branching patterns and canopy shape and size should also be taken up.

3 The dynamic behaviour and role of roots in the functioning of forest ecosystems has only recently been recognized. At the global level, fine root production represents a large and relatively unknown portion of the tree. It was felt that studies on different aspects of root dynamics and growth should be undertaken.

4. For intensive forest management, a clear understanding of nutrient relations and nutrient dynamics of trees in various forests ecosystems is fundamentally important. This includes nutrient availability in soil, its uptake and distribution within the plant body, retranslocation before abscission of short-lived plant parts, litter fall, root turnover and decomposition. There is urgent need to intensify research in nutrient relations with respect to important trees and forests.

5. As wood is the chief product of the trees, the understanding of its formation, structure, properties, durability and utilization for various purposes have been a major concern of wood scientists. There is a need for strengthening our capabilities in this area.

6. Rubber, gums, resins, tannins and essential oils constitute major tree extractives and a national shortage of these is met by annual imports costing the country huge sums in foreign exchange. Studies on the development and fine structure of secreting tissues in relation to seasons, age and location are required to be made to develop simple, improved tapping techniques.

7. In the field of regeneration ecology, studies on soil seed banks, germination behaviour, reproductive allocation and fecundity, survival and growth of seedlings and sprouts as well as various factors like seed dispersal, predation, neighbourhood effects, microenvironmental variables are needed.

8. Out of the large number of economically important tree species growing in our country, a few selected species be identified for research work so that at the end of a reasonable period of time their biology is better understood. Eight tree species were identified for intensive studies.

9. A cooperative approach of bringing together research workers and utilizing their expertise be taken, to ensure generation of meaningful and reliable data.

10. As very few talented researchers are contributing to this difficult area of tree biology, training programmes with faculty drawn from universities and research institutions be organized urgently so that skilled, motivated manpower is available to meet future needs.

A detailed report has been brought out by the Department of Science and Technology in consultation with the Chairman and experts who participated in the brain storming session and their help and advice is gratefully acknowledged.

Parveen Farooqui, Department of Science and Technology, New Delhi.

Profiles of scientific philosophy: Paradigms and serendipity

A. N. Mitra

"Normal science is a rather directed cumulative process which does not as a routine aim at novelties of fact or theory. However, new and unexpected phenomena are detected from time to time, leading to radically new theories. This is in the very nature of the scientific enterprise."

The thematic component[1]

There is a broad consensus that the growth of any quantitative science depends on the healthy interaction between two distinct types of scientific ingredients—theory versus experiment—the former standing for the calculus of logic and mathematics, and the latter for empirical matters of fact, or 'data'. This interplay has been succinctly expressed by the physicist Friedrich Desauer as a five-step process[1].

Step 1. Hypothesize a provisional statement obtained by induction from experience.

Step 2. Refine and structure the hypothesis in terms of some mathematical equations.

Step 3. Draw logical conclusions or predictions from the structured hypothesis which have promise of experimental check.

Step 4. Check these predictions against experience through a suitable experiment.

Step 5. If both tally within expected limits, together with all other allied predictions versus data based on

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A. N. Mitra is in the Department of Physics and Astrophysics, University of Delhi, Delhi 110 007, India

the same general premises, then a warrant is available for the decision that the result has probably universal validity.

Of course no dogmatic view or claim must be entertained about the hypothesis which is best left as an open question, subject to constant scrutiny in this typically inductive method, but such a picture is hardly complete, for it leaves unanswered a vital question as to what constitutes the source of the original induction. In other words, are there any quantitative criteria for 'preselection' of the hypotheses themselves on which the edifice of the inductive method stands? In modern jargon this is called the thematic component, a concept which is far more nebulous than the two components (theory and experiment; or analytic versus empirical) of the inductive process noted above, and often resists a quantitative definition. Yet this component constitutes a vital ingredient underlying the structure of any modern science and has since the days of Kepler and Newton played an undefined underlying role in the shaping of all major scientific theories to date. (And, in the twentieth century, it has served as a powerful tool for breaking down the Cartesian perceptions of a dichotomy between creative ideas at the scientific and humanistic levels.) Indeed, Kepler's two-fold philosophy of the universe as a mathematical harmony on the one hand and a central theological order on the other, which he liberally supplemented with the mechanistic image of the universe (in terms of the mass of observational data he had inherited from Tycho Brahe), speaks of the thematic component. More authentic evidence of the thematic dimension comes from the five rules of Newton's *Principia*:

(a) *Rule of simplicity and economy.* Avoid hypotheses that are not essential to explain observed facts.

(b) *Principle of uniformity of nature.* Similar effects must be assigned to the same cause.

(c) *Universality principle.* Properties common to experimentally accessible bodies pertain to all bodies in general.

(d) *Rule against new hypotheses.* Propositions induced on the basis of experiment cannot be ignored merely by proposing contrary hypotheses.

(e) *A (suppressed) rule on the definition of hypothesis.* (No quantitative criterion was suggested.)

This last rule represents a covert acknowledgement of the underlying role of the highly subjective and apparently arbitrary nature of the thematic component, quite distinct from the analytical versus the empirical, in any scientific pursuit. Unlike the conventional relation between the analytical and the empirical, wherein the latter is supposed to constitute a 'test' of the former, a thematic hypothesis or proposition is a statement which can neither be directly verified nor falsified and yet cannot be wished away[4].

The contingent plane: theory versus experiment[1]

In an attempt to provide a perspective to the thematic component *vis-a-vis* the analytical and the empirical, Holton has suggested the following picture—Let the empirical and analytical statements in respect of a given problem be represented as the x- and y-coordinates on a two-dimensional $(x-y)$ plane. This plane may be used to analyse the concepts of science (such as force) and the propositions of science (e.g. the law of gravitation). The concepts are like points and the propositions are analogous to curves (line-elements) in the $x-y$ plane. Thus a force has an empirical (x) dimension (since it can be measured), as well as an analytical (y) dimension since, e.g. it obeys a mathematical (vector) law of addition. The proposition (e.g. law of gravitation) gives a relation between x and y in the form of a suitable curve in this plane, based on the (vector) concept of the force. The $x-y$ plane is called the *contingent plane* because the meaning of concepts and statements in it are *contingent* on their having both empirical and analytical significance. Contingency analysis is thus the study of the relevance of concepts and propositions in x- and y-dimensions.

Non-contingent dimension

In such a description the thematic component, according to Holton, plays the role of a non-contingent dimension, say z-axis, to collectively characterize the complex thought processes which, to say the least, are orthogonal to the contingent plane. In this three-dimensional space, any physical concept has also a thematic (z) component, over and above the x- and y-components. In the above example of force, its thematic component may, for example, be associated with the general idea of 'potency' which transcends its concrete manifestations in the xy-plane. In a similar way, the thematic aspect of a general scientific proposition (a curve in the xy plane) such as a given law of conservation may be expressed by the notion of a 'symmetry' or invariance. (To those who are familiar with Noether's theorem about the connection between symmetries and conservation laws, it is perhaps fair to point out that the purely thematic aspect of 'symmetry' transcends these quantitative details.)

What is the basic difference between thematic (z-axis) propositions and those in x-y plane? Though immune to direct verification, their induced action on the contingent plane (by articulating the mathematical laws in a concrete testable form) provides an indirect basis for acquiring *confidence* or otherwise in their viability.

Thematic classifications: principles and prejudices[4]

It would be too simplistic to characterize the thematic component (which has played a pivotal role in shaping the destiny of science throughout its history) by merely adding an extra dimension (the z-direction) to it. Nevertheless it is possible within such a one-dimensional model to get a limited insight into the nature of thematic proposition by classifying them under three different categories[4]—principles, postulates and prejudices—roughly in accordance with the degree of confidence these have acquired within the scientific community in a particular discipline. (I apologize for my emphasis only on physical science which stems from my personal background limitations, but this need not give the impression of less relevance of these concepts to the other fields.) The principles have undergone so many acid tests through precise feedbacks from the contingent plane that their speculative and subjective aspects have by and large been forgotten and they are now taken almost for granted as an integral part of the scientific discipline concerned. Some celebrated examples from physics are:

i) The principle of extremum (Hamilton, Fermat)
ii) Principle of relativity (Lorentz-invariance)
iii) The uncertainty principle (quantum mechanics)
iv) Principle of symmetry (law of conservation)
v) Principle of causality (in space–time)

These principles have received such a high degree of phenomenological support that 'faith' in these thematic propositions has been continuously strengthened over a long time. By and large these themata have acquired the status of an integral part of the language of physics, matched by highly structured formulations, even though in the initial stages their viability was more or less an article of faith.

Prejudices: beauty and naturalness

At the other extreme lie less established thematic propositions—termed prejudices[4]—which, while suggestive enough to appeal strongly to intuition, are nevertheless rather nebulous in character, i.e. not articulated enough to lend themselves to concrete mathematical formulations. These themata have much wider sweep than those in the first category (principles) and in all probability provide the driving force (inspiration) for the latter, while a more formal understanding of their deeper character may well be a subject for a distant era (if at all). In the present era, or perhaps even in the foreseeable future, confidence in these themata must be entirely a matter of faith, sustained only from past experience. Some examples in this category are[4]:

a) 'Naturalness' as a selectivity criterion
b) Beauty and simplicity in physical theories
c) Duality in physical sciences (and beyond)[3]
d) Bootstrap philosophy
e) Shell structure and infinitum.

Even in this listing items (a) and (b) have rather intuitive and universal appeal while the others are more specialized concepts abstracted from the physical sciences where their manifestations have been debated in different degrees. Perhaps the most telling example of a 'beautiful' theory—confirmed by experiment—is Einstein's field equations in the General Theory of Relativity which he had derived as an *exact* theory[5] through a pure thought process dominated by considerations of mathematical elegance and simplicity, but with no eye whatsoever on experimental data. This would certainly go against the normal way of construction wherein the new theory should merely allow for small relativistic corrections to the Newtonian predictions (which would be enough for the data). Instead Einstein relied entirely on creative thought, appealing to the intrinsic beauty of Nature, and if in the process his exact theory also happened to be in conformity with observation, this fact was merely incidental to the entire exercise, and in any case did not come as a surprise to Einstein who had already been convinced about its truth by virtue of its sheer elegance and beauty. An equally convincing example was Weyl's philosophy of harmonizing truth with beauty but always giving precedence to the latter over the former whenever there was a conflict[5]. Indeed there had been one occasion for conflict when he projected his two-component theory of the neutrino on grounds of sheer 'beauty' at a time (1929) when 'truth' was apparently on the other side (parity violation in physics was an anathema at that time). On the other hand, beauty was vindicated (and reconciled with the higher truth) three decades later with the advent of the V–A theory of weak interactions which had to make essential use of the Weyl-formalism.

The other items are more technical and will not be discussed but nature of such thematic propositions may be typically illustrated by item (e) which purports to *generalize* from the firmly established result of shell-structure of up to *three* distinct stages of matter:

Atomic (electrons and nuclei) → nuclear (hadrons) → hadronic (quarks)

While from the purely philosophical point of view, such a hypothesis is highly plausible—indeed natural—the rigid yardstick of the physics discipline makes it imperative to test each further stage exhaustively (with concrete experimentation) before the validity of the proposition can be generally accepted. And at the present state of the high energy art, the probing capacity stops roughly at the stage of 10^{-16} cm (sixteen zeros after the decimal) which is roughly the upper limit of the size of a quark or an electron! Thus this hypothesis is probably doomed to remain at the speculative stage for all time to come, unless new probing methods resolve the issue earlier (than infinity). The second category of themata (postulates) occupies a position somewhat intermediate between the other two categories.

The above discussion based on a one-dimensional classification of thematic propositions provides only a limited view of the richness of their structure. To obtain a deeper insight into their anatomy it is clearly necessary to adopt a bigger strategy designed to uncover the *hidden dimensions* than was possible within this simple one-dimensional model. A task of this magnitude was undertaken three decades ago by Thomas Kuhn[2] who extracted considerable information on the very structure of scientific revolutions by delving deep into their actual history. The key elements of his analysis are Normal Science, Paradigms, Anomalies, Crises and Revolutions. The rest of this article is devoted to a summary of his main findings, one in which Paradigms (often found through serendipitous discoveries) have played a fundamental role.

Inadequacy of methodology: the arbitrary element[2]

It would be tempting to suppose that the history of science is a mere collection and catalogue of facts, theories and methods. The historian who chronicles these developments by accumulation would then face a two-fold task to determine:

i) Who discovered (and when) each fact or law or theory?
ii) What were the margins of error, myth and superstition that have *inhibited* faster growth of science texts?

Unfortunately the historian would often face inner contradictions in the resolutions of such issues and must *choose* between alternative forms of belief structures which are all scientific in their respective rights, but are not necessarily compatible with one another. This kind of problem raises doubts about viewing the development of science as a cumulative process of accretion, in preference to a more complex form of evolution with an element of competition in which the fittest of the ideas are given the best chance to survive. This in turn requires the formulation of new sorts of questions designed to trace less-than-cumulative lines of development for the sciences through a process which gives greater weight to opinions with a maximum internal coherence as well as the closest possible fit to nature. Such a process by its very nature is a highly subjective exercise and gives science a very different image than the traditional objective image with which one normally tends to associate this discipline (thus lending respectability to the use of terms like prejudices[4] employed in the foregoing).

What aspects of science will be highlighted by the new thinking? First and foremost is the recognition of insufficiency of standard methodological directives, howsoever well-developed on the basis of past successes in the field, to dictate a unique substantive conclusion to related scientific questions. An excellent illustration is provided by the fate of the so-called GUT (grand unification) theory of all interactions—weak, electromagnetic and strong—under one common umbrella. After the successful unification of the weak and electromagnetic interactions, there was a world-wide activity designed to extend identical concepts to the strong interaction sector, dictated by some major centres of scientific power. The stakes were high since the theory made a definite prediction about the *finite life-time* of the proton, one which was also experimentally testable. The result of this world-wide activity unfortunately proved negative: No proton decay was detected to date within the predicted range of its lifetime.

What fair conclusion can be drawn from this episode? For the immediate example on hand, the idea of an unstable proton may well be right but nature is unlikely to respond in a rubber-stamp fashion to the dictates and methodology of the once-successful (electroweak) theory, howsoever appealing and persuasive its tenets may be. Perhaps the novelty that had characterized the electroweak theory is not enough for the next stage of unification.

The same lesson is valid for more general situations as well. It is not difficult to imagine that fresh ideas may well be forthcoming from scientists in other fields whose ignorance in the field under consideration could serve as a blessing in disguise as they are less likely to be encumbered by the set pattern of thinking which normally characterizes a particular field of science. At least the early developmental stages of most sciences have been characterized by continual competition between a number of distinct views of nature, each partially compatible with the 'contingent plane'. What differentiated these various schools was their mutually incommensurable ways of seeing the world despite the role of the contingent plane in restricting the range of admissible scientific belief. Yet the latter does not alone determine a particular belief structure or thema espoused by a scientific community at a given time which is often traceable to an apparently arbitrary element born out of some personal or historical coincidences.

Normal vs extraordinary science[2]: hidden role of serendipity

Such elements of arbitrariness notwithstanding, a given scientific group nevertheless can practice its trade within a fairly well defined set of 'received beliefs' as well as a commitment to a set of objectives and methodology designed to analyse and explore those beliefs. At least in the mature sciences, answers to such questions are firmly incorporated in the formal training that prepares the student for professional practice. The rigour and rigidity of this training is largely responsible for the characteristic efficiency and sense of direction of the normal research activity: A sustained and devoted attempt to force nature into 'conceptual boxes' supplied by professional education. On the other hand, there also arises the opposite question: Can research proceed without such boxes, which are after all not free from a degree of arbitrariness in their historic origin? Indeed this element of arbitrariness which cannot be wished away. does have an important effect on scientific development. For Normal Science which represents the bulk of scientific research is based on the assumption that the scientific community already knows what the world is like and is willing to defend that assumption whatever the cost. To that end Normal Science often suppresses fundamental novelties which are necessarily at odds with its basic commitments. And yet, so long as these commitments retain an element of arbitrariness, the inherent 'governor mechanism' that characterizes normal research ensures that novelty cannot be indefinitely suppressed. For example a situation may arise when a 'normal problem' which is supposed to yield to standard rules and procedures fails to respond to the continued efforts of an entire scientific community (the episode of proton decay mentioned above mildly illustrates this situation). A similar situation may arise with an experimental equipment, designed for normal research, failing to perform in the expected manner. Such 'anomalies' cause normal research to go off the track; this is a signal that the profession can no longer evade anomalies that subvert the existing tradition of scientific practice. This sets the stage for *extraordinary investigations* that finally compel the profession to adopt a new set of commitments—a new Paradigm—for the practice of science. Kuhn has termed such extraordinary situations which cause major *paradigm shifts* as Scientific Revolutions. Famous examples of these are associated with the names of Copernicus, Newton, Lavoisier and Einstein who demonstrated convincingly the true significance of scientific revolutions in the field of physical sciences. In the words of Thomas Kuhn[2]:

> Each of them necessitated the community's rejection of one time-honoured scientific theory in favour of another incompatible with it. Each produced a consequent shift in the problems available for scientific scrutiny and in the standards by which the profession determined what should count as an admissible problem or as a legitimate problem-solution. And each transformed the scientific imagination in ways that we shall ultimately need to describe as a transformation of the world within which scientific work was done. Such changes, together with the controversies that almost always accompany them, are the *defining characteristics* of scientific revolutions.

These characteristics are of course brought out rather tellingly from the Newtonian or chemical revolutions, and in the modern context by the aftermaths of Relativity, Quantum Theory, Parity Violation and the Quark hypothesis. In less dramatic fashion, the same features can be retrieved from more specialized sectors of investigation, such as Maxwell equations which led to an entirely new (electromagnetic) theory of light, or the continued resistance of quarks to lend themselves to direct observation which led eventually to a new doctrine in physics—the doctrine of *Confinement*. The invention of other new theories regularly evokes the same response from the specialists who are directly concerned with their impact: A change in the rules governing the prior practice of normal science, which necessitates the reconstruction of the prior theory and the re-evaluation of prior fact, requiring a regular team work extended over long periods.

Nor are such characteristics of revolution the exclusive preserve of theoretical inventions. An unexpected discovery like that of oxygen or of X-rays, and (in the more modern context) of say, CP-violation (through a specialized decay process), does not simply add an item to the population of the scientist's world: It changes the entire perspective of his traditional experimental procedures and his concept of entities with which he had hitherto been familiar, requiring in turn a *shift* in the theoretical network through which he is used to dealing with the world. In this way scientific fact and theory are intertwined and an unexpected (serendipitous) discovery is not merely factual in its input but qualitatively transforms (apart from quantitatively enriching) the scientist's world.

At this stage it is useful to offer a formal definition of serendipity: A serendipitous event may be defined as a result of great importance which was discovered rather unexpectedly with no conscious concern for the original motivation for the investigation. There is a strong element of surprise associated with the significance of such discoveries. Some examples were already cited above: The unexpected discoveries of oxygen and X-rays were serendipitous events. More specialized examples in physics are provided by (i) the Lee–Yang discovery of parity violation from the relatively innocuous investigation of the so-called tau-theta puzzle, and (ii) the unusual experimental finding of Fitch and Cronin which demolished the belief in the validity of the joint CP conservation (charge conjugation and parity) in weak interactions. An outstanding example of serendipity in the domain of astrophysics is the discovery by Wilson and Penzias of the 3 K background radiation during their effort to refine their detecting instrument to eliminate the microwave background. Even the famous Davisson–Germer finding which established the wave nature of electron was serendipitous, since these experimenters were merely 'cleaning' the material through baking (which yielded the desired crystalline purity). On a more theoretical plane, Dirac's electron theory serendipitously paved the way to quantum field theory through the positivity requirement of both energy and probability.

Paradigms in normal science[2]: priority over rules

Normal science means research based on scientific achievements which are acknowledged by a scientific community as providing the foundations for its further practice. These achievements which may be termed *Paradigms* share two common characteristics according to Kuhn: (i) They were sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity, (ii) They were sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve.

Paradigms thus provide models from which emerge particular patterns and cultures of scientific research, such as Newtonian dynamics, corpuscular optics, etc. Paradigms undergo substantial changes only through scientific revolutions and this is the developmental process of a mature science. A celebrated example is the story of successive transitions from corpuscular to the wave theory of light and back to the former through a quantum theoretic synthesis of the two. Each school derived strength from its relation to some particular metaphysic and each emphasized—as paradigmatic observations—the particular cluster of optical phenomena that its own theory could naturally explain. Paradigm-based research is a highly directed process and strongly affects the structure of the group that practices the field. Thus when in the development of a natural science an individual or group first produces a synthesis able to attract most of the practitioners of the generation, the older schools gradually disappear mainly due to the members' conversion to the new paradigm. Through this process paradigms help in continually sharpening the definition of the field under study. The necessary fall-outs are the formation of specialized journals, societies and the claim for a special place in the curriculum. Among other consequences of such specialization, an individual scientist functioning within a well-defined paradigm-based group does not have to build up his field anew, leaving the job to standard textbooks, and can directly concentrate on further research on various details whose findings are best left to professional journals. This is a typical scenario of normal, paradigm-based research which is directed to the *articulation* of those phenomena and theories that the paradigm already supplies. This is convincingly illustrated by the classic example of the research pattern during the Eighteenth century designed to articulate the Newtonian paradigm, simultaneously at the experimental and theoretical levels. The experiments of Boyle, Coulomb and Joule and the apparatuses of Cavendish and Atwood illustrate the former. The investigations of Euler, Lagrange, Laplace and Gauss in the eighteenth century and with further refinements by Hamilton, Jacobi and Hertz in the nineteenth, illustrate the theoretical efforts to reformulate the Newtonian paradigm.

The pursuit of Normal science is necessarily constrained by the existence of *rules* of the game which must be carefully distinguished from the paradigms on which it is based. The distinction is roughly one of procedure versus substance. Paradigms could determine normal science even without the intervention of discoverable rules. For example, the student of Newtonian dynamics can often discover the true meaning of terms like force and mass, not so much from the definitions given in the textbooks as by direct observation and *applications* to problem solving.

At the research level too, the supremacy of paradigms over rules may be felt through the observation that paradigms can guide research directly through modelling as well as through abstracted rules. Normal science can proceed without rules as long as the community accepts the results without question. This is so except during periods when the paradigms themselves are felt to be insecure. Such periods which are marked by debates over legitimate methods, problems and standards of solution, usually occur before and during scientific revolutions, a classic example being the transition from Newtonian to quantum mechanics. And when a new paradigm finally takes over, the new rules again become subservient to the former.

Anomalies and crises: scientific revolutions

Normal science is a rather directed cumulative process which does not as a routine aim at novelties of fact or theory. However, new and unsuspected phenomena are detected from time to time, leading to radically new theories. This is in the very nature of the scientific enterprise which has a built-in technique for producing such surprises. For paradigm-based research this ensures a unique mechanism for inducing paradigm change. Indeed a novel result, produced inadvertently by a game played under one set of rules requires the elaboration of another set for its assimilation. After it becomes a part of science, the enterprise will have changed its original character. In this respect it is quite futile to make a distinction between a theoretical invention or an experimental discovery which are necessarily intertwined. Nor are such discoveries *isolated events but extended episodes with a regularly* recurrent structure. A discovery begins with the awareness of *anomaly*, i.e. with the recognition that nature has somehow violated the paradigm-induced expectations that govern normal science. It then continues with an exploration of the area of anomaly and closes only when the paradigm theory has been adjusted so that the anomaly becomes the expected thing. Assimilating a novel fact demands a non-trivial adjustment of theory which amounts to seeing nature in a different way. This is not a painless process since in science novelty usually encounters resistance against the background of expectation.

Just as ordinary anomalies lead to scientific discoveries requiring adjustment of existing theory, or paradigm shifts, similarly bigger and more profound anomalies or crises, are responsible for major paradigm changes, resulting in the emergence of new theories. In a sense the difference is not merely one of degree but often of kind since the emergence of new theories with large scale paradigm destruction produces a sense of professional insecurity among the practitioners of that science. Examples are the Copernican revolution, Lavoisier's oxygen theory of combustion, and Einstein's final rejection of the ether theory in favour of the special theory of relativity. In each case the novel theory has proved a direct response to crisis. Major responses have more often come from pure Gedanken (thought) experiments than those carried out in the laboratory. These are vividly brought about by the extraordinary researches of Einstein and Bohr designed to expose the old paradigms to existing knowledge in ways that isolate the root of the crisis with a clarity unattainable in the laboratory. The resulting transition to a new paradigm constitutes a *scientific revolution*.

Conclusion: natural selection process[2]

What is the nature of a scientific revolution which causes paradigm change? There is an obvious parallel with political revolutions characterized by the destruction of the old system in favour of the new. Like the choice between competing political institutions, that between competing paradigms proves to be a choice between incompatible modes of community life. And, as in political revolutions so in paradigm choice, there is no standard higher than the *assent of the community*. Therefore to examine how scientific revolutions are effected, it is necessary to check not only the impact of nature and of logic, but also of the techniques of *persuasive argumentation* within the scientific community. This last point almost shatters the myth about the intrinsic objectivity of science in favour of a more chauvinistic view of 'survival of the fittest', much like the Darwinian concept of the origin of species. In such a view, one which Kuhn strongly advocates, there is no ultimate goal such as absolute Truth in the march to which each stage in the development of scientific knowledge represents a better approximation. Instead the process of natural selection, by conflict within the scientific community, of the fittest way to practice future science offers a more realistic characterization of its evolutionary pattern.

Science thus has a dual personality: the detached aspect of objective investigations and the subjective or ego aspect of competing paradigms. The following couplet is a partial attempt to capture this dual characteristic:

I am dancing with my ego phase.
I am a particle, my ego a wave.

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