Theory Pursuit: Between Discovery and Acceptance[1]

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1. Introduction

Scientists typically do something other than accept or reject their theories, they pursue them. Throughout the greater part of the nineteenth century numerous chemists devoted their research energy and resources to the development of Daltonian theory, declaring themselves willing to make use of the atomic theory in their research but reluctant or unwilling to accept it. When Frankland, for example, declared that he did not want to be considered a "blind believer" in the atomic theory and could not "accept it as true", but that he had been—and planned to continue—using it "as a useful ladder", he had arrived at a decision shared by many of his colleagues that while the theory was not acceptable, it was promising and worthy of pursuit.[1] And when Van't Hoff measured the kinetic-molecular theory by its fruits in the 1880's, he judged that it barely gave the then-current 4% interest rate, and so was an unpromising theory, unworthy of pursuit (van Nelsen 1960. p. 151). Scientific estimations of promise and lack of promise lead to scientific decisions to pursue or not to pursue theories. Yet if we appeal to traditional methodologies in philosophy of science we are hard-pressed to construe this behaviour as rational. Such methodologies have tended to focus exclusively on one modality of rational scientific appraisal—that of theory acceptance and to suggest that it is rational for scientists to work with only the theories they accept.[2]

Historical episodes such as these underscore the fact that enriching our appreciation of scientific appraisal is not only desirable, it is vital if we are to do justice to the complexities and subtleties which characterize the relationship between scientists and theories. Philosophical accounts of scientific rationality need to recognize a second cognitively legitimate modality of scientific appraisal—theory pursuit. And normative methodological proposals must be extended to the rationality of pursuit. But what is it to pursue a theory? What epistemic and pragmatic commitments distinguish theory pursuit from theory acceptance? And what happens when theories are pursued? Confining itself to questions such as these, the essay which follows is a foray into this 'nether region' (Laudan 1980, p. 174) between discovery and acceptance. It does not attempt however, to identify the indices of promise which must be consulted to determine whether or not a theory warrants pursuit. Clearly though, the task of formulating a criterion of theory promise is a crucial one.[3]

PSA 1990, Volume 1, pp. 467-483

2. Three Base Cases of Pursuit

We might begin by considering three base cases of actual theory pursuit: C.L. Berthollet's pursuit of affinity theory, and the initial pursuit of Daltonian atomism by J.J. Berzelius and W.H. Wollaston. In the first decade of the 19th century C.L. Berthollet conducted a critical—and ultimately devastating—review of the basis and results of established affinity theory. His work opened with a declaration of allegiance to the positive (Newtonian) heuristic which had guided 18th century affinitists, according to which the forces of affinity were regarded as analogous to gravitational force.[4] Yet his examination of the results of that theory, including the apparently irreconcilable inconsistencies of many chemical reactions with the rules of elective affinity, suggested that this positive heuristic had been played out. His work was clearly within the affinitist tradition, the aim of it being to show that the chemical action of bodies does not depend solely on their affinity, but also on their quantity (Berthollet 1801, p.138) and to replace Bergman's determinations of affinities with a better method. What he wanted to do was to reorient affinity theory.[5]

According to Dalton, showing the importance of determining atomic weights was the one great object of his *New System*. The most immediate empirical problem confronting the theory was the accurate determination of such weights, and the work of J.J. Berzelius was instrumental here. He considerably advanced experimental techniques and set new high standards of accuracy and comprehensiveness with his 1814 publication of an extensive set of atomic weights (Berzelius 1813-14). However, in that and the subsequent year he presented a detailed review of the theory's empirical and conceptual shortcomings. He stressed that it was not his intention to refute the atomic theory and that "it would be rash to conclude that we shall not be able hereafter to explain these apparent anomalies in a satisfactory manner" (Berzelius 1813-14, p.450). His intent was

> to lay open all the difficulties of that hypothesis that nothing might escape our attention calculated to throw light on the subject (Berzelius 1815, p. 127).

W.H. Wollaston's work on the oxalates constituted another valuable contribution to the early empirical problem-solving ability of Daltonian atomism. But in the same paper in which he published his results on the oxalates, he expressed the need for an atomic geometry (Wollaston 1802-08, p. 39)[6] By doing so, he was fingering an important inadequacy in Daltonian theory, one perceived by a number of his contemporaries. In order to arrive at a calculation of atomic weights from empirical data on combining weights, Dalton had proposed his rule of greatest simplicity, justifying it by appeal to the "mutual repulsion of atoms among themselves".[7] The resulting diverse geometrical configurations of atoms were criticized as fanciful since experiment could not determine whether a given compound were really 'binary' or 'ternary'—as Wollaston would point out later, (Wollaston 1814, p.7) having abandoned a year-long project to develop an atomic geometry and, with that project, any further serious pursuit of Daltonian atomism.

These three cases may take us an initial step toward answering some of the questions raised above concerning what transpires during theory pursuit. Clearly one of the things which happens to scientists is that their research and problem interests are shaped by the theories on which they are working. The theories in turn undergo development, both empirical and conceptual, in accordance with the research and problem interests of the scientists. So there are at least two activities in which scientists engaged in pursuit might be involved, or alternatively, at least two ways in which a theory may be affected by scientific pursuit: the theory's empirical abilities will be

https://doi.org/10.1086/psaprocbienmeetp.1990.1.192725 Published online by Cambridge University Press

probed, refined and extended, and efforts will be made to enhance its conceptual well-foundedness. We might pause at this point to see how our three base cases exemplify each of these aspects of theory pursuit before examining them at greater length.

Turning first to how scientists' problem interests are shaped by, as they serve to shape or develop, theories under pursuit, consider the work of Wollaston and Berzelius on early Daltonian atomism. Valuable as Dalton's theory was in explaining the weight relations in chemical combinations, his chemical atoms inspired a lengthy programme of weight determinations: they served, with the assistance of the balance and the improvement in analytical techniques, as readily quantifiable chemical units. As Sir Humphrey Davy, in his 1826 tribute to Dalton, observed:

> With respect to the weight or quantity in which the different elementary substances entered into union to form compounds, there was scarcely any distinct or accurate data. Persons whose names had high authority differed considerably in their statements of results, and statical chemistry, as it was taught in 1799, was obscure, vague and indefinite, not meriting the name of a science. To Mr. Dalton belongs the distinction of first unequivocally calling the attention of philosophers to this important subject ... thus making the statics of chemistry depend upon simple questions in subtraction and multiplication, and enabling the student to deduce an immense number of facts from a few well-authenticated, accurate, experimental results. (Davy 1840, Vol. VII, pp.94-95)

The extensive set of atomic weights produced by Berzelius' and Wollaston's early research on the oxalates, as well as the latter's year-long project to develop an atomic geometry, clearly indicate that the problem interests of these chemists not only had been shaped by, but contributed to, the development of Daltonian theory. Moreover, when they did abandon their pursuit of the theory, their problem interests were modified without being wholly abandoned. Wollaston would continue to study the proportion of elements in compounds and the ratios in which elements combined — producing a "synoptic scale of equivalents" which allowed chemists to draw upon the valuable empirical laws of definite, equivalent and multiple proportions without implying any commitment on their part to Dalton's mechanistic atomism. This also reflected the failure of his attempt to develop an atomic geometry which could improve upon Dalton's inadequate and empirically unsupported account of the simple geometrical and mechanistic considerations explaining why "atoms combine only in certain proportions". Berzelius too came to regard as inadequate the 'mechanism' of chemical combination proposed by Dalton. He thought it important to "combine researches respecting the cause why atoms combine with researches into the cause why they combine only in certain proportions" (Berzelius 1815, p.122). In the dualistic electrochemical theory of affinity which he developed, the role played by individual atoms was an important one. But the theory was an attempt to provide for what Dalton's theory had neglected, and what Berzelius had come to regard as the essential factor to be considered in the search for an explanation of chemical phenomena—the powers or forces associated with individual particles of matter.[8] Berzelius' problem interests had been modified along affinitist lines.

Turning next to how theories are affected by scientific pursuit and the related issue of what scientists do when engaged in pursuit, it is evident that the empirical abilities of Daltonian theory were probed, extended and refined by the accurate experimental research of both Wollaston and Berzelius. When the empirical abilities of a theory are developed in this way at least two things happen to the theory (this is even more apparent if we look not at the individual pursuits of Wollaston, Berzelius, and other chemists, but at the cumulative effects of such pursuits on the atomic theory over the

https://doi.org/10.1086/psaprocbienmeetp.1990.1.192725 Published online by Cambridge University Press

course of the century): new empirical challenges confront the theory, and its proper domain of application is itself carved out, explored and extended. Berzelius made an important first effort to extend the range of Daltonian theory by analyzing 13 organic compounds yet, as he noted, the success of the theory here was a qualified one—from the data he obtained he was forced to conclude that although formulae could be given for them in accordance with Dalton's theory, the law of definite proportions did not seem applicable. Berthollet's effort to reorient affinity theory by showing that "the combinations which are formed when forces are opposed, do not therefore depend on affinities alone, but upon the proportions of the substances which act" [17] was an effort to extend the problem-solving domain of affinity theory (though that extension was not successfully secured along these lines until the law of mass action was formulated after 1864). Berthollet's documentation of the inconsistencies between the rules of elective affinity and many chemical reactions demonstrated, however, the degree to which the positive heuristic that had been guiding affinitist research had been played out: it was no longer successfully securing empirical problem solutions or advancing the attempts of chemists to quantify their field along affinitist lines.

In each of these cases it is evident that when scientists pursue a theory they are generally careful to draw attention to its various empirical, as well as conceptual difficulties with the intent (to paraphrase Berzelius) not of refuting the theory, but of furthering work on it. Many of the conceptual problems faced by the atomic theory were openly addressed by Wollaston and Berzelius. Both chemists were acutely aware that the mechanistic atomism which Dalton had tendered was inadequate and unsatisfying as an account of the process of chemical combination, and this contributed to their decisions to abandon its pursuit. Both of them developed alternatives to the theory which permitted them to benefit from Dalton's valuable empirical laws without committing themselves to the conceptually problematic chemical atoms.[9] Berzelius would do this by expressing Dalton's laws in terms of 'volumes' rather than 'atoms', noting that "in the present state of our knowledge the theory of volumes has the advantage of being founded upon a well-constituted fact, while the other has only a supposition for its foundation" (Berzelius 1813-14, p.450) while Wollaston would confine himself to the analytical values, or 'equivalents' yielded by experiment.

With these three base cases as illustrative of scientific pursuit, we are now in a position to formulate some more general and detailed observations regarding our earlier queries. What commitments do scientists make when they engage in theory pursuit? What impact does such pursuit have upon scientific theories? And how are individual scientists, as well as the relevant scientific community, affected by theory pursuit? Following this we can then turn more directly to the question of the rationality of pursuit.

3. Epistemic and Pragmatic Commitments

There are both epistemic and pragmatic commitments required when one engages in theory pursuit, and they stand in contrast to those required by theory acceptance. Minimally, when scientists decide to pursue a theory they are deciding to work on it. The converse is also true: a decision to work on a theory constitutes a decision to pursue it. But neither of these is true of theory acceptance. A scientist's decision to accept a theory does not entail a decision to work on it, although the latter decision may also be made. Nor is it the case that a scientist who decides to work on a theory has also thereby decided to accept it. To work on a theory is to engage or to make use of it in some portion of one's research activities, whether these are conducted in laboratories, armchairs, or professional meetings. There are thus fairly substantial pragmatic commitments involved in theory pursuit, and in this, I would contend, pursuit differs markedly from theory acceptance.[10] The only pragmatic implications of theory accep-

https://doi.org/10.1086/psaprocbienmeetp.1990.1.192725 Published online by Cambridge University Press

tance are a readiness to defend the theory and a willingness, at least, to use it in research. In this respect, the acceptance of a scientific theory differs from that of a moral code. Strong pragmatic commitments follow from the latter: one must at least try to live by the moral code one finds most worthy of acceptance.[11] But one's research life need not be lived in accordance with the scientific theory one regards as most worthy of acceptance. The reason is that there may be other promising theories available, theories that deserve to be developed.

There are extensive commitments involved in theory acceptance, but they are epistemic ones, such as: the belief that the theory provides the best of all available explanations; the belief that it is empirically adequate, or the most effective problem-solver in some domain; and, if one is a realist, the belief that the theory is (approximately) true and that the theoretical entities posited by it exist. By contrast, the epistemic commitments of theory pursuit are minimal: epistemically, pursuit need involve no more than the belief that a theory is promising in some domain.[11]

The claims I have made regarding the epistemic commitments involved in theory acceptance are fairly standard, but those regarding the limited pragmatic implications of theory acceptance may be contended. (Certainly they are at odds with Van Fraassen's views, and possibly those of Laudan as well.) After all, if a scientist accepts some theory T, acknowledging that T provides the best explanation of, or has the highest degree of problem-solving effectiveness for, the phenomena in some domain, doesn't it seem odd, if not less than rational, for that individual not to make use of T to guide his or her research activities? I'll not attempt to mount a full defense of the claim I have made regarding theory acceptance here. This would be a lengthy endeavour and my principal concern at present is to develop an account of theory pursuit. But I can make a few remarks by way of response to these possible objections.

If it were the case that scientists, in accepting a theory T, thereby committed themselves "to the further confrontation of new phenomena within the framework of that theory" (van Fraassen 1980, p.88), then other promising theories would fail to be developed (or, at least, we would have to regard their development as less than rational). One does not have to have strong convictions about the importance of theory proliferation to scientific progress to find such a scenario disturbing. (Even Kuhn would now agree that rival theories play a role in promoting puzzles into the crisis causing anomalies that may end the hegemony of the accepted theory.)[13] If scientific progress has anything at all to do with the generation of better theories, and if we claim that when scientists accept a theory they must confront all new phenomena within the framework of that theory, then it would seem that once an acceptable theory in some domain had been found, 'progress' would be restricted to the kind of "mopping up" and puzzle-solving activities characteristic of Normal Science. The hegemony of T would be ensured, challengeable only by less than rational behaviour. We would, moreover, be forced to regard the behaviour of scientists throughout much, if not most, of the history of science, as less than rational.

But my claim regarding theory acceptance has been a somewhat stronger one. I have maintained not only that a scientist may accept a theory T without working exclusively on that theory, but that she or he may do so without working on it at all—without, that is, thereby being committed to using T to guide or structure any portion of his or her research activities. Perhaps the strongest reason for maintaining this is that there doesn't seem any reason for ruling it out as a rational research option. Provided that there are other promising theories available that deserve to be developed, why *should* it be a requirement of theory acceptance that scientists devote some of their research time, effort and money to the accepted theory? It seems desirable, and indeed quite likely, that some significant portion of the relevant community devote themselves to the accepted theory. But I am unable to see any compelling reason for requiring that individual scientists who accept T spend at least some of their limited time and resources working on it, when other promising theories are available and worthy of development. I have claimed that they should be ready to defend T and should be willing, at the least, to work on it, but I don't believe that the pragmatic implications of theory acceptance are—or need be—any stronger than this.

4. Empirical Refinement and Extension: The Role of the Positive Heuristic

Scientists engaged in theory pursuit, it was noted above, are preoccupied with the refinement and extension of a theory's empirical abilities and the enhancement of the conceptual resources which it provides for empirical problem- solving. Both of these activities deserve closer scrutiny. Certainly an important contribution which scientists typically make to developing theories is a frank acknowledgement of their conceptual shortcomings.[14] Serious attempts to improve a theory's conceptual well-foundedness follow once a pursuit decision has been reached. In each of the base cases sketched above this occurred, as it did with numerous other chemists throughout the 19th century. Efforts to enhance a theory's conceptual standing may take the form of explicating or finetuning its concepts, increasing its consilience, and appropriating the conceptual resources of theories in other domains. Since I have discussed these strategies elsewhere (Whitt 1989) I'll focus my attention here on the type of efforts which might be made to enhance the empirical abilities of a theory undergoing pursuit.

The refinement and extension of a theory's empirical abilities customarily proceed along lines suggested by the positive heuristic of the theory. The notion of a positive heuristic should be distinguished from that of a methodology. A particular methodology is supplied by, and is part of what characterizes, the research tradition in which a scientist works. It provides the scientist with general norms which guide and constrain research activities (including theory construction, theory testing, and experimental procedure) in a particular field. The methodology sets out broad directives, both prescriptive and proscriptive, specifying how a scientist *qua* scientist is to conduct research in a given field and what procedural necessary conditions must be met if research results are to be readily recognized as legitimate.

As Laudan has noted, the methodology of a particular research tradition will also serve, at least broadly and partially, to delimit the domain of application of that tradition's constituent theories. It does this by indicating what counts as legitimate empirical problems for theories within the research tradition. The inductivist and empiricist methodology of 19th century affinitist chemistry, for example, specified as legitimate empirical problems those concerned with the observable reactions of chemical reagents.

> Thus, to ask how this acid and this base react to form this salt is to pose an authentic problem. But to ask how atoms combine to form diatomic molecules cannot conceivably count as an empirical problem because the methodology of the research tradition denies the possibility of empirical knowledge of entities the size of atoms and molecules. (Laudan 1980, p.87)

Given the non-inductivist, non-empiricist methodology of 19th century atomic chemistry however, questions about the combining properties of certain entities not directly observable could be, and were, recognized as legitimate empirical problems.

Positive heuristics, by contrast, are supplied not by the research tradition itself, but by the specific theories which constitute it. Like a methodology, a positive heuristic

https://doi.org/10.1086/psaprocbienmeetp.1990.1.192725 Published online by Cambridge University Press

provides scientists with research directives. However, the research directives proffered by the positive heuristic are comparatively specific, focused on particular and limited areas of research. The positive heuristic singles out—from the set of methodologically-legitimated empirical problems addressable by the theory—certain problems or types of problems, targeting these as primary research problems and relegating the compliment of this subset to a lesser or secondary importance. A "research policy, or order of research, is set out" (Lakatos 1978, p.135) by the positive heuristic, enabling scientists to live with or ignore certain anomalies while setting a premium on the solution of others. It may also specify a means by which the primary research problems it targets are to be solved. Thus it guides or directs the research of scientists along specific lines, or towards a specific type of empirical problem, and indicates what resources might be brought to bear in refining and extending the theory's empirical abilities along these lines. The positive heuristic of Dalton's theory, for example, directed the research of chemists pursuing that theory towards the determination of atomic weights, and it specified the means available for arriving at such determinations, *viz*. Dalton's "rule of greatest simplicity".

The ways in which theories express their positive heuristics, or the forms which their positive heuristics take, may vary. Lakatos speaks of a positive heuristic as a model, simulating reality, which one knows is bound to be replaced with further development of the theory, but he also suggests that it can be formulated as a "metaphysical" principle, such as "the planets are essentially gravitating spinning-tops of roughly spherical shape" (Lakatos 1978, pp.135-6). Kuhn seems to be describing something very similar to this in his discussion of one of the components of the disciplinary matrix, i.e. the shared commitment to, or belief in, heuristic or ontological models such as: the molecules of a gas behave like tiny elastic billiard balls in random motion; heat is the kinetic energy of the constituent parts of bodies. He observes that all such models have similar functions:

> Among other things they supply the group with preferred or permissible analogies and metaphors. By doing so they help to determine what will be accepted as an explanation and as a puzzle-solution; conversely, they assist in the determination of the roster of unsolved puzzles and in the evaluation of the importance of each. (Kuhn 1970, p. 184)

The positive heuristic of a particular theory may be partially modified or replaced by subsequent theories in the research tradition. Partial modification might occur, as it did in the case of Dalton's theory, when the means specified for resolving the targeted, primary research problems are found wanting and ineffective, or objectionable in some way.[15] Replacement may be necessary when the positive heuristic has been played out or exhausted, when it has, as Lakatos puts it, 'run out of steam'. This will usually be signalled by the accumulation of anomalies among the primary research problems targeted by the positive heuristic. Berthollet's effort to re-orient affinity theory, for example, was a response to the build up of such anomalies in Bergman's theory of elective affinity.[16] A new positive heuristic may be introduced partly as the result of impressive developments in certain experimental techniques. An instance of this was the 'galvanization' of affinitist studies in the first decade of the 19th century by the voltaic pile: a new positive heuristic was introduced according to which the forces of affinity were regarded as analogous to those of electricity.[17] Later in the century, affinitist theories would acquire yet a different heuristic: with Berthelot's bomb calorimeter and with the improvement of measurement techniques in thermochemistry, thermal methods were used to solve problems of affinity.[18]

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It was noted above that when scientists pursue a theory they will also attempt to extend its empirical problem-solving abilities to new kinds of phenomena, to use it to explain new classes of facts, and if possible, to do this in a manner which increases the consilience of the theory. Clearly, one way this might occur is by departing from the policy or order of research specified by the positive heuristic. Wollaston's project to develop "a geometrical conception of the arrangement of the elementary particles in all the three dimensions of solid extension" (Wollaston 1802-8, p.39) might be regarded as such a departure, albeit a premature one. But it is important to realize that such extensions of the theory may be won through efforts to develop it along lines specified by the positive heuristic as well. This is in fact what occurred in the case of 19th century atomic theory. Efforts to arrive at an accurate determination of atomic weights were finally rewarded around 1869 with the adoption of Cannizzaro's method of calculating atomic weights, and a system of atomic weights based thereon. With this, agreement and consistency in the assignment of chemical formulae became possible and concerted attention could be focused on such phenomena as isomerism whose very recognition had been problematic as long as disagreement over chemical formulae was acute. Chemists were then able to devote themselves to questions concerning the relation of atoms and molecules, to use atomic theory to address structural problems and to develop accounts of chemical bonding.

5. Some Aspects of Theory Pursuit

The pursuit of divergent scientific theories within a given scientific community can have transformative influences which are marked and lasting. A particularly striking instance of this can be seen in the developments which took place within the chemical community during the 19th century. Theories in two different research traditions—affinitist and atomist, were actively pursued over the course of the century. It was quite possible to do chemistry at the beginning of the century without incurring the ontological and methodological commitments of Daltonian atomism. And, although the community had been transformed during this time from a small community of generalists many of whom could do competent work in any area of the science to a large community of specialists, it was still possible to engage in chemical research at the end of the century without incurring the ontological and methodological commitments of Latter-Day atomism. Failure to recognize the simultaneous pursuit of theories in divergent chemical research traditions has contributed to the construal of this century as one of atomists vs. a motley crew of skeptical detractors and nay-sayers. The fact that some, if not most, of this 'opposition' had other commitments and problem-interests has tended to be overlooked.

One result of the pursuit of both atomic and affinitist theories was the development of two different approaches to chemical problem-solving — molar and particulate.[19] The former, unlike the latter, is consistent with both a continuous and a discrete view of matter. Which approach it is reasonable to use will be influenced by the kind of chemistry (physical or organic) one is doing and by the aspect of chemical processes being addressed (chemical reactions undergoing change in form and distribution of energy, or undergoing change in form or distribution of matter (Schelar 1966, p.123)). Similarly, early in the century, whether it was reasonable to pursue affinitist rather than Daltonian theory turned in part upon one's problem interests. If one were interested in studying the forces involved in chemical reactions it would not have been reasonable to guide and direct one's research activities in accordance with Daltonian theory. However, if one were interested in studying combining weights, the proportions of elements in compounds and the ratios in which elements combined, it would have been reasonable (though it would not have been necessary) to guide one's research in accordance with Daltonian atomism.

A number of chemists throughout the century contributed to theories in both research traditions. The case of Berzelius is instructive as an example of how the research or problem interests of scientists figure in pursuit decisions and of how, by pursuing these interests, the domain of proper application of theories may be carved out.

For a good part of the 19th century, the controversy in chemistry was over the issue of appropriate problem-solving domains, over whether theories of chemistry were primarily concerned with chemical processes and the forces involved in them or with the determination of atomic weights and the arrangement of individual particles of matter.[20] While Berzelius' own electrochemical theory of affinity granted a considerable role to individual atoms, that theory clearly lay within the affinitist research tradition. What Berzelius (and chemists like him) found disturbing about Daltonian theory was not Dalton's assertion of the existence of atoms, but what he had to say about their nature and centrality in chemical research.

Berzelius' first contributions to chemistry were in the area of galvanic research. His early studies of the effects of electricity in organic and inorganic nature led him to conduct an investigation of the nature of ammonia amalgam. Combining proportions proved useful as analytical tools in this investigation, and it was this which first aroused his interest in Dalton's *New System.* Yet, after contributing work which provided important, if qualified, empirical support for Daltonian theory (Berzelius 1813-14), Berzelius abandoned it— although he would continue to make use of atomistic concepts in his electrochemical theory. Central among his reasons for doing so was the failure of Daltonian theory to address problems of affinity and the inadequacy of its account of the simple mechanical and geometrical considerations governing chemical combination. The theory failed to address what he regarded as essential,

> being occupied with a part of the phenomena, (when it) ought to embrace the whole ... when we treat atoms in a chemical theory, we ought to endeavour to combine researches respecting the cause why atoms combine with research into the cause why they combine only in certain proportions. (Berzelius 1815, p.122)

For Berzelius, "the ideas on the relation of the atoms to their electrochemical properties ... constitute(d) an essential part of" chemical theory:

> The different relations of bodies to electricity will henceforth be the basis of all chemical systems... (This problem) will soon become the general object of our researches, and gives us reason to hope for a new dawn in chemical theory. (cited in Levere 1971, p. 143)

Electricity was the key to chemical affinity, and experiment had shown that electricity seemed to obey the same quantitative laws as ponderable matter in chemical combinations. Berzelius presented a detailed argument demonstrating that the law of definite combining proportions was compatible with important earlier work in the affinitist tradition — specifically with Berthollet's mass law. Berzelius had hoped to secure a consilient explanation of these laws, but his model of chemical combination was unable to do more than assume them successively (Levere 1971, p.146). Berzelius' own electrochemical theory did offer a considerably more detailed account of chemical affinity than had previous affinitist theories. Perhaps too much so, for the degree of specification which it provided of the electrical mode of chemical combination was such as to make later modification difficult (Levere 1971, pp.166-7). The stress which his theory placed upon the role of electrically polarized atoms resulted from his attempt to extend the laws of combining proportions to the realm of organic chemistry. The complex chemical constitution of organic compounds posed particularly serious difficulties for Dalton's account of chemical combination. On Berzelius' theory, it was the electrical natures of individual atoms, rather than their arrangement, which accounted for molecular properties.

The controversy between Berzelius and Dalton over the appropriate problem-solving domains for chemical theory would continue throughout much of the century. Attenuated by the fact that, in the first half of the century, the chemical community was still small and composed of generalists, it would contribute to the subsequent diversification of that community. Latter-Day affinitists were primarily inorganic and physical chemists who, in their explanations of chemical phenomena, gave principal emphasis to the role of forces in chemical reactions. To address these research interests, they developed a molar approach to chemical problem-solving. Organic chemists, pursuing Latter-Day atomic theories, gave principal emphasis to the spatial arrangement of individual particles of matter in explaining chemical phenomena and employed a particulate approach to chemical problem-solving to further these research interests. In this way, over the course of the century, the differing research or problem interests of chemists eventually led to the development of two different approaches to chemical problem-solving.

Moreover, as a result of their pursuit of theories in both research traditions, the domains of proper application of atomic and affinitist theories were slowly carved out. The particulate approach of Latter-Day atomism was able to provide an account of chemical processes as involving changes in form or distribution of matter and of the arrangement of atoms and molecules in three dimensional space. This was especially valuable in elaborating and resolving structural problems such as isomerism. The molar approach of Latter-Day affinitism was able to provide an account of chemical processes as involving changes in form and distribution of energy. This was of particular value in investigating chemical equilibria and allowed chemists to address the observed behaviour of large quantities of substances independent of any reference to the atomic or molecular constitution of the substances (Schelar 1966).

The foregoing observations regarding how the research interests of scientists figure in their pursuit decisions, and of the way in which the appropriate problem-solving domains of theories are delineated as a result of pursuit, may help us to address more profitably the question of the rationality of theory pursuit.

6. Rationality and the Contextual Differentiation of a Scientific Community

We have already seen that a pursuit decision involves the scientist in certain pragmatic and epistemic commitments. Scientists guide their research activities in accordance with a particular theory in the belief that that theory is promising with respect to a certain set of problem interests which they bring to the theory and which figure in their decisions to pursue one theory rather than some other. It is important to realize that pursuit differs from acceptance in the following respect. Since acceptance involves belief that the theory provides the best explanation, and since only one among the theories in contention can be accepted as best, appraisal of theory acceptability must be comparative. Should there be a tie, we can say only that two of the competing theories are equally worthy of acceptance: we cannot rationally accept them both. However, since pursuit involves belief that a theory is promising (in the domain or problem-relative way indicated above) and since several competing theories may be able to demonstrate promise in this respect, appraisal of theory pursuitability need not be comparative; a theory may be determined to be promising irrespective of how it fares against the competition. Should there be a tie, we can say that two theories are equally promising, and so equally worthy of pursuit. We can, moreover, rationally pursue them both.

There is, however, a troublesome ambiguity in the preceeding paragraph. "We" may be understood collectively, as referring to what it is rational for a particular community of scientists to do. Alternatively, it may be a kind of royal "we", to be understood individually, as referring to what it is rational for an individual scientist to do. The ambiguity is interesting because it suggests the need for a distinction between individual and community rationality.[21] To see this need, consider what happens when we introduce the distinction. If we are dealing with the rationality of acceptance, community rationality can be seen to distribute over individual rationality. That is, if it is rational for a given scientific community to accept T because it provides the best of all available explanations, then it is also rational for each individual scientist to accept T as the best explanation. Community rationality does not distribute over individual rationality though, when it comes to the rationality of pursuit. If it is rational for a given scientific community to pursue several theories T1...Tn because they are promising, it does not follow that it is rational for each individual scientist to pursue several theories T1...Tn. (Or at least it does not follow if "rational" is understood here as a requirement rather than a permission. And it is understood as a requirement in the case of acceptance; if T is worthy of acceptance, then an individual scientist would be behaving irrationally by rejecting it.) If several different theories are worthy of pursuit in a given community, we do not want to make it a requirement that each member of the community pursue several different theories; a scientist would not be behaving irrationally by pursuing only T1.

This, then, is another point of contrast between theory pursuit and theory acceptance. In the case of theory acceptance, what it is rational for the community to do (e.g. accept T) is also rational for the individual scientist to do, and to be rational the scientist must accept T. But in the case of theory pursuit, it does not follow from the fact that it is rational for the community to pursue several theories, that an individual scientist must, to be rational, pursue several theories. Having introduced the distinction between individual and community rationality, we might return to the earlier difference noted between appraisals of pursuitability and of acceptability (namely, that the former, unlike the latter, need not be comparative), and consider whether the claim here needs to be qualified in light of this distinction: are comparative assessments of the promise of theories required at either the individual or the community level? Once again, there seems to be no reason to require comparative assessments at the level of the community. To simplify matters, suppose a given community C at tn is faced with 7 candidate theories, some of which are competitors in the same problem domain, some of which are not. The question is, which of these theories are promising and so, worthy of pursuit? To arrive at such a determination, some criterion of theory promise, or fertility, will need to be applied to each. This will winnow out some of them — those unable to establish clear claim to being promising as problem-solvers in their respective domains. Suppose that leaves us with a set T of 4 theories, only two of which (T1 and T2) are competing in the same problem domain. This then is the set of theories which it is rational for C to pursue at tn. Since community rationality does not distribute over individual rationality, what this means presumably, is that an individual scientist S who is a member of C must, to be rational, pursue only theories which are elements of T and not pursue any of those which have been winnowed out as unpromising.

But which theory (or theories) ought S to pursue? What makes it rational for S to pursue one rather than another of the promising alternatives? Clearly here a comparative assessment seems required. And, I think, we can provide it in light of the discussion earlier regarding how the problem or research interests of scientists figure in their pursuit decisions. What makes it rational for different members of a scientific community to invest their time and resources in the pursuit of one theory (or of certain theories) rather than another are the differing problem or research interests which they bring to their work. These differences among individual members of C may arise from a number of factors: the training they have undergone; the previous work they have done (including the types of problems on which they focused, who they worked with, the experimental techniques in which they have become skilled); and the problem contexts in which they are currently immersed as a result of their most recent research. The contextual differentiation of a scientific community is the result of such factors. They were, in the case of the 19th century chemical community, largely responsible for the transformation of that community from a small group of generalists many of whom were able to do competent work in different areas of chemistry to a diverse community of specialists many of whom confined their research efforts to specific areas (e.g. to organic, rather than physical, chemistry).

Keeping in mind the relevance to pursuit decisions of the problem interests which scientists bring to their work, we can return to S who must reach a decision about which of the 4 promising theories in T to pursue. We have said that only two of these theories are competitors, that is, they are both promising in the same domain, or with respect to the same problems. Suppose further that these are the problems which interest S, those which, as the result of the various factors mentioned above (her training, previous work, current research), she is most concerned to address and hopes to resolve. Clearly S could rationally defend her decision not to pursue T3 and T4, by acknowledging that while those theories are promising in their respective domains, the problems in those domains are not among her primary problem interests. Perhaps S is a late 19th century organic chemist, and T1 and T2 are theories in the atomic research tradition, while T3 and T4 are theories in the affinitist tradition. Unlike her contemporary, Van't Hoff, her problem interests are fairly narrow, structural ones—confined, say, to problems of geometric isomerism. Given those problem interests, she has rationally defended her decision not to pursue T3 and T4, for, while the latter may be promising theories, they are not promising in the domain of problems of most interest to her. And it would not be rational for her to use those theories to pursue her problem interests.

This leaves S with T1 and T2, both of which are promising in the domain of problems on which she has focused. Is a comparative assessment of T1 and T2 needed at this point? Must S, to be rational, pursue only one — the more promising — of the two? There seems to be no reason to require such a comparative assessment, to insist that she behaves rationally only if, after appraising their fertility, she works to develop the more promising of the two. If both theories have been able to establish themselves as promising problem-solvers in the domain of problems which concerns her, then she may be well-advised to pursue them both. With further development, the theory which was at tn, the less promising of the two, may prove itself after all the more effective problem-solver in that domain. If we are to give theories, especially new theories, a chance to develop and to prove their empirical problem-solving abilities we cannot insist that there is, at tn, only one theory which it is rational for S to pursue (the one which has demonstrated the most promise, or which "has a higher rate of progress than its rivals" (Laudan 1977, p.111)).

It is important to realize though, that while S is not rationally required to choose, say, T1 rather than T2, she could rationally defend such a choice. She may wish to concentrate all of her research efforts on one theory, and might defend her choice by arguing that, upon appraisal, T1 has proven itself the more fertile, hence more promising, theory. To complicate matters for her here, we might give her a colleague C who shares the same problem interests as S and who similarly wishes to concentrate his research efforts on one theory. C, however, having consulted the various indices of promise and comparatively appraised the two theories, has determined that T2 is the more promising theory. Thus he will defend his decision to pursue T2 by arguing that

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it is the more fertile or promising theory of the two with respect to his set of problem interests. S and C then, are both able to defend their pursuit choices but they disagree as to which is the more promising theory.

I have introduced this complication to make a final point regarding the rationality of pursuit. Before making it let me summarize the discussion so far. I have argued that the contextual differentiation of a scientific community is the result of differences in research orientation and problem interests which are to be found among the individual members of that community, and which arise from a variety of factors that permit and encourage the specialization of research efforts. At any given time it is likely that there are a number of different theories which it is rational for such a community to pursue and comparative appraisals of theory promise are not required to determine these. However, since some of these theories may be promising in different domains or with respect to different sets of problems, and since the problem interests and research orientation of scientists differ, some comparative assessment is required by an individual scientist who must select among the theories that are promising in the community those which, given her problem interests, it is rational for her to pursue. Once these have been determined, no further comparative assessments of promise are required of the scientist. However, while rationality does not require that she make a further comparative assessment and pursue only the most promising of the competing theories, it certainly permits her to do so. And she may rationally defend her choice by arguing that it is the most fertile, hence most promising theory. The final point I have wanted to make here, by introducing a colleague who challenges her choice, is that there is room for rational disagreement among scientists as to which theory is most promising in some domain. To paraphrase Kuhn (1977, p.324), when scientists do decide to pursue, from among several competing theories, only that theory which is most promising, they may nevertheless reach different conclusions even though they are fully committed to the same criterion of theory promise, or fertility.

The reason for this, I suggest, is due to individual differences in the weighting of the various indices of promise which themselves arise from the type of differentiating factors noted above (training, problem-concentration, previous work, etc.). As a result, one scientist may place considerable weight on the ability the theory has shown to clarify its concepts. Another may weight more heavily the kind of explanation which the theory affords. Another may stress its positive heuristic, or its dynamic consilience, and so on. That scientists committed to the same criterion may rationally disagree as to which of the competing theories being appraised is most promising, is a welcome consequence. Such comparative assessments need not be made, but they often are made: scientists often do decide to concentrate their research efforts on developing the theory they have determined to be the most promising. The different conclusions they may reach as a result of according different weights to the various indices of promise helps to insure that a number of different promising theories will be further developed and be given a chance to demonstrate the nature and extent of their empirical abilities.

Notes

[1]See B. Brodie (1868-69, p.435). Other such 19th century chemists include W.H. Wollaston and J. Berzelius, whose work is described below. A more extensive account of these developments can be found in (Whitt 1990).

[2]Indeed, since most theories require considerable development before they can be regarded as candidates for acceptance, we will be forced to conclude that much of the

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history of science is irrational should we suppose that theory acceptance exhausts scientific rationality. Larry Laudan was the first to stress the need for enriching our accounts of theory appraisal along these lines. See especially his (1977, pp. 108-114) and (1980, pp.173-83).

[3]Some initial discussion of part of what might figure in such a criterion can be found in Whitt (1989).

[4]As he stated:

Les puissances qui produisent les phénoménes chimiques sont toutes dérivées de l'attraction mutuelle des molécules des corps à laquelle on a donné le nom d'affinité, pour la distinguer de l'attraction astronomique. Il est probable que l'une et l'autre ne sont qu'une même propriété. (Berthollet 1803, p.1)

[5]He attempted to effect such a re-orientation by supplying affintity theory with a new positive heuristic which directed chemists' attention to the importance of considering not only the affinities, but the relative quantities of the reacting substances, as well as the properties which influenced the direction of a reaction. According to the positive heuristic provided by Berthollet's theory, the forces of chemical affinity were analogous to gravitational forces in that they were proportional to the relative masses of the reacting bodies.

[6]This constitutes Wollaston's so-called 'prophesy', in which he observed:

I am further inclined to think, that when our views are sufficiently extended to enable us to reason with precision concerning the properties of elementary atoms, we shall find the arithemetical relation alone will not be sufficient to explain their mutual action, and... be obliged to acquire a geometrical conception of their relative arrangement in all the three dimensions of solid extension.(Wollaston 1893, p.39).

[7]The 'rule of simplicity' is set out in (Dalton 1808, p.214). The justification for it in terms of mutual repulsion of atoms was offered in (Dalton 1811, p. 147).

[8]For further discussion of Berzelius' response to Dalton's theory, see (Melhado 1981, p. 214).

[9]The central concept of Dalton's theory suffered from ambiguity, as well as internal and external inconsistencies. Dalton had used the term 'atom' ambiguously to refer to the smallest particle of both elements and compounds. Since the latter particles were divisible and structurally arranged and since Dalton held that there was a one-to-one equation between the chemical elements and physical atoms (which were solid, indivisible and unstructured particles), the theory was internally inconsistent. It also endorsed a theory of matter blatantly at odds with the prevailing Newtonian physics of the time; ignored the century-long preoccupation of chemical orthodoxy with the concept of affinity together with that tradition's methodological proscriptions; and violated the definition of 'element' advanced some twenty years earlier by the eminent chemist Lavoisier. For more on this issue see Whitt (1990).

[10]Bas van Fraassen has argued otherwise, with regard to theory acceptance. While he maintains that the only belief involved in acceptance is the belief that the theory is empirically adequate, he stresses that more than belief is involved:

> To accept a theory is to make a commitment, a commitment to the further confrontation of new phenomena within the framework of that theory, a commitment to a research programme, and a wager that all relevant phenomena can be accounted for without giving up that theory. (van Fraassen 1980, p.88)

I offer some reasons for rejecting such a strong version of the pragmatic dimensions of theory acceptance below.

[11]As has been argued, *contra* Gregory Trianosky, in Whitt (1984).

[12]The matter of how one rationally arrives at and defends one's belief that a theory is promising clearly needs careful attention.

[13]John Nicholas has discussed this in his "Puzzles, Anomalies and Scientific Crisis" (manuscript).

[14]We have seen this in the cases of Berthollet, Berzelius and Wollaston discussed above. Since a theory's conceptual well-foundedness or viability is one of several indices which must be consulted in order to assess its promise, such an acknowledgement of a theory's problematic conceptual standing may of course figure in an argument against pursuit of the theory.

[15]A number of chemists—including Wollaston, Berzelius, Gay-Lusaac and Berthollet—were highly critical of the use of Dalton's 'rule of greatest simplicity' as a means for determining atomic weights. Since there was no means of estimating the numbers of atoms which combined to form compounds, Dalton had simply assumed that such combination would always be of the simplest type. They criticized this as conjectural, not fully warranted by the facts. Berzelius would make use of two other methods for determining atomic weights (via Gay- Lusaac's empirical law of combining volumes and the Dulong-Petit law) neither of which relied on the rule of greatest simplicity. For a discussion of this see (Gardener 1979).

[16]According to the positive heuristic of Bergman's elective affinity theory, the forces of affinity were analogous to gravitational forces in that the forces of attraction between minute particles of bodies varied according to their distance apart. But they were disanalogous in that affinity was independent of the masses of reacting substances. Thus chemists, in arriving at determinations of relative affinities, were directed to discount as relevant the quantities of the reacting substances, and to attend solely to the relative intensities of the affinities of substances. Elective affinity was, then, a constant, invariable force which alone determined the direction of a chemical reaction. Berthollet attempted to re-orient affinity theory by supplying it with a new heuristic which directed chemists' attention to the importance of considering not only the affinities, but the relative quantities of the reacting substances, as well as the properties which influenced the direction of a reaction. According to the positive heuristic of Berthollet's theory, the forces of chemical affinity were analogous to gravitational forces in that they were proportional to the relative masses of the reacting bodies. In this way Berthollet was able to explain a number of reactions of alkalies and alkaline earths with acids which were anomalous under Bergman's theory.

[17]As Sir Humphrey Davy noted, the Voltaic battery was:

> an alarm bell to the slumbering energies of experimenters in every part of Europe, and it served no less for demonstrating new properties of Electricity and for establishing the laws of this Science, than as an instrument of discovery in

other branches of Knowledge; exhibiting relations between subjects before apparently without connection and serving as a bond of unity between chemical and physical philosophy.(Davy 1840, Vol.VIII, p.271)

[18]For further consideration of this see (Schelar 1966).

[19]See Schelar's excellent discussion (*ibid.*). She does not use this terminology, however, nor is she addressing the same issues under consideration here.

[20]See (Levere 1971).

[21]For a discussion of individual vs. group rationality, see (Sarkar 1982).

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