Author/s:
Stokes, Terence Douglas

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VI INVENTION AND INTRATHEORETIC APPRAISAL

(132) The advent of the SBS model of the structure of DNA provides the opportunity for a case study of a crucial process in science which has been neglected - the development of radical new theory out of established belief. It will be evident from chapters I and II above that the 'warped zipper' structures for DNA developed independently in New Zealand and India were indeed a radical alternative to the Watson-Crick double helical structure. And, as is clear from chapter IV, prior to the SBS models the established view of the scientific community was that DNA was double helical.[32] Moreover, chapters I and II revealed that in both India and New Zealand, the 'warped zipper' had its origins in dissatisfaction with the received view of the structure of DNA. There are two reasons why circumstances like these have not attracted much attention from philosophers of science:[33]

(133) Firstly, post-Popperian philosophers of science have almost invariably come to the conclusion that the epistemic appraisal of theories is always comparative (<u>inter</u>theoretic).

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32 Thus Hamilton (1968) maintained that "X-ray diffraction and model building have elevated the Watson-Crick pairing hypothesis to the level of experimentally established fact.... [and] gone a long way to establishing that the double helical feature and base pairing scheme is unique [p.636, <u>q</u>.<u>v</u>., (90)]..."

33 With the notable exception of T.S. Kuhn, whose views will be discussed later.

Of course, theories may be altered and developed solely as a consequence of their interaction with their domains (that is, without reference to, or in the absence of any competitor). But Kuhn, Lakatos and Laudan - to take just three major figures - have all insisted that this kind of interaction does not account for the way that theories are abandoned or adopted. Rather, they argue, this occurs on the basis of their success or failure compared with their competitors in the same domain.[34] Thus the key epistemological question is not, as it was for Popper, under what conditions will a theory be falsified by predictive failure? Instead the significant issue is how do we tell which is the preferable theory?

(134) The appraisal of the SBS model of DNA by the specialist scientific community confirms the current philosophical emphasis on intertheoretic assessment. Most significantly, none of those who criticised the double helical model, however trenchantly, suggested its rejection unless they were proposing an alternative [q.v., III]. Even Gorski, who reached the conclusion that strand separation by unwinding "should...be envisaged as an impossibility [(1975), p.94, q.v., (75)-(79)]", felt himself obliged to advance a bizzare alternative in order to be taken seriously [Gorski (1976), q.v., (80)]. And, once the SBS model had been advanced, its supporters were required to produce a 'crucial experiment' distinguishing favourably between their

34 See Kuhn (1970), Lakatos (1978), and Laudan (1977).

structure and that of Watson and Crick [q.v. (108) and (114)].[35] There is, then, no consolation for the falsificationist in the appraisal of models of DNA by the scientific community.

(135) One reason for an interest in the development of novel theory out of established theory derives from the Popperian view that the motivation for devising a novel hypothesis is the failure of an existing hypothesis to survive testing. But if the success or failure of an hypothesis were measured only by reference to extant competitive alternatives, it would be hard to find a place for scientific creativity. Even so, the Popperian falsificationist is uninterested in the actual generation of novel theory. And this is because both those who favour intra-theoretic appraisal (rigorous testing of the predictions and explanations of a single theory), like Popper, and intertheoreticians like Laudan reject the possibility of the misleadingly named 'logic of discovery'.

(136) With this view we come to the second of the two reasons for the philosophical lack of interest in how new theories are devised. Reichenbach (1938) distinguished between what he called the 'context of discovery' and the 'context of justificiation' in science. He held that only the latter is of epistemic and,

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35 Many, if not most philosophers of science now hold, with Lakatos, that there "are no such things as crucial experiments, at least not if these are meant to be experiments which can instantly overthrow a research programme [(1970), p.173, emphasis in the original]."

hence, philosophical interest. Although as McLaughlin observes, "Reichenbach is usually regarded as the primary source of the discovery / justification distinction [(1982b), p.199n]", Popper had made a very similar point somewhat earlier. He maintained that

> The initial stage, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor to be susceptible of it.

Whereas

> As to the task of the logic of knowledge...I shall proceed on the assumption that it consists solely in investigating the methods employed in those systematic tests to which every new idea must be subjected if it is to be seriously entertained [(1972), p.31. Originally published in 1934].

(137) Despite this agreement between Popper and Reichenbach on the proper object of philosophical enquiry in science, Popper chose to translate the title of the work in which he made these remarks, Logik der Forschung, for the first English edition of 1959 as The Logic of Scientific Discovery.[36] Although for Popper the expression 'logic of scientific discovery' applied only to Reichenbach's context of justification, it has come to refer in philosophical use to both Reichenbach's contexts, discovery and justification.[37] To avoid these terminological confusions, McLaughlin has suggested the following conventions:

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36 As McLaughlin (1982a) points out, a more straightforward translation of the German would have been 'the logic of scientific research'.

37 Popper and Reichenbach did not, of course, agree on the nature of the 'context of justification'. The expression itself reflects Reichenbach's inductivism. Popper would no doubt prefer something like 'context of falsification' or 'context of refutation'.

> First, 'discovery' should be confined to the sense of 'initially encountering in nature' some object or property. Second, what Reichenbach called the 'discovery' of hypotheses - i.e., 'hitting upon' them - should be called the invention of hypotheses. This term has the merit that no oddity is involved in speaking of a hypothesis being invented and later falsified. Third, what Popper...meant by 'discovery' should be called the appraisal of hypotheses. This term has the virtue of neutrality as between falsifying (Popper) and justifying (Reichenbach) a hypothesis. [See note 37.] Then I can speak of the context (and logic) of invention, and the context (and logic) of appraisal, without the ambiguities and methodological commitments deriving from Reichenbach's unfortunate pair of terms [(1982a), pp.71-72, emphasis in the original].

(138) Here McLaughlin's proposals concerning invention and appraisal will be adopted. The term 'discovery', however, is inerradicably transcontextual. It necessarily entails an ontological and hence an epistemological commitment - even when confined to the sense of 'initially encountering in nature'. When what is thought to be an object or property is apparently encountered it is certainly said have been disovered. But, just as an hypothesis may be invented, accepted, and later rejected by the scientific community, so too may an object or property. This is especially often the case for the more 'theoretical' entities. But no hard line may be drawn between observational and theoretical entities. Thus, in the late nineteenth century, many astronomers of high repute were certain that they had seen the intraMercurial planet Vulcan. Similarly, Galileo believed that he had seen 'jug-handles' protruding from Saturn. A celebrated case is Priestly's claim to have discovered dephlogisticated air and Lavoisier counter-claim to have discovered oxygen. Only those objects and properties which do exist are discovered by the first person to encounter them and to know that they exist (i.e., under a correct description).

(139) Crick raises the issue of discovery in his Foreword to Olby's (1974) The Path to the Double Helix. He remarks:

> If Watson had never come to Cambridge, who [else] would have discovered the structure [of DNA]? .... After all, the structure was there waiting to be discovered - Watson and I did not invent it [p.vi].

To this Jevons has - correctly I think - responded:

> The double helix structure of DNA, I would contend, is a theory about nature, not a fact - a very well corroborated theory, it is true, but falling short of the final and unalterable certainty implied by the word 'fact' as commonly used [(1979), p.16][38]

Clearly, the difference between these two views lies in their respective assessments of the epistemological and, consequently, ontological status of the double helix. But, supposing that Crick were right in his appraisal (i.e., supposing that DNA is double-helical), would it not be strange to describe their model as having been invented? Here again, I think that Jevons has the correct view. He says: "The first creation of...a theory cannot be a discovery because it was not previously there to be discovered [idem.]." This is true even if the theory is wholly sound (corresponds with reality). But when a theory, having been invented, is subsequently adopted, it is said to have been discovered. So events in both the context of invention and that of appraisal are required. Thus we may say that Watson and Crick invented a structural hypothesis which, upon appraisal, was accepted; the conjunction enabling a claim of discovery.

(140) There is a loose sense of 'discovery' whereby the first

38 Though these comments were written after the SBS models of DNA had been advanced, Jevons was not then aware of them.

person to encounter an object, property or state of affairs - or to invent a theory - is credited honorifically with its discovery even despite what later seems to be an inadequate or wholly incorrect description. But the discovery is then re-described in currently accepted terms. Thus Galileo is credited with having discovered the rings of Saturn (not its jug-handles), and Columbus is considered to have discovered the Americas (not the Western passage to the East Indies, and despite the considerable priority of the Amerindians). Nevertheless, epistemic claims are sometimes paramount - especially when one claim to subsequently taken to be more correct than another. Thus Lavoisier, and not Priestly, is generally held to have discovered oxygen - which emphasizes the transcontextual character of discovery.[39]

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39 So Brannigan (1981) observes that:

> The attribution of the status, discovery, is founded on the processes of social recognition by which the announcement of an achievement is seen to be a substantively relevant possibility, determined in the course of motivated scientific investigations or schemes of research, whose conclusion or outcome is convincingly true or valid, and whose announcement is, for all appearances, unprecedented [p.77].

His study explores the aspects of discovery which lie within the context of appraisal: "we should explain how certain achievements in science are [socially] constituted as discoveries - and not how they occurred to an individual [p.11, emphasis in the original]." This is undoubtedly an important goal, to which Brannigan has made a valuable contribution. But he throws the baby out with the bath water by treating all accounts of the invention of a novel idea - even those which offer a logical analysis - as psychological because they are all "mentalistic" [pp.43-45]. This renders philosophy to psychology, collapsing the distinction between rational and non-rational "mentalistic" processes. Brannigan correctly claims that philosophical and psychological explanations are incomplete; but that does not make them irrelevant or even subsidiary to sociological inquiry.

(141) As McLaughlin observes,

> an apparent majority of empiricist-oriented philosophers since the 1930's has insisted that there can be no logic of invention, and that the study of invention is no business of philosophy...[(1982a), pp.72-73].

The development of radical theoretical innovation out of established belief has, therefore, been of little interest both for this reason, and because of the (somewhat more recent) insistence upon a comparative (intertheoretic) logic of appraisal outlined above [(132)-(135)].

(142) Given this, in order to show that the genesis of the 'warped zipper' models of DNA in New Zealand and India are of potential philosophical interest, I will argue that, (i) the appraisal of scientific theories is not and ought not to be entirely intertheoretic, and that (ii) the invention of new scientific theory in the circumstances where a single theory dominates a given domain is and ought to be of epistemic and hence philosophical interest.[40] The present chapter is henceforth concerned with establishing (i). I will take up (ii) in the next.

(143) Perhaps the strongest pragmatic argument favouring intertheoretic comparison as an essential feature of scientific

40 I do not subscribe to the widespread and, more often than not, implicit view that the philosophy of science is identical with the epistemology of science (there are, to take but one obvious example, pressing ethical considerations which arise out of the practice of science). Nevertheless, I take it that it is sufficient (though not necessary) to establish the philosophical legitimacy of the context of invention that it have a demonstable epistemic content.

appraisal is that scientists, for the most part, act according to this view. They do not, in the main, abandon well established and successful theories unless and until there is what they take to be a preferable alternative. Not only is this well attested historically (it is, for example, true of the Watson and Crick double helix [q.v., esp. (114)], there are also powerful grounds for such behaviour. First and foremost, whatever its deficiencies, a well established and successful theory *ipso facto* accounts unproblematically for a good deal, maybe practically all of its domain. Thus, as Laudan says, if "the occurrence of even one anomaly for a theory should force the rational scientist to abandon it", then "we should find ourselves abandoning our entire theoretical repertoire in whole-sale fashion, and thereby [be] totally unable to say anything whatever about most domains of nature [(1977), pp. 26 and 27-28]." Moreover, as Kuhn and Lakatos have emphasised, it is rare indeed for a theory not to be confronted by many recalcitrant anomalies. And all of this has more than an abstract theoretical relevance; for scientists do not merely seek to understand nature, they also wish to *use* their current best understanding to control and manipulate it. This is the commercial and political basis of science, the source of its funding.

(144) I shall not further rehearse these and other important arguments buttressing the claims of intertheoretic appraisal against those of intratheoretic assessment of scientific theories. Rather, I will accept that the scientific community does not and should not abandon well established and successful

theories unless they are presented with a better alternative. This is not to say that scientists will persist with theories which give a grossly inadequate account of important (especially practically important) parts of their domains. Such theories are manifestly not successful in significant respects, nor are they likely to become or to remain well-established. Moreover, as was mentioned at (133), theories alter and develop solely as a result of their interaction with their domains. Indeed, if Kuhn and Lakatos are correct, this is precisely what normally occurs.[41] This apart, a theory is only rejected in favour of a better.

(145) For all the cogency of the intertheoretic position on appraisal, it does not address one crucial question; namely, where do the competitors, by which a theory is comparatively assessed, come from? This question does not trespass upon the context of invention, for what is at issue is not <u>how</u> alternative theories are devised but <u>why</u>. And this issue is patently within the ambit of traditional epistemology. One response, Popper's, is that new theories are and should be devised when old ones have been refuted by failed predictions (<u>modus tollens</u>). In a modified form, this is Kuhn's view too. He holds that new 'paradigms' emerge out of a 'crisis' of confidence in the old one; a crisis provoked by a number of recalcitrant and significant anomalies, and exacerbated by the apprearance of

41 Here, I do not consider the situation where a theory emerges in a domain where no previous account existed, or where no previous account received general assent - this being the situation when Watson and Crick advanced their model of DNA.

alternatives [see Kuhn (1970)].

(146) Actually, Kuhn occupies something of a middle position between inter and intratheoreticians of appraisal. Whilst insisting that the rejection of what he claims is usually a single dominant paradigm by the scientific community only occurs when a superior alternative is available, Kuhn nevertheless addresses the question of how the successor paradigm comes into existence. Thus, like Popper, Kuhn provides an account of the <u>motivation</u> for scientific creativity. But the intertheoretic methodologists have argued that there are serious flaws in Kuhn's analysis of the genesis of novel paradigms. Indeed one of them, Laudan, felt that he could side-step the issue. In his view:

> Virtually every major period in the history of science is characterised both by the co-existence of numerous competing paradigms, with none exerting hegemony over the field, and by the persistent and continuous manner in which the foundational assumptions of every paradigm are debated within the scientific community [(1977), p.74].

Moreover, he says,

> Numerous critics have noted the arbitrariness of Kuhn's theory of crisis: if (as Kuhn says) a few anomalies do not produce a crisis, but "many" do, how does the scientist determine the "crisis point?"[<u>idem</u>.]

Again, Lakatos observes: "There is no particular rational cause for the appearance of a Kuhnian 'crisis' [(1970) p.178]." Moreover, Lakatos accuses Kuhn of giving a sociological and psychological rather than rational methodological explanation of theory genesis, of the "psychologism" of attempting to reduce the philosophy of science to the psychology of science [idem.]; a charge repeated by Popper (1970).[42]

(147) The plain fact of the matter, pace Laudan, is that at least at some times, and in some fields, science is characterised both by the existence of a single theory which exerts hegemony over an important domain, and by a weak and sporadic debate over its foundational assumptions. The case in point is the Watson-Crick double-helical model of the structure of DNA. I have argued elsewhere [Stokes (1982)] that the double helix may fruitfully be viewed as what Kuhn calls an 'exemplar'; in his view the most important constituent element in a paradigm.[43] At no stage prior to the publication of the SBS models of DNA in 1976 - that is, for twenty three years after the Watson-Crick model was first advanced - was the helical character of the DNA exoskeleton doubted. And, I have argued [idem.], the received view of the double helix and the reaction to what little criticism of it that there was are in accord with Kuhnian expectations. I will return to this issue in a later chapter. For present purposes it is sufficient to note that the account of the testing and refinement of the double-helical model given in chapter IV, and the description of the disciplinary attitude toward the unwinding problem to be found in chapter III is

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42 Popper (1972) introduced the term 'psychologism' to refer to those who claim a legitmate philosophical place for the study of the context of invention.

43 Kuhn's refinement and elaboration of his original concept of a paradigm may be found in his (1977a)

inconsistent with Laudan's claims at (146). Throughout the period from 1953 to 1976 molecular biology was dominated by a single conception of the structure of DNA, Watson and Crick's. The debate concerning its foundational assumptions, though extant, was weak and sporadic. And the development of molecular biology since 1953 is unquestionably a major period in the history of science"!

(148) Thus Laudan and his fellow intertheoreticians cannot side-step the question of why novel theories such as the 'warped zipper' structures of DNA are and ought to be devised by resort to the historical claim that alternatives are omnipresent. They are not always. It is hard to determine empirically the frequency of such cases, but there are general grounds for supposing that it is not low. The scientific community quite often believes that it has discovered the truth, or some reasonable facsimile thereof. Moreover, scientists - as has already been noted - <u>use</u> their theories, both for practical (technological) purposes and as a basis for further research. Together these two activities account for the bulk of scientific activity, and neither can occur without the presumption of a secure theoretical base. Even Popper - who would have it that the business of science consists quint- essentially in detecting significant falsehoods - attempts to take account of this with his theory of 'corroboration'.[44] We may, therefore, recognise that wide-

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44 See Popper (1972), chapter 10. However, Popper's theory of
Footnote continued

spread confidence in a theory with a co-relative low level of criticism of it will arise whenever a scientific community believes that an hypothesis or theory has achieved the status of 'fact'. (This, as we have seen, was claimed for the Watson-Crick model of DNA by Hamilton (1968) [q.v., (90)]).

(149) This being so, we appear to have reached something of an impasse. On the one hand, acceptance and rejection by the scientific community appears intertheoretic. And this obviously requires that there be a minimum of two theories in any given domain. On the other hand, I have claimed that we should expect that this will not always be the case; and that it is not the case in the example under detailed consideration here. Indeed, we should expect there to be but a single theory in a given domain on just those occasions where a scientific community has behaved according to the intertheoreticians' requirements and accepted that theory as preferable to its competitors. No doubt appraisal is sometimes a matter of plumping for the best of a not very satisfactory set of alternatives. But there will also be those occasions when the choice is decisive and all concerned are content.

(150) All of which returns us to Kuhn's view - that of the only major intertheoretican who has tried to elude this apparent

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44 Continued
corroboration is merely consistent with frequent acceptance of a theory. Nonetheless, use of a theory as a basis for further research, and in practical application constitutes a kind of indirect testing.

impasse. Rodley and Reanney, in their (1977) pamphlet describing the SBS model to a lay audience, described the Kuhnian perspective as follows:

> In science, theories can often run into trouble after periods of considerable success. New information becomes increasingly difficult to accomodate within the framework of existing theories and adjustments have to be made to the original theory. This may continue for some time and it may lead to a better theory. What can also happen is that a theory may be so stretched that is is ultimately seen to be unworkable. A new theory must then be sought to explain existing information [p.49].

But, as was indicated at (146), this conflicts with other aspects of science which Kuhn has also stressed. Quoting again from Rodley and Reanney (1977),

> Once a theory becomes reasonably well established there is a strong tendency for it to dominate completely the area of science it applies to. Awkward features are overshadowed by successes. There are many instances of this type where a theory has been proposed and confirmed over and over again by experiment so that it seems beyond criticism. Newtonian mechanics is the classic example [<u>idem</u>.].

Rodley and Reanney believed that these remarks cover their own situation:

> The structure of DNA may turn out to be a[n]...example, although it should be pointed out that the major deficiency of the [double-helical] theory [unwinding] was recognised at the outset. ....However, the extraordinary success of the model undoubtedly diminished concern about this awkward feature, and particularly with the discovery of nucleases and ligases [nicking and closing enzymes] this is now not thought to be a problem. We are at the stage of development in DNA chemistry where it is very unlikely that current workers [will] consider the possibility of interpreting their results in terms of any model other than the double helix [<u>ibid</u>., pp.49-50, see esp. (62), (82) and (85)].

(151) These comments were made after the SBS model had been developed and published. But, if they apply, then they apply with greater force to the situation prior to that. The general point is this: Kuhn regards science as profoundly conservative; and, in his view, to provide motivation for change there must be a generally felt 'crisis' of confidence in the adequacy of the status quo. Popper, too, recognised the conservative instinct, and sought to combat it with an ethic which strongly reinforced seeking falsifying anomalies - for Popper reviled scientific conservatism. But Kuhn embraces it, elevates it as the reason for science's success. And there certainly are positive features to the conservative side of science. But, because of his emphasis on conservatism, Kuhn must require a significant breakdown in the effectiveness of a paradigm as a pre-condition of creative theorizing - normally, in his view, a waste of time and effort. However, the very forces which drive scientists toward consensus on the adequacy of theory, and which minimize the importance of its difficulties, maximizing perception of its successes, must mitigate strongly against the possibility of a wide-spread disaffection developing.

(152) So it does not appear promising to suppose, with Kuhn, that new theories are motivated by such a general dissatisfaction with old ones (though, doubtless, should such a state of affairs exist, it would provide a powerful stimulus for novelty). Yet it seems implausible and counter-intuitive to suppose, with Popper, that the genesis of new theory is opaque to analysis, essentially irrational and intuitive; the result, perhaps, of the turmoils of the unconscious - that it is some-

times appropriate to an (unfelt) need, sometimes not; sometimes relevant and superior to extant theory, sometimes not. Above all, it seems bizzare to suppose the origins of novel theory bear no relation whatsoever to extant theory; that, as the result of some random process, they appear out of thin air; that physical theories are as likely to occur to botanists as to physicists, as likely to be devised by those who search deliberately as to those who do not.

(153) Such a view of the genesis of new scientific ideas is implausible and counter-intuitive precisely because the process does not look thoughtless and due to pure chance. New theoretical ideas seem to occur to people who are looking for them and who are trained and experienced in the relevant field. Moreover, when these novel hypotheses occur, they tend to relate to the kind of problems which those who sought them wanted to solve - though, of course, they are sometimes poor solutions for one reason or another (perhaps, in solving the desired problems they create more and worse difficulties). As a rule, physicists devise physical theories related to pre-existing physical problems rather than biologists or carpenters who are unaware of the problems and unable to understand the theories. We hear, of course, of the great ideas that come out of the blue in the bath or getting on the bus. But are we inclined to suppose that this is typical; or conclude that no significant thought preceded such sudden revelation? I think not. Both more ordinary, and even extraordinary theorizing seems more plausibly to be the product of hard thought about specific problems - even if we cannot quite say how the thought relates to the outcome and its

adequacy, and even if we are prepared to admit that an element of the non-rational is present.[45]

(154) In his writing Kuhn speaks very nearly always of scientific communities, rather than of individual scientists (hardly surprising given the sociological focus of his attention). However most, if not all, philosophers of science refer in a similar way to an ideal type of the scientist. Thus, for example, whatever criteria of theory appraisal issue from an epistemology, apply to <u>any</u> and all scientists. Whatever is correct behaviour for one scientist is incumbent upon all. Allowance may be made for situations were all of the relevant evidence is not available; either to a given scientist or to the community as a whole. And this may be used (as it is, for example, by Lakatos) to explain the absence of unanimity in practice. Even so, there will still be criteria according to which, when sufficient evidence <u>is</u> available, consensus is in principle obligatory.

(155) The assumption that the canons of method have a universal and compulsory application is plausible enough. In an analogous way, the civil and criminal law is a catholic obligation. What equity requires of the law, reason seems to dictate to science. None should be above the law, and none should pretend superiority to reason. But reason is univocal - even if only in principle - where there is but one goal in whose service

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45 In making such an admission, we are not conceeding much. There is no algorithm to guarantee victory in chess. Yet it is hard to think of a game where reason is more important.

it is. And surely the pursuit of empirical knowledge is singular enough a goal? Perhaps; but it by no means follows that empirical knowledge can only be gained, or is even best gained by unanimity of opinion and action. We have already seen that the prevailing intertheoretic view of appraisal demands that there be at least two theories in any domain and, along with them, groups of scientists that support one theory and oppose the other. This supposedly institutional state of affairs occurs despite allegedly common principles by which agreement might be reached. Thus we are to believe that the crucial existence of competing alternative theories depends wholey and solely on a dearth of evidence which would settle the matter.

(156) As I have argued, the matter is at least sometimes settled; leaving us with the impasse outlined at (149), namely a single well-established and successful theory dominating a particular domain. Thus, the very unanimity of judgement that intertheoreticians expect when their criteria of assessment are applied they sometimes obtain - producing within the logic of their own approach a special urgency to the question where do new theories come from? For, <u>ceteris paribus</u>, the answer would appear to be nowhere in particular.

(157) Notice, though, that scientific communities do not devise novel theories. Even on Kuhn's schema, that is achieved by <u>individuals</u> (though, on Kuhn's view, not many will be trying). However opaque the context of invention may be, we can at least say that original thought is a subjective and not an intersubjective experience (notwithstanding that it can be

co-operative, and that different scientists may independently arrive at the same new idea). Similarly, of course, each individual scientist makes her or his own assessment of the adequacy of hypothesis. It does make good sense to think, as most philosophers do, of acceptance and rejection of scientific theories as being a collective phenomenon. But it makes no sense at all to regard the invention of scientific theories in this way. We consider the appraisal of any given theory as being the product of a kind of informal vote taken by the group of scientists qualified to judge the issue. No individual scientist is given this responsibility - though each partic- ipates, and individuals may well receive credit for having conducted what come to be regarded as decisive experiments. By contrast, the scientific community as a whole never takes collective responsibility or credit for devising theories, whether or not it accepts them.

(158) Yet the intertheoreticians' view prevents us from entertaining the possibility that one scientist might legitimately set about creative thought. For example, Laudan [(1977), pp.118-119] points out that arbitrary elimination of the threat posed by anomalies (apparently falsified predictions) to a scientific theory is always possible. He argues that, in the absence of any competitor which can solve the problem, the problem-solving capacity of the theory is undiminished. Thus no scientist can justifiably reject such an arbitrary move. Consequently, Laudan's approach prevents any motivation arising for the development of new theory because of the unresolved anomalies in an old one. Popper [(1972), pp.78-84] draws exactly

the opposite conclusion, arguing that no scientist can justifiably accept arbitrary elimination of anomalies. But, given the ubiquity of anomalies in scientific theories pointed to by Laudan, Lakatos and others, Popper's position paralyses science by preventing any theory being accepted and used.

(159) This 'catch 22' situation is avoided if we hold that, whilst it is true that the scientific community as a whole will, and should, only reject a theory when another, preferable theory is available, individual scientists will and should at least tentatively reject a well-established and successful theory just in order to devise a preferable alternative. It will easily be appreciated how helpful this approach is. On the one hand, we can yield to the powerful arguments favouring intertheoretical appraisal. We can admit that, because of their need to use well-established and successful theory as a basis for further research, and in order to control and manipulate nature, the scientific community as a whole (whose main interests these are) will cleave to such a theory unless they are presented with a preferable alternative which also permits practical application and research to continue. On the other hand, we can bend to the strong intuition that scientists devise novel hypotheses because they are dissatisfied with specific aspects of extant theory. It is also possible, on this formulation, to adopt what seems useful from Kuhn's view of the genesis of new hypotheses whilst avoiding the difficulties associated with it. In the the circumstances where a single theory dominates a particular field we can accept that wide-spread discontent is unlikely to develop - whatever difficulties there are - just because the theory is

established and successful. Thus 'crises' in the scientific community as a whole are not to be expected. Yet it remains possible for individuals who are worried by specific difficulties to regard them as warranting a search for an alternative. With Kuhn, then, we can see novel theory as arising out of perceived defects in existing theory. But, unlike Kuhn, the process will be conceived as rational (for there are reasons for the rejection of established theory) and not psychological or sociological.

(160) In Kuhn's view

> Science has its elite and it may have its rear guard, its producers of Kitsch. But there is no scientific avant-garde, and the existence of one would threaten science....
>
> The function of crisis in the sciences is to signal the need for innovation, to direct the attention of scientists toward the area from which fruitful innovation may arise, and to evoke clues to the nature of that innovation. Just because the discipline possesses this built-in signal system, innovation itself need not be a prime value for scientists, and innovation for its own sake can be condemned [(1977b), p.350]

I accept Kuhn's claim that science places no prime value on novel theorizing. But I do not accept the suggestion that innovation is or ought to be stimulated by a kind of collective anxiety mounting toward hysteria. Kuhn sought to provide a social explanatory framework. But he overemphasizes the unanimity of scientific judgement almost to the point of caricature. Even so, he is correct insofar as he directs attention to the conservative nature of science, a conservatism resulting from the need to use and develop theories. And since this is the main occupation of scientists, few engage in innovation when a theory still works. Moreover, whilst pract- ical application and development proceed, no 'crisis' can be expected. Yet, <u>pace</u> Kuhn, even without 'crisis' science needs and has an avant-garde which <u>is</u> pre-occupied by the problems in theories, rather than by its potential for refinement, extens- ion and use.

(161) A scientific avant-garde does not threaten science simply because its activities do not impinge upon the majority unless and until they yield results. Results which eliminate difficulties and permit continued development and application by that majority. And the existence of a scientific avant-garde permits an orderly and rational progress in science from predecessor to superior successor theories. That is, its defense and warrant.

(162) Thus the context of appraisal is not entirely intertheoretic. To be sure, for most scientists it is and should be. But, for the innovative minority, the avant-garde, appraisal must necessesarily be <u>intra</u>theoretic; for without the judgement that an established and successful theory has defects that justify a search for an alternative, more successful theory the majority will only be presented with the opportunity to make <u>inter</u>theoretic comparisons by chance. This would introduce a profoundly non-rational element to the progress of science.

(163) The decision that existing theory has deficiencies that warrant development of alternatives is not only within the context of appraisal - as the motivation for innovation - it is

also within the context of invention. And, as a rational decision, it is clearly within the bounds of philosophy. In chapters VIII and IX I will explore the New Zealanders' and Indians' reasons for deciding that the Watson-Crick model of DNA exhibited difficulties that justified looking for an alternative. But, as indicated earlier (142), the next chapter will examine the claim that the context of invention is beyond the ambit of the philosophy of science. For, with the establishment of the philosophical legitimacy of the motivation for innovation, we are only at the edge of the context of invention.

VII THE LEGITIMACY OF INVENTION

(164) Having shown that intratheoretic assessment is legitimate and lies within the context of invention, as well as that of appraisal, I turn now to the second part of the argument scheme outlined at (142). I claim that, contrary to the most common philosophical position [q.v., (135), (136) and (141)], the invention of new scientific theories in the circumstances where a single theory dominates a given domain is and ought to be of epistemic and, hence, philosophical interest. To a large extent, the analysis of the invention of the SBS models in New Zealand and India, beginning with the next chapter, will substantiate this claim. The present chapter deals with the issue in more general terms, criticizing the grounds used to support the received philosophical stance - for these have typically been abstract, even a priori in character. In this chapter I will not attempt to demonstrate that there is a logic of invention. Rather, I hope to show that, among the arguments which have been used or implied against it, none preclude the possibility of close empirical study revealing a logic of invention.

(165) A word or two must be said concerning what is meant here by the expression 'logic of invention'. I do not take it to mean an algorithm whose application deductively yields sound ('true') novel theory. Indeed I will later argue that a logic of invention cannot be an algorithm because it must be ampliative. I shall, instead, take it that there is or there may be a

'logic' to invention just insofar as there is or can be a part for reason to play in the process of devising a new theory. That part may be great or small; but, just insofar as there is a place for rationality in theorizing, scientific creativity has a logic.

(166) I have argued in the preceding chapter that reason is involved in the context of invention. There I suggested that the motivation for devising novel theory lay in specific dissatisfactions with extant theory which lead individuals (rather than the relevant scientific community as a whole) to contemplate the possibility that the theory may be fundamentally incorrect and so in need of replacement. Where no such alternative theory already exists, one must be invented. A scientist's reasons for wanting to devise a new theory are within the context both of appraisal and invention, since they are simultaneously grounds for rejection of established theory (appraisal) and the rational motivation for trying to invent a new and superior theory.[46] Of course, the fact that a scientist has reasons for attempting to devise a new and better theory does not mean that she or he will succeed; nor does it mean that the supposition that extant theory cannot deal with perceived difficulties is true. The capacity of individuals to theorize varies, as does their ability to devise cunning experiments to

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46 In his (1982a), McLaughlin discusses the "dual status" of some scientific reasoning. See especially pp. 74 - 75, and 82 - 83.

assess theories. But this does not reflect on the rationality of so doing. Nor, of course, does error prove irrationality. We are often lead into error by reason; but poor reasoning is nevertheless reasoning.

(167) Why, then, do most philosophers of science reject absolutely the legitimacy of the context of invention? In a way this is not an easy question to answer - since the view is much more often honoured than it is argued. Some have assumed that any element of the irrational or non-rational necessarily present in the context of invention means there can be no logic of invention. This appears to be Popper's opinion. He argues that "there is no such thing as a logical method of having new ideas, or a rational reconstruction of this process"; evidently because "every discovery contains 'an irrational element', or 'a creative intutition' [(1972), p.32]". However, all this shows is that invention cannot be <u>completely</u> rational; it amounts to the view - already accepted - that any logic of invention cannot be an algorithm. But even deductive appraisal cannot be algorithmic since, ultimately, the premisses must derive from some non-deductive source or else the argument will be circular. And this hardly shows that there is no such thing as a logical method of <u>testing</u> new ideas!

(168) Perhaps it comes down to this: there is, in the view of most philosophers of science <u>nothing</u> about the way that the scientist actually devises an hypothesis that has any bearing on whether or not the outcome is true or false, probable or improbable. How a scientist proceeds from the decision that a new

hypothesis is called for to actually invent it is beyond the pale of philosophy because the creative process has no bearing upon the epistemological merit of its outcome. Thus Reichenbach pointed to the "well known difference between the thinker's way of finding his theorem and [the validity of] his way of presenting it before a public [(1961), p.6]." Similarly, Laudan points to the failure of those who seek a logic of invention to convincingly demonstrate that their proposals are relevant "to the unquestionably important philosophical problem of providing an epistemic warrant for accepting scientific theories [(1980) p.182]". This, in Laudan's view, is why most philosophers have abandoned the logic of invention.

(169) Whether or not this is the way to express the anti-inventionist (to use McLaughlin's term) intuition, if there is no connection whatsoever between a scientist's manner of inventing an hypothesis and its veracity, then, clearly, there cannot be a logic of invention. But, just to the extent that there <u>is</u> a connection of any sort between how a scientist devises a theory and its epistemological status, then there may be a logic of invention.

(170) There is a noteworthy asymmetry between the epistemic status of an hypothesis and its invention.[47] The moot question here is whether the means by which an hypothesis is devised

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47 I am indebted to F. John Clendinnen for bringing this to my attention.

bear on its epistemic status; but its epistemic status does not depend logically on its origins. An hypothesis is true or false regardless of whether it was rationally or non-rationally invented. Moreover the epistemic status of an hypothesis may be assessed independently of its origins. If there is a logic of invention, and if it is applied, then it will enhance the epistemological prospects of the outcome (since it will not be an algorithm, it cannot guarantee the outcome.) And, given an hypothesis - however it may have been arrived at (reason, chance or guess) - its truth or falsity be appraised regardless of the method used to invent it. Thus a logic of appraisal unrelated to invention (e.g., the hypothetico-deductive method) is possible. But, the existence of such an independent logic of appraisal does not preclude a logic of invention. On the other hand, as is pointed out by McLaughlin [(1982a) and (1982b)], a logic of invention can serve both to arrive at and (insofar as it enhances the probable epistemic status of its outcome) to validate an hypothesis. Laudan (1980) makes much of the discovery by philosophers of the possibility of an independent logic of appraisal; but such logic of appraisal in no way undermines the possibility of a logic of invention - except in the psychological sense that it permits the latter possibility to be ignored.

(171) Popper asserts that the weakest inventionist position - the view that there may be a logic of invention - commits a category mistake, identifying as logical (philosophical) what is actually empirical (psychological). This alleged category mistake Popper calls "psychologism". In his view, epistemology

"is concerned not with questions of fact..., but only with questions of justification or validity...", concluding from this that the "question of how it happens that a new idea occurs to a man... may be of great interest to empirical psychology; but is irrelevant to the logical analysis of scientific knowledge [(1972), p.31, emphasis in the original]." But, as McLaughlin says,

> the logical/empirical distinction can be drawn with equal facility in both the context of invention and the context of appraisal...[and this] suffices to rebut the charge of psychologism - i.e., of conflating empirical and logical matters... The point is that in either context one can raise: (a) empirical (e.g., psychological) questions about the causes of a scient- ist's actions, including his/her inventings-of, as well as acceptings-of, rejectings-of, or perseverings-with, certain hypotheses; and (b) logical questions about the inferential relations between a hypothesis and guiding considerations, plausibility considerations and data statements [(1982a), p.75, emphasis in the original].

(172) Popper believed that there are no answers to logical questions about the reasons, as distinct from non-rational motives, for scientists' 'inventings-of' hypotheses. He held that there are only psychological answers to empirical questions about the causes of their inventings, there being no reasons-as-causes of invention. He holds to the absence of reasons-as-causes a priori, not a posteriori. So any search for them is based on a category mistake ('psychologism'). But Popper is in error here. Given that there are some reasons which are causes, say of accepting or rejecting a theory, then it is an empirical question, and not a logical one, whether there are any reasons which are causes of scientific invention. Popper is reluctantly prepared to admit that scientists sometimes accept or reject

theories as the result of psychological and sociological influences. However, in his view, such decisions (which are within the empirical, though not the logical domain of the context of appraisal) are as much beyond the pale of the philosophy of science as is the context of invention [see Popper (1970)]. The extent of such non-rational causal influences upon scientific assessments is, like the extent of logic in the context of invention, an empirical matter; but one essential to establishing the domain of the philosophy of science. In respect of appraisal, Popper concedes this - remarking, for example, that he and Kuhn "disagree...about some historical facts, and [consequently] about what is characteristic for science [(1970), p.54]." Specifically, they dispute the extent of psychological and sociological influence in the context of appraisal.

(173) Thus Popper charges Kuhn with "sociologistic and psychologistic tendencies and ways [(1970), p.58]." Notice, though, that the ground upon which Popper levels this allegation differs from that which he used as the basis for the same claim against those who consider that there may be a logic of invention. In the latter case it was founded upon the supposed category mistake of conflating logical with empirical questions [q.v., (171)]. But, against Kuhn, the argument turns on an empirical dispute about the nature of science. In reality, however, the possibility of both a sociology of scientific appraisal and a logic of invention must be settled on empirical and not a priori grounds - are scientists influenced in their appraisory judgements by sociological or psychological factors, and do scientists employ logic (reason) in devising hypotheses?

(174) Of course, if scientists' judgement _is_ affected by sociological and or psychological factors, that does not show that it _ought_ to be. To so argue would be to commit the naturalistic fallacy of trying to derive 'ought' from 'is'. But, demonstration of the empirical claim _enables_ a normative evaluation. Similarly, to show that there _is_ a part played by reason in the context of invention by examination of actual inventions of scientific ideas does not establish that such a part _ought_ to be played. It simply shows that there _is_ a logic of invention, and opens for debate the question of whether such a logic is a good thing. However, no philosopher of science - certainly not Popper - doubts the efficacy of reason, especially where epistemological issues are at stake.[48]

(175) There is another anti-inventionist _a priori_ argument against the possibility of a logic of invention which must be countered before the way is cleared for empirical examination of the question. Deductive arguments cannot proceed in their conclusions beyond what is contained in their premisses. Thus, however long the chain of deductive argument, it can never be creative. In contrast, inductive reasoning, since it extends its conclusions beyond what is entailed by the evidence, is creative

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48 Indeed Kant maintained that one cannot argue against the efficacy of reason since, in seeking to persuade by reason, such an argument presumes what it seeks to controvert in its conclusion (and it is not a _reductio_). He completed the transcendental argument by claiming that one cannot argue in favour of the efficacy of reason since such a conclusion must be among the premisses, begging the question.

in essence. Thus it would seem that only ampliative inference in the form of some kind of induction can be involved in any logic of invention.[49] And it is for this reason that a logic of invention cannot be an algorithm which unerringly arrives at truth [q.v., (165) and (170)]. But, if Hume is correct and induction is unjustifiable, then induction cannot provide an epistemic warrant in either the context of invention or that of appraisal. Thus, the possibility of a logic of invention seems to require as a necessary condition the justification of induction.

(176) However, it is by no means true that those who have looked with favour upon the context of invention have been inductivists, whereas those who have opposed the possibility have been deductivists who reject induction as an acceptable form of reasoning. Reichenbach, to whom the recieved, anti-inventionist view is generally sourced, was an inductivist. More recently Simon (1977) has argued for a non-inductive logic of discovery. He follows Hanson, whom McLaughlin calls "the most famous recent exponent of inventionism [(1982b), p.206]". Hanson held that a non-inductive, non-deductive form of reasoning was involved, which he termed 'retroduction' [cf., Hanson (1961) and (1967)]. Yet induction, and its warrant as a form of inference _is_ central to the debate over the legitimacy of a logic of invention.

(177) Laudan (1980) provides an extended defense of the anti-

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49 I assume that the only acceptable ampliative inference is some form of induction - but not only induction by enumeration [see below (183)ff.]

inventionist position. According to Laudan, at those times in the history of philosophy when science has been conceived as generating "statements concerning observable regularities [p.179]" by simple enumerative induction, the programme for a logic of invention had a rationale, a philosophical legitimacy. But, when hypothetico-deductive confirmationist or falsificationist philosophers conceived of theories "chiefly as grandiose ontological frameworks, replete with unobserved entities, inductive logics of discovery have been ignored or, in some cases, their very existence denied [ibid.]".

(178) Even then, Laudan notes, there were those who sought non-inductive, 'self-corrective logics of discovery' (for example, Hanson). These

> involve the application of an algorithm to a complex conjunction which consists of a predecessor theory and relevant observation (usually one that refutes the prior theory). The algorithm is designed to produce a new theory which is 'truer' than the old [idem.]

However, according to Laudan,

> a century and a half of exploration by a sucession of major thinkers failed to bring the self-corrective program to fruition.... No one was able to suggest plausible rules for modifying earlier theories in the face of new evidence so as to produce clearly superior replacements [p.180]

(179) For Laudan, the philosophical point behind all logics of invention, as well as all logics of appraisal, is to establish the epistemic warrant for scientific theories. Thus, given a conception of science as generating regularities by enumerative induction from observation, he accepts that the evidence and rule of inference which generated such regular- ities also justifies them. But, with most contemporary philosophers of science,

Laudan accepts that scientists are mainly concerned with "deep-structural theories" - which he holds induction cannot generate. Thus Laudan believes that hypothetico-deductive appraisal for confirmation or disconfirmation of theories has triumphed over inventionism. Believing that it lacks a philosophical rationale in terms of epistemic warrant, Laudan asks: "Why should the logic of discovery be revived?"

(180) With McLaughlin [(1982a) and (1982b)], I will accept two of Laudan's main points concerning the nature of a rationale for a logic of invention: that it must rest on a conception of theorising more sophisticated than can be accomodated by simple induction by enumeration; and that its philosophical legitimation must rely on providing a plausible account of how the way in which a theory is devised bears on its epistemic value. Moreover, McLaughlin has shown that attempts by Hanson and Simon to develop a non-inductive, non-deductive 'retro- ductive' or 'abductive' self-corrective logics of invention are either covertly inductive or unhelpful.[See McLaughlin (1982a), pp. 82-84; and (1982b), pp. 205 - 209].

(181) That most, if not all interesting science is more than mere generalization from observation is hardly controversial. In particular, here we are concerned with trying to explicate the genesis of novel theory from perceived defects within established theory. Thus the interaction between theory and observation is crucial. A logic of invention, to be at all helpful, must be such that its "application...to a complex conjunction which consists of a predecessor theory and a relevant observation

(usually one that [apparently] refutes the prior theory)....is designed to produce a new theory which is 'truer' [better] than the old [Laudan (1980), p.179]." Laudan, however, conceives of such a process as algorithmic [idem]. Historically, it was thought of that way. But, as I have said, an inductive view of a 'self-corrective logic of discovery', in principle, does not constitute an algorithm [q.v., (175)].

(182) Hanson's proposed "retroductive [i.e., non-deductive, non-inductive] schema" for the invention of an hypothesis, H, is this:

> (1) Some surprising, astonishing phenomena p_1, p_2, p_3 ... are encountered.
> (2) But p_1, p_2, p_3 ... would not be surprising were a hypothesis of H's type to obtain. They would follow as a matter of course from something like H and would be explained by it.
> (3) Therefore there is good reason for elaborating a hypothesis of the type H: for proposing it as a possible hypothesis from whose assumption p_1, p_2, p_3 ... might be explained.
>
> [Hanson (1961), p.33; quoted in McLaughlin (1982a), p.83.]

This schema might help us understand a scientist's initial appraisal of a hypothesis H after it had been invented. But, as McLaughlin notes, it "sheds no light whatever on what considerations suggested or pointed to (advanced) H or H-type hypotheses [(1982a), p. 84]". H is a given within the premisses of the "retroductive schema". Any adequate schema of invention must show how H is suggested in the first place, not merely why it is helpful once we have it. The point is not merely to provide a post hoc subjunctive rationale for the plausibility of the invented hypothesis, but to invent it. Hanson tells us nothing about how to do this.

(183) As we have seen, in his account of the programme for a logic of invention based on enumerative induction, Laudan accepts that the evidence and rule of inference play the joint role of producing and warranting descriptive generalizations. Nor does he deny the legitimacy of this form of inference. Rather, Laudan's argument rests on the (undoubted) limits of enumerative induction. [See esp. Laudan (1977), pp. 178ff.] McLaughlin holds that a logic of invention for 'deep-structure' theories can play a similar role if the conception of the the inductive inference utilized is more sophisticated, employing principles of induction by analogy and inductively warranted rules of choice based on criteria such as simplicity, parsimony etc. [see McLaughlin (1982b), p. 203].

(184) Laudan observes

> If there is general scepticism today about the viability of a logic of discovery, it is in part because most of us cannot conceive that there might be rules that would lead us from laboratory data to theories as complex as...the structure of DNA [(1980), p.178].

It is therefore apposite to quote at some length McLaughlin's reconstruction of how a scientist might be led from laboratory data to a structure for DNA. These reconstructions of "advancement arguments" - based on Watson (1968) - also illustrate the ways in which analogy and simplicity are important in the invention of new hypothesis. Each is an inductive inference (indicated by the double line between premisses and conclusion). The premisses consist of "guiding considerations", Q_{1-n}.

(185)

Structure of DNA Molecule - Analogical Advancement Argument

(Q_1) The structure of the DNA molecule is unknown, but one of its major chemical constitutents is a form of nucleic acid.
(Background information)

(Q_2) In chemical composition, DNA is analogous to TMV (tobacco mosaic virus), which also has a form of nucleic acid as a major chemical constitutent.
(Analogy claim)

(Q_3) The TMV molecule is helical in structure.
(Datum)

(H) The DNA molecule is helical in structure.

[McLaughlin (1982a), p.88, emphasis in the original.]

(186)

Structure of DNA Molecule - Simplicity-based Advancement Argument

(Q_1) The structure of the DNA molecule is unknown, but it is probably crystalline.
(Early X-ray diffraction data from Wilkins)

(Q_2) Crystals have regular structures.
(Background information)

(Q_3) "The simplest form for any regular polymeric molecule...[is]...a helix."
(Simplicity claim - Watson (1968), p. 131)

(H_1) The structure of the DNA molecule is a helix.

(Q_4) The thickness of the DNA molecule indicates that it is composed of more than one chain, i.e., that it is a compound helix.
(X-ray data)

(Q_5) The simplest compund helix is a double helix.
(Simplicity claim)

(H_2) The structure of the DNA molecule is a double helix.

[McLaughlin, loc. cit., pp.88-89][50]

(187) For McLaughlin, "the 'logic of invention' is mainly induction [(1982a), p.83]." And that is not a view that I want to contest - for the reason that I have emphasized, namely that only ampliative (inductive) logic is creative. Nevertheless, deductive reasoning still has a part to play in the creative process. For example, though the choice of which anomalies to regard as being of a kind that might indicate a new theory is needed may be based on past experience with similar (analogous) anomalies in similar (analogous) theories (i.e., be inductive), the 'cognitive threat' - as Laudan calls it - which all candidate anomalies (apparent failed predictions) pose is modelled on modus tollens. This classic, deductive argument has the the form 'if p (here a theory) implies q (here a prediction), and if not-q (here, a failed prediction), then not-p (the theory or, rather, some indeterminately large part of the theory, is false).

(188) The classic conception of enumerative induction, such as that developed by John Stuart Mill, is of a machine-like generator of hypotheses. Laudan takes this to mean that they are

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50 These reconstructions do not, and are not intended to provide a full account of the actual process of reasoning employed by Watson and Crick. Rather, they are some among many inductive arguments leading toward the double-helical structure which may be discerned in Watson's (1968) account. For example, another prominent analogy was that drawn between Pauling's alpha-helix model of the protein keratin and the methodology which led him to it.

algorithms [cf., his (1980), p.178]. Whether or not this is so, the conception of inductive inference being employed here is by no means machine-like. It does not generate hypotheses so much as suggest them via high-level analogies and principles of simplicity and parsimony. In these inductions, frequency is not necessarily the only, or even the most important consideration. Thus, for example, when Watson and Crick chose to model their methodology on Pauling's (see fn50), they had but one exemplar, and one helical model for a biological macromolecule. Similarly, it is not the sheer number of anomalies that confront a theory which is important, it is their significance for an individual scientist's concerns. Moreover, when a theory is established and successful, with a long record of resolving anomalies, it is perhaps to be expected that - to use Lakatos's metaphor - the "ocean of anomalies" in which it was launched and progressed will have shrunk to a lake, or even a puddle.[51]

(189) However, having accepted that invention must neccessarily be inductive in this broader sense, the programme for a logic of invention seems vulnerable to the argument outlined at (175); namely, if Hume has shown that induction is unjustifiable then, eo ipso, no logic of invention can be justified. It must, I think, be admitted that this is a powerful objection to inventionism; but it does not require a frontal assault upon Hume in order to turn it aside. Hume admitted the ubiquity and

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51 Thus, for example, Kelvin could detect but "three clouds" on the horizon of classical, Newtonian mechanics shortly before Einstein developed Special Relativity.

inordinate practical difficulty of dispensing with induction - which he attributed to habit. If induction is constitutive of the practice of science - invention <u>or</u> appraisal, then that is a brute fact with which philosophers must perforce deal. Perhaps the onus does not rest alone on those who use that form of reasoning to show that it is justifiable or rational. The inability of philosophers to provide a warrant for induction should not be confused with the lack of a warrant.

(190) As Laudan observes that there is "a <u>heuristic</u> problem about science: how can we maximize the rate at which new and promising theories and laws are generated [(1980), p. 182, emphasis in the original]?" But this is not a <u>philosophical</u> problem in his view: "the case has yet to be made that the rules governing the techniques whereby theories are invented (if such rules there be) are the sorts of things that philosophers should claim any interest in or competence at [<u>ibid</u>.,]." However, if it transpires from analysis of empirical studies that there is <u>anything</u> about the invention of theories which makes them more or less "promising" (i.e., probable or useful), then Laudan's heuristic problem is a philosophical problem - even if the factors turn out to be psychological or sociological (as Kuhn holds to be the case). His heuristic problem becomes an epistemic problem to the extent that the <u>ratiocination</u> associated with the generation of new theories turns out to be relevant to their promise. If that reasoning turns out to be inductive - as I maintain it must - then, rather than eliminating the heuristic problem as a philosophical problem, it is doublely a legitimate, if knotty philosophical problem.

VIII UNWINDING AS AN ANOMALY

(191) I have argued that the starting point for any logic of invention is an explication of the rational grounds upon which the decision to try to devise a new theory may be made. And, in the circumstances where a single well-established and successful theory occupies a given domain, I have suggested that those grounds lie in deficiencies that individual scientists perceive in the received view of the domain. The paradigm example of such a defect is the empirical anomaly. Classically, an empirical anomaly arises for a theory T, when one of its empirical consequences (predictions of future, or explanations of past states of affairs), p, is disconfirmed. T implies p, not-p is observed, and so, by *modus tollens*, T is false.

(192) It is well recognized by philosophers of science that the situation is much more complex than this simple *pro forma* indicates. At every point there may be - and there often is - debate among scientists. Is the problematic prediction or explanation really an empirical consequence of the theory? Does observation or experiment really disconfirm it? In practice, theories are rarely axiomatized to the point where they yield unequivocal theorems. Moreover, they only render empirical predictions and explanations when under specified 'initial' and 'continuing' empirical conditions. There is, in this, a great deal of room for disagreement over both theoretical interpretation and application. In addition, the results of testing themselves require interpretation before they can be taken to

be disconfirmatory. For example, the failure to find an expected result can be attributed to poor experimental design and/or execution.

(193) Even when there is agreement over the interpretation and application of a theory, and over the reliability and significance of the disconfirmatory evidence, modus tollens only reveals that in some greater or lessor degree the theory is deficient. It does not indicate whether the fault lies in the theory itself or in the the empirical premisses needed to yield predictions or explanations (and these are always required). This opens the way for a fresh debate over the issues mentioned in the paragraph above. Moreover, even if the fault is agreed to lie in the theory, modus tollens does not indicate in what respects the theory is in error. This can motivate fresh efforts to pinpoint the problem with experiments designed to test separate theoretical claims.

(194) To these complications must be added technical difficulties - for instance, the limitations of instrumental accuracy - and the possibility of debate over their nature and significance in a given case. Nevertheless, both individual scientists and, more rarely, the scientific community do sometimes conclude that it is probable or, more commonly, that it is possible that a genuine empirical anomaly exists - that a theory does have empirical consequences which should be and are not confirmed by experiment or observation. However, partly because this judgement is normally neither unequivocal nor unamimous, it is usually tentative. Consequently, it is more appropriate to speak

of apparent empirical anomalies than of falsifications.

(195) Clive Rowe encountered the unwinding problem as an apparent empirical anomaly, an inconsistency between experimental results and the Watson-Crick model of the conformation of DNA [q.v., (1)-(12), and esp. (6)]. Electron micrographs of intact, but partly strand separated circular DNA molecules indicated that strand separation had simultaneously begun at and proceeded in both directions from multiple points, forming one or more loops.[52] Rowe's model of a circular molecule with two helical strands (resembling a quoit) seemed irreconcilable with the θ-structures. Leaving aside the problem of obtaining a single strand loop in the first place (try separating the strands of an intact quoit), even with such a loop built in place unwinding at one end produced winding up at the other [see Figure 10].

(196) Watson discusses this anomaly in the second edition of his widely used text [(1970),q.v., (8) and (74)].[53] Referring

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52 Where one single strand loop crosses the horizontal diameter of a molecule, the micrograph resembles the Greek letter theta ('θ'); so they are called 'θ-shaped structures' even where more than one single strand loop is observed.

53 As co-author of the double-helical model, Watson might be suspected of bias. But, in this field, virtually everyone with a claim to expertise would be subject to such a prima facie suspicion - for each has a reputation built on refinement, testing and application of the Watson-Crick model. Specialists are the opinionmakers in the wider scientific community. Because it is their model, and because of their very considerable subsequent contributions, Watson and Crick's views have special weight. Of course the critics have their own axes to grind; but all of this is only to be expected where a single theory like the double-helical structure for DNA dominates a given field.

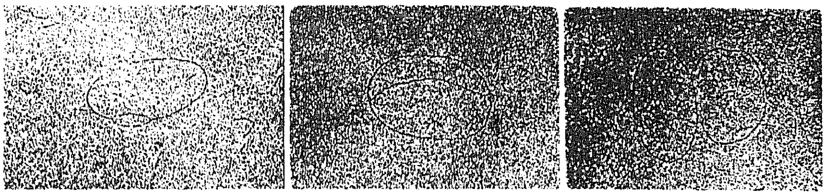
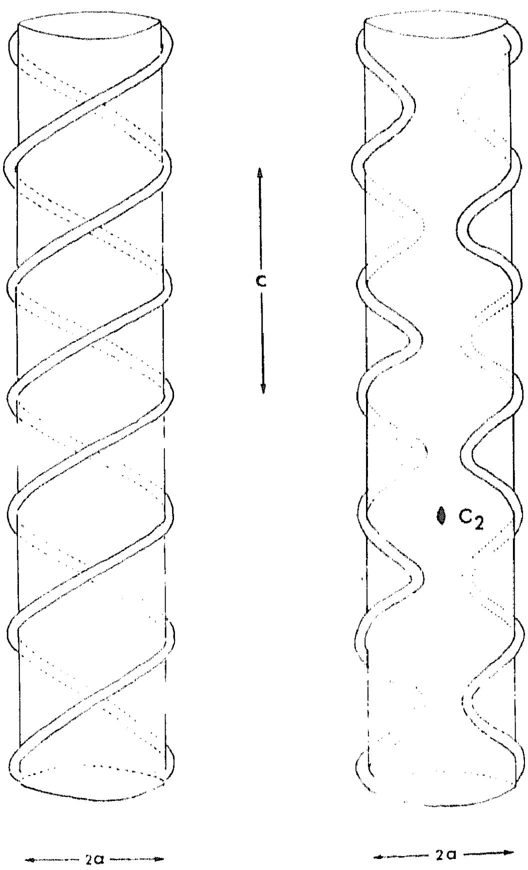


Figure 10: Electron micrographs of three replicating co-valently closed circular DNA molecules, exhibiting θ-structure single-strand regions.



Left: Figure 11(a), a schematic model of the double helix.

Right: Figure 11(b), a schematic model of the 'warped zipper'.

to Cairns' (1963) suggestion for a solution, Watson asserts that this "particularly puzzling...dilemma....demands the presence of a molecular swivel(s) about which the non- replicated [double strand] material can rotate [p.284]." But he acknowledges that problems remain: "Unfortunately this idea is very difficult to translate into a precise molecular form. Particularly difficult to comprehend is the process occurring when replication passes over the supposed swivel region [idem]." Accordingly, Watson believed that the "possibility must also be considered that the θ-shaped structures do not exist within cells but only form during isolation of the replicating material [idem]."

(197) In the terminology of the methodological debate, any hypothesis which eliminates or minimizes an anomaly without change to the core of the theory under threat is called an 'auxiliary hypothesis'. There are two varieties: those which are acceptable, and those which are are termed '*ad hoc*' or 'objectionably *ad hoc*'.[54] Thus, according to Popper,

> ...only those [auxiliary hypotheses] are acceptable whose introduction does not diminish the degree of falsifiability or testability of the system in question, but, on the contrary, increases it. [(1972), p.83]

(198) Popper, and those of his view, generally refer to this as the requirement of 'independent testability' of auxiliary hypotheses. If they are not to be *ad hoc*, or objectionably *ad*

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54 '*Ad hoc*' means 'for this' specific or short-term purpose. A pejorative connotation is added when important general or long-term considerations are thought to have been ignored. In respect of scientific hypotheses, a pejorative sense is usually intended - though some emphasize it with the word 'objectionably'.

hoc, such hypotheses must have testable consequences other than those they are designed to account for (the anomalous data). Some only require testability in principle, others confirmation of the independent empirical consequences. Because ad hoc, or objectionably ad hoc hypotheses are held to be motivated by a desire to preserve theory illegitimately by insulating it from its domain, philosophers have entered the lists to defend or attack important putative historical examples.[55]

(199) By contrast, Laudan has argued that whenever "a theory encounters a refuting instance, it is possible to modify the interpretive rules associated with the theory so as to disarm the 'refuting' data [(1977), p.118]." Such a manoeuvre, for Popper, is an unacceptable "conventionalist strategem" because it insulates the theory from falsification [see his (1972), pp.78ff]. But Laudan maintains that "the modification of a theory arbitarily in order to eliminate a refuting instance is open to criticism only if such a move would lead to a diminished problem-solving efficiency [(1977), p.119]." Moreover, he holds, "that can generally be shown to happen only if the refuting instance is solved by some [other] theory in the [same] domain [ibid.]." Then, Laudan reasons, the theory that has legislated an apparent refuting instance out its scope arbitrarily can be

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55 For example, Popper named the Lorenz-Fitzgerald contraction hypothesis (an attempted solution to the indetectability of the aether wind) as objectionably ad hoc [(1972), p.83.]. This sparked a wrangle with several participants in The British Journal for the Philosophy of Science; vol. 10., 1959-1960, pp.48 - 50 and pp.228-229; vol. 11, 1960-1961, pp.143-145 and 153-157; and vol. 27, 1976, pp.329-362. See also Philosophy of Science, vol. 47, 1980, pp.1-37.

seen to have solved one fewer problems than that which embraces and accounts for the problematic data. So, he concludes, "a refuting instance only counts as a serious anomaly when it has been solved by some theory or other [ibid.]."

(200) Similarly, Lakatos (1970) argued that theories typically progress in and despite an "ocean" of unresolved anomalies. From time to time some of these would be resolved - thereby converting them into "corroborating instances", and warranting patience with the remainder. Furthermore, Lakatos maintains that if a modus tollens model of falsification is accepted, the "hard core" of the theory will not be rejected; rather, a "protective belt" of auxiliary hypotheses will be modified. And then all "we need in addition to this is that at least every now and then the [resulting] increase in [empirical] content should be seen to be retrospectively corroborated [(1970), p. 134]".

(201) Watson's attitude toward the θ-structures anomaly [q.v., (189)] exemplifies a number of the responses which post-Popperian philosophers have thought typical and appropriate Above all, the θ-structures are not taken to falsify the Watson-Crick model of circular DNA molecules. In Lakatos's terminology, the double helix is an element in the 'hard core' of molecular genetics immune from modus tollens. Instead, an auxiliary hypothesis is proposed to protect it - for example Cairns' molecular swivels. Notice that, for Watson, the anomaly "demands" this solution. The molecular swivels are vaguely specified, and Watson finds it hard to see how they can be understood in structural terms. Moreover, Cairns' idea evidently

had no empirical consequences other than those it sought to explain - the anomalous data itself. This is the hall-mark of the _ad hoc_ or objectionably _ad hoc_ hypothesis.[56] There being no alternative theory which can account for the θ-structures anomaly, Watson conforms to Laudan's methodological expectations in concluding that the "occurrence of [the]...anomaly raises doubts about, but need _not_ compell the abandonment of, the theory...[Laudan (1977), p.27]".

(202) Admitedly, because of its difficulties, Watson is prepared to entertain the possibility that the molecular swivel solution is wrong. But, as Lakatos would expect, this does not - as he would put it - re-direct _modus tollens_ to the double helix itself. Rather, Watson suggests that the formation of the θ-structures may be an artefact of the _in vitro_ preparation. The only reason given for this suggestion were the problems with the swivel hypothesis. Moreover, even if the θ-structures were an artefact, this would not solve the difficulty - since the occurence of the structures still remains to be explained. But, because they are viewed as artefacts of experimental technique and not natural phenomena, the importance of the anomaly is diminished. When Watson's (1970) comments were written, the θ-

56 Is is not that such an hypothesis _cannot_ be the correct solution. Rather, because independently untestable solutions are readily available to those who would save the theory by insulating it from its empirical domain, they are held to be short-sighted tactics which defeat long-term scientific strategy. For this reason, an hypothesis whose independent consequences cannot be tested due to present and foreseeable instrumental limitations is nearly as undesirable as one for which no one can discover any testable consequences in principle [_q.v._, (197) and (198)].

structures anomaly was far from novel. Indeed Cairns' proposed solution, with all its attendant difficulties, was then seven years old. Yet there is only puzzlement in Watson's tone; no suggestion that the passage of time made the problem more urgent or serious.

(203) In the third edition of his text (1976) - by then appearing in ten languages beside English - Watson expanded and changed his treatment of the θ-structures anomaly. He noted that experimental evidence now indicated the formation of superhelices in the unseparated regions of circular DNA molecules. Watson thought that these would absorb the windings of the single-stranded areas. He now rejected Cairns' molecular swivel hypothesis because of the difficulty in explaining how replication could pass through a swivel region.

(204) This problem he had mentioned in the second edition (1970), but not counted decisive. The difference between the two attitudes may be traced to the advent of an alternative auxiliary hypothesis, a model utilizing superhelices and cutting and splicing enzymes. These substances, as well as the superhelices had, Watson noted, been isolated in circular DNA (though their functions were still speculative). Nonetheless, Watson conceded that

> Still...to be elucidated are the events which permit separation of two parental strands at the end of replication. One of the two strands has to break, but how this occurs without generating an incomplete linear daughter helix remains an intriguing dilemma [(1976), p.237].

However, the enzymatic model remains the currently favoured solution to the θ-structures anomaly. For example, Denhardt (1979) [q.v., (82)] speaks of Cairns' molecular swivel hypothesis as having been "recast" in enzymatic terms (p.196). But the difficulty that Watson pointed to [q.v., (197)], remains.

(205) From the point of view of proponents of the Watson-Crick model of DNA, the discovery of cutting and splicing enzymes and the development of the enzymatical explanation of replication of circular molecules would, no doubt, be seen as a vindication of their relatively complacent attitude toward the θ-structures anomaly. As Lakatos would expect, they had every confidence in their theory's capacity to survive essentially untouched when some fortuitous experimental findings combined with ingenuity to improve on some rather shaky auxiliary hypotheses. And, as Laudan would expect, appraisory decisions were made between competing auxiliary hypotheses. Cairns' molecular swivel was "demanded" by the anomalous data just until another solution which avoided its difficulties was available.

(206) Moreover, one suggested auxiliary hypothesis - that of θ-structures as an *in vivo* artefact - simply disappears when the need for *any* solution, however dubious, is removed by a respectable resolution with some confirmed empirical consequences. [Watson does not even discuss the artifact hypothesis in his (1976).] This suggests that temporizing in the face of an anomaly by *ad hoc* manoeuvering is a strategy with merit. For both Lakatos and Kuhn it is the past success of a theory at overcoming anomalies eventually that justifies filibustering - or even ignoring an anomaly.

(207) Popper, of course, could not and does not accept such an inductive warrant. Watson's response to the θ-structures anomaly also fails Popper's tests of acceptable auxiliary hypothesis on a number of grounds. The molecular swivel hypothesis is entertained despite a vague specification which generates no testable independent empirical consequences. The _in vivo_ artefact alternative does not even solve the anomaly, but merely shifts it into a less critical location. Yet Watson's response is the response of the specialist scientific community. He writes in an authoritative, popular textbook and, as such, is reporting the activities and reflecting the evaluations of the community. In science, the place for controversial evaluations is the critical review. The textbook presents the consensus.

(208) The response of Rowe and his colleagues in New Zealand to the θ-structures anomaly was, in contrast, classically Popperian. Rowe identified the interlaced character of the Watson-Crick double helix as the central problem. And he noted that, topologically, it was because _both_ helices were supposed to be right-handed that unwinding was essential. Accordingly, he considered the possibility of _left_-handed helices - at that time, rejecting it because there was no experimental evidence of them [_q.v._, (9)]. In doing this, Rowe was entertaining the possibility that Watson and Crick might have been fundamentally wrong; i.e., that their double-helical structure for DNA was false. Since Rodley could not dispell his doubts, and as he came across more difficulties for the double helix, Rowe became convinced of the need for an alternative model of DNA which did not

require unwinding, a conclusion that Rodley also came to share [*q.v.*, (17)].

(209) Two aspects of the New Zealander's attitudes need to be stressed: Firstly, unlike the specialist scientific community as a whole, Rowe and Rodley were prepared to explore seriously the consequences of accepting that the θ-structures anomaly might be a falsification of Watson and Crick's model for DNA; namely that an alternative model might better describe the evidence, and that thought about what such a model might be was justified. Secondly, they arrived at this conclusion on the basis of much the same evidence as had led the community to an opposite conclusion (both men read widely in the literature [*q.v.*,(11), (12) and (17)]).

(210) In considering the different responses to the θ-structures anomaly of the New Zealanders and the scientific community it is important to note that *both* accepted that the anomaly was a threat to the double-helical model. Where they differed was in their subsequent responses. The specialist community assumed that the anomaly was *unlikely* to be a falsification, and sought to establish that it was not. Rowe and Rodley, starting from the same assumption, reached the conclusion that these efforts were unsatisfactory and were *unlikely* to improve. They then *entertained* the possibility that a solution lay in an opposite assumption; *viz.*,that the resolution might lie in an alternative model.

(211) Note the provisional and inductive nature of this

characterization. The community as a whole did not conclude that the θ-structures anomaly had been solved within a double-helical model; only that it was likely to be, and likely to be a solution of a certain sort. The New Zealanders did not conclude that the anomaly could not be solved within a double-helical model; only that it is unlikely to be. And they could not say what the solution would be like; only what it would <u>not</u> be like - it would not require unwinding. This points to a judgment of probabilities in science, rather than a declaration of truth or falsehood.

(212) It should not be surprising, given that scientists are judging the probable outcomes of various courses of action, that they differ in their conclusions. But, where most of them reach the opposite conclusion to that of a couple of individuals, there seems to be a <u>prima facie</u> question of whether there is anything to be said for the judgment of the latter group. Moreover, the majority were specialists in the field whereas the dissenters were not - Rowe was a technician, and Rodley an inorganic chemist [<u>q</u>.<u>v</u>., (1) and (10)]. I have argued elsewhere that Rowe and Rodley's lack of specialist training is relevant to understanding why <u>they</u> acted as they did. As non-specialists, they were less affected by non-cognitive sociological pressures favouring the established model of DNA [<u>q</u>.<u>v</u>., Stokes (1982), pp.221-222 and 224].

(213) But non-cognitive factors did not determine the conclusions reached by Rowe and Rodley. Indeed, non-membership of the scientific community specializing in the structure of DNA

makes it considerably less likely that they would consider technical problems in the Watson-Crick model at all. Moreover, Sasisekharan, whose group's considerations were partly guided by the unwinding problem [q.v., (46)], was a specialist in the study of the conformation of DNA [q.v., (35)]. Rowe and Rodley's reasoning vis-a-vis that of the bulk of specialists must be appraised strictly in methodological terms. Such assessment is not majoritarian, and may decide for either (or neither) group.

(214) The two different judgments of the import of the θ-structures anomaly are, as we have seen, each supportable on a different methodological basis. Rowe and Rodley's interpretation may be based upon a Popperian view of the correct way of dealing with anomalies; the specialist community's approach is in keeping with aspects of Laudan and Lakatos's attitude. Numbers do count here, in one way. They may indicate what is normal in science. And we may well be loath to declare typical method unsound. But here we are not forced to that conclusion.

(215) In Chapter VI, we saw that what is methodologically apposite for the majority of scientists who are concerned with development and application of theories is not incumbent on each and every member of the scientific community. A dominant need for development and application of theory results in adoption of intertheoretic criteria of appraisal. On the other hand, once a theory has been selected as superior to all its competitors (thus ending intertheoretic appraisal), there is no (rational) motivation for questioning it because there are no longer any competitors. Accordingly, there is no (rational) motivation for

devising novel theory. But this dilemma was resolved by allowing that a scientific avant garde, focusing on the inadequacies and deficiencies of a theory (e.g., anomalies) rather than its merit and possibilities, might adopt such intratheoretical criteria of assessment to warrant development of new theory. And as a permanent minority (in the situation where extant theory continues to be comparatively successful), such an avant garde would not threaten science.

(216) This view of methodology enables us to recognize that both the philosophical views of anomalies we have discussed (i.e., as critical and relatively unimportant) have something of value to say. Similarly, we can endorse both the judgment of the majority of specialist scientific community regarding the θ-structures anomaly and that of Rowe and Rodley. The kind of stipulations which Popperians make against objectionably ad hoc auxiliary hypotheses are designed to prevent the isolation of a theory from falsification. One good reason for this is that such isolation must stifle the progress of science by providing no motivation for the development of new ideas:

> Hypotheses are nets: only he who casts will catch.
> [Popper (1972), p.11]

Conversely, Laudan notes that if

> the occurrence of even one anomaly should force the rational scientist to abandon it....we should find ourselves abandoning our entire theoretical repertoire in wholesale fashion, and thereby [be] totally unable to say anything whatever about most domains of nature [(1977), pp.26 and 27-28; q.v., (143)]

(217) However, science will be neither paralysed nor stifled if we allow that most scientists will not take a recalcitrant anomaly as grounds for challenging a theory, whereas some will. Rather science will be rational, functional and progressive if

the former group accept _ad hoc_ solutions to anomalies (provided no more satisfactory auxiliary hypothesis or alternative theory is available) whereas the latter find the same _ad hoc_ hypotheses objectionable because of their different focus of attention. So viewed, Watson's discussion of the θ-structures anomaly is mainstream science, whereas Rowe and Rodley's is _avant garde_. Both are interested in achieving the best possible account of the structure of DNA, but from different points of view. For Watson, and most molecular biochemists, a crucial measure of success is the _utility_ of the structure. This de-emphasizes the importance of problems such as the θ-structures anomaly. The θ-structures, for them, are _a_ problem in our understanding of DNA. But to count the anomaly as warranting serious doubt about prior achievements is, from their point of view, to throw the baby out with the bath water. For Rodely and Rowe, however, the θ-structures are _the_ problem. This different set of priorities lead them to different conclusions on the same evidence. Neither view, I submit, is incorrect - given the differing perspectives involved. On this view, measures of '_ad hoc_ness' serve the dual function of selecting among alternative auxiliary hypotheses (for the majority) and indicating that a more radical approach is desirable (to the _avant garde_).

(218) What must now be examined is the precise role that the θ-structures anomaly played in the genesis of the 'warped zipper' model of DNA. I have already noted that it had a role in the thought of both the Indians and the New Zealanders [_q.v._, (213)]. But its role differed in degree and kind. For the New Zealanders, concern over the formation of θ-structures has

already been shown to have provided the initial motivation for the development of their version of the SBS model by causing first Rowe then Rodley to doubt the adequacy of the attempts to account for them. Rowe located the heart of his problem straight away - the necessity for unwinding during replication given a double-helical interpretation of DNA was what made the occurence of θ-structures hard to understand. This widened and directed the scope of his enquires.

(219) Before he had approached Rodley, Rowe considered the question of the <u>rate</u> of unwinding required to account for replication <u>in vivo</u>, reporting the results of his calcuations to Probine more or less as the marvel that it is [q.v., (5) - (7)]. These figures duplicated those of Gorski [q.v., (77)], of which Rowe was then unaware. At this point, Rowe was worried more by the need for unwinding at all than by the rate required. Still working within double helical constraints, Rowe considered a model where one helical strand had a left-handed and one a right-handed sense; a model which had the advantage of not requiring unwinding for separation. But he rejected this idea because his literature search failed to reveal detection of left-handed structures [q.v., (9)]. Nevertheless, the idea of a model of DNA's structure which did not require unwinding persisted in Rowe's mind. And Rodley, too, found it intriguing [q.v., (11)].

(220) Rowe tended to concentrate on critical examination of the technical literature more than Rodley, whose interest was primarily in seeing whether an alternative model could be built.

But both read with care; and, because neither Rowe nor Rodley had specialist training in the field, they came afresh to the technical papers and were surprised by the mere fact that there was some criticism of the Watson-Crick model. As they read, so their concerns broadened to include the rate of unwinding - especially given the length and convolution of the DNA molecule *in vivo* [*q.v.*, (12) and (17)].

(221) Rowe and Rodley's reading of, and response to the history of problems in the the conformation and replication of DNA significantly and independently recapitulated that of Gorski [*q.v.*, (75) - (80)], whose (1975) and (1976) are the most thorough reviews of the literature on unwinding to that time available. Gorski was Professor of Plant Physiology in the Department of Molecular Biology at the Jagellonian University of Cracow, Poland from 1946 to 1977. His interest in the problem of unwinding developed as a result of encountering Cairns' calculations of the length of DNA in the chromosome of *E. coli* in the course of preparing lectures in biochemistry [F. Gorski to T.D. Stokes, 21/2/80]. Cairns's figure was nearly 1mm (970 microns), and Gorski

> realised immediately this length... must be a source of considerable difficulties if - as is generally assumed - the DNA strand separation is the effect of the activity of a rotatory unwinding mechanism [*ibid*.]

(222) As a result of the research reported in his (1975) [*q.v.*, (75) - (79)], Gorski - like Rowe and Rodley - concluded "that the separation of DNA strands based on a rotatory unwinding mechanism is an operation that is not impossible but almost impossible [F. Gorski to T.D. Stokes, 21/2/80]." In

classic Popperian vein, he then urged the need for development of an alternative mechanism [q.v., (79)]. Gorski had, of course, thought about what such an alternative might look like:

> It occurred to me that a way out of the difficulty could reside in [an] essential modification of the Watson-Crick model consisting in the inclusion of left-handed helices in the DNA structure [ibid.]

(223) This, as we have noted, was Rowe's first thought too [q.v., (212)]. And, like Rowe, Gorski rejected the idea because his research indicated that DNA is always right-handed. Gorski was frustrated by the lack of response to his (1975) attempt to stimulate interest in developing an alternative account of replication based on a mechanism other than unwinding. It may have been that his paper, though in English, was published in the Polish journal Folia Biologica, and so simply did not come to the attention of Western specialists in the structure of DNA. Nevetheless, there was no response from specialists in the eastern European scientific community either. But, the analysis of anomalies in the present chapter suggests that very few of those to whom Gorski addressed his plea for an alternative account of replication would have been interested. Rather, most would have been receptive to calls for solutions which preserved the unwinding required by the double helix - the necessity for which they had all long accepted.

(224) In an attempt to shock the scientific community into taking notice of his criticisms of replication by unwinding, Gorski (1976) resorted to a bizzare alternative model of his own, based on microscopic local space-time deformation. He admitted that this was an outlandish proposal - arguing only

that it was no more so than the received view of the process [q.v., (80)]. Nevertheless, the ploy was as unsuccessful as his original, straightforward critique. And, on my analysis, this is not suprising.

(225) The parallel between the work of the New Zealanders Rowe and Rodley, and that of Gorski is obviously close - though they worked quite independently. Essentially, in both cases, various problems with unwinding that had been constantly but sporadically aired in the literature from 1953 [q.v., III], led to the conclusion that replication (probably) did not take place by unwinding. Gorski, Rowe and Rodley all saw that this meant that something might be wrong with Watson and Crick's double-helical model of the structure of DNA. All three saw that, since the right-handed helices appeared to require unwinding to separate the two strands, the problem might lie in the 'handedness'. So they were all prepared to doubt a fundamental aspect of the received view of the structure of DNA.

(226) Yet Gorski, Rowe and Rodley all rejected this received view; and they reached their conclusion from a study of the technical literature wherein the question had been debated, and a consensus reached [q.v., (36) - (38), (68), and (97)]. It was a consensus of error - as later results were to prove, results which were employed in an attempt to resolve the θ-structures problem itself [q.v., (100) - (103)]. But, in accepting one aspect of the specialist community's view - that left-handed helices were not stereochemically possible - Gorski's could not proceed beyond his objection to unwinding to a (seriously

intended) solution which did require it. For all practical purposes, he felt that he had to accept that DNA was a double, right-handed helix.

(227) Rowe and Rodley eluded the same trap by a hair's breadth. Although they accepted that neither exoskeletal stand of DNA was not entirely left-handed, they did not conclude, as Gorski did, that that meant it must be a right-handed double-helix. Rather, they had concluded from their critique of unwinding that it probably did not occur, and also that a right-handed double-helix entailed unwinding (Gorski's tongue-in-cheek local space-time deformation model might have done him a disservice here). Consequently, Rowe and Rodley concluded that DNA at least might not be a right-handed double-helix. The alternative they devised, the SBS structure, does contain left-handed elements, half-helices. But, once they had invented the 'warped zipper', the New Zealanders' built molecular models of it successfully. That, and the supportive crystallographic evidence, dispelled any lingering doubts about left-handedness.

(228) For the New Zealanders, the θ-structures anomaly led to the broader problem of unwinding. This, in turn, led them to conclude tentatively that Watson and Crick had been wrong. In particular, they concluded that Watson and Crick's model might be incorrect because, in Rodley's phrase, its two strands were 'topologically dependent' - i.e., they entailed strand separation by unwinding. Thus unwinding as an empirical anomaly led the New Zealanders several stages along a train of reasoning which eventually led to the SBS model of DNA. Firstly, it provided the

motivation for developing an aternative model of DNA. Secondly, directed their attention to the specific aspect of the Watson and Crick structure for DNA in need of change: its interlacing, topological dependence. In turn, this broadly identified what any alternative model must be like; namely, it must not be such as to require unwinding in order to achieve strand separation

(229) The Indians were never in danger of having their reasoning abruptly terminated by the conclusion that, since DNA is not left-handed, both exosketetal strands must be right-handed. They reached the point of considering the evidence for the handedness of DNA quite differently from Rowe, Rodley, or Gorski. The Indians were not, initially, concerned principally by the problems associated by the necessity for unwinding on the Watson-Crick hypothesis. Their concern lay in the methodology by which models of DNA were developed. But that did not mean that they were unaware of the problems inherent in unwinding during replication. Quite the opposite. Once he had decided that systematic study of the conformational possibilities of DNA was in order, Sasisekharan had to set up criteria by which the results were to be judged. These will be discussed in detail later [chapter XI]. Here, however, we should note that among them was avoiding unwinding. Sasisekharan says: "We always had in the back of our minds the various problems associated with the double helical model.... For example, the unwinding process [q.v., (46)]."

(230) Moreover, whilst teaching, Sasisekharan came across an early discussion of exactly the same method of eliminating

unwinding as Gorksi, Rowe and Rodley had devised - a left-handed and a right-handed helical strand [q.v., (50) and (51)].[57] As we will see, this sparked the Indian invention of the SBS model. And Sasisekharan and his colleagues were not impressed by the rejection of the proposal on the grounds of the impossibility of left-handed helices in DNA - for it was precisely Sasiekharan's doubt over the way in which they had been eliminated in more recent discussion that motivated his project.

(231) So, apart from not being involved in the Indians' initial motiviation, the unwinding problem had a similar function for both groups. For both the New Zealanders and the Indians; the unwinding problem was at the core of a nest of problems which would only be eliminated by a model of DNA that did not require unwinding. It is to the motivation of the Indian investigation of the structure of DNA that I now turn. This reveals a kind of reason for the rational development of novel theory quite different from the empirical anomaly with which we have been concerned here.

57 It would appear that Gorski and Rowe were unaware of this proposal, advanced and rejected by Watson and Crick [q.v., (68)].

IX METHODOLOGY AS A CONCEPTUAL PROBLEM

(232) Laudan (1977) points out that most philosophers of science have focused their attention on empirical problems in science and, in particular, they have stressed the role of the empirical anomaly [cf. idem, p.26]. Against this, Laudan urges that what he calls 'conceptual problems' have been "at least as important in the development of science as empirical problem solving [idem, p.45, emphasis in the original]." In Laudan's view,

> Conceptual problems arise for a theory, T, in one of two ways:
>
> 1. When T exhibits certain internal inconsistencies, or when its basic categories of analysis are vague and unclear; these are internal conceptual problems.
>
> 2. When T is in conflict with another theory or doctrine, T', which proponents of T believe to be rationally well founded; these are external conceptual problems [idem, pp. 48-49, emphasis in the original].

(233) There are non-empirical features in both the mainstream and the avant garde response to the θ-structures anomaly and, more generally, to the unwinding problem. Watson says of Cairns' molecular swivel hypothesis, for instance, that it is "very difficult to translate into a precise molecular form"; and that it is "particularly difficult to comprehend...[the swiveling] process occurring when replication passes over the supposed swivel region [(1970), p.284, q.v., (196)]." Here, Watson is not pointing to an apparent contradiction between empirical evidence and the expectations of theory. Rather, he is indicating vagueness and unclarity in the specification and operation of the

molecular swivel hypothesis.

(234) Earlier, we saw that Watson admited that problems remained in explaining replication in circular DNA molecules, even when the swivel hypothesis had been "recast" in terms of superhelices and (cutting and splicing) enzymes [q.v., (204)]. He noted: "One of the two strands has to break, but how this occurs without generating an incomplete linear daughter helix remains an intriguing dilemma [(1976), p. 237, emphasis added]." This is a non-empirical, conceptual problem because severance of one of the exoskeletal strands is not an experimental obervation (indeed, no such breaks appear to occur). Rather, when Watson says they 'have to' occur, he means not merely that replication theory requires breaks, but that he cannot conceive of θ-structure formation unless the strands are severed.

(235) Similarly, as we have seen, Rowe, Rodley and Gorski's different evaluation of the unwinding problem was based not only on empirical evidence clashing with theoretical expectation, but also on conceptual difficulties similar to those which concerned Watson - specifically, in the case of Rowe and Rodley, with regard to the θ-structures anomaly. It is not only in details that the unwinding problem conforms to Laudan's notion of a conceptual problem, unwinding is at its centre a non-empirical problem. The reason for this is straightforward - there is no direct empirical evidence that unwinding occurs [q.v., (73) and (79)]. Consequently, there is no direct experimental evidence of the rate of unwinding. Thus, when Rowe and Gorski both worried about the rate of unwinding required, their concern was the

plausibility of a consequence of the Watson-Crick model of DNA in the light of what was known experimentally about the time taken to complete replication. As we have seen, the mere fact that unwinding had not been detected experimentally despite the elucidation of many other aspects of replication was, in itself, a matter for concern among the scientific community [q.v., (72) and (73)].

(236) Nevertheless, no scientist ever suggested rejecting the Watson-Crick structure for DNA because its concomitant in explaining replication, unwinding, could not be detected. That was because direct experimental detection of unwinding was not expected - as a result of the limitations inherent in the electron microscopy process. This also helped to defuse the concern over the rate of unwinding. Calculation of the rate of unwinding - as of of other factors such as in vivo convolution of the molecule, torque effect etc. - was necessarily based on many speculative assumptions.

237) The features of the unwinding problem which I have characterized above as conceptual problems are probably best seen as being what Laudan calls internal conceptual problems [q.v., (232)]. They involve concern over vagueness, lack of clarity, conceptual ambiguity and indeterminacy within the Watson-Crick based theory of replication. Laudan holds that internal conceptual problems are, in general, of less decisive importance than external conceptual problem because a certain amount of vagueness, lack of clarity etc., is to be expected in all but the most highly axiomatized theories [cf. (1977), pp.49-

50]. In the case under discussion here, there are two other factors at work. If conceptual problems rest on limitations in experimental techniques it is unlikely that they will be taken to be sure grounds for rejecting a theory. More importantly, the problems with unwinding did not prevent the development and application of molecular genetics. It is consistent with Laudan's overall emphasis on intertheoretic appraisal that he should view internal conceptual problems as of lesser importance than the external variety.

(238) The conceptual difficulties in the Watson-Crick based theory of replication mentioned thus far are mixed with unexpected and unexplained experimental data which, for example in the case of the θ-structures, form anomalies which are not straight-forwardly empirical. This suggests that, even where there appears to be a simple clash between the predictions of a theory and experimental results, we need to be sensitive to the presence of non-empirical features.

(239) The problem with which we will be principally concerned in the present chapter is an example of one sort of <u>external</u> conceptual problem. It will be recalled [from (232)] that Laudan holds external conceptual problems to derive from a clash between two theories, both of which the scientist has grounds to consider rationally well-founded. They may both be first order scientific theories, one may be a second order normative theory of scientific methodology, or a third order world-view [see Laudan (1977), pp.54-64]. The problem which impelled Sasisekharan on the course which led to the Indian variants of the SBS

model of DNA was an external conceptual problem resulting from a conflict between a first order theory about the structure of DNA and a second order theory about how such a structure should be devised, tested and refined.

(240) In Laudan's view,

> Every practicing scientist, past or present, adheres to certain views about how science should be performed .... These norms, which a scientist brings to bear in his assessment of theories, have been perhaps the single major force for most of the controversies in the history of science, and for the generation of many of the most acute conceptual problems with which scientists have had to cope [(1977), p.58, emphasis in the original].

These conceptual problems arise when norms of method conflict with extant theories. For example, a Baconian inductivist who believes that theory must be derived directly from observation will find her or his methodological views clashing with a theory that proposes unobservable entities. The first order theory may, however, be consistent with other accepted canons, resulting in a situation where the scientist has good reasons to believe in both theories. Laudan notes that, where a scientific and a methodological theory conflict, the resolution may lie in modification of either to accomodate the other. Nevertheless, Laudan insists,

> If a scientist has good grounds for accepting some methodology and if some scientific theory violates that methodology, then it is entirely rational for him to have grave reservations about the theory [ibid., p.61].

(241) In discussing the relative weighting of conceptual problems, Laudan specifies three criteria which are relevant to deciding the importance of any particular clash between a

scientific theory and a methodology (a fourth criterion relates to the situation where both theories are first order and in the same domain). They are: the logical relation between the two theories, the empirical effectiveness of the scientific theory, and duration without resolution of the conflict. In deciding the significance of the logical relation between two conflicting theories, Laudan points to the importance of the relation desired. For example, the relation may be compatibility where reinforcement or entailment is expected. With respect to the empirical effectiveness of a first order theory, Laudan maintains that the more successful the theory is, and the more problems would be created by abandoning it, the less important its conflict with a methodological doctrine will seem. For Laudan, the longer a conceptual problem persists without satisfactory solution, the greater the problem will seem. (See Laudan, (1977), pp.65-66.)

(242) Sasisekharan's concern over the adequacy of the Watson-Crick structure was, as we will see, a methodological conceptual problem in that there was a tension between the model and the methodological standards according to which he felt it should be judged.

(243) In chapter II we saw that the development of the SBS model of the structure of DNA in India had a paradoxical beginning. Mitsui _et al_. (1970) had suggested that the poly d(I-C): poly d(I-C) might have a _left_-handed, double-helical structure [_q_._v_., (37)]. One main basis for this proposal had been the Mitsui group's inability to convert this D-DNA into either the A

or the B form of DNA [q.v., (36)]. But Sasisekharan was able to convert poly d(I-C): poly d(I-C) into a B-DNA [q.v., (38)]. Since B-DNA was known to be interconvertable with A-DNA, and because it was held that a left-handed, double-helical model of A-DNA was stereochemically unviable, Sasisekharan appeared to have re-established the status quo; namely, that all forms of DNA are right-handed double helices.

(244) But an important part of the argument of Mitsui et al. remained unanswered. The CD spectra of A and B-DNA are similar, whereas that of D-DNA differs significantly [q.v., (36)]. And this difference was still not explained. Sasisekharan found, however, that the puzzle did not evoke interest among his colleagues [q.v., (39)]. Thinking about why this should have been so led Sasisekharan to consider the methodology underlying the development, testing and refinement of models of DNA.

(245) This Sasisekharan characterized as follows:

> A model was built...[in order] to calculate [its] Fourier transforms, and if it agrees with the data one is happy and says 'OK. I have a structure'. [q.v., fn 10]

Reference to Hamilton's discussion at (88) shows that this was a fair description. When process of checking and refining the fit of the double-helical models of A, B and C-DNA to the diffraction data had occured, Hamilton concluded that the evidence now uniquely determined the Watson-Crick structure [q.v., (90)].

(246) Sasisekharan was unconvinced. The fit between the Fourier transforms of the refined Watson-Crick model and the

X-ray diffraction photographs is by no means perfect: it is merely in good agreement, and as Sasisekharan observes, "that does not mean that there cannot be another structure consistent with the data." He was not the first to question the technique by which the double-helical model had been refined and confirmed. Donohue had strongly criticized it as incapable of eliminating structures known to be incorrect on other grounds, and as being inherently biased in favour of whichever hypothesis it was used to test [q.v., (91)]. A flurry of other specialists defended the established technique [q.v., (92) - (94)]. They conceded much of Donohue's argument, relying on what was claimed to be the superiority of the agreement between the diffraction data and any alternative to the double helix hitherto proposed.

(247) Donohue, in his rejoinder, pointed out that if "it is sufficient to consider various models, and then choose as correct (after adjustment) one that gives satisfactory best agreement with experiment...[then] one can never be certain that a model sufficiently close to the true structure has been constructed [(1970), p.1702]." Sasisekharan's concern was the same as Donohue's had been. What concerned them both was that this procedure led to a severe limitation on the consideration of alternatives to the basic Watson-Crick configuration for DNA. Sasisekharan's conclusion was that Mitsui *et al*. "had not systematically explored the possibilities", and that this was *typical*. It seemed to him, as it had seemed to Donohue, that a thorough search for a structure for DNA that fitted the evidence as well or better than that of Watson and Crick was methodologically desirable, but had not been performed.

(248) We have already noted that Wilkins, Crick and Arnott, in responding to Donohue, conceded that in assessing the merits of the double helix its comparative merit was important, but placed the onus of producing alternative structures for such comparisons firmly on the shoulders of those who doubted the "generally accepted" view of matters [q.v., (93) and (95)]. Sasisekharan had additional evidence from his own research to substantiate Donohue's claim that refinement technique, applied to a single proposed structure, had a tendency toward built-in bias. His preliminary investigation had suggested that the flexibility of the bond angles of DNA was considerably greater than had been previously allowed [q.v., (40)]. And the measure of fit between any model of DNA and the diffraction data, the 'reliability index' ['R', q.v., (116) and n11], actually discriminates in favour of models with invariable parameters and against those which permit flexibility.

(249) Another factor for Sasisekharan's was the way in which the refinement and testing of the double helical model of DNA had failed to take full advantage of the development of computer technology. In the past, the sheer number and complexity of the calculations involved had limited what was practical. Sasisekharan felt that this was no longer so. He thought it was now feasible to study systematically with computers all possible sterochemically viable configurations of DNA; beginning - as he had done in Madras - with the (deoxyribose) sugar ring, progressing to the basic building block of DNA, the dinucleotide monomer, and finally to the polymer structure itself. The Fourier transforms of all feasible models could then be compared

with the diffraction data. Should any alternative have fit with the data comparable to that of the Watson-Crick structure, it could be compared with the double helix with respect to problems like unwinding.

(250) As a specialist in the conformation of biological macromolecules, Sasisekharan knew that a number of others in the field had made their reputations developing and deploying the established technique for testing and refinement of DNA (especially Struther Arnott). To Sasisekharan this meant that there was, in a sense, no justification for his research unless it was clearly methodologically superior to established procedures - whether its results confirmed or overturned theirs.

(251) Thus it was that Sasisekharan his co-workers launched on a systematic study of the possible conformations of DNA; a study which, as we saw in chapter II, led to the development of the SBS model of DNA. Sasisekharan percieved a clash between the <u>kind</u> of evidence which supported a scientific theory - the double-helical model of the structure of DNA - and the <u>kind</u> of evidence which he thought ought to buttress it. Though concerned by the problem of unwinding [<u>q</u>.<u>v</u>., (222)] Sasisekharan, like most professional molecular biophysicists, did not reject the Watson-Crick model because of it. The New Zealanders began their work on an alternative structure for DNA because of what they saw as a clash between theory and data leading to a tentative conclusion that Watson and Crick had been fundamentally wrong. Sasisekharan did not. He began his inquiries in the belief that the technique by which the fit between evidence and theory was

assessed could be improved. Sasisekharan sought to develop a different and superior methodology to that originally due to Watson and Crick and developed by Wilkins, Arnott and others. The New Zealanders, as non-specialists, were hard put even to emulate the standard technique of refinement and testing. The Indians sought to surpass it - whatever the outcome.

(252) Though clearly a methodological conceptual problem in Laudan's terms since it was a clash between a first order scientific theory and a second order normative theory, Sasisekharan's doubts did not involve a straightforward choice between accepting either the normative theory or the scientific theory. He had doubts about the process of refining and testing models of DNA <u>itself</u>; doubts which were methodological in origin. Thus it was not initially a question of rejecting the Watson-Crick structure according to the usual standards. Rather, Sasisekharan wanted to improve the methodological criteria of judgement and then apply them. In the end Sasisekharan concluded that there <u>was</u> an alternative model which seemed to compare well on the evidence with the double helix. But, the methodological standard by which <u>both</u> were appraised was his new one. Moreover, Sasisekharan's attempted improvements were not merely technical. They were enabled by advances in the technology available to him, but they sprang from a fundamental methodological stance. Sasisekharan held that no structure for DNA should be accepted unless one could be reasonably sure, not merely that it was better than any alternative that happened to have been proposed, but better than any that could be proposed.

(253) Laudan specifies five "cognitive relationships" between any two theories; entailment, reinforcement, compatibility (indifference), implausibility and inconsistency. He holds that, in all cases but that of entailment, if the desired relationship differs from that which appears to obtain, a conceptual problem can occur - with the most severe "cognitive threat" posed by inconsistency when entailment is desired [(1977), p.54]. Sasisekharan's doubts centred on the reinforcement provided by methodology for the Watson-Crick model. According to Laudan a theory, T, is said to reinforce another theory, T_1, insofar as "T provides a 'rationale' for... T_1 [(1977), p.54]." The view of the specialist scientific community was that, when applied to the double-helical structure, the refinement and testing techniques that had been developed provided a satisfactory rationale (justification-for-belief-as-correct) for it. Sasisekharan's view was that this reinforcement was insufficient since it was conceivable that an unknown different model would be as well supported by an application of standard techniques to the empirical evidence. Donohue, as we have seen, shared this view. But he took it no further. Sasisekharan, however, advanced the methodological principle of exhaustive elimination of alternative structures as the criterion of an adequate rationale for acceptance of a model of DNA. Moreover, he sought to apply this criterion to the relevant data, to instantiate it. Nevertheless, it must be acknowledged that Sasisekharan's doubts about the real extent of reinforcement provided by the established refinement and testing techniques were not as weighty a conceptual problem as is possible on Laudan's schema. Moreover, until Sasisekharan had developed and applied his methodology, he could

not have more than a suspicion of something amiss.

(254) Laudan's second criterion for assessing the weight of conceptual problems is this:

> When a conceptual problem arises as a result of a conflict between two theories, T_1 and T_2, the seriousness of that problem for T_1 depends on how confident we are about the acceptability of T_2. If T_2 has proven extremely effective at solving empirical problems and if its abandonment would leave us with many anomalies, then matters are very difficult for the proponents of T_1. If, on the other hand, T_2's record as a problem solver is very modest, then T_2's incompatibility with T_1 will probably not count as a major conceptual problem for T_1 [(1977), p.65].

There is nothing modest about the problem solving record of the molecular theory of inheritance and reproduction based on the Watson-Crick model of the structure of DNA. Moreover, the principles and techniques by which its adequacy was tested - the principles and techniques that Sasisekharan was questioning - have been used to solve the structure of many biological macromolecules other than DNA; one example being RNA [q.v., (97)].

(255) Laudan's second criterion helps explain the limited concern which Donohue's critique of the methodology backing the Watson-Crick structure produced among specialists. It also helps us understand why the specialist community, though accepting some of Donohue's methodological qualms, placed the onus on him to produce a superior model. Donohue's doubts were discounted against the powerful - if indirect - evidence provided by the achievements based on the double helix. It was this overwhelming

practical success which underwrote specialist confidence.

(256) Yet both Donohue and Laudan assume that any critic of a scientific theory from a methodological base must have grounds which persuade all or most members of the specialist community. Either there are objective grounds for doubting a theory on the basis of its failure to conform to methodological standards or there are not. The scientist and the metascientist assume that if their arguments are good, then the same level of doubt, more or less will be shared by all. This need not be so.

(257) As was argued in chapter VI, and again in chapter VII, the standards by which a scientist should assess the adequacy of a theory or hypothesis depend upon her or his purposes in doing so. On this view the adequacy of the refinement and testing techniques that were used on the double-helical model of DNA depends upon the interests of the scientist judging them. For those professionally engaged in developing and applying these techniques, they were satisfactory just because of the empirical success that they had enabled. Thus Hamilton (1968) makes the point that although it may be arguable whether the Watson-Crick model is "the actual structure of DNA or still a structural hypothesis .... [,i]t is nevertheless relevant to current research" in as much as the "derivation of the A conformation of DNA greatly aided the elucidation of the RNA double helix which is very similar [p.636, <u>q</u>.<u>v</u>., (255)]."

(258) Donohue, by contrast, was spurred into advancing his critique by the elimination of his own alternative base-pairing

model on the basis of the standard testing procedures [q.v., (91)]. Because his interests were not directly methodological, Donohue did not propose an alternative procedure. It was enough for his purposes that doubt be cast on the decisiveness of the refutation of his model of base-pairing. Not sharing his interests, other specialists were unimpressed.

(259) Sasisekharan's concerns were, initially, strictly methodological. He had some reason to suppose that when a more rigorous standard was applied there would be room for alternative structures (that is, to suspect a clash between two first order theories judged by the same methodology). But, as was noted at (39), he was as unable to persuade his immediate colleagues of any methodological deficiency as Donohue had been. And for the same reason - methodology was, for them, a means to an end they were successfully achieving. The focus of Sasisekharan's attention was the means and not the end. As we will see in Chapter XI, because of this, Sasisekharan was prepared to let the end be what it may - provided that he was satisfied with the means. Rather than seek to persuade the specialist community as a whole of the need for a more rigorous method, Sasisekharan was content to be persuaded himself, and to get on with devising and applying a methodology for ascertaining the structure of DNA.

(260) Sasisekharan did not ignore his peers. Rather than seek to persuade by criticism of established methodology alone (like Donohue), Sasisekharan saw the need to successfully deploy a methodology which would compare favourably with the customary approach. If the results confirmed the Watson-Crick structure,

then so much the better according to Laudan's criterion [q.v., (254)]. In the meanwhile, Sasisekharan did not expect to persuade the specialist community by publishing his methodological worries [q.v., (250)].

(261) When scientists assess the adequacy of a scientfic theory in the light of their interests, they do not necessarily do so consciously. Scientists may consciously decide to act as if a theory were true, because it is adequate for their purposes, realising that these interests do not require or establish truth. But the unconscious operation of scientists' interests may very well eliminate such caution. Few like to work with theories that are merely instrumentally useful. Moreover, the inventers of a successful theory, or those who have made their reputation developing it, are very unlikely to arrive at such a conclusion having first thought that they had 'discovered' the truth. In the case of the Watson-Crick model, Hamilton (1968) illustrates the drive for certainty very clearly. He is determined that the status of the double helix - "model or reality?" - be settled. Like Wilkins, Crick and Arnott, Hamilton sees the utility of the double-helix as evidence of its veracity, buttressing the direct verification methodology and technique.

(262) Laudan's third criterion for assessing the importance of a conceptual problem is its "age". With Lakatos and Kuhn, Laudan shares the (inductive) belief that the past successes of a theory in eliminating problems warrants confidence in the eventual elimination of newly apparent difficulties [(1977),

pp.65 - 66]. Accordingly, Laudan believes that an old and unresolved conceptual problem is more weighty than a newer one. Again, this view is underpinned by the assumption that, by and large, the weight of a conceptual problem should be the same for all. Among conceptual problems, handicapping is, so to speak, weight for age. However, if even the existence of a conceptual problem in the mind of a scientist - let alone its severity - is a function of her or his interests, then what becomes relevant is the length of time that a perceived conceptual problem has stood in the way of a particular scientist pursuing her or his interests. Depending on how central those interests are to the scientist's overall concerns, even a short period may seem intolerable. On the other hand, where the interests of a scientist are only peripherally interfered with by a conceptual problem, the inductive argument against treating it seriously will appear applicable.

(263) We have seen that the motivation for Sasisekharan to embark on the research that was to lead to the Indian SBS model of DNA was his perception of a methodological conceptual problem. Specifically, he questioned the adequacy of the standards which lay behind the refinement and testing techniques that had been used to warrant belief in the Watson-Crick structure; and he held that the technnology now existed to implement more rigorous and exhaustive principles of refinement and testing. Sasisekharan, as a specialist in the field, was naturally impressed by the indirect support for the double helix derived from the successes of the account of inheritance and replication based on it, and from the apparently successful solution of the

structure of other biological macromolecules. He was, therefore, in the position that Laudan describes, namely of having good grounds for believing both in a first order theory and in the possibility of a better second order theory.

(264) Donohue's earlier criticism of these techniques, and the specialist commmunity's appraisal of it, support Sasisekharan's judgement. But the weight of these considerations in any given scientist's mind, I have argued, depends on her or his purposes. Most scientists' interests mitigated against a focus on methodological deficiencies - even though they were admitted in some degree - since those interests were well served by the successes of the Watson-Crick structure. But Sasisekharan's concerns were avant garde. He was primarily interested in the adequacy of methodology, and in improving on it.

(265) In their (1977), written after the development of the 'warped zipper' to establish the legitimacy of their enterprise to a wide audience, Rodley and Reanney observe: "The more deeply embedded a theory is in the thinking of scientists, the more necessary it is that...[it] be constantly re-examined to see that it still fits the facts [p.50]." This is the role in which Sasisekharan cast himself before the development of the SBS model. We might well suppose it an essential task - though not one required of all, or even most scientists.