The Route to Normal Science

In this essay, 'normal science' means research aimed at resolving a group of related problems. For the 'normal-science' world, the history of scientific practice today is described as a gradual accumulation of knowledge and practice, with the scientific community gradually moving towards a more unified and predictable set of standards. This process involves the development of a paradigm, which is a set of shared assumptions and practices that guide research in a particular field. The American sociologist Thomas Kuhn introduced the concept of paradigms to describe the way in which scientific communities develop and change over time.

The route to normal science involves the following stages:

1. The excitement of uncontrolled research: In the early stages of a scientific field, there is an abundance of research, with many different approaches being pursued.
2. The consolidation of a research field: As research progresses, a consensus begins to emerge around a particular set of research questions, methods, and results.
3. The development of a paradigm: Once a consensus has been reached, a paradigm is formed, which is a set of shared assumptions and practices that guide research in a particular field.
4. The normal-science phase: Once a paradigm has been established, the focus shifts to the refinement and expansion of the paradigm, with the goal of extending its applications and understanding.

Kuhn argued that the normal-science phase is characterized by a focus on the refinement and expansion of a paradigm, rather than the discovery of new research questions or methods. This phase is marked by the development of increasingly elaborate and sophisticated models, which are used to explain a wide range of phenomena. However, Kuhn also noted that the normal-science phase is not static, but rather is subject to periodic crises that can lead to the development of new paradigms.

The route to normal science is not a linear process, but rather a series of stages that are characterized by changes in the structure of scientific practice. As scientific communities develop and change, they also adopt new ways of thinking and doing research, which can lead to the development of new paradigms. The route to normal science is therefore an ongoing process, with scientific communities continually adapting and evolving in response to new challenges and opportunities.

In summary, the route to normal science involves the development of a paradigm, which is a set of shared assumptions and practices that guide research in a particular field. This process involves the development of increasingly elaborate and sophisticated models, which are used to explain a wide range of phenomena. However, the normal-science phase is not static, but rather is subject to periodic crises that can lead to the development of new paradigms. The route to normal science is therefore an ongoing process, with scientific communities continually adapting and evolving in response to new challenges and opportunities.
The Structure of Scientific Revolutions
The Study of Scientific Revolutions

The Routine in Normal Science

The Study of Scientific Revolutions

The Routine in Normal Science
The Routledge History of Science, 3rd Edition


Chapter 7: The Route to Normal Science

For what is Normal Science? It is the period during which scientists
are engaged in the process of refining and extending the theoretical
framework of a discipline. During this time, scientists build on the
theoretical foundations established in the preceding period of
paradigm change. They work within the constraints of the existing
paradigm, making incremental improvements and advances that
build on the core ideas of the paradigm. Normal Science is
characterized by the pursuit of research questions that are
consistent with the paradigm, and it is during this period that
scientific progress is typically measured.

In contrast, the period of paradigm change is marked by the
emergence of new theoretical frameworks and the questioning of
existing paradigms. During this time, scientists may engage in
the process of paradigm shift, where new theories and
methodologies are developed that challenge the existing
paradigm. This period is often characterized by significant
advances, but it also involves significant risk and uncertainty.

The study of scientific revolutions highlights the dynamic
interaction between these two periods, as the process of
paradigm change leads to the eventual overthrow of old
paradigms and the establishment of new ones. This process
is not linear, and it often involves periods of societal
resistance and controversy.
The Structure of Scientific Revolutions

The route to normal science, with detailed explanations for understanding scientific revolutions and the process of scientific change. This involves the development of new paradigms that eventually replace old ones as a result of accumulating evidence and new discoveries. The key to understanding scientific revolutions is recognizing the transition from one paradigm to another, where the old paradigm is eventually abandoned in favor of a new one that better explains the data and phenomena under study.

The route to normal science is characterized by the gradual accumulation of evidence that supports a particular paradigm. This process is driven by the pursuit of explanatory power, where scientists strive to develop theories that can explain a wide range of observations. However, as new evidence emerges, it may challenge the existing paradigm, leading to the emergence of a new one. This transition is not always smooth and can involve periods of crisis and reorganization.

The Structure of Scientific Revolutions is a seminal work in the philosophy of science, providing a framework for understanding the dynamics of scientific change and the role of paradigms in scientific progress. It challenges traditional views of scientific progress and offers a more nuanced perspective on the nature of scientific knowledge.

It is crucial to understand that the route to normal science is not a linear process but rather a complex one involving periods of crisis and reorganization. This understanding is essential for anyone interested in the history and philosophy of science, as it provides insights into the nature of scientific inquiry and the process of paradigm shifts.
The purpose of this section is to explain the process of scientific revolutions. This involves understanding the nature of scientific change and the factors that lead to new scientific paradigms. The key idea is that revolutions in science are not just random events but are part of a larger process of change that is driven by a number of factors, including new evidence, changes in the social context, and interactions between scientists. The process of a scientific revolution involves the rejection of an old paradigm and the adoption of a new one.

In the example text provided, the author discusses how the development of new scientific ideas can lead to a shift in the dominant paradigm. This can occur when new evidence emerges that challenges the existing framework, or when new social and cultural contexts arise that call into question established beliefs. The process of change is not always smooth, and it often involves conflicts and disagreements among scientists. However, over time, the new ideas gain acceptance and become the basis for a new scientific paradigm.

In summary, scientific revolutions are an integral part of the scientific process, and they reflect the dynamic nature of scientific knowledge. Understanding the process of a scientific revolution is crucial for anyone who wants to understand how science works and how it evolves over time.
What then is the nature of the more professional and exquisite

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The Structure of Scientific Revolutions

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The Structure of Scientific Revolutions
The Structure of Scientific Revolutions

nomenon of heating by compression had been established, all further experiments in the area were paradigm-dependent in this way. Given the phenomenon, how else could an experiment to elucidate it have been chosen?

Turn now to the theoretical problems of normal science, which fall into very nearly the same classes as the experimental and observational. A part of normal theoretical work, though only a small part, consists simply in the use of existing theory to predict factual information of intrinsic value. The manufacture of astronomical ephemerides, the computation of lens characteristics, and the production of radio propagation curves are examples of problems of this sort. Scientists, however, generally regard them as hack work to be relegated to engineers or technicians. At no time do very many of them appear in significant scientific journals. But these journals do contain a great many theoretical discussions of problems that, to the non-scientist, must seem almost identical. These are the manipulations of theory undertaken, not because the predictions in which they result are intrinsically valuable, but because they can be confronted directly with experiment. Their purpose is to display a new application of the paradigm or to increase the precision of an application that has already been made.

The need for work of this sort arises from the immense difficulties often encountered in developing points of contact between a theory and nature. These difficulties can be briefly illustrated by an examination of the history of dynamics after Newton. By the early eighteenth century those scientists who found a paradigm in the *Principia* took the generality of its conclusions for granted, and they had every reason to do so. No other work known to the history of science has simultaneously permitted so large an increase in both the scope and precision of research. For the heavens Newton had derived Kepler’s Laws of planetary motion and also explained certain of the observed respects in which the moon failed to obey them. For the earth he had derived the results of some scattered observations on pendulums and the tides. With the aid of additional but *ad hoc* assumptions, he had also been able to derive Boyle’s Law and an important formula for the speed of sound in air. Given the state of science at the time, the success of the demonstrations was extremely impressive. Yet given the presumptive generality of Newton’s Laws, the number of these applications was not great, and Newton developed almost no others. Furthermore, compared with what any graduate student of physics can achieve with those same laws today, Newton’s few applications were not even developed with precision. Finally, the *Principia* had been designed for application chiefly to problems of celestial mechanics. How to adapt it for terrestrial applications, particularly for those of motion under constraint, was by no means clear. Terrestrial problems were, in any case, already being attacked with great success by a quite different set of techniques developed originally by Galileo and Huygens and extended on the Continent during the eighteenth century by the Bernoullis, d’Alembert, and many others. Presumably their techniques and those of the *Principia* could be shown to be special cases of a more general formulation, but for some time no one saw quite how.*

Restrict attention for the moment to the problem of precision. We have already illustrated its empirical aspect. Special equipment—like Cavendish’s apparatus, the Atwood machine, or improved telescopes—was required in order to provide the special data that the concrete applications of Newton’s paradigm demanded. Similar difficulties in obtaining agreement existed on the side of theory. In applying his laws to pendulums, for example, Newton was forced to treat the bob as a mass point in order to provide a unique definition of pendulum length. Most of his theorems, the few exceptions being hypothetical and preliminary, also ignored the effect of air resistance. These were sound physical approximations. Nevertheless, as approximations they restricted the agreement to be expected

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between Newton's predictions and actual experiments. The same difficulties appear even more clearly in the application of Newton's theory to the heavens. Simple quantitative telescopic observations indicate that the planets do not quite obey Kepler's Laws, and Newton's theory indicates that they should not. To derive those laws, Newton had been forced to neglect all gravitational attraction except that between individual planets and the sun. Since the planets also attract each other, only approximate agreement between the applied theory and telescopic observation could be expected.\textsuperscript{10}

The agreement obtained was, of course, more than satisfactory to those who obtained it. Excepting for some terrestrial problems, no other theory could do nearly so well. None of those who questioned the validity of Newton's work did so because of its limited agreement with experiment and observation. Nevertheless, these limitations of agreement left many fascinating theoretical problems for Newton's successors. Theoretical techniques were, for example, required for treating the motions of more than two simultaneously attracting bodies and for investigating the stability of perturbed orbits. Problems like these occupied many of Europe's best mathematicians during the eighteenth and early nineteenth century. Euler, Laprange, Laplace, and Gauss all did some of their most brilliant work on problems aimed to improve the match between Newton's paradigm and observation of the heavens. Many of these figures worked simultaneously to develop the mathematics required for applications that neither Newton nor the contemporary Continental school of mechanics had even attempted. They produced, for example, an immense literature and some very powerful mathematical techniques for hydrodynamics and for the problem of vibrating strings. These problems of application account for what is probably the most brilliant and consuming scientific work of the eighteenth century. Other examples could be discovered by an examination of the post-paradigm period in the development of thermodynamics, the wave theory of light, electromagnetic theory, or any other branch of science whose fundamental laws are fully quantitative. At least in the more mathematical sciences, most theoretical work is of this sort.

But it is not all of this sort. Even in the mathematical sciences there are also theoretical problems of paradigm articulation; and during periods when scientific development is predominantly qualitative, these problems dominate. Some of the problems, in both the more quantitative and more qualitative sciences, aim simply at clarification by reformulation. The \textit{Principia}, for example, did not always prove an easy work to apply, partly because it retained some of the clumsiness inevitable in a first venture and partly because so much of its meaning was only implicit in its applications. For many terrestrial applications, in any case, an apparently unrelated set of Continental techniques seemed vastly more powerful. Therefore, from Euler and Lagrange in the eighteenth century to Hamilton, Jacobi, and Hertz in the nineteenth, many of Europe's most brilliant mathematical physicists repeatedly endeavored to reformulate mechanical theory in an equivalent but logically and aesthetically more satisfying form. They wished, that is, to exhibit the explicit and implicit lessons of the \textit{Principia} and of Continental mechanics in a logically more coherent version, one that would be at once more uniform and less equivocal in its application to the newly elaborated problems of mechanics.\textsuperscript{11}

Similar reformulations of a paradigm have occurred repeatedly in all of the sciences, but most of them have produced more substantial changes in the paradigm than the reformulations of the \textit{Principia} cited above. Such changes result from the empirical work previously described as aimed at paradigm articulation. Indeed, to classify that sort of work as empirical was arbitrary. More than any other sort of normal research, the problems of paradigm articulation are simultaneously theoretical and experimental; the examples given previously will serve equally well here. Before he could construct his equipment and make measurements with it, Coulomb had to employ electrical theory to determine how his equipment should be built. The


\textsuperscript{11} René Dugas, \textit{Histoire de la mécanique} (Neuchatel, 1950), Books IV–V.
II. Normal Science as Puzzle-Solving

The Structure of Scientific Revolutions
The Structure of Scientific Revolutions

failure as a scientist—then why are these problems undertaken at all? Part of the answer has already been developed. To scientists, at least, the results gained in normal research are significant because they add to the scope and precision with which the paradigm can be applied. That answer, however, cannot account for the enthusiasm and devotion that scientists display for the problems of normal research. No one devotes years to, say, the development of a better spectrometer or the production of an improved solution to the problem of vibrating strings simply because of the importance of the information that will be obtained. The data to be gained by computing ephemerides or by further measurements with an existing instrument are often just as significant, but those activities are regularly spurned by scientists because they are so largely repetitions of procedures that have been carried through before. That rejection provides a clue to the fascination of the normal research problem. Though its outcome can be anticipated, often in detail so great that what remains to be known is itself uninteresting, the way to achieve that outcome remains very much in doubt. Bringing a normal research problem to a conclusion is achieving the anticipated in a new way, and it requires the solution of all sorts of complex instrumental, conceptual, and mathematical puzzles. The man who succeeds proves himself an expert puzzle-solver, and the challenge of the puzzle is an important part of what usually drives him on.

The terms ‘puzzle’ and ‘puzzle-solver’ highlight several of the themes that have become increasingly prominent in the preceding pages. Puzzles are, in the entirely standard meaning here employed, that special category of problems that can serve to test ingenuity or skill in solution. Dictionary illustrations are ‘jigsaw puzzle’ and ‘crossword puzzle,’ and it is the characteristics that these share with the problems of normal science that we now need to isolate. One of them has just been mentioned. It is no criterion of goodness in a puzzle that its outcome be intrinsically interesting or important. On the contrary, the really pressing problems, e.g., a cure for cancer or the design of a

lasting peace, are often not puzzles at all, largely because they may not have any solution. Consider the jigsaw puzzle whose pieces are selected at random from each of two different puzzle boxes. Since that problem is likely to defy (though it might not) even the most ingenious of men, it cannot serve as a test of skill in solution. In any usual sense it is not a puzzle at all. Though intrinsic value is no criterion for a puzzle, the assured existence of a solution is.

We have already seen, however, that one of the things a scientific community acquires with a paradigm is a criterion for choosing problems that, while the paradigm is taken for granted, can be assumed to have solutions. To a great extent these are the only problems that the community will admit as scientific or encourage its members to undertake. Other problems, including many that had previously been standard, are rejected as metaphysical, as the concern of another discipline, or sometimes as just too problematic to be worth the time. A paradigm can, for that matter, even insulate the community from those socially important problems that are not reducible to the puzzle form, because they cannot be stated in terms of the conceptual and instrumental tools the paradigm supplies. Such problems can be a distraction, a lesson brilliantly illustrated by several facets of seventeenth-century Baconianism and by some of the contemporary social sciences. One of the reasons why normal science seems to progress so rapidly is that its practitioners concentrate on problems that only their own lack of ingenuity should keep them from solving.

If, however, the problems of normal science are puzzles in this sense, we need no longer ask why scientists attack them with such passion and devotion. A man may be attracted to science for all sorts of reasons. Among them are the desire to be useful, the excitement of exploring new territory, the hope of finding order, and the drive to test established knowledge. These motives and others besides also help to determine the particular problems that will later engage him. Furthermore, though the result is occasional frustration, there is good reason
If we can accept a consistent theory based on the iron law of conservation of energy, then we have a solution to the cross problems, and so on, and really, discovered.

The discovery of scientific revolutions is a central theme of this book. It is based on the idea that scientific progress is not a linear progression of increasing knowledge, but rather a series of abrupt changes, or "scientific revolutions," in the way we think about the world.

The Structure of Scientific Revolutions

Normal Science vs. Puzzle-solving
Normal Science as Puzzle-Solving

The Structure of Scientific Revolutions

Only a change in the rules of the game could have provided an


4. The example is discussed at length near the end of Section X.
The role of scientific revolutions

Vol II, No 2


Professor, See, "Science, Reason and Moral Goodness: An Essay on Progress."

Philosophical Reviews (1928), 14, 176.

For the moment make the

of the revolution in general, see these papers. The evolution of the

paradigms can provide insight into the substance of these.

research traditions. Hence, I suggest, derive from paradigms, but

research traditions, rules, suggest, derive from paradigms, but

paradigms have a role in science. They are the source of coherence for

theories and points of view in the science of the time. Paradigms

introduce shared paradigms among scientists, thus dividing the

scientific community into two: those who are committed to the

dominant paradigm and those who are not. This is not necessarily a

deterministic activity, but it is a highly influential activity.

In the paragraphs of a scientific specialty, where the

scientific practice is in another way, the illumination may

illuminate the nature of research and other relations. A

discussion of puzzles and the nature of puzzles. The nature of puzzles

are like, the context can be studied with an interest in the social

history. When the contextually different, it is how to bring the

hole, but it is not necessarily different, it is how to bring the

social context to the meaning of the meaning. A context

is a network of commitments - consistent, diverse.

The existence of the strong network of commitments all how

ones which become valuable for science at all times.

of the theories. Undoubtedly, these are still other "theses."

man of the observational elements, or a hybrid of science

research traditions, and if these must change, then to a new role?

empirical data, and if the scenery displays pockets of ep-

empirical art, in turn, lead their commitment, the

read the precision and scope with which it has been ordered.

fixed at a still higher level, there is another set of commitments,

observed in the study of mechanistic, open, and real.

alleged change. Similar effects of commitment can be

The philosophy of Scientific

 revolutions