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ASPECTS OF WILLIAM WHEWELL'S

PHILOSOPHY OF SCIENCE

by

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A thesis submitted for the Degree of
Master of Arts

The Department of Philosophy
(School of General Studies)
The Australian National University

June 1974.
ACKNOWLEDGEMENT

I would like to express my thanks to Dr. Richard Campbell of the School of Philosophy (General Studies) in the Australian National University, and to the staff of the School of the History and Philosophy of Science in the University of New South Wales, for their advice and encouragement in the preparation of this study.
"Between the idea
And the reality
Between the motion
And the act
Falls the shadow."

from "The Hollow Men"

by T.S. Eliot
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ABBREVIATIONS

PIS1  The Philosophy of the Inductive Sciences, 2 Vols, 1st ed., 1840.
PIS2  The Philosophy of the Inductive Sciences, 2 Vols, 2nd ed., 1847.
NOR  Novum Organon Renovatum, 1858.
PD   On the Philosophy of Discovery, 1860.
FA1  "On the Fundamental Antithesis of Philosophy", 1844.
FA2  "Second Memoir on the Fundamental Antithesis of Philosophy", 1848.
OI   Of Induction, With Especial Reference to Mr. J. Stuart Mill's System of Logic, 1849.
USI  The History of Scientific Ideas, 2 Vols, 1858.
NTLM "On the Nature of the Truth of the Laws of Motion", 1834.
RH   "Review of Herschel's A Preliminary Discourse on the Study of Natural Philosophy", 1831.
CAAI "Criticism of Aristotle's Account of Induction", 1850.
PW   An Essay and a Dialogue on the Plurality of Worlds, 1854.
HCNP "On Hegel's Criticism of Newton's Principia", 1849.

Note: For full details of these works consult the booklist below, pl52f.
The name of William Whewell is certainly more familiar to most philosophers nowadays than it was a few years ago. No longer merely regarded as an opponent of Mill, Whewell, over a period of recent years, has emerged as a philosopher of merit in his own right. Indeed this latest judgement has now proceeded to the point where an influential modern thinker can claim in a major work that "throughout this book the view is being maintained that theory can fruitfully be looked upon as the imaginative construction of models according to well-chosen principles, and that, in many ways, the theory of "Ideas" in Whewell's sense, is more helpful in the theory of theories and scientific method generally, than the logic of statements".

The favour with which aspects of Whewell's thought are being greeted in some quarters, as an aid in the construction of an alternative philosophy of science to that represented by the tradition of Mill, is a noteworthy phenomenon. Its explanation, however, would be a large task involving no less than a critical summation of the whole of Nineteenth and Twentieth Century British philosophy up until the present. Such a task is obviously beyond the scope of this thesis. Yet if any such summation were to be undertaken in the future, the problem of the historical status of Whewell's philosophy within

1. The contemporary literature on Whewell begins with the appearance of two articles by C.J. Ducasse in Phil Rev, 60 (1951), reprinted in Blake, Ducasse, and Madden (ed.). Theories of Scientific Method.
its own Century would be an issue of importance. This thesis, therefore, is an attempt to place Whewell's thought within the context of Nineteenth Century philosophical problems and issues, particularly as they are related to matters of science. Consequently, it is more inclined to be a history, though a critical one, than an attempt at direct philosophising. Such a history is required, apart from the intrinsic interest of the topic itself, by any attempt to take the overview of Nineteenth and Twentieth Century British philosophy that has been suggested. This is particularly the case in connection with the problems which Whewell's philosophy, as a species of Kant's, could not solve in its own day, and which continue to trouble philosophers who look back favourably in the direction of Kant.

As a history, the thesis opens in Chapter I with a sketch of Whewell's life, followed by a general statement of the motives and intentions of Whewell's philosophical stance. Chapters II and III treat in turn the two pillars of Whewell's thinking, necessary truth and induction, and trace his attempts to reconcile his particular viewpoints on each. The problems associated with this reconciliation are brought out most fully in Chapter IV, and the Final Chapter deals with the fate of Whewell's philosophy, conditioned as it was by the problems which it faced, during the later Nineteenth Century.
CHAPTER ONE

THE GENERAL CHARACTER OF

WHEWELL'S THOUGHT

1. WHEWELL'S LIFE AND WRITINGS

Whewell's career and interests can be traced through several stages which, though they overlap, are well defined. It will be seen that Whewell's early academic concerns were with research in mathematics and the natural sciences, and that it was in these fields that he first established himself as a man of considerable ability. It will become clear, however, that as Whewell's life progressed, his academic and professional circumstances and his intellectual interests were changing, although, not surprisingly perhaps, they were undergoing a parallel development. The most important aspect of this for an understanding of Whewell's thought is the shift which occurred in his interests at about the middle of his life, and which took him away from a first-hand acquaintance with scientific research, and led him more towards the history and philosophy of science, a trend which was continued and accentuated throughout the remainder of Whewell's life, and which ultimately brought him into the regions of general philosophy and ethics.

William Whewell was born in Lancaster on May 24th, 1794. The eldest child of a carpenter, he showed
an early aptitude for mathematics, and through the efforts of a local clergyman was sent to Lancaster grammar school and later to nearby Heversham in preparation for taking the matriculation examination for Trinity College, Cambridge. He seems to have been an excellent student, showing a keen interest not only in mathematics but in classics as well. In October 1812 he was entered at Trinity, thus beginning his lifelong association with the College and the University.

From the time of his entry Whewell's academic success was unimpaired. In 1814 he gained the Chancellor's Medal for English poetry and in 1816 took his B.A. as second wrangler, although it had been anticipated that he would be senior. Upon taking his degree, and in accord with common practice, he tutored privately for several years. Meanwhile, in October 1817 he became a Fellow of Trinity, and in the next year was appointed assistant Tutor, as well as a lecturer in mathematics at the College. He took his M.A. in 1819, and in 1823 was elected a full tutor, a position he held until 1839. In 1825, appropriately enough on Trinity Sunday, he was ordained Deacon, and exactly one year later, Priest. Whewell's biographers provide no reason for his deciding to enter the Church, and none is evident from his letters, but it is likely that he did so out of a genuine conviction, rather than with an eye to advancement within the University.

Whewell's character was unique. Physically he was a big man, from all reports over six feet, and as an undergraduate he was conspicuous as "a tall, ungainly youth,
with grey worsted stockings and country made shoes". In his boyhood and early manhood he was not particularly healthy, and suffered from "an obstinate derangement of the digestive organs", which apparently was only successfully treated by a Cambridge physician sometime around 1813. From that time onward his health was never in jeopardy and he gained a robust, impressive physique. A Cambridge legend told of a prize-fighter who once exclaimed, "what a man was lost when they made you a parson". His general good health no doubt contributed to the dynamism which characterised him.

Whewell's physique was matched by his manner, which was widely regarded as overbearing, and occasionally likened to Dr Johnson's. De Morgan once remarked that Whewell was "conspicuous as a rough customer, an intellectual bully, an overbearing disputant", and Sydney Smith, after his manner, once inquired of him, "when are you coming to thunder and lighten at the tables of the metropolis?" His brusqueness made him a formidable adversary in conversation, "if that can be called conversation where one man talks and the rest listen", and provoked a good deal of hostility during his early years as Master of Trinity. This was compounded by his unfortunate inability to recall faces, which naturally gave offence to the Fellows he failed to recognise. His entire lack of finesse and intrigue made him incapable of suspecting it in others, and, deficient in tact

and not sufficiently careful of other people's feelings he tended very often to offend the more sensitive souls with whom he came into contact. His friends were aware of these faults and their consequences, and tried to warn him of them over the years. Whewell, however, seems to have been quite blind to them, and it was only with increasing age and the sorrow of his first wife's death that his character became noticeably milder. In time he gained the warm affection of the College and the University, "the sight of his white head towering above the rest always being greeted with loud applause". ¹ Trinity would not have missed his eccentricities for anything, and he was the subject of several student jokes. Shortly after the publication of his *Plurality of Worlds* in 1853 the following rhyme appeared:

"If you through the Realms of Space should have travelled, And of Nebulous films the remotest unravelled, You'll find, as you tread on the Bounds of Infinity, That God's greatest work is the Master of Trinity". ¹

Despite his faults Whewell was never thought unfair, or unwilling to acknowledge a defeat; indeed quite the contrary. He was always patient and gentle with pupils, and was popular as a tutor. Even De Morgan acknowledged as much: "it was the same with all who wanted teaching: it was only on an armed enemy that he drew his weapon". ² He was always willing to promote the well-being of intelligent younger men and never thought it beneath him to convey his own thoughts to them for consideration. For example, while on the "Beagle", the young Darwin received communications

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1. Ibid., p551.
from him on tidal theory, as well as his 1833 Presidential Address to the British Association; and Darwin's remarks to Henslow, that Whewell "has always shown, in the kindest manner, an interest in my affairs", might have been said by a good many men. He always had many friends, and retained their friendships carefully. His intimates included G.B. Airy, J.D. Forbes, Richard Owen, John Herschel, Richard Jones, J.C. Hare and William Wordsworth. Amongst his acquaintances he could number, apart from most of the scientists in Europe, some of the best minds in British scholarship, literature and politics. He never once, however, met John Stuart Mill.

Whewell was not content to walk the narrow path of specialisation, and preferred to cultivate a universalism in knowledge. He knew several languages, both ancient and modern, and was particularly fluent in German. At the same time he had a genuine command of mathematics, natural science, the philosophy and history of science, ethics, political philosophy and international law, theology, architecture and aesthetics, educational theory, poetry and economics. To each of these he contributed something of originality, or at least impressed them with the character of his thought. His learning was legend in his own time, and as much as Macaulay and William Hamilton may commonly have been regarded as the two most erudite scholars in Britain in the period, Whewell occasionally was said to

surpass them, being once described as "'a genius known
to grapple with whole libraries', a sort of Cambridge
Leibnitz". John Herschel, himself no mere figure, summed
up the feelings of his contemporaries when he remarked of
Whewell:

A more wonderful variety and amount of knowledge
in almost every department of human inquiry was perhaps
never in the same interval of time accumulated by any
man...and that not merely a general and superficial
acquaintance, but one which an exact and conscientious
application such as most men devote to some favourite
branch of study, alone can give.

Whewell began his professional career as a
scientist and mathematician. The first edition of An
Elementary Treatise on Mechanics appeared in 1819, and a
Treatise on Dynamics followed in 1823, both books being
contributions to the ultimately successful attempt by Whewell
and his friends, Babbage, Herschel and Peacock, to introduce
the analytical methods of continental mathematics into
Cambridge. Throughout the 1820's Whewell published a con-
siderable number of papers in mathematics and in the optics
of crystals, mostly in the Transactions of the young Cambridge
Philosophical Society. His first academic appointment was in
1828 as Professor of Mineralogy, a position which, though
divorced from his interests in mathematics and philosophy,

1. Butler, A.: "Review of Whewell's The Philosophy of the
Inductive Sciences", Dublin Univ Mag., Pt I (Feb 1841), p197.
Proc Roy Soc Lond., 16 (1868), plii. A manifestation of the
high regard in which Whewell was held by the scientists of
the day is the frequency with which he was consulted on
matters of nomenclature, as "the person generally recognised
as the best authority on scientific language" (Todhunter,
op. cit., Vol. I, p89). For Lyell he invented the terms,
"acene", "eacene", "ineeacene" and "pleiocene", to design-
ate the main geological strata, and for Faraday, "anode",
"cathode" and "ion". To Whewell as well we owe the term
"scientist".
he confronted with characteristic verve. He was prepared to lecture and publish on the subject, rather than, as was usual with many university chairs at that time, to treat the office merely as a sinecure. He resigned the position after four years, in order to devote himself to matters more to his liking. "If I can work myself free", he said, "so as to have a little command of my own time, I think I shall be wiser in future than to mortgage it so far. Quiet reflection is as necessary as fresh air, and I can scarcely get a breath of it". ¹ It was in the years immediately following that he researched, wrote and published his two most important works on the history and the philosophy of science.

Although by resigning the Mineralogy chair Whewell was for the first time showing signs of the trend which was ultimately to take him in directions away from science, the change was gradual, and may even be said never to have been wholly complete while ever he continued to be active in science administration and to publish scientific papers. As an administrator, for example, he was indefatigable, and always viewed the social structuring of scientific research with special concern (NOR,px-xi). ² In 1819 he had been a founding member of the Cambridge Philosophical Society and was Treasurer from 1826-42 and President from 1843-45. ³ He was elected to the Council of the Royal

1. Ibid., Vol I, p59.
2. A list of abbreviations for Whewell's books and journal articles is given above, p1...
Society in 1831, and in 1837 followed Lyell as President of the Geological Society. From its foundation in 1831 he was active in the British Association for the Advancement of Science, being Secretary in 1833, Vice-President from 1835-37 and President in 1841. There were publications to accompany these activities. In 1833 a long paper on the tides appeared in the *Philosophical Transactions*, to be followed by fourteen others up to 1851. Tides were one of Whewell's passions, and he was accorded scientific recognition for his work by being presented the Royal Society's Royal Medal in 1837. He continued as well to produce papers for various publications on a wide range of scientific subjects. Nevertheless, these were made at an increasingly reduced rate, the bulk of them being done before 1840. A rough breakdown of the whole of his writings indicates this quite clearly. During the decade of the 1820's, 72% of Whewell's published work dealt with scientific topics, while only 13% dealt with philosophical, ethical or historical topics. Over Whewell's career these figures altered radically. In the thirties the figures for science as opposed to philosophy, etc. were 60% and 22% respectively. In the forties they were 30% and 39%; in the fifties 10% and 55%, and after 1860, 0% and 40%.¹ The most significant change occurred around 1840, by which time Whewell had completed his *History and Philosophy*, and had accepted the Moral Philosophy chair.

One of the manifestations of this trend is the development which Whewell's opposition to the theories

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¹. See Appendix, Note A.
of the transmutation of species underwent. As early as 1831, in his review of Lyell's Principles of Geology, Whewell made a general attack on the doctrine of uniformitarianism in geology and paleontology. What is significant in this is that Whewell could carry on the discussion at a scientific level, his knowledge of the sciences being adequate to the task. By 1844, however, with the appearance of Chamber's infamous Vestiges of Creation, Whewell's line of attack had taken another turn, and the Indications of a Creator, which he published in 1845 as a reply to Chamber's book, was almost entirely a republication of the philosophical arguments from the Philosophy of the Inductive Sciences of five years earlier, in which Whewell believed he had anticipated Chamber's hypothesis and effectively to have countered it. This may have had much to do with the semi-popular, non-technical tone of the Vestiges itself, but, nevertheless, the lack of detailed scientific example is conspicuous. By 1860, when Darwin and Wallace's work appeared, Whewell, replying to friends who urged him to raise his voice with theirs in opposition to the Origin, absolutely surrendered any hope of doing so on a scientific level, remarking that, "a person who ventures into the controversies at present agitated ought to have a great deal of specific knowledge which I do not possess".

Whewell's drift away from the front lines of scientific research has its parallel even in his beloved physics, and in 1854 we find him complaining quite generally to Forbes that he was no longer acquainted with the up and coming men in science nor with their special concerns.¹

The rising importance for Whewell of the history and philosophy of science may be dated from the decade of the 1830's. It is true that he had been acquainted with the writings of Descartes, Locke, Berkeley, Hume and Reid at a very early period,² and that, even from his youthful days at Cambridge, he had shown an interest in history and philosophy.³ Nevertheless, there is no doubt that the most important work, as well as the greatest bulk of it, was produced after 1834 when Whewell first opened his notebooks on his "Induction".

Initially Whewell planned the History of the Inductive Sciences and the Philosophy of the Inductive Sciences as the one book. Although this plan went astray, his general intentions for the constitution of the work remained the blueprint for his whole future career in philosophy. Addressing his friend, Richard Jones, the economist, in July 1834, he wrote:

You are to understand that I am to consist of three Books. Book 1, History of Inductive Science, namely, Astronomy, Mechanics, Physics, Chemistry and Botany, historiographized in a new and philosophical manner. Book 2, Philosophy of Inductive Science... It will be dry and hard I fear, as it must contain most of the metaphysical discussions which have been attended to...

¹ Ibid., Vol II, p403.
³ See Todhunter, op. cit., Vol I, p11. Whewell by 1830 had already published one or two works of an historical nature; for example, the article "Archimedes", for the Encyclopaedia Metropolitana in 1826.
of late, but it must also contain all the analysis of the nature of Induction and the Rules of its exercise, including Bacon's suggestions. Book 3, Prospects of Inductive Science. The question of the possibility and method of applying Inductive processes, as illustrated in the philosophy of Book 2, to other than material sciences; as philology, art, politics, and morals.1

In August these remarks were amplified:

Book 1 was intended to exhibit for each science its main "epoch" of discovery, with the "prelude" and "sequel" to it, in order to show how "in all great inductive steps the type of the process has been the same". Book 2 was intended to explain the philosophical basis of the act of discovery:

I expect to show clearly that in order to arrive at knowledge or science we must have, besides impressions of sense, certain mental bonds of connexion, ideal relations, combinatory modes of conception, sciential conditions, or whatever else you can help me to call them: they are what I called Ideas in my former letter. Thus space is the ideal relation on which the science of geometry depends; time, cause, likeness, substance, life, are ideal relations on which other sciences depend.2

And the third Book, with the findings particularly of the second in mind, was projected to aid the discovery of "the analogy which exists between these (natural) sciences and our knowledge respecting morals, taste, politics, language, and generally all hyperphysical knowledge".3

It is clear from this that during the 1830's Whewell had sketched for himself an extensive programme of philosophical work, and that he had commenced writing with the intention of completing the matter within a short time and going to only three volumes. But this plan went astray through other matters coming in the way, and he complained

1. Quoted Todhunter, Ibid., Vol I, p90.
2. Ibid., Vol II, p186.
3. Ibid., Vol II, p186; cf., OI, pp5-6.
that his tide work and academic commitments, for example, kept him from his beloved "Induction". 1 The writing of the History ran into three healthy volumes and did not appear until 1837. The Philosophy involved him in three more years of preparation before its two large volumes came to light in 1840.

In the following years Whewell continued to write extensively in the Philosophy of science. An important paper had already appeared in the Transactions of the Cambridge Philosophical Society, 2 and was followed in 1844 and 1848 by the two crucial memoirs on "The Fundamental Antithesis of Philosophy", 3 and by others. Whewell continued to add to and amend his philosophical work even as late as 1860, so that the third edition of the Philosophy, for example, was in fact published as three separate works, the third volume, particularly being consideraly augmented. 4

The unexpected bulk of the History and the Philosophy caused Whewell to delay his plans for an ethics and politics, but not to abandon them. 5 The two volume Elements of Morality, Including Polity appeared in 1845, and several other works on moral and political philosophy

1. Ibid., Vol II, pp234-235.
3. For a discussion of these two papers see below, Ch. 4, pp 89-90.
4. The third edition consisted of The History of Scientific Ideas, 2 Vols (1858); Novum Organum Renovatum (1858); and The Philosophy of Discovery (1860).
5. Nor were aesthetics and philology entirely abandoned; see Architectural Notes on German Churches; the paper, "On Certain Analogies Between Architecture and the other Fine Arts", in Papers Roy Inst Arch, (1863), 175-181; and the sections in the Philosophy on "The Language of Science".
appeared during the period, notably, a three volume translation of Plato's Dialogues. What should be emphasised here is that Whewell viewed the transition from the philosophy of science to ethics and politics as a natural one, and had foreseen it even as early as 1834. Indeed, as consciously a "system-maker" he could not fail to regard such a transition as absolutely essential for the completion of his work, so that once the philosophy of science was out of the way, ethics appeared to be a matter of course. Thus to Hare in September 1840 he wrote: "You ask whether I am idle now that I have got rid of my Philosophy. Not at all. This is not a world for people to be idle in, who see, or fancy they see, their task." 1

Whewell's acceptance of the Knightsbridge Professorship of Moral Philosophy at about this time should not impress us then as anything other than a natural progression for him. He had accepted the chair in 1838 and immediately set about business, relating that, "...so far as I am aware no duties pertaining to the office have ever been performed: at any rate none had been performed for an interval of a century..." 2 That was all changed of course. When in 1855 Whewell finally resigned the chair it was only to devote himself more fully to the University's administration, in which he had been active for some time previously.

Towards 1841, after having completed his five large volumes in the history and philosophy of science,

2. Ibid., Vol I, p120.
and conscious that he was now a man approaching fifty amongst men in their twenties, Whewell had thought of giving up Trinity and taking a country living in Masham in Yorkshire. "College rooms are no home for declining years", he wrote to Hare in March.⁴ After a brief perusal of Yorkshire, however, he declined the opportunities it offered. Almost immediately his fortunes changed dramatically. In June he was engaged to Cordelia Marshall, and they were married on October 12. Within days Whewell received letters from his friend, Jones, and from others, informing him that Christopher Wordsworth, brother of the poet and Master of Trinity, had resigned. Whewell's friends began to campaign for his preferment to the position, but belatedly as it turned out, since the Prime Minister, Peel, had already recommended Whewell to the Queen. On the 18th Whewell has able to accept the position, so that quite suddenly his future was settled. He retained the Mastership until his death a quarter of a century later.

From this point onwards Whewell's time was increasingly turned to administrative matters within the University.² His energy seems never to have flagged, however, and he continued to lecture and to publish. His tenure as Master was not without difficulties, as was seen,³ but nevertheless he was able to achieve several cherished aims, not the least being the foundation of the "moral sciences" tripos and "natural sciences" tripos. Though a reformer in his own way, he was generally thought conservat-

1. Ibid., Vol II, p207.
2. He was Vice-Chancellor twice, in 1843 and 1856.
3. above, p 3-4.
ive politically, especially in his opposition to the changes suggested by the University Commission of 1850-52, many of which were finally adopted at Cambridge.

In 1855, after a very long illness Whewell's wife died, a loss which devastated his personal life. Nevertheless in July 1858 he was married again, this time to Everina Frances, Lady Affleck, the sister of his friend Robert Leslie Ellis. She too was taken from him by illness in 1865. He had no children by either marriage. This last loss seems to have affected him greatly, and with his own advanced age, he was then 71, to have hastened his death. Always "a bold and careless rider", he was thrown from his horse on February 24, 1866, and after a short illness died on March 6. He was buried in the Ante-Chapel in Trinity.

Whewell's academic and intellectual development were closely connected. Beginning his career in science and mathematics, he found his interests increasingly turned towards the history and philosophy of these subjects, and so devoted a good deal of his time, particularly from 1832 onwards, to this study. The historical and philosophical basis of science, however, was only a portion of a wider system of philosophy which Whewell was attempting to work out, so that the later part of his career, for example, the period when he was Professor of Moral Philosophy, must be seen as the natural development of intentions which had been formulated at an earlier date, the fulfilment of a "task" which Whewell believed he saw for himself. In beginning now to sketch something of Whewell's philosophical position it will be necessary first to comment on the nature of this "task" and then to con-
sider the framework on which Whewell constructed his general theory of science.

2. GENERAL OUTLINE OF WHEWELL'S PHILOSOPHICAL VIEWS

In the attempt to understand the motive which prompted Whewell to write as he did on the philosophy of science, a remark once made by Coleridge will not be out of place here. In the early part of his *Biographia Literaria* Coleridge had observed:

> The process by which Hume degraded the notion of cause and effect into a blind product of delusion and habit, into the mere sensation of proceeding life (nisus vitalis) associated with the images of the memory; this same process must be repeated to the equal degradation of every *fundamental idea* in ethics or theology.¹

Now Whewell was likewise sensitive about the threat which Hume's philosophical position seemed to pose for all forms of knowledge, particularly scientific, religious and ethical knowledge; and he was in no doubt at all that Hume's analysis of causation especially might be applied with devastating consequences to every "Fundamental Idea" which such knowledge involved (PISl, I, p72). Once done, he was certain this could only lead to a general scepticism, a loss of faith in the ability, not only of science, but of religion and ethics as well, to provide certainty. It was against this kind of scepticism, represented primarily in Whewell's mind by the figure of Hume, but including as well the philosophical utilitarians and positivists, that Whewell's entire philosophical sojourn can be seen to have been directed.

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Indeed, it might even be said to have been his philosophical raison d'être.

That this is the case is clear from the following incident. In 1834, when Coleridge died, it was proposed by several of his admirers, and particularly by John Sterling and Julius Charles Hare, who hoped Coleridge's metaphysical speculations might provide the outlines of a scheme of religious philosophy, that a commemoration be established in the form of a prize essay on the subject of "the philosophy of Christianity". Hare consulted Whewell on the matter, but Whewell's enthusiasm was only lukewarm. The specific reasons for this are unimportant, but the associated comments in his reply to Hare throw a clear light on his own intentions in philosophy, and because of this may be quoted at length. It should be pointed out that these remarks were written at a time when Whewell was just beginning to prepare and write his History and Philosophy of science:

At the present moment some coherent system of views, capable of being admitted into the English minds of the present day,¹ and bearing on the great problems which the phrase ("the philosophy of Christianity") suggests, would be of inestimable service to men's religious and intellectual stability. You must... allow me (however) to say that I think Coleridge's views somewhat fail in this, and all the German philosophy that I have seen fails still more... Coleridge appears to me to assume, and require, for the understanding of his religious speculations, an intellectual discipline different from that which the English have hitherto had; Schleiermacher and the best of the Germans undoubtedly do so. I conceive therefore that the truths, which may be found in the writings of these men, must be taken up in the mind of some genuine Englishman, and given out in a suitable form, before they will take a national hold upon us.²

1. The italics are Whewell's.
Here surely then was Whewell's "task". What was required, he thought, was an English mind capable of assimilating and restating in language familiar and acceptable to an English public, the substance of the thought of Coleridge and "the Germans". No one was more willing or inclined to do this than Whewell himself.

Now the substance of Whewell's theory of scientific method is contained in his twin contentions that the propositions of the established sciences are necessary truths, and that these propositions are arrived at by inductive reasoning, such reasoning being somehow demonstrable. To the modern ear, accustomed to Hume's critique of necessary connection, and the problems which it poses for induction, the conflation of "necessary truth" and "induction" in Whewell's thought will perhaps appear surprising, even anachronistic. Nevertheless, it is clear that this is what Whewell desired, and it is mainly this aspect of his work therefore which needs to be explained.

Whewell, it has been seen, began his career as a practising scientist, and as such he professed an allegiance to so-called "Baconian" principles of induction. Indeed, Whewell tended to believe that his "very business" was induction, and throughout the turbulence of his philosophical career remained unshakeable in the conviction that discoveries in science could only come about by means of some kind of inductive act. There is no doubt that in matters of induction Bacon was a professed source of inspiration for Whewell (PIS1, II, pp394-395). Nevertheless, there is a definite discrepancy between what Whewell often
said about the importance of Bacon for scientific method, and what Whewell's own philosophy reveals about his debt to him. For although several aspects of Whewell's theory of induction would seem to have roots in Bacon's thought — particularly the notion that induction is a gradual process of experimentation "ascending continuously and by degrees" (PIS1,II, p395) — in its final working out it can be seen to consist of important elements which are not at all Baconian. This is the case especially at the points where Whewell attempted to demonstrate, as he very often did, that induction can give rise to truths which are true necessarily. Let us see this.

Initially Whewell appears to have believed that he could provide a guarantee for the truth of inductive propositions from within the arena of induction itself, and all his claims concerning his "Logic of Induction" were directed towards this end. It should be stressed, however, that at the same time he was attempting this, he was developing another theory which ran parallel to and was intimately connected with his theory of induction, and which ultimately, as will be seen, he had to rely upon entirely once he had come to realise that his other attempts to guarantee the truth of inductive propositions would not succeed. This parallel theory was his theory of necessary truth. Since it has been suggested that this plays such an important role in Whewell's thought it will be necessary to say something about its genesis and its general character.

1. See for example NOR, ppix-x, and also the ensuing discussion, Ch 3, pp65-74.
As was indicated earlier, the point of departure for Whewell's philosophy was the sceptical impasse into which Hume's arguments seemed to lead. Now Whewell was not content to accept this position, and was convinced that a way could be found of avoiding it. Thus, he argued that far from destroying our knowledge of a natural necessity, Hume had actually, though inadvertently, helped to clear the ground upon which the only sure means for establishing that necessity could be stated (PISl, I, pp163-165). Hume, Whewell felt, had correctly shown that the origin of the idea of a natural necessity could not be in sense experience, but was mistaken in concluding from this that cause and effect cannot be known to be more than a constant conjunction. For what Whewell preferred to conclude from Hume's reasoning was, not that there is no such thing as a necessary connection in Nature, but that there was some source of the idea of necessary connection other than sense experience:

Our inference from Hume's observation is, not the truth of his conclusions, but the falsehood of his premises...not, that we can have no idea of connexion or causation, because, in his language, it cannot be the copy of an impression; but that since we have such an idea, our ideas are not the copies of our impressions(PISl, I, p72).

Whewell's difference with Hume here is obviously the Kantian one, as he himself was prepared to admit (PISl, I, pp165f.). Hume, Whewell felt, was the culmination point of a tradition in British philosophical thinking, usually associated with Locke's name, and had developed the consequences of the principles of that tradition to their legitimate conclusions(PISl,I, pp163-164). But Hume's con-

1. Above, p16.
elusions are merely sceptical ones, he said, and are only avoided by Kant's philosophy. For Whewell, Kant was the "first and by far the most important figure in the whole reaction to the 'sensational school' ", and he saw Kant's solution to the difficulties presented by Hume in quite simple terms:

...roused, by the skepticism of Hume, to examine wherein the fallacy lay which appeared to invalidate all reasonings from effect to cause...(Kant was able to show) how this inquiry terminated in a conviction that the foundations of our reasonings on this and similar points were to be sought in the mind, and not in the phenomena; - in the subject and not in the object (PIS1, II, pp478-479).

Reading Kant in this way, Whewell believed he could argue that causality is an inseparable condition of all experience, and that inevitably a connection between events is required by our very ability to apprehend them as events (PIS1, I, pp165-166). Uncaused events then to Whewell are quite literally inconceivable. Hence arises the necessity of the connection of cause and effect (PIS1, I, pp166-167). Causation therefore becomes a universal and necessary truth, or in the special terminology which Whewell borrowed from Coleridge, a "Fundamental Idea" (PIS1, I, p169).

What is important about this is that to Whewell's mind Kant's analysis of causation was a model for guaranteeing belief on "similar points", essentially in guaranteeing the truth and necessity of the other Fundamental Ideas which Whewell believed were entailed by natural science, ethics and theology, in fact in all the disciplines which involved inductive reasoning. This was the critical move which Whewell made for his claims concerning necessary truth,
and it provided his ultimate line of defence for his commitment to induction. It is the business of the following chapters to explain how this was so. Since Whewell's theory of necessary truth bulks so large for his theory of induction, and because his statement of it involves the forging of certain technical notions, upon which his analysis of induction depends and without which it would appear incomprehensible, the relation between the two theories can best be examined by considering first in Chapter Two Whewell's arguments concerning necessary truth, and then to follow this in Chapter Three with an examination of his proposals for induction.
CHAPTER TWO

NECESSARY TRUTH

The central thesis of The Philosophy of the Inductive Sciences is embodied in the very first of the aphorisms which Whewell's wrote to neatly present the conclusions of his reasonings - "Man is the Interpreter of Nature, Science, the right Interpretation". This claim itself involves two subsidiary claims; that the knowledge of Nature involves the active participation of the human mind and cannot be gained without this participation, and that the resultant conclusions we reach and which make up the disciplines of science are, in Whewell's terms, "right, sure, certain and necessarily true". These two claims Whewell felt are both true and compatible, and upon them he sought to construct a philosophy of science which would provide mankind with a guide to the discovery of new truths.

The starting point of Whewell's theory of science took an avowedly Kantian form. To begin with, Whewell, like Kant, never really doubted the truth of the Newtonian world view, especially Newton's three laws of motion. That there were therefore laws and theories which were true, universal and necessary Whewell was convinced (PIS1, I, p4). This being so it made sense to him to ask such a Kantian question as how knowledge of such universal and necessary truths is possible, and to answer it in the way that Kant had, by maintaining

1. See above, Ch 1, pp20-21.
that the necessity of such laws is dependent upon the conditions of knowledge in general, upon the peculiar nature of the knowing mind. In Whewell's terms,

The grounds of universal and necessary truths which we are able to assert in various departments of knowledge, reside in certain general forms of apprehension, or relations of our conceptions; as Space, Time, Cause; and these I term Ideas; or when ambiguity is to be guarded against, Fundamental Ideas. (PIS1, I, pxiii)

or again,

...the necessity and universality of the truths which form a part of our knowledge are derived from the Fundamental Ideas which these truths involve. (PIS1, I, p63).

There are at least two matters here which require clarification, (i) what did Whewell mean by the term "Fundamental Idea"?, and (ii) how were the propositions of science to be derived from these Ideas? Let us take these in turn.

Every science in Whewell's view rests upon appropriate or Fundamental Ideas, mathematics upon the Ideas of Space, Time and Number; mechanics upon the Idea of Cause, the sciences of light, sound and heat upon the Idea of a Medium; the classificatory sciences upon the Idea of Likeness; Biology upon the Ideas of Life, Vital Force and Final Cause; and so on. What is important in this is that Whewell construed these Fundamental Ideas not as ideas in Locke's sense of objects of the understanding - still less as innate ideas in Locke's sense - but rather as the consequence of the peculiar structure, metaphorically speaking, of the mind itself. They are not concepts, but are rather the necessary ways of perceiving, "...not Objects of Thought, but rather Laws of Thought"(PIS2, I, p29). The mind, Whewell believed, being of
a particular nature, can only perceive the data of sensation in a particular way and no other. Hence arises the universality and necessity of the truths it recognises.

Some of the Fundamental Ideas, notably Space, Time and Number, Whewell likened to Kant's "forms of intuition" (PIS1, I, px), while others such as Cause and Likeness corresponded to the Kantian Categories, though Whewell nowhere actually employed the term. His arguments for instance, particularly concerning Space and Time, were the Kantian ones, and he was prepared to speak of the Ideas of Space and Time as informing our sensations (PIS1, I, pp31-32). For Whewell then the Fundamental Ideas were no more than Kant's constitutive principles under a new name.

A Kantian as far as his Fundamental Ideas were concerned, Whewell was a Kantian no further. Although he admitted to adopting some of Kant's views, to the extent that, as he said, "the chapters on the Ideas of Space and Time in The Philosophy of the Inductive Sciences, were almost literal translations of chapters in the Kritik der Reinen Vernunft" (PD, p335), Whewell expressly denied that he was in any way a disciple of the German philosopher (PIS1, I, px); also (PD, p334). And it is quite clear, for instance, that he did not follow Kant into the depths of the critical Philosophy, nor even so far as to accept only twelve Categories. Indeed, on this last point, Whewell believed that the number of possible Fundamental Ideas is indeterminate, and that, consequently, so is the number of disciplines which might be developed in the

1. See also, Ch 4, p 96.
future once more Fundamental Ideas have been recognised.

The extent to which Whewell's philosophy differed from Kant's, however, is most clearly seen in what was the central and largely novel contention of Whewell's theory of necessary truth, the claim that,

...in the progress of that exact speculative knowledge which we call Science, Facts which were at a previous period merely Observed Facts, come to be known as Necessary Truths...or, if I may be allowed to use a briefer expression...facts are idealized (FA2, pp614-615).

That is to say, empirically observed truths can become necessary ones, or as Whewell elsewhere put it, "a posteriori truths become a priori truths" (PD, pp357-358).

Now this view is a clear departure from Kant, and obviously conflicts with the Kantianism of Whewell's conception of the Fundamental Ideas. For if necessary truths are seen to be so only in the course of the development of empirical science, how then can they be the conditions of knowledge as Whewell would have them be? This astonishing view can only be understood by comprehending the character of necessity which Whewell believed the Fundamental Ideas bestow on some factual propositions.

Before beginning to see this however, something ought to be said concerning our second question, which asked, how are the propositions of the sciences to be derived from their respective Fundamental Ideas? How, for example, are the definitions and axioms of geometry to be derived from the Fundamental Ideas of Space, Time and Number which the science is said to depend upon? On the general point Whewell

would appear to be excessively vague. He claimed that initially we frame from the Fundamental Ideas certain "ideal conceptions" which are, he said, "notions" which "the mind forms, and which it expresses by means of general terms" (PIS1, I, p36). He gave as examples of these "ideal conceptions", an angle, a circle, a central force, a neutral salt, a rose, a reptile. But as well, Whewell wanted to include the conceptions of space, time, number and cause, and it is clear from what he wanted to say about these, though he himself saw it none too clearly, that as "conceptions" they were to be held separate somehow - it was not obvious how - from the Fundamental Ideas of Space, Time, Number and Cause (PIS1, I, pp36-37). The latter are Laws of thought, the former are concepts. At the same time it is not at all clear from Whewell's language exactly how conceptions are formed, for Whewell says merely that they are "modifications and limitations of the Fundamental Ideas themselves"(PIS1, I, p38). The conception of a circle, for example, Whewell claimed is determined by "relations involved in the Idea of space"; the conception of attraction at a distance was said to involve "necessary principles derived from the Idea of cause", and so on. This, however, seems to be all that Whewell had to say on the matter. Nor is the situation any clearer at the point of transition from conceptions to axioms and definitions.

The difficulties and obscurities in these views, just as those involved in Whewell's claim that a

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1. This parallels rather roughly Kant's distinction in the Critique between space as a "form of intuition" and "space" as a concept.
posteriori truths become a priori truths, are to some extent alleviated by an understanding of the nature of the necessity which is said to be derived from the Fundamental Ideas. Let us turn to this then.

Whenever Whewell spoke of necessary truths he characterised them in at least three different ways - (i) their necessity is not an analytic one; (ii) the evidence for their necessity is never empirical; and (iii) they are known by intuition, but this intuition is progressive.

(i) For Whewell necessary truths are not necessary by being analytic statements. Their necessity is not a logical one, resulting from an analysis of the terms involved. They are not Hume's "relations of ideas" or Kant's "analytical judgements". That this was Whewell's view is clear from his comments on the historical origin of scientific theories and laws, as well as from his comments on the nature of mathematical truth. For example, Whewell believed that the Newtonian laws of motion could not derive their necessity from being analytically true for the reason that in the Seventeenth Century men of great scientific ability had actually denied their truth and had even proposed other laws in their stead. To Whewell's mind this was a great fact requiring an explanation. Obviously, he felt, the necessity of the laws could not arise out of their being logically self-evident, since, were this the case, the famous controversies in the history of science would have taken place (NTLM, pp. 149-150). Thus the apparent contradiction which the history of science presents to us, that is, "that a law should be necessarily true and yet the contrary of it conceivable", could not be explained away by regarding
necessary truths as possessing merely a logical necessity, since the debates in the history of science, Whewell seems to have felt, have never been simply over the meaning of words. These were real, not trivial disagreements.

This is fully brought out by Whewell's discussion of the necessary truths of mathematics, in an argument which he believed was exactly applicable to the other sciences (PIS2, I, p61). His discussion of mathematics was similar in many respects to Kant's discussion of the non-analytic nature of the judgements of pure mathematics in the Prolegomena and in the second edition of the Critique. The necessity of the propositions of mathematics, Whewell claimed, is not due to the meaning of the terms involved; it is not a matter of definition. The evident truth of the proposition "3+2 = 5" is not a result of the terms of the proposition being "identical", since if this were so we would be forced to agree that it was a matter of definition that 39 and 27 make 66, and who would seriously be willing, he said, to do this? Thus for Whewell the truths of mathematics are guaranteed not because mathematical statements are "identical propositions", but for some other reason. As he said, "it is not the logical but the philosophical, not the formal but the real foundation of necessary truth, which we are seeking"(PIS1, I, p69).

(ii) Necessary truths are not therefore analytically true. But neither, said Whewell, is their necessity founded on experience. This last claim would appear to conflict with

1. For Whewell the definition of "5" is not "3+2", but rather "4+1".
2. Whewell, as will be seen in Ch 4, afterwards attempted to reconcile this claim with his position on the categorical necessity of the Fundamental Ideas.
Whewell's belief that necessary truths are seen to be such in the course of the development of purely empirical science.

Whewell was careful to point out that by saying the grounds for necessity are never empirical, he was using the term "experience" in a precise and particular way:

I here employ the term Experience in a more definite and limited sense than it possesses in common usage; for I restrict it to matters belonging to the domain of science. In such cases, the knowledge which we acquire, by means of experience, is of a clear and precise nature; and the passions and feelings and interests, which make the lessons of experience in practical matters so difficult to read aright, no longer disturb and confuse us (PIS1, I, p59).

It is clear from this that Whewell did not mean by "experience" simple sense impression, but the regulated experience of scientific observation and experiment. We can understand therefore what he intended by the claim that "...in no case can experience prove a proposition to be necessarily or universally true" (PIS1, I, p59). The reason that the universality of any proposition cannot be known by experience is that no matter how much experience may confirm the proposition, thus showing it to be general, the proposition strictly can never be proved, since human experience can be of only a limited number of cases, and however numerous these may be there will always remain the possibility of a disconfirming instance being uncovered sometime in the future. 1

Since this were so, Whewell believed that experience would be still less capable of proving a truth to be necessary. That a general proposition is universal, experience cannot show; even less can it show that a general proposition must be true.

1. Whewell even believed this true of propositions in mathematics; see PIS1, I, p60.
"Experience cannot bestow that universality which she herself cannot have, and that necessity of which she has no comprehension" (PIS1, I, p61). Even more to the point, no experience, that is, no regulated experiment or observation, could prove a necessary truth, since no possible experience could disprove it. Whewell believed, for example, that anyone understanding the principles of statics clearly would be led to attribute an error in procedure to any experiment which apparently contradicted them, rather than believe them disproved (HSI, I pp 217-218).

Thus for Whewell the evidence which establishes a necessary proposition is different from the evidence which establishes a contingent proposition. The apparent anomaly between Whewell's claim that a) necessary truths are not derivable from experience, and b) in the course of the development of purely empirical science facts of experience are later seen to be necessary, is explicable when it is realised that Whewell was using the term "experience" in two senses. In the first claim, as was just seen, he was using "experience" in the sense of "regulated scientific experience"; while in the second claim he was using "experience" in the sense of simple "sense experience". It was only in this second sense that Whewell was able to say that "some truths known by experience are later known to be necessary", while at the same time holding that the grounds of their necessity could be experiential. This second sense could not, of course,

1. cf., Kant in the Critique, B1 - "But though all our knowledge begins with experience, it does not follow that it all arises out of experience".
provide the grounds for the necessity of a proposition any more than the first could, since "experience" in this sense is even more idiosyncratic and subject to confusion and controversy than regulated experimentation. Necessary truths must derive their necessity from some other source.¹

Now amongst the scientists who were Whewell's contemporaries, the idea that there exist truths which are known to be such independently of experience was anathema. Whewell was well aware that his views for this reason would arouse some antipathy, and even declined the offer of the Presidency of the British Association for the Advancement of Science in 1840, the year in which his Philosophy was published. Writing to Roderick Murchison, the geologist, he remarked,

"...I cannot expect, I know it is impossible, that men of science should assent to my views at present: and those who have laboured hard in special fields will naturally feel indignant at having a person put at their head, recommended only by what they think vague and false general views. I believe this would do much to disgust and repel men of science..."

The extent of the gulf which separated Whewell from the majority of the very best scientists in Britain is clear from the fact that John Herschel, Whewell's lifelong friend from their early Cambridge days, and one of the best-known, influential and respected men of science in Britain during the period, in reviewing Whewell's History and Philosophy expressed his polite but decided disagreement with

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1. Ultimately they are intuited; see below, pp 36-39.
Whewell on the a priori nature of scientific knowledge.¹

Whewell, on learning that Herschel was to be his reviewer, had written to him attempting to explain his intention:

...My argument is all in a single sentence. You must adopt such a view of the nature of scientific truth as makes universal and necessary propositions possible; for it appears that there are such, not only in arithmetic and geometry, but in mechanics, physics and other things. I know no solution of this difficulty except by assuming a priori grounds; but I am most willing to look at any proposed solution...²

Herschel naturally expected to be able to supply Whewell with an alternative solution to the problem. First, he did not doubt the existence of necessary truths, in geometry at least. Their necessity, he remarked, "has never, we believe been questioned, nor is it our disposition to do so now".³ And like Whewell he felt their necessity was something more than analyticity could provide. It resulted, Herschel said, from their correspondence to reality: "All true propositions about realities are necessarily true, provided their subject-matter be necessarily such as it is, since every reality must be consistent with itself."⁴ It was not with the necessity of geometrical propositions that Herschel was concerned, but with their universality, or rather, as he phrased it, "...with the nature and grounds of our conviction of their universality." Now Herschel ought to have asked this question of the necessity of some propositions

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4. Ibid., p206.
as much as of their universality, not only because Whewell always linked the two, but because Herschel's remarks about the grounds of necessity did not touch Whewell's analysis of the problem, for they are just what Whewell was calling into question. "We may not be able to see", Whewell said, "what are the properties of real things; and therefore may have no conviction of their necessity", so that Herschel's analysis of the grounds of necessity "does not apply to our conviction of necessity" (PIS2, II, p670). 1

Whatever the justice of this remark, we might examine the basis of Herschel's dissent from Whewell's claim, that experience alone can only show truths to be general and not universal. His argument, one which Mill later repeated, 2 was that facts of experience are abstracted in such a way as to become axiomatic propositions, due to the "inductive propensity" of the mind, its "plastic faculty", its "irresistible impulse to generalize ad infinitum", in the main because of "our involuntary application of the law of continuity to fill up, by the same ideal substance of truth every interval which uncontradicted experience may have left blank in our inductive conclusions". 3 In more modern terms, Herschel was arguing that facts corresponding to reality are collected and grouped together, and from them we induce from a general to a universal proposition, the leap involved in moving from a finite number of observed instances to a proposition making an assertion about all instances in the class, being the result of our adherence to the principle of

1. cf., Locke's remarks about the knowability of real essences in the Essay, Bk 3, Ch III.
the uniformity of nature, and thus expecting whatever we
have observed in the past to be true of all instances, even
unobserved ones.¹

Now Herschel was perhaps on his weakest
ground when he attempted to apply this argument to mathe-
matics by claiming that mathematical axioms are entirely
derived from experience. For the realm of mathematics was
really the strong-point of the so-called "intuitionist"
position. Nevertheless, Herschel, like Mill after him, chose
the example of mathematical axioms to make his point about
the universality of propositions, and argued that the truth
of such axioms as "two straight lines cannot enclose a space"
and "two straight lines which cut one another cannot both
be parallel to a third" are known by experience, because the
concepts involved in the axioms are themselves entirely
empirical.² The term "straight", Herschel said, involves
only the notion of uniformity of direction, and "the unity
of direction, or that we cannot march from a given point by
more than one path direct to the same object, is a matter
of practical experience long before it can by possibility
become matter of abstract thought".³

Whewell's reply to this was as follows. To
know that a line is a straight line is not simply a matter
of observation, since "straightness", he claimed, is not a
pure empirical concept at all. To know that something is
straight involves having some independent test of straight-
ness. Observation cannot provide this, for to say that we

¹. For Whewell's version of what is involved in inductive
    inference see below, Ch 3, pp 49-50.
observe a line to be straight is to say in effect that we measure a fact of observation against a standard of straightness that we accept. The suppositions of straightness is thus prior to all so-called observation of it:

...It is said that it (straightness) is a matter of experience long before it is a matter of abstract thought. But how can there be any experience by which we learn these properties of a straight line, till our thoughts are at least so abstract as to conceive what straightness is? (PIS2, II, p674).

Thus Herschel's analysis of the universality and necessity of mathematical propositions, and by implication the propositions in other sciences, didn't worry Whewell, and he felt confident in denying, in the manner he had, that the necessity of scientific propositions did not derive from experience.

(iii) If the necessity of necessary truths were not determined either by logic or experience, how do we come to recognise it? Whewell's answer to this was that it is intuited. When, he said, a Fundamental Idea is clearly and distinctly conceived by the mind it gives rise to "inevitable convictions or intuitions" which are necessarily true and which are then the axiomatic foundation of the propositions of the science concerned(PD, pp336-337). How does Whewell argue this point?

In the only concise statement of what he meant by a necessary truth, Whewell said,

1. cf., the remarks on "facts" below, Ch4, pp101-113.
necessary truths are those in which we not only learn that the proposition is true, but see that it must be true; in which the negation of the truth is not only false, but impossible; in which we cannot, even by an effort of imagination, or in a supposition, conceive the reverse of that which is asserted (PIS1, I, p54).

What Whewell had in mind here was that whenever we try to frame the opposite of a necessary truth in any "clear and distinct" manner, we find that we simply cannot do so without inevitably becoming involved in paradoxes, ambiguities, unclarities and the like. Such attempts were said by Whewell to be not only "false" but also, quite literally, "impossible". By this Whewell did not mean that the negation of a necessary truth could not be believed, for it was obvious that this had often been the case. It was just that the negation could not be conceived sufficiently well to be consistent. For example, Whewell claimed (PIS2, I, pp59-60) that Hobbes had once mistakenly believed that he had discovered a means for doubling the cube (i.e., of finding two mean proportionals between two given lines). Yet the problem, Whewell said, could not be solved by plain geometry, and Hobbes' error therefore was merely an indication that his geometrical conceptions were ill-founded. "Such a mode of conceiving the opposite of a geometrical truth", Whewell concluded, "forms no exception to the assertion, that this opposite cannot be distinctly conceived" (PIS2, I, p60).

1. Although much of Whewell's language here concerning clarity and distinctness of conception would seem to have an obvious Cartesian ring to it, this is partly illusory. Whewell was certainly acquainted with Descartes' philosophical works but, curiously, was no admirer of them. Rather he was more influenced by Plato's writings, especially in his later life. The problems which he saw arising out of his statement of an intuitionist position, in the long term were certainly given more of a Platonic than a Cartesian solution; see below, Ch 4, pp 92-93, also Footnote 2 p93.
But how do we know when our conceptions are clearly and distinctly conceived? The criterion for this, Whewell said, was simply one's ability to recognise the necessary connection between an idea and its relevant axioms; in Whewell's words, to "see the necessity of the Axioms belonging to each idea", and to accept the axioms "in such a manner as to perceive the cogency of the reasonings founded upon them" (PIS1, II, p183). For Whewell this criterion was not one simply of performance, of being able successfully to follow a train of reasoning to its conclusions, having once acquiesced in the original assumptions, the axioms. Whewell's language sometimes gives this impression. Thus, "...a person has a clear Idea of Space who follows the reasonings of geometry and fully apprehends their conclusiveness" (PIS1, II, pp183-184). But this is misleading, for necessity is neither a matter of logic or of hypothesis. Speaking of axioms, Whewell says,

...these verbal enunciations of the results of the idea(s) cannot be made to depend on each other by logical consequences; but have a mutual dependence of a more intimate kind, which words cannot fully convey. It is not possible to resolve these truths into certain hypotheses, of which all the rest shall be the necessary logical consequence. The necessity is not hypothetical, but intuitive. The axioms require not to be granted, but to be seen (PIS1, I, pp91-92).

Thus Whewell's intention was that the connection between the Idea and its axioms must be intuited, and, ultimately, that the truth of all that follows from the axioms can be guaranteed by the original categorical necessity of the Fundamental Ideas. The situation may be briefly put in the following way: the "explication" of conceptions is an historical process which manifests itself in scientific controversy over
the nature and meaning of the terms expressing certain Fundamental Ideas, and which once resolved results in the formulation of axioms. These axioms are not logically derivable from the Ideas, they "cannot be deduced from any verbal definition of the idea" (FISI, I, p182), but require some sort of guarantee for their truth. This is provided by the "explication", which clarifies the Ideas to such an extent that the connection between the Ideas and the axioms is intuited, or seen, to be necessary. When this intuition becomes possible our concepts are said to be suitably clarified, and the explication suitably complete.1

Now the claim that there are propositions in science whose opposites are inconceivable sparked a deal of controversy from Whewell's opponents, particularly Mill and De Morgan. Thus Mill was quick to retort that the so-called inconceivability of the opposite of necessary truths is merely an "illusion", the result of "habit" and "our earliest inductions from experience", of the "laws of association", and so on.2 Further,

...Dr Whewell himself says that we are continually led, by the natural progress of thought, to regard as inconceivable what our forefathers not only conceived but believed, may even (he might have added) were unable to conceive the reverse of. He cannot intend to justify this mode of thought: he cannot mean to say that we can be right in regarding as inconceivable what others have conceived, and as self-evident what to others did not appear evident at all. After so complete an admission that inconceivableness is an accidental thing, not inherent in the phenomenon itself, but dependent on the mental history of the person who tries to conceive it, how can he ever call upon us to reject a proposition as impossible on no other ground than its inconceivableness? Yet he not only does so, but has unintentionally afforded some of the most remarkable examples which can be cited of the very illusion which he has himself so clearly pointed out.3

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1. This notion is thoroughly Platonic and is reminiscent for example of Plato's discussion in the Meno, especially 82B-85D.
2,3. See footnotes 2 & 3 on p40.
To such criticism Whewell was particularly sensitive, and he was concerned to avoid what he felt was a misunderstanding of him. For Whewell believed that it was no telling criticism of his views to argue that if there are necessary truths their necessity ought to be immediately evident to all men at whatever stage of mental development. He was at pains to point out that the recognition of necessary truths involves "due preparation and thought", and felt that, as a consequence, it is really no surprise that while well-trained scientists with some alacrity of mind are able clearly and distinctly to conceive the necessity of the connection between axioms and the Ideas upon which they rested, there are some people who are not able to do so. The intuition of necessary truths, Whewell said, is "a rare and difficult attainment" and requires a mind that is "scientifically cultivated, at least to some extent" (PD, p339). Nor did this fact mean that for Whewell truth is merely dependent upon the particular state of mind of the person who contemplates it. He was adamant that knowledge is real, not illusory, and is known because it is true, not true because it is known:

...We are not surprised that attention and care and repeated thought should be requisite to the clear apprehension of truth. For such care and such repetition are requisite to the distinctness and clearness of our ideas: and yet the relations of these ideas, and their consequences, are not produced by the efforts of attention or repetition which we exert. They are in themselves something which we may discover, but cannot make or change (FAI, p180).

3. Ibid., p160. For De Morgan's version of this see his "Review of The Philosophy of Discovery", Athenaeum, No1694 (April, 1860) p501.
1. This realist strain in Whewell's thought is not always on the surface, but it runs strong and deep, and determines much of his thinking. Several of the crucial issues arising out of Whewell's gravitation towards a form of realism, as well as the general tenability of his often obscured commitment to it, will be discussed more fully in Ch 4, particularly in the consideration of his views on "facts"; see below, pp 101-113.
Whewell put much emphasis on this theory of the **progressive intuition** of necessary truth, for he believed that it successfully answered two problems which his philosophy had suggested to his critics. First, as was just seen, to the objection that necessary truths must be immediately evident to all men at all times, Whewell could answer that propositions may be axiomatic and necessary but require time and the perfection of thought to bring them to light, and that consequently axioms may arise gradually in the progress of knowledge. Thus, for Whewell, the history of science is accommodated by the fact that new axioms and new sciences are always possible as the Fundamental Ideas upon which they rest are rendered clear and distinct, simplified and organised by suitably ingenious men of science over a period of time (PD, pp347-348). Second, and equally important, to the objection that necessity is not known *a priori* but is an illusion produced by habit and the familiarity of experience and observation, Whewell could reply that "it is not any special results of the science...but the existence, the possibility, of any science, which establishes the necessity of these axioms" (PD, pp349-350). The very fact that a science exists, that in the course of history certain Ideas have initiated inductive sciences which really do explain and predict the phenomena they were intended to, leads us to know that those Ideas have at least come to be clearly and distinctly intuited, and are, hence, necessary.

It can be seen from this discussion what Whewell believed necessary truths to be. They are in effect constitutive principles derived from the form of the knowing mind, from the Fundamental Ideas. Because of the limitations which our mental constitution imposes on our ability to know, we are
constrained necessarily to conceive things in a particular way. It is not logic that is the constraining influence, for it is always possible to state the logical opposite of necessary truths without contradiction. Rather the constraining influence is ourselves, our mental constitution. We simply cannot know other than by means of those principles which make knowing possible. The clarification of our Ideas may take time, but this is unimportant since once our Ideas are well-constituted, and articulated in a system of science their necessity becomes clear.

It is this theory of necessity which Whewell attempted to link with his commitment to inductive science, and it is because of it that his theory of induction has such a novel aspect. The features of this attempted union will be examined in the following chapter.
CHAPTER THREE

INDUCTION

...Do not go and conspire with Peacock or any body else to tell our friends that I am bewildered with German philosophy, as you once raised an outcry with the accusation of a priori metaphysics. If you do so you may easily give people an impression, which you will not be able to remove when I have convinced you, as I certainly shall at the first opportunity, that everything which I believe is most true, philosophical, and inductive...

Letter to Richard Jones, October 1825.

Whewell's theory of induction has been characterised by several of his commentators as an early version of a contemporary hypothetico-deductivist position. Now while it is true that certain aspects of Whewell's inductive theory have an undeniably modern ring to them, especially Whewell's general claim that induction consists of a colligatory act and, related to this, the important emphasis which he placed on the creative element in scientific discovery, it is essential nevertheless to recognise that there were significant claims which Whewell made for induction which a modern hypothetico-deductivist theorist would not be likely to make, particularly that inductive inferences give rise to

2. The notion of "colligation" is fully explained below, pp 49-52.
necessary truths. He did not believe, as De Morgan seems to have felt he did,¹ that inductive propositions possess merely a "moral certainty", but rather that induction provides no less than rigorously certain and demonstrable truths.² Whewell's general philosophical position must be seen to be opposed quite consciously to all forms of conventionalism (PIS2, II, p675),³ and the recognition of this will prevent us from being misled into allying Whewell's position on induction more closely than is warranted to that of a modern hypothetico-deductive theorist.

Whewell's remarks concerning "induction" indicate that he was using the term in at least two different senses. Often he rather loosely spoke of induction as the act of framing an hypothesis (PIS1, II, p212). More usually he would assume that it was the complete process of the conjecturing and testing of hypotheses. Both senses of the term were recognised by Whewell's contemporaries.⁴ For convenience, and in order to separate it from the wider sense of "induction", the act of framing a suitable hypothesis will be referred to by Whewell's technical term of "colligation".

Whewell's broad usage of "induction" to cover "the whole of the method of discovery"⁵ may be seen to comprise three well-defined, and progressive stages - first, the clarification of the elements of knowledge by analysis; second, the colligation of facts by means of a conception; and third, the

2. His "Logic of Induction" was an initial attempt to show this; see below, pp 65-74.
3. The best statement of this attitude was in the "Remarks on a Review", and is discussed at length in Ch 4, pp 75-78.
verification of the colligation. Whewell’s several claims for his theory of induction, and the relation which holds between this theory and the theory of necessary truth, may best be examined by concentrating on each of these stages in turn.

1. **CLARIFICATION OF THE ELEMENTS OF KNOWLEDGE BY ANALYSIS**

   Essentially, Whewell believed that there are two elements involved in knowledge, Ideas and Facts. The union of Ideas and Facts, for Whewell, produces science. This union, however, is of a precise kind and consists of "colligating" or "binding together" the Facts by means of particular "conceptions". Whewell, however, believed, that prior to this activity each of the elements of knowledge must be rendered suitable for use in the colligation. He felt that in a true scientific induction, Ideas must be "clear and distinct", Facts must be "certain and sufficiently numerous" (PII, II, p169). The means by which each of these requirements is reached were termed by Whewell the "explication of conceptions" and the "decomposition of facts".

   Something has already been seen of what Whewell meant by the "explication of conceptions" in considering his intuitionist criterion of necessity, and those remarks will not be repeated here. We may, therefore, turn

1. Each of these stages has its analogue in Whewell’s historiography, and corresponds more or less closely to what Whewell in the History called the "Prelude to the Inductive Epoch", the "Inductive Epoch" itself, and the "Sequel to the Inductive Epoch".
2. See below, pp49-52.
3. Above, Ch 2, pp37-39.
4. See, however, Appendix, Note B.
immediately to his notion of the "decomposition of facts".

Now facts for Whewell had a peculiar status. Believing himself a Kantian concerning perception he had argued throughout the whole of the Philosophy that the distinction between facts and theories is spurious, since all perception of external objects is in some sense mind-dependent:

...the difficulty which we have been illustrating, of distinguishing facts from inferences and from interpretations of facts, is not only great, but amounts to an impossibility...We cannot obtain a sure basis of facts by rejecting all inferences and judgements of our own, for such inferences and judgements form an unavoidable element in all facts. We cannot exclude our ideas from our perceptions, for our perceptions involve our ideas.

(PISl, II, p196)

Whewell, however, recognised a problem in this, for, he said, it seemed to be a maxim of scientific enquiry that facts are the basis of science. Yet how could this be if the distinction between facts and theories could not be maintained successfully? Now Whewell believed he could solve this problem, and that the solution lay in discerning and separating the Ideas which, in his term, "inform" the facts, from the facts as they are themselves. There are quite precise and rigorous "rules", he said, for doing this: (i) facts must be divested of emotional or partisan considerations, and involve only "intellectual conceptions"; (ii) facts must be rendered as precise as possible, and the most suitable way of doing this is by the application of the clear and unambiguous notation of mathematics; i.e., facts must be mathematised, and (iii) any other Idea or Conception which may be involved with the facts must be made as rigorously precise as possible. The rendering

1. "Theories" are said to result from the "superimposition" of conceptions upon facts; see below, pp 49-52.
2. The statement of this problem - which Whewell came to recognise as one which was fundamental to the whole of his philosophical stance - is treated separately and more fully in Ch 4. For the present it is treated briefly, as an aspect of Whewell's general position on induction.
of facts by these means Whewell described as the "decomposition of facts". Essentially it was a programme for an analysis of facts into their basic elements, Ideas on the one hand, and what Whewell called "elementary facts" on the other (PII2, II, pp33-34).

There are at least two problems which arise in connection with the decomposition. First, Whewell is very unclear as to the exact status of his "rules". They are said to offer "precision", but just how this is guaranteed is not particularly obvious, since Whewell does not seem to invest them with any rigorous logical force. Rather he seems to have thought of them as merely methodological rules, as particular instances of the general maxim that facts and the Ideas which inform them must be distinguished. But this is confusing, for at the same time it is apparent that Whewell wants to claim for these "rules" something more than a mere methodological status, since he maintains that they are "necessary" preludes to discovery (PII1, II, p199). This juxtaposition of "necessary" with "maxim" is unfortunate and is obviously problematic. Whewell, however, does not appear to have recognised any difficulty here.

The second difficulty connected with the decomposition appears to be more serious, and ultimately to lead to a dilemma. If by decomposition facts are rendered into their components, into Ideas and "elementary facts", a question automatically arises concerning the nature of these "elementary facts". On the surface the most natural inclination is to answer that they themselves are facts. But if "elementary facts" are facts, a difficulty occurs, for in Whewell's view all facts are informed by Ideas, so that are not "elementary facts" equally so informed?
And if they are, ought not a decomposition of them to be possible, on Whewell's own principles? Yet the problem is that no decomposition of the type Whewell suggested could ever rid facts of their ideal element without destroying them altogether as facts, since it is a necessary condition of facts that they involve Ideas. So either it must be admitted that "elementary facts" are not really facts at all, which seems peculiar, or that they are not truly elementary, and can be further decomposed, which does not solve the problem, but only removes it one step. This would appear to be a fundamental difficulty for Whewell, and, as will be seen, it can be traced to Whewell's ultimate inability to solve the general problem of how to distinguish theories from facts.

2. THE COLLIGATION OF FACTS BY MEANS OF A CONCEPTION

Conceptions suitably explicated, and Facts suitably decomposed, Whewell believed, did not in themselves constitute scientific knowledge. A further, essential step was required to unite the two, and to produce the general propositions of science. This essential step Whewell proposed to call the "Colligation of Facts", claiming that,

...we may apply this term to every case in which, by an act of the intellect, we establish a precise connexion among the phenomena which are presented to our senses. The knowledge of such connexions, accumulated and systematized, is Science (PIS1, II, p202).

1. See below, Ch 4. In §5 of that chapter it will become obvious that Whewell's language concerning "elementary facts" must be read to mean that he regarded them as other than facts in the usual sense; see below, pp 108-110.
1) **COLLIGATION AS THE TYPE OF INDUCTION**

Whewell sometimes described induction as the process of inferring general truths from particular facts (PIS1, II, p213). However, he was consistently opposed to the view that in such an act the proposition expressing a general truth is simply the result of an empirical generalisation from base facts. Nor, Whewell said, is it a mere description of them. Nor again is it a simple enumeration of them. For the very import of "induction", Whewell thought, is that it involves the introduction of a new, previously unthought of element into the reasoning, or as he put it, the "suggestion of a conception not before apparent" (PIS1, II, p253). This new element, he claimed, binds the facts together, as it were, and presents them in a new light. Prior to this act the facts are known, but are isolated and disconnected, and so have little or no significance - "The pearls are there, but they will not hang together till someone provides the String" (PIS1, II, p214) - and until a connection between them has been forged no proper induction can be said to have occurred:

...in every inference by Induction, there is some Conception superinduced upon the Facts: and we may henceforth conceive this to be the peculiar import of the term Induction (PIS1, II, -215).

Whewell believed that this new element, a "clear and appropriate conception" in his language, is not derivable from experience and could not consequently be said to pre-exist, as it were, in the factual data. Anticipating Mill's criticism here, Whewell argued that it could not, for example, be said of the Greek conception of epicycles or of Kepler's conception of the distance-time relationship for the planets that these were

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1. See below, pp54-59. Whewell's remarks were first published in 1840, three years before the appearance of the System.
contained in the factual material. Rather, in Whewell's opinion, each was actually "something additional" to it. Each was a creation of the discoverer's mind, framed to combine disconnected facts into a comprehensible whole. Nor could any rule be given to facilitate this process, for it involved an act of invention, a leap beyond the available data (PIS1, II, p214), and was a step "out of the reach of method" (PIS1, II, p257).

This act of invention, Whewell believed, manifests itself in the procedure of hypothesising. The essence of inductive discovery being, for him, the creation of a new conception, a facility in inventing hypotheses, far from being a fault to be eschewed by the discoverer, is in fact quite indispensable. "To try wrong guesses", he said, "is, with most persons, the only way to hit upon right ones" (PIS1, II, p220). Essentially, Whewell was concerned to steer a safe course between two equally menacing headlands, between an extreme austerity concerning hypotheses on the one hand, and between the opposite extreme of a tenacious or mindless adherence to favourite hypotheses. The only means by which the two extremes could be avoided, Whewell believed, was by a rigorous testing of any hypothesis framed to account for a set of phenomena - "the character of the true philosopher is not that he never conjectures hazardously, but that his conjectures are clearly conceived and brought into rigid contact with the facts" (PIS1, II, p220).

Now Whewell's view that there is an ideal element involved in all colligation, and that this is somehow the result

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1. It was one of Whewell's favorite remarks, that "Truth arises more from error than from confusion". Even false hypotheses are not without value on this view, in that they at least bind together facts which otherwise would have had no unity (even though that unity may later be seen to be false), and might thus prepare the ground for some assumption in the future which is seen really to connect them.
of a particularly creative mind, obviously expresses something more than the familiar Kantian notion of forms of the knowing mind. Initially, Whewell appears to have viewed colligation as something analogous to the procedure of Kant in applying the Categories of the Understanding to the materials provided by the Sensibility, and to have intended that colligation possess the same rigor and necessity that Kant's procedure appeared to have. However, the connection between the Sensibility and the Understanding which Kant outlined must have seemed to Whewell to be rather too mechanical a notion, and to pay scant regard to the creative contributions of scientific genius.¹ Thus, Whewell appears to have sought some alternative means of allowing for this kind of ingenuity in his theory of science. Hence his reliance upon the notion of invention. Indeed, creative invention plays a fully operative role in Whewell's thought, and is perhaps the point at which Whewell was most conspicuously influenced by Coleridge's notions of the imagination. Whether this past point be so or not, there are, of course, serious problems associated with any attempt to reconcile a fully Romantic notion of invention with Kantian forms of the knowing mind. Whewell, however, seems to have believed that by simply denying that there were only twelve Categories of the Understanding, the way would be clear for the adoption of his claims concerning inventive ingenuity.

It was because he felt so strongly that the inventive act in induction had been overlooked that Whewell lay such emphasis on it in his own formulations. It is in the

¹ One of his major criticisms of Bacon's account of induction was framed along such lines: see PD, pp137-138.
light of this that his later polemic against other theories of induction must be viewed, particularly his attack on Aristotle's account in the *Prior Analytics*. Whewell believed this was inadequate since it emphasised only the evidence for inductive inferences, rather than what Whewell took to be the crucial feature of the matter, the invention of the inductive proposition itself. Whewell repeatedly argued, for example, that in Kepler's discovery of what is called the First Law, what is of paramount importance is the inventive act involved in applying the conception of an ellipse to the orbits of the planets, and not so much the verification of the invention, which though important of course, is necessarily subsidiary (logically and temporally) to the framing of the conception itself; "The Invention of the Conception was the great step in the discovery; the Verification of the Proposition was the great step in the proof of the discovery"(*PISl*, II, p217). This point leads us to say something concerning Mill's well-known attack in the *System* on Whewell's account of induction.

**ii) MILL'S CRITICISM**

In the second chapter of Book III of the *System*, following some preliminary discussion on the nature of induction in general, Mill turned to three cases of what he termed "Inductions Improperly So Called", and, having overturned the first two with customary aplomb, he then engaged the third, which was Whewell's, in order to uproot it as well.

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1. This was written in 1850, and the imputation is clear that it was not so much Aristotle that Whewell had in mind, as Mill, whose *System* had appeared a few years earlier.
Mill advised:

There remains a third improper use of the term Induction, which it is of real importance to clear up, because the theory of Induction has been, in no ordinary degree, confused by it, and because the confusion is exemplified in the most recent and elaborate treatise on the inductive philosophy which exists in our language.¹

Ostensibly, Mill's purpose was a negative one, merely to show where Whewell was wrong, and, indeed, "confused". It is at least clear what Mill thought his objection to Whewell was:

The error in question is that of confounding a mere description, by general terms, of a set of observed phenomena, with an induction from them.²

Mill went on to explain that he meant by this no more than that when Whewell was using the term "induction" he was doing so incorrectly, because he was often applying it to situations which properly were not inductive situations at all.

The substance of Mill's understanding of this criticism is made clear from the ensuing discussion. In essence his objection was as follows: Whewell identifies induction with the colligation of facts; but this is mistaken, because colligation is simply a means of describing facts in general terms, colligation is equivalent to description; now description and induction are not the same thing, since no real inference is involved in description; colligation consequently cannot be real induction, so that what Whewell calls induction is not induction at all. Does this criticism succeed?

There was an agreed upon example. Whewell, in the first edition of the Philosophy, had introduced a discussion

¹ System, op. cit., p191.
² Ibid., p191.
of Kepler's laws as a classic case of what he felt constituted a genuine induction - the colligation of facts by means of a conception, the imposition of a previously unthought of element upon the existing data. Mill in 1843 joined issue with this example and attempted to turn it against Whewell by arguing that what Whewell called the colligation of facts is not induction at all, since colligation is merely a "descriptive operation", and does not involve any genuine inference, which, for Mill, is the mark of real induction. Kepler's laws, Mill said, are not real inductions since they are merely descriptions of the phenomena of orbital motion.

Taking the first law as his example, he argued, that in the same way a navigator decides that his disconnected data concerning bays, headlands and beaches indicates that the land he is mapping is an island, so Kepler decided that his disconnected data concerning the times, speed, and positions of Mars indicates that the orbit traversed by the planet is an ellipse. In neither case, Mill claimed, was any inference involved, since each of the general propositions embraced no more than the facts at hand. To his mind they were simple summaries of the facts:

... That the land in question is an island, is not an inference from the partial facts which the navigator saw in the course of his circumnavigation; it is the fact themselves; it is a summary of those facts; the description of a complex fact, to which those simpler ones are as parts of a whole.

Now there is, I conceive, no difference in kind between this simple operation, and that by which Kepler ascertained the nature of the planetary orbits; and Kepler's operation, all at least that was characteristic in it, was not more an inductive act than that of our supposed navigator.

1. Ibid., p192.
2. Ibid., p191.
To be sure, Mill did not deny that an induction was involved in these two cases, or at least in the case of Kepler. He claimed, however, that it was not located at the point which Whewell suggested, but elsewhere. It was located, he said, at the stage where, in inferring that because the observed places of Mars are correctly represented by points on an ellipse, Mars will then continue to revolve in the same ellipse; and, again, in inferring, before the gap had been filled up by further observations, that the position of the planet during the time which intervenes between two observations, must coincide with the intermediate points of the curve. "For these were facts which had not been directly observed. They were inferences from the observations", and hence constituted real inductions. 1

But if this contention of Mill's were true, surely it is equally true, since we never actually observe the elliptical orbit of Mars in the heavens, that when we make the claim that the orbit is an ellipse, we do so on the basis of inferring from the observational data a situation "which had not been directly observed". The elliptical orbit of Mars consequently must be an induction, if not on Whewell's terms, then on Mill's own. However, Mill anticipated just such a remark, and countered, that although the orbit is not actually directly observable, in principle it is, in the sense that if a planet were to leave behind it a visible trail in space and if we as observers could obtain a position out of the plane of its orbit so as to be able to see the whole trail, making up the complete orbit, we would see that the path of

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1. Ibid., p192.
the planet's orbit is in reality an ellipse. And even if we could only see successive portions of the trail at a time, we would be able to add them together, as it were, and come up with an elliptical orbit. He concluded that no inference beyond the data is involved either way, and, consequently, the claim that the orbit of the planet is an ellipse is not a claim conveying a genuine induction.

But this is not at all convincing, for surely it is a contradiction to claim that in seeing (in Mill's sense of imagined seeing) that the orbit is in reality an ellipse, there is no inference involved. For when Mill suggests that in principle the elliptical orbit might be seen, if only our vantage point were suitable, he is obviously invoking something more than direct observation, since an imagined experience is not experience at all. On this point Mill would have been in even greater difficulty had he chosen as his example, as Whewell had done originally (PISl, II, p214), either the second or third of Kepler's laws, rather than the relatively uncomplicated, easily imagined law of the ellipse. Thus, a supporter of Whewell's view might well find Mill's criticism unconvincing in this regard, and simply repeat Whewell's claim: Is not an "ellipse" an intellectual conception, in essence unobservable, and consequently separate from the data which it colligates, even something perhaps which may have been imposed on the data? This is the whole crux of Whewell's position, for if the conception is by its very nature unobservable, it must be something additional to the data.

1. Ibid., pp193-194.
2. Ibid., p194.
Now it is important to note that Mill was well aware of this crucial feature of Whewell's position, while not of course agreeing with it. In disagreeing, however, he did not want to deny that intellectual conceptions play some role in the whole matter; he did not want to deny, for example, that the conception of an ellipse was necessary for Kepler's discovery. "No one", he said, "ever disputed that in order to reason about anything we must have a conception of it". What Mill did want to deny, however, was that the concept of an ellipse was something additional to the data, in the manner Whewell imagined. For it was Mill's view that the ellipse was "in the facts before Kepler recognised it; just as the island was an island before it had been sailed around. Kepler did not put what he had conceived into the facts, but saw it in them".

We are beginning here to see the grounds upon which Mill was constructing his general criticism. His notion of a conception, in these passages at least, has a tendency to realism, which, though Mill's language is sometimes muddy, may be easily discerned in the following remarks:

...A conception implies, and corresponds to, something conceived: and though the conception itself is not in the facts, but in our mind, yet if it is to convey any knowledge relating to them it must be a conception of something which really is in the facts, some property which they actually possess, and which they could manifest to our senses if our senses were able to take cognisance of it.

An intellectual conception then for Mill is an expression for an underlying reality. But Whewell, up to this point at least, would have agreed, since this was his opinion as well" and the point of difference between the two men

1. Ibid., p192.
2. Ibid., p194.
3. Ibid., p193.
4. Ibid., p193.
5. See Section 1, Ch 4 below p76.
thus rests on the bridge each wishes to draw between the conception and the reality which underlies it. For, as was suggested, each regards conceptions in quite a different way.

Mill explains that a conception for him is a generalisation or abstraction from facts, an empirical generalisation, and as such contains no more than the facts themselves. It is, as he says, merely "the sum of the different observations". He will not therefore allow that the conception somehow pre-exists, or is a construction of the mind imposed on the facts. For only the facts pre-exist. This is true to such an extent, that he felt that "the conception itself is often obtained by abstraction from the very facts which, in Dr Whewell's language, it is afterwards called in to connect".

Thus, it is clear that Mill was willing to acquiesce in Whewell's terminology concerning the colligation of facts, and to admit that facts may be bound together by means of a conception, only if such a colligation be regarded as a "descriptive operation", as a means of summing up a number of factual details in a single proposition, and not as a procedure involving the imposition of an extra-factual element onto the data. Mill's criticism of Whewell's views therefore would seem to amount to no more than this, that Whewell's claim, that colligation is the type of induction generally, is wrong, for the reason that the conception involved in all colligation is not something over and above the base facts, but a simple generalisation or summary of them; it merely describes the facts. Mill seems to suggest that because Whewell has not seen

2. Ibid., p194.
this his account is unavoidably both wrong and confused. But Whewell of course is not touched. For his notion of a conception, its origin and its relation to reality is quite distinct from Mill's. ¹ This is not to say of course that Whewell's notion is therefore immune from criticism. ² It is just to point out that because Whewell viewed conceptions in a manner different to Mill, and because conceptions played so essential a role in his theory of induction generally, Mill, in criticising that theory, ought properly to have been concerned to refute Whewell's notion of a conception. Yet this is just what he failed to do, unless it be regarded as a criticism that a separate and distinct notion of what a conception is (i.e., Mill's own) be set up against Whewell's. But this is no criticism, since Mill would also have to show that his view of a conception is all that a conception can be. While this is not done Whewell can happily embrace much of what Mill has said concerning the nature of conceptions; for example, that a conception is an expression for an underlying reality, and again, that it summarises and includes, and so consequently is a description of, all the relevant facts. Secondly, even where he disagreed with Mill, and in particular with Mill's claim that a conception is no more than the facts it includes, Whewell could offer a defusing reply:

...The fact of the elliptical orbit was not the sum of the observations merely; it was the sum of the observations, seen under a new point of view, which point of

¹. See Whewell's reply to Herschel on the origin of the conception of "straightness", Ch 2 above, pp35-36.
². For a criticism of it, see especially Section 3 of Ch 4 below, p 94.
view Kepler's mind supplied. Kepler found it in the facts, because it was there, no doubt, for one reason; but also, for another, because he had, in his mind, those relations of thought which enabled him to find it (OI, pp33-34).

This reply was defusing simply because it asserted that part of the question which Mill's criticism had not contravened—conceptions are expressions for an underlying reality, yes, but they are also relations of thought, elements supplied by the mind. It is this "also" which is of importance, for Whewell thought that because Mill had not considered it his attack was essentially incomplete. It was almost embarrassing:

Mr Mill has not noticed, in any considerable degree, what I have said of the formation of the Conceptions which enter into the various sciences (although) he has, in general terms, denied that the Conception is anything different from facts themselves (OI, p35).

Mill's criticism therefore, as it stood, was impotent, since Mill, rather than attacking Whewell's notion of a conception as the promise of an exposé of improper inductions seemed to suggest could be done, had simply offered an alternative view. The game therefore becomes one of deciding between two opposing positions, and not as it ostensibly was, one of recognising the exposed error in Whewell's account. Certainly, Whewell had used the term "induction" in a new and unusual sense, and Mill was on strong ground in arguing, as De Morgan did later, that Whewell had unnecessarily confused

1. Whewell replied on another level as well, by denying the distinction which Mill wanted to draw between "explanation" and "description". For Whewell, not only are different descriptions mutually compatible, as Mill would have it, but different explanations as well; see OI, pp23-26 and the important paper THHS.
2. These two claims on the face of it conflict. Their attempted reconciliation was a major problem for Whewell's philosophy, as will be seen in the following Chapter.
3. See below, pp71-72.
the traditional issue of induction by characterising it as an act of colligation. But Mill was not content with this. For although he was quite clear on what Whewell intended by the term "colligation", he wished to deny that this kind of colligation was involved in situations — such as that of Kepler's laws — which Whewell called inductive. But Mill could not succeed in showing this, his major point, at all, while ever the basic conflict over the nature of conceptions remained. 1

The third and final stage in Whewell's broad treatment of induction must now be considered.

3. **VERIFICATION OF THE COLLIGATION**

Despite Mill's remarks to the contrary, 2 Whewell did concern himself with the proof of inductive propositions. He did so by appealing to three "tests of hypotheses", and more particularly to his "logic of induction":

i) **THREE CRITERIA**

An hypothesis, Whewell said, may be tested for its truth by asking, (a) does it explain the phenomena it was framed to explain?; (b) does it predict phenomena not

1. The pivot upon which the disagreement over induction turned is actually Mill and Whewell's opposed notions of "fact". It is from this issue that their conflict over the nature of conceptions stems. Mill's, and Herschel's, view is that conceptions have their origin in sense experience alone; Whewell's is that they depend on sense experience, but involve an additional, ideal, element as well. Both views spring from different opinions concerning the "given" in perception. On this point Mill's opinions are well known, and are put forward at length in Book I of the System. Whewell's position, which is basically Kantian, has been partly treated in Ch 2 above, pp24-26, but a fuller discussion will be undertaken in Section 5 of Ch 4, below.

previously considered in the framing of the hypothesis?; and
(c) does it tend to simplify a system of laws and theories?
Whewell did not go into any great detail about the first of
his claims, that hypotheses must explain phenomena. It is
clear, however, from what he did say, both here and else­
where, that he had in mind a model of explanation similar
to the modern "covering-law" theory. This comes out most
clearly in his discussion of verification as an aspect of the
"logic of induction", which will be treated fully in a short
while. Whewell was adamant, however, that hypotheses must
do more than merely explain phenomena; they must also predict
phenomena as yet unobserved. The reason for this, so he
believed, was straightforward, in that, by giving our assent
to a particular hypothesis, we are committed to holding it true
of all particular instances, past, present and future. It was
because an hypothesis does make predictions in accord with
future observations, Whewell said, that we are entitled to
claim that it has been verified, and is therefore "right and
useful"(PISI, II, p228).

There are two aspects of this second criterion.
An hypothesis may predict phenomena either of the same kind
or of a different kind to those which it was originally intro­
duced to explain. It was on the latter that Whewell placed
most emphasis. His example was Newton's inverse square law,
which, he said, as well as giving a physical account of why
the planets move in their orbits as they do, that is, accord­

1. See below, Pp65-73.
ing to Kepler's Third Law, at the same time also embraces and thereby explains Kepler's First and Second Laws, a feat which Newton did not purposely set out to do, but which was a consequence of the generality of his colligation. Such a situation in which "inductions from classes of facts altogether different have thus jumped together", Whewell proposed to call the Consilience of Inductions (PIS I, II, p230). Inductions exhibiting such consilience, he believed, can only occur with theories that are true, and this seemed sufficient reason to him to claim consilience as a proof of the truth of inductive inference: "no example can be pointed out in the whole history of science, so far as I am aware, in which this Consilience of Induction has given testimony in favour of an hypothesis afterwards discovered to be false" (PIS I, II, p232).

Whewell's third test of an hypothesis was based on a consideration of its power of inclusiveness. Thus, a true hypothesis, he believed, may be distinguished from a false one on the grounds that as it develops over a period - that is, as new discoveries are made - the additional material is early incorporated into the original supposition with little or no modification to it, such that a system of colligations, in the form of laws and theories, is built up, which, rather than becoming more and more cumbersome and complicated, actually becomes more coherent, unified and simplified. In effect, the elements required to explain new classes of facts are already contained in the original supposition. The opposite, Whewell said, occurs with a false hypothesis. New facts which arise, which it was not intended to deal with, and which it does not predict, must generally be explained away by introducing ad hoc
assumptions, which as time goes on, and as they increase in number under new circumstances, eventually render the original hypothesis cumbersome, complex and unmanageable, until it eventually breaks down under the strain, and a new and simpler one is introduced. Such, Whewell imagined, was the case with the epicyclic theory of planetary motion, the Cartesian theory of vortices, the phlogiston theory of combustion, and the corpuscular theory of light. The complexity and incongruities into which each of these fell, and the consequent insecurity of the systems of colligations involved, did not have, in Whewell's view, "the character of truth" (PIS2, II, pp68-72).

It was especially on this criterion of simplicity, and on the criterion of consilience, that Whewell lay most stress. If an hypothesis passed these two tests, the belief in its truth, he claimed, was "irresistible". The two criteria are in fact aspects of the one situation, for a successful consilience leads to a convergence of colligations (true hypotheses) through successive stages of generality towards one all-embracing generalisation. The network or system of laws and theories thus produced will, in Whewell's meaning of the terms, exhibit "simplicity and unity".

It is not at all clear, nevertheless, from what has been said so far, just how Whewell's three criteria can guarantee the truth of hypotheses. For if Whewell were to rest content with saying that the test of a true hypothesis is that it leads to true predictions (PIS1, II, p228), he is open to the objection, that as an elementary tenet of logic even false hypotheses can lead to true predictions. Whewell would
them be faced with a real difficulty, one which cannot be avoided by saying either, that false theories which do predict well contain some element of truth, and that time will show wherein their falsity lies (PIS I, II, pp228-229), or by intimating that because history reveals no example of consilience where the hypotheses afterwards were revealed to be false, these hypotheses cannot then be false (PIS I, II, p232). For if time were the only arbiter, how is it ever possible to be sure that any hypothesis or theory, even Newton's laws of motion and the theory of gravitation, which are undoubtedly the best examples of perfect consilience, is in fact true, when there is always apparently the chance that in time a particular class of facts may arise which can not be accommodated? Internal evidence suggests, however, that Whewell was aware of this elementary problem, or, at least, that some kind of answer to it is available from Whewell's writings. For Whewell proceeded to elaborate a lengthy and thorough discussion which, under the heading of the "Logic of Induction", clearly he believed, provided the mechanism by which the truth of colligations could be guaranteed, which, indeed, would provide the "Criterion of Inductive Truth".

4) THE "LOGIC OF INDUCTION"

Whewell believed that structured scientific theories form a hierarchial pattern, which begins at the lowest level with the colligation of observed particulars, and rises upwards in a triangular arrangement through more and more comprehensive and inclusive colligations, to reach finally the highest and most inclusive colligations, for example, Newton's
theory of universal gravitation. It was Whewell's contention that such an inductive hierarchy could be written down in the form of what he called "Inductive Table", in which the subsumed colligation would be marked off from the subsuming colligation by a kind of bracket, analogous to the way in which the conclusion of a syllogism can be marked off from its premises by a line. Read from top to bottom, Whewell said, this hierarchical arrangement exhibits the order of discovery, while inverting the arrangement reveals the order of verification of the propositions, since at one and the same time each of the lower order colligations is both derivable deductively from the one of the next highest order, and at the same time, is the inductive evidence for that higher order colligation. Thus, for example, Whewell's proposed Inductive Table for Astronomy demonstrates that the science began from such simple observed facts as "the Earth appears to be immoveable", "the Moon's bright part is of the shape of a ball enlightened by the Sun", "the tides ebb and flow", etc. These ordinary facts were then colligated by successive inductive steps, proceeding historically from the Greeks, through Copernicus and Kepler, to Newton's theory of universal gravitation. Such compact tabular arrangements, Whewell felt, exhibit the logical structure of Induction, in the sense that one can see by an inspection of the Table that the facts, and the inferences from them, are set down in such a manner that the evidence support-

1. Whewell provided examples of two from what he thought were the most complete sciences, astronomy and optics. A reproduction of the Table of Astronomy is given in the Appendix, Note C.
ing the inductive inference is manifest (PIS2, II, pp76-80).

Whewell believed that when the form of inductive reasoning has been laid bare in this manner it corresponds roughly to the syllogistic form of deductive reasoning, and offers a "Criterion of Inductive Truth" for the propositions it contains, which is at least as good as that provided in deduction (PIS2, II, p93).

How did Whewell think this was possible?

There are several features of the Inductive Tables which must be kept in mind if some of the claims which Whewell made concerning them are to be understood, and particularly the claim that they exhibit the logic of induction. Firstly, they display the three tests of hypotheses just considered - explanation, consilience, and simplicity. Secondly, and more importantly, they display the historical development of scientific theories, for the inductive inference contained in each step in the Tables was seen by Whewell to involve more than a simple collection of facts. This inference consisted of a new mental element contributed by the discoverer. Thus Whewell's particular meaning of the term "induction", that is, in the sense of a colligation by means of a conception, will help us to understand why for Whewell the "logic of induction" was the logic of discovery as much as the logic of proof.

Now Whewell defined the "logic of induction" as,

...the analysis of doctrines inductively obtained, into their constituent facts, and the arrangement of them in such a form that the conclusiveness of the induction may be distinctly seen... (PIS1, II, p247).

His meaning is made clearer when we learn that he was using the term "logic" to mean,
...a system which teaches us so to arrange our reasonings that their truth or falsehood shall be evident in their form (PIS1, II, p247).¹

From this it can be seen that Whewell viewed logic, deductive as well as inductive, in an unconventional way, even for the mid-Nineteenth Century, as essentially involved with decision procedures concerning the truth and falsity of scientific propositions. Although his use of language is not always consistent, his meaning is clear;

...In deductive reasonings, in which the general principles are assumed, and the question is concerning their application and combination in particular cases, the device which thus enables us to judge whether our reasonings are conclusive, is the Syllogism; and this form, along with the rules which belong to it, does in fact supply us with a criterion of deductive or demonstrative reasoning. The Inductive Table... in like manner supplies the means of ascertaining the truth of our inductive inferences, so far as the form in which our reasoning may be stated can afford such a criterion (PIS1, II, pp247-248).

Whewell seems to have believed that once inductive reasoning has been cast into its appropriate form the truth of inductive inference becomes luminous. Thus, he seems to have viewed logic as a kind of schematisation of previously worked-out trains of reasonings,² which somehow provides decision procedures for the truth and falsity of the propositions involved. Whewell did not ignore questions of the validity of scientific argument, but he obviously felt that such matters were not defining features of what he meant by either inductive or deductive logic.

Yet how does the "form" of inductive reasoning allow us to decide whether inductive propositions are true or not?

¹ cf., RH, p379.
The answer to this question illustrates the close linkage between Whewell's theory of induction and his intuitionist position on necessity. It was Whewell's claim that at each inductive step exhibited in the Tables, a general theoretical truth "includes" the particular facts which constitute it. But in order to satisfy ourselves that the facts are so included, Whewell claimed that a special "act of attention" is involved (PIS1, II, p248). This act of attention is simply, in Whewell's terms, the ability to intuit that certain facts are consistent with a particular general theoretical view. When we do this, we see that a theory "does really and exactly include the particular facts from which it is collected" (PIS1, II, p249). In this way the "form" of inductive reasoning is conceived by Whewell to provide a means of verifying the truth of particular inductive steps.

Whewell always believed that there had in fact been cases where a completely accurate intuition of the necessity of an inductive inference had taken place, thus constituting, for him sufficient proof of the induction. He was careful, however, to point out that the intuition of the necessity of an induction "finds its place in the mind gradually, as the contemplation of the consequences of the proposition, and the various relations of the facts, becomes steady and familiar" (PIS1, II, p255). Science, he said, does not begin with necessary truths, but arrives at them over a period of time. Initially, the discoverer makes an assertion which is set at the head of a train of reasoning as an hypothesis, and from which, step by step, consequences are derived, which either confirm or disconfirm the original assertion. If the latter occurs, the assertion is overthrown.
If the former occurs, the assertion may be said to be demonstrated. In this way the initial assertion which was put forward as a candidate for scientific necessary truth is intuited to be such. Whewell thus felt free to claim of the Inductive Tables that they constitute "the Criterion of Truth for the doctrines which they include" (PIS1, II, p258). For while the connection between high order propositions and lower order ones, down finally to observation statements, is intuited to be necessary, the general propositions merely collect together ascertained truths, "...so that the most general theory is only the assertion of a great body of facts, duly classified and subordinated" (PIS1, II, p258). By saying that the Tables can function as a criterion of truth, Whewell was saying no more than that they are schematisations of what is intuitable as necessary. 1

But there is a problem, for a proper inductive act in Whewell's view is characterised by more than an ability simply to "include and explain". For, in order to be true it must as well exclude the possibility of alternative forms of explanation. Whewell held that the ultimate "formula" of a true and valid induction is that "the several Facts are exactly expressed as one Fact if, and only if, we adopt the Conception and the Assertion of the inductive inference" (PIS1, II, p255). He hoped, as has been said, to give to induction a demonstrative certainty akin to that exhibited by deductive reasoning. Yet it is difficult to sympathise with Whewell's view of the matter while ever

1. Ibid., pp20-22. Deduction for Whewell, as for Descartes, likewise involves intuition. He believed that however long the chain of reasoning might be, we must see at every step in the reasoning the correctness of the appropriate move. Where Whewell distinguished induction from deduction was in claiming that the difference between them was only a difference in direction; see PIS1, II, pp257-258.
he held the view of induction he did. For it remains unclear just how the "form" of inductive reasoning could guarantee that a theoretical proposition must necessarily be of one kind and no other. If Whewell meant, as he undoubtedly did, by an inductive proposition, one which not only included the particular facts it was intended to explain, but also necessarily involved a new conception, and was a way of viewing the facts in a new light, his schematisation simply does not work. It does not capture and guarantee the certainty of inductive propositions at all, since in principle it ought always to be possible to view facts through a different conception and in a different light. Whewell's schematic logic could only work as he desired it to, if a general theoretical proposition were an expression of a complete and perfect enumeration of the facts it hoped to explain, which was exactly what Whewell denied of it.1

This point was forcefully brought home in a Review which De Morgan produced in 1858,2 in which De Morgan, although he fully admitted Whewell's philosophy of science as a methodology of scientific discovery, could only deplore the usage which Whewell wanted to make of the terms "logic" and "induction". First, against "induction" De Morgan argued that the term had been used traditionally to signify a generalisation from a group of particular cases to a universal claim concerning them. He went on to point out that, therefore, Whewell's notion of the colligation of facts by means of a new conception, although undoubtedly valuable as an account of the method of discovery, could not properly be called induction: "Let him have power to invent his meaning, and we have not a word to say. But our

protest is against the confusion between the old meaning and the new". 1 In the same way, De Morgan felt that Whewell's use of the expression the "logic of induction", to designate a schema of propositions exhibiting the conceptions of discovery arranged historically, was a travesty of the term "logic", since, he said, "nothing can more completely offend against the conception of a logical formula, as it has been in all time, than the notion that it is or is not a formula according to the nature of the matter which it contains." 2 The imputation of these criticisms was clear. While ever Whewell persisted in describing his method of colligation as a form of induction, he would never be able to guarantee the truth of such colligations by invoking his Inductive Tables, since those Tables do not provide the logical means of verification at all, and only emphasise the introduced conceptions.

Now it is important to recognise that Whewell had some notion of this problem and that he had tried to face up to it. After giving an explanation of his Inductive Tables, he restated the point that proper inductions are more than simple enumerations of particular instances, and followed on by posing the moot question, how then we can know that a particular proposition is necessarily implied by a set of data (PISI, II, pp254-255). Whewell's answer to this was straightforwardly Platonic; we recognise the truth of inductive propositions by so preparing and disciplining our minds that the relevant conceptions are

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1. Ibid., p44. This is not exactly the same objection as Mill's in his chapter entitled "Inductions Improperly So Called". Unlike Mill, De Morgan did not object to Whewell's analysis of colligation as such - he tended to agree with it - but only to Whewell's terming that process one of "induction". 2. Ibid., p44.
clearly conceived and the truth of the proposition intuited (PIS2, II, pp90-91). This reply of course arose naturally out of Whewell's claim that scientific investigation is essentially a process involving the clarification of concepts, so that proof, in the sense of logical proof, was not the ultimate aim of scientific reasoning. It is not really surprising then that Whewell should have undervalued such logical difficulties as the one introduced above. This would explain his private reply to De Morgan:

My object was to analyse, as far as I could, the method by which discoveries have really been made; and I called this method Induction, because all the world seemed to have agreed to call it so, and because the name is not a bad name after all. That it is not exactly the Induction of Aristotle, I know; nor is it that described by Bacon... I am disposed to call it Discoverers' Induction; but I dare not venture on such a novelty, except in the indirect way in which I have done. With such a phraseology I think my formulae are pretty near the mark, and my Inductive Tables a good invention. But I do not wonder at your denying these devices a place in Logic; and you will think me heretical and profane, if I say, so much the worse for Logic".2

As "formulae" of inductive truth, therefore, Whewell's Inductive Tables cannot be looked upon to perform a logical function at all. The truth of inductive propositions must devolve, therefore, upon an intuition of their necessity.

This answer, however, does not seem altogether to have satisfied Whewell,3 for he was led to admit that even though an enquirer with a sufficiently endowed and prepared mind could reach the state where he was convinced that an inductive inference was inevitable, he was yet faced with a spectre - the possibility that "...there may be other ways of expressing the substance of the truth established, besides that special Proposition which he has under his notice" (PIS1, II, p255). Effectively,

1. See above, Ch 2, pp36-39.
this amounted to a backdown on Whewell's part, a tacit admission that the "form" of inductive reasoning cannot guarantee that an inductive proposition must be of one kind and no other.\(^1\) And by acknowledging this, Whewell appears to be surrendering entirely his stated hope of providing the logical grounds for the truth of inductive inferences. Yet while this is so, Whewell certainly did not discount the possibility of some guarantee for their veracity. He did not abandon, either here or subsequently, the firmly-held belief that induction gives rise to truths that are necessarily such; he merely recognised the need for a more effective guarantee for this than had as yet been provided. This is an aspect of a larger problem for Whewell, and that is the problem of providing a justification for his intuitionist claims concerning necessary truth, and, especially, of providing a foundation for his Fundamental Ideas. Whewell's full recognition of this issue, and his search for an ultimate solution of it, will be considered immediately in the ensuing Chapter.

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\(^1\) The admission was made explicit in the later editions of the *Philosophy*; see PIS2, I, p.23, p.140n.
CHAPTER FOUR

"THE FUNDAMENTAL ANTITHESIS OF PHILOSOPHY"

1. THE PROBLEM STATED

To Whewell's claim, that the Fundamental Ideas are the constitutive elements in knowledge, from which it derives its necessity, it could be objected, how then can these Ideas guarantee the objectivity of this knowledge? For if the Fundamental Ideas are merely the subjective conditions of knowing, how is it that the science which is constructed out of them can have a reality which is independent of them, as Whewell claimed that it had? How, for example, can the theories resulting from induction, a process wholly dependent upon the sagacity and creativity of individual minds, be said to accord with the external world, to the exclusion of all other possible theories, while the character of this external world is known only by means of such induction? What is the guarantee that induction can accurately reveal reality? To put the matter concisely, what is the bridge between the subjective constitutive elements in knowing and the objective reality of what is known?

Whewell was a sufficiently astute thinker to recognise this as a problem and to realise that such a bridge was required if his claims for induction, necessary truth and the objectivity of knowledge were to be saved from the attacks
of his critics. The issue had been brought home to him by the Review which Herschel had written of the Philosophy in 1841, part of which was considered previously.¹ In that Review, Herschel mainly had called into question Whewell's claim that the concept of space is not derived from experience but is somehow prior to it, and had opposed to this his own, basically realist, view:

...The reason...we apprehend things as without us is, that they are without us. We take for granted that they exist in space, because they do so exist, and because such their existence is a matter of direct perception which can neither be explained in words, nor contravened in imagination: because, in short, space is a reality and not a matter of mere convention or imagination.²

Now Whewell was concerned to combat this criticism, for he by no means felt that scientific knowledge is a matter of convention. Quite the contrary, he was engaged in claiming that the propositions of science are necessarily true, and that the knowledge they entail is real knowledge. By simply terming space, for example, an Idea, he did not want to give the impression that this Idea, or any other, was devoid of reality, anymore than he wanted to give the impression that he denied the existence of the external world:

...Our real knowledge is knowledge, because it involves Ideas, real, because it involves Facts. We apprehend things as existing in space because they do so exist: and our idea of space enables us so to observe them, and so to conceive them (PIS2, II, p675).

Whewell in fact stated two claims which he recognised his philosophy ought properly to harmonise: i) that scientific knowledge is real knowledge, in the sense that it makes true claims about an externally existing universe; and

1. See Ch 2 above, pp32-36.
ii) that scientific knowledge is necessarily true (PIS2, II, pp675-676). Whewell, no less then Herschel, was inclined towards realism; but, unlike Herschel, he was at the same time inclined towards idealism, and his attempts to reconcile the two positions are illustrative of the difficulties involved in such a schizophrenic stance. The "Remarks on a Review" (PIS2, II, pp669-679), which was Whewell's published reply to Herschel, is proof of Whewell's recognition of a serious problem, but is also an expression of his temporary inability to offer any coherent solution to it. His second claim, he thought, had a ready-made basis in his Kantianism, that is, in his belief that the necessity of scientific knowledge is a result of "some property of the observing mind" in apprehending nature. But concerning his first claim, Whewell at that moment could do no more than assert his belief in the existence of the external world and in the ability of scientific knowledge to represent it (PIS2, II, p676). He was in despair:

...The reality of the objects which we perceive is a profound, apparently an insoluble problem. We cannot but suppose that existence is something different from our knowledge of existence:—that which exists, does not exist merely in our knowing that it does:—truth is truth whether we know it or not. Yet how can we conceive truth, otherwise than as something known? How can we conceive things as existing, without conceiving them as objects of perception? Ideas and Things are constantly opposed, yet necessarily co-existent. How they are thus opposite and yet identical, is the ultimate problem of all philosophy. Knowledge requires Ideas. Reality requires Things. Ideas and Things co-exist. Truth is, and is known. But the complete explanation of these points appears to be beyond our reach... (PIS2, II, p676).

1. My italics.
Whewell's recognition of this problem was a crucial point in the development of his thought. Indeed, the remainder of his philosophical career after 1841 can only be properly understood as an attempt to meet and to solve it, in effect to give, if not a "transcendental deduction" of the Kantian kind, then some other kind of "deduction" of the Fundamental Ideas.

As was said, the etiology of this problem for Whewell was mainly the Scylla and Charybdis, of conventionalism and subjective idealism on the one hand, and mere contingency on the other, which Herschel's Review had revealed to him. Now Whewell saw this aspect of the problem as part of a larger question, one which involved what he came to term the "Fundamental Antithesis of Philosophy". It is with Whewell's understanding of this expression, and the problems and issues arising out of it, that the remainder of this Chapter will be concerned. At this stage then it might be well to present Whewell's mature summary of the Antithesis, in order to facilitate an understanding of what he had in mind. The following statement was given in 1848:

The doctrine of the Fundamental Antithesis, which I have endeavoured to expound..., is briefly this:

That in every act of knowledge (1) there are two opposite elements which we may call Ideas and Perceptions; but of which the opposition appears in various other antitheses; as Thoughts and Things, Theories and Facts, Necessary Truths and Experiential Truth; and the like: (2) that our knowledge derives from the former of these elements, namely

1. See above, p 76.
our Ideas, its form and character as knowledge, our Ideas of space and time being the necessary forms, for instance, of our geometrical and arithmetical knowledge; (3) and in like manner, all our other knowledge involving a development of the ideal conditions of knowledge existing in our minds: (4) but that though ideas and perceptions are thus separate elements in our philosophy, they cannot, in fact, be distinguished and separated, but are different aspects of the same thing; (5) that the only way in which we can approach to truth is by gradually and successively, in one instance after another, advancing from the perception to the idea; from the fact to the theory; from the apprehension of truths as actual to the apprehension of them as necessary. (6) This successive and various progress from fact to theory constitutes the history of science; (7) and this progress, though always leading us nearer to that central unity of which both the idea and the fact are emanations, can never lead us to that point, nor to any measurable proximity to it, or definite comprehension of its place and nature (PA2, p617)

A full understanding of everything Whewell here had in mind will become clearer in what follows. For the present, however, it is important to realise that many of the things Whewell had to say concerning the Antithesis in the years following the first appearance of the Philosophy were not so much new claims for him, as reformulations of his basic epistemological position. The antithesis was explicit enough in the first edition, although not given that special title. Indeed it is only an expression of his basic Kantianism. At the same time, however, a new emphasis does become apparent in Whewell's writings after 1840, concerned particularly with how the antithesis might be used to solve the "ultimate problem of all philosophy". The only substantial changes between the first (1840) and the second (1847) editions of the Philosophy

1. Or at least a good part of it was - those parts represented by sentences 1 to 3 in the quotation above; see PIS1, I, p18f. The remaining sentences, it will shortly be seen, are developments out of it, and constitute Whewell's attempts to shore it up against collapse.
2. See PD, p332.
are those passages of Book I dealing with the antithesis. Whewell was candid about the significance of these changes. They were written, he said, with a view to bringing out the antithesis as "the basis of the work", for, he continued, "this doctrine, and its relation to the rest of the work, have become more clear in the years which have elapsed since the first edition" (PIS2, I, pxi). 1

It was then both as an attempt to explain and clarify his basic epistemology for puzzled critics such as Herschel, and as a means of solving the so-called "ultimate problem of all philosophy", that Whewell set out in the second edition to rework the antithesis and present it in perhaps clearer terms than he had expressed it in the first edition. On the whole, it can be said that Whewell's views on the antithesis did not change radically over the mature period of his philosophical life, but were merely augmented by proposed solutions to problems which either he had not seen initially, or if he had seen them, he recognised as not adequately met by his views as they then stood.

2. WHEWELL'S NOTION OF THE FUNDAMENTAL ANTITHESIS

Whewell's account of the antithesis, both in the first and later editions of the Philosophy is paradoxical.

1. R.E. Butts was the first to recognise the significance of these changes, along with the connection between Herschel's Review and Whewell's two Memoirs on the Fundamental Antithesis; see his paper "Necessary Truth in Whewell's Theory of Science", op. cit., p. 173f. This present account draws largely on his, but with an important difference. Much needs to be said, by way of explanation of Whewell's intellectual development, about his relations with the German philosophies of Shelling and Hegel, for Whewell's development loses an absolutely essential dimension if this matter is not taken into account. See below, pp 95-101.
For what in fact the antithesis involved is the reconciliation of what on the face of it would appear to be two irreconcilable claims: i) that the terms of the antithesis are distinct and can be made sharp and clear, thereby revealing it as fundamental to any account of knowledge; and ii) that while this is the case, in both thought and practice the distinction between the terms is obscure and imprecise, to such an extent that strictly speaking it can't be maintained at all. Now while Whewell himself admitted that this represented a paradox (PIS2, I, pl7), he obviously felt that the difficulties which were involved were surmountable, and that underneath no confusion reigned. This, at least, was what he set out to establish.

Let us look in turn at the two sides of Whewell's paradox, and then consider the way in which he sought to reconcile them.

i) THE TERMS OF THE ANTITHESIS ARE NOT CLEARLY DISTINGUISHABLE

Whewell made this claim in various forms. The best and clearest is in his discussion of the relation between "theory" and "fact", and their connection with the problem of perception. Here Whewell's views were, and remained, essentially Kantian. The perceiving subject is both passive and

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1. The elements of the antithesis were expressed by Whewell by various sets of terms, such as "theory" and "fact", "idea" and "sensation", "form" and "matter", "subject" and "object", "induction" and "deduction", "experimental truth" and "necessary truth", and the like. These couplets were not considered by Whewell to be identical, but were said to express different aspects of the antithesis. Because of their importance, and because they were the terms which most often cropped up in the Nineteenth Century discussions of Whewell's work, the discussion will confine itself to using the pairs, "theory" and "fact", and "subject" and "object".
active, passive in first receiving sensations, and active in transforming them into what are termed perceptions. In Whewell's words, "...I must both passively receive or have received impressions, and I must actively combine them and reason on them" (FA1, p172). This is the case even with the simplest and apparently most straight-forward of perceptions. In all cases of perception, he felt, some extra-factual, element is involved, some "inference", some "reasoning" (FA1, p173).

From this Whewell wished to draw the conclusion, that what we call a "fact" of perception cannot be divorced from an accompanying theoretical aspect. What the scientist commonly calls "fact" - the orbital motion of the planets, the attraction of the earth for the moon, and the like - Whewell argued, is in reality highly theoretical, for the reason that such claims can hardly involve less of inference, and indeed must involve more, than what is involved in simple perception. In this way he believed that "fact" may be said to merge imperceptibly into "theory".

But at the same time "theory" merges imperceptibly into "fact":

It is not a fact as well as a theory that we see the light reflected from the surface of a dew-drop, and do we not by our common language acknowledge it to be so?... Is not the greater or less resemblance of one plant to another a fact?... and is not, therefore, a classification, which is merely a collection of such resemblances, a fact also?... And with regard to..., the theoretical motion of the earth, is not that also a fact, if the theory be true? (PIS1, I, pp19-20).

L. Whewell would not allow even common sense to contradict his view. In a rather Platonic remark, he claimed that the senses are notoriously misinformative, to such an extent that a theory at apparent odds with fact (if such a thing can be the case while ever the distinction between them remains so imprecise), as the theoretical motion of the earth appears to be at odds with our experience of a stationary earth, may be said to correct the facts for us (PIS1, I, p20). In a sense theories are more truly factual than the facts themselves.
Both these claims, that there is no fact which is entirely devoid of theory, and that there is no theory (no true theory) that may not in turn become a fact, led Whewell to deny that there was any coherent distinction between the two terms:

...the opposition of fact and theory - a contrast which at first appeared so broad and plain - as we examine it becomes wavering; the application of the distinction full of difficulty. That which is a fact under one aspect is a theory under another. The most recondite theories, when firmly established are accepted as facts; the simplest facts appear to involve something of the nature of theory (PIS1, I, p21).

What must now be said is that in giving this account of the relation of theory to fact, Whewell lapsed inadvertently into the language of a subjective idealist, and seemed, for example, to be denying the reality of the external world. His words could not have been less unequivocal:

Does any one fancy that he sees a solid cube? It is easy to show that the solidity of the figure, the relative position of its faces and edges to each other, are inferences of the spectator; no more conveyed to his conviction by the eye alone, than they would be if he were looking at a painted representation of a cube. (PIS1, I, p23)

Or again:

...The scene of nature is a picture without depth of substance, no less than the scene of art; and in the one case as in the other, it is the mind which, by an act of its own discovers that colour and shape denote distance and solidity (PIS1, I, pp23-24).

Surely, or so it must have seemed to his readers, Whewell was denying the independent reality of the external world. Such, at least, was Herschel's reading of him. In fact, however, all Whewell really meant to be claiming by these remarks, was that the external world, which he believed does

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1. John Grote seems also to have been confused by Whewell's language. See his *Exploratio Philosophica*, Vol I, pp228f.
have an independent reality, is only known to man through a "mask of theory" (PIS2, I, p42). Indeed, it was his view that this "mask", far from obscuring the true nature of the external reality, actually, somehow, reveals it to us in a most clear and precise form. Just how was his essential problem.

But if such was Whewell's intention, it was not exactly obvious in his various claims at this point, and his critics may be forgiven for thinking his attempts to state his position to be "subjectivist", especially when he used the ambiguous expressions he did.

ii) THE TERMS OF THE ANTITHESIS ARE DISTINCT

If it was his idealist tendencies which suggested to Whewell that the distinction between the terms of the antithesis was obscure, it was his inclination towards realism which led him to assert the distinction as a genuine one. History, science and philosophy, he said, all demanded it. The distinction was fundamental for history and philosophy since, "...almost all the discussions of philosophers have been employed in asserting or denying, explaining or obscuring this antithesis" (FAI, p171). More particularly, it was fundamental for science, he believed, since to deny it would be to deny the existence of an external world, without which admission knowledge would be a chimera and philosophy impossible:

...No knowledge can exist without the practical union of the two (terms of the antithesis), nor any philosophy without their speculative separation. (PIS1, I, p33)

Whatever at any rate may have been Whewell's motives, the reasons he gave for maintaining the independence of the two sides of the antithesis must be considered. It seems
that initially the only positive ground he could give was one of familiarity, that is, of our acquaintance with facts as facts, and with theories as theories. In 1840, for example, he claimed that the intelligibility of the distinction rested on considering theory as conscious, and fact as an unconscious inference from Phenomena (PIS1, I, p24). Four years later the same point was put in the following way:

Theory and Fact are the elements which correspond to our Ideas and our Senses. The Facts are Facts so far as the Ideas have been combined with the sensations and absorbed in them: the Theories are Theories so far as the Ideas are kept distinct from the sensations, and so far as it is considered as still a question whether they can be made to agree with them. A true Theory is a fact, a Fact is a familiar theory. (FA1, p173)

The ground therefore for the distinction between theory and fact is, that while both fact and theory involve the union of Idea and Sensation and on that account are not clearly distinguishable, their separateness is guaranteed because with facts the union of Ideas with Sensations is intimate, while with theories the union is not as intimate.

An example may better help to understand Whewell's meaning. Man, he said, is the "interpreter" of Nature, but he often interprets without being aware that he is doing so, and this is largely the case with ordinary perception. But it is often the case with scientific judgements as well. Thus, what a scientist may regard as a fact may be more clearly seen by others to be an interpretation, albeit a scientific one, of natural phenomena, as a theory in other words. But why does the scientist regard what is essentially a theoretical interpretation as a fact? The answer is that the theory is so familiar to his mind, it is so much the means by which he interprets Nature, that he no longer regards it as a theory.
He, more than the common observer, regards it as intimately connected with phenomena, a regard which is so often reinforced when the theory is being used that it becomes entirely familiar, and therefore leads him to view the theory not so much theory, as fact. We should expect then that the only time that a scientist will recognise theory as interpretation is when there is some question as to how correct an interpretation of phenomena a particular theory is. But as soon as a theory is regarded as giving a correct account of phenomena, when it is no longer a matter of contention that Ideas can be made to agree with Sensations, then the scientist comes to look on a theory as a fact. That is why Whewell can say that "a true Theory is a fact, a Fact is a familiar theory".

The point need not be laboured that, even if it were clear, such a ground for the distinction between theory and fact will hardly guarantee each's independence or reality. For so stated Whewell's arguments slip easily into conventionalism, and it would be sufficient for a critic to object that while ever the distinction between theory and fact rests on the state of a person's consciousness, and while ever there is no reason for believing one person's consciousness more adequate than another's in determining the truth of a situation, the independence of theory and fact is not ensured. Whewell himself was only too painfully aware of this:

Yet the distinction thus stated is far from being rigorous and permanent...Education, habit, the degree of self-observation, the circumstances of the case, all serve to make the person unconscious or conscious of mental acts in innumerable degrees...Hence if this greater or less consciousness of our own internal act be all that distinguishes fact from theory, we must allow that the distinction is still untenable. The boundary-line again melts away; the difference is unsubstantial; the opposition loses its significance as we examine it(PISI, I, pp24-25).
One is left wondering from all this just what a "fact" and a "theory" are for Whewell. On one side of the antithesis he is denying the distinction between the two, and, if his Kantianism is to be taken seriously, seems to be saying that since all facts "involve something of the nature of theory", then there is really no such thing as pure fact. This however would seem to lead him to deny for science any semblance of objectivity. On the other side of the antithesis he is arguing for the independence of theory and fact, attempting thereby to ensure the claim that it is only from facts that knowledge can be said to gain its objectivity.

The problems associated with this difficult situation are, on the whole, a consequence of the tensions inherent in the two sides of the antithesis. Indeed, Whewell himself neatly put the contradiction when he said that the position he was defending entailed "an antithesis of inseparable elements". His attempts to avoid the paradox were like those of a man who is constantly shifting the weight of his body from one foot onto the other, and back again, in order to relieve the pain of two thorns, one lodged in each foot. The game is useless while both thorns remain. That is why so many passages in the Philosophy appear confusing and contradictory, for what they represent, figuratively, is the raising and replacing of Whewell's tender feet - the antithesis is real, yet the terms of the antithesis are not clearly distinguishable, yet the antithesis is real, ... and etc. Surely this is the type of the "shuffling" of which Hegel spoke in his famous criticism of the Kantian dualism:
In every dualistic system, and especially in that of Kant, the fundamental defect makes itself visible in the inconsistency of unifying at one moment, what a moment before had been explained to be independent and therefore incapable of unification. And then, at the very moment after unification has been alleged to be the truth, we suddenly come upon the doctrine that the two elements, which, in their true status of unification, had been refused all independent subsistence, are only true and actual in their state of separation. Philosophising of this kind wants the little penetration needed to discover, that this shuffling only evidences how unsatisfactory each one of the two terms is.

It bears repetition, that Whewell himself recognised something of this. But out of reasons of conviction he could not look for a way out of the difficulty by denying one or other side of the antithesis. For such a move, although possibly it may have resulted in consistency for his views, would have been at the expense of his dearest beliefs concerning science. Rather, he looked around for a means of preserving the antithesis by shoring it up, specifically, by attempting to provide a bridge between each of the two elements involved. What he wanted in fact was a kind of objective idealism.

Such a move was rather the usual one for many of the mid-Nineteenth Century philosophers. Hegel's attempts, of course, come immediately to mind. Yet it would be a mistake to imagine that Whewell's struggle was exactly the same as Hegel's, or that the solutions of each man have a close community. Whewell in fact actually rejected Hegel's solution, more, so it will appear, out of an inability to perceive Hegel's position as cojent than out of any genuine disagreement between his own and it. At least, it is clear that the objectivity

2. See Section 4 below, pp95-101.
which Whewell sought for his idealism was not the "objectivity" of Hegel. This can be seen by first examining Whewell's proposed solution to the problem which had plagued him, and afterwards by examining his rejection of the Hegelian solution to what in many respects is a like problem.

3. WHEWELL'S SOLUTION TO "THE ULTIMATE PROBLEM OF ALL PHILOSOPHY"

The possible means of solving his problem had occurred to Whewell as early as 1844, though not yet the actual solution. In the first Memoir on the antithesis, which formed the basis for the reworked section of Book I of the Philosophy, he remarked that, if it were true that in knowledge two elements were involved, yet involved in such a manner that even thought cannot render them separate, then they are, in a sense, dependent on one another. In Whewell's words, "...it must always be possible to derive one of these elements from the other, if we are satisfied to accept, as proof of such derivation, that one always co-exists with and implies the other" (PAI, p175). Neither element, then, may be said to have primacy over the other. For example, we might be tempted to claim that since space is a form of the knowing mind, all sensible intuition is dependent upon it, and is a modification of the "Idea". But as true as this is, Whewell said, such a claim is an incomplete and essentially one-sided account of knowledge, since every Idea, as well as limiting is, equally, limited by the sensory material which it informs. The idea of space then

is no more prior to particular sensations than these sensations are prior to the idea of space (FA1, pp.175-176). It was in this sense that Whewell claimed for the two elements of the antithesis a derivability from one another. In 1848, in the second Memoir, the point was taken a step further. In the summary of the antithesis which was given initially, sentence (4) reads, "but... though Ideas and perceptions are... separate elements in our philosophy, they cannot, in fact, be distinguished and separated, but are different aspects of the same thing". Shortly after, Whewell remarked that this "thing" is the "original basis of truth in which both elements are involved and identified... while each of the separate elements is subjected to conditions arising out of their union" (FA2, p.618). Clearly then the two Memoirs represent Whewell's search for a kind of "third man" as a guarantor for his claims concerning the antithesis. At the same time it is clear that the derivability of the two elements of the antithesis from a third, more pervasive "thing" was only asserted, and had not yet been proved. For Whewell had not characterised this "thing" in any way, although, obviously, this was what now needed to be done.

The complete solution was offered only in 1853, in the suppressed pages of a popular and controversial work entitled Of the Plurality of Worlds. The relevant pages were

1. See above, pp.78-79.
3. Together with the Bridgewater Treatise of 1833, the Plurality was the work which made Whewell's name familiar to the general reading public. As the title suggests, it was concerned with the question of extra-terrestrial life, and in it Whewell defended the claim that no scientific evidence could be advanced for the popular belief in the existence in the universe of beings other than ourselves. It initiated a great controversy, and was attacked, particularly, by Sir David Brewster, cont'd p. 91
suppressed by Whewell on the advice of Sir James Stephen, the Cambridge historian, who apparently thought them too metaphysical, and not pertinent to what was intended as a popular work. Nevertheless, Whewell slipped one or two short passages conveying their tenor and intent into the final version of the book (PW, p109), and it is from these that it is discovered that Whewell had his solution by 1853. But because these passages are short and are contained in a work separate from that in which the problem they were intended to solve was stated, and since the substance of the suppressed pages was included by Whewell in the Philosophy of Discovery in 1860, and there tied explicitly to the problem being considered, the earlier version of the solution will be passed over and the later and more explicit version will be examined.

In the Philosophy of Discovery, in the Chapter titled "The Theological Bearing of the Philosophy of Discovery", Whewell took the problem up again and asked his Kantian question:

...When we are come to the conclusion that science consists of facts idealized, we are led to ask, How can this be? How can facts be idealized? How can that which is a fact of external observation become a result of internal thought? How can that which was know a posteriori become known a priori? How can the world of things be identified with the world of thoughts? How can we discover a necessary connexion among mere phenomena?

Or to put the matter otherwise: How is it that the deductions of the intellect are verified in the world of sense? How is it that the truths of science obtained a priori are exemplified in the general rules of facts observed a posteriori? How is it that facts, in science, always do correspond to our ideas?...It being established, then, that in the progress of science, facts are idealized - that a posteriori truths become a priori truths...the question recurs which I have already asked: How can this be? (PD, pp354-358)
Whewell's answer was given by his considering what was, for him, a more fundamental question, that of how the world external to us, and the world of our mental experience have come to be what they are. And, of course, for Whewell, the Doctor of Divinity, the preacher of Trinity College Chapel, the theist of the Bridgewater Treatise, the answer to that question was a foregone one. They were so because God created them. More to the point, he created them in adaptation to one another:

...the truths which exist or can be generated in man's mind agree with the laws of the universe, because He who has made and sustains man and the universe has caused them to agree; - that our Ideas correspond to the Facts of the world, and the Facts to our Ideas, because our Ideas are given us by the same power which made the world, and given so that these can and must agree with the world so made. (PD, pp358-359)

Thus, God is Whewell's ultimate solution, the final justification of his faith in induction, his "third man" from whom facts and Ideas are derived and in whom they are combined and unified. God, who created the world, also created the laws of nature which prevail among the world's phenomena. They are manifestations of Ideas in the Supreme Mind. But God also created man. Thus, Nature and man may be said to share something of the Divine Mind. The adaptation, therefore, between man's intellectual discoveries in science and the world of nature, between theory and fact, is not surprising and is only a consequence of God's beneficence in providing a bridge between the two. For he has created man with a capacity for an accurate acquaintance with the world of nature. Taking the Idea of Space as an example: a priori geometrical truths based on that Idea are verified universally by phenomena for the following reason, that the Supreme Mind has constructed the
universe according to the idea of Space, the universe conforms to the Idea of Space. At the same time, man, who is also a product of God, also has an Idea of Space; it is "necessarily evoked and awakened in the human mind existing in the universe". Since space then is a constituent both of the universe and of the human mind, and since in both cases it has its origin in God, everything following from the idea, both in the universe and in the human mind, will necessarily coincide, since God wills it. The same is the case with the other Fundamental Ideas, time, force, matter, substance, final cause, and so on. In all these cases these Ideas of the human mind are also ideas of the Divine Mind, by which the universe was constructed:

Man has these thoughts, and sees them verified in the universe, because God had these thoughts and exemplifies them in the universe (PD, p371).

1. There has been a subtle shift in Whewell's usage here of the term "Idea". It is quite clear that the ideas of the Divine Mind are not Ideas in the sense of Kantian constitutive principles, since, if they were, this would imply God's limitation, a consequence which Whewell could not have accepted. Rather, God's ideas must be looked at as "objects of thought", as concepts. The link between the ideas which God has, and man's ideas, is not broken however. For it is easy to imagine that although God, being a free agent, could have created the world according to any set of concepts which he might have chosen, once he had decided upon certain concepts with which to work, He would have imposed these upon man as Fundamental Ideas. Whewell's claim, that man is more or less obliged to see the world as God has made it, still holds therefore.

2. It should be pointed out that this solution to the problem of the ultimate ground of the antithesis was by no means original with Whewell. In the Nineteenth Century it had already been anticipated in Carlyle's rather confused "philosophy of symbols" (See Sartor Resartus, Book 3, Ch III), and perhaps also owed something to Coleridge's mysticism. But it had an even longer history, being explicit, for example, in Galileo's writings, with which Whewell was thoroughly acquainted. Its origin, however, extends as far back into history as the Neo-Platonist philosophers, Philo and Augustine, who, aware of the problems involved in the Christian's acceptance of a sphere of impersonal, immutable essences, interpreted the Platonic ideas as the thoughts of the Divine Being Himself. Thus, Augustine remarks: "The ideas are archetypal forms or stable and immutable essences of things, which cont'd p 94
But there are several problems associated with this claim. To begin, Whewell has not provided in any detailed way an explanation of how his solution is to work in a particular case. His discussion is confined to generalities, and he seems to assume that it is sufficiently stated to ensure his meaning. Indeed, he has no more to say on the matter than the meagre amount which has been considered above. Secondly, Whewell did not even recognise the difficulties for his position - as Descartes had in a similar circumstance - were God to be a mal génie, deceiving us into believing that our Fundamental Ideas have a veracity which they do not really possess. And finally, it is not clear why, even if it were thoroughly and consistently worked out, a theological justification of the Ideas would achieve what Whewell desired, since Whewell gives no substantial justification of the epistemological status of his theological claims. With them, therefore, he is involved in exactly the same kind of difficulty as with the justification of the Fundamental Ideas, though at one further remove. 1

Whewell's solution to "the ultimate problem of all philosophy", with its mixture of Platonism and theology, has now been expounded and criticised. There remains however a residual matter of some importance which need to receive some consideration if the full extent of Whewell's philosophy is to be appreciated. This is the issue of Whewell's rejection of the solution which Shelling and Hegel had developed to over-

cont'd p93. have not themselves been formed, but, existing eternally and without change, are contained in the divine intelligence". On this last point see especially, Copleston, F.: A History of Philosophy, Vol 2, Pt I, p75. Whewell himself believed his final solution to be strongly Platonic; see PD, p359.

1. On this last point, see Butts, R.E., "Necessary Truth in Whewell's Theory of Science", op. cit., p179.
come difficulties they recognised in Kant's philosophy, and Whewell's consequent choice of a more traditional type of solution to his own, basically Kantian, problems. These two issues, it will be seen, are closely related.

4. THE REJECTION OF SHELLING AND HEGEL

Whewell spoke German sufficiently well to be acquainted with the latest developments in German thought. It is plain, for example, that during the 1840's, at just the time when the problem of the antithesis was occupying his thinking most fully, he was reading Fichte, Shelling and Hegel.\(^1\) Since he did know these writers, since his own views in so many ways owed so much to Kant, and since the criticisms being leveled at Kant, particularly, as was seen,\(^2\) by Hegel, concerned just that aspect of the Kantian epistemology which Whewell had fully embraced, the Kantian dualism, it is essential to ask how Whewell responded to the challenge their opinions represented.

Whewell's understanding of the whole of post-Kantian German philosophy rested explicitly on a commitment to a kind of perennial philosophy(FA2, p617). Each system of philosophy, Whewell believed, since it comes after other systems, is in a peculiar position to criticise them, in the sense that, since every system itself aspires to be true, it will attempt to include within its framework everything it

\(^{1}\) see FA2, p 617f.  
\(^{2}\) see above, p 88.
takes to be true in all preceding systems. Contemporary German philosophy, he felt, had done just this, and particularly in connection with the development of the notion of the fundamental antithesis (FA2, p617). Exactly how this was so can best be seen by considering in detail the positions which Whewell assigned to several important thinkers in the working out of the antithesis.

Kant, Whewell said, was the first philosopher to recognise that knowledge derives its form and character as knowledge from Ideas. This was his great achievement. However, the fault in Kant's philosophy, he believed, lay in the claim that "noumena" were unknowable:

...Besides the phaenomenon which we see, Kant acknowledged a noumenon which we think of; but this assumption, for such it is, exercises no influence upon his philosophy. Things in themselves, are in his Drama, merely a kind of mute personages...which stand on the stage to be pointed out and talked about, but which do not tell us anything, or enter into the action of the piece...Kant compares the change which he introduced into philosophy to the change which Copernicus introduced into astronomical theory...But...we may say that Kant, in explaining the phenomena of the heavens by means of the motions of the earth, has almost forgotten that the planets have their own proper motions... (FA2, pp619-620).

This criticism is paralleled in Whewell's view of Fichte. Fichte, he felt, had merely developed Kant's subjectivism to its logical conclusion, to a point where everything is arrived at from the conditions and standpoint of the observer:

...he makes the whole drama into a kind of monologue; in which the author tells the story, and merely names the persons who appear...(FA2, p619).

Fichte, like Kant, had emphasised the subjective aspect of knowledge, while forgetting that knowledge had its objective side, its "own proper motions". This type of subjectivism Whewell was concerned to avoid.
Now Shelling and Hegel, Whewell felt, at least had seen these defects in the idealism of Kant and Fichte, and had sought to avoid them. They had reasserted the importance of an "objective" as well as a "subjective" element in knowledge, by stressing both Ideas and Sensations. Shelling had done so, Whewell said, by claiming that Ideas and Facts are different aspects of the same thing, and Hegel had done something of the same, in so far as his philosophy was "rather a more methodical and technical exposition of Shelling's Philosophy of the Absolute than a new system"(FA2, p618). However, both Shelling and Hegel had added to this by identifying Ideas and Facts, and by locating this identification in the so-called "Absolute". But this was a point, Whewell claimed, "which we can never reach". His reasons were plain; to locate the objectivity of knowledge in the Absolute, Whewell felt, was to revert to a species of "Cartesianism"(FA2, p620). By this Whewell did not mean that Shelling and Hegel had adopted Descartes' metaphysics, but rather that they were examples of armchair-philosophers, practicing what Bacon had termed the "anticipation of nature":

...Shelling, going upon the principle that the whole of drama is merely a progress to the denouement, which denouement contains the result of all preceding scenes and events, starts with the last scene of the piece, and bringing all the characters on the stage in their final attitudes, would elicit the story from this(FA2, p619).

Hegel, Whewell claimed, was guilty of doing the same, and although he was acutely aware of the importance of treating knowledge historically, nevertheless, he had been careless of historical detail.¹ Such a thinker, who had completely misrepresented the detail of the "historical progress and

¹. On this point see Appendix Note D.
actual condition" of knowledge, who had attempted to extend his views to include that detail "only by conjecture, by a sort of divination", who had slighted "all scientific truth and inductive reasoning", surely was not a thinker who could be looked to to provide a sound and workable philosophy (FA2, p620)? Such speculations appeared to Whewell to be, as he said some twelve years later,

...more barren, and more certain to be barren, of any results which have any place in our real knowledge, than the most barren speculations of the schoolmen of the middle ages: which indeed they much resemble in all their features - their acuteness, their learning, their ambitious aim, and their actual failure. (PD, p321)

Thus, Whewell read Shelling and Hegel's reaction to Kantianism as correct only up to a point. They had been right to claim the knowability of "noumena" and they were right to assert that the two sides of the antithesis were different aspects of the same thing. But they went too far in identifying the two sides with the Absolute. To do so was a kind of "rash and blind" a priorism in Whewell's view. But the question now is, why exactly did Whewell think that Hegel and Shelling were wrong to do this, why were their speculations to be and forever to remain more barren than those of the schoolmen?

The answer to this question has already been anticipated in several of the passages from Whewell which have been recently quoted. It is the key to Whewell's rejection of Hegelianism, and, indeed, it could be said that it is the key to the long delay to the acceptance of Hegel in Britain.\(^1\) It also helps to explain why Whewell went on to adopt the mixture of Platonism and theology which comprised his solution to the problem of the antithesis, as well as helping to explain

\(^1\) See footnote 1, p 101 below.
the fortunes of Whewell's philosophy at the hands of the 
generation of idealist philosophers who followed him in 
Britain. The answer is, Whewell felt that the philosophy of 
the Absolute was impious. How was this opinion reached? 

It was stated earlier, in explaining Whewell's 
solution to the antithesis that he claimed man had a capacity 
for an accurate acquaintance with the Divine Ideas. But this 
is not quite the case, or, at least, it is the case, but with 
some important reservations. For in places Whewell appears to 
have been bothered that his claims concerning the coincidence 
of the human with the Divine mind might be thought presumptuous, 
and "too limited and material a notion of the Divine Mind to be 
accepted by a reverent philosophy" (PD, p 360). In order to 
avoid giving this impression, therefore, Whewell placed an 
important limitation on his claims. Coincidence with the 
Divine Ideas is complete, he claimed, "only so far as the human 
idea goes", so that for God, His Ideas "may easily have so much 
more luminousness and comprehensiveness as Divine Ideas may be 
supposed to have compared with human" (PD, p 360). What Whewell 
seems to have had in mind here is not the Seventeenth Century 
notion that man, although limited in an extensive acquaintance 
with the Divine Mind, may have an intensive acquaintance with 
it on certain subjects. Such a position was explicitly denied 
by Whewell (PD, pp 360-361; 371). Rather, he seems to have 
felt, that in the case of the idea of space, which is the first, 
most fundamental, and certainly the most clear and comprehensive 
of all the ideas on which the sciences are based, that such an 
idea is only an imperfect representation of the Idea of space.

1. An account of the later Nineteenth Century idealists' reaction 
to Whewell's philosophy is developed at length in Ch 5.
which the Divine Mind possesses:

That the Idea of Space in the human mind, though sufficiently clear and comprehensive to be the source of necessary truths, is far too obscure and limited to be regarded as identical with the Divine Idea, will be plain to us, if we call to mind the perplexities which the human mind falls into when it speculates concerning space infinite. An Intelligence in which all these perplexities should vanish by the light of the Idea itself, would be infinitely elevated in the clearness and comprehensiveness of intellectual vision above human intelligence, even though its Idea of Space should coincide with the human Idea as far as the human Idea goes. (PD, p361)

Ultimate knowledge then is not possible, and Whewell is led to remark:

...in every case, man, by following the train of thought involved in each Idea, runs into confusion and seeming contradictions. It may be that by thinking more and more, and by more and more studying the universe, he may remove some of this confusion and solve some of these contradictions. But when he has done in this way all that he can, an immeasurable region of confusion and contradiction will still remain; nor can he ever hope to advance very far, in dispelling the darkness which hangs over the greater part of the universe. His knowledge, his science, his Ideas, extend only so far as he can keep his footing in the shallow waters which lie on the shore of the vast ocean of unfathomable truth. (PD, p371).

This religious motive was an important one for Whewell, and it explains why he felt Shelling and Hegel's philosophies to be presumptuous and of no use for the purpose of providing a solution to his "ultimate problem". Although his understanding of Shelling and Hegel's religious claims was certainly unsubtle - it does not seem to have extended much beyond a general condemnation - Whewell, nevertheless, recognised that their remarks concerning the Absolute referred to an Absolute Being. Assuming then that they were claiming an acquaintance with Him and with His purposes, assuming in effect that Hegel and Shelling had in some way reduced and domesticated God, Whewell felt entitled to dismiss a good deal of their philosophical stance as "Cartesian", that is, anticipatory and
presumptuous in the worst possible way. No matter to what extent man's Ideas correspond to the Divine Ideas, Whewell believed, they will always fall short of a complete and intensive correspondence. Consequently, man's knowledge of the universe, and in particular of its final tendencies and purposes, is and always must remain essentially incomplete. To assume otherwise, as he thought Shelling and Hegel had done, is to assume too much. 1

5. FACTS

Having considered Whewell's justification of the Fundamental Ideas, and some of the difficulties associated with it, one or two remarks might be developed now concerning the relevance which a consideration of Whewell's "ultimate problem" has for some modern discussions in the philosophy of science. For although the conclusion has been reached, that Whewell's proposed solution to the question of the bridge between thought and reality is unsatisfactory, Whewell's statement of the problem may be viewed as a particularly perceptive and clear-sighted step in the right direction. Certainly, at any rate, it is a significant expression of what is a modern issue, for some of

1. The claim that Hegel and Shelling were misread by many of their British opponents as a threat to conventional religious opinion, in seeming to propose too radical an acquaintance with God, and that this mistaken reading was a prime reason why the recognition of them by British philosophers was delayed, is confirmed by a remark of J.F. Ferrier: "(Our British philosophers) have misconceived the nature of 'the absolute', and have failed altogether in their attempts to refute the philosophy which expounds it. They have supposed that the question concerning 'the absolute' was a question which referred to the quantity or amount, and not one which referred to the quality or nature of knowledge and truth"; see his "Biography of Hegel", Works, Vol III. pp566-567.
the chief concerns which recently have engaged philosophers of science, though couched in Twentieth Century terminology, are just those with which Whewell struggled in the Nineteenth Century: - what precisely is the connection between the products of intellection and external reality?: in what way may science be said to provide objective knowledge?: what exactly is "given" in perception?: and so on. The means by which this claim can best be illustrated is by considering more fully than has already been done Whewell's understanding of the term "fact".

For Whewell's rather confused understanding of this term is the doorway to some interesting and pertinent problems in the philosophy of science.

Whewell's language concerning facts is problematic. At one point he seeks to deny the distinction between fact and theory, while at another point he argues for their independence. These claims are certainly puzzling and contradictory, but perhaps to some extent the issues which they involve can be clarified. Initially, so Whewell believed, all science begins with what he called "common observation", not amounting to much more than the simple perception of objects - the appearances of stars and planets, the usual distinctions of well known plants, and the like (PIS1, II, pp202-203). But perceptions for Whewell are never "pure" in the sense of being altogether devoid of a theoretical aspect, since, as Whewell continually claimed, even at the level of the most basic perceptions, sensations and Ideas are always united. Whewell did believe however that this initial level in scientific procedure

1. See Section 2 above, pp 81-87.
was distinguishable from subsequent levels, since at the initial stage the "union" of sensations and Ideas, he said, occurs unconsciously; "...this is so far generally acknowledged", he remarked, "that according to common apprehension, the mind is passive rather than active in acquiring the knowledge which it receives concerning the eternal world" (PIS2, I, p26).

Now, Whewell pointed out, it is this level of common observation which is often termed "the facts", and is the touchstone to which philosophers often resort in their attempts to confound the arguments of their opponents. This kind of appeal to "the facts", Whewell suggested, is understandable while ever the theoretical element which such "facts" involve is not generally recognised. Nevertheless, for Whewell, such appeals to an allegedly incontrovertible body of fact are misconceived; this is clear once we realise that the so-called "facts" are never so completely devoid of theory as to be pure fact. Such a claim of course has rather a modern ring about it, and compares for example with Feyerabend's attack in his essay, "Problems of Empiricism", on the idea that "observation statements must possess an unchangeable core of meaning", and with Hanson's statement in Patterns of Discovery that all causal talk is "theory-laden".

Despite this impurity, as it were, of perception, the level of "common observation" remained for Whewell the genuine basis upon which the theories of science are constructed. Theories, it was seen, are the result of the colligation of

2. Hanson, R.N.: Patterns of Discovery, p2.
"facts" by means of a conception. It is only this procedure which for Whewell constitutes science proper, and it is distinguished from the level of unconscious "common observation" only in that colligation involves an explicit and conscious union of Ideas and sensations.

Thus the union of Ideas and sensations is essential at every level in the ascent from the initial "facts" of "common observation" through to the highest level theories. What is of importance here is that low level "facts" once colligated are said by Whewell to be idealised (FA2, p615), and to become in effect "true theories" (PIS2, I, pp39-40). By claiming this, Whewell is free to argue that "facts" from one point of view are "theories" from another (PIS1, II, p259).

Thus colligation, he said, produces a scientific proposition which for everything beneath it in the scheme of Inductive Tables will be a "theory", colligating as it does all the lower level "facts"; while for everything above it in the Tables it will be a "fact", being itself subsumed in a colligation which occurs above it. For example, Kepler's laws of celestial kinematics, and Galileo's laws of terrestrial motion are highly theoretical inductions which explain and predict a large number of observations. Nevertheless, Whewell would have said, at the same time they themselves are "facts" which were employed by Newton in his wider theorization. The terms "fact" and "theory" then, for Whewell, have a relative reference - their difference "is a difference of relation", he said - and cannot therefore be applied rigorously to particular situations and withheld from

1. As was seen in Ch 2 this plays an integral part of Whewell's theory of necessary truth.
others (PIII, II, pp258-259).

It would not be irrelevant here to digress a little, and to consider an important purpose to which Whewell applied this stance on the meaning of "fact". For the relational nature of facts was a prime feature of Whewell's polemic against positivism, especially the positivism of Mill and Comte. Thus, for example, Whewell maintained against Mill's argument, that because the elliptical orbit of Mars is "a fact which we could see if we had adequate visual organs and a suitable position" it can not therefore be the result of induction, that although the orbit might quite correctly be regarded as a "fact", it is, no less, a "fact" reached through an inductive act. And Whewell could say this just because he held that "facts", even those of the adequate-visual-organ-and-suitable-position kind, are always dependent upon some Idea. Thus Whewell chides:

In truth, as I have repeatedly had occasion to remark, all attempts to frame an argument by the exclusive or emphatic appropriation of the term Fact to particular cases, are necessarily illusory and inconclusive. There is no definite and stable distinction between Facts and Theories; Facts and Laws; Facts and Inductions. Inductions, Laws, Theories, which are true, are Facts". (OI, p23)

The same general point is made against Comte's positivism, and in particular against his attack on what for Whewell was the essential role of metaphysics in scientific advancement. Two related arguments were levelled by Whewell. First, Comte's analysis of the progress of knowledge into the theoretical, metaphysical and positive stages, and his injunction to scientists to abandon the first two stages, particularly the search for causes, and to confine themselves

1. Mill, System, op. cit., p194; see also Ch 3 above, p56.
to the third, of seeking only for the laws of phenomena, is proved false, in Whewell's opinion, by the actual historical development of science:

There is no science in which (Comte's) pretended order of things can be pointed out. There is no science in which the discoveries of the laws of phenomena, when once begun, have been carried on independently of discussion concerning ideas... There is no science in which the most active disquisitions concerning ideas did not come after, not before, the first discovery of laws of phenomena (PD, p227)

Second, Whewell claimed that Comte's positivism is not only false in practice, but as well is "contrary to sound philosophy in principle", since his "positive" stage does not actually avoid the metaphysics which he flattered himself that it did. Thus Whewell says, "such a rule would defeat its own object; for the laws of phenomena, in many cases, cannot be even expressed or understood without some hypothesis respecting their mode of production" (PISI, II, p268). Whewell's reasons here are an echo of his stand on "facts" which has been discussed; even the simple statement of what the phenomena are involves the employment of theoretical and metaphysical notions and can not be achieved without them.

Thus, Whewell goes on to protest that it is to no avail that Comte should claim that what is being expressed by his "positive" stage is simply the description of facts, as though this were to shun forever the excrescence of metaphysics:

M. Comte, Whewell said, will say that Newton's discovery of gravitation only consists in exhibiting the astronomical phenomena of the universe as one single fact under different points of view. But this fact involves the idea of force, that is, of cause (PD, p228).

Or again,

M. Comte's favourite example of physical research is Thermotics, and especially Fourier's researches with regard to heat (but) the flux of caloric, of which Fourier
speaks, cannot be conceived otherwise than as implying a material flow. M. Comte apologizes for this expression, as too figurative, and says that it merely indicates a fact. But what is the flow of a current of fluid except a fact? And is it not evident that without such expressions, and the ideas corresponding to them, Fourier could neither have conveyed nor conceived his theory?(PD, p232)

Because Whewell believed that even the simple expression of phenomena involves theoretical notions of one kind or another, he felt that it was a pedantic and barren injunction on Comte's part to try to persuade scientists that they ought to refrain from the search for causes. Of course Whewell recognised that some causal theories will turn out to be wrong, but this is the very thing scientists must be allowed to resolve. And to try to prohibit the legitimate, indeed, inevitable metaphysical arguments in which scientists are involved when they employ Ideas, Whewell said, would only be to attempt to "secure ourselves from the poison of error by abstaining from the banquet of truth"(PD, 233). The point was not to abstain from metaphysics, but in using it to recognise it for what it is:

Physical discoverers have differed from barren speculators, not by having no metaphysics in their heads, but by having good metaphysics while their adversaries had bad; and by binding their metaphysics to their physics, instead of keeping the two asunder. (PIS2, I, px)

The retreat from metaphysics then, which Comte and other positivists sought, for Whewell is an impossibility; if it were ever effected it would mean simply that science had ceased to exist(PIS1, II, p269).

After this long diversion into positivism we should return to our chief concern, Whewell's understanding of the term "fact". At this stage of the discussion we may
perhaps be in a position to make some sense of Whewell's notion of "elementary facts", which, along with Ideas, were said to constitute the products of the "decomposition of facts". This notion had earlier proved a difficulty. However, appreciating the relational view which Whewell mainly had of "facts", and knowing that "facts" for Whewell were the product of the union of Ideas and sensations, we are able to recognise that "elementary facts" are not really "facts" in the relational sense, and of which we might claim that they too ought to be decomposable, but, rather, are actually "sensations".

But what then are "sensations" for Whewell? They are described at one point as the "matter" which "we receive from without" (PIS2, II, p27). But this is ambiguous because it is not clear from this whether in Kantian terms "sensations" are the manifold of experience, or intuitions, or conceptualised intuitions. Whewell did not explicitly approach the matter of defining "sensations" from the direction of Kantian terminology. He did distinguish them, however, from perceptions, by claiming that they were "the impressions upon our senses" which the Ideas are to connect (PIS2, I, pp25-27). "Sensations" then would seem to be distinct from "common observation", and certainly from colligated facts, both of which involve Ideas, that is, some kind of intellection. What then Whewell ultimately appears to have had in mind by "sensations" is the non-intellectualised given in perception, upon which the Ideas are to act, a kind of Kantian manifold. "Sensations" therefore

2. See above, p47.
may not be construed as a kind of "protocol sentences", constituting the starting point for a calculus. Indeed, they are not "sentences" at all, but are simply "given", and introduced into a system of scientific thinking as the requisite "material" upon which theorisation depends. If this is so, Whewell here would seem to assume that an externally existing world of real objects acts as the source of these "sensations", inevitably and automatically giving rise to the same kind of "sensation" for each respective external existent. Thus he says, "a colour, a form, a sound are not produced by the mind, however they may be moulded, combined, and interpreted by our mental acts" (PIS2, I, p26). Such things, Whewell assumes, somehow exist independently of our knowing them and are only shaded, not fundamentally altered, by our mental apprehension of them. And it is because of this belief that Whewell feels he can claim that "sensations" provide theories with a guy-rope as it were anchored in reality.

Thus by employing the expression, "elementary facts", Whewell slips into a rather commonsense usage of the term "fact", against which his relational use was fundamentally opposed. For Whewell's "elementary facts" are just that "unchangeable core" to which he could direct attention if ever he were asked to justify his belief that natural science provides us with genuine objective knowledge. It appears probable as well, that it was because of his commitment to this kind of "factual

1. Whewell's use of 'matter' has a rather Aristotelian ring about it. Sensations are the pure matter for which the Ideas provide the forms, and just as for Aristotelians matter never exists except as informed, so sensations are never received in perception except as Idealised. This does push Whewell's talk of "sensations" towards the Kantian manifold rather than towards Kantian intuitions.
core" that Whewell was so peculiarly myopic in his failure to recognise that the redescription of facts, that is, the successful colligation of facts by means of alternative conceptions - a contingency he conceded\(^1\) - ought to imply, on Whewell's own principles of the theory-laden nature of observation, that such redescribed "facts" are actually quite different from the "facts" as they were originally conceived, and that, for example, a colour, a form and a sound really are "moulded, combined and interpreted by our mental acts". Or as Feyerabend states it, "...the meaning of observation sentences is determined by the theories with which they are connected. Theories are meaningful independent of observations; observational statements are not meaningful unless they have been connected with theories".\(^2\)

This myopia is one of the fundamental difficulties which face a philosophy of science of Whewell's type, and the modern theories of science which have most in common with Whewell's - Hanson's for example, and, to a lesser extent, Feyerabend's - are not free of the same curious blindness. Hanson in Patterns of Discovery has redrawn the attention of philosophers to the theory-laden nature of observation, and like Whewell has tried to point up some of the consequences of this for scientific discovery. At the same time, in another work\(^3\) he has sought, just as Whewell did, to map out a "middle way" between what he saw as the two extremes in philosophy,

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1. See Ch 3 above, p 73.
rationalism and empiricism, which point, as Plato and Aristotle do in Raphael's painting of "The School of Athens", either "upwards" or "downwards". But in doing this, Hanson affirms, "the scientific encounter...is more than a scratching amongst the data of experience, yet never so much more as to become indistinguishable from artistic or even mystical experience". The difficulty with this however is that Hanson does not really offer any convincing reasons why, accepting the theory-ladenness of observation, he should draw this distinction between scientific and "artistic or even mystical" knowledge. It is just that he refuses to surrender the scientist's appeal to some form of experience, and, while acknowledging the theory-ladenness of observation, simply makes a point of avoiding the consequences which Hegel drew when he remarked, "so much the worse for the facts". Even Feyerabend, who at least professes to be unperturbed by the possibility that "empirical science" may prove to be a chimera once the idea of a factual core is abandoned, is led to insist that the consequence of such a claim is "by no means as absurd as it sounds", and to seek to mitigate it by what can only be described as vague and ambiguous gestures. Thus, after his apparent farewell to a reliable empirical science, he remarks that nevertheless "it is a prejudice to assume that this means the end of all objective knowledge. Empirical knowledge is only one of the many forms of knowledge". And elsewhere he makes the assertion that the conventional basis of many scientific propositions is "a measure

1. Ibid., p83.
3. Ibid., p209.
of the way in which the universe is to be seen", but that "this anthropocentric element is completely harmless (and) has nothing to do with the truth of the theory". Yet it is not clear by what means such claims are to be vindicated should reference to some kind of "unchangeable core of meaning" be abandoned. At any rate, at this point, Feyerabend does not seem to offer any alternative suggestions, which might save his faith in an objective science, "empirical" or otherwise, and which fit neatly with his effective attack on the notion of a factual core. This is not to claim that non-empirical, objective truth is not possible, but only to enquire after the means by which it can be established.

It is just this question which arises out of Whewell's confused position on "facts". How can one hold to a view which represents "facts" as being a product of intellect-ion, while still retaining a faith in the possibility of objective science? Must the notion of an empirical "given" in perception be surrendered, and if so, to what can we appeal in order to guarantee the objectivity of knowledge? Whewell's statement of the apparent paradox which is involved in the "Fundamental Antithesis" reveals, at least, an awareness of these problems, even if it fails to provide a satisfactory solution to them. And it is for this reason that the "Fundamental Antithesis" was described as a particularly perceptive and clear-sighted step on Whewell's part, and one which has a contemporary relevance.

1. Ibid., p215.
Something of the difficulties which are involved in Whewell's philosophy of science have now been examined. Faced with the dilemma of reconciling a basically Kantian epistemology, and inductivist view of scientific discovery, with his belief in the objective truth of the laws and theories of science, Whewell had sought to claim that a "Fundamental Antithesis", between the products of intellection and the external world, is inevitably entailed by all philosophy. This move, however, involved Whewell in the paradox of at once denying and asserting the independent reality of the two elements, and in attempting to avoid this, Whewell, who on conservative religious grounds had rejected Shelling and Hegel's identification of the two elements, was led finally to resort to a Divine Agency as guarantor for the objectivity of Knowledge. Because of Him, Whewell's faith in necessary truth and in induction was saved. The move, however, although it imparts a certain amount of coherence and intelligibility to Whewell's views, does not satisfactorily resolve the paradox at an epistemological level, since it itself is an ontological claim requiring an epistemological justification, one which Whewell fails to provide. Nevertheless, the problems which Whewell's antithesis entails are seen to be significant ones, which continue to demand a solution, if the idea of objective knowledge is not to be altogether abandoned.

The consideration of Whewell's philosophy of science being now completed, the final Chapter will examine the fortunes of that philosophy during the second half of the Nineteenth Century, and will concentrate particularly on the consequences which arose out of Whewell's rejection of Hegel's position.
By the turn of this Century, Whewell's name had dropped into the obscurity of past history. No great Twentieth Century philosopher, or even a minor one, quoted him, or so much as referred to him in passing; not Bradley or Alexander, not Whitehead or Russell, not even Collingwood, who at least had a shared interest in the history and philosophy of science. This absence, until recently, of Whewell's name from the indexes of the bulk of the best works in philosophy published in Britain since his time would seem to be suitable evidence for the claim that his version of a philosophy of science did not survive Mill's criticisms of it in the System. For Mill, it is suggested,\(^1\) had suitably exploded the pretensions of Whewell's theory of induction and necessary truth, and his analysis of the origin of axioms, and thereby had lain Whewell's philosophy to rest for all time.

Now, whether or not Whewell deserved such a fate at Mill's hands, it is quite clear that the reputation of the System amongst a particular group of philosophers during the

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remainder of the Nineteenth Century, accounts to some extent for the disappearance of Whewell's philosophy from the face of the philosophical earth. Yet, this fact can be over-rated, as it tends to bypass a problem which has at least equal significance with the verdict of Mill's followers for a full understanding of the fate of Whewell's philosophical claims. For Mill's reputation amongst certain Nineteenth Century British philosophers, no less than Whewell's amongst others, might be said to have failed to survive him. All that is meant by this, is that the empiricist followers of Humeanism in the Nineteenth Century did not constitute the only philosophical movement in Britain during the period; that there was an important group of idealist philosophers whose opinions, at the expense of Mill's, became the dominant one in the British Universities in the late part of the Century;¹ and that, therefore, it is necessary to consider the opinions which these philosophers held of Whewell's philosophy. That Whewell's name should have disappeared from the writings of the followers of Mill is not surprising. What is surprising, however, and what requires at least as much explanation, is that Whewell's name should not appear in the writings of those late Nineteenth Century idealist philosophers, such as Caird, Bradley and Bosanquet, whose positions might be thought most closely allied to Whewell's own. This is not to ignore the historical significance of Mill's attacks, but is only to argue that in any understanding of Whewell's place in the

¹. In 1893, for example, Bradley could remark, in a less than carefully veiled reference to Mill, that "...things among us are not as they were some few years back. There is no established reputation which now does much harm to philosophy" (Appearance and Reality, Preface, pxii).
history of philosophy, the writings of a class of his contemporaries, besides the followers of Mill, needs to be looked at. And so the question is posed, what was the fate of Whewell's philosophy at the hands of other Nineteenth Century idealists in Britain? The answer to this question will be given by considering Nineteenth Century idealism in Britain in broad terms first, and then by following this with an examination of the relevant aspects of four idealist philosophies, those of Ferrier, Grote, Caird and Green.

1. "IDEALISM"

The development of idealism in Britain has been represented to pass through three stages;¹ the initial stage, involving the recognition by the literary figures of Coleridge and Carlyle of the merits of German thought, and in particular that of Kant and Shelling; the middle stage, beginning in the 1840's, which saw the transition from a literary to a more formally philosophical presentation of issues, and in which the dominance of Hegel's thought becomes increasingly apparent over that of Kant - the period in the main being represented by the figures of Morell, Ferrier, Grote and Stirling;² and the third and culminating stage, in which idealism is seen to flourish and become the dominant form of philosophising in the Universities, this being repres-

² Whewell's place within this group is unrecognised.
ented by the figures in particular of Green, Caird, Bradley and Bosanquet. Although this simplistic breakdown has its faults, tending not leastwise to see Coleridge and Carlyle's contributions as somehow outside the sanctum of genuine philosophical thinking, and, again, tending to overlook the institutional status which idealism enjoyed throughout the whole of the Century, it will be helpful for expository purposes to make use of it. In the discussion which follows, however, comment will be restricted to the second and third stages mentioned, since it is these which are most relevant to the question at hand.

There is some divergence of opinion concerning how the idealist philosophers are to be characterised in general. On the one hand, there is a line of interpretation which emphasises the continuing impact of German thought during the Century in almost every intellectual field, from the fine arts, to history, to natural science, to practical politics, to philosophy. Copleston,¹ for example, who is an adherent of this view, regards as manifest the influence on the British thinkers of developments in German philosophy. Kant's influence, Copleston feels, was dominant in Britain towards the beginning of the Century, but was eclipsed towards the middle and end of the period by an increasing respect for Hegel's thought, and he describes the situation as one where "Kant was seen as looking forward to Hegel or was read... through Hegelian spectacles".² On the other hand, there is a

¹. A History of Philosophy, op. cit.
². Ibid., p174.
line of interpretation, initiated by J.H. Muirhead in the 1930's, which regards the Nineteenth Century idealist movement as a native British reaction to empiricism, materialism, positivism and agnosticism, and comprised of a union of conventional Christian religious beliefs and feelings, with a type of neo-Platonism, a reaction, which Muirhead claims is traceable back through Carlyle, Coleridge, Berkeley and Cudworth, to the Cambridge Platonists in the Seventeenth Century, and even to Duns Scotus. This view sees the idealist movement as one which, though not always homogeneous in its constitution, intentions and direction, nevertheless provided a touchstone for many, especially religious, opponents of the traditional form of British empiricism represented in the figures of Hobbes, Locke, Hume, and their Nineteenth Century followers. Both of these interpretations take some account of the other, allowing its opponent a kind of one-eyed validity. The two schools, however, are perhaps best read not as polar, but as complementary views, and this can be done, without at the same time failing to see the point of each. The story of Nineteenth Century idealism in Britain is certainly not one in which Kant and Hegel, and Plato were read as opposing one another. On the contrary, they were seen as allies to enlist in a common cause. The situation might be represented not too imprecisely, and without denying the genuine originality of the thought of the British

2. Ibid., Preface, pp13-16.
3. Witness Whewell's attempts at a reconciliation of Kant and Plato as shown in Ch 4.
figures, by saying that the idealists in Britain tended to look at their problems, whether theological, political, ethical or epistemological, in the light of interpretations which they placed on the thoughts of Hegel, Plato and Kant. Idealism then can be seen as the flowing together of two streams, one foreign, the other domestic, to form a strong current of opposition to the threatened inroads of positivism, materialism, scepticism and agnosticism. 1 This can be illustrated.

When we turn our attention to the second stage of idealists which includes Ferrier and Grote, and ask how much their thought owes to other philosophers and how much is genuinely innovative, we are faced initially with some difficulty. This is particularly the case with the issue which will concern us most, that is, how much their reaction to Whewell's basically Kantian philosophy of science was the reaction of Hegel to the Kantian dualism. 2 For these idealists, and those too of the third stage, are commonly regarded as "more or less Hegelians," 3 while it is not often clear in what respects they were so. On the one hand every one of the idealists would seem to owe something to Hegel; on the other hand they tended to mitigate their debt by claiming their own originality and independence, by warning against too restrictive an adherence to Hegelian positions, and even by professing genuine inability to understand what Hegel's philosophic posit-

1. This is not necessarily to characterise the thinkers involved as religious conservatives, or to assert that the innovations which they introduced were not in their own way radical.
2. i.e., Hegel - "...Philosophising of this kind wants the little penetration needed to discover, that this shuffling only evidences how unsatisfactory each one of the two terms is". See above, Ch 4, p 88.
ion actually was. Ferrier's philosophy, for example, was in many outward respects Hegelian, and it was generally held, not leastwise by so competent an authority as J.H. Stirling, the Hegel scholar, that Ferrier was greatly influenced by the German. Yet Ferrier himself maintained that his philosophy was "Scottish to the core", and thereby seemed to be denying a fundamental debt to foreign philosophies. Indeed, Ferrier professed that Hegel was something of a mystery to him:

...I have read most of Hegel's works again and again, but I cannot say that I am acquainted with his philosophy.

Or again,

Hegel, - but who has ever yet uttered one intelligible word about Hegel? Not any of his countrymen, - not any foreigner, - seldom even himself. With peaks, here and there, more lucent than the sun, his intervals are filled with a sea of darkness, unnavigable by the aid of any compass, and an atmosphere, or rather vacuum, in which no human intellect can breathe.

Despite these protestations, however, there is a tone of responsiveness in Ferrier's invocation of Hegel's name, and a definite hint of indebtedness in the remark concerning "peaks, here and there, more lucent than the sun". This becomes clearer from a passage in which Ferrier introduces Hegel's name, not so much to deny the influence of his thought, as to lament the unnecessary obscurity into which

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1. See the annotations to his translation of Schwegler's *Handbook Of The History Of Philosophy*, pp357;360.
2. Though not the commonsense variety espoused by Reid, against which Ferrier was at fundamental odds. See the Introduction to his *Institutes of Metaphysics*, p29f; also p428f.
that thought has been cast by Hegel's tortured terminology:

His (Hegel's) faults, and those of his predecessors subsequent to Kant, lie, certainly, not in the matter, but only in the manner of their compositions. Admirable in the substance and spirit and direction of their speculations, they are painfully deficient in the accomplishment of intelligible speech, and inhumanly negligent of all the arts by which alone the processes and results of philosophical truth can be recommended to the attention of mankind.¹

This is a candid admission on Ferrier's part of the influence of Hegel's philosophy on him. Yet, at the same time, this admission does not mitigate at all against Ferrier's claims concerning the originality of his own thought. Indeed, these claims can only be properly understood when Ferrier's use of Hegel was been admitted, for the nature of Ferrier's originality is largely the manner in which he applied what he had derived. This is the important point. As much as the working out of his, essentially Berkeleyan,² problems owed to his own ingenuity, the key to his solutions he took from the Germans, and not so much from Kant as from Hegel. And this is the case with all the late Nineteenth Century idealists,³ for each was to use the German inspiration to deal with peculiarly native problems and issues. This helps put into better perspective Copleston and Muirhead's interpretations of the Nineteenth Century idealist movement in Britain.

It is necessary here to remark on how Hegel was applied by his British followers, before going on to treat the matter in its connection with Whewell's philosophy of

¹. Ibid., p92.
². See below, p124.
³. Remembering that certain modifications might have to be made when this argument is extended to each of them.
science in detail. The form which this growing interest in Hegel assumed was basically a conviction that ultimate reality was in some sense spiritual, and that "things in themselves, being the expressions of the one spiritual reality, which manifests itself in and through the human mind, are essentially intelligible, knowable". 1 Amongst the early idealists this notion was tied intimately to the less general one which viewed "subject" and "object" as correlative rather than opposite concepts; both were regarded as rooted in the one ultimate spiritual principle, generally construed as God. It is due to this latter consideration that the epithet "objective", rather than "subjective", has been applied to the version of idealism which these thinkers represent. As a whole, the movement was the natural home for a conscious attempt to preserve the beliefs of traditional religion from the threatened inroads of scepticism, of whatever form, and, therefore, was often engaged in a criticism of what it viewed as the subjectivist tendencies of certain of the followers of Kant.

Now the followers of Kant in Britain at this period were largely of two sorts, representing what might be seen as the two poles of Kant's philosophy, described by the adjectives "transcendental" on the one hand and "critical" on the other. The "critical" Kantians in Britain were led by Sir William Hamilton, who accepted Kant's apparently telling attacks on the pretensions of dogmatic metaphysics, and who asserted as the basis of his philosophical position an absolute

ignorance of "things in themselves". Hamilton's best known disciple was Henry L. Mansel, the Oxford philosopher and Dean of St Paul's, whose infamous Bampton Lectures, "The Limits of Religious Thought", in 1858 set out to deny the possibility of a rational knowledge of God, and caused a stir amongst theologically orthodox hearts, provoking innumerable replies and defences of rational religion, not the least from one W. Whewell.¹

The second group of Kant's followers in Britain tended to ignore the "critical", and to follow the "transcendental"/metaphysical aspect of Kant's thought. This group is best represented by the figure of Coleridge, and Whewell himself. Ironically, this aspect of Whewell allied him—as much as he professed that his claims differed fundamentally from theirs—with Shelling and Hegel, who likewise developed the non-critical aspects of Kantianism, though of course claiming to have incorporated the spirit of Kant's critical philosophy into their own positions, thus to avoid the lapse into old-time metaphysics.

The idealists, Ferrier and Grote, Caird and Green tended to react to these two professedly Kantian positions in a determinate manner. What this essentially amounted to was the adoption of the stance of Hegel, both against Hamilton's form of Kantianism, and, more to the point, against Whewell's.

¹. Mansel's scepticism as to "things in themselves" explains his reaction to Whewell's philosophy of science, which he described as a "stumble on the threshold of the Critical Philosophy"; see Prolegomena Logica, Appendix, Note A, p.311). Whewell's reply in PD was along the lines of his general criticism of Kant's denial of the intelligibility by "things in themselves" considered above, Ch 4, p.96.
As was suggested,¹ Ferrier's general philosophical position may be regarded, without denying the inspiration of Hegel, as "a development of Berkeley in the light of later discussions".² In Britain he was largely responsible for resurrecting Berkeley's name from the confusion and misunderstanding into which it had fallen over the previous Century.³ This can be seen from the general argument of his major work, the Institutes of Metaphysics. In that book Ferrier wanted to maintain two propositions; (i) that we cannot think of the universe as "dissociated from every us You cannot perform the abstraction"⁴ and (ii) each of us can dissociate the universe from himself in particular. From these two propositions, Ferrier claimed, it follows that, though "each of us can unyoke the universe (so to speak) from himself...he can do this only by yoking it on, in thought, to some other self".⁵ This was an essential move for Ferrier to make, because, in a manner after Berkeley, he wished to argue that the universe is unthinkable except as existing in synthesis with the divine mind.⁶

At least as good a starting point as the Institutes for an understanding of Ferrier's thinking, however,
is a long essay which appeared, not coincidently, in 1841, just one year after the publication of Whewell's Philosophy. That essay bore the significant title, "The Crisis Of Modern Speculation". In it Ferrier clearly showed an enthusiastic appreciation of post-Kantian German philosophy, and though neither Shelling nor Hegel's names appear, the spirit of their thoughts pervades every word and is unmistakable. The essay was straightforwardly Ferrier's attempt to come to term with the exact same problem which Whewell occupied himself with in the fundamental antithesis:

The great endeavour of philosophy, in all ages, has been to explain the nature of the connection which subsists between the mind of man and the external universe: but it is to speculation of a very late date that we owe the only approach that has been made to a satisfactory solution of this problem. In the following remarks on the state of modern speculation, we shall attempt to unfold this explanation, for it forms, we think, the very pith of the highest philosophy of recent times.  

Ferrier took the traditional problem of perception as the locus of his discussion, and argued that the familiar explanations of it all fail to provide an adequate account, for the good Hegelian reason that they are all rife with contradiction; they all have in common, he said, "one fatal and fundamental oversight", and are, consequently, "involved in inextricable perplexities in their efforts to unravel the mysteries of perception". This fault, Ferrier

1. Ferrier nowhere mentions Whewell by name, and this fact makes it more difficult to establish a connection between the thoughts of the two men. What seems most profitable, therefore, is to consider some of Ferrier's reflections on philosophy which most closely touch problems with which Whewell was dealing, and thereby to show the climate of thought which Ferrier's influential speculations brought about, and in which Whewell's philosophical opinions were judged.
3. Ibid., p268.
claimed, lay in the question they had asked themselves, which traditionally was the question, what is the relation between the external world and man's perception of it? This question was an impossible one to ask, he said, and what was required for a solution of the problem was the asking of another, the only legitimate question. The clue to what that question would be, he suggested, lay in the modern German philosophy. This is the train of Ferrier's argument, and some of its main points must be examined.

It was impossible to ask the traditional question of perception, Ferrier claimed, for the reason that all the so-called "representative theories of perception" are founded on a distinction which had been laid down between "objects as they are in themselves and objects as they are in our perception of them," and there is a fatal and fundamental oversight involved here. For in laying down this distinction, previous thinkers had not taken into consideration whether the distinction had any legitimate basis. Now it was at this point, we might say, that Ferrier came most closely to an actual criticism of Whewell's position concerning the fundamental antithesis. He first denied the distinction between "subject" and "object", and then paradoxically argued that the distinction "is not to be rejected and set aside altogether", since "unless we made some sort of discrimination between our perceptions and outward objects, no consciousness or knowledge would be possible". This too is what Whewell believed. But for Ferrier

1. Ibid., pp263-264.
2. Ibid., p269.
3. Ibid., pp269-270.
4. See above, p 84.
there was a rejoinder, and it is this which both links him
with, and divides him from Whewell. This was, that the
distinction, however real, must be allowed "only a relative
validity":

It gives us but one-half of the truth. We deny that
it is an absolute, final, and permanent distinction;
and we shall show that, if by one law of intelligence
we constantly separate the subject and the object, so1
by another law we as constantly blend them into one.

This puts Ferrier close to Whewell because
it is a striking parallel to the paradox which Whewell set
out in his discussion of the antithesis. 2 It separates
him from Whewell because the manner in which Ferrier claimed
the union is effected is fundamentally at variance with what
Whewell was prepared to allow. For Ferrier's general conclu-
sion was that both "subject" and "object" are seen to be the
two aspects of the one thing, and have an essential identity. 3
This is a claim which Whewell adopted too. 4 Yet he, unlike
Ferrier, was not able to see, or at least was not prepared
fully to accept, the consequences of it. For he tried to tie,
as Ferrier did not, a quasi-identified "subject" and "object"
to a form of realism which sought to preserve a distinction
between ourselves and the external world. To Ferrier this
kind of thing was completely untenable. The question concern-
ing the linkage between ourselves and the external world cannot
even be asked on his view, and must become, "by the laws of the

1. Ibid., p270.
2. See above, Ch 4, pp80-87.
4. See FA2, p617; and above, Ch 4, p 90.
very thought which thinks it", another question. It can't be asked, he said, for the reason that unless "subject" and "object" are able clearly and consistently to be held separate in thought - something which he felt was not possible - we can't sensibly frame a question concerning their union. We can't possibly be in a position to inquire into the nature of the connection between mind and matter, just because "it is not possible for thought to construe, intelligibly to itself, the ideal disconnection which must necessarily be presupposed as preceding such an enquiry". And Ferrier continued, significantly:

He who can maintain his equilibrium between these two opposite views without falling over either into the one (which conduces to idealism) or into the other (which conduces to materialism), possesses the gift of genuine speculative insight.

Whewell then, on Ferrier's analysis of the matter, would have been doubly damned, for not only was he unable to maintain his equilibrium, he fell over into both pits, in his vain attempts to unite them. Had he been addressing his remarks to Whewell specifically, this then might have

1. Ferrier's formulation of this second question is rather confusing; it is stated thus: "What is the nature of the connection which subsists between the mind of man plus the external universe on the one hand, AND the mind of man plus the external universe on the other. Or differently expressed, What is the connection between matter-and-mind (in one), and mind-and-matter (in one)? Or differently still, What is the connection between the subjective subject-object and the objective subject-object?" (Works, op. cit., Vol III, p281.) The difficulties of this are perhaps not quite so abstruse as Ferrier's circumlocutions would seem to indicate. All in fact that he wished to maintain is that thought and reality are not two separate entities, but are simply the one situation viewed from two different aspects.

3. Ibid., pp285-286.
been Ferrier's conclusion: in one fundamental respect Whewell was right, and that was in his denial of the independent reality of the terms which his antithesis involved. But otherwise, Whewell was mistaken and had failed to grasp fully the necessary consequences which this first claim involves. As a result therefore, his analysis of perception has lapsed into the old dualism entailed by all "representative" theories, since it poses for the antithesis what inevitably is an unintelligible question, one which cannot be asked, let alone answered. And this, Ferrier might have said, is the reason for the confusion in Whewell's philosophy, the confusion in which any dualist philosophy is necessarily involved, as Hegel has shown.

These claims were implicit in Ferrier. However, they were made explicit in the writings of a fellow traveller, John Grote, and made so in a context in which Whewell's problem of the fundamental antithesis was specifically discussed. Grote's philosophical views are in a large measure similar to Ferrier's, as Grote himself acknowledged.¹ Both were keen to preserve rational religious belief against sceptical attacks; both maintained the reality of "things in themselves" by claiming that the human mind and the world are somehow in "adaptation";² and both took epistemology as their starting points, specifically, that is, the problem of perception. And, most importantly, the source of Grote's "Hegelian" criticisms of Whewell is Ferrier.

². Ibid., p58f.
By and large Grote was more inclined to be sympathetic towards a view of knowledge like Whewell's, than one such as Mill's: "I think it is his view on the whole which is the right and the fruitful one". He tended to describe Whewell's position as "dynamic", while Mill's by implication was to be looked upon in some sense as static. He felt as well that Whewell, unlike Mill, had faced up to the inevitability of metaphysics. Yet these sympathies were tempered by a few very real criticisms, criticisms which were not so much detractions as proposed amendments, the pointing up of contradictions in Whewell's position in order to speed their working-out, so that the truth in his system might be preserved and implemented.

Grote parallels Ferrier in being willing largely to accept Whewell's denial of the distinction between the two elements of the fundamental antithesis. But, just as Ferrier might have, he thought Whewell mistaken in the reticence he showed in embracing this claim, and especially in his attempts to join with it the claim that the two elements have some kind of independent reality. On this point in particular, he could not accept Whewell's remark that knowledge requires the

1. Ibid., p225.
2. Ibid., p204-205.
4. Grote found something to say by way of criticism on almost every aspect of Whewell's philosophy. Thus, he questioned the efficacy of some of Whewell's claims concerning necessary truth and the criteria of truth, especially the "inconceivability" of its opposite and the notion of "clarity" (Ex ploratio, Ibid., p206, pp217-219). He upheld Mill's criticisms of Whewell's claims that induction can result in necessary truth (p233). He also questioned Whewell's objections to Descartes' philosophy, from which, Grote suggested, Whewell's own is not so greatly removed (p225n). On the other side, Grote was happy to accept Whewell's version of the advancement of science, the notion of colligation, the importance of hypotheses (p204), and the general idea of the mind "informing" sensation (p57).
union of the two, while philosophy requires their separation. For one of Grote's fundamental claims was that there are not two "elements" in knowledge at all, but only "two aspects of knowledge (looked at) from different sides". Whewell's language then, in maintaining the dualism, is inevitably involved in insuperable difficulties from Grote's point of view. It involves the "wrong psychology" which in the long run leads to "relativism". This is an echo of the Hegelian point, and Grote's discussion involves variations upon it. Let us see this.

Grote felt that much of Whewell's language could be read to suggest that the antithesis involves only a "contrast" of terms rather than a strict opposition of them. This is especially the case with Whewell's discussion of the relation of theory and fact, which to Grote's mind is the best statement of what is true in Whewell's thinking. He agreed fully with Whewell's claim that the facts under one aspect are theories under another, and that well-attested theories are constantly being transferred to the side of fact, "fact" here apparently being synonymous with truth about nature. These were claims close to his own heart. But he could not help feeling at the same time that they were claims only properly understood by regarding fact and theory as "aspects" of the one unity (knowledge) and not as separate portions of it, and that Whewell, although he had glimpses of this, had really not seen it clearly enough, and consequently had stumbled on the true

1. Ibid., p226.
2. Ibid., p229.
3. Ibid., pp227-228.
path. For Grote was prepared to take Whewell at his word concerning the inseparability of theory and fact, and to argue that if they really were inseparable as Whewell said, this must also be the case with all the terms which Whewell used to illustrate the antithesis. ¹ "My view", said Grote, "...is precisely expressed by the antithesis as it exists between Theories and Facts".²

As far as Grote was concerned, Whewell was guilty of a contradiction when he claimed that it is possible to know of the independent reality of an external world, while yet maintaining that this world is known only through the activity of the mind. Grote puzzled, for example, over those passages in Whewell where it was claimed that "there is a mask of theory over the whole face of nature",³ and asked himself what exactly it was that Whewell was expressing here. Was it that the mask is something distinct from, say, the face of nature? But surely not, for this would lead us to say that nature herself is some sort of unknowable substratum, and to conclude that the knowledge we possess (i.e., the mask) is merely "relative" or subjective, since, for all we can say, one mask is as good as another depending on one's standpoint.⁴

Now Grote was quite aware that this sort of consequence was far from Whewell's intention. What therefore did Whewell mean? Perhaps he meant that nature is the mask itself, which was Grote's own position? But this claim, which Grote believed was closest to the spirit of Whewell's thinking, would seem to be denied by

¹. Ibid., p228.
². Ibid., p230.
³. See above, Ch 4, p 84.
Whewell, who made much of "unmasquerading Nature" and of "penetrating Nature's disguise". Such language, Grote said, lands him back in the position of asserting the existence of some sort of unknowable substratum. Just what Whewell's actual position was, therefore, Grote hesitated to say. Clearly what he felt however was that Whewell was oscillating between a "subjective" idealist position, in which mask and nature are kept separate, and a genuine "objective" idealism, in which mask and nature are identified, neither of which views Whewell seemed willing to accept. This kind of oscillation and confusion, Grote shortly after remarked, was unavoidable so long as Whewell insisted on employing a dualist type terminology which opposes nature to what is known, things to thought about them, facts to theories. And it could be avoided only by some radical identification of the terms. 1

The antithesis is wrong in one way: that it divides a part of knowledge, as belonging to the mind, from a part as not belonging to it: whereas all knowledge is right thought, if we look at it so, as on the other hand all knowledge is fact exhibiting itself to us or at which we are present, if we look at it so. 2

Grote was sure that the dualist aspect of Whewell's position could be operated on so as to leave the body of Whewell's thought intact. He had no doubts that the antithesis was a fruitful and eminently instructive way of looking at knowledge. The "wrong psychology" associated with it was not essential, he said, "and has got...its antidote with it". 3

1. Ibid., p229.
2. Ibid., p233.
3. Ibid., see for example p240.
Ferrier died in 1864, Whewell in March 1866 and Grote in August of the same year. With their deaths the second phase of Nineteenth Century British idealism may be said to have closed. At the same time the third phase was commencing. The influential two volumes of Stirling's Secret of Hegel appeared in 1865, while the Oxford idealists, Caird, Green and Bradley were beginning to make their ways in the world, Green being appointed to his tutorship at Balliol in 1866, Caird to the Professorship of Moral Philosophy in the University of Glasgow in the same year and in 1870, Bradley, the doyen of the movement to the fellowship of Merton College which enabled him to devote a lifetime to philosophy.

Although Ferrier and Grote held the view, it was in the writings of Caird and Green that the idea of a unity underlying the distinction between "subject" and "object" became dominant. And it is only with their work that a British "school" of idealism may be said to have been founded.¹ Caird, for example, took to heart the Hegelian antipathy towards an "'either...or'" manner of philosophising, and, a genuine dialectician, always looked around for a third way. He set himself the task of destroying dualisms on all sides - particularly those which it seemed to him involved an opposition to religion - and out of the wreckage to construct a unified system of thought and belief, one in which so-called "materialism" and "idealism", science and

¹. For the influence of Green's school see especially Collingwood's Autobiography, Ch 3.
religion, would be ultimately reconciled. He looked to Kant as the inspiration for this program, and to Hegel for its fulfilment. Kant, Caird said, though the mainspring from which all genuine idealism flowed, was also the source of some of its difficulties. It was he who had introduced the untenable and unsatisfactory dualism of knowable phenomena and unknowable "things in themselves", which, Kant's successors had shown, must be expurgated from philosophy if it were to be rendered true and consistent. When this is done, Caird claimed, we can see that the real significance of Kant's thinking lies in his insight into the fact that objectivity exists only for a self-conscious subject. From this we are led (by Hegel) to regard "subject" and "object" as together forming, in Caird's terms, a "unity-in-difference", so that all unsatisfactory dualisms are thereby overcome. 1

Caird claimed that it was one of the tasks of the philosopher to show how science pointed to this basic principle of the synthesis of "subject" and "object" in a "unity-in-difference". However, Caird's own concerns with the problem lay chiefly within the religious sphere, so that he did not take it up in any connection with natural science, and consequently with Whewell's thought. 2 What shall be done then is to consider Thomas Hill Green's version of the same stand, since Green did relate it directly to the methodology of science, and to the dispute in which, beginning thirty years before, Mill had engaged Whewell.

2. The philosophical basis for Caird's theology is not in general much different from Whewell's, cf., Caird's The Evolution of Religion, Vol I, pp65-67, and Whewell's views as given in PD, Ch 30.
As far as Green was concerned, Caird's interpretation of the position of Kant in the history of philosophy happened "exactly to correspond" to his own. Both Caird and he objected, he said, not to Kant's "idealism", ...but to that incomplete development of his idealism which is shown by his partial retention, after all that he had shown of the action of thought in the constitution of experience, of that antithesis between the world of experience and the world of ideas... To this incompleteness is to be ascribed, for instance, what is most readily and reasonably objected to in the 'Aesthetic' - its separation of pure from empirical intuition.¹

Green believed that this same antithesis governed modern British philosophical thinking. To it he attributed the failure of British philosophy to advance beyond the point reached by Hume, to a position which took cognisance of the achievements both of Kant and of Hegel. In failing to do so, he said, Philosophy was not only "anachronistic", but inevitably resulted in scepticism. For while ever thought and reality were opposed and while ever abstraction were the function of thought, we must be led to conclude that the most abstract thought or "highest idea", will have the least reality; "in short, the more we think, the less we shall know".² The only way Green could suggest to avoid this was to recognise that "thought is things and things are thought". This he construed to mean that the world is a unity, one inter-related whole, a system of relations themselves related throughout to a single subject. That the world is so related, he said, must be the case "because on no other supposition is its unity explicable."

². Ibid., Vol III, pp144-145.
Accordingly, when Green's comments on Whewell's philosophy are examined, it ought not to be a surprise to see that he was generally sympathetic to most of Whewell's claims against Mill. At the same time, however, it is to be expected that he would be critical of Whewell for failing to break away from his allegiance to the dreaded antithesis. In fact a good deal of Green's own polemic against Mill can be seen to have actually taken the form of a restatement of several of Whewell's arguments, amended to the point where Green felt the gaps left vulnerable by the antithesis would be properly fortified.

The chief areas of conflict between Whewell and Mill had been over the nature of mathematical truth, the origin of axioms, and induction, and it was at these points that Green joined hands with Whewell. As regards necessary truth and the origin of axioms, Green said, Mill and his followers were quite mistaken, and in fact had misapprehended the question involved. Green's reasoning was as follows: A "clear-headed Kantist" - and apart from himself, Whewell was the only writer cited in this regard - could agree with Mill, he said, in claiming that axioms are derived from experience, because "... his (the Kantist's) doctrine is that geometry has to do just with that condition of experience which renders it an outward experience". 1 The real question is thus not as Mill supposed, whether geometry does or does not have anything to do with outward experience, but rather, a) from what it is in experience that geometrical axioms are derived, and b) how they

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1. See above, Ch 3, p59.
are derived from it. Mill's answers to these two questions are, respectively, "sensation" and "generalisation", and it is only here, Green said, that Mill is in genuine conflict with the "Kantist". For when we judge these answers we see that they are mistaken. Thus, for example, Green continued, with any of the axioms of geometry involving the notion of a straightline Mill says we must hold that seeing innumerable lines nearly straight, we suppose them to be quite straight, and then generalise from this supposition. But this is wrong for in these cases the generalisation can't be said to result from the evidence of the senses, since we never actually see a perfectly straight line. In fact, "geometrical forms are not, as Mill says, 'copies of impressions', but alterations of them". But how is such an alteration to be accounted for, Green enquires? How can I see a line to be nearly straight, if I have no prior idea of straightness? Thus Mill's account will not work.¹

When these remarks are compared with those made by Whewell, they are found to be almost identical. Whewell had said, "...it does not appear how we can compare our ideas with the realities, since we know the realities only by our ideas".² Now Green quoted this remark, by way of Mill's reply,³ and suggested that Mill was right to equate Whewell's statement here with a "representative" theory of

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2. PD, p289; cf., Whewell's reply to Herschel in RTAR, p674.
perception (Mill: "the 'doctrine of perception by means of ideas', which Reid gave himself so much trouble to refute"), since this is essentially what Whewell's language entailed. For Whewell would have only been right, Green said, if he had claimed that the idea and the reality were one and the same. For otherwise he is involved in all the old problems of a "representative" theory. Green went on to point out that Mill himself was by no means free of adherence to representationalism, and continued:

"...It is implied in all language which speaks of us as knowing things through sensation, and describes sensations as impressions made by external matter. It never can be entirely got rid of save by the recognition of 'matter' as a congeries of relations constituted by thought. When Whewell talks of a 'reality known only by our ideas', he is obscurely thinking of matter in this way. The worst of him is that he is still hampered by the notion that there is one sort of reality known by our 'ideas', another by our 'eyes' ".

Thus Whewell's account of the origin of axioms and the nature of geometrical truth is largely correct, but only if the allegiance to the antithesis of thought and reality is done away with, and replaced by a recognition that "thought is things and things are thought". This same sentiment is repeated in connection with induction.

Green was charitable to Whewell's views of induction, acceding practically all that Whewell claimed for them. Thus he felt that Whewell was right in claiming that Kepler's and Newton's discoveries were both examples of induction. And on the whole he felt that Whewell's notion of induction as a "colligation" was essentially correct, as opposed to

Mill's of a resemblance amongst groups of sensations. Indeed, as far as Green was concerned, Mill's several attempts at a definition of induction, as well as his famed axiom of the uniformity of nature, were outright misconceptions.⁷ Nevertheless, Green has some criticisms of Whewell. He appears to have felt that overall, and even in detail, Whewell was correct, but that he had often spoilt his case against Mill by "writing as if the antithesis between ideas and facts were a valid one; as if the 'superinduction' of ideas upon facts were merely an operation that had to be performed ex parte nostra in order to give science", whereas the true doctrine is that "it is only the 'colligating' action of thought which constitutes those relations in which the 'facts' consist".⁸

Green was here attempting to render "colligation" more tight and consistent than Whewell had done, and essentially to point out that colligation was not so much an action imposed upon external facts in order to bind them together, for this involves an inconsistency, but was an action which actually suggests or reveals those facts in the first place. By working this amendment, Green shows himself prepared to reject Whewell's preferred means for ensuring the reality of knowledge - the appeal to God as a kind of "third man" - and to replace it with what for him was the only viable and consistent means for doing so. For as far as Green himself was concerned, colligation in his new sense involved no surrender of objectivity or reality:

1. Ibid., Vol II, p281.
2. Ibid., Vol II, p285.
"The wider the colligation, the greater the range of facts unified by a conceived relation, the less becomes the possibility of the relation as conceived by us being other than the relation as it really is, or as it is according to the true conception".¹

Although Whewell had wanted to maintain something similar to this, Green's remarks suggest that the dualism involved in Whewell's notion of colligation would never give him what he sought. For while every fact and theory were held separate, Whewell was always open to the objection, which he himself had suggested, and which Mill took up, that several different conceptions may serve equally well to colligate the same set of observed facts, and, consequently, that the true explanation of these facts can never be ensured. And Green, in claiming to see the way out of this difficulty, was arguing that because colligation is not a colligation imposed on independently existing "facts" or "phenomena", but actually "constitutes those relations in which the 'facts' consist", the epithet "true" of any colligating theory is ensured. And only in this sense will the real test of rival colligations be, as Whewell would have it, the breadth of the one over the other. For all genuine colligations, in being such, are in some sense "true". Green's argument was merely the application of Hegel's dictum: "what is reasonable is actual; what is actual is reasonable".²

This is why Green believed Whewell's claim that Kepler's and Newton's discoveries are both examples of inductions was essentially correct. For he felt that Kepler and Newton did colligate facts - in his sense of "colligation" -

1. Ibid., Vol II, p294.
2. Ibid., Vol II, p295.
the only real difference between them being that Newton had suggested a wider colligation, one which happened to take in Kepler's original one. ¹ No doubt, Green said, there is a distinction between what Mill Called a "description" of planetary motions and an "explication" of them — a distinction which Green felt Whewell had correctly pointed out and designated as, that between the laws of phenomena and the laws of their causes. But the real question for Green was whether Mill had given the right account of it. But he had not, for "the point is, that in the same sense in which Kepler 'saw' the ellipse in the facts, Newton 'saw' the law of gravitation in the facts". ² Echoing Whewell, Green claimed that the conception of the relation between the positions of Mars as points in an ellipse could not have been abstracted from Kepler's observations of them because, until they had been determined by this conception, they did not have the common characters from which it could be abstracted. All Mill's distinctions then, between observation and inference, between abstraction or description and generalisation, break down:

"...There is inference in the simplest observation, if everything is inference which goes beyond sense, for already in such observation there is a determination of phenomena in the strict sense (as = appearances) by conditions consisting in understood relations, and there is no more inference than this in any induction". ³

Green's version of the development of science was not then so very different from Whewell's. Both men saw it as the progressive colligation of facts by means of the

¹. Ibid., Vol II, p294.
². Ibid., Vol II, p291.
³. Ibid., Vol II, p294.
superinduction of conceptions. Green's proviso, however, was that Whewell must be rendered more precise and consistent, and that essentially every fact itself must be recognised to be "constituted by such a superinduction", so that the conceptions involved in colligation must not be said to exist in our minds before or apart from their existence in fact. Whewell, Green felt, merely spoilt his otherwise just case by writing as if the antithesis between ideas and facts were a real one.

It is clear from this discussion that, while Mill and his Nineteenth Century followers in Britain had discounted and dismissed Whewell's version of a philosophy of science, there was a sizeable group of important and influential thinkers in the second half of the Century, who were prepared to treat Whewell's views sympathetically, and indeed who often compared them favourably with Mill's own. These men, Ferrier and Grote in the middle of the Century, and Caird and Green during its latter half, did find fault with certain aspects of Whewell's philosophy, and notably with the dualism involved in his fundamental antithesis which stemmed from Whewell's adoption of portion of Kant's position. Their criticisms of this have a basic similarity, all being framed, in the attempt to develop Hegel's notion of an idealism that is genuinely "objective", along the lines of a conscious adoption of Hegel's criticisms of the dualism involved in Kant's "subjective" idealism. Nevertheless, when these later idealists point up Whewell's faults, they did so not so much
out of a desire to expose Whewell's position as unworkable, as Mill had sought to do, but rather so as to work towards the amendment of a position they were convinced was basically sound and tenable. This goes a long way towards explaining why Whewell's name does not often appear in the writings of thinkers who might be seen as most closely allied to him. For Whewell's claims, it is clear, were not ignored by these men, but rather were absorbed, in an amended form, into the general body of their own basically similar positions.
Whewell's attempted reconciliation of his positions on necessary truth and induction can be said to have failed, at least in that full sense which he himself desired of it. It has been seen in Chapter II that Whewell, beginning with a basically Kantian epistemology, sought to establish the categorical necessity of the Fundamental Ideas entailed by the sciences, and, consequently, the truth of the axioms and propositions flowing from them. In Chapter III it was shown that this was tied an inductivist account of scientific discovery which emphasised the creative use of the imagination, as embodied in the notion of the colligation of facts by means of clear and appropriate conceptions. The "subjectivist" tendencies of this alliance were pointed up in Chapter IV, as was Whewell's attempt, by means of his Fundamental Antithesis of Philosophy, to avoid them. In the same Chapter the paradoxical position into which this attempt led Whewell, particularly concerning the relative signification of the terms "theory" and "fact", and the untenability of Whewell's "ultimate solution" to the general problem, were traced to the interpretation which he gave in his Fundamental Antithesis to his Kantian starting-point. Finally, in Chapter V, it was shown that the fate of Whewell's philosophy of science in the later Nineteenth Century was conditioned by the weaknesses inherent in the Fundamental Antithesis.
Whewell's position in the history of Nineteenth Century British philosophy has been seen to be a unique and important one. Far from being merely an opponent of Mill on a few isolated matters, it is obvious that Whewell possessed a fully worked out, though not always consistent, philosophical position, one which was important for two reasons. It was the first example in Britain of a thorough application of Kantian principles to the specific matter of a philosophy of science, and it was framed by a thinker who had a first-hand acquaintance with the present and past state of scientific inquiry. To both of these distinctive features must be attributed the success, as well as the failure, the virtues as well as the faults, of Whewell's philosophical position.
Note A

TABLE OF PERCENTAGE DISTRIBUTION OF WHEWELL'S PUBLISHED WORKS, ARRANGED BY DECADE.

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These figures are rough estimates merely, since the basis of calculation has been the number of items falling into each category, rather than the relative bulk or importance of each item. Thus, in one sense the figures are biased, in that a minor work, such as a single short paper on a scientific topic, weighs as heavily in the Table as a large and important work, such as the two volume Philosophy. The bias actually works against the point being made in Chapter One, page 8 of the text above, and if that bias were to be eliminated by calculating the breakdown of Whewell's works on an alternative basis, i.e., one which would take account of the significance rather than the quantity of items, Whewell's drift away from science into philosophy and ethics in his later life would be even more apparent.

Note B

Whewell's notion of the "explication of conceptions" has been treated partly in Chapter Two, in connection with the clarity and distinctness of conceptions. Apart from this aspect, however, Whewell held that the explication would also reveal concepts to be "appropriate". The point of this last claim is worth one short comment.

That conceptions must be appropriate Whewell had no doubt. He was no reductionist. "Every attempt", he once remarked, "to build up a new science by the application of principles which belong to an old one, will lead to frivolous and barren speculations" (PIS1, II, pp184-185). It was because
scientists had often attempted to apply certain Ideas to sciences which could not accommodate them — he particularly had in mind here the attempts to make use of physical and chemical notions in physiology\(^1\) — that advances in these sciences were slow.

Now, the natural question for a critic to ask of Whewell is how do we know which concepts are appropriate to a science and which are not? Whewell anticipated this and was able to answer by arguing, from a general Kantian standpoint, that particular conceptions are appropriate, since they are "certain peculiar modes of apprehension" and are therefore "strictly determined by the subject of our speculations" (PIS1, II, p186). He did not embark on any general attempt to urge this, but was content to allow that "to a certain extent we can only know that conceptions are appropriate when we discover in practice that they do successfully combine the facts" (PIS1, II, p186). Whewell, of course, expected that the "explication" would produce something more than expressions which were recognised to be sufficiently clear and appropriate merely to be useful in scientific systems. For him scientific argument resolved confusion into truth and error (PIS1, II, p173), and the ultimate guarantee for the appropriateness of certain conceptions rested with the categorical nature of the Fundamental Ideas themselves.

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1. See his criticism of Mill's use of mechanistic examples from Liebig's work in physiology, OI, pp46-48.
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In his paper, "On Hegel's Criticism of Newton's Principia", delivered to the Cambridge Philosophical Society in 1849, Whewell argued that Hegel's championing of Kepler in the Encyclopaedia was a gross mistake and a complete misrepresentation of the merits of Newton. He was convinced that Hegel knew little or nothing about physics, and not much more about the other sciences, and that his speculations on the subject were worthless. He read Hegel as attempting to oppose to Newton's theory his own rather obscure and confused planetary theories, and attacked him accordingly (HCNP, pp.696-697). It was to this attack by Whewell that James Hutchison Stirling, that arch-vindictor of Hegel, traced the antipathy of scientists to Hegel's thought, and their unwillingness to take cognisance of its worth. In a small volume, Lectures on the Philosophy of Law, Together with Whewell and Hegel, and Hegel and W.R. Smith, which was published in 1873, Stirling subjected Whewell's attack to a thorough analysis and concluded that Whewell had simply misread the German metaphysician, whose purpose, Stirling said, was not so much to offer alternative scientific explanations to those of Newton, of whom Hegel had a good understanding and whose merits he certainly respected, but rather to insinuate his own metaphysical claims between the tissue of an intact and breathing Newtonian science.

The question of the scientists' continuing antipathy to Hegel's philosophy is an interesting one, and is still alive today, as evidenced by Popper's The Open Society and its Enemies.
BOOKLIST

I. SELECTED WORKS OF WHEWELL.

* Astronomy and General Physics, Bridgewater Treatise, London; William Pickering, 1834.

* "Comte and Positivism", Macmillans Mag, (March 1866), 333-362.

* Criticism of Aristotle's Account of Induction", Trans Cam Phil Soc, 9 (1850), 63-72.

* The Elements of Morality, 2 Vols, 4th ed., Cambridge; Deighton, Bell and Co., 1864.


* "Grote's Plato", Frasers Mag, (April 1866), 412-423.


* The History of Scientific Ideas, 2 Vols, Being the First Part of the 3rd ed. of The Philosophy of the Inductive Sciences, London; Parker and Son, 1858.

* Indications of the Creator, 2nd ed., London; Parker, 1846.


* Of Induction, With Especial Reference to Mr J. Stuart Mill's System of Logic, London; John W. Parker, 1849.


* "Of the Platonic Theory of Ideas", Trans Cam Phil Soc, 10 (1857), 94-104.


* "Second Memoir on the Fundamental Antithesis of Philosophy", Trans Cam Phil Soc, 8 (1848), 614-620.

* "On Hegel's Criticism of Newton's Principia", Trans Cam Phil Soc, 8 (1849), 191-706.


* On the Philosophy of Discovery, Being the Third Part of the 3rd ed. of The Philosophy of the Inductive Sciences, London; John W. Parker, 1860.


* "On the Transformation of Hypotheses in the History of Science", Trans Cam Phil Soc, 9 (1851), 139-147.


* The Philosophy of the Inductive Sciences, 2 Vols, 1st ed., London; John W. Parker, 1840.


II NINETEENTH CENTURY WORKS PERTAINING TO WHEWELL'S LIFE AND PHILOSOPHY OF SCIENCE.


* Caird, E.: *The Philosophy of Kant*, Glasgow; James Maclehose, 1877.


* Clark, W.G.: "William Whewell, in Memoriam", *Macmillans Mag*, No 78 (April 1866), 545-552.


* Ferrier, J.F.: Philosophical Works, 3 Vols, Edinburgh; Blackwood, 1883.


* Herschel, J.F.W." "Review of Whewell's History and Philosophy of the Inductive Sciences", Quarterly Rev, 68 (1841), 177-238.


* Smith, N.C. (ed.): Letters of Sydney Smith, 2 Vols, Oxford; Clarenden Press, 1853.


III SELECTED GENERAL WORKS TREATING WHEWELL'S PHILOSOPHY OF SCIENCE.


IV MISCELLANEOUS


