Reason and the Search for Knowledge

Investigations in the Philosophy of Science

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EDITORIAL PREFACE

An impressive characteristic of Dudley Shapere's studies in the philosophy of the sciences has been his dogged reasonableness. He sorts things out, with logical care and mastery of the materials, and with an epistemological curiosity for the historical happenings which is both critical and respectful. Science changes, and the philosopher had better not link philosophical standards too tightly to either the latest orthodox or the provocative upstart in scientific fashions; and yet, as critic, the philosopher must not only master the sciences but also explicate *their* meanings, not those of a cognitive never-never land. Neither dreamer nor pedant, Professor Shapere has been able to practice the modern empiricist's exercises with the sober and stimulating results shown in this volume: he sees that he can be faithful to philosophical analysis, engage in the boldest 'rational reconstruction' of theories and experimental measurements, and faithful too, empirically faithful we may say, to both the direct super-highways and the winding pathways of conceptual evolutions and metaphysical revolutions. Not least, Shapere listens! To Einstein and Galileo of course, but to the workings of the engineers and the scientific apprentices too, and to the various philosophers, now and of old, who have also worked to make sense of what has been learned and how that has happened and where we might go wrong. We think that Shapere's title for this selection of his essays is a description of himself as it is of his work and his book. Can more be asked of a philosopher?

October 1983

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PREFACE

The essays collected in this volume were written over a period of nearly three decades, from "Philosophy and the Analysis of Language," originally a term paper for Morton White's course on Analytic Philosophy at Harvard in 1953 or 1954, to "Modern Science and the Philosophical Tradition," which has been presented as a public lecture on various occasions during the last two or three years. In spite of the time spanned, however, the papers present a unified perspective on the nature of science, and of human knowledge in general. Although some exceptions will be found, the changes that have taken place in my viewpoint turn out to have consisted, for the most part, in a gradual broadening and deepening of perspective rather than in major shifts of doctrine. But not all of the viewpoint which I have developed is represented in the papers included here; other aspects of it will be found in recent publications, and still more will appear in future work. In order that the ideas found in the present papers may be placed within that larger framework, I have tried to give a rather extensive, though still all too sketchy, outline of it in the Introduction to this volume.

It is impossible to name all the students, colleagues, and friends who have contributed to the development of the ideas presented here. Some people must, however, be given special thanks: Robert Cohen, for encouraging me to publish this collection; Morton White, for constant help and enlightening discussions; and Hannah Hardgrave Shapere, for her valuable suggestions about the work, and for her support while I was putting it together. The University of Maryland generously provided me with a Faculty Research Grant to pursue these investigations, and the National Science Foundation has also supported them on several occasions. I also wish to express my special gratitude to the Institute for Advanced Study, Princeton, New Jersey, for providing the time and the environment, in 1978–79 and again in 1981, without which my thoughts might never have coalesced.

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"Mathematical Ideals and Metaphysical Concepts" appeared in *The Philosophical Review*, Vol. LXIX, No. 3 (1960), pp. 376–385; "The Structure of Scientific Revolutions" also came out in that journal, Vol. LXXIII, No. 4 (1964, pp. 383–394. Both are reprinted here by kind permission of *The Philosophical Review*.

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"Unity and Method in Contemporary Science" was originally published as "Unification and Fractionation in Science: Significance and Prospects," in *The Search for Absolute Values: Harmony Among the Sciences*, Proceedings of the Fifth International Conference on the Unity of the Sciences (Washington, D.C., 1976), pp. 867–880, and is reprinted here by permission of the International Cultural Foundation Press, copyright 1977 by the International Cultural Foundation, Inc.

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"Remarks on the Concepts of Domain and Field" is a revised version of the opening pages of "The Influence of Knowledge on the Description of Facts," *PSA 1976*, Vol. 2, East Lansing, Philosophy of Science Association, 1977, pp. 281–298. "Reason, Reference, and the Quest for Knowledge" appeared in *Philosophy of Science*, Vol. XLIX (1982), pp. 1–23. Both these articles are reprinted here by permission of the Philosophy of Science Association.

The other articles contained in this volume have not previously been published. "Interpretations of Science in America" was a Sigma Xi National Bicentennial Lecture in the years 1974–77. "Alteration of Goals and Language in the Development of Science" is a completely rewritten and greatly extended discussion of a case dealt with in "The Influence of Knowledge on the Description of Facts." "The Concept of Observation in Science and Philosophy" is a brief summary of an article appearing in *Philosophy of Science*, Vol. XLIX, pp. 485–525 (December, 1982); that article, in turn, is part of a chapter of a book of the same title, to be published by Oxford University Press. "Modern Science and the Philosophical Tradition" has been presented as a public lecture on various occasions.

INTRODUCTION

Section I of this Introduction summarizes some of my criticisms of certain important movements and doctrines in the philosophy of science of the last three decades or so. It also includes a sketch of the ways I have conceived some of the fundamental problems of the philosophy of science in the light of those criticisms. Section II outlines the view of scientific change which emerges in the essays included in this volume, while Sections III and IV, in addition to some further points to be found in the included essays, outline a broader viewpoint, one which is for the most part, especially with regard to the ideas discussed in Section IV, not represented explicitly in these essays. (To avoid confusion with the "Parts" into which the body of this book is divided, I refer to the divisions of this Introduction as "Sections.") I must emphasize that that larger viewpoint is only sketched here, largely without the detailed development and argument it requires; its full elaboration and defense will be found in other writings, most of them not yet published. But without this overview, many of the ideas found in the present essays might appear fragmentary and unrelated; it is therefore offered here for the sake of completeness, in order to provide the reader with a framework within which the ideas developed in the present papers should be understood. Throughout the Introduction, I refer to places in the included articles where the ideas mentioned are developed, and I also refer, especially in Sections III and IV of the Introduction, to other recently-published and forthcoming work.

Logical Empiricism dominated the philosophical interpretation of science in the United States from the 1930's until well into the 1950's. (Logical Empiricism is often referred to as Logical Positivism; since my work focusses on the connections of that movement with traditional empiricism, I will generally use the former term except when explicitly comparing or contrasting Logical Empiricism with traditional empiricism, or when discussing doctrines characteristic of the former but not of the latter movement.) Despite numerous internal disagreements, often of a quite serious nature, adherents of that movement agreed on certain very general approaches, or at least on a very general program for the interpretation of science. However, beginning in the 1950's and reaching a peak in the early 1960's, those more fundamental approaches themselves began to be criticized, and by about 1970 had been widely rejected. In my own view (partly presented in Part I of "Notes Toward a Post-Positivistic Interpretation of Science" and "Interpretations of Science in America"), the most important weaknesses of the Logical Empiricist program were the following. First, its focus on the formal (logical) structure of theories ignored the developmental aspects of scientific ideas, and positivists even denied that those developmental aspects were of any philosophical interest. On the contrary, it seemed to me (in company with a number of other writers at the time) that there are often reasons for the introduction of new scientific ideas, and that those reasons need not necessarily be the same as those involved in the justification of those ideas. Second, the Logical Empiricist distinction between "scientific" terms – those occurring "within" science – and "metascientific" ones – those used in "talking about" science, – and the positivist insistence that the primary concern of the philosophy of science was the analysis of the latter, seemed to me highly questionable. Could it not be that "metascientific" concepts themselves change, and, further, that those changes come about, in at least some cases, in response to new substantive views about nature? New investigations by historians of science, as well as my own studies, seemed to me to indicate that such changes might be deep and pervasive. And finally, the Logical Empiricist – and traditional empiricist – distinction between 'theory' and 'observation,' at least in the forms in which it had generally been understood, as a mutually exclusive (as well as collectively exhaustive) classification, was indefensible. The problem was not merely that – as many critics were asserting at the time – there was no clear criterion for making the distinction – that many, and perhaps all, of the positivist-empiricist examples of "observational" terms arguably contained a "theoretical" component, and that many (at least) of their examples of "theoretical" terms sometimes functioned, in science, as "observational." Those objections seemed to me to be essentially correct (though their bases or implications seemed to me not to be well understood). But there was a more fundamental objection, which could not be interpreted as a mere failure to make a distinction adequately. Rather, it questioned the very program of making the distinction as an unalterable, mutually exclusive one. For even if there were any such thing as an observation term wholly pure of any theoretical presupposition or component, that term would, by that very purity, be rendered completely irrelevant to the theories with which those observation terms were supposed to be concerned. An important aspect of this argument, of course, was the failure of the positivist movement to specify how observation terms *could* be relevant to theoretical terms. The latter were admittedly not definable in terms of the former; and the "partial interpretation" view proposed by Carnap seemed to suffer from so many difficulties as to be untenable. But on the other hand, to deny the distinction between a pure, uninterpreted observation-language on the one hand and a theoretical language on the other would threaten to betray the original motivation of the entire empiricist movement: namely, to show how the meanings of our theoretical ideas could be objectively based in experience. Similar questions arose with respect to the *acceptability* of theoretical claims (as distinguished from the *meanings* of theoretical terms). Such acceptability or rejectability seemed to me, from my examination of cases in science and its history, to require antecedent "theoretical" beliefs; yet if acceptance or rejection was to be "objective," should not the "observations" on which empiricism rested that acceptance or rejection be *free* of bias by any antecedently-accepted "theory"? There thus seemed to be a conflict between what I later called (in "Interpretations of Science in America") the *Condition of Objectivity*, according to which the bases of test (acceptability) of a scientific idea must be "independent" of the idea to be tested, and the *Condition of Relevance*, according to which the bases of test (the "observational evidence") had to be "relevant" to the idea to be tested. The original positivist and empiricist programs emphasized the former condition at the expense of the latter; and that very fact meant that *that* version of the empiricist-positivist program, at least, was misguided in principle.

During this period of criticism of Logical Empiricism, the 1950's and early 1960's, a number of alternative views were being proposed. The positions advanced by Stephen Toulmin, Paul Feyerabend, and Thomas Kuhn were particularly influential. But while I was sympathetic, at least in part, to their recommendation that philosophy of science must rest on examination of the history of science, the conclusions those writers thought followed from that examination did not seem in fact either to follow or to be acceptable. Their views were often extremely ambiguous, admitting of a variety of interpretations; some central ideas were not explained at all. Far from following from an examination of the history of science as they claimed, their views seemed shaped by the fundamental ideas involved, vague though they were; their conclusions rested ultimately on conceptual unclarities and confusions. For example, Feyerabend and Kuhn claimed that some presupposed unitary "high-level background theory" or "paradigm" functions in every specific scientific situation within a scientific "tradition" or "community" to shape or determine or govern all aspects of science as practiced by members of that tradition or community, including the meanings of terms, what counts in that tradition or community as observational, the problem-field, the methods of approach, the programs of research, and the standards of possible and correct solution of problems. (Because of the generality, unity, and pervasiveness of this supposed governing viewpoint, I have referred to their view as *Global Presuppositionism*.) But they never attempted to make clear the precise way or ways in which such "shaping" or "determination" or "governance" takes place. Is the relation supposed to be one of logical implication, or is it weaker than that, perhaps only one of psychological association? Or does the manner of governance vary from circumstance to circumstance within the tradition or community? This idea of an overarching governing viewpoint had disastrous implications for their interpretations of science, in particular with regard to two central notions. In the first place, since the meanings of all terms ("theoretical" and "observational," if not also logical ones) were supposed to be "determined" by the high-level background theory or paradigm, Feyerabend and Kuhn concluded that the meanings of all those terms differ incommensurably from one high-level theory or tradition or community to another. Yet, though the concept of "meaning" thus played a fundamental role in their views, they made no effort to analyze it. (Or at least no successful effort: *cf*., "Meaning and Scientific Change.") Any similarities between the use of a term in two different traditions were simply relegated by fiat to being not "part of the meaning." No criterion for distinguishing what is from what is not "part of the meaning" was given; no account was provided of the fact of the similarities of usage. Secondly, although their lack of analysis of the sense or senses in which the governing viewpoint governed left their notion of scientific reasoning vague, it nevertheless seems that, however that claim is interpreted, it must follow that there could be no reason for the replacement of one such governing viewpoint by another. For even what counts as a "reason" in favor of or against a scientific idea (or problem or method, *etc.*) is supposed to be determined by the governing viewpoint; and by the incommensurability thesis, what one tradition counts as a "reason" would be incommensurable with what is so counted by another tradition. Given their lack of analysis of these and other fundamental ideas, the most reasonable interpretations of their positions imply an extreme relativism in which there is no progress, or even any knowledge, in science. Such an outcome could only be expected from positions which deny the existence or possibility of *any* theory-independent observation-language without at the same time showing how the objectivity of scientific test could be preserved. The problem with their views was thus the reverse of that of positivism: they focussed on the Condition of Relevance to the exclusion of the Condition of Objectiity. (These criticisms are detailed in "The Structure of Scientific Revolutions," "The Paradigm Concept," and "Meaning and Scientific Change," and, with increased generality, "Interpretations of Science in America." More restricted but nevertheless similar views of Toulmin are examined in "Mathematical Ideals and Metaphysical Concepts.") Since in the final analysis the views of Feyerabend and Kuhn flowed from the above vague and confused ideas rather than from any studies of cases from the history of science, it seemed that their relativistic implications might be avoidable, and that a better view might involve a more adequate analysis of the concepts and roles of "presupposition," "meaning," and "reason."

The complementarity of both the difficulties and insights of the two warring parties, Logical Empiricism and its "Global Presuppositionist" critics, emerged in "Interpretations of Science in America," and reflected what I had already seen as a program for the philosophy of science. On the one hand, the problem was to develop a view which would preserve the objectivity and rationality of science, and the possibility of its attaining knowledge – goals which the empiricist-positivist tradition had rightly tried to achieve. Yet those aims could not be fulfilled through the paths those movements had taken: the pure, uninterpreted "given," the theory-free "observation-language" by which they had attempted to satisfy the original motivation of empiricism, to account for the objectivity of the knowledge-seeking enterprise, would have to be rejected. Account would have to be taken of the roles of presupposed beliefs, and the extent and character of those roles. The notions of objectivity and reason, and of the possibility of scientific knowledge, would have to be reinterpreted in the light of those roles – but not, however, rejected in favor of the relativism into which the critics of empiricism and its positivist descendants had fallen. The ways in which the presupposed beliefs are chosen would have to be understood (an understanding which the critics of positivism had in effect denied was available). And while some philosophers of science seemed on the verge of abandoning the theory-observation distinction entirely because of the criticisms that had been levelled against it, I thought that it would have to be reinterpreted and reassessed in the light of the roles played by presupposed beliefs in the determination of what was to count as "theoretical" and as "observational."

The problem was of course not merely a matter of philosophical reflection; it was a matter of constructing a view of science which would be adequate to the ways in which science itself proceeds. The positivistic tradition too often ignored real science in its focus on purely logical issues; its critics distorted science and its history through conceptual confusions. Yet science itself has managed to avoid relativism and skepticism while at the same time developing, more and more, especially in the twentieth century, an attitude of complete openness to new ideas, of willingness to abandon any idea in favor of any new one, no matter how strange or contrary to presupposition, whenever such changes were called for. And increasingly in the twentieth century, scientists have seen such changes as governed by observation. Clearly, the construction of a new view of science would have to be based on a close examination of cases in science and its history as well as on attention to philosophical issues.

In the years during and following the writings included here in Part I, I made a number of studies of cases in the history of science and contemporary science. Most of those studies were largely exploratory in nature, trying partly to extract philosophical lessons from the cases and partly to test various philosophical interpretations which seemed to offer some promise of dealing with the issues I have just outlined. However, my view of the role of such case studies gradually evolved beyond using them merely as bases for criticism or generalization. That later conception of the role of case studies diverges in fundamental ways from that of most historians and philosophers, as will be described in Section III, below. (It and its criticisms of usual conceptions are detailed in "What Can the Theory of Knowledge Learn from the History of Knowledge?")

Of the case studies made during those years, only a few have been included in the present volume. Two such papers dealing with case studies, "Scientific Theories and Their Domains" and Part II of "Notes Toward a Post-Positivistic Interpretation of Science," require special comment. The analyses of cases given in those two articles have been important for the development of my views, in ways that will become apparent later. Nevertheless, as they stand, those analyses are limited in certain ways. In "Scientific Theories and Their Domains," "patterns of reasoning" play a prominent role in the interpretation of the cases examined. But those patterns are presented there without any indication of their source or justification, and I would now insist that such be provided. The concept of a "domain" itself has undergone considerable refinement over the succeeding years (as will be seen in "Remarks on the Concepts of Domain and Field" and "Alteration of Goals and Language in the Development of Science.") For some time, indeed, I rejected the notion; I only readopted it around 1980, as I gradually came to see that, properly understood, it provides an important ingredient in the analysis of 'reason' for which I was searching (see below, Section II). Again, Part II of "Notes Toward a Post-Positivistic Interpretation of Science," and, in a somewhat different way, Part IV of "Scientific Theories and Their Domains," present an interpretation of scientific "idealizations" or "simplifications" – a group of functions of scientific ideas to which I now refer more generally as "conceptual devices." At the time I wrote those essays (the last half of the 1960's), this topic was in many ways isolated from the problems with which I was centrally engaged, as I have outlined those problems above. That idea too, however, like that of "domains," has now found its place in the larger context of thought which I will outline in Section III.

As my studies of scientific cases developed, I gradually came to understand more definitively what I had supposed in a general way from the outset: that the issues with which I was concerned were only special cases of far more general ones in the history of philosophy; that the doctrines which I thought must be opposed were only specific versions of ones which had permeated philosophy since the time of Plato. These generalizations of issues and doctrines began to be presented in "The Character of Scientific Change" (written in 1978). They, rather than the more specific and transitory doctrines of Logical Empiricism and its critics, now became the focus of my work. I now saw the empiricist doctrine of a pure, uninterpreted and unassailable given and the positivist doctrine of a theory-free observation-language as being in the same company as other "absolutist" views in the history of philosophy: the Platonic claim that there are unalterable Ideas in terms of which experience must be interpreted; the Kantian view that experience presupposes certain Categories and Forms of Intuition; the view that science, or more generally, the knowledge-seeking (and/or the knowledge-acquiring) enterprise, must necessarily accept certain presuppositions, like the "Principle of the Uniformity of Nature" or the "Principle of Limited Independent Variety"; the idea that there is an unalterable "scientific method"; the view that a certain particular set of concepts must appear in any fundamental scientific theory; that the goals of science are established once and for all, presumably at the beginning of inquiry, or perhaps by virtue of the "very concept" of an inquiry. But also in that company was the Logical Empiricist notion of a set of unalterable "metascientific concepts" which are definitory of science, past, present, or future. All such views have in common the idea that there is something about the scientific (or, more generally, the knowledge-seeking or knowledge-acquiring) enterprise that cannot be rejected or altered in the light of any other beliefs at which we might arrive, but that, on the contrary, must be accepted before we can arrive, or perhaps even seek, such other beliefs. I came to call this idea "the Inviolability Thesis." But what could be the justification of such allegedly inviolable constituents, methods, presuppositions, or whatever of science? My own investigations had convinced me that an understanding of "observation" was at hand which was far more adequate, both to philosophical issues and to the way science has developed in its history, than the "pure, uninterpreted – and inviolable – given" or "theory-independent observation-language" doctrines or any of the alternative views which had been proposed to replace them. (See "The Concept of Observation in Science and Philosophy," of which only a brief summary is included in the present collection. The full article is in *Philosophy of Science*, December, 1982; that article will be part of Chapter II of a book of the same title, *The Concept of Observation in Science and Philosophy*, forthcoming from Oxford University Press.) And as for necessary presuppositions, whatever their claimed nature, history is littered with the ruins of allegedly *a priori* necessities or impossibilities which have been overthrown by scientific developments. One way of attempting to justify such necessities has been through "transcendental" arguments. These have in common with scientific explanations the attempt to show how experience, or inquiry, or whatever, is possible. But unlike explanations in science, a transcendental argument claims that the explanation given is the *only* one that could be given – that it is *necessary* as an explanation of the phenomena in question. And the argument that would establish *that* claim can only be an *a priori* one, subject to all the doubts that such arguments are heir to. Some of the general reasons for suspicion of *a priori* claims are given in "Modern Science and the Philosophical Tradition."

The relativism into which the views of Feyerabend and Kuhn degenerate, on the other hand, has its kindred in the various forms of relativism and skepticism that have arisen so often in the history of philosophy. The aim of variants on the Inviolability Thesis from Plato to the present day has always been at least partly to counter skeptical and relativist arguments; and despite the failures of Inviolability counterarguments, this motivation behind them seemed to me a valid one. Relativist and skeptical arguments have usually been as confused and unconvincing as those of their absolutist opponents; and, like the latter, they take no account of the evident achievements of science. The real point of both skepticism and relativism lies in their exposure of shortcomings in our understanding of the nature of knowledge and of the knowledge-seeking process, shortcomings which proponents of the Inviolability Thesis, in any of its forms, have done nothing to alleviate. Specific arguments against relativism and skepticism are found in many of the papers included here, and new ones, in some cases more powerful than the present ones, will be given in the forthcoming book, *The Concept of Observation in Science and Philosophy*.

The problem with which I have been most centrally concerned can therefore be summarized as follows. Given the failure of all absolutist and relativist-skeptical arguments hitherto presented, can an account of the knowledge-seeking and knowledge-acquiring enterprise be given which, while not relying on any form of the Inviolability Thesis, will also not collapse into relativism or skepticism? More specifically, can an account be given of the scientific enterprise which will not make that enterprise subservient to some unalterable given or presupposition? And the focal issue in the attempt to construct such a view must be this: Is it possible to understand science (or, more generally, the search for knowledge) as able to proceed *rationally* without presupposing criteria of what is to count as "rational," criteria which could not be *arrived at* in the course of seeking knowledge, but which must be assumed in order to engage in that enterprise at all, or at least to engage in it successfully? It has been the supposed impossibility of constructing such a view that has led many philosophers to adopt some version or another of the Inviolability Thesis, as the only possible alternative to relativism or skepticism.

My investigations have thus stemmed not from an assumption that the Inviolability Thesis is unavoidable if we are to escape relativism and skepticism, but rather from the very opposite standpoint: from the attempt to ascertain how much of the knowledge-seeking and knowledge-acquiring enterprise can be understood without accepting anything inviolable. If as a result of these studies it turns out that we must accept some such inviolable principles, that will be a conclusion, not a starting-point, of the inquiry. My starting-point has not, of course, been chosen gratuitously; the reasons for its choice, as will be seen in the essays included in this volume, lie partly in the failure of the alternative views and partly in an examination of science and its development.

With this statement of issues providing a background, then, I will now outline the general view of science and scientific change which has emerged, and which is presented in a more detailed way in the papers included here.

II

At any stage in human history, beliefs are available which have (thus far) proved successful and free from reasons for doubt. (The roots of such beliefs will be detailed in the book, *The Concept of Observation in Science and Philosophy*; in particular, it will be shown there that no bars to the attainment of knowledge necessarily result from the employment of such beliefs. As to "success" and "reasons for doubt," more will be said in what follows.) Increasingly, such views have become the bases on which we build our further beliefs. The ways in which those views achieve this status are of primary importance for understanding the nature of science and scientific reasoning. For a central feature of the development of science – a feature which indeed has evolved into one of the major distinguishing marks of what we call "science" – consists in the acquisition of further such beliefs, of relevance-relations between them, and of the organization of such beliefs (those which have been found to be successful and free from doubt) into areas for investigation and well-founded beliefs relevant thereto. That organization becomes more and more characteristic of the knowledge-seeking enterprise; and it comes more and more, as science develops, to be made in terms of beliefs which have been found to be relevant to one another. It is the process by which *areas or fields of scientific investigation are formed*. Increasingly, as science develops, such areas come to consist of two distinguishable parts: (1) a body of information to be investigated (this I call the "domain" of the area or field – see "Scientific Theories and Their Domains," "Remarks on the Concepts of Domain and Field," and Part II of "Alteration of Goals and Language in the Development of Science"); and (2) a body of "background information," that is, a body of successful and doubt-free beliefs which have been found to be relevant to the domain. (This idea plays a role in most of the essays included in Part III, but is discussed most systematically in the article, "The Concept of Observation in Science and Philosophy"; its role can be gathered to a considerable extent from the summary of that article which is included in the present volume.)

But it is precisely these developments – the formation of domains and of background information relevant thereto – that *constitute* the development of the rationality of science. For one of the most fundamental aspects of the idea of a "reason" in general is the following: to count as a reason, a claim must be *relevant to the subject-matter under consideration or debate. And thus the clear delineation of a subject-matter, and of the body of other*