HYPOTHETICAL AND INDUCTIVE HEURISTICS

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> What would be our reasons for accepting H? These will be those we might have for thinking H true. But the reasons for suggesting H originally, or for formulating H in one way rather than another, may not be those one requires before thinking H true. They are, rather, those reasons which make H a *plausible type of conjecture*.
> Other *kinds* of hypothesis were available to Kepler: for example, that Mars' *color* is responsible for its high velocities, or that the dispositions of Jupiter's moons are responsible. But these would not have struck Kepler as capable of explaining such surprising phenomena. Indeed, he would have thought it unreasonable to develop such hypotheses at all, and would have argued thus. (Norwood Russell Hanson, 1961)

1. *Introduction*

Of the several conceptions of a 'logic of discovery' is the view that it consists of a kind of evidence and mode of appraisal distinct from that required for justifying scientific hypotheses, laws or theories. N.R. Hanson in a number of works has suggested that novel concepts, theories, etc. need to be selected from as worthy of further pursuit. This kind of appraisal is distinct from selection as worthy of belief both in terms of kind of valuation offered, pursuitworthiness versus beliefworthiness, as well as in terms of the appropriate kind of evidence or reasons to be given, in this case explanatory power versus empirical confirmation. Hanson's suggestion bifurcates scientific methodology into two components, that of prior appraisal - discovery - and that of posterior appraisal - justification.

Yet if science is construed as an enterprise consisting of epistemic as well as practical objectives, that is science seeks to know as well as to manipulate and control various parts of the world, it would appear that there should be some unity within the criteria by which an instrument of research, whether a substantive theory, a logical principle, a mathematical procedure, or a physical instrument, is to be considered epistemically promising and those by which it is to be considered epistemically successful. Criteria for promise seemingly should be among, or at least linked in a manner indicating that success is likely, to those appropriate for success. Promising instruments seemingly are just those that are most likely to succeed, and the discovery of promise should be a first step in the process of discovering success. Thus criteria for promise should not be different in principle from or totally unrelated to criteria for success.

Epistemic concerns may be divided into those of conception or formulation of belief, on the one hand, and those of evidence, on the other. Accordingly, scientific inquiry can be divided into the search for concepts or formulations of hypotheses, laws or theories, that is the search for appropriate theoretical terms, on the one hand, and the search for evidence, that is, criteria of truth or acceptability, on the other. This division has been thought to coincide respectively with the distinction between discovery, or the generation of concepts and hypotheses, and justification, or the generation and implementation of criteria for the believability or truth of the hypotheses, etc. thus generated. It is often held that 'logic' and thus philosophy is involved only in the second part of scientific inquiry, not the first.

However, in the post-positivistic traditions of philosophy the sufficiency of 'logic', that is abstract *a priori* formalisms for both demonstrative and non-demonstrative reasoning, has come into question. The logical empiricists' rejection of a 'logic of discovery' can be construed as a first step in this direction, for they rejected the use of formal logic as a means of constructing scientific concepts from those describing sensations or experimentally accessible objects. A similar point can be made in regard to evidence once it is realized that the links between many entities described in science and their observable effects are causal processes that cannot be discovered by *a priori* logical analysis of theoretical concepts. It thus follows that the search for evidence can be broken down into the search for hypotheses and theories that will provide causal links between events accessible to our senses and the principal objects of inquiry and the search for implementations of these causes in the laboratory or the field for the production of evidentially relevant sensory experiences. Both of these may involve conceptual research as a sub-enterprise. Accordingly the search for evidence can include objectives of conceptualization or formulation as well as the practical objectives of producing observable effects or rendering scientific entities distinctly accessible to observation. Efforts at justification are thus no less efforts directed to discoveries than are efforts at conceptualization.

Thus scientific discoveries, that is, epistemic novelties, can consist of either or both novel conceptualizations and novel empirical achievements. Though deductive logic, and the putative probabilistic theories of inference to which the word 'logic' is sometimes extended in the expression 'inductive logic' are insufficient to account for generative thinking in science, it does not follow that there are not other kinds of normative guidelines for the direction of the various aspects of scientific inquiry. This essay will focus on guidelines for conceptual inquiry in science, particularly what has been known as the 'hypothetico-deductive' and 'abductive' methods. It will be argued that these methods have certain heuristic disadvantages not possessed by certain traditional 'inductive' method of inquiry. However, this position does not exclude hypothetical procedures form all scientific inquiry, but rather supports the position that heuristics are local and not universal in their application in science, and that their evaluation inevitably must be in terms of expected costs and benefits in particular circumstances (Wimsatt, 1980).

Appropriate values for heuristic appraisal include these: They should have power in that in appropriate circumstances they are capable of producing novelty. Also they should be relevant to epistemic objectives insofar as the normative guidance they provide assists in the production of true or credible theories. That is, the novelty a heuristic provides should be the kind of novelty that is likely to succeed. Success includes surviving in the competition among scientific enterprises for further productivity of items of epistemic value, including efforts at elaboration, extensions to new domains and empirical testing. Heuristics should also narrow the options of conceptual or empirical inquiry available to a scientist who has undertaken a given problem so that he or she has a course of action that is defensible with good reasons and can be carried out in time that is humanly and technically possible in regard to other resources available in the circumstances.

2. *Hypothetico-deductive methods of scientific inquiry*

The 'hypothetico-deductive method' (hereafter referred to as HD) is often thought as a method for testing hypotheses or theories whose initial formulation is already in hand, that is a procedure appropriate for the context of justification, not for the context of discovery. According to the procedure, a hypothesis or theory should be tested by seeking observations of empirical consequences drawn logically from it. C.G. Hempel (1966) defends this position with the exclusion of any method for generating the hypotheses, or the concepts they employ. In general, Hempel rejects the possibility of any routine for generating scientific knowledge.

Karl Popper is also among the prominent advocates of this method, though, as is well known, for him the 'context of justification' consists of elimination by falsification and excludes justification by some non-demonstrative 'confirmation'. Though he explicitly disavows any logical method for generating new ideas, and thus apparently disavows a 'logic of discovery', Popper's views can plausibly be construed as a reaction to the restrictions placed by logical positivists and empiricists on the introduction of novel concepts. Traditional and logical empiricists restrict novel conceptualizations to those that can be 'defined' or 'constructed' from sensory or observational terminology. By contrast, Popper is in agreement with Einstein's suggestion that conceptualization in physics is the 'free creation of ideas' to which there is no 'logical path' from empirical concepts (1958, p. 32). Popper thus rejects empirical constructivism.

However, that Popper's methodology lacks a procedure for generating novel concepts does not keep it from being a 'logic of discovery'. Similarly Popper's attack on what he calls inductivism, the view that scientific knowledge is generated from an accumulation of unguided observations and generalization, is an attack on a heuristic for discovery that is not only restricted to empiricist concepts but also has no heuristic for selecting and designing observational and experimental enterprises.

The heuristic Popper recommends does not restrict conceptualization to terms and laws that apply to observable domains or terms that can be defined or otherwise logically constructed from such observables. The only restraint on the invention of scientific theories is that it be accompanied with a conception of empirical procedures that can, by 'logical' analysis, be seen to entail the falsity of the laws thus conjectured. Also experimental and observational programs are to be guided in advance by speculative theories and the general epistemic objective that they be demonstrated false by empirically sanctioned claims.

P.K. Feyerabend suggests quite plausibly in another context (1974) that various components of a candidate scientific belief system and the technical powers of scientists to conceive and carry out reliably veridical empirical programs that are evidentially relevant to these ideas are generally logically and epistemically independent of one another. Popper is thus mistaken to assume that elaboration of empirical consequences is just a matter of logical analysis of properly chosen, that is, testable ideas, laws or theories. Rather, links between theory and experiment are substantive auxiliary beliefs some of which may be derived from theories other than that whose test is sought. Feyerabend concludes that empirical connections can develop independently and can have independently varying degrees of credibility or reliability, that is varying values as evidence for truth. Thus the beliefs that connect a theoretical term to empirical concepts, e.g. dynamical or optical principles whereby observable effects of motion can be determined, are logically independent of those that implicitly define a term, in this case geometrico-kinematic principles for describing location and motion, and one set can be more or less epistemically adequate than the other. Contrary to positivist doctrine, a scientific concept need not logically entail knowledge of how it is to be tested. Kinematic principles for describing motion are distinct from and logically independent of dynamical and optical principles describing the manner in which motion is caused and the effects of motion upon observers' senses or other bodies.

It also might be added that since there cannot be deductive closure on epistemic states (one can believe or know a proposition without believing or knowing all of its logical consequences), even if one could sensibly ascribe a 'logical' relation between a newly invented concept and observable circumstances their discovery is independent of the initial formulation of the hypothesis.

The independence of discoveries of scientific concepts and their empirical connections thus removes all of Popper's 'logical' restrictions on new hypotheses or on concepts for their formulation. Furthermore, the implementation of these restrictions in scientific inquiry is putting the heuristic cart before the horse, particularly if considerable resources are required in the search of empirical connections. If such research can be rationally appraised, there must be some way of initially appraising the new concept as particularly promising apart from its known empirical implications. Otherwise such resources would have to be committed blindly, without reason, to any silly idea that came along. A rationale for such appraisal is lacking in both Feyerabend's and Popper's methodologies.

This can be considered a heuristic objection to so-called hypothetico-deductive methods. Such objections might be dismissed as of no philosophical concern. However, even the most ardent opponent of philosophies of discovery must acknowledge that efforts to test a theory constitute a rational enterprise, unless, of course he or she were to deny any form of scientific rationality. Still a defender of scientific rationality is likely to be concerned about whether reasoned choices can be made about which novel ideas merit efforts toward empirical application and test.

This conclusion should not be seen as requiring that one should never assume radically contrarian positions *vis à vis* the various assumptions of the scientific establishment in a given discipline, and to multiply as many different such conceptions as possible, as Feyerabend recommends. A philosophical position advocating scientific heuristics should not exclude the possibility of more or less fortuitous discoveries, and Feyerabendian flights of fancy could produce fortuitously something that turns out to be profound in its scientific merit. However, Feyerabend's historical claims that scientific revolutionaries, such as Galileo, did this cannot be reconciled with Galileo's writings. Also such flights of fancy can only be attributed a low probability of success given the usual limited human resources because the contraries to established paradigms are indefinite in number. As Feyerabend also acknowledges, by being 'counter-inductive' they lack any link with epistemic criteria. In short, their heuristic merit is on the lower end of the scale.

3. '*Retroductive*' *methods*

Thus the principal heuristic shortcoming of hypothetical methods is the absence of any rational selection among concepts or conjectures to be tried or pursued. Hanson (1958, 1961) was aware of this and suggested (see the quotations at the beginning of this paper) that the plethora of 'silly' hypotheses and concepts permitted by a-heuristic variants of the HD method can be significantly reduced by requiring that hypotheses and concepts be initially filtered by an assessment of their explanatory power. Thus Hanson defends C.S. Peirce's 'abductive' or 'retroductive' methods as a means of discovering which novel conceptualization or hypothesis is promising, or worthy of further research, particularly for the empirical connections and technologies needed for observational and experimental testing.

Hanson can be said to have proposed a heuristic for investigating hypotheses in sciences. Any procedure permitting effectively and without misleading bias the prior reduction of pathways to be explored in a search,, in this case hypothetical conceptions or laws for which empirical connections are to be sought, has heuristic value. The question that should now be addressed is whether Hanson's procedure can actually be valued as a heuristic.

Hanson makes two points that can be summarized thus: (1) The heuristic permissiveness of the HD method can be restricted by requiring that novel conceptualizations exhibit their explanatory power before they are seriously considered. (2) There is a mode of appraisal distinct from acceptance or rejection, viz. entertaining a hypothesis or employing certain terms in its formulation, for which there is also a distinctive kind of evidence, viz. its apparent explanatory power. This kind of evidence could warrant a certain rational choice in scientific research, that is the choice of formulation H over H' as the one for which testable applications can then be sought. If H has greater explanatory power than H', then programs of research for H should be given the breathing space needed for developing or improving the auxiliary hypotheses and the technology needed for testing.

Hanson's position gives rise to a number of questions: (i) Is explanatory power the only ground for entertaining a hypothesis or employing certain concepts in its formulation? Might there be other kinds of evidence for initial plausibility? (ii) If a concept or formulation requires some development to determine its explanatory power, how do we decide which concepts deserve such development? This latter question concerns the heuristic value of retroductive methods, and is similar to the question raised above regarding the a-heuristic variants of hypothetco-deductivism.

Problem (ii) is particularly evident in the deductivist account of explanation, where deductions presuppose quantitative or especially deductively fruitful qualitative expressions for concepts and formulations of hypotheses. In general, non-ad hoc explanations should appeal to laws which in different circumstances give different outcomes, and these distinct applications are items that may require extended research for their discovery. The search for such expressions thus can be a project requiring considerable resources. But then is there any criterion for choosing those concepts most worthy of the resources needed for their development?

In short, determining the explanatory power of a hypothesis seems to require some of the same kind of development that testing a hypothesis requires, at least short of the technological development required for gaining empirical access to certain applications. These considerations raise doubts about the heuristic import of Hanson's abductive procedures that are similar to the doubts already raised against Popper's methods.

(iii) A third question raised by Hanson's position is whether there is a mode of appraisal distinct from epistemic appraisal or judging the believability of a hypothesis, viz. the appraisal of the worthiness of a concept or hypothesis to be worked out? There are two parts to this question: (a) Is there a mode of appraisal of laws, concepts and hypotheses which is distinct from acceptance with varying degrees of assurance? (b) Are the criteria for this other mode of appraisal distinct from epistemic criteria?

It seems that a good case can be made for an affirmative answer to (a). One may work upon several different hypotheses without believing any of them in the hope that one might, with sufficient development and testing, either be believable or instrumental in determining what is believable, e.g. by turning out to be false. One may work upon competing hypotheses, and some (e.g. Chamberlin, 1904, Popper, 1958), Feyerabend, (1962, 1974) have held that inventing and exploring as many as possible mutually exclusive and even radically different hypotheses is a good heuristic. It has the advantage of skirting the biases of research programs confined to one conceptual system, theory or hypothesis, though it can also be costly, as pointed out earlier. However, one can hardly consistently accept a set of mutually exclusive hypotheses. Thus initial heuristic appraisal as distinct from decisions to believe seems well established.

However, as to (b), it is far less plausible to hold that criteria for heuristic appraisal are qualitatively, or more than quantitatively or circumstantially distinct from probative criteria. Particularly for the epistemic objectives and a search for optimal means for achieving these objectives, these criteria for prior appraisal should be those that indicate a promise or potential that criteria for final appraisal will be fulfilled. Such promise is undeniably given by the partial fulfillment of epistemic demands. Also grounds for initial epistemic appraisal need not exclude a multiplicity of mutually exclusive hypotheses to be entertained, for, as is generally acknowledged, hypotheses are empirically underdetermined, particularly by one or a few items of empirical evidence.

Question (iii) can be put in more concrete terms: Is explanatory power evidence, albeit not conclusive, for the truth or believability of a hypothesis? Some philosophers, most recently Achinstein (1987a,b) have denied that explanations are probative. Achinstein's argument appeals to the point that everyday actions can have various good explanations without there being any decisive evidence for the truth of any these various hypotheses. Being paid a large sum of money can very well, if true, explain my writing this essay, as can being required to do so for promotion, etc., etc., but some of these good explanations are not likely to be true. Rather than their explanatory power, evidence for these hypotheses must be either direct, e.g. testimony regarding my predominating motives, or indirect via some background belief about my circumstances or general circumstances bearing upon philosophical writing projects, combined with some theory of normal human motivation. A consequence of this position, which Achinstein is willing to accept, is that successful explanatory applications of a hypothesis or theory, even one with prior credibility, does not further enhance its credibility. However, this consequence seems very much at odds with arguments that seem common and influential in scientific practice, e.g. those Darwin gives in the *Origin* to the truth of his hypotheses based upon alleged superior explanatory power.

Some other considerations still seem to weigh against the epistemic import of explanatory power: Darwin's hope in 1837 for a naturalistic explanation of the biogeographical distribution of species in a genus is that there are non-ad hoc laws that apply in this domain. (See DeBeer, 1960, 1967 for transcriptions of 'Darwin's Notebooks on the Transmutation of Species, 1837-39', hereafter NTS, with page numbers and Darwin's designations B,C,D,E of the four volumes.) Genuine laws have counterfactual applications, and if they are explanatory they should be non-ad hoc in that they can be applied to different circumstances giving different outcomes, the various items in a domain of inquiry to be explained. But hypotheses or concepts that are newly introduced in the search for laws and explanations in an otherwise unknown domain gain no initial probative support from the *a priori* determination of their explanatory power because there is no reason for thinking that the new domain has the causal structure manifest in non-ad hoc explanations. That is explanatory hypotheses in the new domain are not believable just on the basis of their explanatory power. This consideration supports Achinstein's doubts about the epistemic import of abductive argument.

Perhaps this point can be made clearer by a little historical fiction: Darwin can be imagined to have 'played' with some ideas and hypotheses for which there was no epistemic warrant. Suppose some concepts C, C', ... gives a system of non-ad hoc laws L, L', ... and that these laws are applied to a variety thought experiments, e.g. about the possible split of a population of wolves into long and short legged varieties, the split of a population of birds into varieties with various kinds of beaks adapted to various feeding habits, etc., etc.. These thought experiments can demonstrate the explanatory power of the law-concept complex C, C', ..., L, L', ..., apart from any evidence that the conditions imagined exist or that they actually produce any adaptive splitting of an initial population. This conceptual exercise can give good reason to hope that biogeographical ecology actually has this kind of lawlike organization, a causal organization that Darwin recognized as similar to the dynamics of celestial bodies. It may also lead to the discovery of means of establishing this kind of organization in this domain by leading Darwin to the kind of evidence that needs to be sought in actuality. However, without any reason for believing L, L', ... are true in some observable domain, or that canine predators ever live in open and scrubby country and have long and short legged varieties, there is no reason to believe that biogeographical ecology has this lawlike structure. With these thought experiments selection can be shown, if true, to explain various possible distributions of species, but such hypothetical explanations do not provide any evidence for their truth. Many naturalists and philosophers in the nineteenth century believed that the domain of living organisms contains phenomena that are causally ordered differently from the domains of physics, chemistry, and even geology and climatology. Rather than non-ad hoc applications of natural laws, they sought, implementations of Divine Benevolent Will in various geographical circumstances.

However, in the NTS Darwin appealed not only to the patterns of explanation found in Newtonian astronomy, but also the kinds of explanation offered in Lyellian biogeography in defending his objectives for explaining relations among spatiotemporally proximate species (NTS, B100ff). Lyell's biogeography is an effort to explain the present distribution of animals and plants by reproductive and migratory powers, by the occurrence of species extinctions from natural causes, and by the disposition of various biogeographical and ecological barriers to migration. Hence Lyell's program dealt with the same subject matter as Darwin's own, the explanation of the present distribution of plants and animals throughout the world. Lyell's own program of explanation by migration and extinction establishes that the domain is lawlike in some respects, in respect to causes bearing upon migration and extinction, though it does not necessarily follow that it is lawlike in all respects, in respect to the production of new species. With the information thus far specified in this paragraph, there is no general law governing regularities in migration and extinction that would support the belief that comparable regularities exist in the production of new species. There is no plausible inductive principle according to which if a domain is lawlike in one respect, it is likely to be lawlike in other respects as well.

Darwin can and does make a stronger case for laws of speciation than just the premises that there are laws governing migration and extinction. His first premise is that the presence of mobile species over a wide range, including remote places such as the Galapagos Islands, is, as Lyell demands, naturally explained by their powers of locomotion and the geographical circumstances within their range. But secondly, varieties and species of a less mobile genus are often exclusively present in spatiotemporally proximate habitats. Furthermore, the older their habit or the barriers separating their habitats, the greater the differences among these representative varieties and species. Also, it has been acknowledged since the work of Linnaeus in the 18th century that local varieties can be produced by natural means, that is, causes in their production are lawlike. Given the observable continuity between local varieties and local species, i.e. likeness in the effects of geographical or ecological separation, it would be arbitrary it insist that the local species had been specially created but the local varieties had colonized by migration and varied by natural means. This reasoning is a kind of inductive extension of the genetic laws governing the production of local varieties to the production of species, essentially an inductive extension in time where the effects of the laws in the short run are the divergence of local varieties and in the long run are the formation of species.

Achinstein's suggestion that arguments to explanations or to the best explanations have no epistemic force also needs further qualification. We might concede that in Darwin's case just the observations of Galapagos finches is alone insufficient to justify belief in his suppositions about the causal structure of local speciation, just as good explanations of everyday action are not given evidence when applied to one instance. However, Darwin also observed local species of mockingbird, armadillo, ostrich, llama, and others, indicating that there can be a causal regularity in this domain that could explain these several occurrences. A system of laws and concepts that explained these is not only conceptually non-ad hoc, that it is addressed to more than just one case that might have initiated the search for an explanation, but also it is empirically non ad hoc as well, that is it is confirmed in independently empirically certifiable circumstances. This independently empirically certified application of his explanatory scheme is as much evidence, as are the inductive arguments cited above, that biography is in respect of speciation lawlike in its structure. Actually the finch case alone would indicate that genetic evolutionary laws are not empirically ad hoc, for they explain differences in the beaks of several related species in different ecological niches, each of which is empirically certified independently.

Analogously, if large cash awards are known empirically to be commonly awarded for writing essays in philosophy, then that such an offer would explain my present efforts would be evidence for this hypothesis, which is an instantiation of a pattern already recognized to occur or to be likely. Also if other behaviors are explained by this hypothesis, e.g. my borrowing large sums at the time I write, then it will warrant the hypothesis, particularly if some of these other behaviors exclude some of the rival explanatory hypotheses.

Thus appealing to the explanatory power of an hypothesis has no epistemic import if (i) it does not explain actual phenomena and (ii) there is no reason to believe that the domain of its application has a lawlike causal structure. Achinstein's view that the appeal to the explanatory power that a hypothesis would have, if true, is not a form of evidence for likely truth seems correct. At least it can be construed as a hope that a domain has a lawlike structure, but without some initial evidence that the domain is actually such, the hope is only that the new domain will be patterned in a way that is practically and epistemically advantageous to the inquirer, viz. it can be explained in familiar patterns and unobserved phenomena can be predicted. In such circumstances the hope has only a pragmatic and not an epistemic basis. Also his suggestion that empirically ad hoc applications of explanatory hypotheses, i.e. the demonstration that H explains just one empirically certifiable phenomenon P, has no import as evidence is correct, for even though initial conditions I can be observed independently of P, this one application of H does not demonstrate the lawlike character of the domain in which I and P occur. But this claim that other explanatory applications of H are not thus probative seems just false, at least by the criteria for explanatory credibility developed above. If H is applied to circumstances and phenomena I', P', which are respectively independently empirically certifiable from I, P, then evidence is provided that the domain is lawlike. That is, there is a law in the domain which, when applied in different circumstances, gives different outcomes. Once it is established that a domain has a lawlike causal structure in respect relevant to the concepts used in H, that H explains P under I is a reason for believing H.

Even though explanatory sufficiency in one empirical application is insufficient to warrant belief, it does not follow that this sufficiency is not the kind of evidence that can, in other circumstances,be probative. In short, though in some circumstances certain kinds of evidence warrant only consideration and not acceptance, that evidence can still be of a probative kind. This should be particularly plausible if it is recalled that in epistemic research we are looking for believable hypotheses, so any initial induction of H's believability is *prima facie* evidence that it is worth pursuing as a means to extending knowledge, for it suggests that such pursuits might be successful. If H is initially known as an explanation of just P in circumstances I, though this is no ground for believing H it is grounds for choosing H as worthy of working, on, that is seeking other empirical connections I', ...P', ... as evidence that this domain of inquiry is so causally structured that H has explanatory power and is likely to be true. Thus, Hanson and Peirce are correct in their suggestion that abduction provides grounds for further searching for tests for a hypothesis, but these are grounds that show it more likely than otherwise that H is true.

However, the abductive procedure still has a heuristic shortcoming pointed out earlier. It does not filter those ideas that warrant an initial search for empirical connections in some domain. Furthermore it presupposes and in no way generates concepts, laws or hypotheses that constitute the pool to be abductively filtered.

The inductive procedure that Darwin used also provides initial evidence that the biogeographical domain is lawlike in respect to speciation. Thus in this instance inductive heuritics meet the condition that scientific heuristics bear upon the credibility of the hypotheses they suggest. However, it is not clear that they have heuristic power in the sense of promise or capability at generating conceptual novelty. This point will be addressed in the sections to follow.

4. *Induction and analogy as heuristics*

Several research programs in geology and natural history made successful application of a research strategy which can be traced to Issac Newton's 'Rule's of Reasoning' fọr Philosophy (1962, Book III, pp. 398-400). This strategy seeks to establish *vera causae* in observable domains and to extend the application of these causal principles to domains where only the originally observed effects can be observed. The first three of Newton's rules are of particular interest here.

Newton states these as follows:

> Rule 1: We are to admit of no more causes of natural things than such as are both true and sufficient to explain their effects.

This rule restricts explanations to 'true causes' (*vera causae*), which are causes that can be established by empirical means within the 'reach of experiment'. The suggestion of parsimony in Newton's words can be read in several ways, one of which is the claim that the simplicity of nature implies a minimal number of causes (laws or agents, forces) over the various domains of the Universe, so that the inference of like causes in observable and unobservable domains is one of the consequences of this simplicity. It can also be read as restricting causes to those just sufficient to generate the effect. Thus it excludes superfluous causes. To contemporary thinkers this *a priori* assumption of the simplicity of nature is dubious, and accordingly the epistemic import of this inference is in doubt. Other ways of vindicating this principle might be appropriate, as will be discussed in the next section.

> Rule II: To the same effects we must, as far as possible, assign the same causes.

This rule permits the inference form like observed effects to an unobservable cause in cases where the cause can be linked to this kind of effect in empirically accessible domains. Thus if we can link centripetal forces to circular trajectories in terrestrial experiments, we may infer that centripetal forces cause circular motions in the heavens.

> Rule III: The qualities of bodies, which admit neither intensification nor remission of degrees, and which are found to belong to all bodies within the reach off our experiments, are to be esteemed the universal qualities of all bodies whatsoever. (1962, p. 398).

This rule permits the extension of laws over subjects which when divided can be described in the same terms. If the mechanical attributes of bodies, e.g. extension, figure, mass, locomotion, are not diminished by division, i.e. if each property remains applicable to parts as well as the whole, then these properties apply to wider domains, if not to the whole universe.

It is thus that Newton and others infer the properties of atoms and favor mechanical laws in theories of atomic behavior.

Although Galileo Galillei used a procedure similar to Newton's in inferring mechanical effects of the Earth's motion, he also expressed (1838) an awareness that one could not always infer from mechanical properties of small objects, e.g. the strength of materials, to those of large objects or conversely. This precaution was borne out in the 19th and 20th centuries in which it became apparent that a number of physically significant properties could be assigned to macroscopic systems, e.g. temperature, entropy, that cannot be meaningfully attributed to the lack of any apparent epistemic foundation for assumptions of simplicity and uniformity, the method of analysis that Newton grounds on Rule III cannot be based on deductive principles, for it commits the fallacy of division, and it breaks down in important areas of physics, chemistry and biology.

However, the several Newtonian inductive procedures where not only successfully applied by Newton himself in reasoning about the heavens, heat and light, but also by geologists and biologists, most notably Darwin. In geology James Hutton in the eighteenth and Charles Lyell in the nineteenth century used a what has been called (Rudwick, 1969) and 'actualistic' strategy. This procedure extends *vera causae* established for present day climatological and geological occurrences to presumably like occurrences in the past whose effects can be observed today and can be validated as evidences of the past by present day observation and experiment. Such causes are also observed today to be 'uniform' or slow and gradual in their action and local in their occurrences and effects, and these features are also presumed to hold for causes acting in the unobservable prehistory past (Kavoloski, 1974, Laudan, R., 1984).

In his NTS Darwin applied much the same actualistic procedure in inferences from observed domestic plants and animals and genetic relations among humans to represent and past causal processes in wild an in non-human organisms. Plants can be observed in the garden to flourish in some soil, humidities, conditions of light and shade, and competition form other plants and animals, but to perish or to suffer lowered fertility in other such circumstances. Members of a given species also vary when they sexually reproduce under favorable circumstances (B3f). The principle that adaptation to circumstances determines survival and propagation, which might be called Darwin's principle of survival of the adapted, is then extended to wild species, which, like the domestics, depend upon their surroundings for nourishment and are subject to death or low fertility form predators, diseases and other environmental adversities (B38, 64). Human individuals and families have the same dependencies for survival and successful reproduction, and humans must produce fertile offspring to be represented in subsequent generations. Repugnance to marriage, infertility or death can prevent a family from being thus represented (B145–148). Since wild plants and animals are similarly subject to failure in fertilization, disease and premature death, those 'families', viz. reproductive lines or varieties, that are most fertile because of superior abilities to attract a mate, to resist disease, and to successfully propagate offspring, will be represented in subsequent generations at the expense of those that are less fertile. Also from human demographics Darwin observes that relatively few of the families prevailing earlier in history can be represented now, especially if a constant population is assumed, presuming also that the average family has more than two offspring. Some human families must inevitably suffer extinction if not by death, at least by infertility. Applying this reasoning to species, Darwin concludes that species extinction is the inevitably consequence of species splitting, which might be called Darwin's principle of inevitable extinction. Thus species extinction should be a regular occurrence in geological history.

After rereading in the Fall of 1839 Thomas Malthus' *Essay on the Principle of Population* Darwin extended Malthus' principle of universal superfecundity to all living organisms, and applying his principle of inevitable extinction now to individuals of a species, he concluded that death of individuals within a species before they reproduce and perhaps other sources of infertility must prevail inevitably within the populations that constitute every species. From this conclusion and the premises that sexually reproductive organisms vary, and vary in their adeptness to circumstances, as established in the first part of Notebook 'B', and the principle of survival of the adapted, Darwin deduces the law of natural selection, viz. that everywhere in biological nature there is a continuing 'sorting out' of adapted characteristics and these characteristics are preserved because they favor the propagation of offspring, viz. the survival of populations constituting a species.

Darwin thus extends concepts borrowed from observable genetic processes in domestic plants and animals and from observations in human demographics to the wild in an effort to discover 'laws' governing natural speciation. The principles he gets from this effort are (1) that species' probable survival and reproductivity are proportional and (2) that superfecundity in families and in species entails extinction in each. The effects of

(3), the principle that adaptive variation occurs at birth at various rates are, by (1), survival and extinction. The specific causes of reproductive success and failure are various, but each can be established as *vera causae* from observations on domestic species and human beings. By a similar process of extension from Malthus' observations about human beings, Darwin (4) claims that all species, even the most slow to reproduce, tend to multiply geometrically thus causing universal and inevitable population pressure. Universal superfecundity is thus also *a vera causa*. Malthus' principle gives an antecedent for (2), a driving force producing pervasive extinction in nature. From these premises, Darwin deduces an effect, the sorting and propagation of adapted structure.

Also, Darwin was quite aware that propagation, extinction and survival are terms that apply to individuals, families, races or varieties and to species. The reverse of Newton's method of analysis is composition or 'synthesis'. Thus a similar heuristic to that Newton allowed for investigating atoms can apply in composition, viz. when wholes can be described in the same terms as their components, look for laws like those governing components. As individuals, domestic animals and plants, human beings and wild organisms of all species share in sexual reproduction, disease, problems of nutrition and predation. Lyell had noted that extinction of species consists of the gradual extinction of the individuals that compose it, that is a gradual decrease of a population to nothing. The converse of this process would be the propagation of a species, which, according to Darwin's compositional heuristic could consist in the successful propagation of individuals resulting in an increase of the population. Thus Darwin arrives at the principle of propagation of the adapted, or the 'survival of the fittest'.

A second procedure Darwin shares with Newton is one of extending *vera causae* over an established natural kind. For Newton this procedure allows him to extend, by reference to Rule II, according to which like causes should be sought for like effects, terrestrially established laws of motion and gravitation to the celestial region, as in the case of inferring centripetal celestial forces from their occurrence in terrestrial motions such as that of a stone in a sling. Similarly Darwin reasons that genetic laws governing one of the groups, humans or domestic animals and plants, should also govern the other, groups of wild organisms. Lyell's actualistic heuristic also permits the extension of laws established as *vera causa* in time, and for Darwin this means that the genetic laws found in humans and domestic organisms can be extended into the indefinite geological past.

Finally, the deduction of natural selection from 'phenomena' such as universal superfecundity, using principles of variation, inevitable extinction and survival, is comparable to Newton's deduction of uniform terrestrial gravitation from Galileo's mechanics and inverse square gravitation from Kepler's laws. This procedure is 'inductive' not in the sense of inference from numerous instances (enumerative induction) or in the sense of generalization of extension, but rather in the sense of inference to theory from observational premises. As in Newton's case, this inference is not purely logical, but requires a background of laws such as inertia, in Newton's case, and variation, extinction and survival, for Darwin.

In sum, there are several conditions under which these several inductive heuristics are promising: (a) When some properties of a system transfer to components or to compounds under analysis or synthesis, one should search for like causal and other laws governing both compound and component. (b) When natural kinds are established by the clustering of determinable properties that either vary in degree or take on discrete or qualitative values for members of a natural kind, then laws applying to one component also can be expected to apply to other components of this natural kind. Newton's *vera causa* procedure is a special case of (b): Common descriptions of two observable effects establish that they are of a common natural kind. The observable cause of one of the effects can then be described as a law which, by (b), can be extended to the domain in which the cause cannot be observed.

5. *The status of inductive heuristics*

In recent writing 'induction has been considered in two ways: (i) As in Reichenbach's discussions induction is fundamentally enumerative procedures in which the credibility of a generalization is roughly propositional to the number of its observed instances. Also (ii) logical empiricist writers such as Carnap and Hempel had thought of induction, particularly inductive logic, as a procedure for confirming hypotheses that are not restricted to generalizations from observation. In contrast to Reichenbach, the logical empiricists regarded induction as a procedure for justification, not discovery, and they recognized that Reichenbach's empiricist inductivism provided only means to defending generalizations in observational terms and no means to generating theoretical terms. Yet, as should be apparent from the preceding sections, none of this writing has taken into consideration the apparent heuristic power in conceptualization in Newton's, Darwin's and others' use of more broadly 'inductive' procedures.

This section will attempt an understanding of 'inductive' heuristics in a context in which the dichotomy between 'observational' and 'theoretical languages' that is typical of the logical empiricist tradition of this century is not present. According to this dichotomy, observational languages are universal in that they are common to all scientific disciplines as well as to pre-scientific thought. They are also unchanging and fixed, and at the same time contain only expressions designating properties and entities that are 'directly' accessible to observation, that is, accessible to the unaided senses. They are also both semantically and epistemologically fundamental: Any new theoretical terminology must be definable in observational terms in some appropriate logical sense of 'definition' and only sentences in these terms can appear as premises in empirical arguments to theoretical conclusions. This dichotomy has been successfully challenged by showing that distinctions between 'direct' and 'indirect' observations are at best vague and generally dependent upon highly variable epistemic and technological circumstances. Furthermore theories are needed to interpret observational claims in science, and accordingly the meanings of these sentences are as variable from discipline to discipline and in time as are the theories that dominate. It will be shown that without this dichotomy and the epistemic and semantical empiricism that accompanies it, inductive heuristics can be understood as procedures for discovery, particularly for conceptualization, and yet not necessarily be subject to the limitations that lead Popper, Carnap and Hempel to reject logics of discovery in general and inductivism in particular.

Reichenbach gave a general formulation of enumerative induction that covers statistical as well as non-statistical inference. For a sample A_n of a reference population A, if the observed frequency of property B approaches a value r as n, the size of the sample, gets ever larger, then postulate r as the limiting frequency of B or the probability of an occurrence of B in the population A. In the special case that r=1, this procedure reduces to simple enumerative induction. The enumerative aspect of this procedure is contained in the supposition that larger samples of A are more likely to show frequencies closer to the appropriate limiting frequency and hence the larger the sample the greater should be the confidence that the limiting frequency is within an interval [r–e,r+e], where r is the observed frequency and e is an arbitrarily small deviation of r from the limiting frequency. Also the larger the sample the smaller e should be.

However, in either this special case or in general, enumerative induction can only allow the inference of generalizations over a given reference class A for a given property B. Thus, as usually understood, this procedure cannot introduce new concepts beyond what are assumed to be observable properties or conditions A and B, and hence its productivity is limited in that in many circumstances it could generate only very weak or uninteresting explanatory hypotheses. One cannot use it to generate hypotheses that explain the attributes A or B in terms of something more fundamental.

Enumerative induction, and other forms of induction to be discussed below, need not be considered restricted in power to conclusions about observable objects. That is, the terms A and B appearing in inductive premises need not be considered 'observational' terms, as was usual in the logical empiricist tradition. Contrary to the empiricist assumption that a given term must be observational or not so in all of its applications, the term for reference class A could apply to entities for which observation is possible in certain subdomains, e.g. A_i, $i=1,...,n$, for some finite and relatively small n, so that whatever falls outside of these subdomains is technically unobservable, or in principle unobservable relative to known causel processes linking the objects of investigation to human senses and available techniques of experimental manipulation. Thus the domain of material bodies may be partitioned into those observable microscopic, submicroscopic, atomic, nuclear or sub-nuclear size without necessarily prejudicing frequency of some other property B associated with A. Another plausible generalization of Reichenbach's procedure would be to allow the unobservable portion of A to be only partially so, e.g. the motions of Newton's celestial bodies are observable but the centripetal forces acting on them are not. Similarly B can be extended to include various abstract or second-order properties, e.g. the properties of being describable in newtonian mechanical terms, subject to laws of energy conservation, describable in demographic terms, subject to laws of reproductive genetics. B might also be a property of properties, laws or systems of description, e.g. those of being a scalar or vector field theory in Euclidean space, or invariant under Galilean or Lorentz transformations. One ore several laws, such as Newton's laws of motion and gravitation, Coulomb's law for electrostatic attraction, or Lorentz's and Maxwell's laws for electromagnetic interaction, could thus be inferred to hold throughout the magnitudes represented in A event though they are not all observable. For example, in the extension of mechanical laws form observable A_i to all of A, it is supposed that Newtonian kinematic terms, i.e. the descriptive terminology for particle location and motion, can be applied in the description of matter throughout A. One might also infer, as did Galileo, from the invariance of mechanical behaviors on ships and on shore to the invariance of mechanical behaviors on rotating and stationary earths, or, as did Einstein, from the Lorentz-invariance of Maxwell's equations to like invariance of proper equations of mechanics. In Einstein's case the premise from which the inference is drawn is not 'observational' in the sense usually employed by empiricists. Thus the more generalized view of this inductive procedure should require just that the premises be known, not necessarily by observation or experiment upon natural objects and events.

Reichenbach's 'justification' of induction is also suggestive as to how the 'context of discovery' should be construed. His argument for induction is that, although the procedure cannot be guaranteed to succeed in any circumstance, without any prior knowledge of A other than a random sample it is the best procedure available for inferring from the observed sample to unobserved parts of A. If circumstances are such that it will fail, that is if the whole domain A is not like the observed sample A_n in the respects required for successful extension of what is observed in the sample, a uniformity that is the 'bias' of this induction, the inductive procedure will show it thus by failing or by justifying some other procedure with a different bias, e.g. one dependent upon some systematic change in A, possibly a newly defined reference class A' or property B'. If other procedures are more efficient in producing or justifying such extensions, they will be demonstrated so by enumerative induction. Enumerative induction is thus fundamental to all induction and the best, or the only rational procedure available for inquiry into unobserved domains. That is, it offers the best hope for the deliberate extension of concepts and laws to unobserved domains without guaranteeing that such extensions will succeed.

Reichenbach's 'pragmatic' justification places a presumption upon methods that presuppose continued uniformity in A as observed in the sample A. With no indication to the contrary, the best chance of success in discovering what occurs in the unobserved portion of A is to presume that it is like A_n, which in this case is to suppose that if the frequency of B converges to a limit as n grows large, then ever larger samples in the unobserved part of A will show continued convergence. If divergence is ultimately to occur this method will show it by failing or by justifying the association of some other property B' with A or a newly defined reference class A' for which frequencies will converge with larger samples. With no indication to the contary, laws established in observable A_n can be presumed to hold in unobserved portions of A, and thus initial evidence in the discovery process can consist just of evidence that a certain law is known in some domain. The heuristic power of this principle can be enhanced by adding the qualifications observed in Darwin's and Newton's inductions to the application of this principle: (a) the terminology in the two domains is the same, (b) there is reason to believe the two domains contain the same natural kinds or (c) both domains have some observed similarity.

These conditions can be construed as prima facie indications that the known and unknown domain constitute one reference class A.

In dealing with problems of how to conceptualize unobserved portions of A, Reichenbach's account, when extended beyond the logical empiricist restrictions mentioned above, would warrant trying concepts B known to apply to known portions of A. Concepts are tools of research in that they provide a means of classifying, relating and predicting that is essential to the conduct of scientific research, including the search for laws and the design of experiments and observations for applying and testing these laws. One's best bet in investigating an unknown portion of A is to try the concepts or laws that have proven successful in known parts of A. As far as we know, only concepts with a track record are likely to be successful in further applications, though of course we can be wrong in this assumption in any particular case. Aside from this consideration, there is no reason for choosing to investigate one or another of that indefinite number of actual or possible concepts with no such record. This is the epistemic warrant for trying these concepts. Concepts that depart radically from those known to be successful, that is, concepts that might be recommended by a Feyerabendian 'counterinductive' strategy can suffer two disadvantages: (i) the epistemic disadvantage of having no credible track record and for which there is no way of estimating their likely success, and (ii) if they are conceived indefinitely as just something other than established concepts they suffer the heuristic disadvantage of providing no definite guidelines for classification, and no relations to other domains that might be used as a source of 'auxiliary' principles for prediction and experimental design.

Reichenbach never specifies how large a sample A_n is required for confidence about unobserved portions. Presumably in problems of conceptualization this sample need not be very large, e.g. it may consist of the solution of a few, or maybe six or seven important problems, particularly if there is no similarly promising rival conceptualization B' available at the time. If we follow Laudan (1977) and emphasize the importance of problems as weighing in the assessment of evidence we can avoid assuming that enumerations of known instances only weigh in the epistemic promise of known instances. With this relaxation, the Reichenbachian procedure is no longer restricted to enumerative inductions, that is inductions whose reliability, accuracy and credibility are dependent on the size of the known sample.

Newton's 'Rules of Reasoning in Philosophy', particularly Rule III, include the expression of assumptions about the uniformity of the entire domain of material bodies, whether atomic, laboratory-sized or planetary or cosmic in magnitude, in respect to both their description in terms of particle kinematic language and their subsumption under laws holding macroscopically, such as the laws of motion and gravitation. In his application the observed sample A_n includes those bodies within the reach of our experiments, and observed attributes B, such as central gravitation apparent in terrestrial observations and experiments. Newton's views on the laws of motion and terrestrial gravitation are defended by reference to experiments with pendulums, inclined planes and projectiles performed by Galileo and Huygens. Thus he argues:

> Hitherto I have laid down such principles as have been received by mathematicians, and are confirmed by abundance of experiments. By the first two laws and the first two Corollaries, *Galileo* discovered that the descent of bodies varied as the square of the time ... and that the motion of projectiles was in the curve of a parabola; experience agreeing with both, unless so far as these motions are little retarded by the resistance of the air. When a body is falling, the uniform force of its gravity acting equally, impresses, in equal intervals of time, equal forces upon that body, and therefore generates equal velocities; and in the whole time impresses a whole force, and generates a whole velocity proportional to the time. And the spaces described in proportional times are as the product of the velocities and the times; that is, as the squares of the times. (1962, Vol. 1, Scholium to Law III. Also Vol. II, p. 408, contains references to Huygens' experiments with pendulums).

As Newton acknowledges, Galileo showed that the gravitational components, i.e. the vertical components, of projectile motions are such that the velocity is as the square of the distance. Newton notes that this principle is a consequence of the assumptions that uniformly accelerated velocity is proportional to elapsed time, i.e. $v_v=v_0+gt$, where 'g' denotes the uniform acceleration of gravity at the Earth's surface and v_0 is the initial velocity. he also adds that the distance covered by a moving object is, by kinematic definition, the product of its average velocity and the time of motion, i.e. $s=v_{avg}t$. Combining these propositions in view of the relation $v_{avg}=1/2(v_0+v_f)$ gives Galileo's squared time rule as a consequence when $v_0= 0$, the case of a body in free fall, i.e. $s=1/2gt^2$. However, also given the kinematic definition of uniform acceleration $v_f=gt$ for gravitational acceleration g from rest, if the distance is as the square of the elapsed time, $s=Kt^2$, it can be inferred that uniform acceleration is occurring provided $K=1/2g$. Newton's reasoning from Galileo's experiments is thus not just inferring a hypothesis from its consequences, but is, as he claims in various methodological remarks, a deduction from the phenomena. It is not a purely 'logical' deduction in the sense that it depends only on abstract *a priori* principles of some deductive logic, however. Rather, the deduction depends upon a conceptual system for describing particle motions, particle kinematics, which includes substantive assumptions about physical space and time and particle continuity.

This kind of argument does not in the least resemble Reichenbach's enumerations of instances in reference class A. Newton attaches no epistemic weight to the number of repetitions of Galileo's experiments. That is, repetitions are not performed to increase certainty, or to assure convergence toward unity of the frequency of association between distance and velocity squared, but are addressed only to the need to identify and abstract from the systematic effects of air resistance, and to control the effects of random experimental error, such as errors in timing or in measuring distances. If Newton's procedure can be described as 'inductive', it is not the enumerative procedure advocated by Reichenbach. But it does allow the extension of observed regularities to unobserved domains with the provisos mentioned above. Thus, there is a sense in which Newton is correct in including 'deduction from the phenomena' among 'inductive' procedures. The deductions support, without enumeration, premises concerning known portions of A from which conclusions are drawn about unknown portions.

Thus Newton's 'Rules' permit inferences to entities and causes that are, with then current technology and auxiliary theory, in principle unobservable. They permit and purport to warrant the introduction of descriptive and explanatory terminology, e.g. the kinematics of point-particles, and dynamic concepts such as central attractive and repulsive forces and gravitational forces into celestial and atomic domains which are out of the 'reach' of then available experiment. In this sense they may be called 'inductive', but this usage of the term should not entail enumeration. Newton's methods can also be regarded as conceptual heuristics, viz. procedures recommending that mechanical concepts borrowed from observable domains can be applied in research directed to conceptual problems raised by efforts to describe and explain behaviors of certain classes of unobservable material objects, events and causes.

6. *Analogical Methods*

William Whewell pointed out in reviewing Lyell's *Principles of Geology* in 1831 that causal hypotheses should not be restricted to *vera causae*, and that there should be no *a priori* assurrance that the same causes now operative always have been so in the past (Ruse, 1979, Chapter 3, Kavaloski, 1974). Also at the time he wrote this criticism there was some geological evidence, e.g. recent fossils high in the Alps, that suggested to geologists such as Adam Sedgewick that the Alps were raised by causes far more violent than those observably acting today.

This attack on an application of Newtonian inductivism points to possible limitations of *vera causa* strategies to domains throughout which certain kinds of uniformity can be expected or hoped for. Unobservable domains need not be describable in the same terms as those observed, much less contain events and processes subject to the same cause. That the procedures of Newton's Rule III can break down has long been known as fallacies of composition and division, where it is recognized that parts need not be described in the same terms as wholes and conversely. This breakdown is particularly evident in the history of physics in which it was eventually learned that macroscopic quantities, such as temperature and entropy, cannot be ascribed to all components, such as molecules and atoms, and that the mechanical quantities of location, momentum, energy and duration, which ideally have exact values in macroscopic application, do not have equally determinate values in all circumstances when applied to atomic and sub-atomic components. Similarly Newton's Rule II can fail when various causes can produce similar effects, as would be implicit in Newton's own acknowledgment of magnetic, electric as well as gravitational forces, all of which, by the second law of motion, can have the same mechanical effects, viz. centripetal acceleration.

One might seek a compromise between, on the one hand, Whewell's more permissive willingness to include as hypotheses non-natural catastrophic causes, particularly in cases where the effect is biological organization, and, on the other, Lyell's and Darwin's actualistic procedures. It might be hoped that such a compromise would retain some of the heuristic virtues of inductivism without the 'anarchy' of unrestricted hypothetical heuristics. The nineteenth century scientist and philosopher John Herschel's more liberal variant of the *vera causa* procedure may be just the compromise needed:

> If the analogy of two phenomena be very close and striking, while, at the same time, the cause of one is very obvious, it becomes scarcely possible to refuse to admit the action of an analogous cause in the other, though not so obvious in itself. (1830, 142).

"Analogy" is a rather elastic term, particularly if 'analogous' effects and causes are analyzed as happenings that are 'similar'. Presumably most happenings of interest to science, particularly to naturalists and biogeographers, have many attributes. Since it would be methodologically perverse to 'close the book' on the possible discovery of more such attributes, we can say that they are indefinite in number. Similar happenings are plausibly thought of as those that share some but not all of their attributes. Thus there are an indefinite number of ways in which any pair of happenings might be similar. It would then appear that the requirement that unobserved processes have properties 'analogous' to those that are observed leaves open an indefinite range of possible hypotheses. This lack of constraint on possible hypotheses is exactly the weakness attributed to the 'method of hypothesis' earlier in this essay. Without further constraint regarding preferred hypotheses or preferred relations of analogy it appears that hypothetical and analog methods are equally heuristically inefficacious, and thus neither is superior to the other as a 'logic' of discovery.

Herschel seems to be aware of this problem, as indicated in the following remarks:

> The classifications by which science is advanced, however, are widely different from those which serve as bases for artificial systems of nomenclature. They cross and

> intersect one another, as it were, in every possible way, and have for their very aim to interweave all the objects of nature in a close and compact web of mutual relations and dependence. As soon, then, as any resemblance or analogy, and point of agreement whatever, is perceived between any two or more things, - be they what they will, whether objects, or phenomena, or laws, - they immediately and *ipso facto* constitute themselves into a group or class, which may become enlarged to any extent by the accession of such new objects, phenomena or laws, agreeing in the same point, as may come to be subsequently ascertained. It is thus that the materials of the world become grouped in natural families, such as chemistry furnishes examples of, in its various groups of acids, alkalies, sulphurets, &c... (1830, 134).

Thus the 'particularly striking analogies' that Herschel highlights are those based upon shared properties that cluster the objects of investigation into 'natural classes', viz. classes defined by the common possession of several unchanging characteristics, such as biological species, or several characteristics that differ only in degree, such as newtonian material bodies, and classes distinguished by polar characteristics (1830, 135) as are acids and bases.

Qualitative likeness with difference only in degree is entirely compatible with Newton's 'like' causes and effects, for celestial bodies and atomic corpuscles both possess mechanical qualities but differ from ordinary bodies only in magnitude. Newton's constraints would also permit geological actualism without the unformitarian assumption.

Presumably for extending causal relations beyond observable domains, these preferred classes should be partially defined by characteristics with causal efficacy bearing upon interactions between objects within one class or between objects in different classes. Thus mass, location, electric and magnetic charge distinguish material bodies as natural classes because these quantities are also causally efficacious in explaining motions and trajectories. They are preferred or 'intelligible' attributes because in their terms laws of motion can be formulated. 'Like' or 'analogous' effects and causes thus can be those that are described in these same terms and by the same laws that vindicate the choice of these terms, as Newton suggests in Rule II and III. Thus 'intelligible' attributes, or attributes with causal efficacies, are among those that will define natural kinds likely to support inductive extensions of laws. The search for such attributes can be considered guided by a sub-heuristic to inductive heuristics prescribing extensions of laws across natural kinds. The former heuristic is useful in preparing for an application for the latter one.

Mary Hesse's 'Real analogies' and 'experimental identifications' (1974, p. 266) are another means of defining natural kinds. As a means to such identification one might seek to identify two apparently distinct properties or entities and to establish that they are interchangeable in different contexts, wherein the apparent differences emerge. Thus static and current electricity, electromotive force and static potential are respectively identified, and conduction currents and displacements are respectively effects of the application of this potential across respectively conductors and insulators. This project of identifying the two kinds of electricity and conducting investigations into the consequences of this identification was initiated by Michael Faraday in the 1820's.

This procedure for identification is based upon the general causal conception that one law, when applied to the same or similar entities under different circumstances will generate different effects, a conception exemplified in the well known application of Newton's laws to a projectile in various initial conditions to give parabolic, elliptical or hyperbolic trajectories.

The method of 'real analogy', however, is only one heuristic among several for generating 'experimental identification' or empirical grounds for extending natural classes. In some cases the apparent differences that distinguish classes mask underlying and more essential similarities not because one law is being applied in different circumstances but because inquirers may initially be unaware of common causal processes essential to the existence of the elements of the distinguished classes. Thus Darwin realized that the fundamental process for explaining the existence of any living organism, whether human, domestic or wild, is reproduction, whether sexual or a-sexual. Accordingly one should expect similar laws governing the production of humans, as well as wild and domestic animals and plants, regardless of apparently distinguishing 'moral', sensory and vegetative capabilities. Once this natural class, the class of sexual reproducing organisms, is identified even these distinguishing characteristics can be seen as the same kinds of characteristics, viz. adaptations or means by which their occurrence is preserved in the reproductive process.

In this case the strategy is to find one fundamental causal process among apparently diverse classes and to identify in these classes one natural kind on the assumption that causal processes are more important in picking natural classes than are outward appearances. The next step is to show that these different appearances contribute in the same way to the operation of the causal process by which the natural class was initially identified.

This identification then sets the stage for *vera causa* inductions. In Maxwell's case, an important inference was from laws governing magnetic interactions of conduction currents to laws for like interactions in displacement currents. In Darwin's case some of the several important inferences from domestics and humans to wild animals and plants have already been described. Also Herschel's suggestion that 'laws' can be compared and classified indicates that he might allow what Maxwell called 'physical analogies' in his inferences to *vera causae*. Physical analogies consist of "... that partial similarity between the laws of one science and those of another which makes each of them illustrate the other..." (quoted in Hesse, 1974, p. 261). They do not require that the phenomena compared be described in the same terms, as does Newton's 'likeness' of cause and effect. Coulomb's law of electric attraction is physically analogous to Newton's law of gravitation in its distribution of force through space, though it falls short of identity with gravity because of its origins in charges rather than masses and it includes the possibility of electric repulsion as well as attraction. Still Coulomb's law would have 'inductive' support in Herschel's methodology. Herschelian methods would also justify the suggestion that the electric stress in a medium should be conceived so as to be source free, as is magnetic field intensity. In this case distinct terms, magnetic field intensity and electric displacement, are given common spatiotemporal configurations, inferences are drawn to like laws governing the distribution of these fields.

These physical analogies are particularly heuristically powerful when there is a mathematical formalism and a set of mathematical techniques or heuristics for discovering consequences of particular distributions of quantities or for solving problems involving the application of physically analogous systems of description to particular cases. Accordingly, inverse square central forces, whether electrical or gravitational, produce parabolic, elliptical, or hyperbolic orbits under analogous initial conditions, a physical analogy exploited in the early stages of the development of the nuclear theory of the atom. Vector and scalar field theories have methods for dealing with fields originating and terminating in sources and sinks, for rotations or vortices, and for the propagation of waves, which were originally developed for the mechanics of fluids and theories of sound, but then were applied by Lord Kelvin and James Clerk Maxwell in drawing consequences of Faraday's laws for the behavior of electric and magnetic fields.

In sum, these analogical heuristics emerge from the examples just discussed:

1. Identities between entities or common membership in a natural kind can be established by these means:

a. Show that the entities have like properties that are, under a system of background beliefs B or a law-concept complex C, essential to their identification or causally linked to their occurrence or behavior.

Thus the velocity c and transverse vibrations are essential to the existence and propagation of light. Also electromagnetic vibrations are transverse and must be propagated at c in free space. It follows that probably light is an electromagnetic vibration. Similarly, by reference to reproduction, susceptibility to disease, ecological and behavioral similarities, Darwin identifies humans and non-humans as a common natural kind.

b. To strenghten these 'real analogies' show that apparent differences are either importantly the same kind of attribute common to all in a given natural kind or that they emerge as causal consequences of one or several things having different circumstances or different kinds of organizations.

Thus increased intelligence in humans is just greater adaptive ramifications of the perceptual powers, the instincts and the habits found in non human species. The magnetic effects of charges are the consequence of charges being in acceleration, and hence these effects emerge only in circumstances when static or moving charges are subject to acceleration.

2. Once 'real analogies', identity or common membership in a natural kind are established, inductive presumptions that laws manifest in one member can be extended to others on the grounds that common causally essential properties are good indications that common laws hold.

Laws governing electric and magnetic behavior in various materials should hold or have specific consequences for light. Laws governing humans should govern non-humans, and conversely.

3. Once common membership in a natural kind is established, concepts known applicable to some members may be extended to others, or other concepts may be found that can thus be extended.

Human families may fail to propagate because failure to find a mate, premature death from various causes such as accident or disease, or other causes of infertility. Conversely, attractiveness to the opposite sex, resistance to disease, and other causes of fertility can explain fecundity which in turn makes propagation more probable. The same causes of propagation or failure to do so can be extended to races and species, which are just larger and more distantly related families.

Magnetism is material bodies is construed as polarization of their small parts, as can be inferred by Newtonian division. These characteristics of magnetic substance are, by Faraday's conceptualization, equivalent to the concentration of lines representing magnetic field intensity and the initiation and termination of lines of magnetic force. The lines of magnetic field intensity represent magnetic conditions in matter and in free space, and thus one natural kind, a magnetic field, can exist with or without the presence of matter. In electrified bodies polarization also occurs between oppositely charged components of molecules. As in magnetic fields in free space, the presence and various directions of electric forces can be observed in free space. As in the case of magnetic dipoles, the presence of electric dipoles can be represented by lines of electric 'displacement' which never terminate but become concentrated in electrified bodies. Thus concepts initially applied to matter, viz polarity, are represented as concentrations of lines measuring the intensity of some agency so that the agencies they represent can be extended to free space, viz. as continuations of these non terminating lines. Laws governing the interactions between these agencies, as observed with polarizations and intensities in matter, may also be found to apply where these agencies exist outside of matter.

These analogical heuristics are attempts to give the similarities observed between two apparently distinct objects, populations, or systems sufficient import that there is some rational warrant for transferring concepts and laws known or observed in one to the other. The 'uniformity of nature' that the inductivist hopes for need not be the applicability of identical concepts *and* laws throughout the entire universe, as Newton hoped for the universality of mechanical concepts and laws. Though Maxwell held mechanistic hopes similar to Newton's, his and Faraday's procedures of conceptualization, e.g. Maxwell's 'method of physical analogy', did not restrict them to mechanical concepts for electric and magnetic fields. The distribution of these fields under various circumstances and their interactions in electromagnetic induction were observed and formulated in qualitative laws by Faraday without any essential speculative hypotheses as to their possible mechanical composition. Thus qualitative laws governing the distribution of magnetic fields about conduction currents as well as natural magnets, the induction of conduction currents from other such currents and from changing magnetic fields, and the distribution of static electric fields about various charged material bodies were drawn from experiment in Faraday's researches. The concepts used to express these laws, viz. force or flow intensities, their concentration or dilation in space, their sources or sinks or the absence thereof, and their rotations, are formed with spatiotemporal and dynamical configurations, and these configurations are borrowed from fluid kinematics. Fluid kinematics provided a spatiotemporal and dynamic framework within which to fit concepts sought for describing and explaining the distributions and interactions of forces, fields and currents observed in the electromagnetic experiments. The mathematical formulations of fluid kinematics already contained the conceptualization needed for quantitative expression of the elements of this framework, and thus provided both further constraints and positive procedures to aid Maxwell's search for quantitative electromagnetic laws. Specifically, Maxwell sought vector and scalar quantities that fit Faraday's field concepts and that could be dealt with by established mathematical procedures. Before its completion and subjection to further test, the credibility of Maxwell's project was based largely upon a hoped for links between the behavior of electric and magnetic phenomena within and outside of matter similar to the linkage between fluid statics and dynamics. Maxwell also hoped for uniformity in the sense that the same laws should govern the behavior of electromagnetic agents when acting within and outside of matter. The first uniformity need not have entailed an identity of mechanical and electromagnetic terminology, just an identity of part of this terminology, viz. the spatiotemporal distribution and behavior of entities and forces which are more specifically defined in different terms respectively for mechanical and electromagnetic applications. This identity is an instance of Maxwell's 'physical analogy'.

A procedure similar to Maxwell's was used repeatedly at the turn of the 20th century in attempts to theorize about atoms and radiation. Again, abstract principles such as Lagrangian and Hamiltonian energetic principles, principles of action, and the laws of thermodynamics were presumed to hold for atoms and radiation fields and descriptive terms were sought that would provide empirically satisfactory models of these entities and their interactions. In these developments there was a division among the prominent investigators in the terminology chosen for the models: Some, such as Heisenberg, initially preferred to compute the magnitudes or frequencies and the probabilities or intensities of changes in energies without constructing any mechanical models of the atom. Others, such as DeBroglie and Schrodinger, sought models in terms of wave mechanics, were the traditional mechanical concepts of location, energy and momentum were defined in terms of the wave concepts of superposition, interference, frequency and wavelength. These wave concepts shared at least some general dynamic and spatiotemporal features with mechanical and electromagnetic waves, though their substantive terms could not be identified with those in the classical waves because of their dimensionality and their use of imaginary variables. Still others, such as Bohr, thought that traditional mechanical terms could be applied only in special experimentally realizable circumstances and that both wave and particle terminology is required for models of atomic and radiation processes. Finally, Einstein defended the position that there should be no restriction on the kinds of terms used to describe radiation fields and atoms and that physicists should hope for deterministic laws in terms that can be radically different from those that describe macroscopic processes. Still his researches began with the extension of established principles, such as the conservation laws, Lorentz invariance, and general covariance, to new domains of inquiry.

Conclusion

In this essay traditional scientific methods were considered as heuristics, that is procedures with applicability limited to certain kinds of problems in science, where problems are defined in terms of initial epistemic conditions and unfilled epistemic objectives. (See Kleiner, 1985, for a theory of problems.) Heuristics reduce the available options for research in a field of inquiry making the search for the solution of a problem practicable in the sense that it can be done with deliberation in an appropriate time and with available conceptual, technical and human resources and an expenditure of these resources commensurate with the importance of the project. Heuristics for discovery in science should also have intrinsically the potential of producing conceptual or evidential novelty of the kind sought in a given problem. Finally, for problems raised in the effort to obtain knowledge of a subject, heuristics that provide initial evidence or initial steps in providing evidence for some conceptualization or proposition is useful for that end.

It was argued that the so-called 'hypothetico-deductive' method, though seemingly maximally promising for the production of conceptual novelty in the sense that it operates with no restrictions on concepts to be considered, balls short on providing practicable guidelines. In being totally permissive this method does not delimit prior conceptual possibilities or plausibilities sufficiently to be promising in guiding the solution of conceptual problems. On the other hand, inductive methods modeled on empiricist formulations of enumerative inductive procedures are relatively high in practicability, that is they give explicit instructions to accumulate empirical claims like those already bearing on the subject in question, or perhaps vary the circumstances in which these empirical claims are made, but they promise no conceptual novelty because their conclusions are expressed in the same terms as the relevant empirical premises. However, if the terminology appearing in the premises and conclusions of this procedure are not restricted to empiricist's 'observation language', the procedure can be used to reason from epistemic success of laws, terminologies and metatheoretical properties in one domain to initially credible applications of these items in domains newly under investigation.

The *vera causa* method restricts novel scientific hypotheses to applications of laws or terminology, or both, that are known in observable circumstances. Since the procedure does not require that the same terminology be used, it permits novel conceptualization but restricts it by allowing only concepts that satisfy known abstract laws, such as energy or momentum conservation and entropy increase. Accordingly, inductive methods can and did guide investigations in which classical mechanical models could be dispensed with, as in the production of some of Faraday's and Maxwell's laws for electricity and magnetism in the generation of quantum theories. Also, as in Newton's reasonings about celestial and atomic domains, the extension of a successful terminology to a new domain can be regarded as a promising conceptual heuristic under certain specifiable circumstances, e.g. when known and unknown domains form a common natural kind or when the terminology applies to parts as well as wholes. Still neither of these methods can be applied in domains in which both empirically established laws and terminology cannot be expected to be applicable. In these cases one may have to resort to more abstract similarities with established laws as constraints on plausible hypotheses, what Maxwell called 'physical analogies' between different laws, in the hope that known spatiotemporal or kinematic configurations can be extended to unknown domains and that mathematical heuristics for formulating and solving problems can be transferred from one domain to the other.

Reichenbach's 'vindication' of induction appeals to both it heuristic and its epistemic power. With no evidence to the contrary, the investigators best bet is to assume similarities between known and unknown parts of the domain of inquiry. These similarities provide positive guidelines for the investigator to direct his activities, something which is entirely lacking in Feyerabend's contrarian or 'counterinductive' heuristic. However, for heuristic import these similarities must be of certain kinds, e.g. likeness of terminology and law or membership in a common natural kind in Newton's and Darwin's case, likeness of law, or likeness spatiotemporal configuration in Maxwell's case and in the case of 20th century quantum mechanics, or likeness of metatheoretical characters in case of relativity theories and relativistic quantum theories.

Heuristics for identifying phenomena as belonging to a common natural kind, Hesse's 'experimental identities', can be regarded as a kind of sub-heuristic of inductive heuristics because inductive and analogical arguments gain epistemic strength if they are across established natural kinds. There are also sub-heuristics for defending these identifications, e.g. by citing common causally important properties and by arguing that differences are either accidental or are consequences of applying causal laws in different circumstances. Thus in some cases heuristics may be defended as necessary steps, or steps that enhance effectiveness in the application of heuristics already values.

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