PROCRUSTEAN BEDS OF SCIENTIFIC STYLE*

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The growth of knowledge depends in part on our scientific institutions. One aspect of these institutions is the stylistic standards they require. These standards are not discussed: they are now deemed a successfully transparent medium through which knowledge is conveyed. Furthermore scientists are uncritical of their own institutions and few intellectuals of any stripe consider alternatives. The sociologists of science focus their attention on measurement and explanation of success. Since style is not measurable and its virtue has already been explained, it is of no further interest to them. Some interest has occurred recently, but primary concern seems to be with technical matters such as print face and does not often touch the relevancy for scientific theory of the choice of style. In the social sciences style has posed a serious problem but they have hoped merely to imitate physical sciences.

One consequence of these various approaches and attitudes is to ignore entirely an important problem. This problem is: how do the canons of scientific style help and/or hinder the growth of knowledge? This problem only arises if one supposes at least provisionally that there is some reason to suppose that the efficacy of various styles to help produce scientific growth varies and that there are differing choices available to us now. If there is some possibility of improving the growth of knowledge by improving the canons of scientific style, then we have a potentially interesting problem. Or, one can approach it the other way, if one supposes that there are institutional barriers to the growth of knowledge, style is one candidate for the location of such a barrier.

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My own interest in the problem of this paper arose in this latter way. My impression is that unwarranted discrimination and narrow-mindedness typify the scientific community. These defects occur due to rigid enforcement of fashion, strict limits on debate, and suppression of dissident opinion. The methods of doing so are to limit access to journals, especially prestigious ones, and to limit the career opportunities of those who thereby do not publish properly. Even if they overcome the initial barriers, further difficulties may be produced. The most damaging effect of such institutions is to dampen the hopes, spirits and ambitions of anyone who would dare to break the rules. Even though rule breakers may sometimes even be highly rewarded, if the risks and difficulties are great enough, then the effect such adventurers may have will be minimal: even if they change the reigning fashion, they do not thereby change the fact that fashions reign.

This bleak view is only a highly personal though not unique impression. It is quite difficult to render such impressions more precise or to test them against current practices in scientific communities. This is so because a precondition for finding unwarranted discrimination is finding an agreed criterion of what choice will count as warranted. But if we disagree here, as we very likely will, then we cannot decide empirically whether such discrimination exists; we can only show that we disagree with what the scientific community deems to be warranted discrimination.

A different approach to the possible inhibition of growth and discrimination may lead to a more fruitful discussion, however. Suppose we seek to test the hypothesis that discrimination and unnecessary limits exist by developing explanations of how this occurs. Such explanations may then be tested. These tests would not settle the question of the degree of discrimination, if any, but would provide testable theories of how to improve scientific discussion. In short, we may seek theories of how to improve without a theory of the current scope of discrimination.

A study of scientific style and how it may inhibit scientific growth and free discussion provides an opportunity for a discussion of how improvement may be possible. We may evaluate the appropriateness of scientific style for increasing the rate of the growth of scientific knowledge. If we find obstacles we may seek to remove them. We might then even test whether the removal of putative causes of discrimination, by itself, served to improve matters. This may of course not be feasible: social experiments in the community of science are rare.

In order to carry through such a program I will here suggest an explanation of how current standards of style limit the type of

research that may be readily published and, as a consequence and more generally, the growth of knowledge.

My primary aim is to present a plausible conjecture, the trouble spots which this conjecture reveals and how they may be avoided. I will present the conjecture concerning existing canons of style in four ways in the first four sections of my essay. First, I will explain how they—the canons—inhibit growth (by imposing conformity with existing doctrine); second how they have come to be established and maintained (by the establishment of a mistaken view of science and adoption of a new still mistaken view); third, why social science endorsed them (to gain prestige) and the defect this shows (frameworks are immunized from criticism); fourth, the now current, revised and still mistaken (Establishment) ideology (elitism in science) which is used to justify current practice (by exhibiting the success of the system). In the fifth section I will discuss the need for some institutional change. I will provide examples of how improved theories of rationality require such change. Section Six will present very briefly explanations of why the foundation of contemporary style, the attempt to fix frameworks must fail to produce either clarity or even limited rationality; I will offer explanations from different points of view (methodology, sociology, psychology). In Section Seven I use explanations from the same points of view to make a case for more liberal and pluralistic standards of style reflecting a more open scientific society. The problem of scientific style is thereby presented as that of a choice between an open and a closed scientific society.

1. How Style Inhibits Growth

Established scientific style retains the old ideal of a Cartesian science. This ideal is a complete science stated in a single axiomatic framework. From certain axioms it has been hoped that we could deduce true explanations of all phenomena. Scientific style is adopted so as to communicate the closest approximations to this ideal that we have and to do so as such approximations. One way to do this is to present all theories as if they were mathematical or logical systems. Newton and Spinoza each provide models since they each attempt to present systems of the world which begin from broad principles and proceed to deduce detailed explanations of various phenomena.

But this will not do as it stands. Each writer cannot present his own system; science would disintegrate. Yet each writer cannot be completely bound by existing theory as science would stagnate. In order to resolve this dilemma posed by the conflicting ideals of system and growth scientific writing needs to modify the axiomatic

format to allow additions. New deductions from established principles do not pose any serious stylistic problems. But these are not sufficient. New theories not deducible from known principles are needed to achieve growth. These new theories cannot be too diverse however or the approximation of scientific theories to an axiomatic system would break down.

The standard solution to this problem is to require that new additions to scientific knowledge be set in some already existing framework. Any advance should constitute an improvement of the system of science. This can be done by simplifying an already existing system, by making an established system more precise or, most typically, by creating new modifications of some existing system which increases its scope. This latter task is most commonly achieved either by deducing new experimental results and/or by creating bridge principles or middle range theories between a broad framework and experimental results.

These types of accomplishments are often valuable. The defect of the style which science uses to present them however is that it limits scientific discussion to these tasks. There are at least two useful tasks which are not allowed for by the standard view of scientific advance and style. The inhibited tasks are critical discussion of existing systems and additions or new conjectures which do not fit established theory. Neither are completely prohibited and both occur, sometimes in obvious cases with stunning success. Yet stylistic standards tend to discourage and prohibit such work. This occurs because scientific style requires brevity, a standard way of setting up one's problem, standard vocabularies, and standard ways of presenting results. All of these various requirements impose limitations: they each presuppose that additions are intended to fit into and add to existing frameworks. Brevity is demanded only because the background to any research is presumed to be common knowledge. It needs no review, interpretation or specification. Problems must be set up in such a format by providing cryptic statements of problems which presume common background knowledge. Likewise standard vocabularies seek to put new results in existing molds and standard ways of presenting results presume that they are all of particular types of research. And all these stylistic standards are conducive to the presentation of narrow changes only.

These stylistic requirements make critical discussion difficult, slight modification always valuable, and innovation always dubious. They do this because any slight modification is easily presented. It can be stated briefly, its problem can presume background knowledge and be stated cryptically, the vocabulary of existing practice will be easily adapted to it and it will be presentable as a standard

type of addition. The adaptation of any modification to contemporary standards of style will be accomplished with ease regardless of the value of the result. No standards of innovation are needed when modifications are evaluated. There is no risk in publishing: the worst that can be said is that the result is trivial. But nobody bothers to catalogue or note trivial results. A more likely fate is that it will be deemed respectable and ignored. A very large and respectable literature of individually irrelevant pieces cannot be ignored, however. Cumulatively such efforts have weight by setting norms for respectability and interest.

If, in the face of this literature and its standards, one attempts critical discussions and/or to make serious innovations the same stylistic standards serve to block rather than encourage publication. Brevity is not possible since either type of result will probably require more extensive introduction. New perspectives will have to be set forth. Problems calling for more drastic change will not be easily presented in a cryptic form since some change in the standard background knowledge may be needed. Standard vocabularies may prove too restrictive. Standard methods may be inappropriate and new ones require digression. Finally, standard ways of presenting results will not work for research which is critical, interpretative, conjectural or contradicts established views because they cannot be easily cited as additions to established doctrine.

Large innovations, critical work, new conjectures and interpretations do at times succeed, of course, and this needs explanation. I will give two. The first explains such success as due to the unintended effectiveness of conservative standards; the second explains how innovations may be allowed by isolating the discussion of new frameworks. The explanation of the success is that existing standards which favor narrow research include standards which are not only applicable to other types of research but which also may deem these other types superior to the narrow results which are ordinarily favored. This situation creates the possibility for change, though success on these intellectual grounds may not be sufficient since at times the Kuhnian criterion which forbids any serious innovation in Normal Science may be invoked. The way in which existing standards may favor innovative work is by endorsing research which appears to be only modification but whose impact and/or incorporation turns out to be difficult. The problems such research raises may attract interest precisely for this reason. The interest of maintaining an existing framework may therefore lead to change when it fails.

Kuhn explains such phenomena differently. He claims that the logic of the situation is relatively unimportant. In order to explain

change he appeals to mere sociological aspects. This kind of explanation fits more appropriately a type of innovation also allowed, i.e. the creation of new frameworks which exist alongside but do not compete with existing ones. This type of innovation is possible because change of existing institutional patterns is not required even when new frameworks are created. The ability to isolate discussions which occur in various frameworks makes the introduction of such frameworks to some degree possible because the old narrow standards may be quickly reformed and adapted to the combined new and old frameworks.

The traditional standards of openness to debate, plurality, criticism, conduct and reporting of experiments, presentation of others' work, fair interpretation, and of writing all serve to place some limits on dogmatism, demands for conformity and discrimination. These may occasionally serve to open things up to a degree. Some opportunities for their use are always present and thus underground attempts at change may have some chance at success.

In existing scientific societies there are limits to the damage that the institutional policies can do. These limits are a product both of traditional standards which are antithetical to existing policies and the unintended power of techniques of implementing existing policies. Some protest may occur and even at time achieve victory. Such victories are limited, however, as even the case of Einstein shows; the policies that cause the need for them are never undermined. The overcoming of the rules is used to endorse them.

2. The Origin and Maintenance of Stylistic Standards

The story of the development of stylistic standards explains how standards have evolved and become entrenched. The following short overview of this development presents again the same analysis just explained from a different point of view. It goes further because it explains what problems scientific style was designed to solve and how it has been maintained.

Scientific style became a problem as the new science of the 17th century sought to become established in England. The Baconian effort to identify and distinguish the new science required, or so they thought, a self-conscious effort to avoid the verbal, stylistic and rhetorical excesses of past philosophy. The products of such style simply failed to yield the certain knowledge the new science sought. The problems of how to write had already been solved in varying ways by Copernicus, Galileo, Kepler and others. These various solutions seemed inadequate, however, for a new, puritan science which wanted to attain knowledge by proper and cooperative research. The Baconian ideology needed to be part of the standards

of style and these standards had to form the basis for a new discipline which could be distinctly identified and institutionalized.

The history of this development has already been ably discussed by R.F. Jones, and, following him, Robert Merton. The story they tell is of the incorporation of the Baconian ideology against a Puritan background into the established science. The style had to eschew methaphysics and speculation; it had to be sparse and clear and report only what one knew on the basis of observation. Argument and disputation were not desired.

This very narrow view of style reigned over much of the scientific writings of the next two and half centuries. Many facts were collected and reported without much notice for their theoretical importance. Yet this was obviously not sufficient: theory was needed and had to be built up if the new science were to be successful. The ideal theory should follow from the facts. It should also be precise and true. Mathematics served as an independent model for scientific style. It fit the Baconian mold as long as it was deduced from the facts.

A Baconian-mathematical ideal solidifed its hold for centuries with Newton's phenomenal success in *Principia.* Newton showed beyond question that the Baconian approach could establish the truth. He showed how broad axioms proven by factual inquiries could be presented in an axiomatic, mathematical and empirical form. This phenomenal success, however, posed an immediate problem: what next? The problem occurred because Newton had already found *the* principles governing the operation of the universe. One could not go further or deeper into the discovery of such principles since the truth was already known. On the other hand, even though one could, and perhaps should, continue to collect and describe facts, this could still not be deemed sufficient. It was preparatory. But preparatory for what?

The solution to the problem of how to extend Newton's science even though established science could provide axioms was achieved by explaining how further development of the established system was possible in various ways. These ways of improvement were mathematical improvements, extension of the theory through deduction of new theorems, discovery of new facts, and the most important of all, the construction of new theory which would serve as a bridge between established system and fact.

New theory which served as a bridge between Newtonian principles and new phenomena was needed because even though the new theory was comprehensive it was not complete. Chemical phenomena were not very adequately understood—given their own views—and later electricity and magnetism became of central

concern. The most important form of writing thus became the addition of new theory which could extend the details of Newtonian theory. There is leeway in the formation of theory as, for example, the developments in chemistry demonstrated. Yet, when completed these developments should constitue mid-range theories which could extend the framework of Newtonian science. The method of doing such research presumes that one works both down from the principles of a framework and upward from observation of the facts. The methods are analogous to analysis and synthesis in mathematics. The aim is a complete deductive structure. The style of the presentation of such research must presume a framework and general type of problem situation: it is the style already discussed.

This view of scientific advance was rendered secure by the success of Newton. Lavoisier's revolution in chemistry might have posed a challenge but since it was isolated it posed no real threat. The first real challenge to it was made by Faraday. The story of Faraday's conflict with established science has been told by Agassi. Faraday's field theories of electricity were non-Newtonian and therefore deemed outside of science. His theoretical research did not fit either the framework or the style of established science. In compensation for ignoring his theoretical work his factual discoveries were highly praised. The objection to Faraday's work could only be overcome—and then only partially—when Maxwell put the theory in a mathematical form. The objection that it was not Newtonian could still be made but stylistically it was closer to the acceptable form. Nevertheless, Faraday introduced a stylistic change which is evident in Einstein's papers.

The division between the Faraday-Maxwell theory and the standard Newtonian framework was only overcome with Einstein's revolution. The new theory reconciled the stylistically acceptable but theoretically unacceptable Faraday-Maxwell theory with the established framework by changing that framework. The embarrassment of a theory acceptable on some standards but unacceptable on others was removed. Yet a problem remained. Previously the requirements of style were consistent with the requirement that all research be Newtonian. The new theory had not only overthrown the established framework but had not found a single new one to replace it. The rationale for restricting research to a framework thus broke down and thus the rationale for traditional standards of style.

The institutions of science survived the double shock of the theory of relativity and quantum theory with remarkable ease. This was accomplished by the reinvention and adoption of a theory of science which would return scientific style to exactly the same spot it was prior to the double revolution. The change that was made was

to adopt a theory of science as the discovery of mathematical formalisms which could be interpreted as extending over particular ranges of phenomena. The aim of building true theories was abandoned and instead merely true predictions were deemed a success. The great virtue of this switch for scientific institutions was that it enabled them to keep their standards for research, their views of style and of accomplishment all in tack. Science was shaken but the institutions could survive with little change.

The ease with which conservative standards were maintained was rendered possible in the following way. Since, according to the newly adopted theory of science (conventionalism), scientific theories are merely tools we use to make true predictions, we can justify clinging to not only one framework but many. This enables us to adapt the style of Newtonian science to the new science. The standard problems of science can still be interpreted as problems of extending established frameworks by the old techniques. The traditional standards of style thus withstood the challenge of the incredible success of the stylistic and theoretical innovation of Faraday and Einstein. The new theory was institutionalized and the new style precluded.

3. The Style of Social Science

Social science has attempted to imitate the program and style of physical science; the overview of contemporary standards of style may be presented once more as an analysis of this program. Each social science and even each point of view within each social science creates its own axioms. Within their various frameworks social scientists pursue the favored style. They attempt to improve their own framework by extending its scope, by simplifying and/or creating bridge principles between the selected framework and the range of events it is hoped it will apply to.

The central defect and problem of social science has been analyzed by thinkers as diverse as Noam Chomsky and Robert Merton as the failure to achieve adequate middle range hypotheses. This analysis of the problems of social scientific theories is correct; yet even though correct it reinforces the difficulty. The analysis is used by these thinkers to encourage the maintenance and extension of existing frameworks. It is thought that new mid-range hypotheses and not new frameworks are needed. The Chomsky-Merton analysis is used to reinforce each separate system as well as a system of separate systems. Since the standard and correct analysis of the difficulty is not taken far enough; it promotes standard views of style which are precisely those standards which have lead to the failure to form mid-range theories. This occurs because the problems of finding

mid-range theories are posed only within frameworks and not also as problems of appraisal of frameworks.

Even though social scientists develop divergent frameworks for scientific research their research techniques block any serious discussion between alternatives. The difficulties are never located as defects of the frameworks. This leads to virtual inability to dispense with any framework once it is institutionalized. New frameworks can however be added. There is no methodological block and institutional problems can be overcome by establishment figures such as E.O. Wilson.

Critical discussion is blocked because the attempt to be scientific leads each group to work and publish on their own framework; there is no common forum. There is a clear reason for the failure to find a common forum: there is no theory, or even attempts to develop theories within social science of how frameworks can be critically evaluated. In social science theories or frameworks (there is persistent failure to distinguish these) only have to be scientific. Given established contemporary views of science, it is so easy to show that a theory is scientific that any institutionalized framework can at least be adequately defended. It only has to generate detailed research.

Surprisingly the attempt to generalize the scientific practice of physical science to social science provides a test of this practice. Unfortunately the difficulties of social science are always blamed on their failure to properly imitate physical science but never on their success in doing so. The attempt to generalize the methods of the natural sciences to the social sciences shows the weakness of the methods of natural science. It shows that research aiming only to improve bad frameworks is inadequate. Scientific frameworks have not arisen through the methods imitated by the social scientists since the critical discussion that produced physical theory is not deemed part of real science by physical scientists.

4. The Contemporary Ideology

The foregoing analysis of scientific style demands a complementary analysis of the ideology of science which is used to justify current practices. A new ideology is needed because of the recognition that mere conventionalism, i.e. the theory that the aim of science is merely true and useful predictions, is insufficient. This theory must be supplemented with a theory of the choice or imposition frameworks. This is necessary because it is conceded that selection of frameworks, and not merely addition of true, useful predictions, plays a role in science. The problem of how to choose

frameworks is difficult for establishment thinkers because the purpose of restricting the aim of science to true and useful predictions was to preclude theoretical debate and the need for proving that theories are true or probably true. Traditionally any established scientific theory could be deemed justified by empiricial evidence. But since theories change this view can no longer be accepted. On the other hand when new theories are deemed to be sometimes admissible we are faced with a regress. In order to justify some theory we must appeal to background knowledge to justify our choice. The background knowledge cannot in turn be justified without a regress. How then can we choose? or, how can we justify the imposition of established frameworks?

The most common way to treat the problem of selected frameworks regardless of one's philosophy is to explain it away. The way in which this is done is to view all background knowledge as unproblematic and common to all scientists. Background knowledge thus merely poses a problem for the novice. At advanced levels everyone knows just what the background knowledged is and it may be entirely ignored. Only an incompetent or ignorant scientist would need such a discussion. This view is institutionalized in the educational process. Textbooks are intended to convey the proper background knowledge that any scientist is supposed to have. Disagreements with such textbooks is a sign of misunderstanding. The exception of course is when the textbook is not sufficiently up-to-date to include the latest scientific results. Textbooks are rewritten, allegedly for the sole purpose of adding up-to-date results. Any changes in theory must be made surreptitiously in order to maintain the standard view of the feasibility, purpose and function of these texts.

This standard procedure of science has recently been redescribed with a slight twist by Thomas Kuhn. This redescription preserves all the procedures but completely changes the theoretical justification of scientific practice to take account of the deliberate lack of justification of scientific theory. On Kuhn's view the education in background knowledge is not uncontroversial, but it is necessary and dogmatic. It must be accepted by all scientists. Kuhn compares it favorably to religious education. A framework is institutionalized and therefore deemed scientific. Rationality only occurs within the framework to which the scientific community has committed itself. The attempt to save the procedures of the Cartesian ideal of the transparency and form of axiomatic theories, the attempt to save this severe rationalism, leads to a severe irrationalism. Kuhn is not alone in this move. Prior to Kuhn, Polanyi, Buber and Oakeshott in their respective fields have also endorsed the view of the overriding

importance and necessary community of some framework. This retreat to commitment is thus not only an intellectual retreat from rationalism but also an attempt to preserve institutions in existing patterns long after the rationale and usefulness of these institutions has passed.

5. Can Rationality and Scientific Institutions Be Reconciled?

The theories just discussed do provide a defense for existing scientific institutions. The cost is quite high, however: the most important frameworks of science are no longer deemed subject to desiderata of rationality. Other philosophers are more ambitious. They wish to preserve both scientific institutions and traditional rationality. These attempts however either fail to provide any better defense of rationality or, when they do, require institutional changes. No matter what path is taken our traditional conceptions of rationality are changed to one degree or another.

Those theorists who wish to preserve rationality and our institutions do not often speak of our institutions. They speak of preserving science. Their failure to discuss the institutions explicitly seems to be due to a somewhat naive attitude and a tradition which has not been self-consciously critical of its own institutions since the 17th century. Since science is the model of rationality, if we protect the later we must protect the former.

Two institutionally conservative and philosophically radical theorists who attempt to save rationality in the face of new problems are Mario Bunge and Karl Popper. Both are interesting because they do not dismiss the problems of the rationality of frameworks as the views just discussed do. Each one believes that background knowledge needs to be learned, clear, and relatively stable. Each believes it is relevant for scientific research and that it must change if we are to have scientific growth. Each must thus solve the problem this tension produces: how is progress through change possible within the frameworks of uncertain background knowledge?

Bunge's solution to this problem is to make the discussion of background knowledge part of the rational discussion of scientific theories. This is done by making it clear and explicit and by characterizing the growth of science as the movement of new scientific theories into our background knowledge as they prove successful. In order to proceed on such a course however, we must first specify the background knowledge. Bunge's central problems include: How can background knowledge be properly specified? And how can we use it to provide growth once it has been so specified?

Bunge's program for specifying background knowledge is the

study of foundations. These studies will analyze and specify the frameworks of the various sciences in proper, clear, axiomatic frameworks. He believes that the techniques of modern logic have already been successfully although partially applied to this task. Further development of the theory of the use of techniques is supplied by him and further application is a pressing task for himself and all those who care about preserving rationality.

A difficulty Bunge's view faces which it cannot overcome is that in order to specify such background knowledge we need further unspecified background knowledge. We thus come to a regress which Bunge's theory cannot account for: in solving the methodological problems of science it fails to note the very same methodological problems which occur in the analysis of science and its background knowledge.

Even if we suppose that Bunge does succeed in forming at least some simple, clear axiomatic systematic foundation for a science, difficulties remain. Bunge supposes that such foundations will enable us to apply standards of science in a superior way; we will only seek to change our foundations after we have seen in detail what problems these foundations lead to and only after we have attempted to solve them within the established framework. When problems arise which do require change we may then move. But how can we change?

It would appear that the great precision achieved in foundations does not have the desired effect of making change rational; if we find a difficulty which requires change in the foundations, we are completely at sea when we try to change. We have no theory of how to change foundations even when we note that we can incorporate past scientific theory into background knowledge. The theory of incorporation would require a background knowledge of the relationship of background knowledge to some further problems and/or theory. Thus the difficulty of the regress recurs here as well. Bunge's ambitious attempt to save both rationality and scientific institutions while acknowledging new problems thus fails to accomplish its aims. It fails due to precisely those problems it is introduced to solve.

Karl Popper attempts to incorporate the regress which causes Bunge's attempt to fail in his solution to the very problem its existence raises. He attempts to turn the defect into a virtue. He claims that we may naively and provisionally assume in any discussion that we do in fact have sufficiently precise and common background knowledge to enable us to bring our discussion to a mutually agreeable end. In fact, according to Popper, our provisional assumptions will often turn out to be wrong. But when we encounter this difficulty we may seek to understand and explain our difficulty

by a regress, i.e. a discussion of the background knowledge. He thus uses the background knowledge as not merely a foundation but also an opportunity for improved critical discussions.

Popper's optimism that we already have available the intellectual tools sufficient for the resolution of such disagreements fails to notice the scope of the difficulty. The appeal to the regress resolves a theoretical problem, i.e. how can we proceed when background knowledge is inadequate? This procedure can no doubt work in many cases. But this is not universally the case. Indeed, in those discussions which are the most interesting, it fails. The theory thus accounts for narrow but not deep problems. Let me explain.

If we find disagreement at one level it is always possible to shift to a higher level to seek both an explanation and a resolution of the difficulty. But on Popper's view this shift could only be successful if at a higher level we did find a common background. Yet this condition may fail to obtain at least for a while. We may, for example, find ourselves at sea and proceed back and forth between levels without any firm framework; when we do settle on one problem, all levels may be quite different than when we began.

The discussions of Popper and Bunge thus leaves us at least two problems. The first is: what institutional changes are required if the discussion of background knowledge is to be a normal part of discourse? The second is: how can we proceed rationally in those cases where this method itself breaks down? Attempts such as Bunge's and Popper's to maintain both rationality and traditional institutions succeed only to a degree to solve the former problem. And to the degree that this problem is solved it raises new institutional problems.

6. The Myth of Rationality Within Fixed Frameworks

The theoretical rationale for fixing and maintaining frameworks is as follows. Since we can only have a clear and rational discussion within a framework, and since we cannot rationally evaluate such frameworks, we need to arbitrarily fix some frameworks. Whatever framework we adopt and however we select it, the imposition of the framework is intended to maintain rationality. This view of the virtue of imposed frameworks is endorsed by thinkers such as Quine, Polanyi and Kuhn. Unfortunately such imposition creates loss as well as gain. The loss for rationality in placing limits on discussion are obvious. Yet there is more. It might be plausible that we should limit discussion if by doing so we created clear, albeit limited, discussions. This is not the case however. The limitations produce obscurity within the chosen area of discourse. This happens in the following way. The imposition of a framework always creates a misapprehen-

sion. The imposed framework is supposed to be fixed, but it never succeeds in being so. The context of discussion always changes, whether or not we attempt to fix the terms of debate. The denial of movement and change which takes place regardless of our efforts, leads to distortion and confusion.

The fact of change in spite of fixed frameworks can be explained from methodological, sociological, and psychological points of view. A methodological reason for the futility of attempting to fix a framework is that we cannot find any criterion by which we can identify a framework so as to be able to recognize change. The purpose of maintaining a framework is to fix the meanings of a discussion to provide a guarantee of rationality. In order to do this however, we require some methodology by which to judge new additions; we need to know whether they fit, and thus are permissible, or whether they do not and should therefore be rejected.

The various alternatives suggested such as fixing interpretations and/or meanings with formal analysis and/or behavioral interpretations and/or inarticulate knowledge of experts all fail. They fail due to the existence at a higher level of the very problems they are introduced to solve at a lower level: they fail to explain how a rational discussion of frameworks can adequately determine the identity of some framework or the means of identifying a framework. This discussion cannot take place unless it is itself in the context of a unifying and established framework. Incidentally, the detailed criteria all turn out to be vague. This is not to say we do not know how to be conservative; but this is a sociological phenomenon.

The sociological argument for the failure of a unified framework to encourage rationality is that social change, including social change of personnel will introduce changes in frameworks regardless of any attempt to fix such frameworks. This occurs due to different interests and background of individual scientists as well as due to changing social contexts. This problem is recognized and resolved by Quine, Kuhn and Polanyi by endorsement of reactionary politics—in science at least. It is thought that a rigorous education which precludes all those with odd or non-conforming attitudes can preserve the institutions of science from such unwitting change.

Aside from the obvious perversity of setting out to deliberately destroy diversity, the system must obviously fail in its effort. It must fail due to the lack of a methodology adequate to the task and to the fact that people quickly learn to advance around bureaucratic restrictions. The product will be surface conformity which may or may not be held but which will have no test. Thus diversity is not quite blocked but communication is. Nobody's theory can be intelligently discussed.

The psychological argument against the attempt to fix a framework is that it presumes a theory of learning whose endpoint is the fixing of ideas. This traditionally was thought possible either because the mind was a blank which could produce copies of impressions or because men could see by the natural light of reason clear and distinct ideas. Modern views may employ a stimulus-response model of learning but this is only a modification of the traditional Lockian view. It only changes the description of the process of fixing ideas from "internal" connections of ideas to "external" connections between stimuli and responses.

Recent psychology stemming from Külpe, however, has suggested not only that we impose ideas on the world, even in perception, but that the mind is active in rethinking and reinterpreting. This activity must lead to changes in our understanding or, alternatively, by blocking the activity we may block understanding. This psychology is as yet undeveloped. If adopted, however, it undermines the standard philosophy of science. The use of this philosophy must block understanding if the view that understanding involves continued activity of the mind is true.

The attempt to fix a framework when it is methodologically, sociologically and psychologically infeasible leads to deception. Changes are hidden, distorted and denied. Thus even though such attempts must fail in their intended purpose of achieving clear discussion, the unintended consequences are real and unfortunate. Hidden diversity becomes inarticulate. It is lost in superfitial conformity of jargon which is used to cover, as best it can, changing conceptions. This does succeed in slowing down change but not in achieving clarity. Thus both clarity and growth are sacrificed to a deceptive and stable stylistic conformity.

7. A Case for Plural and Liberal Standards

The alternative to fixed frameworks in closed societies is partial, changing and competing frameworks in an open society. Again there are reasons for endorsing such a policy from methodological, sociological and psychological points of view. Let me make the psychological point of view first, the methodological second and conclude the essay with a discussion from the sociological point of view. If the mind is naturally active, if it continually tests, reinterprets and seeks new points of view in order to comprehend, we ought to incorporate a recognition of these activities into our theories of how to conduct our intellectual endeavors. We could usefully improve our thinking by making the prescriptions more in accord with our natural processes of thought. If we can incorporate into our methodologies and our social policies techniques which use

the natural process of the mind we have a far better chance of creating understanding than if we try to freeze this process.

We do not now have good theories of understanding; all those we have presume that understanding is ultimately like photography. We "take" clear pictures and file them. This view will not do even if it is combined with a theory of the activity by which we produce such "pictures". If the mind is always active and reworking all its inventory, understanding must be at least in part not merely a product of this activity but actually coextensive with it.

Reasons for seeking an alternative to fixing frameworks from a methodological point of view result from an appraisal of what methods can and cannot do. Methods cannot enable us to fix clear ideas. They can however enable us to test, probe, improve and move on. Methods which are designed to fix ideas can succeed by testing and improving. But when they go beyond this they fail due to the regress discussed above. The paradigm of greater success is, of course, logic. But however one views logic itself, the use of logic to fix ideas external to it never succeeds due to the arguments already given.

We can instead develop our ability to test, improve and probe. The use of methods for this purpose requires not only a critical attitude toward existing theories but also the attempt to construct new alternatives. Testing will only be of use if we use it as an impetus for new improvement. The modern version of the Cartesian ideal of fixing clear and distinct ideas thus becomes a pathway to irrationality. Fixing frameworks limits the possible benefits of reason by blocking the discussion of alternatives. The effective use of critical tools is blocked by deeming the most important aspects of theory beyond criticism. The quest for the unreachable goals of a clear and unchanging framework ,ends in dogmatism, jargon and seemingly sophisticated yet highly arbitrary "critical" procedures.

My final case for an open society—a society with critical and changing standards and a liberal, pluralistic outlook—lies in the consequences for rationality of the competing social policies toward innovation. The policy which is now in vogue is to create a closed science. This policy fits in nicely with traditional scientific institutions, especially those of scientific style, even though it is opposed to many traditional values of science. The maintenance of this policy and the institutions which have given rise to it and which it in turn supports can only further erode these values.

The policy of closed science is to set entrance requirements for scientific theories sufficiently high and/or sufficiently restrictive so as to limit entry and guarantee that all theories allowed in are properly there. This later policy is crucial because a theory is only

admitted if it is not deemed to be itself problematical.

Some of the consequences of this policy for rational and honest discussion are disastrous. The crucial defect of the above policy is that the main step which is intended to guarantee rationality, i.e. the entry of a theory to serious discussion, is beyond rational appraisal. The attempt to sharply demarcate between those theories acceptable to scientific society and those theories unacceptable to this society leaves no place within science for discussion of which theories belong and which theories do not. This is decided privately by editors, reviewers and publishers. Once a theory has been certified it is difficult if not impossible to reverse the decision. The decision itself is not subject to any standard.

This theory of admission of theories to science naively presumes that certification of new theories can take place without serious discussions of the merits of those theories. The up-to-date established leaders are presumed to be capable and willing to provide accurate and proper judgments of new work independently of any critical and open discussion. Once a judgment of certification has been made discussion is limited. Once a theory is certified, any research which develops it in detailed ways is deemed progressive. Broad discussions, when they do rarely occur, are spectacular and still ineffective. The broad attack of Chomsky on Skinner helped him to establish his own theory but did not affect the Skinnerian troops. The debate over sociobiology has been spectacular but criticism only reinforces its respectability, which is at any rate guaranteed by Harvard.

The most important kinds of debates do not often take place, i.e. discussion of frameworks. When the virtues or defects of frameworks are discussed, it is already after the judgment of scientific society has been made. Public discussion which follows private judgment, leads to dishonesty since the effective standards are employed privately and never discussed, yet it is erroneously claimed that scientific theories are only adopted through the operation of public and open standards. Public criticism must fail since success would prove the private judgment of the elite to be wrong. Furthermore the claims for private standards of reviewers, editors and publishers must be too high. Since the demarcation between exclusion and inclusion is sharp, the standards applied must be decisive. Yet the disagreement after the fact proves that this is not so. The scientific community thus must hold that its standards are decisive and properly applied while still allowing for public disagreement. This makes public disagreement over fundamental theory quite hostile and unpleasant since it should not occur and when it does is often deemed a matter of integrity and competence.

Since the standards must be clear and they are not, only dishonest or confused attempts to defend them will be possible. This further weakens the possibility of a successful debate. It further leads to standards for proper scientific research which avoid such issues, but accept and presume the proper operation of scientific authority. This in turn leads to policies, such as I have discussed above in regard to scientific style, which are designed to block rationality rather than to serve it.

The rationale for maintaining an authoritative and closed science is that it is necessary for the protection of scientific standards. It is thought that if "high" standards were not maintained for the entrance of scientific theories any pseudo-scientific view might gain recognition and respectability, thereby undermining science. The reaction of physicists to the publication of Velicovsky's work by a respectable publishing company was an outburst of this fear.

This fear is misplaced. It ignores the bad work which succeeds in being institutionalized, in spite of attempts to block it. It ignores the fact that removal of such institutionalized views is rendered exceedingly difficult by established institutional patterns. And it vastly overestimates the interest and thus the viability that pseudo-science has in open discussion. Most pseudo-science is a bore. Only those who wish to maintain fixed views regardless, can be seriously and for any length of time attracted to it. Despite the reactionary posture of much of science this degree of stability should prove too much to bear even for Kuhn's Normal Scientist.

More importantly, for the case for an open scientific society, however, are the alternative solutions to the problems of bad science, however seriously one takes them, that an open scientific society allows for. In an open society publication is only acceptance for the purposes of discussion and not certification. Many theories of low quality will be accepted as they currently are but publication will not carry with it presumption of great merit. Moreover the theory is realistic in its modest presumption that theories are good or bad, in different ways and in varying degrees. This openness allows for various types of realistic appraisals. The all or nothing quality of existing debates and the rigidity this builds into such debates may be lessened. New ideas may achieve a quick forum and old ideas may be easily removed. The lack of dogmatic endorsement thus may greatly lessen if not eliminate the problems posed by bad theories. On the other hand, it may also open up discussion for more valuable proposals.

A further virtue is that it may remove the serious problems of integrity caused by the need to justify and defend the dishonest claims which are a product of the closed society. The overly high and

unwarranted claims to be capable of judging definitively and prior to discussion the value of contributions will be removed. This is not only important for improving the institution internally. It is also an important means of removing the ability of pseudo or bad science to make inflated claims and to justify them by the same dishonest claims made by the scientific community. An open science removes the ability of dogmatists to promote fraud, while a closed society is subject to and provides the means for its accomplishment by others.

Conclusion

Scientific society today embodies on the one hand an authoritarian philosophy and institutions and, on the other hand, traditions of openess and liberality. Philosophers such as Polanyi, Kuhn, Quine and others have argued that an authoritarian and closed society is necessary for scientific growth. Ironically this view requires little change in scientific institutions and Kuhn claims that it describes them. This view has by now lost its naivete. The cruel consequences of totalitarianism need to be recognized in any society, even scientific ones. The alternative of promoting more openness rather than less is an obvious remedy. The conflict this alternative poses is not, as is often thought, a choice between science and non-science. Rather it is a choice between growth and control, liberality and authoritarianism, autonomy and dependence, criticism and conformity. The conflict poses not only problems about how we gain knowledge but also moral and social choices about how we want to live. We may decide to make society more open or more closed. This is currently the most fundamental question of scientific style.