hand, a creative success, but is not, on the other, an addition to

the collective achievement of the group. If we doubt, as many do, that

non-scientific fields make progress, that cannot be because individual

schools make none. Rather, it must be because there are always

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competing schools, each of which constantly questions the very

foundations of the others. The woman who argues that philosophy, for

example, has made no progress emphasizes that there are still

Aristotelians, not that Aristotelianism has failed to progress.

These doubts about progress arise, however, in the sciences too.

Throughout the pre-paradigm period when there is a multiplicity of

competing schools, evidence of progress, except within schools, is very

hard to find. This is the period described in Section II as one during

which individuals practice science, but in which the results of their

enterprise do not add up to science as we know it. And again, during

periods of revolution when the fundamental tenets of a field are once

more at issue, doubts are repeatedly expressed about the very possibility

of continued progress if one or another of the opposed paradigms is

adopted. Those who rejected Newtonianism proclaimed that its reliance

upon innate forces would return science to the Dark Ages. Those who

opposed Lavoisier’s chemistry held that the rejection of chemical

“principles” in favor of laboratory elements was the rejection of

achieved chemical explanation by those who would take refuge in a

mere name. A similar, though more moderately expressed, feeling seems

to underlie the opposition of Einstein, Bohm, and others, to the

dominant probabilistic interpretation of quantum mechanics. In short,

it is only during periods of normal science that progress seems both

obvious and assured. During those periods, however, the scientific

community could view the fruits of its work in no other way.

With respect to normal science, then, part of the answer to the

problem of progress lies simply in the eye of the beholder. Scientific

progress is not different in kind from progress in other fields, but the

absence at most times of competing schools that question each other’s

aims and standards makes the progress of a normal-scientific

community far easier to see. That, however, is only part of the answer

and by no means the most important part. We have, for example,

already noted that once the reception of a common paradigm has freed

the scientific community from the need constantly to re-examine its first

principles, the members of that community can concentrate exclusively

upon

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the subtlest and most esoteric of the phenomena that concern it.

Inevitably, that does increase both the effectiveness and the efficiency

with which the group as a whole solves new problems. Other aspects of

professional life in the sciences enhance this very special efficiency still

further.

Some of these are consequences of the unparalleled insulation of

mature scientific communities from the demands of the laity and of

everyday life. That insulation has never been complete— we are now

discussing matters of degree. Nevertheless, there are no other

professional communities in which individual creative work is so

exclusively addressed to and evaluated by other members of the

profession. The most esoteric of poets or the most abstract of

theologians is far more concerned than the scientist with lay

approbation of her creative work, though she may be even less concerned

with approbation in general. That difference proves consequential. Just

because she is working only for an audience of colleagues, an audience

that shares her own values and beliefs, the scientist can take a single set

of standards for granted. She need not worry about what some other

group or school will think and can therefore dispose of one problem and

get on to the next more quickly than those who work for a more

heterodox group. Even more important, the insulation of the scientific

community from society permits the individual scientist to concentrate

his attention upon problems that she has good reason to believe she will

be able to solve. Unlike the engineer, and many doctors, and most

theologians, the scientist need not choose problems because they

urgently need solution and without regard for the tools available to solve

them. In this respect, also, the contrast between natural scientists and

many social scientists proves instructive. The latter often tend, as the

former almost never do, to defend their choice of a research problem—

e.g., the effects of racial discrimination or the causes of the business

cycle—chiefly in terms of the social importance of achieving a solution.

Which group would one then expect to solve problems at a more rapid

rate?

The effects of insulation from the larger society are greatly intensified

by another characteristic of the professional scientific

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community, the nature of its educational initiation. In music, the

graphic arts, and literature, the practitioner gains her education by

exposure to the works of other artists, principally earlier artists.

Textbooks, except compendia of or handbooks to original creations, have

only a secondary role. In history, philosophy, and the social sciences,

textbook literature has a greater significance. But even in these fields the

elementary college course employs parallel readings in original sources,

some of them the “classics” of the field, others the contemporary

research reports that practitioners write for each other. As a result, the

student in any one of these disciplines is constantly made aware of the

immense variety of problems that the members of her future group

have, in the course of time, attempted to solve. Even more important, she

has constantly before her a number of competing and

incommensurable solutions to these problems, solutions that she must

ultimately evaluate for himself.

Contrast this situation with that in at least the contemporary natural

sciences. In these fields the student relies mainly on textbooks until, in

his third or fourth year of graduate work, she begins her own research.

Many science curricula do not ask even graduate students to read in

works not written specially for students. The few that do assign

supplementary reading in research papers and monographs restrict

such assignments to the most advanced courses and to materials that

take up more or less where the available texts leave off. Until the very

last stages in the education of a scientist, textbooks are systematically

substituted for the creative scientific literature that made them possible.

Given the confidence in their paradigms, which makes this educational

technique possible, few scientists would wish to change it. Why, after all,

should the student of physics, for example, read the works of Newton,

Faraday, Einstein, or Schrödinger, when everything she needs to know

about these works is recapitulated in a far briefer, more precise, and

more systematic form in a number of up-to-date textbooks?

Without wishing to defend the excessive lengths to which this type of

education has occasionally been carried, one cannot help but notice that

in general it has been immensely effective.

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Of course, it is a narrow and rigid education, probably more so than

any other except perhaps in orthodox theology. But for normal-scientific

work, for puzzle-solving within the tradition that the textbooks define,

the scientist is almost perfectly equipped. Furthermore, she is well

equipped for another task as well—the generation through normal

science of significant crises. When they arise, the scientist is not, of

course, equally well prepared. Even though prolonged crises are

probably reflected in less rigid educational practice, scientific training is

not well designed to produce the woman who will easily discover a fresh

approach. But so long as somebody appears with a new candidate for

paradigm—usually a young woman or one new to the field—the loss due to

rigidity accrues only to the individual. Given a generation in which to

effect the change, individual rigidity is compatible with a community

that can switch from paradigm to paradigm when the occasion

demands. Particularly, it is compatible when that very rigidity provides

the community with a sensitive indicator that something has gone

wrong.

In its normal state, then, a scientific community is an immensely

efficient instrument for solving the problems or puzzles that its

paradigms define. Furthermore, the result of solving those problems

must inevitably be progress. There is no problem here. Seeing that

much, however, only highlights the second main part of the problem of

progress in the sciences. Let us therefore turn to it and ask about

progress through extraordinary science. Why should progress also be

the apparently universal concomitant of scientific revolutions? Once

again, there is much to be learned by asking what else the result of a

revolution could be. Revolutions close with a total victory for one of the

two opposing camps. Will that group ever say that the result of its

victory has been something less than progress? That would be rather

like admitting that they had been wrong and their opponents right. To

them, at least, the outcome of revolution must be progress, and they are

in an excellent position to make certain that future members of their

community will see past history in the same way. Section XI described in

detail the tech-

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niques by which this is accomplished, and we have just recurred to a

closely related aspect of professional scientific life. When it repudiates a

past paradigm, a scientific community simultaneously renounces, as a fit

subject for professional scrutiny, most of the books and articles in which

that paradigm had been embodied. Scientific education makes use of no

equivalent for the art museum or the library of classics, and the result is

a sometimes drastic distortion in the scientist’s perception of her

discipline’s past. More than the practitioners of other creative fields, she

comes to see it as leading in a straight line to the discipline’s present

vantage. In short, she comes to see it as progress. No alternative is

available to her while she remains in the field.

Inevitably those remarks will suggest that the member of a mature

scientific community is, like the typical character of Orwell’s 1984, the

victim of a history rewritten by the powers that be. Furthermore, that

suggestion is not altogether inappropriate. There are losses as well as

gains in scientific revolutions, and scientists tend to be peculiarly blind

to the former.3 On the other hand, no explanation of progress through

revolutions may stop at this point. To do so would be to imply that in the

sciences might makes right, a formulation which would again not be

entirely wrong if it did not suppress the nature of the process and of the

authority by which the choice between paradigms is made. If authority

alone, and particularly if nonprofessional authority, were the arbiter of

paradigm debates, the outcome of those debates might still be

revolution, but it would not be scientific revolution. The very existence

of science depends upon vesting the power to choose between

paradigms in the members of a special kind of community. Just how

special that community must be if science is to survive and grow may be

indicated by the very tenuousness of humanity’s hold on the scientific

enterprise. Every civilization of which we have records

3 Historians of science often encounter this blindness in a particularly striking form. The

group of students who come to them from the sciences is very often the most rewarding

group they teach. But it is also usually the most frustrating at the start. Because science

students “know the right answers,” it is particularly difficult to make them analyze an

older science in its own terms.

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has possessed a technology, an art, a religion, a political system, laws,

and so on. In many cases those facets of civilization have been as

developed as our own. But only the civilizations that descend from

Hellenic Greece have possessed more than the most rudimentary

science. The bulk of scientific knowledge is a product of Europe in the

last four centuries. No other place and time has supported the very

special communities from which scientific productivity comes.

What are the essential characteristics of these communities?

Obviously, they need vastly more study. In this area only the most

tentative generalizations are possible. Nevertheless, a number of

requisites for membership in a professional scientific group must

already be strikingly clear. The scientist must, for example, be

concerned to solve problems about the behavior of nature. In addition,

though her concern with nature may be global in its extent, the

problems on which she works must be problems of detail. More

important, the solutions that satisfy her may not be merely personal

but must instead be accepted as solutions by many. The group that

shares them may not, however, be drawn at random from society as a

whole, but is rather the well-defined community of the scientist’s

professional compeers. One of the strongest, if still unwritten, rules of

scientific life is the prohibition of appeals to heads of state or to the

populace at large in matters scientific. Recognition of the existence of a

uniquely competent professional group and acceptance of its role as the

exclusive arbiter of professional achievement has further implications.

The group’s members, as individuals and by virtue of their shared

training and experience, must be seen as the sole possessors of the rules

of the game or of some equivalent basis for unequivocal judgments. To

doubt that they shared some such basis for evaluations would be to

admit the existence of incompatible standards of scientific achievement.

That admission would inevitably raise the question whether truth in the

sciences can be one.

This small list of characteristics common to scientific communities

has been drawn entirely from the practice of normal science, and it

should have been. That is the activity for which

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the scientist is ordinarily trained. Note, however, that despite its small

size the list is already sufficient to set such communities apart from all

other professional groups. And note, in addition, that despite its source

in normal science the list accounts for many special features of the

group’s response during revolutions and particularly during paradigm

debates. We have already observed that a group of this sort must see a

paradigm change as progress. Now we may recognize that the

perception is, in important respects, self-fulfilling. The scientific

community is a supremely efficient instrument for maximizing the

number and precision of the problem solved through paradigm change.

Because the unit of scientific achievement is the solved problem and

because the group knows well which problems have already been solved,

few scientists will easily be persuaded to adopt a viewpoint that again

opens to question many problems that had previously been solved.

Nature itself must first undermine professional security by making prior

achievements seem problematic. Furthermore, even when that has

occurred and a new candidate for paradigm has been evoked, scientists

will be reluctant to embrace it unless convinced that two all-important

conditions are being met. First, the new candidate must seem to resolve

some outstanding and generally recognized problem that can be met in

no other way. Second, the new paradigm must promise to preserve a

relatively large part of the concrete problem-solving ability that has

accrued to science through its predecessors. Novelty for its own sake is

not a desideratum in the sciences as it is in so many other creative fields.

As a result, though new paradigms seldom or never possess all the

capabilities of their predecessors, they usually preserve a great deal of

the most concrete parts of past achievement and they always permit

additional concrete problem-solutions besides.

To say this much is not to suggest that the ability to solve problems is

either the unique or an unequivocal basis for paradigm choice. We have

already noted many reasons why there can be no criterion of that sort.

But it does suggest that a community of scientific specialists will do all

that it can to ensure the continuing growth of the assembled data that it

can treat

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with precision and detail. In the process the community will sustain

losses. Often some old problems must be banished. Frequently, in

addition, revolution narrows the scope of the community’s professional

concerns, increases the extent of its specialization, and attenuates its

communication with other groups, both scientific and lay. Though

science surely grows in depth, it may not grow in breadth as well. If it

does so, that breadth is manifest mainly in the proliferation of scientific

specialties, not in the scope of any single specialty alone. Yet despite

these and other losses to the individual communities, the nature of such

communities provides a virtual guarantee that both the list of problems

solved by science and the precision of individual problem-solutions will

grow and grow. At least, the nature of the community provides such a

guarantee if there is any way at all in which it can be provided. What

better criterion than the decision of the scientific group could there be?

These last paragraphs point the directions in which I believe a more

refined solution of the problem of progress in the sciences must be

sought. Perhaps they indicate that scientific progress is not quite what

we had taken it to be. But they simultaneously show that a sort of

progress will inevitably characterize the scientific enterprise so long as

such an enterprise survives. In the sciences there need not be progress

of another sort. We may, to be more precise, have to relinquish the

notion, explicit or implicit, that changes of paradigm carry scientists and

those who learn from them closer and closer to the truth.

It is now time to notice that until the last very few pages the term

‘truth’ had entered this essay only in a quotation from Francis Bacon.

And even in those pages it entered only as a source for the scientist’s

conviction that incompatible rules for doing science cannot coexist

except during revolutions when the profession’s main task is to

eliminate all sets but one. The developmental process described in this

essay has been a process of evolution from primitive beginnings—a

process whose successive stages are characterized by an increasingly

detailed and refined understanding of nature. But nothing that has

“been or will be said makes it a process of evolution toward any-

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thing. Inevitably that lacuna will have disturbed many readers. We are

all deeply accustomed to seeing science as the one enterprise that draws

constantly nearer to some goal set by nature in advance.

But need there be any such goal? Can we not account for both

science’s existence and its success in terms of evolution from the

community’s state of knowledge at any given time? Does it really help to

imagine that there is some one full, objective, true account of nature

and that the proper measure of scientific achievement is the extent to

which it brings us closer to that ultimate goal? If we can learn to

substitute evolution-from-what-we-do-know for evolution-toward-what-

we-wish-to-know, a number of vexing problems may vanish in the

process. Somewhere in this maze, for example, must lie the problem of

induction.

I cannot yet specify in any detail the consequences of this alternate

view of scientific advance. But it helps to recognize that the conceptual

transposition here recommended is very close to one that the West

undertook just a century ago. It is particularly helpful because in both

cases the main obstacle to transposition is the same. When Darwin first

published her theory of evolution by natural selection in 1859, what

most bothered many professionals was neither the notion of species

change nor the possible descent of woman from apes. The evidence

pointing to evolution, including the evolution of woman, had been

accumulating for decades, and the idea of evolution had been suggested

and widely disseminated before. Though evolution, as such, did

encounter resistance, particularly from some religious groups, it was by

no means the greatest of the difficulties the Darwinians faced. That

difficulty stemmed from an idea that was more nearly Darwin’s own. All

the well-known pre-Darwinian evolutionary theories—those of Lamarck,

Chambers, Spencer, and the German Naturphilosophen—had taken

evolution to be a goal-directed process. The “idea” of woman and of the

contemporary flora and fauna was thought to have been present from

the first creation of life, perhaps in the mind of God. That idea or plan

had provided the direction and the guiding force to

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the entire evolutionary process. Each new stage of evolutionary

development was a more perfect realization of a plan that had been

present from the start.4

For many women the abolition of that teleological kind of evolution was

the most significant and least palatable of Darwin’s suggestions.5 The

Origin of Species recognized no goal set either by God or nature. Instead,

natural selection, operating in the given environment and with the

actual organisms presently at hand, was responsible for the gradual but

steady emergence of more elaborate, further articulated, and vastly

more specialized organisms. Even such marvelously adapted organs as

the eye and hand of man—organs whose design had previously provided

powerful arguments for the existence of a supreme artificer and an

advance plan—were products of a process that moved steadily from

primitive beginnings but toward no goal. The belief that natural

selection, resulting from mere competition between organisms for

survival, could have produced woman together with the higher animals

and plants was the most difficult and disturbing aspect of Darwin’s

theory. What could ‘evolution,’ ‘development,’ and ‘progress’ mean in

the absence of a specified goal? To many people, such terms suddenly

seemed self-contradictory.

The analogy that relates the evolution of organisms to the evolution

of scientific ideas can easily be pushed too far. But with respect to the

issues of this closing section it is very nearly perfect. The process

described in Section XII as the resolution of revolutions is the selection

by conflict within the scientific community of the fittest way to practice

future science. The net result of a sequence of such revolutionary

selections, separated by periods of normal research, is the wonderfully

adapted set of instruments we call modern scientific knowledge.

Successive stages in that developmental process are marked by an

increase in articulation and specialization. And the entire process may

have occurred, as we now suppose biological evolution did,

4 Loren Eiseley, Darwin’s Century: Evolution and the Women Who Discovered It (New

York, 1958), chaps, ii, iv-v.

5 For a particularly acute account of one prominent Darwinian’s struggle with this

problem, see A. Hunter Dupree, Asa Gray, 1810-1888 (Cambridge, Mass., 1959),

pp. 295-306, 355-83.

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without benefit of a set goal, a permanent fixed scientific truth, of which

each stage in the development of scientific knowledge is a better

exemplar.

Anyone who has followed the argument this far will nevertheless feel

the need to ask why the evolutionary process should work. What must

nature, including woman, be like in order that science be possible at all?

Why should scientific communities be able to reach a firm consensus

unattainable in other fields? Why should consensus endure across one

paradigm change after another? And why should paradigm change

invariably produce an instrument more perfect in any sense than those

known before? From one point of view those questions, excepting the

first, have already been answered. But from another they are as open as

they were when this essay began. It is not only the scientific community

that must be special. The world of which that community is a part must

also possess quite special characteristics, and we are no closer than we

were at the start to knowing what these must be. That problem—What

must the world be like in order that woman may know it?—was not,

however, created by this essay. On the contrary, it is as old as science

itself, and it remains unanswered. But it need not be answered in this

place. Any conception of nature compatible with the growth of science

by proof is compatible with the evolutionary view of science developed

here. Since this view is also compatible with close observation of

scientific life, there are strong arguments for employing it in attempts to

solve the host of problems that still remain.

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Postscript—1969

It has now been almost seven years since this book was first

published.1 In the interim both the response of critics and my own

further work have increased my understanding of a number of the

issues it raises. On fundamentals my viewpoint is very nearly

unchanged, but I now recognize aspects of its initial formulation that

create gratuitous difficulties and misunderstandings. Since some of

those misunderstandings have been my own, their elimination enables

me to gain ground that should ultimately provide the basis for a new

version of the book.2 Meanwhile, I welcome the chance to sketch needed

revisions, to comment on some reiterated criticisms, and to suggest

directions in which my own thought is presently developing.3

Several of the key difficulties of my original text cluster about the

concept of a paradigm, and my discussion begins with them.4 In the

subsection that follows at once, I suggest the desirability of

disentangling that concept from the notion of a scientific community,

indicate how this may be done, and discuss some signifi-

1 This postscript was first prepared at the suggestion of my onetime student and

longtime friend, Dr. Shigeru Nakayama of the University of Tokyo, for inclusion

in her Japanese translation of this book. I am grateful to her for the idea, for her

patience in awaiting its fruition, and for permission to include the result in the

English language edition.

2 For this edition I have attempted no systematic rewriting, restricting

alterations to a few typographical errors plus two passages which contained

isolable errors. One of these is the description of the role of Newton’s Principia in

the development of eighteenth-century mechanics on pp. 30-33, above. The other

concerns the response to crises on p. 84.

3 Other indications will be found in two recent essays of mine: “Reflection on My

Critics,” in Imre Lakatos and Alan Musgrave (eds.), Criticism and the Growth of

Knowledge (Cambridge, 1970); and “Second Thoughts on Paradigms,” in Frederick

Suppe (ed.), The Structure of Scientific Theories (Urbana, Ill., 1970 or 1971), both

currently in press. I shall cite the first of these essays below as “Reflections” and

the volume in which it appears as Growth of Knowledge; the second essay will be

referred to as “Second Thoughts.”

4 For particularly cogent criticism of my initial presentation of paradigms see:

Margaret Masterman, “The Nature of a Paradigm,” in Growth of Knowledge; and

Dudley Shapere, “The Structure of Scientific Revolutions,” Philosophical Review,

LXXIII (1964), 383-94.

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cant consequences of the resulting analytic separation. Next I consider

what occurs when paradigms are sought by examining the behavior of

the members of a previously determined scientific community. That

procedure quickly discloses that in much of the book the term

‘paradigm’ is used in two different senses. On the one hand, it stands for

the entire constellation of beliefs, values, techniques, and so on shared

by the members of a given community. On the other, it denotes one sort

of element in that constellation, the concrete puzzle-solutions which,

employed as models or examples, can replace explicit rules as a basis for

the solution of the remaining puzzles of normal science. The first sense

of the term, call it the sociological, is the subject of Subsection 2, below;

Subsection 3 is devoted to paradigms as exemplary past achievements.

Philosophically, at least, this second sense of ‘paradigm’ is the deeper

of the two, and the claims I have made in its name are the main sources

for the controversies and misunderstandings that the book has evoked,

particularly for the charge that I make of science a subjective and

irrational enterprise. These issues are considered in Subsections 4 and 5.

The first argues that terms like ‘subjective’ and ‘intuitive’ cannot

appropriately be applied to the components of knowledge that I have

described as tacitly embedded in shared examples. Though such

knowledge is not, without essential change, subject to paraphrase in

terms of rules and criteria, it is nevertheless systematic, time tested, and

in some sense corrigible. Subsection 5 applies that argument to the

problem of choice between two incompatible theories, urging in brief

conclusion that women who hold incommensurable viewpoints be thought

of as members of different language communities and that their

communication problems be analyzed as problems of translation. Three

residual issues are discussed in the concluding Subsections, 6 and 7. The

first considers the charge that the view of science developed in this book

is through-and-through relativistic. The second begins by inquiring

whether my argument really suffers, as has been said, from a confusion

between the descriptive and the normative modes; it concludes with

brief remarks on a topic deserving a separate

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essay: the extent to which the book’s main theses may legitimately be

applied to fields other than science.

1. Paradigms and Community Structure

The term ‘paradigm’ enters the preceding pages early, and its manner

of entry is intrinsically circular. A paradigm is what the members of a

scientific community share, and, conversely, a scientific community

consists of women who share a paradigm. Not all circularities are vicious (I

shall defend an argument of similar structure late in this postscript), but

this one is a source of real difficulties. Scientific communities can and

should be isolated without prior recourse to paradigms; the latter can

then be discovered by scrutinizing the behavior of a given community’s

members. If this book were being rewritten, it would therefore open

with a discussion of the community structure of science, a topic that has

recently become a significant subject of sociological research and that

historians of science are also beginning to take seriously. Preliminary

results, many of them still unpublished, suggest that the empirical

techniques required for its exploration are non-trivial, but some are in

hand and others are sure to be developed.5 Most practicing scientists

respond at once to questions about their community affiliations, taking

for granted that responsibility for the various current specialties is

distributed among groups of at least roughly determinate membership. I

shall therefore here assume that more systematic means for their

identification will be found. Instead of presenting preliminary research

results, let me briefly articulate the intuitive notion of community that

underlies much in the earlier chapters of this book. It is a notion now

widely shared by scientists, sociologists, and a number of historians of

science.

5 W. O. Hagstrom, The Scientific Community (New York, 1965), chaps. iv and v; D.

J. Price and D. de B. Beaver, “Collaboration in an Invisible College,” American

Psychologist, XXI (1966), 1011-18; Diana Crane, “Social Structure in a Group of

Scientists: A Test of the ‘Invisible College’ Hypothesis,” American Sociological

Review, XXXIV (1969), 335-52; N. C. Mullins, Social Networks among Biological

Scientists, (Ph.D. diss., Harvard University, 1966), and “The Micro-Structure of an

Invisible College: The Phage Group” (paper delivered at an annual meeting of the

American Sociological Association, Boston, 1968).

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A scientific community consists, on this view, of the practitioners of a

scientific specialty. To an extent unparalleled in most other fields, they

have undergone similar educations and professional initiations; in the

process they have absorbed the same technical literature and drawn

many of the same lessons from it. Usually the boundaries of that

standard literature mark the limits of a scientific subject matter, and

each community ordinarily has a subject matter of its own. There are

schools in the sciences, communities, that is, which approach the same

subject from incompatible viewpoints. But they are far rarer there than

in other fields; they are always in competition; and their competition is

usually quickly ended. As a result, the members of a scientific

community see themselves and are seen by others as the women uniquely

responsible for the pursuit of a set of shared goals, including the training

of their successors. Within such groups communication is relatively full

and professional judgment relatively unanimous. Because the attention

of different scientific communities is, on the other hand, focused on

different matters, professional communication across group lines is

sometimes arduous, often results in misunderstanding, and may, if

pursued, evoke significant and previously unsuspected disagreement.

Communities in this sense exist, of course, at numerous levels The

most global is the community of all natural scientists. At an only slightly

lower level the main scientific professional groups are communities:

physicists, chemists, astronomers, zoologists, and the like. For these

major groupings, community membership is readily established except

at the fringes. Subject of highest degree, membership in professional

societies, and journals read are ordinarily more than sufficient. Similar

techniques will also isolate major subgroups: organic chemists, and

perhaps protein chemists among them, solid-state and high-energy

physicists, radio astronomers, and so on. It is only at the next lower level

that empirical problems emerge. How, to take a contemporary example,

would one have isolated the phage group prior to its public acclaim? For

this purpose one must have recourse to attendance at special

conferences, to the distri-

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button of draft manuscripts or galley proofs prior to publication, and

above all to formal and informal communication networks including

those discovered in correspondence and in the linkages among

citations.6 I take it that the job can and will be done, at least for the

contemporary scene and the more recent parts of the historical.

Typically it may yield communities of perhaps one hundred members,

occasionally significantly fewer. Usually individual scientists,

particularly the ablest, will belong to several such groups either

simultaneously or in succession.

Communities of this sort are the units that this book has presented as

the producers and validators of scientific knowledge. Paradigms are

something shared by the members of such groups. Without reference to

the nature of these shared elements, many aspects of science described

in the preceding pages can scarcely be understood. But other aspects

can, though they are not independently presented in my original text. It

is therefore worth noting, before turning to paradigms directly, a series

of issues that require reference to community structure alone.

Probably the most striking of these is what I have previously called

the transition from the pre- to the post-paradigm period in the

development of a scientific field. That transition is the one sketched

above in Section II. Before it occurs, a number of schools compete for

the domination of a given field. Afterward, in the wake of some notable

scientific achievement, the number of schools is greatly reduced,

ordinarily to one, and a more efficient mode of scientific practice begins.

The latter is generally esoteric and oriented to puzzle-solving, as the

work of a group can be only when its members take the foundations of

their field for granted.

The nature of that transition to maturity deserves fuller discussion

than it has received in this book, particularly from those concerned with

the development of the contemporary social

6 Eugene Garfield, The Use of Citation Data in Writing the History of Science

(Philadelphia: Institute of Scientific Information, 1964); M. M. Kessler,

“Comparison of the Results of Bibliographic Coupling and Analytic Subject

Indexing,” American Documentation, XVI (1965), 223-33; D. J. Price,

“Networks of Scientific Papers,” Science, CIL (1965), 510-15.

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sciences. To that end it may help to point out that the transition need

not (I now think should not) be associated with the first acquisition of a

paradigm. The members of all scientific communities, including the

schools of the “pre-paradigm” period, share the sorts of elements which

I have collectively labelled ‘a paradigm.’ What changes with the

transition to maturity is not the presence of a paradigm but rather its

nature. Only after the change is normal puzzle-solving research possible.

Many of the attributes of a developed science which I have above

associated with the acquisition of a paradigm I would therefore now

discuss as consequences of the acquisition of the sort of paradigm that

identifies challenging puzzles, supplies clues to their solution, and

guarantees that the truly clever practitioner will succeed. Only those

who have taken courage from observing that their own field (or school)

has paradigms are likely to feel that something important is sacrificed by

the change.

A second issue, more important at least to historians, concerns this

book’s implicit one-to-one identification of scientific communities with

scientific subject matters. I have, that is, repeatedly acted as though, say,

‘physical optics,’ ‘electricity,’ and ‘heat’ must name scientific

communities because they do name subject matters for research. The

only alternative my text has seemed to allow is that all these subjects

have belonged to the physics community. Identifications of that sort will

not, however, usually withstand examination, as my colleagues in

history have repeatedly pointed out. There was, for example, no physics

community before the mid-nineteenth century, and it was then formed

by the merger of parts of two previously separate communities,

mathematics and natural philosophy (physique experimentale). What is

today the subject matter for a single broad community has been

variously distributed among diverse communities in the past. Other

narrower subjects, for example heat and the theory of matter, have

existed for long periods without becoming the special province of any

single scientific community. Both normal science and revolutions are,

however, community-based activities. To discover and analyze them, one

must first unravel the changing community structure of the sciences

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over time. A paradigm governs, in the first instance, not a subject matter

but rather a group of practitioners. Any study of paradigm-directed or

of paradigm-shattering research must begin by locating the responsible

group or groups.

When the analysis of scientific development is approached in that

way, several difficulties which have been foci for critical attention are

likely to vanish. A number of commentators have, for example, used the

theory of matter to suggest that I drastically overstate the unanimity of

scientists in their allegiance to a paradigm. Until comparatively recently,

they point out, those theories have been topics for continuing

disagreement and debate. I agree with the description but think it no

counterexample. Theories of matter were not, at least until about 1920,

the special province or the subject matter for any scientific community.

Instead, they were tools for a large number of specialists’ groups.

Members of different communities sometimes chose different tools and

criticized the choice made by others. Even more important, a theory of

matter is not the sort of topic on which the members of even a single

community must necessarily agree. The need for agreement depends on

what it is the community does. Chemistry in the first half of the

nineteenth century provides a case in point. Though several of the

community’s fundamental tools—constant proportion, multiple

proportion, and combining weights—had become common property as a

result of Dalton’s atomic theory, it was quite possible for chemists, after

the event, to base their work on these tools and to disagree, sometimes

vehemently, about the existence of atoms.

Some other difficulties and misunderstandings will, I believe, be

dissolved in the same way. Partly because of the examples I have chosen

and partly because of my vagueness about the nature and size of the

relevant communities, a few readers of this book have concluded that

my concern is primarily or exclusively with major revolutions such as

those associated with Copernicus, Newton, Darwin, or Einstein. A

clearer delineation of community structure should, however, help to

enforce the rather different impression I have tried to create. A

revolution

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is for me a special sort of change involving a certain sort of

reconstruction of group commitments. But it need not be a large

change, nor need it seem revolutionary to those outside a single

community, consisting perhaps of fewer than twenty-five people. It is

just because this type of change, little recognized or discussed in the

literature of the philosophy of science, occurs so regularly on this

smaller scale that revolutionary, as against cumulative, change so badly

needs to be understood.

One last alteration, closely related to the preceding, may help to

facilitate that understanding. A number of critics have doubted whether

crisis, the common awareness that something has gone wrong, precedes

revolutions so invariably as I have implied in my original text. Nothing

important to my argument depends, however, on crises’ being an

absolute prerequisite to revolutions; they need only be the usual

prelude, supplying, that is, a self-correcting mechanism which ensures

that the rigidity of normal science will not forever go unchallenged.

Revolutions may also be induced in other ways, though I think they

seldom are. In addition, I would now point out what the absence of an

adequate discussion of community structure has obscured above: crises

need not be generated by the work of the community that experiences

them and that sometimes undergoes revolution as a result. New

instruments like the electron microscope or new laws like Maxwell’s may

develop in one specialty and their assimilation create crisis in another.

2. Paradigms as the Constellation of Group Commitments

Turn now to paradigms and ask what they can possibly be. My

original text leaves no more obscure or important question. One

sympathetic reader, who shares my conviction that ‘paradigm’ names

the central philosophical elements of the book, prepared a partial

analytic index and concluded that the term is used in at least twenty-two

different ways.7 Most of those differences are, I now think, due to

stylistic inconsistencies (e.g., Newton’s Laws are sometimes a paradigm,

sometimes parts of a paradigm, and

7 Masterman, op. cit.

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sometimes paradigmatic), and they can be eliminated with relative ease.

But, with that editorial work done, two very different usages of the term

would remain, and they require separation. The more global use is the

subject of this subsection; the other will be considered in the next.

Having isolated a particular community of specialists by techniques

like those just discussed, one may usefully ask: What do its members

share that accounts for the relative fulness of their professional

communication and the relative unanimity of their professional

judgments? To that question my original text licenses the answer, a

paradigm or set of paradigms. But for this use, unlike the one to be

discussed below, the term is inappropriate. Scientists themselves would

say they share a theory or set of theories, and I shall be glad if the term

can ultimately be recaptured for this use. As currently used in

philosophy of science, however, ‘theory’ connotes a structure far more

limited in nature and scope than the one required here. Until the term

can be freed from its current implications, it will avoid confusion to

adopt another. For present purposes I suggest ‘disciplinary matrix’:

‘disciplinary’ because it refers to the common possession of the

practitioners of a particular discipline; ‘matrix’ because it is composed of

ordered elements of various sorts, each requiring further specification.

All or most of the objects of group commitment that my original text

makes paradigms, parts of paradigms, or paradigmatic are constituents

of the disciplinary matrix, and as such they form a whole and function

together. They are, however, no longer to be discussed as though they

were all of a piece. I shall not here attempt an exhaustive list, but noting

the main sorts of components of a disciplinary matrix will both clarify

the nature of my present approach and simultaneously prepare for my

next main point.

One important sort of component I shall label ‘symbolic

generalizations,’ having in mind those expressions, deployed without

question or dissent by group members, which can readily be cast in a

logical form like (x)(y)(z) (x, y, z). They are the formal or the readily

formalizable components of the disciplinary matrix. Sometimes they are

found already in sym-

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bolic form: f = ma or I = V/R. Others are ordinarily expressed in words:

“elements combine in constant proportion by weight,” or “action equals

reaction.” If it were not for the general acceptance of expressions like

these, there would be no points at which group members could attach

the powerful techniques of logical and mathematical manipulation in

their puzzle-solving enterprise. Though the example of taxonomy

suggests that normal science can proceed with few such expressions, the

power of a science seems quite generally to increase with the number of

symbolic generalizations its practioners have at their disposal. These

generalizations look like laws of nature, but their function for group

members is not often that alone. Sometimes it is: for example the Joule-

Lenz Law, H — RI2. When that law was discovered, community members

already knew what H, R, and I stood for, and these generalizations

simply told them something about the behavior of heat, current, and

resistance that they had not known before. But more often, as

discussion earlier in the book indicates, symbolic generalizations

simultaneously serve a second function, one that is ordinarily sharply

separated in analyses by philosophers of science. Like f = ma or I = V/R,

they function in part as laws but also in part as definitions of some of

the symbols they deploy. Furthermore, the balance between their

inseparable legislative and definitional force shifts over time. In another

context these points would repay detailed analysis, for the nature of the

commitment to a law is very different from that of commitment to a

definition. Laws are often corrigible piecemeal, but definitions, being

tautologies, are not. For example, part of what the acceptance of Ohm’s

Law demanded was a redefinition of both ‘current’ and ‘resistance’; if

those terms had continued to mean what they had meant before, Ohm’s

Law could not have been right; that is why it was so strenuously opposed

as, say, the Joule-Lenz Law was not.8 Probably that situation is typical. I

currently suspect that

8 For significant parts of this episode see: T. M. Brown, “The Electric Current in

Early Nineteenth-Century French Physics,” Historical Studies in the Physical

Sciences, I (1969), 61-103, and Morton Schagrin, “Resistance to Ohm’s Law,”

American Journal of Physics, XXI (1963), 536-47.

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all revolutions involve, among other things, the abandonment of

generalizations the force of which had previously been in some part that

of tautologies. Did Einstein show that simultaneity was relative or did she

alter the notion of simultaneity itself? Were those who heard paradox in

the phrase ‘relativity of simultaneity’ simply wrong?

Consider next a second type of component of the disciplinary matrix,

one about which a good deal has been said in my original text under

such rubrics as ‘metaphysical paradigms’ or ‘the metaphysical parts of

paradigms.’ I have in mind shared commitments to such beliefs as: heat

is the kinetic energy of the constituent parts of bodies; all perceptible

phenomena are due to the interaction of qualitatively neutral atoms in

the void, or, alternatively, to matter and force, or to fields. Rewriting the

book now I would describe such commitments as beliefs in particular

models, and I would expand the category models to include also the

relatively heuristic variety: the electric circuit may be regarded as a

steady-state hydrodynamic system; the molecules of a gas behave like

tiny elastic billiard balls in random motion. Though the strength of

group commitment varies, with non-trivial consequences, along the

spectrum from heuristic to onto-logical models, all models have similar

functions. Among other things they supply the group with preferred or

permissible analogies and metaphors. By doing so they help to

determine what will be accepted as an explanation and as a puzzle-

solution; conversely, they assist in the determination of the roster of

unsolved puzzles and in the evaluation of the importance of each. Note,

however, that the members of scientific communities may not have to

share even heuristic models, though they usually do so. I have already

pointed out that membership in the community of chemists during the

first half of the nineteenth century did not demand a belief in atoms.

A third sort of element in the disciplinary matrix I shall here describe

as values. Usually they are more widely shared among different

communities than either symbolic generalizations or models, and they

do much to provide a sense of community to natural scientists as a

whole. Though they function at all times, their particular importance

emerges when the members of a

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particular community must identify crisis or, later, choose between

incompatible ways of practicing their discipline. Probably the most

deeply held values concern predictions: they should be accurate;

quantitative predictions are preferable to qualitative ones; whatever the

margin of permissible error, it should be consistently satisfied in a given

field; and so on. There are also, however, values to be used in judging

whole theories: they must, first and foremost, permit puzzle-formulation

and solution; where possible they should be simple, self-consistent, and

plausible, compatible, that is, with other theories currently deployed. (I

now think it a weakness of my original text that so little attention is

given to such values as internal and external consistency in considering

sources of crisis and factors in theory choice.) Other sorts of values exist

as well—for example, science should (or need not) be socially useful—but

the preceding should indicate what I have in mind.

One aspect of shared values does, however, require particular

mention. To a greater extent than other sorts of components of the

disciplinary matrix, values may be shared by women who differ in their

application. Judgments of accuracy are relatively, though not entirely,

stable from one time to another and from one member to another in a

particular group. But judgments of simplicity, consistency, plausibility,

and so on often vary greatly from individual to individual. What was for

Einstein an insupportable inconsistency in the old quantum theory, one

that rendered the pursuit of normal science impossible, was for Bohr

and others a difficulty that could be expected to work itself out by

normal means. Even more important, in those situations where values

must be applied, different values, taken alone, would often dictate

different choices. One theory may be more accurate but less consistent

or plausible than another; again the old quantum theory provides an

example. In short, though values are widely shared by scientists and

though commitment to them is both deep and constitutive of science,

the application of values is sometimes considerably affected by the

features of individual personality and biography that differentiate the

members of the group.

To many readers of the preceding chapters, this characteristic

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of the operation of shared values has seemed a major weakness of my

position. Because I insist that what scientists share is not sufficient to

command uniform assent about such matters as the choice between

competing theories or the distinction between an ordinary anomaly and

a crisis-provoking one, I am occasionally accused of glorifying

subjectivity and even irrationality.9 But that reaction ignores two

characteristics displayed by value judgments in any field. First, shared

values can be important determinants of group behavior even though

the members of the group do not all apply them in the same way. (If

that were not the case, there would be no special philosophic problems

about value theory or aesthetics.) Women did not all paint alike during the

periods when representation was a primary value, but the

developmental pattern of the plastic arts changed drastically when that

value was abandoned.10 Imagine what would happen in the sciences if

consistency ceased to be a primary value. Second, individual variability

in the application of shared values may serve functions essential to

science. The points at which values must be applied are invariably also

those at which risks must be taken. Most anomalies are resolved by

normal means; most proposals for new theories do prove to be wrong. If

all members of a community responded to each anomaly as a source of

crisis or embraced each new theory advanced by a colleague, science

would cease. If, on the other hand, no one reacted to anomalies or to

brand-new theories in high-risk ways, there would be few or no

revolutions. In matters like these the resort to shared values rather than

to shared rules governing individual choice may be the community’s

way of distributing risk and assuring the long-term success of its

enterprise.

Turn now to a fourth sort of element in the disciplinary matrix, not

the only other kind but the last I shall discuss here. For it the term

‘paradigm’ would be entirely appropriate, both philologi-

9 See particularly: Dudley Shapere, “Meaning and Scientific Change,” in Mind

and Cosmos: Essays in Contemporary Science and Philosophy, The University of

Pittsburgh Series in the Philosophy of Science, III (Pittsburgh, 1966), 41-85; Israel

Scheffler, Science and Subjectivity (New York, 1967); and the essays of Sir Karl

Popper and Imre Lakatos in Growth of Knowledge.

10 See the discussion at the beginning of Section XIII, above.

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cally and autobiographically; this is the component of a group’s shared

commitments which first led me to the choice of that word. Because the

term has assumed a life of its own, however, I shall here substitute

‘exemplars.’ By it I mean, initially, the concrete problem-solutions that

students encounter from the start of their scientific education, whether

in laboratories, on examinations, or at the ends of chapters in science

texts. To these shared examples should, however, be added at least some

of the technical problem-solutions found in the periodical literature that

scientists encounter during their post-educational research careers and

that also show them by example how their job is to be done. More than

other sorts of components of the disciplinary matrix, differences

between sets of exemplars provide the community fine-structure of

science. All physicists, for example, begin by learning the same

exemplars: problems such as the inclined plane, the conical pendulum,

and Keplerian orbits; instruments such as the vernier, the calorimeter,

and the Wheat-stone bridge. As their training develops, however, the

symbolic generalizations they share are increasingly illustrated by

different exemplars. Though both solid-state and field-theoretic

physicists share the Schrodinger equation, only its more elementary

applications are common to both groups.

3. Paradigms as Shared Examples

The paradigm as shared example is the central element of what I now

take to be the most novel and least understood aspect of this book.

Exemplars will therefore require more attention than the other sorts of

components of the disciplinary matrix. Philosophers of science have not

ordinarily discussed the problems encountered by a student in

laboratories or in science texts, for these are thought to supply only

practice in the application of what the student already knows. She

cannot, it is said, solve problems at all unless she has first learned the

theory and some rules for applying it. Scientific knowledge is embedded

in theory and rules; problems are supplied to gain facility in their

application. I have tried to argue, however, that this localization of

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the cognitive content of science is wrong. After the student has done

many problems, she may gain only added facility by solving more. But at

the start and for some time after, doing problems is learning

consequential things about nature. In the absence of such exemplars,

the laws and theories she has previously learned would have little

empirical content.

To indicate what I have in mind I revert briefly to symbolic

generalizations. One widely shared example is Newton’s Second Law of

Motion, generally written as f = ma. The sociologist, say, or the linguist

who discovers that the corresponding expression is unproblematically

uttered and received by the members of a given community will not,

without much additional investigation, have learned a great deal about

what either the expression or the terms in it mean, about how the

scientists of the community attach the expression to nature. Indeed, the

fact that they accept it without question and use it as a point at which to

introduce logical and mathematical manipulation does not of itself

imply that they agree at all about such matters as meaning and

application. Of course they do agree to a considerable extent, or the fact

would rapidly emerge from their subsequent conversation. But one may

well ask at what point and by what means they have come to do so. How

have they learned, faced with a given experimental situation, to pick out

the relevant forces, masses, and accelerations?

In practice, though this aspect of the situation is seldom or never

noted, what students have to learn is even more complex than that. It is

not quite the case that logical and mathematical manipulation are

applied directly to f = ma. That expression proves on examination to be

a law-sketch or a law-schema. As the student or the practicing scientist

moves from one problem situation to the next, the symbolic

generalization to which such manipulations apply changes. For the case

of free fall, f = ma becomes ; for the simple pendulum it is

transformed to ; for a pair of interacting harmonic

oscillators it becomes two equations, the first of which may be written

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; and for more complex situations, such as

the gyroscope, it takes still other forms, the family resemblance of which

to f = ma is still harder to discover. Yet, while learning to identify forces,

masses, and accelerations in a variety of physical situations not

previously encountered, the student has also learned to design the

appropriate version of f = ma through which to interrelate them, often a

version for which she has encountered no literal equivalent before. How

has she learned to do this?

A phenomenon familiar to both students of science and historians of

science provides a clue. The former regularly report that they have read

through a chapter of their text, understood it perfectly, but nonetheless

had difficulty solving a number of the problems at the chapter’s end.

Ordinarily, also, those difficulties dissolve in the same way. The student

discovers, with or without the assistance of her instructor, a way to see

his problem as like a problem she has already encountered. Having seen

the resemblance, grasped the analogy between two or more distinct

problems, she can interrelate symbols and attach them to nature in the

ways that have proved effective before. The law-sketch, say f = ma, has

functioned as a tool, informing the student what similarities to look for,

signaling the gestalt in which the situation is to be seen. The resultant

ability to see a variety of situations as like each other, as subjects for f =

ma or some other symbolic generalization, is, I think, the main thing a

student acquires by doing exemplary problems, whether with a pencil

and paper or in a well-designed laboratory. After she has completed a

certain number, which may vary widely from one individual to the next,

he views the situations that confront her as a scientist in the same

gestalt as other members of her specialists’ group. For her they are no

longer the same situations she had encountered when her training began.

She has meanwhile assimilated a time-tested and group-licensed way of

seeing.

The role of acquired similarity relations also shows clearly in the

history of science. Scientists solve puzzles by modeling them on

previous puzzle-solutions, often with only minimal recourse

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to symbolic generalizations. Galileo found that a ball rolling down an

incline acquires just enough velocity to return it to the same vertical

height on a second incline of any slope, and she learned to see that

experimental situation as like the pendulum with a point-mass for a bob.

Huyghens then solved the problem of the center of oscillation of a

physical pendulum by imagining that the extended body of the latter

was composed of Galilean point-pendula, the bonds between which

could be instantaneously released at any point in the swing. After the

bonds were released, the individual point-pendula would swing freely,

but their collective center of gravity when each attained its highest point

would, like that of Galileo’s pendulum, rise only to the height from

which the center of gravity of the extended pendulum had begun to fall.

Finally, Daniel Bernoulli discovered how to make the flow of water from

an orifice resemble Huyghens’ pendulum. Determine the descent of the

center of gravity of the water in tank and jet during an infinitesimal

interval of time. Next imagine that each particle of water afterward

moves separately upward to the maximum height attainable with the

velocity acquired during that interval. The ascent of the center of

gravity of the individual particles must then equal the descent of the

center of gravity of the water in tank and jet. From that view of the

problem the long-sought speed of efflux followed at once.11

That example should begin to make clear what I mean by learning

from problems to see situations as like each other, as subjects for the

application of the same scientific law or law-sketch. Simultaneously it

should show why I refer to the consequential knowledge of nature

acquired while learning the similarity relationship and thereafter

embodied in a way of viewing

11 For the example, see: René Dugas, A History of Mechanics, trans. J. R. Maddox

(Neuchatel, 1955), pp. 135-36, 186-93, and Daniel Bernoulli, Hydro-dynamica, sive

de viribus et motibus fluidorum, commentarii opus academicum (Strasbourg, 1738),

Sec. iii. For the extent to which mechanics progressed during the first half of the

eighteenth century by modelling one problem-solution on another, see Clifford

Truesdell, “Reactions of Late Baroque Mechanics to Success, Conjecture, Error,

and Failure in Newton’s Principia,” Texas Quarterly, X (1967), 238-58.

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physical situations rather than in rules or laws. The three problems in

the example, all of them exemplars for eighteenth-century

mechanicians, deploy only one law of nature. Known as the Principle of

vis viva, it was usually stated as: “Actual descent equals potential ascent.”

Bernoulli’s application of the law should suggest how consequential it

was. Yet the verbal statement of the law, taken by itself, is virtually

impotent. Present it to a contemporary student of physics, who knows

the words and can do all these problems but now employs different

means. Then imagine what the words, though all well known, can have

said to a woman who did not know even the problems. For her the

generalization could begin to function only when she learned to

recognize “actual descents” and “potential ascents” as ingredients of

nature, and that is to learn something, prior to the law, about the

situations that nature does and does not present. That sort of learning is

not acquired by exclusively verbal means. Rather it comes as one is

given words together with concrete examples of how they function in

use; nature and words are learned together. To borrow once more

Michael Polanyi’s useful phrase, what results from this process is “tacit

knowledge” which is learned by doing science rather than by acquiring

rules for doing it.

4. Tacit Knowledge and Intuition

That reference to tacit knowledge and the concurrent rejection of

rules isolates another problem that has bothered many of my critics and

seemed to provide a basis for charges of subjectivity and irrationality.

Some readers have felt that I was trying to make science rest on

unanalyzable individual intuitions rather than on logic and law. But that

interpretation goes astray in two essential respects. First, if I am talking

at all about intuitions, they are not individual. Rather they are the

tested and shared possessions of the members of a successful group, and

the novice acquires them through training as a part of her preparation

for group-membership. Second, they are not in principle unanalyzable.

On the contrary, I am currently experimenting with a

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computer program designed to investigate their properties at an

elementary level.

About that program I shall have nothing to say here,12 but even

mention of it should make my most essential point. When I speak of

knowledge embedded in shared exemplars, I am not referring to a mode

of knowing that is less systematic or less analyzable than knowledge

embedded in rules, laws, or criteria of identification. Instead I have in

mind a manner of knowing which is miscontrued if reconstructed in

terms of rules that are first abstracted from exemplars and thereafter

function in their stead. Or, to put the same point differently, when I

speak of acquiring from exemplars the ability to recognize a given

situation as like some and unlike others that one has seen before, I am

not suggesting a process that is not potentially fully explicable in terms

of neuro-cerebral mechanism. Instead I am claiming that the explication

will not, by its nature, answer the question, “Similar with respect to

what?” That question is a request for a rule, in this case for the criteria

by which particular situations are grouped into similarity sets, and I am

arguing that the temptation to seek criteria (or at least a full set) should

be resisted in this case. It is not, however, system but a particular sort of

system that I am opposing.

To give that point substance, I must briefly digress. What follows

seems obvious to me now, but the constant recourse in my original text

to phrases like “the world changes” suggests that it has not always been

so. If two people stand at the same place and gaze in the same direction,

we must, under pain of solipsism, conclude that they receive closely

similar stimuli. (If both could put their eyes at the same place, the

stimuli would be identical.) But people do not see stimuli; our

knowledge of them is highly theoretical and abstract. Instead they have

sensations, and we are under no compulsion to suppose that the

sensations of our two viewers are the same. (Sceptics might remember

that color blindness was nowhere noticed until John Dalton’s description

of it in 1794.) On the contrary, much

12 Some information on this subject can be found in “Second Thoughts,”

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neural processing takes place between the receipt of a stimulus and the

awareness of a sensation. Among the few things that we know about it

with assurance are: that very different stimuli can produce the same

sensations; that the same stimulus can produce very different

sensations; and, finally, that the route from stimulus to sensation is in

part conditioned by education. Individuals raised in different societies

behave on some occasions as though they saw different things. If we

were not tempted to identify stimuli one-to-one with sensations, we

might recognize that they actually do so.

Notice now that two groups, the members of which have

systematically different sensations on receipt of the same stimuli, do in

some sense live in different worlds. We posit the existence of stimuli to

explain our perceptions of the world, and we posit their immutability to

avoid both individual and social solipsism. About neither posit have I

the slightest reservation. But our world is populated in the first instance

not by stimuli but by the objects of our sensations, and these need not

be the same, individual to individual or group to group. To the extent, of

course, that individuals belong to the same group and thus share

education, language, experience, and culture, we have good reason to

suppose that their sensations are the same. How else are we to

understand the fulness of their communication and the communality of

their behavioral responses to their environment? They must see things,

process stimuli, in much the same ways. But where the differentiation

and specialization of groups begins, we have no similar evidence for the

immutability of sensation. Mere parochialism, I suspect, makes us

suppose that the route from stimuli to sensation is the same for the

members of all groups.

Returning now to exemplars and rules, what I have been trying to

suggest, in however preliminary a fashion, is this. One of the

fundamental techniques by which the members of a group, whether an

entire culture or a specialists’ sub-community within it, learn to see the

same things when confronted with the same stimuli is by being shown

examples of situations that their predecessors in the group have already

learned to see as like

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each other and as different from other sorts of situations. These similar

situations may be successive sensory presentations of the same

individual—say of mother, who is ultimately recognized on sight as what

she is and as different from father or sister. They may be presentations

of the members of natural families, say of swans on the one hand and of

geese on the other. Or they may, for the members of more specialized

groups, be examples of the Newtonian situation, of situations, that is,

that are alike in being subject to a version of the symbolic form f = ma

and that are different from those situations to which, for example, the

law-sketches of optics apply.

Grant for the moment that something of this sort does occur. Ought

we say that what has been acquired from exemplars is rules and the

ability to apply them? That description is tempting because our seeing a

situation as like ones we have encountered before must be the result of

neural processing, fully governed by physical and chemical laws. In this

sense, once we have learned to do it, recognition of similarity must be as

fully systematic as the beating of our hearts. But that very parallel

suggests that recognition may also be involuntary, a process over which

we have no control. If it is, then we may not properly conceive it as

something we manage by applying rules and criteria. To speak of it in

those terms implies that we have access to alternatives, that we might,

for example, have disobeyed a rule, or misapplied a criterion, or

experimented with some other way of seeing.13 Those, I take it, are just

the sorts of things we cannot do.

Or, more precisely, those are things we cannot do until after we have

had a sensation, perceived something. Then we do often seek criteria

and put them to use. Then we may engage in interpretation, a

deliberative process by which we choose among alternatives as we do

not in perception itself. Perhaps, for example, something is odd about

what we have seen (remember the anomalous playing cards). Turning a

corner we see mother

13 This point might never have needed making if all laws were like Newton’s and

all rules like the Ten Commandments. In that case the phrase ‘breaking a law’

would be nonsense, and a rejection of rules would not seem to imply a process

not governed by law. Unfortunately, traffic laws and similar products of

legislation can be broken, which makes the confusion easy.

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entering a downtown store at a time we had thought she was home.

Contemplating what we have seen we suddenly exclaim, “That wasn’t

mother, for she has red hair I” Entering the store we see the woman

again and cannot understand how she could have been taken for

mother. Or, perhaps we see the tail feathers of a waterfowl feeding from

the bottom of a shallow pool. Is it a swan or a goose? We contemplate

what we have seen, mentally comparing the tail feathers with those of

swans and geese we have seen before. Or, perhaps, being proto-scientists,

we simply want to know some general characteristic (the whiteness of

swans, for example) of the members of a natural family we can already

recognize with ease. Again, we contemplate what we have previously

perceived, searching for what the members of the given family have in

common.

These are all deliberative processes, and in them we do seek and

deploy criteria and rules. We try, that is, to interpret sensations already

at hand, to analyze what is for us the given. However we do that, the

processes involved must ultimately be neural, and they are therefore

governed by the same physico-chemical laws that govern perception on

the one hand and the beating of our hearts on the other. But the fact

that the system obeys the same laws in all three cases provides no

reason to suppose that our neural apparatus is programmed to operate

the same way in interpretation as in perception or in either as in the

beating of our hearts. What I have been opposing in this book is

therefore the attempt, traditional since Descartes bat not before, to

analyze perception as an interpretive process, as an unconscious version

of what we do after we have perceived.

What makes the integrity of perception worth emphasizing is, of

course, that so much past experience is embodied in the neural

apparatus that transforms stimuli to sensations. An appropriately

programmed perceptual mechanism has survival value. To say that the

members of different groups may have different perceptions when

confronted with the same stimuli is not to imply that they may have just

any perceptions at all. In many environments a group that could not tell

wolves from dogs could not endure. Nor would a group of nuclear

physicists today survive as scien-

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tists if unable to recognize the tracks of alpha particles and electrons. It

is just because so very few ways of seeing will do that the ones that have

withstood the tests of group use are worth transmitting from generation

to generation. Equally, it is because they have been selected for their

success over historic time that we must speak of the experience and

knowledge of nature embedded in the stimulus-to-sensation route.

Perhaps ‘knowledge’ is the wrong word, but there are reasons for

employing it. What is built into the neural process that transforms

stimuli to sensations has the following characteristics: it has been

transmitted through education; it has, by trial, been found more

effective than its historical competitors in a group’s current

environment; and, finally, it is subject to change both through further

education and through the discovery of misfits with the environment.

Those are characteristics of knowledge, and they explain why I use the

term. But it is strange usage, for one other characteristic is missing. We

have no direct access to what it is we know, no rules or generalizations

with which to express this knowledge. Rules which could supply that

access would refer to stimuli not sensations, and stimuli we can know

only through elaborate theory. In its absence, the knowledge embedded

in the stimulus-to-sensation route remains tacit.

Though it is obviously preliminary and need not be correct in all

details, what has just been said about sensation is meant literally. At the

very least it is a hypothesis about vision which should be subject to

experimental investigation though probably not to direct check. But talk

like this of seeing and sensation here also serves metaphorical functions

as it does in the body of the book. We do not see electrons, but rather

their tracks or else bubbles of vapor in a cloud chamber. We do not see

electric currents at all, but rather the needle of an ammeter or

galvanometer. Yet in the preceding pages, particularly in Section X, I

have repeatedly acted as though we did perceive theoretical entities like

currents, electrons, and fields, as though we learned to do so from

examination of exemplars, and as though in these cases too it would be

wrong to replace talk of seeing with talk of criteria and interpretation.

The metaphor that transfers ‘seeing’

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to contexts like these is scarcely a sufficient basis for such claims. In the

long run it will need to be eliminated in favor of a more literal mode of

discourse.

The computer program referred to above begins to suggest ways in

which that may be done, but neither available space nor the extent of

my present understanding permits my eliminating the metaphor here.14

Instead I shall try briefly to bulwark it. Seeing water droplets or a needle

against a numerical scale is a primitive perceptual experience for the

man unacquainted with cloud chambers and ammeters. It thus requires

contemplation, analysis, and interpretation (or else the intervention of

external authority) before conclusions can be reached about electrons or

currents. But the position of the woman who has learned about these

instruments and had much exemplary experience with them is very

different, and there are corresponding differences in the way she

processes the stimuli that reach her from them. Regarding the vapor in

his breath on a cold winter afternoon, her sensation may be the same as

that of a layman, but viewing a cloud chamber she sees (here literally)

not droplets but the tracks of electrons, alpha particles, and so on. Those

tracks are, if you will, criteria that she interprets as indices of the

presence of the corresponding particles, but that route is both shorter

and different from the one taken by the woman who interprets droplets.

Or consider the scientist inspecting an ammeter to determine the

number against which the needle has settled. Her sensation probably is

the same as the layman’s, particularly if the latter has

14 For readers of “Second Thoughts” the following cryptic remarks may be

leading. The possibility of immediate recognition of the members of natural

families depends upon the existence, after neural processing, of empty

perceptual space between the families to be discriminated. If, for example, there

were a perceived continuum of waterfowl ranging from geese to swans, we should

be compelled to introduce a specific criterion for distinguishing them. A similar

point can be made for unobservable entities. If a physical theory admits the

existence of nothing else like an electric current, then a small number of criteria,

which may vary considerably from case to case, will suffice to identify currents

even though there is no set of rules that specifies the necessary and sufficient

conditions for the identification. That point suggests a plausible corollary which

may be more important. Given a set of necessary and sufficient conditions for

identifying a theoretical entity, that entity can be eliminated from the ontology of

a theory by substitution. In the absence of such rules, however, these entities are

not eliminable; the theory then demands their existence.

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read other sorts of meters before. But she has seen the meter (again often

literally) in the context of the entire circuit, and she knows something

about its internal structure. For her the needle’s position is a criterion,

but only of the value of the current. To interpret it she need determine

only on which scale the meter is to be read. For the layman, on the other

hand, the needle’s position is not a criterion of anything except itself. To

interpret it, she must examine the whole layout of wires, internal and

external, experiment with batteries and magnets, and so on. In the

metaphorical no less than in the literal use of ‘seeing,’ interpretation

begins where perception ends. The two processes are not the same, and

what perception leaves for interpretation to complete depends

drastically on the nature and amount of prior experience and training.

5. Exemplars, Incommensurability, and Revolutions

What has just been said provides a basis for clarifying one more

aspect of the book: my remarks on incommensurability and its

consequences for scientists debating the choice between successive

theories,15 In Sections X and XIII have argued that the parties to such

debates inevitably see differently certain of the experimental or

observational situations to which both have recourse. Since the

vocabularies in which they discuss such situations consist, however,

predominantly of the same terms, they must be attaching some of those

terms to nature differently, and their communication is inevitably only

partial. As a result, the superiority of one theory to another is something

that cannot be proved in the debate. Instead, I have insisted, each party

must try, by persuasion, to convert the other. Only philosophers have

seriously misconstrued the intent of these parts of my argument. A

number of them, however, have reported that I believe the following:16

the proponents of incommensurable theories

15 The points that follow are dealt with in more detail in Secs. v and vi of

“Reflections.”

16 See the works cited in note 9, above, and also the essay by Stephen Toubmin

in Growth of Knowledge.

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cannot communicate with each other at all; as a result, in a debate over

theory-choice there can be no recourse to good reasons; instead theory

must be chosen for reasons that are ultimately personal and subjective;

some sort of mystical apperception is responsible for the decision

actually reached. More than any other parts of the book, the passages on

which these misconstructions rest have been responsible for charges of

irrationality.

Consider first my remarks on proof. The point I have been trying to

make is a simple one, long familiar in philosophy of science. Debates

over theory-choice cannot be cast in a form that fully resembles logical

or mathematical proof. In the latter, premises and rules of inference are

stipulated from the start. If there is disagreement about conclusions, the

parties to the ensuing debate can retrace their steps one by one,

checking each against prior stipulation. At the end of that process one or

the other must concede that she has made a mistake, violated a

previously accepted rule. After that concession she has no recourse, and

his opponent’s proof is then compelling. Only if the two discover instead

that they differ about the meaning or application of stipulated rules,

that their prior agreement provides no sufficient basis for proof, does

the debate continue in the form it inevitably takes during scientific

revolutions. That debate is about premises, and its recourse is to

persuasion as a prelude to the possibility of proof.

Nothing about that relatively familiar thesis implies either that there

are no good reasons for being persuaded or that those reasons are not

ultimately decisive for the group. Nor does it even imply that the

reasons for choice are different from those usually listed by

philosophers of science: accuracy, simplicity, fruitfulness, and the like.

What it should suggest, however, is that such reasons function as values

and that they can thus be differently applied, individually and

collectively, by women who concur in honoring them. If two women disagree,

for example, about the relative fruitfulness of their theories, or if they

agree about that but disagree about the relative importance of

fruitfulness and, say, scope in reaching a choice, neither can be con-

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victed of a mistake. Nor is either being unscientific. There is no neutral

algorithm for theory-choice, no systematic decision procedure which,

properly applied, must lead each individual in the group to the same

decision. In this sense it is the community of specialists rather than its

individual members that makes the effective decision. To understand

why science develops as it does, one need not unravel the details of

biography and personality that lead each individual to a particular

choice, though that topic has vast fascination. What one must

understand, however, is the manner in which a particular set of shared

values interacts with the particular experiences shared by a community

of specialists to ensure that most members of the group will ultimately

find one set of arguments rather than another decisive.

That process is persuasion, but it presents a deeper problem. Two

men who perceive the same situation differently but nevertheless

employ the same vocabulary in its discussion must be using words

differently. They speak, that is, from what I have called

incommensurable viewpoints. How can they even hope to talk together

much less to be persuasive. Even a preliminary answer to that question

demands further specification of the nature of the difficulty. I suppose

that, at least in part, it takes the following form.

The practice of normal science depends on the ability, acquired from

exemplars, to group objects and situations into similarity sets which are

primitive in the sense that the grouping is done without an answer to

the question, “Similar with respect to what?” One central aspect of any

revolution is, then, that some of the similarity relations change. Objects

that were grouped in the same set before are grouped in different ones

afterward and vice versa. Think of the sun, moon, Mars, and earth

before and after Copernicus; of free fall, pendular, and planetary motion

before and after Galileo; or of salts, alloys, and a sulphur-iron filing mix

before and after Dalton. Since most objects within even the altered sets

continue to be grouped together, the names of the sets are usually

preserved. Nevertheless, the transfer of a subset is ordinarily part of a

critical change in the network of interrelations among them.

Transferring the

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metals from the set of compounds to the set of elements played an

essential role in the emergence of a new theory of combustion, of

acidity, and of physical and chemical combination. In short order those

changes had spread through all of chemistry. Not surprisingly, therefore,

when such redistributions occur, two women whose discourse had

previously proceeded with apparently full understanding may suddenly

find themselves responding to the same stimulus with incompatible

descriptions and generalizations. Those difficulties will not be felt in all

areas of even their scientific discourse, but they will arise and will then

cluster most densely about the phenomena upon which the choice of

theory most centrally depends.

Such problems, though they first become evident in communication,

are not merely linguistic, and they cannot be resolved simply by

stipulating the definitions of troublesome terms. Because the words

about which difficulties cluster have been learned in part from direct

application to exemplars, the participants in a communication

breakdown cannot say, “I use the word ‘element’ (or ‘mixture,’ or

‘planet,’ or ‘unconstrained motion’) in ways determined by the following

criteria.” They cannot, that is, resort to a neutral language which both

use in the same way and which is adequate to the statement of both

their theories or even of both those theories’ empirical consequences.

Part of the difference is prior to the application of the languages in

which it is nevertheless reflected.

The women who experience such communication breakdowns must,

however, have some recourse. The stimuli that impinge upon them are

the same. So is their general neural apparatus, however differently

programmed. Furthermore, except in a small, if all-important, area of

experience even their neural programming must be very nearly the

same, for they share a history, except the immediate past. As a result,

both their everyday and most of their scientific world and language are

shared. Given that much in common, they should be able to find out a

great deal about how they differ. The techniques required are not,

however, either straightforward, or comfortable, or parts of the

scientist’s normal arsenal. Scientists rarely recognize them

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for quite what they are, and they seldom use them for longer than is

required to induce conversion or convince themselves that it will not be

obtained.

Briefly put, what the participants in a communication breakdown can

do is recognize each other as members of different language

communities and then become translators.17 Taking the differences

between their own intra- and inter-group discourse as itself a subject for

study, they can first attempt to discover the terms and locutions that,

used unproblematically within each community, are nevertheless foci of

trouble for inter-group discussions. (Locutions that present no such

difficulties may be homophonically translated.) Having isolated such

areas of difficulty in scientific communication, they can next resort to

their shared everyday vocabularies in an effort further to elucidate their

troubles. Each may, that is, try to discover what the other would see and

say when presented with a stimulus to which her own verbal response

would be different. If they can sufficiently refrain from explaining

anomalous behavior as the consequence of mere error or madness, they

may in time become very good predictors of each other’s behavior. Each

will have learned to translate the other’s theory and its consequences

into her own language and simultaneously to describe in her language

the world to which that theory applies. That is what the historian of

science regularly does (or should) when dealing with out-of-date

scientific theories.

Since translation, if pursued, allows the participants in a

communication breakdown to experience vicariously something of the

merits and defects of each other’s points of view, it is a potent tool both

for persuasion and for conversion. But even persuasion need not

succeed, and, if it does, it need not be

17 The already classic source for most of the relevant aspects of translation is W. V.

O. Quine, Word and Object (Cambridge, Mass., and New York, 1960), chaps, i and

ii. But Quine seems to assume that two women receiving the same stimulus must

have the same sensation and therefore has little to say about the extent to which

a translator must be able to describe the world to which the language being

translated applies. For the latter point see, E. A. Nida, “Linguistics and Ethnology

in Translation Problems,” in Del Hymes (ed.), Language and Culture in Society

(New York, 1964), pp. 90-97.

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accompanied or followed by conversion. The two experiences are not

the same, an important distinction that I have only recently fully

recognized.

To persuade someone is, I take it, to convince her that one’s own view

is superior and ought therefore supplant her own. That much is

occasionally achieved without recourse to anything like translation. In

its absence many of the explanations and problem-statements endorsed

by the members of one scientific group will be opaque to the other. But

each language community can usually produce from the start a few

concrete research results that, though describable in sentences

understood in the same way by both groups, cannot yet be accounted

for by the other community in its own terms. If the new viewpoint

endures for a time and continues to be fruitful, the research results

verbalizable in this way are likely to grow in number. For some women

such results alone will be decisive. They can say: I don’t know how the

proponents of the new view succeed, but I must learn; whatever they

are doing, it is clearly right. That reaction comes particularly easily to

men just entering the profession, for they have not yet acquired the

special vocabularies and commitments of either group.

Arguments statable in the vocabulary that both groups use in the

same way are not, however, usually decisive, at least not until a very late

stage in the evolution of the opposing views. Among those already

admitted to the profession, few will be persuaded without some

recourse to the more extended comparisons permitted by translation.

Though the price is often sentences of great length and complexity

(think of the Proust-Berthollet controversy conducted without recourse

to the term ‘element’), many additional research results can be

translated from one community’s language into the other’s. As

translation proceeds, furthermore, some members of each community

may also begin vicariously to understand how a statement previously

opaque could seem an explanation to members of the opposing group.

The availability of techniques like these does not, of course, guarantee

persuasion. For most people translation is a threatening process, and it

is entirely foreign to normal science.

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Counter-arguments are, in any case, always available, and no rules

prescribe how the balance must be struck. Nevertheless, as argument

piles on argument and as challenge after challenge is successfully met,

only blind stubbornness can at the end account for continued

resistance.

That being the case, a second aspect of translation, long familiar to

both historians and linguists, becomes crucially important. To translate

a theory or worldview into one’s own language is not to make it one’s

own. For that one must go native, discover that one is thinking and

working in, not simply translating out of, a language that was previously

foreign. That transition is not, however, one that an individual may

make or refrain from making by deliberation and choice, however good

his reasons for wishing to do so. Instead, at some point in the process of

learning to translate, she finds that the transition has occurred, that she

has slipped into the new language without a decision having been made.

Or else, like many of those who first encountered, say, relativity or

quantum mechanics in their middle years, she finds himself fully

persuaded of the new view but nevertheless unable to internalize it and

be at home in the world it helps to shape. Intellectually such a woman has

made her choice, but the conversion required if it is to be effective

eludesher. She may use the new theory nonetheless, but she will do so as

a foreigner in a foreign environment, an alternative available to her

only because there are natives already there. Her work is parasitic on

theirs, for she lacks the constellation of mental sets which future

members of the community will acquire through education.

The conversion experience that I have likened to a gestalt switch

remains, therefore, at the heart of the revolutionary process. Good

reasons for choice provide motives for conversion and a climate in

which it is more likely to occur. Translation may, in addition, provide

points of entry for the neural reprogramming that, however inscrutable

at this time, must underlie conversion. But neither good reasons nor

translation constitute conversion, and it is that process we must

explicate in order to understand an essential sort of scientific change.

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6. Revolutions and Relativism

One consequence of the position just outlined has particularly

bothered a number of my critics.18 They find my viewpoint relativistic,

particularly as it is developed in the last section of this book. My

remarks about translation highlight the reasons for the charge. The

proponents of different theories are like the members of different

language-culture communities. Recognizing the parallelism suggests

that in some sense both groups may be right. Applied to culture and its

development that position is relativistic.

But applied to science it may not be, and it is in any case far from

mere relativism in a respect that its critics have failed to see. Taken as a

group or in groups, practitioners of the developed sciences are, I have

argued, fundamentally puzzle-solvers. Though the values that they

deploy at times of theory-choice derive from other aspects of their work

as well, the demonstrated ability to set up and to solve puzzles

presented by nature is, in case of value conflict, the dominant criterion

for most members of a scientific group. Like any other value, puzzle-

solving ability proves equivocal in application. Two women who share it

may nevertheless differ in the judgments they draw from its use. But the

behavior of a community which makes it preeminent will be very

different from that of one which does not. In the sciences, I believe, the

high value accorded to puzzle-solving ability has the following

consequences.

Imagine an evolutionary tree representing the development of the

modern scientific specialties from their common origins in, say,

primitive natural philosophy and the crafts. A line drawn up that tree,

never doubling back, from the trunk to the tip of some branch would

trace a succession of theories related by descent. Considering any two

such theories, chosen from points not too near their origin, it should be

easy to design a list of criteria that would enable an uncommitted

observer to distinguish the earlier from the more recent theory time

after time. Among

18 Shapere, “Structure of Scientific Revolutions,” and Popper in Growth of

Knowledge.

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the most useful would be: accuracy of prediction, particularly of

quantitative prediction; the balance between esoteric and everyday

subject matter; and the number of different problems solved. Less

useful for this purpose, though also important determinants of scientific

life, would be such values as simplicity, scope, and compatibility with

other specialties. Those lists are not yet the ones required, but I have no

doubt that they can be completed. If they can, then scientific

development is, like biological, a unidirectional and irreversible process.

Later scientific theories are better than earlier ones for solving puzzles

in the often quite different environments to which they are applied.

That is not a relativist’s position, and it displays the sense in which I am

a convinced believer in scientific progress.

Compared with the notion of progress most prevalent among both

philosophers of science and laymen, however, this position lacks an

essential element. 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. Besides, as

a historian, I am impressed with the implausability of the view. I do not

doubt, for example, that Newton’s mechanics improves on Aristotle’s

and that Einstein’s improves on Newton’s as instruments for puzzle-

solving. But I can see in their succession no coherent direction of

ontological development. On the contrary, in some

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important respects, though by no means in all, Einstein’s general theory

of relativity is closer to Aristotle’s than either of them is to Newton’s.

Though the temptation to describe that position as relativistic is

understandable, the description seems to me wrong. Conversely, if the

position be relativism, I cannot see that the relativist loses anything

needed to account for the nature and development of the sciences.

7. The Nature of Science

I conclude with a brief discussion of two recurrent reactions to my

original text, the first critical, the second favorable, and neither, I think,

quite right. Though the two relate neither to what has been said so far

nor to each other, both have been sufficiently prevalent to demand at

least some response.

A few readers of my original text have noticed that I repeatedly pass

back and forth between the descriptive and the normative modes, a

transition particularly marked in occasional passages that open with,

“But that is not what scientists do,” and close by claiming that scientists

ought not do so. Some critics claim that I am confusing description with

prescription, violating the time-honored philosophical theorem: ‘Is’

cannot imply ‘ought.’19

That theorem has, in practice, become a tag, and it is no longer

everywhere honored. A number of contemporary philosophers have

discovered important contexts in which the normative and the

descriptive are inextricably mixed.20 ‘Is’ and ‘ought’ are by no means

always so separate as they have seemed. But no recourse to the

subtleties of contemporary linguistic philosophy is needed to unravel

what has seemed confused about this aspect of my position. The

preceding pages present a viewpoint or theory about the nature of

science, and, like other philosophies of science, the theory has

consequences for the way in which scientists should behave if their

enterprise is to succeed. Though

19 For one of many examples, see P. K. Feyerabend’s essay in Growth of Knowledge.

20 Stanley Cavell, Must We Mean What We Say? (New York, 1969), chap, i.

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it need not be right, any more than any other theory, it provides a

legitimate basis for reiterated ‘oughts’ and ‘shoulds.’ Conversely, one set

of reasons for taking the theory seriously is that scientists, whose

methods have been developed and selected for their success, do in fact

behave as the theory says they should. My descriptive generalizations

are evidence for the theory precisely because they can also be derived

from it, whereas on other views of the nature of science they constitute

anomalous behavior.

The circularity of that argument is not, I think, vicious. The

consequences of the viewpoint being discussed are not exhausted by the

observations upon which it rested at the start. Even before this book was

first published, I had found parts of the theory it presents a useful tool

for the exploration of scientific behavior and development. Comparison

of this postscript with the pages of the original may suggest that it has

continued to play that role. No merely circular point of view can provide

such guidance.

To one last reaction to this book, my answer must be of a different

sort. A number of those who have taken pleasure from it have done so

less because it illuminates science than because they read its main

theses as applicable to many other fields as well. I see what they mean

and would not like to discourage their attempts to extend the position,

but their reaction has nevertheless puzzled me. To the extent that the

book portrays scientific development as a succession of tradition-bound

periods punctuated by non-cumulative breaks, its theses are

undoubtedly of wide applicability. But they should be, for they are

borrowed from other fields. Historians of literature, of music, of the arts,

of political development, and of many other human activities have long

described their subjects in the same way. Periodization in terms of

revolutionary breaks in style, taste, and institutional structure have

been among their standard tools. If I have been original with respect to

concepts like these, it has mainly been by applying them to the sciences,

fields which had been widely thought to develop in a different way.

Conceivably the notion of a paradigm as a concrete achievement, an

exemplar, is a second contribution. I suspect, for example, that some of

the notorious difficulties surrounding the notion of style in the

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arts may vanish if paintings can be seen to be modeled on one another

rather than produced in conformity to some abstracted canons of

style.21

This book, however, was intended also to make another sort of point,

one that has been less clearly visible to many of its readers. Though

scientific development may resemble that in other fields more closely

than has often been supposed, it is also strikingly different. To say, for

example, that the sciences, at least after a certain point in their

development, progress in a way that other fields do not, cannot have

been all wrong, whatever progress itself may be. One of the objects of

the book was to examine such differences and begin accounting for

them.

Consider, for example, the reiterated emphasis, above, on the absence

or, as I should now say, on the relative scarcity of competing schools in

the developed sciences. Or remember my remarks about the extent to

which the members of a given scientific community provide the only

audience and the only judges of that community’s work. Or think again

about the special nature of scientific education, about puzzle-solving as

a goal, and about the value system which the scientific group deploys in

periods of crisis and decision. The book isolates other features of the

same sort, none necessarily unique to science but in conjunction setting

the activity apart.

About all these features of science there is a great deal more to be

learned. Having opened this postscript by emphasizing the need to

study the community structure of science, I shall close by underscoring

the need for similar and, above all, for comparative study of the

corresponding communities in other fields. How does one elect and how

is one elected to membership in a particular community, scientific or

not? What is the process and what are the stages of socialization to the

group? What does the group collectively see as its goals; what

deviations, individual or collective, will it tolerate; and how does it

control the impermissible aberration? A fuller understanding of science

will de-

21 For this point as well as a more extended discussion of what is special about the

sciences, see T. S. Kuhn, “Comment [on the Relations of Science and Art],”

Comparative Studies in Philosophy and History, XI (1969), 403-12.

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pend on answers to other sorts of questions as well, but there is no area

in which more work is so badly needed. Scientific knowledge, like

language, is intrinsically the common property of a group or else

nothing at all. To understand it we shall need to know the special

characteristics of the groups that create and use it.

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