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PARADIGMS, PROGRAMMES AND PROGRESS IN SCIENCE

In this paper, given at the VI Symposium on the Interactions of Christianity and Culture on 24 May 1980, Mr Burgess outlines the views of Popper, Kuhn, Lakatos and Feyerabend on the nature and development of science. He draws attention to prevailing divergences of opinion and asks what Christian attitudes should be.

August 1945 marked a watershed for science. On the 6th a uranium fission bomb was dropped on Hiroshima, followed a few days later by a plutonium bomb on Nagasaki. Suddenly even the most scientifically illiterate became aware of the extent to which science was involved in modern life -- and death.

There was an immediate 'gut' reaction of shocked revulsion against what scientists were doing.

This was followed in the post-war years by the rise of numerous pressure groups such as CND, BSSRS, The Medical Association for Prevention of War, expressing concern about the implications for society of scientific and technological 'progress'.

The past decade or so has seen increasing questioning of a much more fundamental kind: not merely the role of science in society, important as that concern is, but a serious and sustained attempt to evaluate the very nature of the scientific enterprise and of what could be meant by 'progress' in science.

Philosophical attempts to rationalise the nature of scientific knowledge have, of course, interested philosophers for many years. The translation in 1958 of Karl Popper's *The Logic of Scientific Discovery*[1] which refuted Baconian principles of induction, seems to have resulted in at last removing discussion of the philosophy of science from an arid intellectual level, remote from the concerns of practising scientists, to the context even of school science.[2]

Of the more influential of the post-WW2 philosophers of science, Popper, Kuhn, Lakatos and Feyerabend, especially the first two, are outstanding. I shall attempt to outline their contributions briefly. But first I shall say something about the nature of induction.

Baconian Induction

Francis Bacon (1561-1626), abandoning the deductive logic of Aristotle and the schoolmen, first introduced the idea of scientific induction. Truth was not to be dependent on authority; rather it was man's duty to learn from nature. Theories were to be constructed on the basis of ascertained facts and preconceived notions were to be discarded.

Induction is a way of thinking in which a generalisation is derived from observations of particular instances. According to this view, a 'law of nature' is a summary of past experience. As observation becomes more refined and the number of facts increases, inductive generalisations or 'laws' are developed of ever-widening scope. Science is thus a continuously-growing body of reliable knowledge.

In recent years, however, induction as the main principle of scientific thinking has been heavily criticized. A useful summary of objections is given by Chalmers[3]. The most serious criticism is that induction has no logical justification. For example, no matter how many objects have been seen to fall towards the earth, there is no logical ground for believing, or predicting, that an object will do so on the next occasion. The generalisation that heavier-than-air objects, when freed from constraint, will fall towards the earth cannot be logically inferred from any number of particular observed instances. This constitutes the 'problem of induction' as clearly stated by Hume in the 18th century.

There may, of course, be strong psychological reasons for using inductive arguments whether consciously or unconsciously: after all men, and presumably animals too, learn from experience by induction. Nevertheless inductive inferences cannot be justified on logical grounds alone, or so Popper in particular would argue.

Popper avoids the 'problem of induction' by asserting that science progresses by deductive methods. Lakatos adopts a similar view. Kuhn is more concerned with sociological pressures in science, while Feyerabend is vigorously opposed to any stereotype of science, holding to the need for "epistemological anarchy."

Karl Popper[1,5,6,7]

Attempts to assess the nature and methods of science deal with a wide variety of problems which can be conveniently classified as psychological, logical and methodological.

Popper argues that the last two only are the province of the philosopher of science.

(a) *Psychological problems.* These involve matters such as the nature of perception, the immediacy of perceptual knowledge and feelings of "conviction"based perhaps on intuition or induction. Popper refers to attempts to justify logical inferences on the basis of such perceptions as 'psychologism' and considers them invalid as a basis for the logical justification of science. He distinguishes sharply between the process of conceiving a new idea, which involves an irrational element, and the result of examining it logically. He likewise emphasises a dichotomy between "objective science" on the one hand, and "our knowledge" (our awareness of the facts) on the other.

Epistemology is concerned with testing scientific statements by their deductive consequences and not with attempting to derive their justification from (sense) experience, in the manner of the logical positivists.

(b) *Logical Structures.*

Popper sees the initial problem in characterizing empirical science as one of demarcation; that is, agreement on a convention which will distinguish between science and metaphysics.

The now well-known criterion which he proposes is that a system can claim to be called empirical or scientific only if it is capable, in principle at least, of being tested by experience and refuted. Thus, the falsifiability, not the verifiability, of a system marks it out as scientific.

This proposal depends upon an asymmetric relation between verifiability and falsifiability, for although hypothesis cannot be derived logically from the observation of singular facts, no matter how large the number of observations, a single observation is capable of falsifying a hypothesis, provided the observation is reproducible. Thus a million observations of swans which are white does not prove "All swans are white", but a single observation of a black swan would falsify the rule.

The falsifying experiment is usually a crucial one, designed to decide between two hypotheses by refuting at least one of them.

Popper maintains, however, that old theories, well corroborated, are to be retained and tenaciously defended, even if falsified, if they are the best theories that are known at the present time. There must be a "serious struggle for survival" between competing theories, so that the "fittest", in terms of explanatory power and truth content, may survive.[6,1a,4a]

Corroboration. A hypothesis is a provisional conjecture, not necessarily a 'true' statement: it is not verifiable but can be 'corroborated.' This is achieved by assessing the tests which the hypothesis has withstood.

Accepted "basic statements" (viz. empirical facts) must not contradict the hypothesis. A corroborative appraisal can be made in terms of the "degree of testability" of the hypothesis (see below), with special regard to the severity of the tests applied in an attempt to falsify the hypothesis. According to Popper appraisal of a hypothesis cannot be made in terms of probability.

Confirmations of a hypothesis have a significance which depends on their historical context. Hertz confirmed Maxwell's theory about electromagnetic radiation when he first detected radio waves. We do the same today with our radios but contribute nothing of value to science.[3a]

Confirmation of a bold 'risky' conjecture is thus more instructive than confirmation of a well-established theory. Conversely, falsification of a novel prediction is of less significance than falsification of an older, well-tried theory.

Degree of testability. This is a function of the simplicity of a hypothesis, Popper maintains. By "simplicity" he appears to mean the precision and clarity of a hypothesis.

e.g. Compare (1) The planets travel round the sun.
(2) The planets follow elliptical paths round the sun.

Statement (2) is more precise and therefore more simple than statement (1); it is more "risky" - more readily refutable if untrue. Failure to refute (2) would have a higher corroborative value than failure to refute (1).

Verisimilitude. In what sense can we say, within the framework of logic, that one theory is 'better' than another? Popper suggests we might first compare the logical contents of the two theories to be compared. When once a theory has been proposed it will be possible to write down a list of statements which follow logically from it. It will also be possible to list empirically found facts which appear to be inconsistent with the theory. The logical content of a theory will be a combination of the two lists and will provide a basis for comparison with another theory.

A second factor will be the correspondence of the theory to the facts. The term 'verisimilitude' combines the ideas of content and nearness to truth.

We approach truth in science, Popper states, by successive approximations based on trial and elimination of error, much as a computerised missile or satellite obtains guidance "by the relative evaluation of tentative predictions, precisely of the kind demanded by verisimilitude."[7]

(c) *Methodology*

This is concerned with the 'rules of the game' - with how science proceeds.

The distinguishing mark of empirical (viz. scientific) statements, says Popper, is their susceptibility to revision, irrespective of whether they satisfy certain logical criteria.

Methodological rules are conventions which circumscribe empirical science much as the rules of chess govern games of chess. Just as there is a "Logic of Chess", which is hardly pure logic, so there is a "Logic of Scientific Discovery."

The supreme rule is that other rules of procedure must not protect any statement against falsification.

Popper has in mind auxiliary hypotheses of an "ad hoc" kind which merely serve to "save the appearances" without advancing our knowledge.

In illustration Chalmers[3b] mentions an entertaining exchange that took place in the seventeenth century between Galileo and an Aristotelian opponent. Galileo observed the moon with his new telescope and reported that it was not a smooth sphere - as all celestial bodies were supposed by be according to the Aristotelians but that its surface was covered with mountains and craters. His opponent maintained (*ad hoc*!) that an invisible, undetectable substance covered the surface filling the craters and covering the mountains, to an extent that resulted in an overall spherical shape. Galileo was prepared to concede that such a substance was present, but that it was in fact piled up higher on the mountains!

For Popper auxiliary hypotheses are valueless if they decrease the falsifiability of a theory: to be of value they must be more potentially falsifiable than the original hypothesis or theory.

He considers Pauli's exclusion principle to be an "eminently acceptable" example of an auxiliary hypothesis, whereas the Fitzgerald-Lorentz contraction hypothesis he considers as unsatisfactory because it had no falsifiable consequences.

Popper further maintains that the introduction into atomic physics, by Niels Bohr in 1927, of the principle of complementarity was *ad hoc* and for this reason has remained "completely sterile" within physics.[4b]

Theory and Experiment. Experimental work is dominated by theory, according to Popper, and is meaningful only in the context of theory.

What compels the theorist to search for a better theory is the falsification of a theory so far accepted and corrobroated. Examples he gives are, (1), the Michelson-Morley experiment which led to the discovery of relativity[8] and, (2), the falsification by Lummer and Pringsheim of the radiation formulae of Rayleigh and Jeans, and of Wien, which led to quantum theory.[9]

The history of science shows that "it is always the theory and not the experiment ... which opens up the way to new knowledge ... it is always the experiment which saves us from following a track that leads nowhere."

Progress in Science. How then does Popper see scientific knowledge developing? By bold, unjustified (and unjustifiable) conjectures controlled by attempted refutations using severely critical tests.

Thus science at any given time may be thought of as consisting of theories which experience has shown to be those most resistant to criticism and which therefore appear to be the best available approximations to truth.

Every good theory is a prohibition; the more it prohibits the better it is, for the attempted refutations are more severe as a result.

The task of the scientist is to search for 'true' theories - even if he can never be quite sure that they are true when he discovers them.

However, truth is not the only requirement. We look for "interesting truth - truth hard to come by;" truth which has a high degree of explanatory power - which implies that it is logically improbable truth.

Popper assesses some of the widely held ideas current at the present time in the light of these principles. Freudian psychoanalytic theory and Adlerian individual psychology he regards as essentially metaphysical, because unfalsifiable.

The Marxist theory of history, he claims, was falsifiable in its earlier formulations, and was in fact falsified. Followers of Marx then re-interpreted both the theory and the evidence to make them agree.

By contrast, Popper considers Einstein's theory of relativity to be in a very different class. His special gravitational theory predicted that light must be deflected by massive bodies such as the sun. This unexpected and "risky" hypotbesis was confirmed by Eddington's expedition of 1919.

Popper views the general direction of evolution in science as a "quasi-inductive process." By tbis he means that each theory is superseded by a theory at a higher level of universality (the "inductive" direction) but not by inductive inference.

The bigher theory is better testable and contains the older, lower-level theory, at least to a good approximation.

The higher theory is proposed and tested deductively by means of theories of a lower level of universality.

Imre Lakatos[10,11]

Lakatos considers Popper's views on falsification over-simplified. He proposes a form of "sophisticated falsification" whereby not an isolated theory but a *research programme* may be falsified.

A research programme comprises not only a major theory, but all the supporting auxiliary theories: it is the entire structure which is open to falsification.

If such an organized structure leads to novel, unexpected predictions of facts or theories, the programme is said to be progressive, or to constitute a progressive problemshift.

Problem shifts are scientific if they are progressive, at least theoretically so, and are "pseudoscientific" if they are degenerating - that is, do not lead to new predictions.

There can be no falsification before the emergence of a better theory, whatever the evidence may suggest, and considerable hindsight may be needed to ensure that a programme has been in fact falsified.

Like Popper, Lakatos maintains that methodological rules must be introduced to tell us what paths to follow.

These constitute what he calls the *positive heuristic* of the programme. Rules which help us to avoid certain directions of research constitute the *negative heuristic* of the programme.

All research programmes possess a *hard core*. This comprises the general hypotheses that underpin the programme. It is not to be questioned and the negative heuristic of the programme protects the hard core by deflecting research into other areas, notably the protective belt.

The latter term describes the (partially articulated) auxiliary hypotheses which "bear the brunt of tests and get adjusted and re-adjusted, or even completely replaced, to defend the thus-hardened core."

The positive heuristic prevents the scientist from confusion in a "sea of anomalies." It defines a programme involving ever more complicated models which simulate some part of reality - often being blatantly false, but providing fresh insights which can lead to improved models.

Lakatos instances Newton, who first obtained his inverse-square law of planetary attraction from consideration of a fixed point-mass sun with a single point-mass planet. This was developed to allow mutual rotation round a common centre of gravity. Then more planets were added, with subsequent consideration of their shape as spheres. Planetary spin was introduced and finally the non-spherical shape of planets, due to rotation. Newton was fully aware of the limitations of his earlier models but was carried along by the heuristic thrust of the programme.

Refutation of a specific hypothesis is thus seen to be irrelevant. Indeed the positive heuristic may be so powerful that large-scale testing or even consideration of available data may be a waste of time.

Nevertheless, empirical checks are vital, although it may be a long time before interestingly testable versions of the research programme can be formulated.

Science should be a history of competing research programmes with plenty of serious competition, to ensure progress. (cf. Feyerabend).

Competition leads to the question: how can a research programme be eliminated? Only by a rival programme which explains the success of its rival and supersedes it by a further display of heuristic power (explanatory ability). This may become evident only after a long period of time.

For this reason, provided it can be rationally constructed as a progressive problemshift, a budding research programme must not be discarded "because it has so far failed to overtake a powerful rival."

Progress in Science. Mature science is seen as a continuing growth based on a progressive problemshift. Research programmes anticipate novelty; they show heuristic power, unlike "pedestrian, trial and error." The positive heuristic shows how to build protective belts and thus generates "the autonomy of theoretical science." i.e. The problems to be investigated are contained within the protective belt and may have nothing to do with current anomalies (contra. Kuhn).

Lakatos claims that Bohr's research programme of light emission, in early quantum physics, was a progressive programme with a remarkable positive heuristic (although based on inconsistent foundations). Eventually, however, the programme degenerated and petered out. A rival programme - wave mechanics - was introduced and soon led to the discovery of new facts. It replaced Bohr's programme altogether by offering solutions to problems which had been completely out of reach of the older programme.

Thomas Kuhn[12]

The historical context of science is essential to the development of Kuhn's theme. In outline his thesis is that, out of a "pre-scientific" era of independent traditions, sometimes conflicting, there emerges a generally accepted professional consensus of ideas and methods - a *paradigm*.

This provides a framework for the development of *normal science* which is essentially *puzzle-solving* within the constraints of the paradigm.

Gradually *anomalies* arise and a state of tension develops which results in a *scientific revolution*: the paradigm is overthrown and a new one introduced. Normal science is again practised for a time until a new crisis develops which leads to a further revolution of thought, and so on.

Genuine scientific advance occurs only during periods of crisis and revolution; for the remaining time scientists are doing little more than marking time.

Paradigms. The concept is introduced as a body of accepted theory formally transmitted via text-books and teaching. Paradigms are essentially shared beliefs responsible for the behaviour of a community.

In response to critcism, Kuhn attempted to clarify his meaning.[13] He concluded that he had used the word in two different senses (a sympathetic critic claimed to have found more than twenty!)

(1) The entire constellation of beliefs, values, goals, techniques and so on shared by the members of a given community, including training of their successors: the disciplinary-matrix he called it. In this sense the concept is essentially sociological since it governs not so much subject matter as a group of practitioners.

(2) According to its second meaning, a paradigm is a successful practice -- a productive way of thinking or doing things -- shared by many people. In the course of his training a student will be presented with "practice problems" to solve. These will not only give him proficiency, but will help him to gain an insight into the empirical content of his studies. In Kuhn's language he is inducted into the paradigm.

Engagement with a variety of paradigmatic exemplars enable new relationships to be perceived; analogies are grasped, gestalt signals observed.

This, Kuhn argues, is how scientists themselves often solve puzzles, by modelling them on previous puzzle-solutions.

Normal Science. Acceptance of a paradigm (e.g. Aristotle's analysis of motion or Ptolemy's computation of planetary position) leads to mature science, in which practitioners are engaged in esoteric research into problems arising within the paradigm. New sorts of phenomena are not looked for since the paradigm theory not only defines the problems but guarantees that viable ('stable') solutions exist.

"Mopping up operations are what engage most scientists throughout their careers," as they "articulate the paradigm" (explore a relatively small field in depth.) Failure to solve a problem is looked on as failure of the scientist, Kuhn maintains, rather than failure of the paradigm.

Anomalies. Although normal science seeks no novelties of fact or theory, new discoveries are of course made. Now and again expectations based on a prevailing paradigm are not realised: an anomaly comes to light.

Large-scale paradigm destruction is preceded by a period of "pronounced professional insecurity" due to persistent failure in puzzle-solving.

Kuhn cites the state of Ptolemaic astronomy prior to Copernicus as one example of such failure, and the attempts to explain light and colour before Newton as another.

He also draws attention to other factors, such as sociological pressures, which may contribute to the breakdown of normal science.

Resolution of the crisis by acceptance of a new paradigm means that the newer not only replaces the old but is "incommensurable with (it); the profession will have changed its views of the field, its methods and its goals." There is now a new universe of discourse - a revolution has occurred.

Scientific Revolution. Kuhn draws an analogy between scientific and political revolutions. Prior to revolutions of both kinds there is a growing state of unrest as the inadequacies of orthodox solutions to current problems come to light.

Just as political revolutions aim to change political structures in ways that those structures prohibit, so scientific revolutions aim at paradigm-overthrow in ways that conflict with the reigning paradigms.

How are revolutions accomplished? Not, Kuhn suggests, by an immediate consensus of those involved. He points out that there were few converts to Copernicanism for almost a century after Copernicus' death, and Newton's views were not accepted on the continent for at least fifty years after the "Principia" appeared.

Kuhn likens the transfer of allegiance from one paradigm to another, to a conversion experience; the probability of such an experience notoriously decreasing with age. Instead of group conversion at one time, there is "an increasing shift in the distribution of professional allegiances."

Progress through Revolutions. Kuhn denies being a relativist. He appears to accept that objective progress is possible in science, but not towards an ultimate goal - truth. "We may... have to relinquish the notion...that changes of paradigm carry scientists and those who learn from them closer and closer to the truth."

Although we are accustomed to seeing science "as the one enterprise that draws constantly nearer to some goal set by nature in advance," Kuhn questions whether such a goal need be postulated. He suggests that, "If we can learn to substitute evolution-from-what-we-do-know for evolution-toward-what-we-wish-to-know, a number of vexing problems may vanish in the process."

Paul Feyerabend[14,15]

Feyerabend is a vigorous polemicist (in the best sense) who argues against all exponents of scientific methodology. A study of history, he claims, reveals that there is no consistent "scientific method" and the attempts by Popper *et al* to impose or expound one are misplaced. The only way to ensure progress is to take as our motto, "anything goes," - which we may call *epistemological anarchy*.

Feyerabend criticizes science education for isolating domains of knowledge from each other (e.g. physics from metaphysics; both from theology), with a resultant inhibition over boundary-transversal. The would-be scientist is not encouraged to use his sense of humour, imagination, or religion, in his scientific work: even the language he is expected to use is not his own. The scientific facts on which he relies are presented to him as if they are experienced independently of opinion, belief and cultural background. For Feyerabend, however, the world is a largely unknown entity and we should keep our options open.

All universal standards and rigid traditions (and much contemporary science) must be rejected. Uniformity not only endangers the free development of the individual, it impairs the critical power of science, which benefits from a proliferation of theories.

Considerable blame is apportioned by Feyerabend to modern empiricism. Some of its methods "introduced in the spirit of anti-dogmatism and progress are bound to lead to the establishment of a dogmatic metaphysics and to the construction of defence mechanisms which make this metaphysics safe from refutation by experimental enquiry."

Accepted theories should be persistently criticized in a manner which goes beyond the criticism provided by a comparison with the facts - a science that is free from metaphysics is well on the way to becoming a dogmatic metaphysical system.

Variance of Meaning. Decision between alternative theories is based on crucial experiments and is to that extent empiricist. However, experiments may fail to achieve their objective unless viewed against a more general background theory, which supplies a stable meaning for the "observation sentences."

Feyerabend argues that this background theory is itself in need of criticism - which implies that observation languages are not stable. Hence, empiricism cannot be made a universal basis of all our factual knowledge.

Since meanings are not invariant, we must not rate their importance too highly. Semantical flexibility - even sloppiness - is a prerequisite of scientific progress.

Excellence of Science. A further opinion that Feyerabend states vigorously is that science is not sacrosanct; it is not some special kind of knowledge superior to all other kinds.

He claims that the excellence of science must be argued, not gratuitously assumed. Science should be considered as one form of knowledge or belief among others, e.g. magic, myth, religion.

To this end, science must be separated from the State, just as was the Church in earlier times, and for similar reasons. Feyerabend argues for a "free" society in which each person believes and behaves as he chooses, avoiding all claims to absolute truth, and tolerating the beliefs and behaviour of others.

He believes that the relativism thus advocated would not lead to chaos, any more than the gradual removal of religion from the centre of society did.

In science, freedom from a restricting methodology does not mean, he maintains, that research is arbitrary and unguided. The necessary standards arise from the research process, not from some preconceived pattern of rationality.

These standards are developed and examined by the very research process they are supposed to judge.

Neither does science command special respect because of its undoubted pragmatic success. Competing ideologies may temporarily "run out of steam," but need not be eliminated for that reason. Later they may return in fresh triumph, as happened to the philosophy of atomism.

Unfortunately, Feyerabend says, experts and power groups have succeeded in suppressing ideologies other than that of science, so that the supposed 'superiority' of science is due, not to research, but to political and institutional pressures.

General Comments

A comprehensive critique of the views presented cannot be given within the confines of this paper, even if I were competent to tackle the task.

Nevertheless, certain points may be made. The obvious one is that, with such widely divergent views from which to choose, it seems remarkably difficult to reach agreement about the 'scientific method' if such a method exists.

Moreover, the views we have outlined have in the past made little impact on practising scientists.

Medawar[16] notes wryly that "If the purpose of scientific methodology is to prescribe or expound a system of enquiry or even a code of practice for scientific behaviour, then scientists seem to be able to get on very well without it. Most scientists receive no tuition in scientific method, but those who have been instructed perform no better as scientists than those who have not. Of what other branch of learning can it be said that it gives its proficients no advantage; that it need not be taught, or if taught, need not be learned?"

A notable shortcoming in the theses of Popper, Kuhn and Lakatos, who base their arguments upon an interpretation of history, is the paucity of examples used and the almost exclusive reference to physics.

L. Pearce Williams,[17] historian, commenting on the Popper-Kuhn disagreement in particular, asks what practitioners of mature sciences think they are doing (in contrast with what philosophers say they are doing or should do). We simply do not have this information, he says, so that the history of science is unable to bear the load imposed upon it.

The Popperian function of experiment, as a means of falsification or corroboration of a theory; and his view that theory, never experiment, opens up the way to new knowledge, is certainly not that held by P.W. Bridgman, the physicist.[18] For Bridgman, experiments are important for two reasons. Firstly, they make possible the exploration of new territory. Indeed, experiment creates the previously unknown world, as in modern chemistry or nuclear physics - worlds which have no existence outside the laboratory.

Secondly, experiments facilitate understanding; by experiment "we can pick a situation to pieces and analyse it...and thus reduce to order situations which otherwise might be so complicated as to be wholly (in)-tractable."

Moreover, Bridgman argues that it is not necessary to have some clearly stated hypothesis in mind which the experiment is supposed to be testing. In his own work on the effects of pressure, the interest "was almost entirely in discovering what new things there were in fields hitherto unexplored." Although, as he says, there was always some kind of expectation, this could hardly be dignified by the title of 'theory.'

While Popper, Kuhn *et al* have given us valuable insights into the nature of science, the impression they give is that 'science' is a more or less homogeneous activity, whoever is engaged in it. In fact, there is no reason to suppose that the methodology of the theoretical physicist is identical, even in principle, to that of the pharmacologist, and both may well differ from that of the anthropologist.

Mary Hesse[19] points out that, "A science whose aim is application and prediction may have different normative requirements from one which desires truth, beauty or morality. Sometimes comprehensive theories of maximum content are appropriate, sometimes instrumentalist predictions, sometimes inductive inferences. It is a naive reading of the history of science to suppose that different methodologies are necessarily in conflict given their different aims. The logic of science should provide a comparative study of such methodologies, rather than a partisan polemic on behalf of some against others."

In developing the case for a limited form of induction, she says that Popper's view cannot even be stated without inductive assumptions.[20]

The Non-rational Element. Max Born[21] is of the opinion that most physicists are "naive realists"; that is, they get on with observing, measuring, calculating, without bothering too much about philosophical subleties - at least, until they begin to theorize. Probably this is true of scientists in most other disciplines also.

Theorizing, however, particularly at the depth involved in physics, brings up the ancient epistemological problem: to what extent (if any) do our observations of the world give us reliable knowledge of the underlying reality?

Feyerabend[14a] argues in effect that we can never know; the acceptance of one hypothesis in preference to another is little more than a "propaganda victory", in the words of Lakatos.

Kuhn[22] holds that the apparent purposeful design of the human eye and hand is quite illusory. He asks, "What must the world be like in order that man may know it?" and considers the question unanswered - and by implication unanswerable.

Although Popper[4c] views science as a search for 'true' theories, he says we may never know them as true, even if we attain them.

Many others have had similar 'uneasy' feelings about our relationship to the external world. Thus Brillouin[23] quotes with approval Planck's postulates that (a) there exists an outside world independent of us and (b) this world is not directly accessible to us. (Both are aware of the inconsistency). Brillouin adds, "there is no way to avoid the irrational element in science."

Kant of course held a similar view, and the problem is vividly presented by Ryle[24] who points out that what the neurophysiologist who studies perception in the laboratory discovers, and what is really there, are separated by a crevasse which no man can bridge. "While at work in the laboratory he makes the best possible use of his eyes and ears; while writing up his results he has to deliver the severest possible censure upon these sham witnesses. He is sure that what they tell us can never be anything like the truth just because what they told him in his laboratory was of the highest reliability."

Thus it seems that 'modern Gnosticism' holds that matter is not so much evil as simply misleading. The issue is one of the degree to which we can trust our perceptions of nature to give us a reliable understanding of the external world.

Nearly half a century ago Professor Butterfield[25] reminded us that, for Descartes, science is based upon theological considerations. We trust our senses and our rational faculties because we believe that God is no deceiver. The order and intelligibility in nature are a natural consequence of a God who is the author of it all (cf. Colossians 1: 15-20 and Genesis 1).

Without being a naive realist (cf. Hebrews 11: 3) the Christian has every right to challenge those who boldly assert that the world is unknowable; how do they know?

> There are great scientific names committed to the view that this solid and tangible world, which they have studied in so much detail, is unknowable, insubstantial and quite untouchable...upon this same foundation they base a whole religious scheme, which generally deposes man from his central position in Christian thought... The contention that objects cannot be really touched, though it may indicate a significant aspect of the structure of matter, is nevertheless a red herring for scientific philosophers... Microphysics has no bearing on ordinary tangibility. When a savage strikes a scientist he touches him in the only sense that matters even though his hand be made of electrons and suchlike... The Christian...does not look for insecurity specially in the molecular nature of matter, or in the denial of what little his senses do tell him. Nor does he seek for mystery only in scientific abstraction, for he finds

> it in ordinary things, even in matter-of-fact solidity... In this way he avoids the eccentric pessimism which besets those who relegate him to the position of interloper erring vainly in a universe devised as it were by a calculating genius.[26]

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18 Bridgman, P.W., *The Way Things Are*, Harvard U.P., 1969, pp. 130-132.

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20 Hesse, Mary, Ref.19, p.95, "...it is not clear that the notion of a 'severe test' is free of such assumptions. Does this mean 'tests of the same kind that have toppled many generalizations in the past,' which are therefore likely to find out the weak spots of this generalization in the future? Or does it mean'tests which we should expect on the basis of past experience to refute this particular generalization?' In either case there is certainly an appeal to induction." (et seq.)

21 Born, Max, *My Life and My Views*, 1968, p.167.

22 Kuhn, Thomas S., Ref.12, pp. 172-3. Compare the view of Braithwaite, R.B., where 'teleological explanation' is taken to mean 'goal-directed' activity without a director, in *Scientific Explanation*, Cambridge U.P., 1968.

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