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Textual Persuasion: The Role of Social Accounting in the Construction of Scientific Arguments*

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1. INTRODUCTION†

When historians study scientific knowledge they study scientific texts of various sorts, principally formal scientific writings and more personal diaries and notebooks. Sociologists, too, depend, on the examination of texts in a general sense, whether they be laboratory notes or interview transcripts. Observed scientific actions do not afford a pure datum because their meanings are only established as they are interpreted in the light of prevailing scientific beliefs.[1] Understood in the broadest sense, scientific texts (i.e., verbalized or written interpretations of scientific actions and the information gleaned by scientists) constitute the sole evidence for meta-scientific enquiries. The most ubiquitous form of scientific text is the formal paper. Through the medium of such papers, scientists are commonly thought to put forward arguments and counter-arguments relating to the topics at the forefront of knowledge. Until fairly recently these formal papers have been accepted as straightforwardly representing the arena in which knowledge-claims are scrutinized. Scientific knowledge was regarded as really being evaluated just on the bases upon which it was *said* to be assessed in these formal papers.

The sufficiency of these formal texts to provide a complete and trustworthy statement of the procedures whereby scientific knowledge is established, has been severely questioned. For one thing, it has been suggested that formal scientific texts espouse a misleading and a systematically inductivist view of scientific procedure;[2] and historians and sociologists have drawn attention to plausible connections between scientists' beliefs and their social environment which appear to cast doubt on the 'internalist' accounts found in scientific papers.[3] One is led to conclude that the writing in formal texts is by no means

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1 For a consideration of the ways in which both scientific actions and the observations scientists make are given meaning only in the light of subsequent interpretations, see N. R. Hanson, *Patterns of Discovery*, Cambridge 1965 (especially chaps. 1, 2, and 4), and B. Barnes and J. Law, 'Whatever Should be Done with Indexical Expressions?' *Theory and Society*, 3, 1976, 223-37.

2 See P. Medawar, 'Is the Scientific Paper Fraudulent? Yes; It Mis-represents Scientific Thought', *Saturday Review*, August 1, 1964, 42-43, and G. N. Gilbert and M. Mulkay, 'Contexts of Scientific Discourse: Social Accounting in Experimental Papers', in K. Knorr et al (eds.), *The Social Process of Scientific Investigation: Yearbook in the Sociology of the Sciences*, IV, Dordrecht 1980.

3 Amongst the wealth of interesting studies, relating scientific knowledge to social

'degree zero'.[4] Whether one views these texts as a partial portrayal of scientific evaluation or as a form of ratiocination, I suggest that it is important to seek to understand why formal papers possess their particular characteristics. The contention of this paper will be that formal scientific papers should be regarded primarily as contributions to scientific debates. They take the form of arguments, aimed at persuading the reader of the correctness of a specific point of view. Moreover, I shall suggest that the peculiar features of this type of text contribute to the argumentative purpose. I shall aim to reveal the complex organization of a relatively straightforward scientific text, and suggest that only on the basis of a (sociologically informed) theory of reading or accounting can one pass beyond the surface organization of scientific discourse, to scrutinize scientific knowledge and scientists' beliefs more thoroughly.

In suggesting that scientific texts contribute to the arguments put forward in scientific debates, I am not aiming to insinuate that there exist enduring canons of scientific argumentation capable of determining the outcome of disputes. Rather, the principles of scientific reasoning may change over time: for instance, with the emergence of new mathematical techniques, new items of apparatus, or even new philosophical precepts.[5] Moreover, there is no certainty that argumentation, in any regularized, straightforward sense is, in fact, characteristic of all or even a great part of scientific reasoning. For one thing, scientists receive no training in dialectics. Without even considering the interpretative complexity of the notion of argument, it is clear at an empirical level that scientists are rewarded for results and not for dialectical propriety. Indeed, the common idea of sudden scientific insights and the references to a 'logic' of retrodiction by Peirce and Hanson, both reveal a great laxity in the constitution of a scientific argument, even in philosophers' eyes. The sole common feature is that all scientific arguments are liable to presentation in the forum of formal publication. Publications assume very diverse forms, employing illustrations as well as written claims, analogies, citations, and of course vary from the definitive to the speculative.[6] An idealized commitment to the 'norm of argumentative co-operation' falters before such an array of material.

No scientific paper begins from first principles; there is always an assumption of certain indubitable propositions, yet what is taken as unquestionable will vary from one scientist to another. The force of Polanyi's insistence upon the importance of tacit knowledge, and Kuhn's recognition of exemplary puzzle-solutions, is precisely to oppose the possibility of fully explicit argumentation.[7] Nonetheless, scientific papers do afford a crucial locus for the representation

factors and thereby offering alternative accounts of belief to those found in scientists' own formal texts, a good, representative selection is to be found in B. Barnes and S. Shapin (eds.), *Natural Order: Historical Studies of Scientific Culture*, Beverly Hills 1979.

4 See R. Barthes, *Writing Degree Zero*, New York 1968.

5 The importance of such developments is considered in S. E. Toulmin, *Foresight and Understanding*, London 1961.

6 The specific details of scientific publication practices have been irregularly researched. Two interesting, though very different ,examples are: M. Rudwick, 'The Emergence of a Visual Language for Geological Science 1760-1840', *History of Science*, 14, 1976, 149-95, and G. N. Gilbert, 'Referencing as Persuasion', *Social Studies of Science*, 7, 1977, 113-22.

7 T. S. Kuhn, *The Structure of Scientific Revolutions*, Chicago 1970, 43-51, and M. Polanyi, *Personal Knowledge: Towards a Post-Critical Philosophy*, London 1962, 269-324.

and display of scientific arguments, and there appears no advantage in withholding good arguments from publication. Therefore, an analysis of the manner in which scientific texts are composed argumentatively will be central to any project concerned with the nature of scientific evaluation. The nature of arguments, when viewed as the social accomplishment enshrined in a scientific paper, merits analysis.

Hitherto, sociological investigations of scientific papers have been of two principal sorts. On the one hand Gusfield has argued that scientific writing necessarily incorporates rhetorical elements, aimed at persuasion.[8] He concludes that these elements are of such importance that the apparent objectivity of scientific dispute must be compromised, and that the unavoidability of stylistic influence demands that the epistemology of scientific knowledge be rethought. On the other hand, Gilbert and Mulkay have illustrated the way in which social accounting is evident in bio-chemical research papers.[9] They have shown that the presentation is of a particular form: commensurate with a scientistic view of knowledge, and generally operating to present the author's knowledge-claims as unproblematically related to observed data. They conclude that some overall accounting procedure is essential for allowing scientists to put forward a consistent, orderly version of their experiences. Gilbert and Mulkay also demonstrate that alternative ways of accounting for the same knowledge-claims are routinely employed under other circumstances. Thus, under conditions of informal conversation, scientists are far less reliant on an inductivist accounting repertoire; they stress the importance of such factors as practical skills and intuition. Such findings allow one to begin to correlate accounting repertoires with social contexts.[10]

My analysis will be similar in certain ways to both of these forms of investigation. Like Gilbert and Mulkay, I shall seek to examine the accounting procedures drawn on by the author of a scientific paper, and it will be my aim to examine the fine structure of the social accounting present in a single formal scientific text. On the other hand, like Gusfield, I shall be interested in the apparent persuasive or (as he would suggest) rhetorical orientation of the accounting. In this way, I shall attempt to disclose the rationale which connects specific forms of accounting to particular contexts of scientific writing, and the tasks performed by those writings. My interest will be in the systematically persuasive structure of the accounting procedures, rather than in isolated 'stylistic' features. The paper I shall be concerned with is Kirwan's examination of Hall's proof of the Huttonian theory of the earth.[11] It will be impossible to reproduce the paper, and my purpose will not be served by considering only, say, the introduction or some other part, as Anderson[12] and, to a lesser extent,

8 J. Gusfield, 'The Literary Rhetoric of Science', *American Sociological Review*, 41, 1976, 16-34.

9 G. N. Gilbert and M. Mulkay, op. cit.

10 A more extensive analysis of the connections between social context, scientists' accounting procedures, and the manner in which scientific arguments are presented and evaluated can be found in my forthcoming doctoral dissertation, *Contexts of Evaluation: A Sociological Study of the Treatment of Geological Knowledge in the Early Nineteenth Century* (University of York, England).

11 R. Kirwan, 'Observations on the Proofs of the Huttonian Theory of the Earth, Adduced by Sir James Hall, Bart.', *Transactions of the Royal Irish Academy* (henceforth *TRIA*), 8, 1802, 3-27.

12 D. C. Anderson, 'Some Organizational Features in the Local Production of a Plausible Text', *Philosophy of the Social Sciences*, 8, 1978, 113-35, and G. N. Gilbert and M. Mulkay, op. cit.

Gilbert and Mulkay have. Clearly, a certain amount of 'glossing' will be involved, but the paper can be checked in a number of major libraries and is relatively short.

2. THE BACKGROUND AND A GLOSS OF THE PAPER'S CONTEXT

For the first volume of the *Transactions of the Royal Society of Edinburgh*, which appeared in 1785, Hutton prepared a version of his theory of the earth which he had apparently been formulating privately for several years.[13] This theory of the earth is today regarded, at least in Britain, as one of the two greatest milestones in the history of geology. It is claimed that Hutton's theory contained important specific insights, but more fundamentally also the earliest methodologically sound, naturalistic approach to the study of the earth.[14] He considered that the earth formed a perpetual apparatus for the refurbishment of new continents, and the maintenance of a fertile world.

The world's history was, to all intents and purposes, cyclical, and there was no general trend evident in its operations. He considered that all currently visible strata were the product of pre-existing rocks and that there were no original formations visible, only the remains of a succession of former worlds. Earth science was not therefore to be concerned with the direction of history, but was to study the processes of destruction and re-creation which maintained the state of the world. He argued that rocks were fashioned from the products of former erosion, and that the agent responsible for the consolidation of the fragments (generated by erosion and submarine deposition) and for the uplift of the consolidated rocks by expansion, was the heat of a central fire. He viewed volcanoes as representative of this fire, and referred to them as outlets for the pressure generated by the central, thermal action. Lavas he saw as formed by the melting of rocks, and he identified the disputed rock-type basalt as the lava of extinct volcanoes. Certain basalts appeared to be interstratified with ordinary secondary rocks, and these he presented as subterranean lava flows. He went further and identified all compact (we would say crystalline) rocks as of fiery origin, and included granite as a form of igneous rock.

Many features of Hutton's theory were opposed to the taken-for-granted notions of other scientists, generally described as Neptunist theorists. The Irish scientist Kirwan subscribed to this opinion, and opposed Hutton on nearly every issue. Whilst many varieties of Neptunism were available and were contested, I shall describe only Kirwan's general views. Kirwan responded to Hutton's paper in 1791 in rather strong terms, and, it is reported, provoked Hutton into an extension and systematization of his theory which was published in 1795.[15] Kirwan made further comment in 1797 and 1799.[16] Generally, he regarded earth history as directional, and perceived it to be short enough to allow original mountains and rocks still to be existent. These were the primary rocks, and

13 J. Hutton, 'Theory of the Earth', *Transactions of the Royal Society of Edinburgh*, (henceforth *TRSE*), **1**, 1788, 209-304. On the background to the presentation of this theory see E. B. Bailey, *James Hutton—the Founder of Modern Geology*, Amsterdam 1967, 27-31.

14 A claim advanced, for example, by J. O'Rourke, 'A Comparison of James Hutton's *Principles of Knowledge* and *Theory of the Earth*' *Isis*, **69**, 1978, 4-20.

15 R. Kirwan, 'An Examination of the Supposed Igneous Origin of Stony Substances', *TRIA*, **5**, 1791, 51-81, and J. Hutton, *Theory of the Earth: With Proofs and Illustrations*, Edinburgh 1795, 2 vols.

16 R. Kirwan, 'On the Primitive State of the Globe and Its Subsequent Catastrophe', *TRIA*, **6**, 1797, 233-308, and *Geological Essays*, London 1799.

included granite and gneiss, both of which Hutton ascribed to the direct influence of heat. Kirwan viewed them as precipitated from a primeval ocean by obscure and complicated chemical reactions. He believed that these primary rocks had subsequently undergone erosion and with the remainder of the 'brine' deposited the secondary rocks (which Hutton saw as precipitated erosional matter consolidated by heat). Heat was largely irrelevant to Kirwan's vision, and volcanoes were attributed to the influence of local phenomena such as the combustion of coal seams and subterranean chemical reactions. Basalt was a chemical precipitate, but from its stratigraphical position not primary. It was due to unusual local chemical reactions in the secondary brine. It was similar in appearance to lava but was genetically different. However, even lava was not really extremely hot, for it flowed as the result of a partial liquidification, and not full fusion, since, Kirwan believed, fully fused rocks produced only glasses on cooling. This he knew had been the experience of all mineral analysts who had melted rocks with a blow pipe.

The paper to be analyzed here was published in 1802. It concerns the arguments which had been put forward by Sir James Hall in defence of Hutton's general igneous position.[17] Specifically Kirwan is responding to two papers by Hall; one on the origin and nature of granite, and the other on whinstone (viz. basalt and related rocks) and lava.[18] Hall argued that, from field evidence, certain granites did not appear to be primary since they had flowed in veins into secondary rock. Secondly, having shown signs of flow, one must accept that they were of molten origin. He argued that slow cooling might account for their non-glassy character even if they were once fully fused. However, Hall acknowledged one apparent problem. Granite is composed (minimally) of quartz and feldspar with a little mica. Quartz is an extremely infusible substance whereas feldspar is easily melted. Yet, some granites occur where large crystals of feldspar are set in a matrix of quartz. At first sight, it is hard to see how this could result from cooling. Hall's hypothetical answer was that quartz may dissolve in feldspar so that on cooling, the two minerals will begin to solidify contemporaneously. Under such conditions feldspar crystals might emerge. Further, mica might not dissolve in feldspar, and being of a higher melting point, could form first. Hall suggested that his hypothesis could be tested experimentally, but appears never to have carried out these experiments.

His second paper was founded on experiment. He took a number of whinstones (a Scottish term for basalts and related rocks), and melted them; cooling them rapidly he got a glassy substance; cooling them slowly, he got a stony substance (dubbed a crystallite), which, he claimed, was very similar to the original. He argued that, in the absence of a Neptunist explanation of whinstone formation, this demonstration that it could form from the melt (from a magma) constituted evidence that it really must have originated thus. He then went on to analyze lavas, and claimed that the glasses and crystallites they produced were extremely like the whin ones. He argued for the basic identity of whins and lavas. A small and rapidly cooled lava sample, which he had collected from a lava flow on Vesuvius, was originally glassy, and this allowed him to say that lavas were truly molten, but their bulk allowed them to cool slowly and pre-

17 For an account of Hall's support for the Huttonian theory see V. A. Eyles, 'Sir James Hall, Bt. 1761-1832', *Endeavour*, **20**, 1961, 210-16, and C. S. Smith, 'Porcelain and Plutonism', in C. J. Schneer (ed.), *Toward a History of Geology*, Cambridge, Mass. 1969, 317-38.

18 J. Hall, 'Observations on the Formation of Granite', *TRSE*, **3**, 1794, 7 and 8-12, and 'Experiments on Whinstone and Lava', *TRSE*, **5**, 1805, 43-75.

vented them from becoming vitreous. The real igneous origin of lavas thus demonstrated, he argued that no doubt could be harboured about the origin of whinstones. Hall also generated one other type of stone from the melt which he described as a 'liver crystallite'. This was produced by slow reheating of the glass, and was intermediate between a glass and what Hall regarded as a fully crystalline stone. He claimed that this substance might constitute the 'slaggy' crust of lava flows. Hall concluded his paper with a cautious claim to have vindicated Hutton's theory by removing the principal obstacles to its central tenets.

3. APPROACH TO THE PAPER AND GLOSS OF ITS CONTENT

The subject of this analysis is, what does Kirwan's paper, in response to Hall, do; and how does it argue and seek to convince? The paper starts out committed to a particular viewpoint:[19]

> As some positions, which I laid down in my examination of Dr. Hutton's theory of the earth, may seem questionable from the ingenious reasoning employed by Sir James Hall in the third volume of the Edinbrugh transactions to corroborate some of Dr. Hutton's assertions, and may even be thought inconsistent with some of the curious results that occurred in the highly interesting experiments instituted by the worthy Baronet, inserted in the fifth volume of the Edinburgh transactions (a printed transcript of which he has had the goodness to send me) I think it a duty incumbent upon me to examine both the general reasoning employed by him, and the consequences fairly deducible from his experiments; fanciful and groundless as the Huttonian theory seems to me to be, it may, like the researches for the philosopher's stone, be highly useful by suggesting new experiments. [P. 3.]

The first half of Kirwan's paper is concerned with Hall's hypothesis about granite, the second is about Hall's work on whins and lavas. Kirwan concludes that Hall's

> experiments afford no confirmation of the high degree of heat attributed to volcanoes, and still less to the many hypotheses gratuitously heaped on each other by Doctor Hutton, or to the volcanic origin of whins or traps, for the reasons already assigned. [P. 27.]

The falsity of the Huttonian theory is, as far as Kirwan is concerned, never in question. The conviction of Hutton's falsity both provides Kirwan's premiss in writing the paper, and must appear as its conclusion. The argument of the paper is not therefore universalistic, since there is no attempt at uncommitted appraisal; yet for all that, its content is not redundant. It suppresses any doubts about Kirwan's prior treatment of Hutton's work which might have been occasioned by the evidence mustered by the 'worthy Baronet', and by showing that Hall's data offers 'less' than no confirmation of Hutton's hypotheses, provides positive evidence in support of Kirwan's earlier contentions. It is the manner in which this work is done by Kirwan, in the course of the text, that I shall consider.

Gilbert and Mulkay have suggested that a recourse to forms of social accounting is inseparable from a scientific text, and I propose to identify the systems of accounting called upon by Kirwan.[20] It will be essential in addition to identifying these systems, to situate them in the orderliness of the text. I will suggest that there are stages to Kirwan's argument which can be isolated by means of the different forms of social accounting which he calls upon. Nonetheless, in a

19 R. Kirwan, op. cit., 1802 (note 11) 3. Henceforward, the references to this paper will be given by page numbers inserted in the text following quotations from Kirwan.

20 See Gilbert and Mulkay, op. cit., for examples of accounting in scientists' writings.

similar fashion to Gusfield, I wish to claim that the study of this paper must concern itself with the orientation towards persuasion. I will claim that Kirwan's argument is dependent on various forms of accounting procedures. My method for analyzing the paper will consist in re-reading Kirwan's text to identify the kinds of claims he makes about Hall's ideas, Hall's experiments and the competing theories under discussion. I shall take apparent literal claims seriously and seek out seemingly contradictory assertions, which Kirwan makes about Hall's work. An argument is normally read sequentially; it will be one of my aims to reveal the dependence of the argument upon textual ordering. I hope to account for certain statements in the text about Hall's work and the state of geological knowledge, which appear clearly flawed in historical retrospect, and to comprehend the meaning of such inconsistencies. In the case of an historically distant text there are problems of an empirical nature in readership, concerning who can be supposed to have known what, and the tendency to read between the lines. My concern with the text is intended to be rather more general than this; I aim to approach the text as an apparently 'degree zero' account, and to reveal the complex organization which sustains this appearance.

Before proceeding with the analysis it will be beneficial to outline Kirwan's paper. Kirwan first discusses the problem Hall deals with: how it is that large crystals of feldspar are sometimes seen in granites with a fine quartz matrix. He quotes Hall's hypothetical answer, that the quartz may dissolve in the feldspar as salt in water. Kirwan then challenges this explanation on several grounds: that frozen water does not form regular crystals, whereas feldspar does; that often there is too little feldspar in granite to dissolve the quartz; that the feldspathic granites are probably atypical; that quartz remains undissolved in all actual attempts to melt granite; that, unlike salt and water which are entirely separate, quartz is a constituent of feldspar so that feldspar is already saturated with quartz; and that the addition of salt to water makes ice melt sooner, whereas adding quartz to feldspar makes it more infusible.

He then moves on to consider whether the observation that slowly cooling glass becomes stony, provides a model for the cooling of granite. Glass, he claims, is a unity of a siliceous earth with an alkali. Only rapid cooling prevents the earth from recombining when cold. Thus, slow cooling necessarily leads to the destruction of the glassy structure. The stoniness is brought about by massy solidification of the quartz and the evaporation of some of the alkali. Granite is an aggregate stone, and thus a different case. He maintains that the quartz would come out first and should form crystals, yet these are rare in actual granites. Moreover, cooling should lead to sequential deposition and to banded rocks, these are extremely uncommon. Usually all the parts lie intermixed. Anyway, granites contain water which one would not expect were they once molten. Precipitation from water, by contrast, is not successive but contemporaneous, for granite results from its various chemical constituents, attracting each other and forming an insoluble compound.

Moving on to whinstones, Kirwan contends that they are not fully crystalline rocks, and that the different ranges of melting points which Hall obtained for the originals and 'crystallites' are to be accounted for by separate causes not a single one. He then describes the first of Hall's experiments in which Hall claimed to produce a glass from melted whinstone, and then, by heating the glass, produced first the liver crystallite and secondly on re-melting and slow refrigeration a crystallite. Kirwan suggests that one can judge Hall's work either on the basis of its achievements or in relation to its power as a proof of the Huttonian theory. While he allows that his strict business is the latter, he elects to dally a little with the former.

He points out that many researches on molten glass have discovered that it is constantly decomposing whilst molten, and that it eventually becomes relatively infusible because of the evaporation of alkali. Thus Hall's (liver) crystallite is not new, since it constitutes solidification due to continued heat. Hall's new discovery is simply that stones, once molten, can become stony again on slow cooling. The solidification due to heating is only coagulation not a real crystallization, since no fluidity was attained. Kennedy's mineral analysis (printed with Hall's paper in the *Transactions of the Royal Society of Edinburgh*) suggests that whins contain alkali, and this, Kirwan sees, as confirming the interpretation of coagulation.[21] Nonetheless, there is still the resumption of stoniness to account for: this Kirwan does by showing that the whinstones never melted to a real glass. Instead, they formed only an enamel without total dissociation of their constituents. The silica content is too great to be vitrified by the small amount of alkali they contain. Slow cooling allows silica and alkali to separate, as in the case of glass discussed above, and so the stony character is a natural concomitant of the cooling. The other minerals cannot occasion the melting of the silica, for they only combine with it at high temperatures.

The stony appearance from a melt is thus explained as natural, but does this support the Huttonian theory? Kirwin's answer is that it affords no support, only confusing the problem by artificially creating the impression of an aqueous crystallization by the manipulation of heat. The field position of natural whinstones completely disqualifies any possibility that they were genuinely molten, and anyway, this reasoning applies only to whinstones which, compared to aqueous granite and limestone, constitute a negligible proportion of the earth's crust. The apparent similarity of whins and lavas (which he admits do have some sort of association with heat) conceals their real difference. This can be understood when one allows that whins, but not lavas, contain zeolites and calcareous minerals (unstable at high temperatures) and lose, according to Kennedy, both water and alkali at heat. This suggests they never were hot. There are also physical differences which Hall passed over, in terms of porosity, hardness and colour. Then, Kirwan notes that a whinstone in the possession of the Royal Irish Academy, contains a fossil, indicating its certain acqueous origin.

Finally, turning to lavas, Kirwan suggests that Hall has not proven that they are absolutely molten, for the mechanism that Kirwan proposes (that is, that they are borne by volatile fluids), receives no disconfirmation from Hall. Nonetheless, lavas are somewhat heated and he allows that Hall has explained their stony appearance admirably. However, this affords no support to Hutton's idea that whinstones and granites are of an igneous origin. This speculation remains as groundless as ever.

4. READING SCIENCE AND THE STYLE OF ARGUMENTATION

As I suggested above, Kirwan's text would have been read sequentially for its overall argument. Even at the beginning of the nineteenth century, there was a vast amount of literature to be read by the scientist. Davy complained that there was too little time to read everything necessary, and is alleged to have thrown away his books once he had read them.[22] Thus, the close attention which can be

21 R. Kennedy, 'A Chemical Analysis of... Whinstone... and Lava', *TRSE*, **5**, 1805, 76-98. Kennedy was a friend of Hall. His analyses were based on the same rock samples as Hall had used for his own experiments (Hall, op. cit., 1805), and their papers were printed together in the *TRSE*. They were able to interpret their results as mutually supportive.

22 This tale is related in D. J. de Solla Price, *Little Science, Big Science*, New York 1965, p. 15.

directed to Kirwan's text is foreign to the initial conditions of its probable readership. As such rereading becomes, as Barthes suggests, a method:[23]

> Rereading, an operation contrary to the commercial and ideological habits of our society, which would have us 'throw away' the story once it has been consumed ('devoured') so that we can then move on to another story, buy another book, and which is tolerated only in certain marginal categories of readers... rereading is here suggested at the outset, for it alone saves the text from repetition (those who fail to reread are obliged to read the same story everywhere), multiplies it in its variety and plurality; rereading draws the text out of its internal chronology... and recaptures a mythic time.

There are two striking observations made during rereading which might be said to constitute the first compromise of any stylelessness in scientific writing. These can be described as the stylistic and the semantic issues. The former relates very closely to what Gusfield terms the 'rhetoric' of science. The issue here is that if the text is aimed to persuade by virtue of its style or non-essential features, and if such persuasion is thought to be regularly operative, then the idea of scientific advance being constituted by disinterested appraisal is undermined. In the opening paragraph of Kirwin's text, cited above, there is the comparison with the 'philosopher's stone', and also the devaluation of Hall's criticisms by suggesting, in the opening words, that it is only 'some positions' that Hall can challenge. One can only conclude, as Barthes does in the context of a realist literature, that style-less writing is virtually unimaginable.[24] This does not prove by any means that scientists are not capable of dis-entangling persuasion from what is taken as good argumentation. Their elision in the text provides no evidence at this level.

A further aspect of style does exist, which enters at a directly semantic level. One could describe this as the level of editing. Kirwan's text does not include a discussion of Hall's lava sample scooped from a Vesuvian flow, nor does he use the term 'liver crystallite' to make the distinction between the two types of solid produced from the glass, nor mention the issue of the slaggy crust on lava flows. Yet, widespread though Kirwan's editing appears to be, on the basis of my retelling of the text, it is clear that Hall is equally implicated. In the paper which Kirwan criticizes, he omits any mention of the water in whinstones, and evaporation from molten glasses, even though the first of these is brought out by Kennedy, and the second was publicized (according to Kirwan) by Lavoisier.[25] How is one to deal with this semantic level of style; what is it right or proper to include? I suggest that rather than see these stylistic features as a simple 'range' of isolated persuasive (or in Gusfield's terms, rhetorical) devices which threaten pure argumentative rationality, one should relate them to the essential accounting devices of the textual discourse as a whole.

As an example: Kirwan includes the following discussion of Hall's hypothesis concerning the crystallization of granite from the melt; which, according to Kirwan, would necessitate the sequential formation of its constituents:

> among the immense masses of granite that have been observed and examined in various parts of the globe, not above half a dozen have occurred in which the three constituent parts of granite were regularly crystallised, very few in which distinct layers of each were seen, and *none at all* consisting of distinct regular crystals of each, superimposed upon each other. [P. 10.]

23 R. Barthes, *S/Z*, London 1975, pp. 15-16.
24 Ibid., 1968 (note 3).
25 Kirwan, ibid., 1802 17. His point is that Hall should, or at least could, have made reference to these issues.

There are a number of textual features arranged to assist in persuading the reader that Hall's case is untenable. The style is crisp: references are omitted; there is only a casual contrast between the immensity of observed granites and the 'very few' and 'half dozen' which correspond with Hall's anticipations; and the repetition structure of the grammar intones the extent of Hall's error. Rather than interpret these as particular recourses to rhetorical persuasion (which could be omitted from a properly objective version) I suggest that one should read them as denoting, by their very lack of specification, the blatant error of Hall's claim. The lack of references is justified by, and indicates, the obvious falseness of Hall's claims. Only a serious difficulty would merit a full scientific rebuff; and conversely, to treat Hall's claim in all sobriety would be to over-rate it and to demonstrate one's own naivety. I suggest that it is the systematic presence of various ways of indicating and accounting for disagreements and falsity that characterizes the general style of scientific discourse. This systematic mode of presentation provides a solution to the question of why Kirwan appears to fluctuate between contradictory styles of writing, between laborious, formal disputation and literary flourishes. A systematic reading of this scientific text will seek to resolve such surface contradictions in scientific style, and to elucidate the general subordination of individual stylistic elements to the argumentative purpose of the whole paper.

5. ARGUMENTATIVE STRATEGY AND MODES OF ACCOUNTING

As I outlined above, on the basis of historical background and his introduction, Kirwan's concern is to demonstrate that Hall's articles do not provide adequate support for the Huttonian theory. There are two parts to Kirwan's paper, each addressed to one of Hall's articles. Thus, Kirwan's text is singularly directed towards the inadequacy of Hall's work. According to Gilbert and Mulkay, the formal scientific text exhibits a commitment to a scientistic rationality. A corollary of this claim is that in such texts, correctness will be associated with inducing from facts, whereas falsity will be associated with some sort of inadequacy or impropriety. One finds that in both parts of Kirwan's text correct belief is textually presented as arising directly from the facts: this is evidenced in two ways. Either one shows that everyone has found a common result so that it cannot possibly be artifactual, or else one imputes agency, in a grammatical fashion, to facts (or expresses their indubitability otherwise). Thus:

> It has been observed by all those who have attended to the formation of common glass that... it is in a constant state of decomposition. [P. 17.]

> The observation on glass here mentioned is perfectly just and has often been repeated. [P. 7.]

> Various attempts have been made to fuse granites... but in almost all... the quartz remained unfused. [P. 6.]

> [G]ranitic masses not infrequently occur, in which it is evident that the mica must have crystallised contemporaneously with the quartz. [P. 10.]

> That in the dry way argil unites to silex in temperatures below 150°, only in consequence of the previous union of the silex to the alkali, is clearly deduced from this fact, that if the alkali be absent the union will not take place in temperatures below 150°, whereas it takes place by Sir James's own experiment at temperatures below 100°. [P. 21.]

> [The environments of whinstones] forbid us to entertain any doubt of their production in the moist way. [P. 25.]

When it comes to identifying falsity on Hall's part, there is a wider range of procedures available. I suggest that they can conveniently be arranged into six, somewhat overlapping groups:

1. Connection with a theoretically inadequate position.
2. Empirical baselessness.
3. Foundation in improper activity.
4. Misapplication of terms.
5. Overlooking of important (often obvious) material.
6. Employment of bad reasoning.

I shall illustrate each of these accounting modes in turn, before moving on to consider their organization and overall consequence.

1. In the introduction, quoted above, Kirwan announced that the Huttonian theory was 'fanciful', and only of interest in the same way as the 'philosopher's stone' (p. 3). Similarly, at the close of the first section, he notes that 'Sir James has since very wisely declined justifying his theory of the formation of granite by fusion' (p. 13). While no reason is given for Hall's behaviour, it is certainly linked to the falsity of his original commitment. Whatever Hall's reason for desisting (and he furnishes none), Kirwan presents it as an admission of error. Again, Kirwan's closing argument (quoted above), is that Hall's work affords 'no confirmation' (p. 27) of the Huttonian position; nonetheless, he can confidently claim that Hutton has 'gratuitously heaped' hypotheses together into his theory, whereas only extra-textual information could establish this. On three other occasions in the text Kirwan puts forward claims countering the Huttonian theory which do not bear exactly on Hall's work:

> those [granite particles] we are acquainted with . . . essentially contain some particles of water. [P. 11.]

> The *natural* whins, according to Dr. Kennedy's statement, lose five per cent. of water and other volatile matter when heated to redness. [P. 23.]

> the college of Dublin now possesses fragments of basaltic pillars in which marine shells are imbedded. [P. 25.]

Each of these points is present as establishing that granites and whinstones (basalts) could never have been molten since no water could have been retained, nor a calcareous shell prevented from decomposition, at the elevated temperatures required even to soften these rocks. From the beginning, and apart from any of Hall's particular arguments Kirwan knows, and is willing to state, that Hutton's theory is false. The paper by Kirwin is not a consideration of opposing positions but the deliberate dismissal of one possible opinion. Because the untruth of Hutton's theory is acknowledged from the outset, and is anchored in facts (like the water content of crystals of feldspar), there is literally no contest. The narrative of Kirwan's text holds no surprises in the domain of truth, and the *a priori* falsehood of Huttonianism underlies the text's arguments like a safety net. Nonetheless, Kirwan *appears* to deal with Hall's claims and to conclude *on the basis of his discussion*, that Hall's work does not assist Huttonianism. This virtual duplicity is carried through by the other five modes of accounting.

2. While strategy one is concerned with the overall untruth of the Huttonian theory and the consequent folly of any work supporting it, the second strategy aims to demonstrate the particular errors of Hall's work. Baselessness of various kinds is presented in a number of ways in the following extracts:

> *A* Sir James Hall [is] convinced from various observations that granite had once flowed in a state of fusion. [P. 4.]

Yearley, Steven, ***Textual Persuasion: The Role of Social Accounting in the Construction of Scientific Arguments*** , Philosophy of the Social Sciences, 11:3 (1981:Sept.) p.409

B Neither was Saussure a stranger to such granitic veins, but he accounts for their origin very differently from Sir James. [P. 6.]

C Some resemblance betwixt [whinstones] and lavas has long been noticed. [P. 23.]

D Sir James says I have supposed substances that have left no trace of their existence. Other observers however discovered these traces, as Dolomieu and Fabroni.... [P. 26.]

E In far the greatest number of granitic masses the three...constituent masses [feldspar, quartz and mica] lie intermixed with each other in the most confused and irregular manner, and without any appearance of regular crystallisation. [P. 10.]

F if any of the fore-mentioned component parts of granite can be said to be held in solution by the high heat of the solvent, it is surely the quartz; now quartz is scarce ever found regularly crystallized when forming a component part of granite, as all mineralogists attest and is a matter of universal observation. [P. 12.]

The principal mode (seen in *B, D, E,* and *F*) relies upon demonstrating that Hall's empirical information is inadequate. This is either done by contrasting his supposed observations with those of competent scientists (like Saussure) or with a more general facticity, as in *E*, sometimes represented by 'universal' observation, as in *F*. In example *A*, the baselessness finds a purely textual, one could say stylistic, presentation. The lack of foundation for Hall's belief is indicated by the omission of Hall's reasoning. His belief is shown to rest on mere opinion. Textual features also play a part in establishing the empirical inadequacy of Hall's claims, since precise references are opposed to allusions to what 'Sir James says'. In example *C*, it is the novelty and strikingness of Hall's observational basis that it attacked. The antiquity of an observation presumably has no relation to its truth status, yet Kirwan devalues Hall's work by indicating that it has been observed all along, and only in Hall's hands receives an aberrant interpretation.

By contrast, Kirwan occasionally provides a textual grounding of Hall's opinion, although only in cases of agreement. On page seven, Kirwan relates in detail Hall's observations on cooling glass and comments that 'The observation on glass here mentioned is perfectly just and has often been repeated' (p. 7). Truths are permitted a texted base in accurate observation, symbolically manifest in precise recounting, and thus achieve a contrast to error and mere opinion. Nonetheless, even in this case Hall is deprived of ultimate credit since Kirwan presents his observation as perfectly familiar.

3. The most varied critical approach to Hall concerns his scientific actions and competences. In its most basic form, Kirwan provides an amplification and correction of one of Hall's claims, thus:

[Hall has] applied himself to experiments on various species of *whin*, a denomination which in Scotland comprehends grunstein, basalt, trap, wacken, and porphyry. [P. 13.]

Similarly, on page eighteen Kirwan spells out the difference between crystallization and coagulation, indicating that Hall is ignorant of such fine, but consequential distinctions. However, identifying improper action on Hall's part can be employed to do more than simply defame him. It is suggested that Hall performed his trials insufficiently, not recording crucial aspects of the data. Hall made no comment on the fact that glass 'from the instant that it enters into fusion... is in a constant state of decomposition' (p. 17). Equally in one trial the artificial whinstone was said, by Hall, to be so like the original that:

it would be difficult or perhaps impossible to distinguish them, *but for a few minute air bubbles, distinguishable in the artificial*. Neptunists will however consider this as a leading character of distinction. [P. 24, original emphasis.]

In these cases Hall has not provided important information or has underplayed contestable distinctions, but Kirwan goes further in suggesting that Hall has manipulated the reporting of results:

If the specific gravity and other characters of both [natural and artificial] were given, it is probable that other differences might be perceived. It is only in these characters that any difference can be expected, as the internal composition must be the same in both. [Pp. 24-25.]

In this case Hall has omitted the *vital* information, and has supplied only data which is tautologous. Kirwan's point is that the composition *must* be the same if one is only talking about a change of state, so Hall has deleted the really telling details. Hall's transgressions on this point are contrasted with the correct behaviour of M. Pictet and his associates in the Genevan Natural History Society who were sent some of Hall's samples and who published a discussion of them including descriptions of physical characters (hardness, density and the like.[26] Kirwan laments that:

As Sir James has neglected giving a *complete* account of the external characters of the natural whins, which were the subject of his experiments, as also of the regenerated or artificial whins derived from them,... it is difficult for me to compare them with each other. and would indeed be impossible if some account of them had not been given by Mr. Pictet in his valuable Journal Britannique. [P. 24. The journal title is unitalicized in the original.]

This mode of comparison between Hall's infelicities and the adequate and proper actions of others is used to particular effect when Hall's work is contrasted with the work of Dr. Kennedy. Kennedy had published mineralogical analyses of the samples used by Hall in a paper immediately following Hall's article.[27] Kirwan's description of Kennedy's work as 'ingenious, accurate and skilfully conducted' (pp. 14-15), and the suggestion that he 'bids fair to rival the excellence attained by the greatest masters of that sublime and difficult art' (p. 15), serve both to impugn Hall's ability and to prime a later device. Four pages later, one reads that Kennedy has 'discovered that all these [viz. Hall's] whins contain ten per cent. of soda' (p. 19); and another four pages on, it is stated that '*natural* whins, according to Dr. Kennedy's statement, lose five per cent. of water and other volatile matter' (p. 23).

It is precisely by virtue of Kennedy's technical excellence that his testimony securely contradicts Hall's, and this is vital for Kirwan at the two junctures cited above. For the decomposing soda causes the solidification that Hall wishes to attribute to crystallization, and the presence of water suggests that whins had an aqueous origin. Perhaps it is not too exaggerated to suggest that it is precisely the textual separation of the testimony to Kennedy's skill, and the use of Kennedy to contradict Hall, that prevents the device from being disruptively visible on a first reading. Certainly the sequential arrangement of these items is essential.[28] In

26 M. Pictet's descriptions were available to Kirwan through their publication in the *Journal de Physique, de Chemie et d'Histoire Naturelle*, 48, 1799, 313-20.

27 Kennedy, op. cit. (note 21).

28 For considerations on the importance of ordering in other 'everyday' activities see H. Sacks, 'An Analysis of the Course of a Joke's Telling in Conversation', in R. Bauman and J. Sherzer (eds), *Explorations in the Ethnography of Speaking*, Cambridge 1974, 337-53.

general, one can say that in terms of accounting in the text, Hall is revealed to have offered inadequate definitions of terms and to have a rather poor grasp of the fine points of mineralogy. Secondly, he is shown to have provided insufficient data on his experiments, and even to have omitted crucial, and possibly damaging, facts about the samples. Thirdly, it is suggested that he is of limited experimental ability.

4. Redefinition of Hall's usages is foreshadowed in Kirwan's glossing of whinstone as grunstein, etc.; moreover, it shares the same force, in that it highlights Hall's incompetence and can also imply that Hall's arguments owe any plausibility they may have to the fudging of separate concepts. However, it is often directed to a more immediately theoretical end. For example, Hall's observation that in some cases granites have large feldspar crystals with which Kirwan concurred, is nonetheless undermined by Kirwan's observation that 'Granites in which such crystals are observed are called porphyraceous granites, and from that very circumstance judged... not to be ancient granites' (p. 5). Here the categorization of granites is employed effectively by Kirwan to correct and to impugn Hall. A similar instance occurs two pages later where Kirwan invokes a more precise definition of 'glass' to repudiate Hall's suggestion that granites might exhibit thermal behaviour similar to glasses:

> [Hall's observation on glass] is perfectly just... ; but the analogy betwixt this case and the formation of granite... is far from being accurate. Glass consists of a simple earth, namely, the siliceous united to an alkali. [P. 7.]

The notion of glass is presented as being *chemically* specific, and not simply a question of *physical* appearance.

Kirwan also challenges Hall's concept of crystallization on a number of occasions (pp.5, 14 and 18), arguing that Hall's specimens present a '*nisus* towards crystallization [rather] than perfect crystals, which latter the term crystallization generally applied would lead us to expect' (p. 14). In this case the force of the redefinition at the theoretical level is very minor, for according to Kirwan neither the original, nor the artificial whinstones is really crystalline. Later the same redefinition is used more pointedly:

> According to every sense in which this term has ever been employed, whether that operation was perfect or confused, [this covers the aforementioned nisus] it denotes at least an union of particles previously dispersed through a liquid medium.... in Sir James's experiment we find the consolidation to take place in a fragment of glass, which still retained its solid state,... this consolidation must therefore evidently have arisen from some internal change in the constitution of the glasses in which it was observed. [P. 18.]

Since crystallization is defined by Kirwan as dependent not on whether the resultant appears crystalline, but upon the nature of the reaction which brought about the condition, then Hall's 'crystallizations' are peculiar and unlike the natural production of crystals. In a further case, where Hall claimed to have fully melted the glass and then produced crystals on refrigeration, Kirwan reinterprets him in a different fashion:

> To account for this change it is proper to remark that though whins are said to be vitrified in a high degree of heat, yet this is not rigorously true, for in that case they should afford a transparent glass whose fracture would be perfectly polished with a strong lustre,... whereas in truth they melt only into an *enamel* nearly approaching to the perfect vitreous state; even the bottles made of them are nothing more; and hence their superior hardness. [Pp. 19-20.]

The most interesting feature about these redefining devices is the way in which they re-direct attention onto new theoretical issues. Thus, they do not merely

highlight Hall's errors but suggest a corrective interpretation. This feature is shared with the next strategy.

5. The device of pointing out what Hall has overlooked has many similarities to the correction of Hall's empirical errors (discussed under 'baselessness'). However, in the case of 'overlooked material' new information is supplied, which Hall could or should have seen but did not. Thus, here too there is a potential for redirective interpretational work. The oversights may be at a variety of levels as in the following instances.

A [Concerning one of Hall's analogies]; in the first place water (to which feldspar is here assimilated [viz. compared]) is never regularly crystallized when frozen by excessive refrigeration, though indeed vapour may. [P. 5.]

B Again, . . . the felspar should always be in the larger and quartz in the smaller proportion to each other, . . . yet in Switzerland this does not happen, as Mr. Hoepfner attests. . . . [P. 5.]

C Even on the supposition that distinct crystals of quartz, felspar and mica could be produced by fusion, they still would be far from resembling those we are acquainted with, which essentially contain some particles of water. [Pp. 10-11.]

D [natural whinstones] contain much more silex than can be completely vitrified by the small amount of lime and argil that enter into their composition. [P. 20.]

E It has long been observed by all those who have attended to the formation of common glass (and is indeed evident from the fumes that float over its surface) that from the instant it enters into fusion, it is in a constant state of decomposition. [P. 17.]

F I must farther [sic] add that the upright state in which many [natural whinstones] exist, for instance, the basaltic pillars of Staffa, . . . the basis they rest on, sometimes granite, sometimes gneiss, sometimes coal or limestone, and the total absence of all signs of the operation of fire, forbid us to entertain any doubt of their production in the moist way. Nay the college of Dublin now possesses fragments of basaltic pillars in which marine shells are embedded. [P. 25.]

Items *A* through *D* suggest that Hall overlooked considerations which indicate that the Plutonist theory is untenable and which favour the Neptunist position. For example, in *C* the water aligns with an aqueous origin; and *B* and *D* suggest that a chemical process lies behind the formation of rocks. In each case the textual claim is that Hall has overlooked some information, and this is especially clear in *E*, where the phenomenon is *evident*, so that Hall must either be extremely incompetent or never have observed glass properly. Moreover everyone who has *attended* to the process is of Kirwan's opinion. In these cases of oversight, where Hall's claims can be so radically transfigured and Hall himself so disparaged, there is no specific account of why Hall went wrong. There is no evidence for Hall's failings outside of the content of the text; one might say that only by virtue of the narrative is Hall's incompetent, forgetful character revealed. In case *F* this structure is well brought out. The putative igneous origin of whinstone is presented as so implausible that the only variable parameter remaining is Hall's character as an observer. The narrative of error-and-falsity projects staggering oversight onto Hall's person.[29]

6. In the introduction, Kirwan serves notice that he intends 'to examine both the general reasoning employed by him [viz. Hall] and the consequences fairly deducible from his experiments' (p. 3). The scrutiny of Hall's reasoning is the

29 See the comment in Barthes, op. cit., 1975, p. 18, that: 'in narrative . . . the discourse, rather than the characters, determines the action'. I suggest, as a corollary of this, that representations of people's characters may arise as the product of the narrative.

sixth and final strategy. Rather than direct challenges to syllogistic reasoning, Kirwan customarily offers reappraisals of Hall's analogies and identifications of one thing with another.[30] For example, Hall's observation that cooled glass can sometimes eventuate in a stony, opaque substance is approved by Kirwan; nonetheless, his suggestion that this process is similar to that by which granite is formed is contested by Kirwan, since: 'the analogy betwixt this case and the formation of granite from a complete fusion of its ingredients is far from being accurate' (p. 7). Glass is presented by Kirwan as chemically unlike a granite, it is relatively homogeneous and contains two distinct chemical constituents. Granite 'is an aggregate stone' (p. 9) consisting of at least three related substances. The case of cooling glass is simply a question of whether the two constituents have the opportunity to separate. Under conditions of rapid cooling they do not, hence there is no stony growth. With granite the complex inter-relations of the parts render any such simple treatment redundant. With glass the case is so straightforward that the analogy of brine is operative: '[The behaviour of cooled brine] is perfectly analogous to that of glass' (p. 9). Whereas, considering whether the feldspar of granite could dissolve the quartz as water dissolves salt:

> It is plain... that... the felspar cannot but in very rare cases serve as a flux or a menstruum to the quartz,... the full proportion of quartz which can be rendered fusible by its other component earths being already contained in the felspar, and in fact there is no analogy betwixt water acting as a menstruum on salt, and felspar acting on quartz, for water and salt are substances perfectly heterogeneous to each other, whereas felspar and quartz are both earthy substance. [P. 6.]

However, challenges to good reasoning are not all a matter of contested analogies. Kirwan sometimes deduces conclusions from Hall's work which Hall has failed to draw, thus:

> the quartz [as the part of granite likely to solidify first], being congealed in a medium still in a liquid state, I do not see why it should not form regular crystals, which nevertheless scarce ever occur in granite except in cavities. [P. 9.]

Here the suggestion is that Hall has avoided a conclusion unfavourable to his preferred scheme, but it is also apparent that the real conclusion of Hall's work militates against Huttonianism. Elsewhere Kirwan adopts an opposite tack, claiming that a certain consequence: 'is truly deduced from the Baronet's hypothesis, but being contrary to fact discovers the falshood [sic] of that hypothesis' (p. 12). The reconsideration of Hall's deductions occurs again in relation to the whinstones:

> I must observe that the different fusibilities of these [artificial] *crystallites*... indicate a very different state from that in which they originally existed; the former requiring a heat of from 32° [to] 45° and the latter a heat of from 38° to 55°, the reason of which is easily discovered when the two states are deduced from a different origination, but is in vain sought for, when both are to be deduced from one and the same origin. [P. 14.]

Here, the correct reasoning allows a re-interpretation of Hall's conclusions to be effected. This approach reaches a climax further on:

> We are now to examine how far the stony structure assumed through slow refrigeration, by stones previously fused, tends to afford any support to the Huttonian theory. In my

30 This may be because syllogistic reasoning plays little part in the contested parts of scientific thought. See M. B. Hesse, *Models and Analogies in Science*, Notre Dame, Ind. 1970, and S. E. Toulmin, *The Uses of Argument*, Cambridge 1958, especially pp. 146-210.

> opinion it affords none at all; the utmost effect it can produce in an unprejudiced mind is to render the origin of whins ambiguous by making them assume the appearance of a Neptunian origin, when in fact they owe it to fusion. [P. 22.]

And:

> these experiments have no relation whatsoever to granite or calcareous masses which form the bulk of the globe, and afford not the slightest indication of their origins; whins... are... but thinly scattered over the surface of the globe. [Pp. 22-23.]

In these cases the narrowness of Hall's interpretation is linked to his partiality (he is not 'unprejudiced' and boosts geographically insignificant whins to global proportions), and the re-interpretation offered by Kirwan *must* be assented to by reason. In dealing with the question of lavas, Hall's partiality is equally visible, for Hall attempts to cast doubt on Kirwan's view by asserting that the volatile substances invoked by Kirwan to effect the transport of lava are nowhere to be found. Kirwan retorts that: 'It is not to be expected that volatile substances, such as sulphur and petrol, should long remain' (p. 26).

* * * * *

These six strategies present in Kirwan's text share one important similarity: they are all critical devices used to account for Hall's support for false hypotheses and for demonstrating this falsehood. However, in a paper devoted less fully to criticism, there would undoubtedly be other strategies, particularly relating to the introduction of new material. In a discovery account for example, one would not anticipate that these critical strategies would be prevalent. The separation between criticism and the introduction of new material is not, of course, easily drawn. The presentation of new 'overlooked' material is clearly critical, as is the supplying of further references omitted by the first author. However, there is much of Kirwan's text that has gone uncited in my analysis and occasionally Kirwan offers new material of no direct relevance to Hall's own work (so that Hall could not be said to have omitted it). When Kirwan re-interprets Hall's arguments and wishes further to corroborate his own viewpoint, then he can either draw on additional re-interpretations, or on collateral information. The former is more successful critically, but the latter has greater flexibility. The instance of the different melting points of the natural and artificial whins (cited above), is an example of a re-interpretation working on the basis of immanent evidence. What Hall has to put down to vague physical variations, Kirwan views as a systematic and genetically vital difference. On the other hand, Kirwan although in accord with Hall about the analogy between brine and glasses with respect to freezing, nonetheless proceeds to cite two experiments demonstrating this analogy as valid.[31] One could perhaps read this as an implicit criticism of Hall, for not having grounded his own analogies, or as a boost for Kirwan's scientific expertise and wide learning. The most obvious function, however, is to bolster Kirwan's specific understanding of the theoretical *basis* for the analogy. In the following I shall treat the introduction of collateral information, when not primarily critical, as a seventh strategy.

6. SYNTAGMATIC ORGANIZATION OF STRATEGIES AND THE READING OF AN ARGUMENT

What has been suggested so far is that there are certain ways in which Hall's error is indicated and also, for the reader, established in the course of Kirwan's

31 Kirwan, op. cit., 1802 8.

text. For example, Kirwan supplies further details on molten glass which, it is suggested, escaped Hall's attention. The details of what Hall missed are thereby brought in and error is apportioned. The work of Kirwan's text is to correct and condemn. Nonetheless, the forms of error-indication have been grouped into different batches, each with a characteristic combination of correction and condemnation. Baselessness indications seem largely to do with undermining and condemnation. Redefinition and the supply of collateral information have much more to do with correction in a positive direction. I shall now move on to consider whether there is an order to the arrangement of these forms.

Before doing so it will be necessary to consider one obvious objection. One might query why (instead of seeking to analyze the modes of accounting) one should not view the article as expressing Kirwan's literal opinions on Hall's work. Indeed, some might even suspect that I am suggesting that Kirwan's paper is mendacious. I consider that a way through this complex area can be found if one regards only the evidence of the text to see if, in a strong sense, the accounting can be read literally. One of the things for which Hall receives reproof is the lack of documentation given for his experiments. Presumably, one could accept that Hall has broken some (tacit?) rule of conduct relating to the reporting of experiments. Certainly, Kirwan claims that there is something important about the fact that 'Sir James has neglected giving a *complete* account' (p. 24). Now, when it comes to Kirwan's testimony concerning the differences between Natural whinstones and Hall's artificial ones, an important issue is whether the extra alkali, found in the natural ones by Kennedy, really matters. Kirwan claims that it vitally affects fusibility:

> It is true that the whins contain lime also, but though the presence of a certain proportion of lime contributes materially to the fusibility of silex and argil, yet it would be ineffectual in degrees of heat below 120° if an alkali were not present to assist it, as I know by experience. [P. 21.]

Here to the literal re-reader, there is a clear contradiction. There appears to be one rule for Kirwan and another for his opponents. A literal reading would suggest that Kirwan is proceeding hypocritically, for in Kirwan's case, only his knowledge and not his conduct comes to the fore, while Hall is judged by his acts. An exactly similar instance occurs with respect to the question of the fusibility of the quartzose part of granite. Kirwan claims, in a passage already cited, that: 'if the quantity of the quartzy ingredient be increased the whole becomes infusible, as I have experienced' (p. 7). Once again, Kirwan gives no certification of his conduct. Moreover, this asymmetry is present at a purely stylistic level, the 'Baronet' is often described as 'thinking' that something is the case;[32] for Kirwan the text urges that 'on mature consideration however it will readily be seen' (p. 11) that things are as Kirwan says. Again the reader is told that 'Two experiments set this [viz. his] explication beyond all doubt' (p. 8), yet it is not established in the text that either of these experiments is better performed than Hall's, and of course Kirwan had not repeated them. Even in the matter of references, Kirwan naturally cites opinions favouring his own, but excludes those favourable to Hall.

But one might say that I am demanding too much, for surely Kirwan, like every scientist, trusts his own conduct. The point however is that scientists are supposed to offer publicly testable tokens of their reliability.[33] Indeed the asym-

32 Ibid., p. 25 for one of many examples.

33 Here, Kirwan appears to be opposing personal thought with publicly attested knowledge. For ways in which this opposition can be used to characterize science, it is

metry spreads still further. Consider the praise bestowed on Kennedy and the subsequent manner in which his testimony was used against Hall.[34] Kirwan provides no reason to praise Kennedy other than that Kennedy's results are in line with what Kirwan supposes to be true. The same can be said of the comment that 'Neither was Saussure a stranger to such granitic veins, but he accounts for their origin very differently from Sir James' (p. 6); Saussure's view is incidentally in accord with Kirwan's. There is no proof that Saussure knows better. This kind of contrast appears in order when, as on page six, Kirwan can enlist the experience of 'almost all' (p. 6) experimenters,[35] but when there is only one opinion to contradict Hall's the argument is rather poor if taken literally. This is well brought out in the discussion of Hall's views on lava:

> Sir James thinks the cause of the fluidity of lavas, which I formerly suggested, . . . strange and inconceivable. . . . Not having had the happiness of viewing those stupendous torrents, I founded my opinion on the accounts given by the most accurate observers, and particularly of Citizen Dolomieu, who beheld and carefully examined every circumstance relating to them for many years. [P. 26.]

Yet Hall had been to Vesuvius with Dolomieu, and had scooped some lava from the flow himself, and subsequently analyzed it. Where is the proof that Hall's opinion is less worthy than Dolomieu's?

I have suggested that in the text Kirwan is preferred to Hall, and also that other individuals are given preference over Hall on grounds that are exceedingly thin if the text is read literally. In case one imagines that these instances can be contextually accounted for there is one final piece of evidence; Hall himself receives contradictory treatment. This has already been mentioned under the heading of baselessness, where it was pointed out that those of Hall's opinions which were in accordance with Kirwan's beliefs were given a texted basis, whereas those which were not, were deprived of one. Similarly, Kirwan praises Hall for the observation that under certain conditions a melted stone can solidify to an opaque material:

> the saxification . . . assumed by the vitrified stones when slowly cooled [is] by far the most curious fact, for which we are indebted to the ingenuity of Sir James Hall. [P. 19.]

Hall is subsequently lauded in the conclusion because this 'discovery is of great importance to geology' (p. 27). Yet this is the same Hall who overlooked the fumes given off by molten glass which are 'evident' to anyone who 'attends' the matter (p. 17). As was suggested in the section on 'overlooking', the image of Hall's character fluctuates with changes in the narrative. It is a textual cipher, created and sustained by the machinations of the narrative.[36]

A literal reading of the reasons for Hall's falsity is thus difficult to sustain. Nonetheless, the text does not read poorly; therefore, one can conclude that it is not read as a narrative about why Hall, as a matter of biographical fact, is in error. Rather, one reads it as a *demonstration* that Hall's work contributes nothing (or less?) to the Huttonian case. The factor which allows the text to be 'senseful' is Hutton's pre-given falsity and Kirwan's knowledge of the truth.

instructive to examine J. Ziman, *Public Knowledge: The Social Dimension of Science*, Cambridge 1968.

34 Kirwan, op. cit., 1802, pp. 14-15 and 23.

35 If one can even accept this flourish literally.

36 See. Barthes, op. cit., 1975, p. 18 and the contribution to R. Barthes et al, *Poétique du Récit*, Paris 1977. For related issues see H. Sacks, 'Some Technical Considerations of a Dirty Joke', in J. Schenkein (ed.), *Studies in the Organization of Conversational Interaction*, New York 1977, pp. 249-69.

Yearley, Steven, ***Textual Persuasion: The Role of Social Accounting in the Construction of Scientific Arguments*** , Philosophy of the Social Sciences, 11:3 (1981:Sept.) p.409

FIGURE 1: The Syntagmatic Organization of Accounting Modes in Kirwan's Text

mode of accounting \ page	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27
untruth	X								+		X										+		+		X
baselessness		X		X				X		X					X						X			X	
improper action											X											X			
redefinition			X		X							X				X		X				XX			
overlooking		X					X								X				X				X		
bad reasoning	X	X		X	X		X					X								XX				X	
collateral						X			*												*		*		
CONTENT	A	B									C													D	E

KEY

X = example of accounting cited above
* = example transferred from 'untruth' to 'collateral'
+ = example cited above under 'untruth' and used by Kirwan as a counterexample; and therefore transferred to 'collateral'

In the 'content' section:
A = Introductory Section
B = Section on Granite
C = Section on Whinstone
D = Section on Lava
E = Concluding Section

The asymmetry detected in each case (Kirwan needing no behavioural propriety, Kennedy being preferred to Hall, and Hall's vacillating 'character') is ultimately a matter of otherwise known, pre-established truth. The accounting serves not to 'tell it how it was', but to demonstrate 'the sort of thing it must have been', given that the truth about geology is known. The accounting has a primarily functional role; a form of account is selected to indicate the departures from truth in each instance. This functionality supersedes issues of historical verity, and explains why the apparent contradictions of the text's biographical narrative do not matter.[37] Two things are, however, not explained. The first is how the text can be built on contradictions (why a range of devices must be drawn upon, risking contradictions). The second is why certain devices are used *where* they are. In an attempt to answer these questions, I shall consider the sequential organization of the accounting modes. This arrangement is presented in figure one.

I have entered on this chart the occasions of accounting, observed and cited, in my descriptions of the strategies.[38] There is, I believe, no absolutely incontestable arrangement or assignment of these features, but based on the above figure, and using the two largest content groups, we have:

mode / content	Untr.	Basel.	ImpAc.	Redfn.	Overl.	BadRs.	Clatl.
granite	0	4	0	2	3	3	1 (2)*
whinstone	1	1	5	3	3	3	0 (2)*

*Figures in brackets include transfers.

In these figures the only striking difference is in the modes of attributing baselessness and improper activity. Clearly, the latter of these has something to do with the added importance, at the level of content, of experiments in the second half of the paper. Yet Kirwan need not have made a major feature of this, and he certainly need not have excluded baselessness. Instead of treating this difference as based on content, that is, as history or biography, one can see it as related to the narrative purpose of Kirwan's text. Baselessness removes Hall's claims making way for new information; on the other hand allegations of improper activity allow transformational and re-interpretative work. If one accepts that the untruth of Huttonianism is established from the very beginning and is a presupposition of accepting the text; and if one agrees that Kirwan is attempting

37 One can perhaps regard this as an explanation for the historical poverty of histories of science which are dedicated to the rational reconstruction of the growth of knowledge. They are obliged to take the 'biographies' available in formal texts as real contributions to the history of science.

38 I regard my argument as primarily qualitative, although I feel that the quantitative considerations which follow are suggestive. Clearly, the identification of the accounting types, and the allocation of chunks of text to these types are contestable; but despite the possible overlapping of categories, I believe that my argument stands. This is by no means to deny that further studies could isolate a finer structure of accounting. A defence of my qualitative approach would rest on the novity of the form of analysis which I am undertaking.

to correct and re-direct Hall's argument, then I consider that different strategy-diagrams of the two halves of the paper can be drawn up as illustrated in figure two.

Kirwan's discussion of granite operates by the dismissal and supplanting of Hall's original claims. By contrast, his work on whinstone operates by amplification and re-interpretation of Hall's claims. The differential patterns of accounting resources accord with these strategies. The correlation of accounting and purpose indicates still more strongly the primarily functional nature of the accounting forms employed.

To open up the accounting system yet further, one may expand the sequential order to reveal the smaller units of the text, where the issue of apparent contradictions, which I raised above, can be resolved. The two argument halves can be presented in terms of accounting modes as illustrated in figure three.[39]

If one accepts that Kirwan's argument proceeds from supposition of the untruth to conclusion of the untruth of Huttonianism, then each argument may be viewed as a 'V'-shaped curve, plotted onto the diagrams. The argument moves temporally through a range of accounting procedures. One cannot place too much importance on such a construct, but it does suggest one thing. In each section of the text one mode of accounting (or perhaps two or three) is dominant. Under such conditions all other bases for assignment of truth and error are temporarily suspended. Thus, in the case of the apparent contradiction between Kirwan's claims for experimental knowledge, and his condemnation of Hall for omitting a 'complete' account, one finds that Kirwan's seeming contradictions occur on page eight (marked * on Figure three) and page twenty-one (marked +). In the former case, imperfect action is not being considered at all, and in the latter the text is locally occupied with redefinition and bad reasoning. The contradictory devices can be accommodated by the text because it operates with an ordered sequence of (perhaps indistinct) units, each with a prime accounting mode. Only were the author, or his preferred agents, to contravene the mode under consideration, would any problem be apparent in the text. The artificial practice of re-reading brings these possible contradictions to the surface.

Thus, answers are available to the two questions posed earlier. Given that accounting devices are not to be understood literally but against a presupposition of anti-Plutonist conviction, then accounting devices are employed for their utility in furthering the course of the argument. Furthermore, the contradictions arising from a literal reading are avoided because the text is organized sectionally. Against the presupposition of known truth, each section is free of contradiction.[40]

7. ORDERING, EDITING AND CONTEXTS OF GRATUITOUSNESS

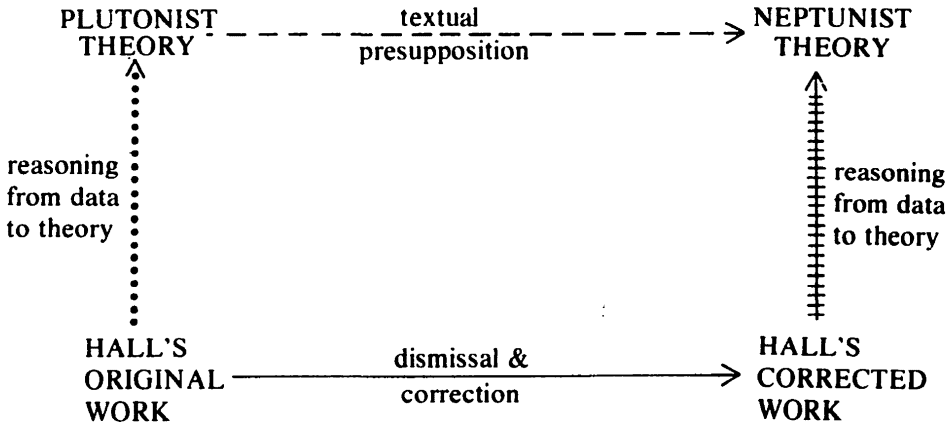
The importance of ordering and organization which has been stressed in relation to the systems of accounting has further textual implications. The general manner in which ordering provides instructions for reading and creates the meaning-supplying context of textual elements has been described by Sacks and by Smith.[41] Two particular ordering-dependent features of Kirwan's text will be

39 In Figure 3 the accounting modes are arranged in different orders in the two halves of the diagram. The ordering arrangement is purely pragmatic and reflects my desire to produce a continuous curve. The point of the diagram, that the accounting modes are grouped about certain stages of Kirwan's text, would be unaffected by a different arrangement—the pattern would merely be less immediately visible.

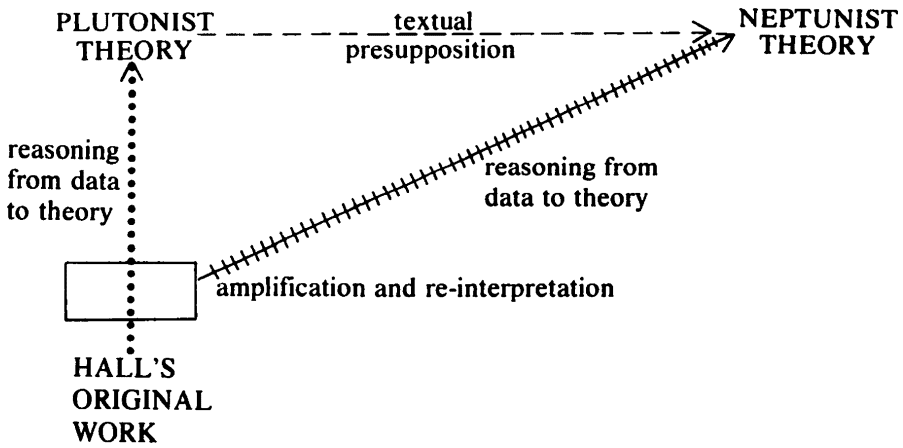
40 As is the case in the three episodes of the joke discussed in Sacks, op. cit. (note 36).

41 Ibid., and D. Smith, 'K. is Mentally Ill', *Sociology*, **12**, 1978, 23-53.

Yearley, Steven, ***Textual Persuasion: The Role of Social Accounting in the Construction of Scientific Arguments*** , Philosophy of the Social Sciences, 11:3 (1981:Sept.) p.409



Strategy One: The Case of Granite

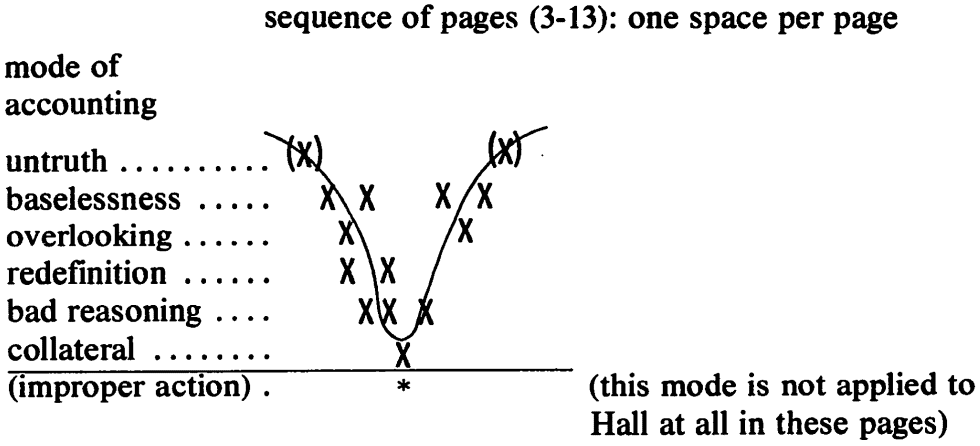


Strategy Two: The Case of Whinstone

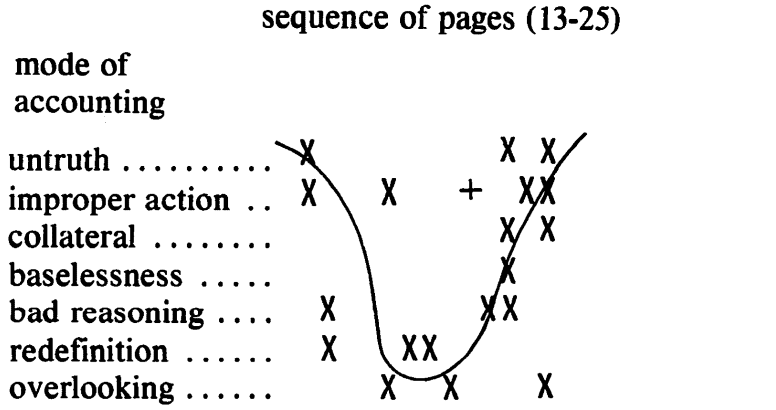
KEY

= The initial, contested message of Hall's text
= The concluding message of Kirwan's text
& = The direction and locus of Kirwan's corrective or re-interpretative work.
= The textual presupposition of the truth and falsehood respectively of the Neptunist and Plutonist theories, which acts as a 'safety net' for Kirwan's arguments

FIGURE 2: Diagrams Representing the Argumentative Strategies Employed in the Two Halves of Kirwan's Text



Distribution of accounting modes in the section of Kirwan's text on granite. (Derived from Figure 1: the counter-examples have been excluded). The symbol * is explained in the text.



Distribution of accounting modes in the section of Kirwan's text on whinstone. (Derived from Figure 1: the counter-examples have been included under 'collateral'). The symbol + is explained in the text.

FIGURE 3: Diagrams Representing the Sequential Distribution of the Accounting Modes in the Two Halves of Kirwan's Text

discussed here. The first relates to editing and the second to the connection of Kirwan's arguments to the underlying Neptunist commitment.

I began this study by relating historians' practice to the espoused rationality of scientific texts. One of the problems created by their method is that scientists, like Kirwan, who have basically gone down in the records as on the side of folly, include glaring errors in otherwise sensible texts. Such comments by Kirwan as, that if Hall were correct about the successive formation of minerals from a melt then one would anticipate common banded rocks, have a recognizable force.[42] However, his claim that:[43] 'the college of Dublin now possesses fragments of basaltic pillars in which marine shells are imbedded' (p. 25) appears almost incredible. Was he not even cautious or circumspect in his identification of the basalt? I consider that the problem of editing, what one includes and how one includes it, insofar as it relates to the forms of accounting and argumentative strategy, provides an answer to the historians' dilemma. I consider that there are at least three inclusions in Kirwan's text that are in an important sense gratuitous. They are the three positive evidences of the untruth of Huttonianism which I classed subsequently as a special form of collateral information. Along with the above quote they are:

[granites] essentially contain some particles of water [and could never be molten]. [P. 11.]

natural whins... lose five per cent. of water and other volatile matter when heated to redness [and could therefore never have been molten]. [P. 23.]

These quotations do not cause dismay like the one concerning basalt, but they fail equally strongly to convince the modern commentator of the outright incredibility of Huttonianism.

The curious thing about these three entries is that if they are certainly true then no other argument is necessary. Hall's work must be disregarded. If they are less than certain, then stated thus baldly they are pathetic and unconvincing. Given that an answer to the editing problem could well be supposed to be 'include the most telling information', these entries are off the mark. The solution to this paradox is revealed by their ordering, for each lies at the end of a section (see Figure one). In the context of the disproof of Hall which has preceded them, they provide a telling back-up to the drift of the argument, *provided* that their truth and importance is not doubted. Given the presupposition of truth and falsity underlying the paper, they are contextually effective. Essentially, only in the position they occupy in this paper of disproof could they be meaningfully introduced and be viewed as completely unproblematic. The certitude with which they are introduced is contextually generated, and does not necessarily reflect Kirwan's real commitment, or what he might say in other debating contexts. The historians' problem is generated by the indexicality of textual elements, and the espousal of scientism in the text.[44]

The manner in which the organized contexts in the paper may allow a tautologous argument to be presented, indicates how fully the truth-commitedness of Kirwan's whole paper may be concealed from the reader. Thus, both the accounting devices and the literal content are steeped in presup-

42 Kirwan, op. cit., 1802, pp. 9-10.

43 For the modern geologist, even more so than for Hutton or Hall, the idea of finding a fossil in basalt (an ancient lava) is obviously nonsense. Kirwan would presumably find more sympathy with the historian of geology if he had expressed even a little scepticism about this 'fossil', especially since he had not seen it *in situ*.

44 On the nature of this indexicality, see Barnes and Law, op. cit. (note 1).

posed truth, and only understood as an independent argument by virtue of textual organization.

The second issue, closely related, is the conclusion of Kirwan's paper, which has now been shown to be present from the very start and a condition of understanding the text.[45] The two sections promoting Neptunism may be quoted:

> Perhaps some may say that the same difficulties occur in accounting for the crystallisation of granite in the moist way; on mature consideration however it will readily be seen, that the causes of coadunition in the dry and the moist way are very different, and that their effects should also be different. For supposing the earths, that enter into the composition of granites, dissolved in the moist way, their precipitation and imperfect crystallization may be ascribed to the union they contract with each other forming masses of each of the constituent ingredients of granite, which water can no longer hold suspended; hence the precipitation of each of the three species of stone is nearly *contemporaneous*, whereas, if the formation of these ingredients should take place in the dry way, it would necessarily be *successive*, keeping pace with the successive dimunitions of heat, and then the above-mentioned consequences would naturally ensue. [P. 11.]

> In my opinion it affords [no support to the Huttonian theory] at all; the utmost effect it can produce in an unprejudiced mind is to render the origin of whins ambiguous by making them assume the appearance of a Neptunian origin, when in fact they owe it to fusion. [P. 22.]

In neither case is there positive interpretative work. The Neptunist theory is assumed correct until shown otherwise, so that the only task is to show that the Neptunian theory is not endangered. As was suggested in Figure two, this may take a different form, in the first case transforming the data before showing it to be congruent with Neptunism, while in the second the data is left to stand but subtly inverted. However, no general proof of the Neptunist position seems required, since the vexed issue of whether these minerals dissolve in water *at all* is not raised in the first quotation. Provided the Neptunist theory can stay intact on specific issues, its overall correctness will not be doubted for an instant. The vulnerability of the Neptunist theory (given that all theories are vulnerable in some way) is protected since it is not mentioned except when the resumé aligns it with fact. Textually, it operates underground until the coast is clear.

8. CONCLUSION

I have argued that, in one particular scientific paper (which I have no reason to regard as anomalous in this respect), the writing is not of 'degree zero'. Kirwan's text does not offer a straightforward presentation of his thoughts, or a colourless statement of his opinions; rather the text contains a great range of generally stylistic features. These features, however, are not haphazardly arranged. It has been my suggestion that they are ordered in such a way as to contribute to and to facilitate the case which Kirwan is putting forward. Thus, the outcome of the issue which Kirwan is debating is actually presupposed in the text's introduction and conclusion. Similarly, this same outcome figures as a pre-requisite for the acceptability of certain observational claims made in the course of the paper, and for the interpretation of many of the accounting procedures found in the text.

45 A similar conclusion is reached in Mulkay and Gilbert, 'Putting Philosophy to Work: Popper's Influence on Scientific Practice', in this volume; where the authors suggest that a pre-existing technical or scientific commitment is necessary before scientists can agree about the implications of Popper's method of falsifications for their actual practice.

Furthermore, the stylistic features and accounting devices provide the means whereby, within the confines of Kirwan's short text, the complex issues under debate can be resolved (for all practical purposes[46]). For example, the indication of Hall's incompetence functions to suggest that, even though the debate might be dragged on, anything else Hall contributes is very likely to be suspect. The sectional distribution of the accounting procedures (Barthes' 'lexias') plays its part by making the role of textual features, in assisting the argument, less visible.

This study suggests that scientists' accounts are by no means the straightforward pieces of evidence that sociologists have often taken them to be.[47] In particular, the testimony which they appear to afford about (*a*) the author's actual beliefs, (*b*) the characteristics or actions of other scientists, and (*c*) the range of arguments and considerations which are of relevance to any particular scientific debate, should not be taken at face value. Given that issues '*a*' to '*c*' comprise the principal kinds of questions which are normally posed by meta-scientific studies, it would be my recommendation that in the immediate future, studies should be directed to understanding scientists' accounting procedures rather than to answering these questions directly.

Clearly, further studies must be carried out to corroborate the claims which I have put forward here on the basis of analyzing just one text. However, I consider that this study demonstrates the interest of analyzing scientific texts, and shows how such analyses may lead to results of sociological significance. For example, it may be that the manufacture, so to speak, of opponents' personal characteristics in the course of accounting for their apparent errors, contributes to the personal antagonisms which accompany many scientific disputes. Similarly, if as I suggest, the readability of formal texts depends upon a prior conviction of the truth and falsity of the general theoretical positions which are said to be assessed in the course of these texts, then this may explain why papers are dismissed as nonsense by their authors' opponents.

46 This is of course an expression favoured by Garfinkel (See H. Garfinkel, *Studies in Ethnomethodology*, Englewood Cliffs 1967). The term lexia is employed in Barthes, op. cit., 1975.

47 For a discussion of the sociological treatment of scientists' accounts of their actions see M. Mulkay, 'Interpretation and the Use of Rules: The Case of the Norms of Science', in T. F. Gieryn (ed.), *Science and Social Structure: A Festschrift for Robert K. Merton, Transactions of the New York Academy of Sciences*, Series II, 39, 1980, New York, pp. 111-25.