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The Structure of Scientific Revolutions, Second Edition Enlarged /Sc
Foundations of the Unity of Science II/2. Internat. Encyclo. Unified
Chicago: University of Chicago Press, 1962, 1970.

1. Introduction: A Role for History

Genetic history of science

p1 Image of science derived from writings of scientists and from textbooks exhibit not process of scientific development but finished products

Questions raised by this type of science-image do not lead to genetic account: present science as uniquely a matter of the observations, laws, and theories exhibited in textbooks

Consider scientific work to be parallel to work of gathering textbook data

Positivist history

p2 Science a stockpile of successive increments and its history adds the list of obstacles to the accumulation

More recent work in history of science finds concept of development-by-accumulation less and less satisfactory

Summary judgments

Past observation and belief, commonly labeled "error" and "superstition", found more and more difficult in its genesis to distinguish from genesis of contemporary science

p3 Out-of-date theories are not in principle unscientific because they have been discarded

New type of history of science

Historiographic revolution in the study of science

New types of questions asked

Course of development not conforming to dev by accretion

Scientific integrity of past work revealed by setting it in the context of its own time: relation of Galilei's views, not to those of today, but to those of his day

Show that old views in their context give maximum coherence to their contextual opinions and closest fit to nature

Horizon

Insufficiency of a methodology to account for sc views

p4 .. the early developmental stages of most sciences have been characterized by continual competition between a number of distinct views of nature.... Observation and experience... cannot alone determine a particular body of scientific 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.

This element of arbitrariness does not indicate that any scientific group could practise its trade without some set of received beliefs.

pp5 "At least in the mature sciences, answers to (basic) questions like these are firmly embedded in the educational institution that prepares and licences 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... (later) we shall want 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 elements of arbitrariness in their historic origins and, occasionally, in their subsequent development."

"Normal science, for example, often suppresses fundamental novelties because they are necessarily subversive of its basic commitments."

"Normal science, the activity in which most scientists inevitably spend most of their time, is predicated on the sassmumptic that the scientific community knows what the world is like....

..so long as those commitments retain an element of the arbitrary, the very nature of normal science ensures that novelty shall not be suppressed for very long.

p6 .. when 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 least to a new set of commitments, a new basis for the practice of science. The extraordinary episodes in which the shift of professional commitments occurs are the ones known in this essay as scientific revolutions.

p6 "... the major turning points in scientific development associated with the names of Copernicus, Newton, Lavoisier, and Einstein. More clearly than most oxther episodes in the histxory 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-honoured scientific theory in favor of another incompatible with ~~xx~~ it. Each prodcued a shift in the problems available for scientific scrutiny and in the standards by whih the profession determined what should count as an ~~xx~~ admissibe 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 ~~xx~~ the controversies that almost always accompany them, are the defining characteristics of scientific revolutions."

6-7 "For the far smaller/professional group affected by them, maxwell's equations were as revolutionary as Einstein's, and they were resisted accordingly. The invextion of other new theories regularly, and appropriately, evoxkes the same response from ~~thx~~ some of the specialists on whose area of special competence they impinge. For these men the new theory implies achange in the prior practice of normal science. Inevitably, therefore, it reflects ~~xx~~ 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 already is known. Its assmilation requires the reconsttucrtion of prior theory the re-evaluation of prior fact, an intrinsically revolutionary process that is seldom completed by a single man and never overnight. No wonder historians have had difficulty in dating precxisely this extended process that their vocabulary impels them to view as as an isolated evxent."

p7 ... 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 does that, but not until the professional community has re-evaluxated traditional experimental procedures, altered its concep -tion of entities which which it has long been familiar, and, in the process, shifted the network of theixxory through which it deals with the owlrd. Scixentific fact and theory are not separable categorically, except perhapps within a single tradition of normal scientific practice. That is why unexpectxed scientific discoveryx p7c is not simply factual in its import and why the scientists' world is qualitatively transformed as well as quantitatively enriched by fundamental novelties of either fact or theory.

p8 II - X: how the complementary notions of normal science and scientific revolutions are to be developed

XI: why scientific revolutions have been so hard to see, from examination of textbook tradition

XII: the revolutionary competition between the adherents of the old normal-scientific tradition and the adherents of the new one -- this (not verification) is the only historical source of change of theory

XIII: reconciliation of development through revolution with the apparently unique character of scientific progress

p9 context of discovery - context of justification

K brought up on this distinction but found it, even when applied grosso modo to actual situations in which knowledge is gained, accepted, assimilated, extraordinarily problematic

II. The Route to Normal Science

p 10 Aristotle's physica, Ptolemy's Almagest, Newton's Principia and Opticks, Franklin's Electricity, Lavoisier's Chemistry, Lyell's Geology

served for a time to define implicitly the legitimate problems and methods of a research field for succeeding generations ofpractitiioners.....

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 practitiioners 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 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 realities which the historian describes under such rubrics as 'Ptolemaic astronomy' (or 'Copernican'), Aristotelian dynamics' (or 'Newtonian'), corpuscular optics' (or 'wave optics'), and so on.

p 11 The study of /11/ paradigms, including many that are far more specialized than those ~~xxx~~ named illustratively above, is what mainly prepares the student for memxbership in the particular scientific community with which he will later practice. Because he ~~xx~~ there joins men who learned the bases of their field from the same concrete models, his subseqent practice will seldom evoke overt disagreement over fundamentals. Men whose research is based on ~~xxxxxxxx~~ shared paxradigms are committed to the same rules and standards for scientific practice.

Basic to the understanding both of normal science and of the associated concept of paradigms are the reasons

why is the concrete aachievement, as a locus of professional commitxment, prior to the various concepts, laws, theories, points of viewx, what may be abstracted from it

in what sense is the shared paradigm a fundamental unit for the ~~xxxxx~~ student of scientific development, a unit that canxnot be fully to logically atomic omponents that might function in its stead.

insight?

In particular, both these related concepts (paradigns, normal science [pp NS]) will be clarified by noting that there can be a sort of scientific research without paradigms, or at least without any so binxding and unequivocal as the ones namexd above.

p 12 Scientific revolut9ons: from light as material corpuscles to light as transverse waves to light as photons (ie quxantum-mechanical entitixies some sharacteristics of waves and some of particles).

p 13 Previously no common ~~xx~~ body of belief that could be taken for granted; each writer began from foundations; was fairly free in his choice of supporting observation and experiment, ~~x~~ for no standard set of methods or phenomena that everyone felt forced to ~~xx~~ employ; hence dialogue addressed as much to writerxs in other schools as to nature; pattern not unfamiliar in a number of fiexlds todayx; and it is not incompatible with significant discovery and invention

pp 13, 14: pre-Franklin electricity

p 15: K suggests thata similar situation characterized the

study of motion before Aristotle, of statics befxore Archimedes, of heat beFore Black, of chemistry before Boyle andBoerhaave, of historical geology before Hutton p 15 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, all 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 ~~xx~~ 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 eso~~x~~teric data retrievable fromestablished crafts like medicine, calendar making, and metallur~~k~~gy (henceimportance of technological development).

p 18 When, in the development of a natural science, an individual or group first produces a synthesis able to attract most of the hext generation's practi~~k~~tioners, the older schools gradually disappear.
p 19 Some may cling to old, but ignored by new workers, left in isolation, do not belong to new science but to old often philosophy

usually associatied with a group's first reception of a paradigm are the/formation ~~xxxxxxxxx~~ of specialists' societies, the foundation of specialists' journals, the claim for a special place in the curriculum -- a~~x~~t up till the time when the paraphernalia of specialization acquired a prestige of its own

W~~x~~hen paradigm established, no need for each writer to build field anew by starting from first principles and jstifying /20/ the use of each concept introduced. That can be left to the writer of textbooks. Given a textbook, however, the creative scientist can begin his research where it (TB) leaves off and thus concentrate on the subtlest and most esoteric ~~xxxxxxxx~~ aspects of the natural phenomena that concern his group.

~~XXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXX~~

No longer will his researches usually be embodied in books like/addressed, Franklin's Experiements..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 men whose knowledge of a shared paradigm can be as~~x~~sumed and who prove to be the ~~xxx~~ only ones able to rea~~x~~d the paper addressed to them.

p 20 ff. Transitions from prehistory to history of a science illustrated.

III. The Nature of Normal Science

p 23 Paradigm
not a model for replication: amo amas amat
like an accepted judicial decision in the common law, it is an object for further articulation and specification
gains its position not from success in dealing with one problem or with many
but from a promise of~~xx~~ success discoverable in selected and still incomplete examples

p 24 Normal science consists in the actualization of that promise, an articulation achieved by extending the knowledge of those fact~~x~~s that the paradigm displays as particularly revealing, by extending the/extent of the match between those facts and the paradigms predictions , and by fur~~hx~~ther articulations of the paradigm itself.

(Such) Moping-up operations are what engage most scientists throughout their careers.

No part of the aim of normal scien~~k~~ce 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. Instead, normal scientific research is directed to the articulation of those phenomena and theories that the paradigm already supplies.

p 25 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.... Again and again complex special apparatu~~x~~s has been designed for such purposes, and the invention, constructi~~x~~on and deployment of that apparatu~~x~~s have demanded first-rate talent, much time, and considerable financial /26/ backing.

p 26 A sec~~x~~ond 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. [More obviously than first, dependent on Paradgm p 27 A third class of experiemnts and observations... consists of empirical work undertaken to articulate the paradigm theory, resolving some of its residual ambiguities and permitting the solution of problems to which ithad previously only drawn attention.

Three cases

p 27 The determi~~x~~tation of physical constants. The universal /28/ gravitational constant, Avogadro's number, Joule's coefficient, the electronic charge. Con~~x~~tinuing work on these topics would not have been carried out without a paradigm theory to define the problem and to guaranteee the existence of a ~~xxxxxx~~ stable solutio n.

p 28 The determination of quantitative laws. Boyle's law relating gas presssu~~x~~re to volume, Coulomb's law of electrical attraction, Joule's formula relating heat generated to electical current and resistance. Not simply a matter of measurement; the measuring presupposes a paradigm; often qual~~x~~itative relation guessed before quantitative relation measured. See note 7, p. 29

p 29 The articulation of paradigms.

A paradigm developed for one set of phenomena is ambiguous in its application to other closely related ones. Series of applications of caloric theory. See note 8 page 29.

p 30 The cal~~x~~culaton of the ~~xxxxxxxx~~ theory's implications in practical fields. Astronomical epheme~~x~~rides, the computation of lens characteristics, theproduction of radio propagation curves. Whence new applications of paradigm or increase of precision of application.

Overcoming dif~~x~~ficulty of transition from theory to nature. Newton's Principia had great generality but the descent to the accurate calculation of particulars necessitated the determination of further less general laws and brought about the development of new theoretical techniques. pp 30-34.

p 33 Paradigm articulation involves simultaneously empirical and theoretical ~~xx~~ work. E.g., Coulomb needed electrical theory to determine how his measuring apparatus was to be built, and the measurements he attained involved a refinement of the theory.

p 34 Normal science is engaged in 2) matching facts with theory, 1) determining significant facts, 3) articulating theory.

IV. Normal Science as Puzzle-solving

p 35 Even the project whose goal is paradigm articulat~~x~~ion does not aim at the unexpected novelty

Example from comparison of Coulomb's and earlier work: earlier acquires significance from later theory; anticipation of later theory enabled C to design new apparatus that made earlier anticipation more accurate; the ~~x~~ same anticipat~~x~~ion supplies the reason why no one was surprised by C's results.

p 36 Why do so many for so many years with such enthusiasm work at normal science?

"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 ~~problems~~ puzzles. The man who succeeds proves himself an expert puzzle-solver, and the challenge of the puzzle is an important part of ~~the~~ what usually drives him on."~~s~~

Puzzles are.. that special category of problems that can serve to test ingenuity or skill in their solution.

No criterion that puzzle be intrinsically interesting or important: eg crossword jigsaw puzzles.

p 37 But existence of solution is a criterion: there is no solution to problem set by selecting at random pieces from different jigsaw boxes

The significance of the paradigm is that it provides a criterion for selecting problems that, as long as the paradigm is held to be valid, ensures the existence of solutions.

Other problems are set aside, even those previously entertained, on the ground that they are metaphysical, that they pertain to a different science, that they are just too problematic.

"One of the reasons why normal science seems to progress so rapidly is that its practitioners concentrate on problems that only ~~k~~ their own lack of ingenuity should keep them~~f~~ from solving."

p. 38 All sorts of reasons why people engage in science as pursuit (see page 37).

"Once engaged, his motivation is of a rather different sort. What then challenges him is the conviction that, if only he is skilful enough, he will succeed in solving a puzzle that no one before has solved or solved so well. Many of the greatest scien~~tx~~tific minds have devoted all of their professional attention todemanding 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 in the proper sort of addict.#"

As the notion of puzzle, so too the notion of paradigm not only ensures the existence of a solution but also sets the rules governing achievement of a solution

[Rules in broad preconcetpual sense p 39]

p 39 "The man ~~xx~~ who builds an instruement to determine optical wavelengths must ... show, by analyzing his apparatus in terms of the established body of optical theory, that the numbers ~~x~~ his instrument produces are the ones that enter theory as wavelengths."

Further example from electron wavelength.

Throughout eighteenth century those that attempted to derive the motions of the ~~mm~~ moon from Newton's theory consistently failed. Eventually some suggested a small correction of inverse square law at shorter distances. But to that would be to set up a new paradigm and not to solve the old problem. Eventually the old problem was solved.~~x~~

p 40 What are the main categories in which such rules fall?

1) Explicit statements of scient~~x~~ific law and about scientific concepts and theories.

2) Also, a multitude of commitments to preferred types of instrumentation and to the ways in which accepted instruments may be employed

p 41 3) Quasi-metaphysical commitments: the post-Cartesian assumption as~~x~~sumed the material universe to be a matter of matter in motion (metaphysical) and consequently determined the general character of all scientific laws and explanstions.

p 42 4) At a still higher level, the scientist must be concerned to understand the world and to extend the precision and scope with which scientists have ord~~x~~ered it This in turn must

must lead him to scrutinize, either for himself or through others, some aspect of nature in great empirical detail. If that scrutiny displays aspects of apparent disorder, these must challenge him to a new refinement of his observational techniques or to a further articulation of his theory.

p 42 "Normal science is a highly determined activity, but it need not be entirely determined by rules. That is why, at the beginning 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.

V. The Priority of Paradigms

p 43 Community's paradigms: a) revealed in its textbooks, lectures, and laboratory exercises b) penumbra occupied by achievements whose status is still in doubt. Core of solved problems and techniques usually clear.

Abstracted from more global paradigms are rules deployed in research. Many are easily found, but this search more difficult and less satisfying than search for paradigms.

p 44 No matter how phrased, apt to be rejected by some members of the group. Hence a source of continual and deep frustration.

Group can agree in their identification of a paradigm without agreeing on or even attempting to produce a full interpretation or rationalization ofit.

Indeed the existence of a paradigm need not even imply that any full set of rules exist.

p 45 Wittgenstein on games (p 44)

Though a discussion of some of the attributes shared by a number of games or chairs or leaves often helps to 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.

(They 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 or merged gradually into one another --

p 45 only, that is, if there were no natural families -- wpuld our success in identifying and naming prove 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 fully discoverable set of rules ~~xx~~ 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 scientixfic corpus

p 46 which the community in question /46/ already 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."

46 "That sicientists do not usually ask or debate what makes a particular problem or solution legitimate tempts us to suppose that, at lexast intuitively, they know the answers."

"Paradigms may be prior to, more binxding, and more complete than any set of rules for research that could be unequivocally abstxracted from them."

Reasons for transition from possibility to fact:

First: The severe difficulty in discovering the ~~x~~ rules that have guided particularx normal-scientific traditions. Quite paralle to determining characteristics of games, etc.

Second: the nature of scientific education. One learns concepts laws theories not in absttraction and by themselves but in conjunction with a historically and pedagogically prior unit that displays them with and through their applications.

p47 Theoretically scientists could abstract rules, but there is little reason to show that they do. That they have done so, can be evidenced only by their ability to do research, and research can be done on the basis of the paradigms without a full set of rules

Rules become important (thirdly) when paradigms themselves are felt to be insecure. This is to be expected and it is what happens.

p 48 Such debates however tend more to define ~~xk~~ schools thaxn to produce agreement. Cf optics, electricity, 17th century chemistry, 19th century geology. Many debates over transition from ~~x~~ Newtonian to Quantum mechanics, from earlier ~~xx~~ electromagnetics to Maxwell's equations, from earlier to statistical mechanics.

When scientiststs disagree about whether the fundamental problems of their fiel d have been solved, the ~~fxxxtixxxxf~~ search for rules gains a function that it does not ordinarily possess. While /49/ paraxdigms remain secure, however, they can function without agreement over rationalization or without any attempted rationalization at all.

p 49 Fourthly, besides large revol t9ons there are small ones.

Were the sciences cl9sely knit logical unities, the existence of the ~~xxx~~ small revolutions would be difficult to understand. ~~I~~ When they are based on paradigms, ~~thxx~~ their interconnections are far less rigid; change in one part need not have immediate repercussions on others.

p 50 Currently all physicists learn quantum mechanics, but all do not learn the same applications of these laws. Changes in basic law revolutionary for all physicists, but change in this or that application revolutionary only for a few.

Is the atom of helium a molecuxle? A distinguished chemist said yes, because it behaved like one in the kinetic theory of gases. A distinguished physicist said no, because it displayed no molecular spectrum.

VI. Anomaly and the Emergence of Scientific Discoveries

p 52 "Normal science does not aim at novelties of fact or theory and when suxccessful 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 develeoped a uniquely powerful technique for producing surprises of this sort."

"Discovery commences with the awareness of anomaly, i. e., with the recognition that nature has somehow violated the paradigm-induced /53/ expectatixons that govern normal sciencex."

p.53 "Assimilating a new sort of fact demands more than an 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."

pp 53-62: discovery of oxygen, X-rays, Leyden jar

Complexity of new phenomena not being capable of corresponding conceptualization until a ~~x~~ new theory evolved.

p 63 Illustration from recognizing playing cards that include red spades and black hearts.

p 64 "Initially, only the an~~x~~ticipated and usual are experienced even under circumstances where anomaly is later to be observed."

64-65 Further development (of ~~a~~ early paradigm), therefore, ordinarily ca~~x~~lls for the construction of elaborate equipment, the development of esoteric vocabulary and skills, and a refinement of concepts that increasingly lessens their resemblance to their usual commonsense proto~~x~~types. That profes~~x~~sionalism leads, on theone hand, to an immense restriction of the scientist's vision and to a considerable resistance to paradigm change. The science has becomeincre~~x~~asingly rigid. On the other hand, within those areas to which the paradigm directs the attention of the /65/ 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 ~~xxxxxxxx~~ detail and recision-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 man who, knowing with precision what he 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 the paradigm is, the ~~x~~ more sensitive an indicator ~~xx~~ it provides of anomaly and hence of an occasion of paradigm change."

VII. Crisis and the Emergence of Scientific Theories

p 67 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 in theory.

p 76 "Philosophers of science have repeatedly demonstrated that more thanone theoretical construction can always be placed upon a giv given collection of data. History of science indicates, 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 stages 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. As in manufacture,so in science -- retooling is an extravagance to be reserved for the the occasion that demands it. The significance of crises is the indiation they provide that an occasion for retooling has arrived."

VIII. The Response to Crisis

p 77 ".. 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 scientiric theories, or that experience and experiment are not essential to the process in which they do so. But it does mean -- what will ultiamtely be a central point -- that the act of judgement 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 simulataneously the descision to accept another, and the judgement leading to that decision incolves the comparison of both paradigms with nature and with each other."

p 77 As anomaly to discovery, so counterinstance to new theory.
p 79 "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 phlogiston theory. And Einstein saw as counter-instances what Lorentz, fitzgerald, and others had seen as puzzles in the articulatin of Newton's and Maxwell's theories."
p 90 "What is the nature of that final stage... must here remain inscrutable and may be permanently so.Let us here note only one thing about it. Almost always the men who achieve these fundamental inventions of a new paradigm have been either very young or very new to the field whose paradigms they change.[15]"

Note 15 adds qualifications to the preceding.

IX. The Nature and Necessity of Scientific Revolutions

p 92 "why should a change of paradigm be called a revolution? In face of the vast and/essential differences between political and scientific development, what parallelism can justify the metaphor that finds revolutions in both?

"... Political revolutions are inaugurated by a growing sense, often estricted to a segment of the political community, that existing institutions have ceased adequately to meet the problems posed by an environment 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. 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 new sorts of phenomena, like oxygen or X-rays. Scientific revolutions, as we noted at the end of Section V, need seem revolutionary only to 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."

<u>p 93</u> Transition from genetic aspect of parallel to aspect of contradictory opposition

"Political revolutions aim to change political institutions in ways that those institut9ons themselves prohibit. Their success therefore necessitates the partial relinquishmentof one set of institutions in favor of another and, in the interim, society is not fully governed institutions at all. Initially it is crisis alonge that attenxuates the role of political institutions as we have already seen it attenuate the roleof paradigms. In increasin numbers individuals becomeincrexasingly estranged from political life and behave more and more eccentrically within it. Then, as the crisis deepens, many of these individuals commit themselves to come concrete proposal for the reconstruction of society in a new institutional framework. At that point 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, <u>political recourse fails</u>. Because they differ about the institutional matrixx within which political change is to be achieved and evaluated, because they acknowledge no supra-instititutional 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 /94/ their being partially extrapolitical or extrainstitutional events."

p 94 "Like the choice between competing political institutions, the choice between competing paradigms proves to be achoice between icnompatible modes of community life.... When paradigms enter, as they must, into a debate about paradigm choice, their role is necessarily circular."

"As in political revolutions, so in paradigm choice,-- there is no standard higher than the assent of the relevant community."

Conflict in fact

p 96 "Normal research, which is cumulative, owes its success to the ab9lity of scientists regularly to select problems that can x be solved with conceptual and instrumental techniques close to those already in existence.... Uranticipated novelty, the new discovery, can emerge only to the extent that his anticipations about nature and his instruments prove wrong. ax Often the importance of the /97/ resulting discovery will itself be proportional to the extent and the stubbornness of the anomaly that foreshadowed it. Obviously, then, there must be aconflict between the paradigm that dx discloses anomaly and the one that later renders the anomaly lawlike."

Conflict in theory

p 97 "There are in principle only three types of phenomena about which a xx new theory might be developed. The first consists of pheneomena already well explained by existing paradigms.... [If they arose no crucial experiemnt possible to establish them] 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 most of t the time, but that research aims at the articulaton of existing paradigms rather than at the invention of new ones. Only when these attempts of articulation fail do scientists encounter the third type of phenomena, the recognized anomalies whose characteristic future is their stubborn refusal to be assimilated to existing paradigms. This type alone gives rise to new theories....

"... the successful new theory must permit predictions that are xdifferent from those derived from its predecessor. That difference coold not x occur if the two were logically compatible. In tx the process of being assimilated, the second must displace the first."

pp 98-110 Argument against an "interpretation, closely associated with logical positivism and not categorically rejected by its successors, (that) 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."

X. Revolutions as Changes of World View

p. 115 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 the planet Uranus provides a first example and one that closely parallels the card experiemnt. ~~X~~ On at least seventeen different occasions between 1690 and 1781, a number of astronomers, including several of `urope's most eminent observers, had seen a star in positions that we now suppose must have been occupied at the time by Uranos. One of the best observers in this group had seen the star on four successive nights in 1769 without noting the motion that could have suggested another identification. Herschel ~~himself~~, when he first observed thesame object twelve years later, did so with a much improved telescope of his own manufacture. As a result, he was able to notice an apparent disk-size that was at least unusual for stars. ~~H~~ Something was awry, and he therefore postponed identification pending further scrutiny. That scrutiny disclosed Uranus' motion among the stars, and Herschel therefore announced that he had seen a new comet! Only several months later, after fruitless attempts to fit the observed motion~~s~~ to a cometary orbit, did Lexell suggest that the orbit was probably planetary. When that suggestion was accepted, there were several fewer stars and one more planet in the world of the professional astronomer."

p 118 "... the principle of economy will urge us to say that after discovering ~~ox~~ oxygen Lavoisier worked in a different world."

p 121 ".. though the world does not change with a change of paradigm, the scientist afterward works in a different world... .. I am convinced that we must learn to make sense of statements that at least resemble these."

p 122 "None of these remarks is inteNnded to indicate that scientists do not characteristically interpret observations andd data. On the contrary, ~~E~~ Galigleo ~~ixtxxt~~ interpreted observations on the pendulum, Aristotle observations on falling stones, Musschenbrock observations on a charge-filled bottle, and Franklin observations on a condenser. But each of these observations presupposed a paradigm."

"But that interpretative enterprise -- and this was the burden of the paragraphbefore 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 ultimatelu leads only to anomalies&to crises. And these are terminated, not by deliberation and interpretation, but a relatively sudden and unstructured event ~~ixx~~ like the gestalt switch. Scientists often speak of the "scales falling from their 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 /123/ occasions the relevant illumination comes in sleep. No ordinary sense of the term 'interpretation' fits these flashes of intuition through which a new paradigm is born. Through such intuitions depend upon the experience, both anomalous and congruent, gained with the old paradigm, they are not logically or piecemeal linked to piecemeal 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 of experiences that will thereafter be linked piecemeal to the new paradigm but not to the old."

pp 124-135 deal ~~x~~ with counter hypotheses.

p 124 "But is sensory experience fixed and neutral? Are theories just man-made interpretations of given data? The epistemological viewpoint that has most often guided Western philosophy for three centuries ~~xx~~ dictates and immediate and unequivocal, Yes. In the avsence of a developed alternative, I find it impossible to relinquish entirely that viewpoint. Yet it no longer functions effectively, and the attemptions to make it do so through the introduction of a neutral language of observations now seem to me to be hopeless."

p 126 "The operations and measurements that a scientist undertakes in the laboratory are not "the given" of experience but rather the "collected with difficulty."

"far more dlearly than the immediate experienc e from which they derive, operations and measurements are paradigm-determined."

p. 128 "The alternative is not some hypothetical fixed 'vision', but vision through another paradigm, one which makes the swinging stone something else."

p 135 ".. his (Proust's) view of the relations between mistures and compounds were 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, accept Dalton's theory simply on the evidence, for so much of that was still negative. Instead, even after accepting the theory (compounds are composed of integral numbers of dofferent kinds of atoms), they had still to beat nature into line, a process which, in the event, took almost another generation. When 9t 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 different worlds."

XI. The Invisibility of Revolutions

p 136 "I have so far tried to display revolutions by illustration, and the examples could be multiplied ad nauseam. But clearly, most of them,... have customarily been viewed, not as revolutions, but as additions to scientific knowledge."

"Both scientists and laymen take much of their image of creative scient9fic activity from an authoritative source that systematically disguises -- partly for important functional reaons -- the existence and significance of scientific revolutions."

".. 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."

p 136 "Textbooks themselves aim to communicate the vocabulary any syntax of a contemporary scientific language. Populariz~~xx~~ations attempt to describe these same applications in a language ~~xkxxx~~ /137/ closer to that of everyday li~~x~~fe. And philosophy of science, particularly in the English-speaking world, analyzes the logical structure of the same completed body of scientific knowledge.... All three record the stable ~~x~~outcome of past revolutions and thus display the bases of the current normal-scientific tradition.~~XX~~ To fulfil their function they need not provide authentic / information about the way in which these bases were first ~~xxxxxx~~ 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."

p137 "Textbooks how~~x~~ever being pedagogical vehicles for the perpetuation of normal science have to be rewritten in whole or part whenever the language, problem-structure, or standards of normal science change. In short, they have to be rewritten in the aftermath of eac~~x~~h scientific revolution and, once rewritten, they inevitably disguise the revolutions that produced them. Unless he has personally experienced a revolution in his own lifetime, the historical sense either of the working ~~xxxx~~ scientist or of the lay reader of textbook literature extends only to the outcome of the most recent revolutions in the field."

p 138 "Yet the textbook-derived tradition in which scientists come to sense their participation is one that, in fact, never existed. For re~~x~~asons that are both obvious and highly functional, scien~~x~~ce 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 thestatement and solution of the texts' paradigm problems. Partly by selection and partly by ~~x~~ distortion, the scientists of ~~xx~~ earlier ages are implicitly represented as having worked upon the same set of fixed problems and in accordance with the same set 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 ~~x~~ after each scientific revolution. And no wonder that, as they are rewritten, science once again comes to seem largely cumulative."

p 138 "The temptation to write history backwardx 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 on the historical context of the inquiry and partly because, except during crisis and revolution, the scientist's contemporary position seems so secure. More historicaldetail, whether of science's present or of its past, or more responsibility xx to the historical details that are presented, could only give artificial status to human ix idiosyncracy, error, and confusion. Why dignixfy what science's best and most persistent efforts have made it possible to discard? The depreciation of historical fact is deeply, and perhaps functionally, ingrained in the ideology of the scientific proxtfession, the same profession that places the highest of all values upon factuxal details of other soxrts. Whitehead caught the unhistorical spirit of the scientific community when he wrote,'A science that hesitates to forgext its founders is lost.' Yet he was not quite /139/ 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."

p 139 "The result is a persistent tendency to make the history of sciencex look linear or cumulative, a tendency that even x affects scientists looking back on their own work For example, all three of Dalton's xxxxxxx incompatible accounts of of the development of his chemical atomism make it appear that he was interested from an early date in just those chemical problems of comgining proportions that he was later famous for having solved. Actually those problems seem only to have occurred to him with their solutions, and then not until his own creative work was very nearly complete. What all of Dalton's accounts omit are the revolutionary x effect ofx 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 x toward the field, a reorientation that taught chemists to ask new questions about and to draw new conclusions from old data."

p. 139 "Or again, Newton wrote that G·lileo had discovered that the constxant force of gravity proxduces a motion proportional to the sqxuare of the time. In fact, G lileo's kinematic theorem does take that form when embebbed in the matrix of xxx,Newton's own dynamical concepts. But Galileo said nothing of thex sort."
p 140 "But9 it is just thisx sort of change in the formulation of questions and answers that accounts, farx more than novel empirical discoveries, for the transition from Aristotelian to Galilean and from Galilean to Newxtonian dynamics. By disguishing such changes, the textbook tendency to make the development of science linear hides a process that lies at thek heart of the most significant episodes of scientific development."

p 141 These questionsxx 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 prexsents as theories. Those theories of course do fx 'fit the facts,' but only by transforming previously accessible information into facts that, for the preceding paradigm, had not existed at all. And this 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 relatinship between the scientist and nature was not quite the same."

p 141 "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, Robexrt Boyle, in whose Sceptical Chymist the attentive reader xx will find a definition of 'element' quite close to that in /142/ use today."
p 142 "According to Boyle, x who was quite right, his 'definition' of element was no more than a paraphrase of a traditional chemical concept; Boyle offered it only to argue that no such thing as a chemical element existxs; as history, the textxbook version of boyle's contribution is quite mistaken. That mistake of course is trivial, kxx though no more so than any other misrepresentation of data. What is not trivial however is the impxression of science fostered when this sort x of mistake is first compounded and

then ~~x~~ 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 modern concept of an element since antiquit~~x~~y. Verbal definitions like Boyle's have little scientific content 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 context. Furthermore, given the context, ~~that~~ they rarely require to be invented because they are already at hand. Both Boyle and Lavoisier ~~xxxxxxxxx~~changed the chemical significan~~x~~ce of 'element' in important ways. But they did not invent the notion /143/ or even change the verbal formulation 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 ~~xx~~ within the context of his work."

XII. The Resolutionof Revolutions

p. 144 Any new interpretation of nature, whether a discovery ~~xf~~ or a th~~x~~eory, emerges first in the mind of one or of a few individuals. It is they who first ~~xxx~~ 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 concentrated on the crisis-provoking problems; usually ~~x~~ in addition they are men so young or ~~x~~ 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....

To see the urgency of those questions (ie how do they persuade others), ~~x~~ 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 (Research worker in normal science is not tesxter of theories xx but a puzzle-solver).

p145 Bx "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 theorxies to explain the evidence at ~~xx~~ hand. That 9nsistence on comparing theories a]so characterizes the historical situation in which a new theory is accepted. Very probably it points one of the directions in which fuxture discussion of verification should go.

In their moxre usual forms however probabilistic verification theories all have recourse to one or another of the pure or neutral observation languages discussed in Section X. (Compare with all other theories that might be imagined to fit the same data, or with all the tests that might be imagined, some such comparison xxx needed to give probability its mathematical meaning)

p146 "Verification is like natural selection: it picks out the most viable among the actual alternatives in a particular historical situation."

p146 Popper sets aside verification and speaks of falsification.

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But either no failures whatever (a test no theory xx can meet) or else relatively fewer failuxxrxes (and then there is need for a criterion of improbability or degrees of falsification).

p 147 Both falsification of earlier and verification of later

"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 hx hesitated more than a decade in concluding that Lavoisier's theory provided the better fit of the two."

p148 "The proponents of competing ~~thxxxixx~~ papadigms are always at least slightly at cross-purposes. Neither side will grant all the non-empirical assumptions that the other needs to ~~xxxxx~~ make its case.... Though each may hope to convert the other to his way of seeing his science and its problems, neither may hoxpe to prove his case. The competition between paradigms is not the sort of battle that can be resolved by proofs."

Incommensurability ~~xxxxxxxxxxxxxxx~~ of pre- and post-revolutionary paradigms

First, ".. the proponents of competing paradigms will often disagree about the list of problems that any cnadidate for paradigm must resolve. Their standards or their ~~xxxx~~ definitions of science are not the same. (Must science explain what causes the attraction between bodies: Aristotelians and Cartesians, Yes, Newtonians, No. Must chemical compounds have their qualities accounted for, Lavoisier etc, No, Phlogiston theoriests, Yes, Current theories, attempt to do so).

p149 Secondly, though new paradigm takes over a great deal of its predecessor, it also reinterprets what it takes over. Anti-Einstein not mistaken in complaining that what E called space was unheard of. For pre-Copernicans the earth was the exemplaxr of what did not move.

p150 Thirdly, there may be involved transition to a new "world"

"One contains constrained bodies that fall slowly, the other pendulums that repeat their motions again and again. In one solutions are compounds, in theother mixtures. One is embedded in flat, the other in a curved matrix of space. Practicisng in different worlds, the two groups of scientists see different things when they look from the same point and in the same direction. Again, that is not to say they can see anything they please. Both are looking at the world, and what they see has not changed. But ~~x~~ in some areas they see different things, and they see them in different relations ~~xxxxxxxxxxxxxx~~ one to the other. That is why a law that cannot even be demonstrate to one group of scientsts may occasionally seem intuitively obvious to ~~xxx~~ another.x 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 the 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 ~~xxxxx~~

p150 swxitch, it must occxur all at once (though not necessarily in an instant) or xx not at all."

"How then are scientists brought to make this transposition? Part of the answer is that they are veryx often not. Copernicanism mase few converts for almost a century after Copernicus' death Newtonx's work was not generally accepted, particularly on the Continent, for more than x half a centxury after the Principia appeared. Priestley never accepted the oxygen theory, nor Lord Kelvin the lectronic theory, and so on. The difficulties of conversion have often been noted by scientists themselves." Quotexs from Darwin, Planckk.

p151 "These facts and others like them are too commonly known to need further emphasis. But they do need re-evaluxation. 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.x The transfer of allegiance from paradigm to paradigm is a conversion experience that canxxnot 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 scientixfic research itself. The source of xxx resistance is the assurance that the older paradigm will ultimately solve all its problems, that nature can be shoxed /152/ into the box the paradigm provides."

p 152 "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 persauded tp 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 xxx again be practising under a single but now different paradigm We must therefore ask how conversion is induced and how xx resisted."

p152 "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 notx previously been undertaken. We shall have to settle for a very partial and impressionistic survey "

p153 "Our concern will not be then with the sort of 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 be made legitimately, this claim is often the most effective one possible."
Copernicus: problem of length of calendar year
Newton: reconciliation of terrestrial and celestial mechanices
Lavoisier: problems of gas-identity and weight relations
Einstein: electrodynamics compatible with revised science of motion

"Claims of this sort are particularly likely to ~~x~~succeed if the new paradigm displays a quantitative precision strikingly
154/ better than/its older competitor."
Kepler: quantitative superiority of his Rudolphine tables over Ptol.
Newton: prediction of quantitative astronomical observations
Planck's radiation law and Bohr's atom

p154 f Claim of problem solving cannot always be made legitimately but later and unanticipated successes extremelyx effective

p155 Second, ".. the new theory is said to be "neater," "more suitable," or "simpler" than the old."

Though more effective in maths than in science, at times this argument may be very important. It may attract a few scientists and their influence can in time be decisive.
The reason for this is that initially the new paradigm solves few problems accurately and ddefenders of the old have the assurance generated by its past successes in overcomeing difficulties

p157 "In short, if a new candidate for paradigm haxd 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 previsously have called the incommensurability of paradigms, and the sciences might experience no revolutions at all."

"But paradigm debates are not really aabout relative problem-solving ability, though for good reasons they areusually couched in those terms. Instead, the issue is which ~~pxx~~ 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 dexcision must be based less on /158/ past achievement than on future promise. The man who embraces new paradigm at an early stage must often do so in defiance of the evidence provided by problem-solving. He must, that is, have faith that the new paradigm will succeed with the many large ~~p xxxx~~ problems that confront it, know only that ~~xx~~ older paradigm has failed with a few. A decision of that kind can only be made on faith."

p158 "This 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 what may easily prove and will be widely regarded as a will-o'-the-wisp.... Something must make at least a few scientists feel tha t the new proposal is on the right track, and sometimes it is only perszonal and inarticulate ~~xx~~ aesthetic considerations that can do that. Men have been converted by them at times when most of the articulable technical arguments pointed the other way." Illustrations from Copernicus, de Broglie, Einstein's general relat.

"If a paradigm is ever to triumph it must gain some first supporters, men who will develop it to the pint where hard-headed arguments can be produced and multiplied. And even those arguments when they come are not individually decisive. Because scientists are reasonable men,one or anotherargument will ultimately persuade many of them. But there is no single argument that could or should persuade them all. Rather than a single group converionsx, what occurs is an increasing shift in the distribution of professional allegiances."

p159 "At the start a new candidate for paradigm may have few supporters, and on occasion the supporters' motives may be suspect. Nevertheless, if they are competent. they will improve it. explore its possibilities, and ~~xhx~~ show what it would be like to belong to the community guided by it. And so it goes on, if the paradigm is one destined to win the fight, the number and strength of the persuasive arguments in its favor will increase. More scientists will be converted, and the exploration of the new paradigm will go on. Gradually the number of experiments, instruments, art9icles, and books based upon the paradigm will multiply. Still more men convinced of the new view's fruitfulness will adopt the new mode of practising normal science, until at ~~ix~~ last only a few elderly hold-outs remain~~x~~. And even they, we cannot say are wrong. Though the historian can always find men -- Priestley for~~x~~ instance -- who were unreasonable to resist for as long as they did, he will not find a point at which resistance becomes illogical or unscientific. At most he may wish to say that the man who continues to resist after his whole profession has been converted has ipso facto ceased to be a scientist/."

XIII. Progress through Revolutions

p160 "Why should the enterprise sketched above more ahead steadily in ways that, say, art, political theory, ~~phixx~~philosophy do not?"

"CAn very much depend on a definition of science? Can a definition tell whether he(a man) is a scientist or not?... Probably questions like the following are really being asked: Ehy does my field fail to move ahead in the way that, say, physics does? What changes in method or technique or ideology would enable it to do so? These however are not questions that could respond to an agreement on definition Furthermore, if /161/ precedent fr m the natural sciences serves, they will cease to be a ~~x~~ sou~~x~~rce of concern not when a definiti~~x~~on is found but when the groups that now doubt their own status achieve consensus about their ~~ix~~ past and present accomplishments. It may, for instance, be significant that economists argue less about their own field as a science than do practitioners of other fields of social science. Is that because economists know what science is? or is it ~~xxxxxxx~~ rather economics about which they agree?

p162 "Viewed from within any single community, however, whether of scientists or non-scientists, the result of successixxful creative work is progress. How could it possibly be anything else? .... No creative school (he is referring to artists, theologians, philosophers) 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 schools make progress, that canmnot be that individuals schools make none. Rather it must be because there are always /163/ competing schools, each of which constantly questions the very foundations of the others."

p163 "With respxect to normal science, then, part of the answer lies 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. W have, for xxampx example, already noted that once the reception of a common paradigm has x freed the scientific community from the need constantly to re-examine its first principles, the members of that community can concentrate exclusively upon /164/ the subtlest and most esoteric of the phenomena that concern it. Inevitably that does increse 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."

p164 "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 discussing now 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.... Just because he is working only for an audience of colleagues, an audience that shares his own values and beliefs, the scientist can take a single set of standards for granted. He 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 (? hexterogeneous) group. Even more important, the insulation of the scientific community from society permits the individual scientist to concentrate his attention on problems that he has good reason to believe he 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. The latter often tend, as the former never do, to defend their choice of a x research problem -- the effects of racial discrimination or the causes of the business cycle -- chiefly in terms of ~~xshx~~ the social importance of achieving a solution."

p164f "... another characteristic of the professional scientific /165/ community, the nature of its educational initiation. In music, the graphic arts, and literature the practitioner gains his educkation by exposure to the works of other artists, principally earlier artists. Textbooks, except compendia or handbooks to original creations, have only a secondary role. In history, philosophy, the social sciences, textbook literautre has a greated 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 anyone of these disciplines is constantly made aware of the immense variety of problems that the members of his future group have, in the course of time, attempted to solve. Even more important, he has constantly before him a number of competing and incommensurable solutions to ~~thxx~~ these problems, solutions that he must ultimately evaluate for himself."

"Contrast this situation with that at least in the contemporary natural sciences. In these fields the student relies mainly on textbooks until, in his thrrd or fourth year of graduate work, he begins his own research.... 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 ~~i~~ their paradigms, which makes this educational technixque ~~px~~ 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, Schrödinger, when everything he needs to know is recapitulated in a far briefer, more precise, and more systematic form in a number of up-to-date textbooks?"

p166 "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. Further more, he is well equipped for another task as well -- the generation through normal science of signifciant crises. ... scientific training is not well designed to produce the man who will discover a fresh approach. But so long as somebody appears with a new candidate for paradigm -- usually a young man 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 ~~xx~~ 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."

p166 "Why should progress also be the apparently universal concomitant of scientific revolutions?"

p167 "When it repudiates a pst paradigm, a scientific community simultaneously renounces, as a fit subject for professional scrutiny, most of the books and articles in which that paradigm has been embodied. Scientific education makes no use of the equivalent for the art museum or the ~~x~~ library of classics, and the result is a sometimes drastic distortion in the scientist's perception of his discipline's past. More than the practitioners of other creative fields, he comes to see it as leading in a straight line to the discipline's present vantage. In short, he comes to see it as progress. No alternative is available to him while he remains in the field."

".. no explanation of progress through revolution may stop at this point. To do so would be to imply that in the sciences might makes right.... If authority alone, and particularly if non-~~xxxxxxxxxx~~ professional authority, were the arbiter of paradigm debates, the outcome of those debates might still be revolution, but it would not be <u>scientific</u> revolution. The very existence of science depends ~~x~~ upon vesting the power to choose 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.

p168 "... only the civilizations that desond from Hellenic Greece have possessed more than the most rudimentary science."

[vastly more study needed] 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 his concern with nature may be global in its extent, the problems on which he works must be problems of detail. Most important, the solutions that satisfy him 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 scientistx's professional compeers. One of the strongest, if still unwritxten rules of scientific life is the prohibxxition 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 x professional achievement has further implications. The gxroup's members, as individuals and by virtue of their shared training and experience, must be seen as sole possessors of the rules of the game or of some equivalent basis for unequivocal judgements. 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 xxx truth in the sciences can be one."

p169 (The foregoing).. does suggest that a community of scientific specialists will do all it can to ensure the continuing growth of the assembled data tjat it can treat with precision and detail. In the process the community will sustain losses. Often some old problems will be banished. Frequently, in addition, revolution narrows the scope of the community's professional concerns, increases the extent of its specialization, and xattenuates 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 doesx so, that breaxdth is manifest mainly in the proliferation of 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 and the precision of individual problem-xsolutions will grow and grow."

p170 "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."

p171 Parallel difficulty in Darwinism: evolution but not to some ideal goal.

p172 "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."

"And the entire process may have occurred, as we now suppose biological evolution did, /173/ 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."

p173 "Anyone who has followed the argument thus far will nevertheless feel the need to ask why the evolutionary process should work. What must nature, including man, 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 these questions, excepting the first, have already been answered. But from another they are as open as theywere when this essay began. It is not only the scientific community that must be special. The world of which that community is part must also possess quite special characteristics, and we are no closer than we were at the start to knowing what those characteristics must be...."

Postscript 1969

p174 ".. almost seven years since this book was first published. ... increased understxkanding on a number of the issues.... On fundamentals my viewpoint is very nearly unchanged, but I now recognize aspects of its initial formulation that create gratuitous difficulties and mixsunderstadmxndings."

Imre Latakos and Alan Musgrave, eds.,Criticism and the Growth of Knowledge (Cambridge 1970). x Reference Criticism.

Frederick Suppe, ed., The Structure of Scientific Theories (Urxbana, Ill. 1970 or 1971). Reference Structure.

T. Kuhn, "Reflections on my Critics," Criticism

T. Kuhn, "Second Thoughts on Paradigms," Structure

Margaret Masterman, "The Nature of a Paradigm," Criticism
Dudley Shapere, "The Structure of Scientific Revolutions," Philosophical REview, 73 (1964), 383-94.

Roger Poole, Towxards Deep Subjectivity, London: Allen Lane The Penguin Press, 1972. On Kuhn and critics, pp. 49-54.

p175 "That procedure (disentangling notion of paradigm from notion of scientific community) quickly discloses that in much of the book the term 'paradigm' ia 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.

1. Paradigms and Community Structure

p176 "If this book were being rewritten, it would therefore ~~pixm~~ 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."

p177 "A scientific community consists on this view (sociological) of the practitioners of a scientific specialty. To an extent unparalleled in most other fields, they have undergone similar education and professional initiations; in the~~x~~ 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, that approach the same subject from incompatible viewpoints. But they are far rarer 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 men uniquely responsible for the pursuit of a set of shared goals, including the training of their successors. Within such groups communication is relatively ~~x~~full and professional judgement relatively unanimous. Because the atten~~x~~tion of different scientific communities is, on the other hand, focused on different matters, professional communication ~~xxxxxx~~ across group lines is sometimes arduous, often results in misunderstandings, and may, if pursued, evoke significant and previously ussuspected disagreement."

Groups, subgroups, sub~~x~~groups (sub), their shifts over time, &c., can be worked out in du~~x~~e course. Significant is:

p178 ". the transition form the pre-to the post-paradigm period in a given field... Before it ~~x~~ occurs, a number of groups compete for the domination of a given field. Afterward, in the wake of some notable scientific achievement, the number of schools is greatly ~~xxxxxxxxx~~ reduced, ordinar~~x~~ily 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 only be when its members take the foundations of their field for granted."

p179 "The members of all scientific communities, including ~~xxxxx~~ 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 normalX 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 di~~x~~cuss as as consequences of the acquisition of that sort of paradigm that identifies challenging puzzles, supplies clues to their solution, and guarantees that the truly clever pratitioner will succeed."

p179 "Both normal science and revolutions are.. community-based activities. To discover and analyze them, one must unrav~~x~~el the changing community structure of the sciences /180/ 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."
p178 acknowledges this neglected in first edition.

p180 "Paetly because... 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. Aclearer delineation of community structure, however, should help to enforce the rather different impression I have tried to create. A revolution /181/ is for me a special sort of ~~xkx~~ change involving a certain sort of reconstruction of group commitments. But it need not be large, nor need it seem revolutionary to those outside a signle 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."

p181 Crises not an absolute prerequisite. They supply a self-correcting mechanism for regi~~x~~dity of normal science. May be generated in one community and felt in another. EG electron microscope, Maxwell's equations.

p182 "Having isolated a particular community of ~~xxpxrtx~~ specialists by techniques like those just discussed, one may usefully ask: what do its members ~~xkxx~~ share that accounts for the ~~x~~relative fulness of their professional communication and the relative unanimity of their professional judgements."

K's early edition would answer 'paradigm' or 'set of paradigms.' Scientists would say 'theory' or 'theories'. K. now would say disciplinary matrix: 'disciplinary because p182 "... I sufggest: '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."

(1) "One important sort of component I shall label 'symbolic generalizations'...."

p183 Eg f = ma; I - V/R; elements combine in constant proportion by weight.

These look like laxws of nature but they may also function as definitions or parts of definitions of the terms they employ. Laws can be revised, but definitions are tautologies. Further, from a genetic viewpoint, the law may be a redefinition of the terms employed; eg Ohm's law involved a redefinition of both current and resistxance.

p184 ".. allx revolutions xxxx 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 he alter the notion of simultaneity x itself?"

(2) p184 ".. a second type of component of the disciplinary matrix... now I would describe... as beliefs in particular models, and I would expand the category, xx models, to include also the relatively heuristic variety: the electric current may be regarded as a steady-state hydronamic system; the molecules of a gas behave like tiny elastic billiard xx balls in random motion. Though the strength of group commitment varies, with non-trivial consequences,, along the spectrum from heuristic to ontological 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."

K notes that members of community need not share heuristic models even: chemists in first halp of XiX century did not all accept atomic theory.

(3) p184 "A third sort of element in the disciplinary matrix I shall here describe as values. /185/ ... Probably the most deeply held values concern predictions: quantitiative predictions are preferable to qualitative ones; whatever the margin of permissible error, it should be consistently satisfied in a given field; and so on. ... whole theories... must first and for~~m~~emost permit puzzle formation and solution; where possible they should be simple, self-consistent, and ~~px~~ plausible, compatible, that is, with other theories currently deployed"... Also socially useful...

p185 ".. values may be shared by men who difer in their / application. Judgements of accuracy are relatively.. stable.. But judgements of simplicity, consistency, plausibility, and so on vary greatly from individual to individual." Eg Einstein vs caeteros on QM.

p186 "Because I ~~s~~ 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 on ~~f f~~ glorifying subjectivity and even irrationality.~~X~~ But that reaction ignores two characteristics displayed by ~~tt~~ value~~x~~ judgements in any field. First, shared values can be ~~imxxmi~~ important even though the members of the group do not all apply them in the same ~~wy~~ way.... Second, individual variability in the application of shared values may serve functions essential to science. The ~~px~~ points at which values must be applied are invariably 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 ~~xxxmi~~ respon~~x~~ded to each anomaly as a source of crisis or embraced each new theory advanced by a ~~mit~~ 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~~x~~ choice may be the community's way of distributing risk and assuring the long-term success of the enterprise."

(4) p186 "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 philologically and autogiographically /187/; this is the component of the group's shared commitments which ~~it~~ first led me to a 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 scientixfic education, whether in laboratories, on examinations, or at the ends of chapters in ~~text~~ 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.x More than other sorts of components of the disciplinary matrix, differences between sets of exemplars provide the community fine-structure of science."

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3. Paradigms as Shared Examples

p187 "He (the student) cannot, it is said, solve problems at all unless he 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 howevwr that this localization of /188/ the cognitive content of science is wrong. After the student has done many problems, he may gain only added facility by soxlving more. But at the start&for some time afterward, doing problems is learning consequential things about nature. In the absence of such exemplars, the laws and theories he has previously learned would have little empirical content."

Ibid Newton's second law embodied in free fall, pendulum, pai of interacting harmonic oscillators, giroscope.

p189 "A phenomenon familiar to both students ~~x~~ of science and historians of science provides a clue. The former regularly report that they have read through a chapter of t eir text, understood it perfectly, but nonetheless had difficulty solving a number of the problems at the chapter's end. Ordinarly, also, these difxficulties dissolve in the same way. The student discovers, with ~~xx~~ or without the assistance of his instructor, a way to see his problem as <u>like</u> a problem he has already encountered. Having seen the resemblance, grasped the analogy between two or more distinct ~~problems~~, he can interrelate symbols and attach them to nature in ways that have proved effective before..... The resultant ability to see a variety of situations as like each other.. is, I think, the main thing a student acquires by doing exemplarxy problems, whether with a pencil and paper or in a well-designed laboratory. After he has completed a certain number... he views the situations that confront him as a scient~~i~~ist in the same gestalt as other members of his specialists' group."

"The role of acquired similarity relations also shows clearly in the history of science. Scientists solve problems by modeling them on previous puzzle-solutions, often with only minimal recourse /190/to symxbolic generalizations."

Galileo down one plane and up another; Huyghens pen~~x~~dulum

p190 "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 scientiric law or law-sketch. Simultaneously it should show why I refer to the consequential knowledge of natuxre acquired while learning the similarity relationship and thereafter embodied in a way of viewing /191/ physical situations rather than in rules and laws."

p191 "That sort of learning (what is meant by 'Actual descent equals potential ascent') is not acquired by exclusively verbal means R ther it comes as one is give words together with concrete examples of how they function in use; nature and words are learned together. To borrow once more michaxel 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."

P

4. Tacit Knowledge and Intuition

p196 "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."

".. what has just been said about sensation is meant literally... 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." (K acknowledges metaphor and its insufficiency but proposes computer programs etc)

p198 "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

p198 "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...."

"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 men who concur in honoring them. If two men disagree, for example, about the relative fruitfulness of their theories, or if they agree about that but disagree about the importance of fruitfulness and, say, scope in reaching a choice, neither can be /200/ convicted of a mistake."

p200 However fascinating in themselves, details of biography or personality are not the point here. The point is:

p200 "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 incommensuarable viewpoints."

Further specification of the nature of the difficulty:

p200 "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 reltions ~~x~~ change. Objects that were grouped in the same set before are grouped in different ~~xxtxx~~ 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...

p201 "Not surprisingly therefore when such redistributions occur, two men whose discourse had previously proceeded with apparently full understanding may suddenly find themselves responding tp the same stimulus with incompatible descriptions and generalizations."

"Such problems, though they first become evident in communication, ~~xx~~ 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 communicaton breakdown cannot say, 'I use the word element... 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 sttement of both their thoeries or even of both those theories' empirical consequences. Part of the difference is prior to the application of the language in which it is nevertheless reflected."

p202 "Briefly put, what the participants in a communication brea~~x~~kdown can do is recognize each other as members of different language communities and then become translators.....

"If they can suffi~~x~~ciently ref~~xx~~rain from explaining anomalous behavior as the consequence of mere ~~x~~ error or madness, they may in time become very good predictors of each other's behavior. ~~X~~ Each will then have learned to translate the other's theory and its ~~language~~ consequences into his own language and simultaneously to describe in his own language the world in which that theory applies. That is what the distorian of science regularly does (or should) when dealing with out-of-date scientific theories."

p203 "If the new viewpoint endures for a time and continues to be fruitful, the result results verbalizable in this way are likely to grow in number. For some men such results alone will be decisive. They can~~x~~ say: I don't know how the proponents of the new ~~x~~ 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."

p204 A further aspect of the matter:

"To translate a theory or world view into one's own language is not to make it one's own. For that one must go native, discover that one is working and thinking in, not simply translating out of, a language that was previously foreign. That transiton 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, he finds that the transition has occurred, that he has slipped into the new language without a ~~xxxxx~~ decision having ~~xx~~ been made. Or else, like many of those who first encountered relativity or quantum mechanics in their middle years, he finds himself fully persuaded of the new ~~x~~ view but neverthelesss unable to internalize it and be at home in the world it helps to shape. Intellectually such a man has made his choice, but the conversion required if it is to be effective eludes him.

He may use the new theory nevertheless, but he will do so as a foreigner in a foreign environment, an alternative available to him only because there are natives already there. His work is parasitic on theirs, for the lacks the constellation of mental sets which future members of the community will acquire through education."

"But neixther good reasons nor translation constitute conversion, and it is that process we must explicate in order to understand an essential sort of scientixfic change."

6. RXevolutions and Relativism

p206 "Later scientific theories are better than earlier ones for solving problems 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."

"One foten hears that successive theories grow ever closer to, or approximate more and more closely to, the truth. Apparently generalizatios like thxat refer not to the puzzle-solutions and the concrete predictions derived from a theory but rather to its ontology, xx 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 by the implausibility of xx the view. I do not doubt 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 /207/ xx important respects , though by no means in all, Einstein's general theory of relativity is geared closer to Aristotle's that either of them is to Newton's."

7. The Nature of Science

p207 ".. that I repeatedly pass back and forth between the descriptitve and normative modes...."

"The preceding pages present a viewpoint or theory about the nature of science... the theory has consequences for the way in which scientists should behave if their enterprise is to succeed. Though /208/ 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."

p209 Differences of science from other fields

"Consider.. (1) the relative scarcity of competing schools in the developed sciences.... (2) .. the extent to which the members of a given scientific community provide the only audience and the only judges of that community's work... (3).. the special nature of scientific education, (4).. puzzle-solving as a goal.. (5).. the value system which the group deploys in periods of crisis and decision."