AIM-ORIENTED EMPIRICISM AND THE 'FATHER' OF THE SCIENTIFIC REVOLUTION: METAPHYSICS AND METHOD IN THE WORK OF GALILEO

A Thesis

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ABSTRACT

This thesis is concerned with that branch of the history of science which takes as its central problem the question of scientific progress, defined as the growth of knowledge and understanding about the world. It is an area of enquiry which has been suppressed, in recent years, by the development of historical methodologies which eschew all epistemological deliberations and their established ramifications.

This thesis, therefore, addresses itself to the following areas. In Chapter One consideration is given to the degree to which the present ascendancy of contextual, social history of science depends upon formulating methodological strategies that deny the very legitimacy of a progress history of scientific ideas. These strategies are shown to depend upon the old definition of internalist, intellectual history of science, which drew upon related areas in the philosophy of science. Some basic arguments in favour of the possibility of progressive histories of scientific ideas, which have been ignored by the discipline as a whole, are rehearsed. Chapter Two is devoted to an account of how a present-day philosophy of science, aim-oriented empiricism, offers a solution to the problem of induction which, by demonstrating that scientific rationality has a historical dimension, provides a suitable historiographic framework for a progress-oriented history of scientific ideas. Chapter Three examines the work of Galileo in the light of this new historiographic framework. Firstly, it is demonstrated to be an option for exegesis, an account of how ideally rational science ought to be which does not rationally reconstruct the past. Secondly, it illuminates Galileo's work in significantly new ways, demonstrating that by making *explicit* the metaphysical dimension already implicit in Galileo's methodology, his work can be shown to have an underlying unity - and be part of a progressive tradition - in ways which other interpretations, distracted by the seeming disunity at the methodological level, fail to appreciate. Finally, Chapter Four considers the possibility of a beneficial, reciprocal relationship between developments in the philosophy of science and in progressive histories of scientific ideas.

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CHAPTER ONE

CRITIQUE OF CERTAIN TRENDS IN THE HISTORY OF SCIENCE

Social History of Science and the Concept of Scientific Progress

This thesis is concerned with that branch of the history of science which takes the central problem to be scientific progress, defined as the growth of knowledge and understanding about the world. The question which started me thinking about these issues, paradoxically, involves the scarcity of present-day contributions to this area. Why does no-one, or at least hardly anyone, consider the progress of scientific knowledge any more? It is not so very many years since such a notion seemed an obvious and reasonably unproblematic form of exposition for the history of science. Indeed there is an interesting disciplinary point to be made here: more than a few historians - and I mean here historians whose subject areas lie outside the realm of science, such as historians of art and architecture, ancient civilizations, politics, warfare, society, and economics - still <u>do</u> regard the history of science as a special case.[1] They tend to hold that because of its peculiar and heavily intellectual, or ideas-oriented, subject matter, it is not altogether susceptible to the same methodological and historiographical pronouncements that non-scientific history is. However, a great many contemporary historians of science would disagree with this because their discipline is increasingly, sometimes it seems overwhelmingly, studied in terms of social considerations which eschew all epistemological deliberations and their established ramifications. Anyone talking or writing about such entities as knowledge and progress (and for that matter, rationality, objectivity, and verisimilitude) is liable to be told that such entities cannot do any analytical work in the history of science. They are peremptory definitions, they are banal,

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[1] This was brought home to me during a conversation with Professor Arthur Marwick at an Open University history symposium held in Cambridge in 1996.

they cannot help the aspiring historian of science to get any kind of a purchase on the past. The question that therefore needs to be asked, is 'is this sort of position entirely unproblematic and are the kinds of criticisms that it levels at the older forms of exposition sustainable?'.

Examinations of explanations of scientific progress and change have occasionally surfaced in recent years. In a book entitled *Scrutinizing Science*, edited by Donovan and the Laudans, a methodological approach is taken to a range of philosophical theories of scientific change. This is done in the expressed hope that a rigorous comparison with relevant historical evidence might produce a survivor among the competing theses that would give an *'empirically well-grounded picture of the workings of science'* and (by implication) provide a historiographical framework too.[1] Although well aware that attempts to bring together histories and theories would raise considerable conceptual and methodological problems, the editors decline to address these problems in any depth.[2] In their summing-up eleven of the original thirty-eight theses survive, rather than the hoped-for one. All eleven are somewhat modified and in fact most of them have only survived the testing in a highly qualified form. David Gooding, who is sympathetic to many of the aims of the editors, called *it 'a worthy start which everyone interested in the philosophical import of historical studies should read with care.'* [3] I think the emphasis has to be very much on the word 'start'. Paul Hoch, who is less sympathetic, regards *Scrutinizing Science* as being like,

> **a new philosophical meta-methodology of rational scientific appraisal which, it is maintained, has been shown to be applicable to all (or nearly all) 'cases' - but which rarely if ever brings in the relevant social processes in the relevant research communities and their relations to broader social/historical structures.**[4]

Clearly any attempt to establish credible relations between history of science and philosophical inquiry must be very careful to address the problems which make such criticism possible.

In addition to this sort of overview there have also been recent works which deal with the growth and development of ideas over a given time span and in a restricted area. For example, one calls to mind Shapiro's *Fits, Passions, and Paroxysms*, which charts the fortunes of the ideas contained in Newton's theory of fits of easy transmission and easy reflection of light up to the 1830s.[5] This book can be said to deal with the growth of knowledge insofar as it details the overthrow of Newton's ideas about coloured bodies and ideas which he rejected, such as the possible chemical nature of light, by a new idea. This idea was that the colours of bodies are due to selective absorption and it led to a new field of study that is still a part of knowledge - absorption spectroscopy. Then there is the collection of essays published in honour of Stillman Drake, *Nature, Experiment and the Sciences, Essays on Galileo and the History of Science*, [6] in which the focus is on authors such as Alhazen, Galileo, Lavoisier, Darwin, Hertz, Einstein and others, all considered to be of the greatest importance in the growth and development of our knowledge of nature and for the 'revolution' they brought about in their scientific fields.

Progress histories as such, of course, are not confined to works which, generally speaking, have some affiliation with the old intellectual, internalist interpretations: mainstream history of science is still broad enough to include some considerations of scientific progress. For example, there are those who use the comparative method. The essence of this approach is to compare in order to detect differences, which will help to isolate the distinctive characteristics of different societies and illuminate the causes of fundamental historical changes. This approach tends to go hand in hand with contextualism, the commitment to the ideal of studying historical episodes and events within their own contexts. Some comparative approaches accept that one of the factors shaping the context is the intellectual factor. One of the best examples I know of, particularly for its comprehensive geographical and socio-cultural scope, is D. A. Goodman and C. A. Russell, *The Rise of Scientific Europe 1500-1800* which, although emphasising the importance for scientific progress of developing social structures, admits that there are plenty of occasions when invoking purely external causes is distorting to the historical record.[2] However,

[2] **For example, it is argued that a socio-political purpose <u>may</u> have been <u>one</u> of a number of factors favouring the adoption and exposition of Newtonianism in England but that to try and uphold the socio-political case more strongly is to** *'lose oneself in a labyrinth of circular arguments, ambiguous labels and unsubstantiated assertions'*, Goodman, D. A. and Russell, C. A., *The Rise of Scientific Europe 1500-1800*, Sevenoaks: Hodder and Stoughton/The Open University, 1991, 258.

interesting though this type of history of science can be, it does not amount to the kind of history of progress which I have in mind, as the following quote must make clear.

> **This book does not pretend to be another history of science, setting out to provide a comprehensive history of scientific ideas. But the chief features of the development of European science are described within a comparative framework. Scientific developments in different countries and at different times are compared and contrasted to discern the effects of varying social, political and intellectual conditions. Various explanations of scientific growth will be examined, and tentative conclusions proposed for the emergence of modern science in Europe, especially in Western Europe, in the period 1500-1800. Large historical problems do not receive immediate, definitive solutions - only gradual illumination.** [7]

Colin Russell exemplifies the approach of the book and of the comparative method itself when he cautions against the *'easy view'* that Newtonianism was the true explanation for the progress of science in Europe in the eighteenth century. In support of this claim he cites the existence of rival movements, such as Hutchinsonianism and Cartesianism, he tracks the differential rate of the dissemination and acceptance of Newtonianism throughout the various countries of Europe, and he emphasizes the complexity of the Newtonian synthesis of science, religion, and philosophy.[8] In *The Rise of Scientific Europe* the growth and development of scientific ideas is not the subject of the historical study but rather part of the relevant context, one of the contributory conditions which has to be evaluated in conjunction with various other explanations for the emergence of modern science. In contextual history as understood today, historical resources for the writing of history are no longer seen as 'internal' and 'external' because that is held to prejudge the question of what is to count as 'science'. The methodology requires the contextual examination of science and scientists in their socio-cultural settings, relying on ordinary historical causes, or natural human processes, to provide the explanations.[3]

However, having said all that, surely it is at least legitimate for the history of science to take, as a fundamental problem, the question of how science has made progress? If this is granted it still leaves open questions concerning what the progress of scientific knowledge

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[3] **The most recent Arts Foundation course produced by the Open University teaches precisely this methodology in the history of science blocks, presenting it as the current orthodoxy. See** A 103, Block 4 and Block Six, Milton Keynes: The Open University Press, 1998. **The course material explicitly denies that knowledge can ever be cumulative and progressive. This is how present-day undergraduates are expected to tackle the history of science.**

might actually consist in and what sort of historiography might be best suited to dealing with it. Is 'progress' simply the increase in the amount and explanatory capabilities of scientific knowledge? Does 'knowledge' mean knowledge of observable entities, or of theoretical entities, or of verified entities? Is 'knowledge' that which enjoys the informed consent of the majority of experts in the field, or is it that which has made the greatest positive contribution to, or (more cynically) the greatest impact upon, human life? Even assuming that it is possible to produce a satisfactory working definition of knowledge, might not a satisfactory, non-Whiggish history of the progress and development of scientific ideas remain elusive? Is it possible to appeal to such entities as truth, reason, rationality, and progress merely in order to state the problems to be addressed, without being drawn into the justification of solutions? This thesis intends to address such questions and, in pursuit of a possible answer, will examine the suitability of a present-day philosophy of science (aim-oriented empiricism) as a framework for doing the history of science.

Completely muddy

Hunt the Historiography

I don't wish, at this stage, to get bogged down in a lengthy analysis of all the current publications that deal with progress in science. What is unarguable is that historiographies dealing unequivocally with the growth and development of explanatory scientific knowledge have declined to the point where they might be regarded as a threatened species. It might be thought that the story of this decline is too familiar to be worth reiterating but I don't believe that to be the case. Almost everyone could cite a few of what they hold to be reasons, some of which may well figure in the following, but how many could offer, or indeed have offered, a wholly comprehensive account? In fact I not only believe that it is instructive to consider the 'how' but that also the 'why' has never yet been properly addressed and could, of itself, form the basis of an entirely separate thesis in which the various possible explanations are carefully analysed in order to try and decide which were the most influential and what form of interplay existed between them. For my own part, I will mostly content myself with the 'how' of the matter and only approach an answer to the 'why' in so far as the elucidation of the various strands which comprise an explanation for the former also serves to shed some light on the latter.

The history of science itself has a long history but for much of that time its accepted structure, its orthodox form, was encapsulated by what is now known as the internalist approach, the belief that improvements and changes in such intellectual entities as scientific beliefs, theories, methodologies, and procedures are primarily, although not exclusively, the result of a logical sequence of progressive developments within science itself. Closely associated with this was the categorizing of the history of science not as a social, political, or economic history but as an intellectual history, a history of ideas. The compatibility of the internalist approach with a progressive, intellectual history is immediately obvious: intellectual developments in science, such as new theories, concepts, or methodologies, are easily attributable to the dynamics internal to science itself. Indeed, this style of dealing with the history of science as the development of intellectual issues internal to itself, flourished until the early nineteen-sixties. It was at this time that weaknesses began to be perceived: history of science defined in this way had too heavily theoretical a character, sometimes making it less than accessible, and it had a tendency to concentrate on a complex array of intellectual issues evolving through the articulation and development of purely intellectual concepts, which meant that social factors (which might well have been relevant) were largely ignored.[4] Indeed, the nineteen-sixties is a good place for the interested inquirer to begin to search for some answers to the problem of the disappearing history of the growth and improvement of scientific ideas.[5]

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[4] **Although, of course, there had been influential broader philosophical critiques: Burtt, E. A.,** *The Metaphysical Foundations of Modern Physical Science* (rev. edn.), London: Routledge and Kegan Paul, 1932; **Butterfield, H.,** *The Origins of Modern Science* (2nd. edn.), New York: Macmillan, 1957; **Cassirer, E.,** *Der Erkenntnisproblem in der Philosophie und Wissenschaft der neuren Zeit,* 2nd. edn., 2 vols, Berlin: Cassirer, 1911.

[5] **History of science developed originally as a study that was integral to the progress of science because it was perceived to be beneficial to science. The fact that there had been continuous development in some branches of the discipline, such as astronomy, engendered an acceptance of the notion that historical sensibility was fruitful for the continuing advance of all areas of science. By the eighteenth century this consciousness of the role that the past had to play in the present development of science had progressed to the point where the historical dimension was often held to be imperative for the effectiveness of any exposition of a particular science. Joseph Priestly maintained that the historical exposition was essential to the effective exposition of the science of optics. (Priestly, J.,** *History and the Present State of Discoveries relating to Vision, Light and Colours*, 1772**). Thomas Thomson wrote,** *'The object of this work is to exhibit as complete a view as possible of the present state of chemistry; and to trace at the same time gradual progress from its first rude dawnings as a science to the improved state which it has now attained. By thus blending the history with the science, the facts will be more easily remembered as well as better understood; and we shall at the same time pay that tribute of respect to which the illustrious improvers of it are*

At a fairly general level it is probably fair to say that the tendency away from intellectual, progressive historiographies initially received much support from five main areas.[6] Firstly, studies on the intellectual context of sixteenth- and seventeenth-century natural philosophy, by scholars such as Yates, Pagel, Rattansi, McGuire, Webster, and Debus, demonstrated that what had been regarded as the internal rationality of science must be widened to include the ideology of all kinds of belief systems.[7] These belief systems, paradoxically, comprised precisely those elements of cultural and social life - alchemy, hermeticism, magic, mythology, religious sectarianism, occultism - which had traditionally been regarded as the antithesis of science. Secondly, around about the same time, external or social history of science began to challenge the accepted notion that it should confine itself to studying necessarily peripheral social factors, such as the educational or financial determinants of scientific work, and began increasingly to define itself as the study of the social conditioning of the theoretical belief systems of science. Thirdly, from the nineteen-seventies onwards, there was the growing influence of the sociology of scientific knowledge. This drew on the work of Mary Douglas, Wittgenstein, Durkheim, Mannheim, and also on anthropology.[8] It was, in addition, influenced by the carefully detailed empirical historical

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justly entitled.'. (Thomson, T., *System of Chemistry*, 1822 edn.). **The nineteenth century saw the decline in the popularity of the historical method of exposition but the gradual rise of the idea that the history of science might be a *bona fide* study in its own right, and this concept was finally enshrined in professional academic respectability around the time of the Second World War. The first man to actually argue in favour of history of science in its own right was perhaps Paul Tannery (1845-1904) but Georges Sarton (1885-1956) is generally regarded as the father of the discipline. The demise of the intellectual tradition in the history of science can be dated to the years following the publication of T. S. Kuhn's** *The Structure of Scientific Revolutions*, Chicago: University of Chicago Press, 1962.

[6] **A good example of this is furnished by Open University history of science courses. Between 1974 and 1988 the OU produced histories concerning how scientific ideas develop and progress within a framework both theological and metaphysical. See,** *Science and Belief: From Copernicus to Darwin*, **AMST 283, and** *Science and Belief: From Darwin to Einstein*, **A 381. Since 1991, they have been teaching a social history of science based on the comparative approach, see Goodman, D. A. and Russell, C. A.,** *The Rise of Scientific Europe*, Sevenoaks: Hodder and Stoughton/The Open University, 1991.

[7] **See, for example, Yates, F.,** *Giordano Bruno and the Hermetic Tradition*, London: Routledge and Kegan Paul, 1964; 'The Hermetic Tradition in Renaissance Science' in *Art, Science, and History in the Renaissance*, ed. C. S. Singleton, Baltimore, 1968: **Pagel, W.,** *Paracelsus: An Introduction to Philosophical Medicine in the Era of the Renaissance*, Basel and New York: Karger, 1958: **McGuire, J. E. and Rattansi, P. M.,** 'Newton and the Pipes of Pan', *Notes and Records of the Royal Society*, 21, 1966, 108-144: **Webster, C.,** *The Great Instauration*, London: Duckworth, 1975: **Debus, A. G.,** "Fludd, Gilbert and the Weapon-Salve', *Jour. Hist. of Med.*, October 1964; 'Renaissance chemistry and the work of Robert Fludd', *Ambix*, vol. xiv, 1967; 'Mathematics and nature in the chemical texts of the Renaissance', *Ambix*, vol. xv, 1968.

[8] **See, for example, Douglas, M.,** *Purity and Danger: An analysis of concepts of pollution and taboo*, London: Routledge and Kegan Paul, 1966; *Natural Symbols*, London: Barrie Jenkins, 1970; *Implicit Meanings*, London: Routledge and Kegan Paul, 1975, part III; *Cultural Bias*, London: Royal Anthropology Institute, 1978: **Durkheim, E.,**

research of such scholars as T.S. Kuhn and partly facilitated by developments in the fourth area, the philosophy of science, which seemed to indicate that it was impossible to reconstruct science as rational and progressive. Fifthly, there was the general failure among academics who were actively concerned with progressive historiographies to produce a historical methodology that was both progressive and non-anachronical. Examples of this, to be considered later, are Bachelard and recurrent history, Lovejoy and invariant historical themes, and Holton and thematic analysis. Hand-in-hand with this fifth academic area went the perennial problems associated with Whig historiography.[9] In order to bring some organisation into the ensuing analysis, I propose to separate those scholarly endeavours which can be said to have had more influence on the demise of the history of scientific ideas and the concomitant development of a social history of science, the first four, from the fifth, which was more significant in weakening the credibility of progressive histories as such. Obviously dividing them into two categories like this is somewhat artificial because all these factors are interrelated. Ideas of rationality and progress, for example, are closely linked insofar as they are primarily related to the concept of knowledge. Progress is progress in knowledge; rationality is defined with regard to knowledge. Before we can address the problem of whether we can know that science makes progress there is the prior problem of what it means to assert that science makes progress towards increased knowledge of truth. All scientific theories so far produced have been (strictly) false and no sense can be made of the idea that one false theory can be closer to the truth than any other. If scientific knowledge cannot progress towards the truth, objectively established, what can it progress towards? Clearly, the problems within the philosophy of science had some influence on the progressive aspect, as well as on the intellectual aspect of the history of scientific ideas and perhaps also on the concept of progressive histories in general. Equally the widening of the

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Selected Writings (1899) (ed and trans. A. Giddens), Cambridge: Cambridge University Press, 1972: **Mannheim, K.,** *Ideology and Utopia: An Introduction to the Sociology of Knowledge*, London, 1936. **Wittgenstein's influence has tended to come through his claim that to understand a language is to understand a 'form of life' and, as developed in Winch, P. G.,** *The Idea of a Social Science and its Relation to Philosophy*, London: Routledge and Kegan Paul, 1958, **stimulated extensive debate about the methodology of the social sciences.**

[9] **Whig history is a historiography dating from the late seventeenth century and it has had great influence in various areas of historical study. Its most obvious characteristic is the elevation, with scant regard for historical context, of any past figure, event, or concept which had advanced ideas which accord with 'present-day' views, that is to say, ideas current at the time in which the historian was writing and which were deemed to be transcendent and eternal by virtue of having been anticipated, and even fought for, for so long.**

ideologies of belief, the more bullish attitude in the social history of science, and the developments in the sociology of scientific knowledge had their part to play, either by directly undermining the concept of a progressive history of scientific ideas or by offering apparently less problematic ways of doing science history. However, I have already stated that I do not propose to examine either the interrelation between the areas under discussion or their relative importance, one to the other, in the decline of histories of the progress of scientific knowledge. The fifth area, being more concerned with the notion of progress in history, sits more logically with considerations about Whig history and to separate the areas in this way makes for easier handling. Where there are significant overlaps and interrelations within my 'social' category which also have important consequences for my 'progressive' category, such as are to be found between the philosophy of science and the sociology of scientific knowledge, I will deal with them.

Influences on the Development of a Social History of Science

The first two developments, the widening of the ideologies of belief and the increased interest in sociological accounts of 'action', also had their role to play in a more significant development and were in their turn, perhaps to a greater degree in the case of social history of science, influenced by it. This development may be characterized in terms of the consensus which, over the past thirty years, has formed around the historiographic framework which sprang from the work in the philosophy of science of Quine, Feyerabend, and Kuhn,[10] and in particular around Kuhn's book, *The Structure of Scientific Revolutions*.[9] The key issue concerned demarcation: could science be analysed as a community activity, just like any other, or not? More particularly, with regard to histories of scientific progress, it was Kuhn's definition of a revolution that was fundamental. A Kuhnian scientific revolution was founded in a theory of the practising scientific community, whose practitioners were persuaded to throw off the existing paradigm by means of values which acted to inform rather than dictate rational choice. His correct diagnosis that there was

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[10] **See, for example, Quine, W. V.**, 'Two Dogmas of empiricism' in *From a Logical Point of View*, Cambridge, Mass.: Harvard University Press, 1953; *Word and Object*, Cambridge Mass.: Harvard University Press, 1960: **Feyerabend, P.**, 'Problems of empiricism', in Colodny, R. G. (ed.), *University of Pittsburgh Series in Philosophy of Science*, Englewood Cliffs, N.J., vol. ii, 1965, and vol. iv, 1970.

something amiss with existing theories of rationality [10] spawned a notion of rationality that concentrated on the <u>impossibility</u> of there being an algorithm which could dictate rational change and thereby function as a device to let objectivity into science.[11] Therefore it seemed that everything within a framework must be unavoidably relative to that framework. In addition to relativist frameworks there were the related factors of the underdetermination of theories and the apparent incommensurability of paradigms.[12] Quine indicated that scientific theories are never logically determined by data, so that there are always in principle alternative theories that fit the data more or less adequately.[11] He further argued that any theory may be falsified by adjusting the extra-empirical criteria for what counts as a good theory; equally any falsified theory can be rescued by apparently contradictory data. As Quine recognises no separate category of *a priori* truth, the extra-empirical criteria, which can be seen to change between different times and different peoples, can have neither empirical nor rational underpinnings.[13] The implication is that they could be explicable in social terms. The apparent incommensurability of competing paradigms, which was stressed by both Kuhn and Feyerabend [12] in the nineteen-sixties and seventies, reinforced this tendency and led to the belief among some historians of science that if no philosophy of science can reconstruct science as rational, then no philosophy of science can be used to reconstruct the progress of science through history as rational. If there is no universally applicable conception of scientific rationality then there can be no way to make rational sense of scientific progress <u>defined as the growth and improvement of scientific knowledge</u>

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[11] **Which isn't to say, of course, that Kuhn argued that science is neither objective nor rational. What he <u>said</u> was that** *'existing theories of rationality are not quite right'* **and** *'there are some limitations of objectivity'*. **See** 'Objectivity, Value Judgement, and Theory Choice', **in** *The Essential Tension*, (1977). **It was the lack of any one, superior <u>algorithm</u> that he emphasised.**

[12] **For a comprehensive list of works up to 1980 which acknowledge the influence of Kuhn see, Gutting, G. (ed.),** *Paradigms and Revolutions*, Notre Dame, Indiana: University of Notre Dame Press, 1980, **particularly that part of the exhaustive bibliography dealing with the philosophy of science, the history of science, and the sociology of science,** 324-333.

[13] **Quine rejected the concept of analytic truths (truths which hold by virtue of meanings) and asserted that** *'any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system'*, ('Two dogmas of Empiricism', 1953). **Later, he developed his most famous doctrine, the 'indeterminacy of translation', concluding that there are no objective facts about which words and sentences have the same meanings.** (*'Word and Object'* 1960)

and understanding about the world.[14] Social factors would, for that reason, seem to constitute a better historiography of science.

Therefore, the original movement towards a social history of science was, to a degree, dependent upon a serious and seemingly unchallengeable critique directed against contemporary philosophy of science by certain philosophers of science. As I have indicated, the trend received additional support from developments in the old 'internal/intellectual' and the old 'external/social' approaches to the history of science. Mary Hesse encapsulates this as follows:

> **Conflicting scientific paradigms or fundamental theories differ not just in what they assert as postulates, but also in the conceptual meaning of the postulates and in their criteria of what counts as a good theory: criteria of simplicity and good approximation, of what it is to be an 'explanation' or a 'cause' or a 'good inference', and even what is the practical goal of scientific theorizing. All such differences are inexplicable by the logic of science, since they are precisely disputes about the content of that logic. The historian must make them intelligible by extra-scientific causation.** [13]

However, it is necessary to say that problems within the philosophy of science were not restricted to being directly influential upon the history of science but were also filtered through the medium of another discipline: the sociology of scientific knowledge (SSK) became an influential strand within social history of science from the early nineteen-seventies. SSK developed its arguments partly on the strength of the lack of a satisfactory logic of science and the concomitant impossibility of thereby getting around the problems of induction (the traditional problem of the rationality of science) and of verisimilitude. The lack of such a logic negated the possibility of establishing any propositions with absolute certainty and even if knowledge is generally agreed to be conjectural there is still no way of getting round the problem of the underdetermination of the theories that go to make up

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[14] **Also influential here is work by Hanson and Polyani, drawing on Wittgenstein and the German gestalt psychologists. During this time Polyani considered at length what goes on in the mind of the scientist. Polyani, M.,** *Personal Knowledge: Towards a Post-Critical Philosophy*, London: Routledge and Kegan Paul, 1958; *The Scientific Imagination*, London: Routledge and Kegan Paul, 1966. **Like Kuhn, he stressed the way in which science as an activity depends on the existence in the scientific community of bodies of tacit knowledge. It was Hanson who suggested that a conceptual revolution in science is analogous to a gestalt-shift, in which relevant facts come to be seen in a new way. Following Wittgenstein, Hanson distinguished between 'seeing that' and 'seeing as' and identified the latter, the gestalt way of seeing, as being significant in the history of science. Hanson, N. R.,** *Patterns of Discovery*, Cambridge: Cambridge University Press, 1958, 5-24; *Constellations and Conjectures*, Dordrecht: D. Reidel, 1973; **Wittgenstein, L.,** *Philosophical Investigations*, New York: Macmillan, 1953, 193-207.

scientific knowledge, no rationale that will select just one theory from equally many others which agree with available evidence. Steven Shapin has summarized the development of some of the principal themes succinctly and drawn out the link between SSK and social history of science.

> **Accordingly, early SSK took it as a primary task to create a legitimate space for sociology where none had previously been permitted, in the interpretation or explanation of scientific knowledge. In just that sense, SSK set out to construct an 'anti-epistemology', to break down the legitimacy of the distinction between 'contexts of discovery and justification', and to develop an anti-individualistic and anti-empiricist framework for the sociology of knowledge in which 'social factors' counted not as contaminants but as constitutive of the very idea of scientific knowledge (e.g. Bloor 1975, Law 1975, cf Fuchs 1992: Ch 2). SSK developed in opposition to philosophical rationalism, foundationalism, essentialism, and, to a lesser extent, realism. The resources of sociology (and contextual history) were, it was said, necessary to understand what it was for scientists to behave 'logically' or 'rationally', how it was that scientists came to recognize something as a 'fact', or as 'evidence', for or against some theory, how, indeed, the very idea of scientific knowledge was constituted, given the diversity of the practices claiming to speak for nature (Bloor 1984a, b, Collins 1981b).** [14]

What SSK also demonstrates, given this scenario, is the somewhat problematic nature of my division between the areas which have been most beneficial to the development of the 'social' and those which have been most deleterious to the survival of the 'progressive'. SSK reinforced what some sections of the philosophy of science seemed to be saying with regard to the impossibility of ever being able to make rational sense of the concept of the growth and improvement of scientific ideas. To attempt, for example, to produce confirmatory evidence for the existence of unobservable entities in a series of well-confirmed theories by <u>invoking</u> the concept of scientific progress within that series, is to invoke progress in order to establish the existence of progress. The growth and improvement of scientific ideas appeared, as a concept, to be in deep waters.

A Closer Look: Social History of Science Over Twenty-Five Years

The 'strong programme' or Edinburgh School, which was perhaps the earliest recognisable group to be covered by Shapin's exposition, accepted the apparently inescapable relativism inherent in the Kuhnian analysis, and the concomitant ban on histories of the progress of scientific knowledge. They also accepted the fundamental methodological principle implied by Kuhnian history of science (and central to much post-Kuhnian history of science), which is the belief that past science must be treated contextually, in its own setting and in its own right. Furthermore, as constructivists, they held that the pattern of reality in whose terms we work and in whose terms we define rationality is, in truth, our own construct. All our constructs and means of construction are culturally specific and can only be judged relative to the culture that produces them. In addition to a commitment to contextualism, constructivism and relativism, the 'strong programme' also promoted itself as,

> **essential to the history of institutions, the *history of ideas*, anthropology, sociology, and cognitive psychology because all of these disciplines account for the diversity of systems of knowledge, their distribution and the manner of their change.** [15] (my italics)

To oppose relativism and constructivism, and to grant certain forms of knowledge, such as scientific knowledge, a privileged status is to pose a *'real threat to scientific understanding of knowledge and cognition'*.[16] Mary Hesse, in the act of giving a certain amount of support to the 'strong thesis', argued that instead of believing that analyses

> **of 'our' language, rationality and science will reveal the presuppositions of any possible language, rationality and science (we ought rather to take note of) the suggestions coming from the history of science and philosophy, the anthropology of other cultures, and Marxist analyses of ideology, that 'our' language may be relatively culture-bound.** [17]

Elsewhere she asserts that we ought to learn from *'cross-cultural understanding'* and in order to show the sort of *'scientific'* understanding of our own culture that we show towards others, we ought, in the words of Hollis and Lukes, *'to grasp it from within, which implies, of course, that objectivity is an internal standard.'* [18] Consequently, any intellectual or value judgements which might be invoked to delineate the special position of scientific knowledge, or to demonstrate how it has progressed, are unavoidably the product of a framework that is being imposed upon the past: this guarantees the impossibility of ever coming to a proper, objective, detached, <u>scientific</u> understanding of scientific knowledge! The 'scientific' task is to investigate *'specific local causes'* of beliefs because there are always inescapable local modes of *'cultural transmission, socialization and social control.'* [19]

The 'strong programme' is further characterized by four principles held to be characteristic of both an adequate sociology of knowledge and a proper account of science. David Bloor set down the principles of Causality, Impartiality, Symmetry, and Reflexivity in *Knowledge and Social Imagery*.[20] The Impartiality Principle is the requirement that there must be no judgement made between any epistemic classification, such as success and failure, truth and falsehood, or rationality and irrationality, all of which require explanation. The Symmetry Principle is the idea that as explanation does not vary with the epistemic status of the belief explained, it can be categorized as symmetrical. The Principle of Causality states that to be truly scientific one must look for the causes of beliefs, treating belief (or knowledge) as if it were an effect, rather after the manner of any scientific study. The 'social' is prior to the 'natural'. The Principle of Reflexivity is the idea that the Causality principle also applies to the beliefs of the 'strong programme' and sociology of knowledge in general - such knowledge is also an object for scientific study. When Barnes summed up the methodological requirement encapsulated by the Impartiality Principle he signalled an important departure from Kuhn's ideas.

> **What matters is that we recognize the sociological equivalence of different knowledge claims. We will doubtless continue to evaluate beliefs differently ourselves, but such evaluation must be recognized as having no relevance to the task of sociological explanation; as a methodological principle we must not allow our evaluation of beliefs to determine which form of sociological account we put forward to explain them.** [21]

This rejection of intellectual or value judgements had the immediate effect of seriously undermining intellectual history of science in a way that Kuhn never intended, for although he denied the possibility of any context-independent standards of inference and evaluation, he believed that evaluative criteria which would allow one to judge theories in terms of their accuracy, simplicity, fruitfulness, etc., offered a way around the problem.[22] Barry Barnes however, explicitly justified the relevance of Kuhn's ideas to a sociological approach to science,[23] seizing on the notion that scientific rationality is founded in the general agreement of the scientific community, and arguing that the values it works with, even though they act to inform choice rather than dictating it, are necessarily and irredeemably context-dependent.

> **Fundamental theoretical transitions in science are not simply rational responses to increased knowledge of reality predictable in terms of context-independent standards of inference and evaluation.** [24]

Barnes regards Kuhn's pronouncements upon rationality and his evaluative criteria as an unfortunate lapse in an otherwise unblemished record of interpreting the development of science as part of a specific form of culture.[25] Drawing heavily, although not equally, upon developments within the four academic areas which comprise my 'social' category, the proponents of the 'strong programme' decided that in any complete account of an episode in the history of science the determining factors had to be social causes. They noticed that in the history of science even the most celebrated scientists appealed to external as well as internal considerations. They noticed that in the philosophy of science recent developments had been much concerned with underdetermination and incommensurability,[15] and the fact that it was apparently impossible to abstract even elementary facts from the observational framework within which they had been identified. They also noticed that much of post-Kuhnian philosophy of science could only explain science and its history in non-progressive, relativist terms and, much influenced by the what **SSK** had to say, they concluded that the only solution must be that science is, in truth, the domain of the sociologist. The decisions which scientists make cannot be based on rational considerations involving an internalist approach and value judgements because that would violate the methodological principle which denies that our evaluations of beliefs can ever be allowed to influence the form of sociological account which is put forward. Kuhnian science was practised within a relativist social framework and, at a stroke, seemed to justify, by being an instantiation of something which must be investigated from within, the entire sociology of knowledge approach.

The upshot of this for histories of the progress of scientific ideas is not promising: all aspects of history, even intellectual ones, can be fully construed in terms of interests of various descriptions, which reduces the intellectual dimension to the status of an epiphenomenon. This suggests that not only are histories of the progress of scientific ideas out of the question but also intellectual histories that adhere to the fundamental methodological principle of treating the past in its own right and on its own terms. The

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[15] This was particularly true of the Kuhn-Feyerabend programme of the 1960s and 1970s.

'strong programme' categorically states that the present-day and historical contexts of justification and discovery are both open to externally-grounded sociological investigation with only a very narrow range of considerations left within the scope of philosophy of science and intellectual history. In practice I suspect that the range narrows down to nothing. The position has shifted from Mannheim's seminal work in the sociology of knowledge, which allowed that studies in cultural science could be grounded in social causes, although studies in the natural sciences, comprising a body of true and grounded beliefs that equated to knowledge, must be based on evidential reasons. [26] The shift proceeded via Kuhnian relativistic frameworks, and came to the conclusion that the relativistic framework is a social unit with socially defined conventions. This makes rationality, objectivity, theory-choice, verisimilitude, etc., into entities which can only be grasped from within the framework which that defines them. The relativistic framework is clearly functioning as a lightly-disguised theory of social causes. How much better, how much more reasonable, to have an explicit theory of social causes, carefully illustrated through the medium of the case study. The social unit and the relativist framework have been welded together into an entity which does only seem amenable to a sociology of knowledge approach, with all that that entails for histories of the growth of scientific knowledge.

Of course the 'strong programme' has been on the receiving end of a certain amount of criticism in recent years but not because of the degree to which is has constructed its own edifice upon the negation of assumptions that are foundational to other disciplines.[16]

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[16] **For a fairly early, perceptive, but not unfriendly criticism of the 'strong programme' see, Hesse, M.,** 'The Strong Thesis of Sociology of Science', *Revolutions and Reconstructions in the Philosophy of Science*, Sussex: Harvester Press, 1982, pp 29-60. **She concludes that, if presented as a debate between rationalists and relativists,** *'the strong thesis of sociology of science is not as strong as some of its proponents have implied and some of its critics have objected'* (p. 56). **If, however, it is interpreted as a debate between evolutionists and critical or hermeneutic theorists, then the strength of the 'strong programme' is its belief that** *'social and historical analysis can provide a valid critique even of our own presuppositions'* (p. 57). **This puts it nearer to the tradition of hermeneutics, which** *'depends neither on uncritical analysis of our language as if it were language as such, not on the incommensurable relativity of languages and forms of life, but on the assumption that cross-cultural understanding and self-reflexive critique are both possible and illuminating.'* (pp 57-8). **Rom Harré objected that even if the programme were to succeed in its sociological reduction of all epistemological issues it would still have** *'committed the "genetic fallacy". It does not follow that because one has given a correct account of how some belief came to be held that we are not entitled to ask about its truth as well ... the revelation of how that belief was caused has no bearing on its value as knowledge.'*, **see Harré, R.,** *The Philosophies of Science*, Oxford: Oxford University Press, 1984, 194. **There are further criticisms of the 'strong programme' to be found in, Hollis, M., and Lukes, S. (eds.),** *Rationality and Relativism*, Oxford: Blackwell, 1982. **Hollis is perhaps the most vociferous with** 'The Social Destruction of Reality', **but there is also Newton-Smith arguing that relativism of truth and of reason are meaningless in** 'Relativism and the Possibility of Interpretation', **and Estler, arguing for the normal canons of scientific method and logic in**

However, I believe this to be an area worthy of critical investigation. Consequently, I now wish to consider whether or not the sociology of scientific knowledge and social history of science have continued to legitimize their own positions and interrelations in terms of a critique involving the four areas that comprise my 'social' category, and particularly with regard to the problems besetting the philosophy of science. A quick résumé of history of science positions taken over the past fifteen years is instructive.

Firstly the 'strong programme' itself ought to be considered. By the late nineteen-seventies David Bloor had developed Barry Barnes's classic formulation, which subsumed intellectual concerns and interests under social concerns and interests (so that there is a very strong sense in which someone's intellectual work is the result of their social interests) into a more fluid account. Influenced by Wittgenstein and the concept of 'rule-following', he analysed the ways in which we actively construct the world through 'forms of life', which are the only things which make it possible to understand the world but which nevertheless give us more agency in that construction. By the mid nineteen-eighties it was clear that neither the founders of the 'strong programme', nor Mary Hesse[17], were radical constructivists. All three have argued for a limited realism, for the recognition that the natural world does affect our perceptions of it. Hesse has argued, against the claims made by Harry Collins and others, that even though knowledge is socially constructed, so that there is no non-social construal of nature against which actual knowledge can be checked, this does not mean that 'nature' cannot properly enter our understanding of how 'nature' is constructed. Just because science is underdetermined by nature does not imply that it is wholly determined by social factors: to believe that it is to prejudge the issue by methodological fiat.[27] The problem has become one of working out the degree to which knowledge of nature is a social construct and the degree to which it might be said to actually

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'Belief, Bias and Ideology'. **Criticism of the 'interest-explanations' of the Edinburgh School, that they were inadequately established on empirical foundations and that there was circularity involved in inferring them from the effects they were intended to explain, are to be found in Woolgar, S.,** 'Interests and explanation in the social study of science', *Soc. Studies Sci.* 11, 1981, 365-94 **and Yearley, S.,** 'The relationship between epistemological and sociological cognitive interests: some ambiguities underlying the use of interest theory in the study of scientific knowledge', *Studies Hist. Philos. Sci.* 13, 1982, 353-88. **Later the work of Bruno Latour pointed out inconsistencies within SSK generally, as in** *Science in Action: How to Follow Scientists and Engineers Through Society*, Cambridge Mass.: Harvard University Press, 1987.

[17] **Mary Hesse, through her work on the Duhem-Quine thesis and underdetermination, and through her demonstrations that observations are necessarily theory-laden, did much to facilitate the work of** SSK.

exist.[18] However, Barnes and Bloor are still perceived as being extreme social constructivists in some quarters and John Pickstone has asserted that to avoid being increasingly misunderstood they need to be as rigorous with their arguments against the exclusion of nature as they once were with their arguments against the exclusion of social factors.[28] The developments which have occurred within the 'strong programme' have been more in the nature of fine tuning than anything else: certainly they have not fundamentally reconsidered their position with regard to the philosophy of science. These days, perhaps, their influence has waned: the actor-network theory is now in the ascendancy. However, they remain[19] and as scholars continue to consider themselves as a part of the 'strong programme'[20] their legacy remains considerable. This subject will be returned to at the end of this section.

Secondly, I have chosen Roy Wallis's 'Introduction' to *On the Margins of Science*,[29] because it is so representative of the kinds of argument being put forward in the early eighties.[21] The argument runs something like this. To begin with, scientific truths are defined as entities which have to be established with absolute certainty. However, there are no logically water-tight grounds for any set of beliefs. As a result of these two 'facts' scientific propositions cannot be established with absolute certainty. Additionally, if scientific propositions cannot represent the truth this implies that there are no criteria for distinguishing truth from error: as a result scientific propositions cannot be allocated to explanatory modes which reflect this distinction. However, it is possible to interpret the

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[18] **Of course, to acknowledge that knowledge is a social construct is not to necessarily admit that it is not genuine knowledge. Popper argued that the social character of science is so essential to its objectivity, rationality, and scientific character that if 'knowledge' is not (a special kind of) social construct then it cannot be scientific knowledge at all. See Popper, K.,** 'Sources of Knowledge and Ignorance', *Conjectures and Refutations*, London: Routledge and Kegan Paul, 3-20; *The Poverty of Historicism*, London: Routledge and Kegan Paul, 1957, section 32.

[19] **Recent contributions include, Barnes, B., 'Realism, relativism and finitism', in D. Raven, L van Vucht Tijssen and J. de Wolf (eds.),** *Cognitive Relativism and Social Science*, New Brunswick, NJ: Transaction, 1992, 131-47; 'How not to do the sociology of knowledge', **in A. Megill (ed.),** *Rethinking Objectivity*, Durham NC: Duke University Press, 1994, 21-35. **Bloor, D.,** 'Left and right Wittgensteinians' **in Pickering A. (ed.),** *Science as Practice and Culture*, Chicago: University of Chicago Press, 1992, 266-82.

[20] **Such as Pickering, who (only slightly tongue-in-cheek) announced,** *'Unlike Groucho Marx, if the strong programme will have me I will be happy to be a member'*, **in Pickering A.,** 'Knowledge, Practice and Mere Construction', *Soc. Studies Sci.* Vol. 20, London: Sage, 1990, 682-729.

[21] **Hollis and Lukes,** *Rationality and Relativism* **has already been mentioned but there was also, Brown, S. (ed.),** *Scientific Rationality: The Sociological Turn*, Dordrecht: D. Reidel, 1981.

history of science to show that critical decision-making can be seen as context-dependent and this sort of historiography is not subject to any of the problems listed above.

Thirdly, there is Bruno Latour, who, by means of the methodological prescription that we should be as agnostic about society as about nature, has actively engaged in an attempt to expunge the old internalist/externalist, social/intellectual divide. In pursuit of this aim he has done a great deal to point out inconsistencies within **SSK** and social history of science, denying that society can in any way be prior to nature, or that social interests can determine intellectual interests, or that social reality can be used to explain scientists' beliefs. Nevertheless, he has unquestioningly accepted the tradition which holds that those two areas of inquiry, **SSK** and social history of science, must be specifically formulated so as not to suffer from the perceived problems in the philosophy of science. He argued that **SSK** was correct in the stance it took over philosophical rationalism; philosophers had simply been wrong to use natural reality to explain scientists' beliefs. It is particularly noticeable that the definition of rationality which Latour criticises is the old problematic one of its being something rigid and infallible, an algorithm which is brought into play by the correct application of a fixed and unaltering 'scientific method'. Indeed, he goes so far as to assert that considerations of the history of science which show the *'illogicality'* of hitherto *'rational'* figures - Descartes' theory of vortices, Newton's alchemical beliefs - entail that *'only this year's scientists are right, sceptical, logical etc.'*. This, he continues, is obviously nonsense, because

> **next year, new scientists will have come along who, again, will have to reprimand their predecessors for having been unfaithful to the rules of scientific method! The only logical conclusion of such an illogical belief being that eventually no one on earth is durably rational.** [30] ?

Whatever one might think of this argument, it is clear that Latour regards the concepts of rationality and scientific method as being untenable because they are somehow fixed and immutable. Science progresses, new knowledge is gained and some old knowledge is found to be false, or only approximately true, but the rules of scientific method are unbending: if no one is 'durably rational' throughout this process then the unavoidable implication is that rationality must be something unalterable. ?

again very muddy

This can be seen in *Science in Action*, where Latour sketches the asymmetry, first pinpointed by Bloor, that exists in scientists' traditional views of 'rational' knowledge and 'irrational' belief. The former was seen as something which needed no explanation beyond the phenomena involved: rational knowledge is something which we have when we discover the true nature of whatever phenomena we are investigating and so the phenomena themselves are the only explanation that is necessary. This being the case, all that is necessary in order to obtain rational knowledge is a sound mind and a sound method. The latter, on the other hand, says nothing of any consequence about phenomena but a great deal about the people holding the beliefs: irrational beliefs require special explanations, which may invoke prejudice, stupidity, sexual or racial differences, psychological problems, indeed social explanations of many descriptions can be utilized to account for the paths that lead to 'irrationality'. Thus it seems that although few minds are in possession of any reliable concept of what reality actually is, the extension of knowledge to everyone is simply a matter of clearing away all the distorting beliefs. [31] Latour then argues that this definition is untenable, that the most ludicrous episodes in the history of science can be made to sound as logical and understandable as any that have traditionally been thought to be rational episodes and concludes,

> **Cognitive abilities, methods, adjectives and adverbs do not make a difference among beliefs and knowledge because everyone on earth is as logical or as illogical as anyone else.** [32]

In this critique Latour <u>does</u> point out weaknesses and inconsistencies in SSK and the social history of science positions. It may be argued that Latour blew apart the lingering division between the social and the intellectual, arguing that both are inextricably linked into 'networks': just as nature can never be used to explain how and why a dispute has been settled, so equally society cannot be used to explain how and why a controversy has been decided. Both social determinism and technical determinism are chimera produced by the <u>diffusion model</u>, which postulates that science, technology, and society are three separate spheres. The old asymmetry in explanation depended on the artificial divisions of the diffusion model but then 'social' explanations are, for the same reasons, equally asymmetrical: the only way to be rid of this problem is to treat nature and society symmetrically.[33] Latour then goes even further and argues that the universal application of the symmetrical explanation is just as dangerous as the old supremacy of the asymmetrical explanation because it utterly ignores, *'the very existence of the scientific network, of its resources, of its ability to sometimes tip the balance of forces'* [34] and fails to query why scientists have always striven to create an asymmetry between claims by trying to make theirs' more credible. He concludes,

> **To sum up, the positive aspect of relativism is that, as far as <u>forms</u> are concerned, no asymmetry between people's reasoning can be recognised. Their dismissal of the charges always has the same pattern: 'just because you do not share the beliefs of someone you should not make the *additional supposition* that he or she is more gullible than you.' Still, what has to be explained is why we do not all share the same beliefs. The accusation has shifted from form to content.** [35]

Consequently an asymmetry remains: it is not the old asymmetry dependent upon the social explanations or the 'formal rules of logic' necessitated by the diffusion model but it still has to be accounted for. It is here that Latour shows that in spite of his criticisms he is still committed to the central tenets of SSK and the social history of science. Perhaps this is not surprising when we recall that at heart he remains an anthropologist of science.[22] He asserts as a methodological principle that anyone investigating the history of science must have no preconception of what constitutes knowledge until the practitioners under investigation have defined it. This means that investigators have to be relativists until a scientific controversy is settled but once that has happened they can become realists, at least insofar as reality is defined as that which *'resists all efforts at modification ... at least for the time being'*.[36] At this point nature can be taken to be *'the cause of accurate descriptions of herself'* because scientists have decided that this is the case. *'Nature talks straight, facts are facts'* and there is no point in trying to whip up a controversy about the status of decided facts.[37] This points to Latour's still being a causal relativist in so far as he refuses to consider the truth or falsity of knowledge claims, preferring to interpret them in whatsoever terms they are interpreted in their local context: when historical agents are unsure of the epistemic status of their work the historian must also be unsure - and when they change the context by deciding what constitutes knowledge of reality, historians can take their word for it. Certainly he addresses the issue of what is called the 'robustness' of knowledge, always a

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[22] **See, for example, the** 'Introduction' **in Latour, B. and Woolgar, S.,** *Laboratory Life: The Social Construction of Scientific Facts* (2nd. edn.), Princeton NJ: Princeton University Press, 1986.

central problem for constructivists. Certainly he has made revisions to **SSK** and the social history of science: his use of ethnomethodology to break down 'society' and 'nature' clearly distinguishes him from older Edinburgh school approaches. However, as asserted above, commitments to central tenets of **SSK** and the social history of science remain. He has questioned some of the foundations upon which these disciplines were built but not all.

By 1990 we find Andy Pickering, in 'Knowledge, Practice and Mere Construction'[38] arguing that according to the philosophy of science, science is objective because theorizing is controlled by evidence. The nature of this control is located in the reasoning faculty of the scientists themselves, therefore scientific rationality is redescribed as a logic or set of rules determining theory choice on the basis of evidence. Consequently, logic serves as a kind of rational status of theory and evidence that guarantees the objectivity of science. Unfortunately, there has never been a satisfactory elucidation of the 'logic of science' - so the objectivity of science is a mere assumption and scientific realism is meaningless. This recycles Wallis's arguments concerning the status of scientific propositions and does it in order to emphasize the degree to which the impossibility of there being any objectivity, rationality, or genuine knowledge of the real world of nature depends upon the lack of a satisfactory logic of science. Pickering holds that typically philosophy of science has consisted of attempts to articulate a logic of science, a set of context- and content-free rules determining the acceptance of theory on the basis of a static comparison with evidence, i.e. logical positivism, Popperian falsification, Lakatos's Methodology of Scientific Research Programmes, Bayesianism, etc.

Fifthly, to a straightforwardly social history of science perspective. In Christopher Dear's , 'Cultural History of Science'[39] there is a statement to the effect that the old view in the history of science was that science equals a self-contained intellectual tradition, to be understood without reference to social realities or any realization that they constitute its conceptual form. The cognitive content of science was held to exist in Popper's Third World and to develop independently of human actions. In the old account of the Scientific Revolution, the ideas of one major figure fed into and were developed naturally by the next major figure, so that at the culmination of the revolution views and approaches which had been implicit became fully explicit. What happened, sooner or later, was only what was always there, waiting to appear. He makes the point that such an approach, based upon a particular (and now discredited) philosophy of knowledge and rationality, was best exemplified not by historians of science but by the philosopher Lakatos, who infuriated many historians with his rational reconstructions of the history of science. There is, in this, nothing that clashes with Latour's definition of rationality, or Wallis's first four points, or with all of the points in Pickering's argument. Dear is continuing firmly in the tradition of establishing social, or socio-cultural, history of science as the very antithesis of all historiographies based in philosophy of science, or influenced by developments within that discipline: he seeks to emphasise the degree to which history of science is a branch of the sociology of knowledge arguing that *'notions of the rational have themselves become problematic in recent years'*.[40] In his view science, over centuries of development, can still be regarded as science, as more or less the same discipline, but any particular episode must be examined as contextually as possible because to bind it into its context is the only way to get a meaningful purchase on it. This has an obvious affinity with Wallis's final point about context-dependency, a historiographical position offered as a palliative to more traditional historiographies dependent upon what appears to be an inescapably problematic philosophy of science.

Finally, Steven Shapin, writing in 1994 formulates his argument in response to academic philosophy's views on the philosophical problem of knowledge and rationality, best summarized in the phrase, *'truth is one and what people have taken to be true is many'*. [41] The result is that any body of locally credible knowledge, which is taken to be true, cannot be the same thing as the 'truth', which should be established with absolute certainty and be, therefore, timeless and unchanging. He argues that this restrictive notion of truth is quite legitimate: truth is supposed to correspond to 'the facts of the matter' and it is not presumed to have a history as it is not supposed to change over time. He quotes Gellner in support of this: *'If truth has many faces, then not one of them deserves trust and respect'*. [42] Of course the immediate effect of this is that the one body of knowledge which has always been held to have special claims to truth, science, has to be re-evaluated. It cannot count as 'truth', as something corresponding to 'how things really are', or 'objective reality', because it changes over time and varies from one community to another.

Communities nevertheless persistently believe in objective reality - and in the possibility of having genuine knowledge of it - and make truth-judgements precisely because it is their aim to single out those statements and beliefs which correspond to reality. The concept of reality is,

> **such a potent normative force, and its normativity becomes invisible as such. That is the condition of its power. Reality cannot serve its justificatory function unless the relevant culture recognises it as separate from, and set above, the behaviour of those who report about it and constitute our knowledge of it. That is why ... there is such intense resistance to the very idea of a social history or sociology of truth.** [43]

However, in Shapin's view *'such intense resistance'* is misplaced. Scientific *'truth'* is, in fact, *'accepted'* or *'accredited'* belief; it varies with respect to time and place.

> **What counts for any community as true knowledge is a collective good and a collective accomplishment. That good is always in others' hands, and the fate of any particular claim that something 'is the case' is never determined by the individual making the claim. This is a sense in which one may say that truth is a matter of collective judgement and that it is stabilized by the collective actions which use it as a standard for judging other claims. In short, truth is a social institution and, therefore, a fit and proper topic for the sociologist's investigation.** [44]

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These social history of science and sociology of scientific knowledge interpretations (and philosophy of science too, if one includes Hesse's support for Barnes and Bloor) are by no means uniform, in spite of their many points of contact. However, such interpretations have all been formulated as a response to problems perceived to exist in other academic disciplines, particularly the philosophy of science. In addition, all of them involve the structuring, interests, operations, and power relations of various social and, loosely speaking, political groups, and are severally said to be manifested in such phenomena as the rise of the experimental method, the creation of social movements from natural philosophy, the influence of royal courts and the role of patronage, the institutionalization of science, beginning with the development and spread of scientific academies, the professionalization of science, the socialization of the laboratory which extends to the point where all of society is a part of science, and the social construction of truth itself.[23] Their

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[23] **See, for example, Shapin S. and Schaffer, S.,** *Leviathan and the Air Pump: Hobbes, Boyle and the Experimental Life*, Princeton NJ: Princeton University Press, 1985; **Stewart, L.,** 'The Selling of Newton', *J. of Brit. Studies*, 25, 1986, 178-92: **Biagioli, M.,** 'Scientific revolution, social bricolage, and etiquette', in **Porter, R. and Teich, M.,** *The*

defining characteristic is perhaps a causal relativism which refuses to consider the truth or falsity of knowledge claims, preferring to see such claims as whatever they are understood to be in their local cultural context. They define the production, perpetuation and development of scientific knowledge as a local business, produced in a physical situation whose distinguishing marks it will carry, and capable of being accounted for in terms of conventional human cognitive capabilities.[24] The absence of a universal scientific algorithm, of formal rules of proceeding, and of a unique scientific methodology has prompted them to, as it were, fill the gaps in various ways. They have concentrated on the particular practices of argumentation and the actions of the interest groups that are involved in the establishment of the truth or falsity of claims to knowledge. They have put emphasis on the embodied character of scientific knowledge, investigating who, or what holds it and how it is transferred between individuals and between knowledge-making devices like scientific instruments and the individual. They do not deny natural realism, as the following quote from Bloor demonstrates,

> **No consistent sociology could ever present knowledge as a fantasy unconneted with our experience of the material world around us.** [45]

They do indeed believe that there is one reality, which is the source of all our perceptions. However, no one particular account, such as that of modern science, is privileged, or can be a standard by which all other beliefs about nature may be measured.[25] It therefore necessarily follows that they are against the concept of the intellectual being interpreted as something narrowly rational, against the idea that a present-day perspective, including any philosophy or methodology of science, can ever act as an interpretative framework for the

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Scientific Revolution in National Context, Cambridge: Cambridge University Press, 1992, 11-54; *Galileo Courtier: The Practice of Science in the Culture of Absolutism*, Chicago: Chicago University Press, 1993; **Tribby, J.**, 'Cooking (with) Clio and Cleo: Eloquence and Experiment in 17th Century Florence', *Jour. Hist. Ideas* 52, 1991, 4176-39; 'Club Medici: Natural experiment and the imagineering of Tuscany', *Configurations*, 2, 1994, 215-35: **Gascoigne, J.**, 'A reappraisal of the role of the universities in the Scientific Revolution', in D. C. Lindberg and R. S. Westman (eds.), *Reappraisals of the Scientific Revolution*, Cambridge: Cambridge University Press, 1990; **Golinski, J. V.**, 'Utility and Audience in 18th Century Chemistry: Case Studies of William Cullen and Joseph Priestly', *BJHS*, 21/1, 1988, 1-32: **Latour, B., and Woolgar S.**, *Laboratory Life*: **Shapin, S.**, *A Social History of Truth: Civility and Science in Seventeenth-Century England*, Chicago: University of Chicago Press, 1994.

[24] **These physical situations, or 'sites of production' can range from the individual cognitive space, to the social space, to the geographical space, to the space of practice, equipment, and phenomenal fields.**

[25] **For a clear exposition of these positions and a very useful bibliography see, Shapin, S.**, 'Here and Everywhere: Sociology of Scientific Knowledge', *Annu. Rev. Sociol.* 21, 1995, 289-321.

history of science, against the concept of intellectual frameworks in general and definitely against the notion of a historiography of science that can illuminate and explain the growth of knowledge and scientific progress.

So, to rehearse the argument so far, we established the degree to which the disciplines of sociology of scientific knowledge and social history of science tended to establish their own legitimacy in two distinct ways. Firstly, they drew upon the work of scholars like Douglas, Wittgenstein, Durkheim, and Mannheim. They also drew upon other disciplines like anthropology and upon distinct developments in history of science like Kuhn's detailed empirical historical research. Secondly, and perhaps less actively (for it often only surfaces in occasional statements, or in brief summaries of philosophical positions which are presented as though almost self-evident), they established their discipline in opposition to difficulties which were perceived to reside in philosophy. This included mainstream philosophy (in the philosophical problem of knowledge) and the philosophy of science (particularly with regard to the problems of rationality, incommensurability, objectivity, essentialism, and realism) and further encompassed developments in related historiographies of science. To return to Shapin for a summing-up:

> **In this way, SSK opposed philosophical rationalism - the view that scientific judgment is sufficiently determined by unambiguous criteria of method - by asserting their contingency and the locality of judgement. Rules did not sufficiently explain scientific judgment; the way in which rules were identified and used was itself a topic for contextual inquiry. ... Indeed, the best way of summing up the thrust of a great deal of work in SSK, *and in related history and philosophy*, produced from the mid-1970s to the present, is to see it as concerned to show in concrete detail the ways in which the making, maintaining, and modification of scientific knowledge is a local and a mundane affair.**[46] (my italics)

However, although the negation of the relative assumptions, when it does occur, is usually the clearest and most baldly-stated part of what is often very intricate argumentation, it has become so central to much present-day history of science that scientific knowledge is a social construct, and therefore not what the older generation of intellectual historians of science could regard as 'genuine', that there is little inclination to analyse and question the foundations that underpin that central belief and they therefore are not routinely criticized. It might, of course, be argued that this continued harping on the shortcomings of philosophy of science, even though no longer very intense, is in fact no longer necessary: it was useful in the genesis of social history of science but that now it is no more than an old habit which should be unlearned. The success of much present-day social history of science might be seen as sufficient justification for its existence. Nevertheless it could equally be argued that successful and fruitful methodologies could become self-legitimating and therefore it might be, as a matter of principle, good practice to keep an eye on one's foundations.

To be more precise, it could described as a little far-fetched to say that a new and seemingly successful methodology has an ideological impact upon practising academics, for there are many definitions of 'ideology' and no academic community is homogenous. The influence that such a methodology has will vary between individuals and there will be some who will not be touched by it, who will reject it entirely. Notwithstanding, a successful methodology must have a certain impact, which not only produces a degree of legitimation for that methodology but will also sanction the idea(s) that presided over its development. Of course, a degree of self-legitimation is unavoidably constitutive of a successful methodology.[26] Such a methodology alters perceptions of what is historically and historiographically possible and, in turn, alters perceptions of what is methodologically possible. In so far as a new methodology, even if in a restrictive sense, solves certain historical problems - and perhaps open up new areas for research - it changes scholars' expectations and confirms them in the correctness of the original legitimating concepts. This has certainly been generally true of the strategies which have informed SSK and social history of science methodological practices. However, if such a methodology continues to be successful historians may well come to feel that it is *per se* a good thing, that more and more historical and historiographical problems will be found to have solutions within its organising framework. They may even begin to think, whilst allowing for a certain amount of evolution and development within agreed (and not necessarily immovable) parameters, that it is the only legitimate organising framework. It is at this point, where the idea(s) that

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[26] This argument is analogous to that put forward in connection with technological determinism, that the success of technological innovation impacts upon people's lives to such an extent, altering their perceptions of the natural order and of the possibilities of technology, that they come to see it as an endlessly good thing, a solution to all problems. Thus is the ideological impact of technology transformed into an ideological legitimation for technology. See, for example, Wilson, H. T., *The American Ideology: Science, Technology, and Organization as Modes of Rationality in Advanced Industrial Societies*, London: Routledge and Kegan Paul, 1977.

presided over the development of a successful methodology are not routinely criticized, that the methodology itself might run the risk of metamorphosing into a dominant ideology. If an ideology is defined as consisting of ideas or beliefs shared by individuals with compatible interests, and if a dominant ideology is said to be one held by most of the people who shape their society's view of the world, then it becomes possible to see how a scholarly methodology might suffer this fate. For the word 'society' substitute the words 'academic discipline/community' and interpret 'compatible interests' as those, broadly speaking, held by **SSK** and the social history of science, and the danger is revealed. Therefore, in pursuit of these considerations, it might be beneficial to outline in more detail some of the main methodological ideas of social history of science, in order to understand what their precise function is.

What Are The Advantages of Constructivism?

A foundational argument in favour of constructivism, broadly speaking, states that a rationalist or realist view of science mistakenly implies that the real world will only respond to 'rational' behaviour, whatever that may be. Once a realist-cum-rationalist starts acting 'rationally' by, for example, devising experiments that really do 'interrogate' nature, he or she automatically gets a purchase on reality, everything falls into place, and this guarantees a body of reliable knowledge of the true nature of the universe that can be progressively improved or expanded upon. However, goes the argument, what actually happens is that nature, or reality, feeds into the experimental set-up intended to interrogate it and thereby forces the experimenter to listen to it and ask it the right questions. Agency is lost and the intrepid investigator is transformed into a 'realist dope' and a 'rationalist dope': nature is pulling the investigator's strings and nothing is said about the creativity and real hard work that goes into this sort of endeavour. Some years ago now, the work of Harold Garfinkel and others, in methodology, pioneered a constructivist view, the belief that the real world is, in some truly fundamental sense, constructed by humans.[47] The corollary of this, bearing in mind that getting an objective purchase on reality is less straightforward than has been traditionally supposed, is that social processes and factors feature heavily in that construction.

Contemporary academic wisdom holds that scientific reasoning must bear some relationship to the cultural assumptions of particular times and places; although science is practised within professional sub-cultures, these do not constitute a world apart, governed by distinctive regulatory mechanisms that realize the absolute values beloved of normative philosophers of science. The methodological principle that translated this doctrine into a broadly diffused research program was articulated in the "strong programme" of the Edinburgh school - the principle of "symmetry": if even the most elementary of facts cannot be analytically abstracted from the observational framework within which they are identified, and all theoretical generalizations are underdetermined by facts, it follows that attention to social factors is no less appropriate in interpretation of apparently fruitful scientific deliberations than in consideration of deviant scientific behaviour. [48]

The arguments concerning the degree of constructivism that can be permitted revolve around the extent to which nature is said to have a hand in constructing knowledge about itself. Some constructivists seem to imply that the actual content of scientific knowledge can only be determined by social processes; for example Harry Collins has claimed that the natural world should be treated as though it did not affect our perception of it.[49] Others do not really deny that the realities of nature play any part in the construction of scientific knowledge, rather they presume that these realities cannot be abstracted from the theories and technologies that frame them. Yet others - and I mentioned Hesse, Barnes and Bloor in connection with this earlier - argue for a limited realism.[27] None of them, to be sure, would ever explicitly claim that scientific knowledge is subjective or irrational because, as we have already seen, subjective/objective and rational/irrational are categories that SSK and the social history of science have criticised the philosophy of science for holding. Rather they would simply define scientific knowledge as the result of an inherently social process of reasoning - inherent because social interests are implicit in the very structure of rational thought, and hence in the processes by which knowledge is generated. Consequently, even those in favour of a limited realism could never be correspondence realists, who claim that when their experiments work and their questions are answered it is because they have got a purchase on reality. A typical constructivist response is that correspondence realists are said

[27] **It might almost be called a realism which has been strategically suspended, in the sense that it amounts to an attempt to see just how far it is possible to construct scientific knowledge without the need to invoke nature.**

to be constructing nature and then quite illegitimately using it in an explanation of how it was discovered, a process known as the splitting and inversion model. Initially scientists talk of an entity as if it is somehow the result of artefacts in a laboratory but later, at some point, this entity is split off from its local context and reinserted as the cause of everything that has gone on in the laboratory. Therefore, runs the constructivist argument, in the history and the sociology of science one should always analyse scientific action as if nature is the <u>outcome</u> of the debates and not the determining cause. There is a logical point to be made here: how can something which is the outcome then be inserted back in time to become the cause of what is going on? That, of course, could be dismissed as a sophism, a neat trick. Harder to argue against is the fact that scientists do, in practice, actively construct the world with conceptual schemes, presuppositions, and ontological frameworks.[28]

However, constructivism does have its problems: for example the tentacles of 'dopery' stretch right into the constructivist camp. Both Barnes and Bloor are open to the criticism that in the act of exposing how older forms of historiography tended to turn scientists into 'realist dopes', they managed to turn scientists into dopes of their own social interests. Interest theory is not immune from 'dopery'. Viewed in this light, the concept of a 'social interest dope' is <u>less</u> plausible than that of a 'realist dope' because 'society' is even harder to specify and define than is 'reality'. This criticism was perhaps more true of Barnes, one of whose axioms was that scientific actions should be analysed as if, in some sense, social interests determine, or indeed actually <u>cause</u>, intellectual interests. Indeed, the very title 'strong programme' arose because this is the strongest kind of causal explanation to be had from a non-realist account of scientific endeavour. Also, in response to criticisms that the 'strong programme' posited a too strongly causally deterministic notion of interests, Bloor interpreted interests as 'constructs' invoked by the historian to explain actions. Although not seen as real entities, constructs are thought to be determining links between the social world and the individual over which the individual has no control. Obviously, any narrowly-defined explanation of what it is that produces knowledge, be it 'the realities of

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[28] **For an account which combines a determined argument for ethnomethodology with a detailed critical survey of recent trends in the social studies of science, see Lynch, M.,** *Scientific Practice and Ordinary Actions: Ethnomethodological and Social Studies of Science*, Cambridge: Cambridge University Press, 1993.

nature' or 'social interests', is going to be susceptible to accusations of dopery because it places limitations on agency. However, even taking this into account, it remains the case that the debate concerning the degree to which nature has a say in constructing knowledge about itself, is far from settled and that constructivism, for all its seeming advantages, is not without its own difficulties.

Methodological Relativism

Constructivism, as the above quotation [48] from Kuklick implies, spreads across many academic disciplines: history of science, philosophy of science, sociology of scientific knowledge and what she calls *'the hybrid discipline of science studies'*.[50] Some practitioners in the history of science, anxious to get around some of the associated problems whilst retaining a constructivist position, adopt a methodological relativist stance. Relativism is an inescapable aspect of constructivism: for realists, nature acts as an independent umpire but for constructivists the only thing behind the stumps or at square leg is an entity called 'community consensus'. They construct knowledge about nature (and nature may or may not have an input into this process) in the acknowledged absence of any universally applicable standards or methods for evaluating knowledge claims. As Latour puts it, *'they believe representations to be sorted out among themselves and the actants they represent'*. [51] In the face of this, methodological relativism (which is not the same thing as the epistemological relativism discussed in the philosophy of science) is regarded as an additional analytical ploy with certain uses. Its aims recognise, as a necessity, that something be done about the way in which the 'dopery' of the historian, howsoever that may be defined, affects the 'agency' of the historical agent and in pursuit of this formulates a careful definition of those areas in which a relativist stance is acceptable. In the pursuit of these aims it betrays, unsurprisingly, the extent to which it was formulated in response to problems perceived to reside in the older forms of exposition. It asserts that rather than get tangled up in the debate about whether or not, or in what measure, nature has any say in the construction of natural knowledge, why not be a methodological relativist? Why not hold onto common sense notions of realism, rationality and progress with regard to both everyday knowledge and scientific knowledge, but take a strictly sociological and anthropological stance when confronted by questions of how knowledge was constructed in the foreign country that is the past? Methodological relativism allows a naturalistic interpretation of variation in belief, for to interpret different belief systems using the same methods is not necessarily the same as saying that they are all equally true. It is surely not unreasonable to take this sort of tactical approach: it doesn't involve any denial that it is possible to have reliable knowledge of the real world, nor does it repudiate concepts like rationality and progress. It doesn't deny ontological questions in any way, but sets them aside. It accepts that such things can, in principle, exist but sees them as being the business of the philosophy of science.[29] Over a number of years the history (and sociology) of science have concluded that such categories are not useful and are best dispensed with. In the early eighties, according to Martin Hollis and Steven Lukes,

> **Recent upheavals in the philosophy of science have turned the historian or sociologist of science into something of an anthropologist, an explorer of alien cultures. It is as if scientific paradigms and theoretical frameworks were strung out in time like islands across an archipelago ... Other minds, other cultures, other languages and other theoretical schemes call for understanding from within. Seen from within they make us doubt whether there is anything universal under the sun. This doubt is also a challenge to the very idea of a single world. Is not the world, as interpreted in our scheme of things, but one of many? Are not our forms of reasoning and tests of truth as parochial as any other?** [52]

This marriage of the social and the anthropological under the banner of methodological relativism produces a history of science that is deemed a more worthy, more noble enterprise than the older forms of exposition. Hollis and Lukes identified in it a 'Romantic' appeal to elevated sentiments. Mary Hesse holds similar views to Hollis and Lukes but further develops them on 'hermeneutic' and 'critical' grounds. She pays rather more attention to the recent upheavals in the philosophy of science and, in identifying the cause of that disruption as one which was also irredeemably tied to a Whiggish stance, (See Fn. 9) she betrays her commitment to what is perhaps the fundamental methodological principle in the whole of post-Kuhnian history of science, the belief that past science must be

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[29] **For an exposition of methodological relativism see, Shapin, S.,** 'Here and Everywhere', 292-3.

Totally off

treated contextually. This is the correct way to approach the history of science - both from the cognitive and moral or political point of view. Hesse attacks those who,

> **Ground their faith in universal rationality on a contingent belief that our language and science are somehow the high points of the historical evolution of ideas.** [53]

because they believe that Western Science is a saga of cumulative Reason. It is, of course, only fair to say that the position, forever evolving, has moved on again. The bald idea of the 'explorer of alien cultures' has been challenged by, for example, Harry Collins, who believes that is indeed important to come to terms with the most detailed languages and practices of the scientific culture under analysis.[54] What it advocated now, since the advent of Latour, is more likely to be a subtle interplay of sociological and anthropological techniques. However, it is worth pointing out that if traditional forms of exposition in the history of science are seen as problematic, it is unlikely that the forms that replace them are going to be more so. They are much more likely to be seen as less problematic. This is surely the way to interpret the tendency to deal with intellectual developments as a part of the contemporary culture. For example, rather than explain the phenomena of Newtonianism in the early eighteenth century in terms of Newton's transformation of physical science, why not argue that it was a tool for Christian apologetics that was covertly intended to reinforce the political status quo and maintain social control? [30]

> **Newton's followers created from his natural philosophy a social movement of many facets within the intellectual and political circumstances of late Stuart and early Hanoverian England.** [55]

Certainly, its practitioners see this perspective as more elevated, cognitively and morally speaking. However, it is also surely perceived - and perhaps rightly so - as a less problematic pursuit.

Anthropological and Sociological Techniques

The sociological influence involves the use of the social context and restricts what may count as evidence to the public domain. Most importantly it holds that in all cultures knowledge is culturally conditioned and constructed. It is determined by the conventions and

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[30] **Compare Jacob, M. C.,** *The Newtonians and the English Revolution 1689-1720*, Hassocks, Harvester Press, 1976, **with Russell, C. A.,** *Science and Social Change, 1700-1900*, London: Macmillan.

processes which that culture has: what is subjective, objective, rational, and progressive in that culture is what that culture defines as subjective, objective, rational, and progressive. The point about anthropological techniques is that they aid this approach: bearing in mind that 'interests' always detract from agency - and that any interests which the historian holds implicitly and is unaware of are bound to be the hardest to identify - they introduce a certain amount of distance between the investigator and the subject under investigation. What is held to be required is a half-way house between being a stranger amongst the 'tribe' and a member of it: a stranger to a tribe cannot <u>learn</u> much, a member of a tribe cannot <u>explain</u> much but hopefully the anthropological attitude maximizes what you can learn and what you can explain, allowing you to present the past in its own right and with the minimum of unintentional bias. A significant and much discussed technique is that of exclusionary language: the purity of the disciplinary discourse and the power of understanding is thought to spring from the purity of the language used. Any mode of speech which binds the academic study of science to the scientific community, or to socio-cultural interests thought to have some bearing on the scientific community, is increasingly regarded as impure. Shapin writes,

> **In order to achieve a properly historical, or authentically sociological, engagement with science, it is now said, we have to reject these ways of going on, to purge our discourse of the elements of common speech ... Some even say that if we cannot find a virtuous speech that allows us reliably to refer to real entities called 'science' or 'society', science studies ought to be reconstituted as an immaculately reflexive discourse about self or its own methods for constituting its subjects and objects.** [56]

presumably a joke?

Anthropological approaches, firmly and consciously divorced from their own cultural contexts and intent upon the culture under investigation, are therefore compatible with and complimentary to - sociological approaches. This is academic inquiry which has put the maximum amount of methodological effort into producing a *modus operandi* that is free from any taint of 'discredited' procedures. The history that typically results from the use of these techniques, it must be said, is synchronic, localized, and largely static. *Leviathan and the Air Pump* uses these sorts of tools and although it does examine changes that take place over time, it is a relatively short time. An analogous situation within the sociology of science occurs in Latour's and Woolgar's *Laboratory Life*, in which the investigation is taken into the laboratory, a sort of social and spatial correlate of the temporal dimension within the history of science.

Historicism: The Regulatory Framework

Historicism is what I, in a loose sense, take to be the programme committed to interpreting historical action in historical actors' terms.[31] To impose our categories on the past is, if one follows Barnes's argument in *Scientific Knowledge and Sociological Theory* [57], to block historical understanding. Shapin agrees, arguing that, '*Historians are supposed to want to know how actors themselves perceived their circumstances*'.[58] He further summarises the role of historicism, calling it,

> **a potent check on the brand of presentism which has blocked properly historical investigations of scientific boundaries. Historicism, for example, argues against the legitimacy of treating religion straightforwardly as a factor external to seventeenth-century science if historical actors acted as if religious considerations properly belonged to science. And historicism encourages curiosity about how the domain of "natural philosophy" was constituted and demarcated from, say civil philosophy and natural history.** [59]

As a result of this careful separation of past and present there is no real problem with the fact that our,

> **notions of 'science' and the 'non-scientific' do not capture the discourse of many past practitioners in whose work we may have an interest. ... A project aimed at interpreting past culture can live as happily with the categories of "natural philosophy" or "ye mathematickes" as with "science".** [60]

Historicism works as a regulatory framework insofar as its commitment to contextualism makes it receptive to the various methodological strategies outlined over the past few pages. Without arguing for the relative merits and strengths of the strategies, one to the other, I think it possible to draw some very general conclusions about the framework and the methodological procedures. For example, historicism argues strongly that present categories and a belief in the true pattern of reality distort the past and should never form

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[31] **Historicism is also the theory that social and cultural phenomena are determined by history; the belief that historical events are governed by laws; and the belief that historical development is the most basic aspect of human existence. However, I am using it in the sense of interpreting historical action in historical actors' terms.**

part of an historiography.[32] Typical constructivist arguments bolster this by demonstrating the weaknesses in those present categories which hold that nature has a big share in constructing knowledge about itself. Historicism is wedded to contextualism: methodological relativism offers us the means gaining some sort of a purchase on the relevant contexts by momentarily suspending our own knowledge of, and theories about, nature in order that we may try and understand the knowledge and theories held by historical actors. With his customary wit, James Moore sums up the situation thus:

> **Contextualist history is anti-heroic, not because it favours anti-heroes, but simply because it gives them a choice. It levels the pitch and lets everyone join in. The contextualist writer is not a referee, imposing today's rules on the past. One is rather like a commentator, following the game of science as it was played, explaining strategies, describing the drama, the brilliant saves, the own-goals. The rules sometimes change, the goal posts move. Uproar ensues. Rival fans invade the pitch. Players are sent off and substitutions made. Order is somehow restored. The game goes on and its present state-of-play is known. But in contextualist history this knowledge does not skew the commentary. The match could have gone differently. By now another side might have been winning. Or in future perhaps another *will* be - who knows?** [61]

We must never theorize the beliefs and actions of historical actors, such as their inclusion of religious considerations into science, as irrational: equally we can never theorize them as rational either. Anthropological tools try to put some distance between the historian and the object of study and ensure that another route of contamination by present categories, that of their unconscious importation by the historian into the area of study, is firmly closed off. Paul Forman has given a clear indication of the absolute centrality of historicism and its various methodological strategies in attempting to free the historian of science from interests and biases.

> **Only by *thoroughly* historicizing scientific knowledge - explaining possession of specific pieces or structures of it, not by appealing to a transcendent reality ... but by reference to mundane factors and human actors - can historians of science move away from whiggery and toward intellectual independence.** [62]

It is difficult to be in other than reluctant in agreement with Shapin, when he writes,

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[32] **The historicist position would perhaps permit occasional footnotes referring to present-day beliefs on the few occasions when they are considered to be relevant, rather, for example, than for the partly tactical reasons given by Henrika Kuklick. See H. Kuklick,** 'Mind over Matter', *HSPS 25:2* 1995.

It is hard to imagine any posture other than a broad historicism compatible with what now counts as 'proper history'.[63]

What Does This Mean for Histories of Scientific Progress?

My agreement with Shapin is a regretful agreement because it turns, for me, on the effort of the imagination that seems necessary to overcome this *status quo*. If the beliefs and assumptions of all the shades of social history of science here considered (from extreme anthropological positions, *via* those that are hostile to the idea that the intellectual can ever be something rational, to those who simply view the intellectual with indifference) are taken together, the overall result amounts to more than a comprehensive critique of traditional, progressive intellectual history of science and more than a failure to provide prescriptions for anyone interested in developing a different strategy. There is, of course, no good reason why traditional history of science should not be soundly criticised and every good reason why a conceptual scheme should provide prescriptions that are self-referential and self-consistent. However, what it amounts to is a denial of the very possibility of formulating any sort of a methodology to facilitate the study of the growth and improvement of scientific ideas over long spells of time. It is this that should give us all cause for regret. It is this that makes the effort necessarily involved in challenging the established position worthwhile. The history of science must never, according to this generalized critique, be interpreted in terms of any present view (common sense or more rigorously philosophical) of what constitutes scientific knowledge. It must not be interpreted in terms of any present methodology or philosophy of science that seeks to redefine scientific knowledge. Such approaches are held to distort the past because they decide in advance what is to count as science. If standards of rationality and objectivity are relative to a culture then to invoke something like the progress of scientific knowledge is to invoke an historiography that is not merely less cognitively sound and more troublesome to handle but is actually identified with a species of Western cultural imperialism that is morally, as well as intellectually, disreputable. An historiography acceptable to the approaches and techniques just outlined must eschew anything Whiggish, anything with a presentist flavour, any framework formulated in terms of a present-day philosophy or methodology of science. This obviously includes any framework which might try and deal with the concept of the progress of scientific ideas because the judgements thereby employed tend to take the form of, '**X** was a bad (good) move as it meant that the discovery of **Y** was put back (brought forward) and **Y** is an important part of current scientific knowledge'. These sorts of judgements can have no methodological role in the social history of science and neither can intellectual entities such as scientific method, scientific rationality, realism, and objectivity, and scientific progress because, once again, such a move has fixed in advance what is to count as science. In the social history of science they become meaningless concepts: entities like progress, reality, and rationality can only have meaning when formulated in terms of what counts as scientific knowledge and what is to count as scientific knowledge is something to be established through historical study. It can also be argued that the constructivist arguments that apply to reality and social interests also apply to progress. If scientific knowledge is a human construct then any 'growth and improvement' in those ideas is also a human construct: anyone believing that scientific knowledge grows by virtue of gaining more and more of a purchase on reality is a 'progress dope'. The strings of such people are being pulled by something that exists outside them. All in all it seems that the only methodologies for dealing with historical change are going to be very similar to that proffered by Shapin. *'If practitioners are so minded'*, he writes, obviously expecting that only a freakish few will be interested in anything so outlandish, they could *'treat the history of "science" as the history of antecedent cultures, however these were labelled and constituted.'* [64] Thus it is, I maintain, that present day history of science is not merely couched in such terms as to offer no prescriptions for anyone seeking to forge a different conceptual scheme, it actually denies the very legitimacy of at least one such scheme - the concept of the progress of scientific knowledge.

So the past three decades have seen a considerable shift in opinion as to what constitutes an acceptable historiography of science. However it is, as the many subtleties of interpretation evident within the confines of constructivism suggest, a shift that is perhaps still travelling hopefully, rather than one which has arrived anywhere definite. R. C. Olby, in 1989, argued that,

> **In the last twenty-five years the history and philosophy of science has gained in rigour, sophistication, and breadth of vision. The profession has won some respect from the scientific community, but its own practitioners have not achieved a consensus as to how their subject should be studied, or even what constitute the legitimate objects of their investigation. When we carry out an intellectual stock-taking, when we look for trends, what do we find? Have all historians of science become social relativists? Are scientists for us just another primitive society with their own "folk reasoning", like Azandi Indians? Are we all talking about social legitimation, negotiated knowledge, laboratory life and myths of origins?** [65]

Although, nearly a decade on, there might be a reasonable consensus concerning a number of these issues, paradoxically the field seems more than ever divided by perceived methodological differences.

> **The weight of the world's injustices is dumped firmly on the shoulders of those maintaining 'incorrect' methodological views. This is not a practice for the cardiologically challenged. Fundamental issues of methodological propriety are fervently debated. Choices between ... explanatory versus interpretative goals, stress on structure versus agency, micro versus macro foci, theoretical versus empirical methods - all are often fought out ...** [some practitioners] **discover that the practice has contained social-theoretical entities such as 'interests', and announce their gleeful despair that 'definitive' descriptions or explanations of science can ever be attained, while other practitioners express bemusement that anyone could ever think to construct accounts free of theorizing or pretending to definitiveness.** [66]

So the question remains as to whether there are <u>any</u> current trends which might offer solace to someone interested in formulating a progressive history of scientific ideas. Perhaps some further considerations will cast more light on the matter.

Whig Historiography and Ideas of Progress

Another way of addressing the problem is to move the investigation into the second of my two areas and inquire how far social history of science avoids the problems of progress that are most commonly identified with Whig historiography. Whig historiography has been and continues to be a popular Aunt Sally, whose fundamental role in the scheme of things is to be severely dealt with by sociologists and historians of science alike. Barry Barnes sees T. S. Kuhn's historiography as a reaction against Whiggism: in trying to take the past on its own terms, as far as possible, he detects, on Kuhn's part, a rejection of what were, in the nineteen-sixties, tendencies to set the past into a false relationship with the present, to view all historical change as progress, and to allow the present to pull the past into conformity with itself.[67] Writing in 1982, Barnes maintained that Whig historiography persisted longer in the history of science than in any other branch of history, even up to the (then) present, because of a continuing predisposition on the part of its practitioners to insist that the knowledge of modern science is the true pattern of reality, a pattern which has always been in existence and which could have guided or prompted the thoughts of earlier thinkers in a non-problematic way. According to Barnes, Whig historiography survived at least into the early nineteen-eighties because of a failure on the part of historians of science to accept that the pattern of reality is our own construct, which is specific to our culture and can only be judged relative to that culture. He would hardly make that complaint today. We saw in the previous section that the pendulum has swung to the point where the historiographic validity of any framework which defines what is meant by scientific knowledge is denied, so that a history of the growth and development of scientific ideas is no longer valid.[33] However, whether or not one agrees with Barnes's assessment, it is fairly obvious that he designates Whig history as invalid because it assumes the validity of present-day frameworks. Historiographies associated with discredited philosophies of science are themselves discredited. There is nothing surprising about this but it is worth recording some evidence that academics do reject Whiggism on the strength of that association. Latour, for another, writes,

> **Using the physics of the present day there is unanimity that Blondlot was badly mistaken. It would be easy enough for historians to say that Blondlot failed because there was "nothing really behind his N-rays" to support his claims. This way of analysing the past is called Whig history, that is a history that crowns the winners, calling them the best and the brightest and which says the losers like Blondlot lost simply because they were wrong ... (because) Nature herself discriminates between the bad guys and the good guys.** [68]

Some academics go further and try to define Whig history more precisely, seeing it as but one in a possible series of interactions between present and past. I am thinking here of

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[33] Which is not, of course, to say that historians are never to be found writing about the 'growth' or 'development' of a particular area, but they do so without offering, or seeing the need to offer, a rigorous philosophical defence of their usage. It remains difficult to see how it might be possible to construct a progressive history of scientific ideas.

the two influential papers by Wilson and Ashplant in *Historical Journal* 1988, which, in endeavouring to elucidate just what relationship might legitimately exist between the present and the past, succeed in denying the timelessness of anything, repudiating the very idea that there is any truth in a given text, or indeed any meaning, that projects forward to our own time.[69] They begin by identifying, in the unavoidable location of the historian in his own time, an inherent epistemological problem. They decide that present-centredness, the position of the historian within the perceptual and conceptual categories of the present, is intrinsic to historical research but that the real historiographical error lies not in that but in having 'both eyes in the present'. Butterfield's 'Whig fallacy' does not capture the error in its entirety, being but a special case of the whole error and defined as the specific fusion of present-centred categories with present-favouring values.[34] For a solution the authors recommend what they term a 'source generating process', which involves the use of techniques to show, as far as possible, the sense in which a piece of work is of its own time, whilst trying to ensure that the historian's questions, unavoidably formulated within twentieth-century perceptual and conceptual categories, actually connect with it.[35] The act of employing methodological tools designed to help the epistemologically-challenged historian to get a purchase on something that is firmly contextualized, without disturbing that context, implicitly denies that the search for intrinsic rationality or truth within a text, or any attempt to locate that text within a progressive history, can have any validity. The notion that there is such a thing as explanatory scientific knowledge, as opposed to socially-constructed belief, and that this knowledge progresses, increases, or grows, is a notion that contemporary historians of science are reluctant to sustain.

However, in further pursuit of the question posed at the end of the section entitled **What Does This Mean for Histories of Scientific Progress?** it might be worthwhile to

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[34] **Butterfield was the first to campaign against the writing of history from the ideology of the present time. He further recognised that the peculiarly progressive nature of science made it particularly susceptible to a Whiggish interpretation. See Butterfield, H.,** *The Whig Interpretation of History*, London: G. Bell and Sons Ltd., 1931; *The Origins of Modern Science*, (2nd. edn.), New York: Macmillan, 1957.

[35] **Ashplant and Wilson explain it as follows;** *'Thus the historian's initial questions provide the motive force which animates the enquiry. That dimension of historical research which investigates the source-generating process provides the means whereby those substantive questions can be so modified, away from their initial present-centredness, as to mesh with the past through the relics it has left'.*, **Ashplant, T. G. and Wilson, A.,** 'Present-Centred History and the Problem of Historical Knowledge', *The Hist. Jour.*, 31, 2, 1988, 270.

consider the problem of Whiggishness and progress history and investigate further such matters as present-day perspectives, histories of progress and the whole question of the relationship between the past and the present. The following questions, aimed at elucidating whether or not a progressive history of scientific knowledge is possible, could be asked. Is there a function for a present-day perspective in the history of science which is neither Whiggish, nor confined to 'acceptable' anachronical interpretations, which allow that the goal of history can only be legitimately linked to the present situation in very particular circumstances? Is it, in other words, possible to have a present-day perspective which illuminates the past and allows the construction of a continuous progressive history <u>without</u> rationalizing it or falling into anachronism? Conversely, is it possible to utilise the past, when it has been interpreted in this way, in order to aid the understanding of present-day problems in a way that goes beyond the confines of anachronical history? Is past-present interaction a two-way process? Related to these considerations is the question of intellectual or value judgements, traditionally associated with a given, usually present-day, perspective which has designated in its own image such entities as rationality, objectivity, scientific progress, etc. It is widely held that such entities can have no explanatory role in the history of science because they unavoidably bring the past into a false relationship with the present. Is such a relationship necessarily false and is the role necessarily explanatory? To sum all this up, is there a progressive historiography of scientific ideas which is not irredeemably flawed? These are the questions to be addressed in the next section.

Historiographies and Problems Regarding Progress History

Has anyone writing so far about progress history demonstrated that it is possible to steer a course through the minefield of Whig history, present-centredness, histories of progress, and the whole issue of the relationship between past science and present science? Histories which sanction the growth and development of scientific ideas and involve a present perspective have been shown to be problematic. Diachronic interpretations which are connected to a present-centred view are saved from anachronism by being limited to those cases where it is felt necessary to teach the technical contents of past sciences to present practitioners. Attempts <u>have</u> been made to try to resolve the apparent incommensurability that exists between the two different types of historiography, the contextual historiography which judges the past in its own terms and the progressive historiography which concerns itself with the progress of knowledge. Gaston Bachelard, a strong critic of historicism, has tried to legitimize the concept of a history of science that is directly linked to current science. He manages to get round the problem of having a continuist, teleological account by doubling factual history with an evaluatory history, where the criterion of value lies in the values of modern science. This he terms 'recurrent history', which is an assimilation of past science through the modernity of science that simultaneously has the result that history is continuously rewritten.[70] Recurrent historiography is directed towards what Bachelard calls 'sanctioned history', which is seen as a double of traditional 'obsolete history', or the mere description of earlier occurrences. If obsolete history is taken in isolation it can never constitute proper history of science and can only be of interest to antiquarians. However, recurrent history still decides whether earlier science is valid or not in the light of present knowledge. Consequently, although Bachelard could, on occasion, be quite attentive to historical context, his methodology on the whole is not. According to Fichant, a disciple of Bachelard, obsolete history is *'the history of the thoughts that have become unthinkable in present-day rationality'*, while sanctioned history is *'the history of the thoughts that are always topical or can be made topical if they are evaluated in terms of the science of the day'* [71] The optical theories of Descartes are today regarded as false and so cannot constitute a proper object of study for recurrent historiography; conversely, the wave theories of Huygens and Fresnel can be studied because they can be made 'topical' in the required manner. In declaring that, *'The historian of science is necessarily a historiographer of truth.'*, they maintain that they are seeking the truth in history, which will form part of a continuous progression up to the more 'truthful' present.[72] Bachelard and his school may, through the assimilation of past science through the modern science and the continuous rewriting of history, have avoided a teleological continuist history but they have not avoided the problem of drawing the past into a false relationship with the present. This is because they take as given that the task of the historian of science is to judge the value and truth of the subject - and they insist that those values and truths are only to be found in modern science. There is nothing in Bachelard to persuade anti-Whig historians that they have not been right all along.

Another, perhaps more stimulating way of organising the history of ideas, so that it encompasses some of the elements that make up a culture and simultaneously traces them through time, is through the thesis of invariant historical themes. The invariance thesis maintains that history can be interpreted as a variation on a comparatively small number of fixed topics or unit-ideas that appear repeatedly in significant branches of culture. According to A. Lovejoy, just as the multitude of chemical compounds can be understood to be the result of the combinations of a few kinds of atoms, so the complicated and exceedingly diverse forms in the history of scientific ideas can be thought of as combinations of a small number of unit-ideas.[73] This version of the invariance thesis can focus on long periods of time and cover apparently very different scientists and natural philosophers.

> **The postulate ... is that the working of a given conception, of an explicit or tacit presupposition, of a type of mental habit, or of a specific thesis or argument, needs, if its nature and its historic role are to be fully understood, to be traced connectedly through all the phases of men's reflective life in which those workings manifest themselves, or through as many of them as the historian's resources permit. It is inspired by the belief that there is a great deal more that is common to more than one of these provinces than is usually recognized, that the same idea often appears, sometimes considerably disguised, in the most diverse regions of the intellectual world.** [74]

Sachs has argued that the field concept is just such an invariant unit-idea, which can be traced from the theological and philosophical ideas of Maimonides in the twelfth century and Spinoza in the seventeenth century to the modern field theories that developed from Faraday to Einstein. [75] Sambursky thinks that, *'the inner logic of scientific patterns of thought has remained unchanged by the passage of centuries and the coming and going of civilizations'*. [76] Thus the concept of space in Einstein's theory of relativity is said to be similar to that found in Aristotle and Newton's ether resembles the pneuma of the Stoics. Furthermore, both ether and pneuma bear some resemblance to modern field theory.

On the whole, the invariance thesis is not without its problems. Kragh has summed them up well.

> **Unit ideas are the result of comparative analyses fabricated by the historian. ... The selection of the historian and his interest in historical constancy may result in unit-ideas whose constancy in time is an illusion since the actual historical context in which they appear is disregarded.... Concepts and ideas are rarely or never quite the same over a long period of time. Although the names given to them by historians might be unchanged, fundamental concepts often develop beyond recognition through the historical process.** [77]

The example given is of the circle doctrine, crucial in astronomy from Plato to Kepler but also employed, in the form of cyclic models, in disciplines as diverse as religion, economics, physiology (Harvey and the circulation of the blood), and physics (Galileo and circular inertia). Moreover, Kepler's discovery of the elliptical orbits of planets undermined the view of circularity as the most privileged, most natural form of motion.

> **It is an example of a conceptual theme that has functioned for a long time as a unit-idea, but which is not really invariant. ... Historians who attempt to trace the circle doctrine, viewed as an invariant idea, up to modern science are forced to interpret history of science in an artificial way.**[78]

We are forced back to the conclusion, yet again, that the use of the invariance thesis in the service of tracing scientific progress over long periods of time, *'tends to press modern concepts and forms of thought down on earlier science instead of studying it in terms of its own premises'*. [79]

Gerald Holton further developed the thesis of invariance into a 'thematic analysis'.[80] He argues that one can fruitfully explicate ground-breaking scientific work in terms of foundational, latent, even subconscious concepts, methods, and beliefs. These themata are extra-scientific as they can be the result of all sorts of non-scientific influences and they are not amenable to empirical verification or rational argument. There are very few themata in the history of science and the emergence of a new one is a rare occurrence. Holton concentrates on themata presented as opposing pairs, such as evolution/devolution, hierarchy/unity, reductionism/holism, symmetry/asymmetry, and plenum/vacuum. Thematic analysis, however, tends to concentrate on short periods of time and confine itself to individuals. The themata are said to function as the origins of ground-breaking scientific work; however, as they lie outside science, it is difficult to see how they as origins, or indeed how science as a whole, can be theorized as rational.

Neither Bachelard and his followers, nor the various proponents of the invariance thesis, have reconciled the conflicting demands of a contextual, non-anachronistic history with those of a history of intellectual, or any other, progress. The former, in maintaining that the *'historian of science is necessarily a historiographer of truth'*, accept that the knowledge of modern science is the true pattern of reality. Therefore those scientific ideas which are 'always topical' partake of the pattern of reality of which modern scientific knowledge forms the apogee, and those which, *'can be made topical if they are evaluated in terms of the science of the day'* are being distorted by *'modern concepts and forms of thought'*. The first looks blatantly Whiggish and the second necessitates the rationalizing of history. There is no way of evaluating blind alleys in this sort of history of science, even though they may have proved fruitful for the progress of science.[36] The supporters of the invariance thesis, in its various manifestations, also draw the past into a false relationship with the present. They disregard the historical contexts through which a unit idea progresses, contexts which could illuminate the degree to which such fundamental concepts develop. Themata tend to concentrate on individuals over short periods of time, which is not much good for progress history, and provides no answer to the age-old problem of how something which is fundamentally extra-scientific and therefore not part of the rational core of science can be foundational to 'ground-breaking' developments in science.

So far, this introductory chapter has focused on what the strengths of present-day social history of science are perceived to be, paying particular attention to the manner of its formulation in opposition to problems identified in more traditional ways of doing history of science. Indeed I have thought it worth emphasising the extent to which social history and sociology of science positions continue to be forged in the face of difficulties that began to be identified thirty years ago in the philosophy of science and related historiographies. Within my self-appointed 'social' category, which concerned developments in (and

[36] **Notable examples of 'blind alleys' in the seventeenth century include hermeticism, alchemy, natural magic, and the occult. A good example is one that was used earlier in the text, that of Paracelsus who, in the sixteenth century, formulated a complex cosmology rooted in Gnostic, hermetic, and biblical sources. See Pagel, W.,** *Paracelsus: An Introduction to Philosophical Medicine in the Era of the Renaissance*, Basel and New York: Karger, 1958. **Graham Rees has argued that Francis Bacon, in the early seventeenth century, developed a semi-Paracelsian cosmology of his own, which was never widely accepted. See Rees, G.,** 'Francis Bacon's Semi-Paracelsian Cosmology', *Ambix* 22, 1975, 81-108.

relationships between) the history of science, the sociology of scientific knowledge, and the philosophy of science, I have tried to summarise the present trends in the history of science - and give some indication of their development - fairly, albeit briefly. I hope I have given some good reasons why its practitioners believe it to be a superior method of exposition. Now it seems that within my self-appointed 'progressive' category there is yet another reason for the ascendancy of social history of science: <u>no-one appears to have formulated a progressive historiography of science that is not irredeemably flawed</u>. This further enhances the status of social history of science, reinforcing the arguments that it is cognitively superior by adding to its avoidance of Whiggism, present-centred perspectives, and 'upheavals' in the philosophy of science and associated historiographies, an avoidance of all attempts to construct a progressive historiography. It really does begin to look as though anyone seeking to formulate historiographies of science ought to confine themselves to the methodological relativist/constructivist/social/anthropological camp. Within these parameters, as already indicated, there is by no means a consensus concerning precisely how history of science should be done: witness the subtleties of interpretation in constructivism, the vigorous methodological debates, and the way in which academics continue to harp on the difficulties identified in the philosophy of science, repeatedly building up slightly different expositions from common foundations. This leaves plenty of room for manoeuvre. However, my underlying intention was to build up the social history of science position in order to see if it could be wholly or partly demolished, for in its (even partial) demise, which must call into question its original legitimating ideas, there might lurk the possibility of constructing a progressive history of scientific ideas. With this in mind it is worth pointing out that social historians of science should not lose sight of the fact that their histories and historiographies are by no means free of problems.

Problems Associated With Social History and Historiography of Science

For all that social history/historiography of science has been formulated with the intention of avoiding the difficulties inherent in older historiographies, a number of shibboleths inhere in - and a number of problems result from - this sort of historiography. There are undoubtedly problems associated with the use of sociological-cumanthropological tools in the history of science. For one thing, relations between sociology of science and social history of science have not yet been properly spelled out. Sociologists of science, like Pickering and Collins, have little to say about history. The application of sociology of science to social history of science can be very problematic. *Leviathan* is still perhaps the *locus classicus*. Rosemary Sargent has argued that the more 'sophisticated' historians are, methodologically and historiographically, the more tools they use to be 'ideologically' sound and interpret a culture in its own terms, the more open they are to accusations that what they find is but an artefact of the tools they use which, in turn, is being (illegitimately) used to support the use of those tools.[81] In other words, there is a destructive circularity at work in which the tools find the evidence which justify the use of the tools. Far from being able to hear the past, all that can be heard is the scraping of the tools which are being used to try and uncover it. Also, the anthropological techniques designed to make the inquirer invisible can become reflexive, conjuring up timeless techniques for all cultures, including past cultures, and running the risk of becoming too global to explain anything at all. Then again there is the problem of maintaining the means of communicating with present-day scientists and lay-people when the present-day categories of communication are rejected. Shapin has pointed out that the use of exclusionary language, intended to achieve *'a properly historical engagement with science'*, guaranteeing the integrity of the discipline and enhancing the power of our understanding, has undeniable limitations.

> **If in seeking historicist understanding we did no translation, if we did not avail ourselves of any modern knowledge, and if our accounts used the actual concepts, categories, and methods recognized among seventeenth-century natural philosophers, we might well achieve historicist purity, but at a cost: we would certainly fail to communicate our understandings to our own academic colleagues and to constituencies outside the academy ... as the discourse of our academic disciplines becomes more pure so it becomes more irrelevant to anything outside the disciplinary boundaries we have constructed. The price of purity is privacy.**[82]

Elsewhere he writes,

> **We can develop and put in place arcane languages, but we cannot ensure that others will hear us. Communicative orders are grounded in local natural attitudes and local realist idioms. If we wish effectively to speak to a specified community, we are obliged to share its realist idiom.**

And if we want to communicate at all then we are obliged to employ some version of the realist mode of speech.[83]

There are further complications that result from any subversion of the notion of knowledge, in that any argument employing it is likely to run into difficulties. Certainly the tools employed and the practices adopted are not intended as a route to the truth, merely as a check on prejudiced lines of inquiry that might distort the historical record: notwithstanding, the result from the use of such methodological devices is surely intended to be a secure, worthwhile body of information. Our changing knowledge of nature is now seen as a cultural process, instead of a purely cerebral one, something which is made and remade by historical actors throughout the ages. Yet it retains, to all intents and purposes, the status that historical knowledge has traditionally had. What, after all, is the rationale of censoring 'biased' lines of inquiry if it is not to discover an unprejudiced (and therefore superior) route to historical knowledge? Social historians of science have criticised the older forms of exposition for imposing later intellectual standards on the past: they have determined that what constitutes 'scientific knowledge' can only be ascertained through historical study, if the problem of begging the question of what counts as science through history is to be avoided. In the light of this, can the body of information that results from these careful methodological investigations itself count as 'historical knowledge' or must its validity be judged in terms of some other form of study? To maintain that history of science must work contextually and produce, not accounts of the growth and development of science through time, but rather accounts of the accounts and actions of past scientists, is still begging the question, this time the question of what counts as history of science knowledge! If social history of science criticism of traditional intellectual history's handling of 'scientific knowledge' is correct, why does not social history of science knowledge, given the presuppositions which attend it, require similar validation? In shifting from accounts of the historical development of science to accounts by past scientists of what science consisted of, a move away from the discipline and towards the individual pronouncing or reporting upon it, social historians of science have simply moved the problem down a referential level.

Secondly, there are lessons to be learned from philosophical problems concerned with knowledge that should give methodological relativists pause. If academic philosophy of science has indeed failed to prove that theorizing can be uniquely determined by the evidence, there being no justificationary relationship between the two, then there can be none between evidence and theorizing in any area of enquiry, including social history of science. Worse still is the lack of a universal algorithm for getting at the truth about nature, which entails that there can be no one methodology which will give a direct line to the 'truth', or ensure totally reliable scientific knowledge. Now this need not entail any denial of the existence of knowledge or of the study of its progress and development and improvement over time. Knowledge of the natural world and knowledge of the past and knowledge of how knowledge about the natural world developed over time could all still be possible. What is not possible, in the absence of an algorithm for pinning down the truth, is a guaranteed methodology for pursuing any of these histories. Social historians of science, whilst accepting that there is, within philosophy and the philosophy of science in particular, no fail-safe, prejudice-free method for getting at knowledge of the truth, seem unable to jettison the notion that there nevertheless is a reliable, prejudice-free method for doing history. There may be no consensus as to what it precisely is, although some methodological strategies are widespread, but its existence is seemingly not questioned. How else are we to interpret the methodology wars cited by Shapin?

> **Just because what is at stake is nothing less than the proper interpretation of our culture's most highly valued form of knowledge - its truth - the struggle for interpretative rights has become fraught and bitter. Names are called and mud is slung. [84]**

Notwithstanding, this belief in a reliable historical methodology surely implies the existence of, if not historical truth, at least a privileged body of historical knowledge which the methodology exists to uncover. Perhaps the widest consensus presently available is that the function of social history of science is to demonstrate how science is historical, how it has been made. Consequently, the methodology to bring this about must be one which will uncover two main processes, that by which historical actors come into agreement with one another and that by which their agreements turn into orthodoxies.[37] However, whatever

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[37] As was mentioned in footnote 3, this is now presented to first-year undergraduates as the aim and associated methodology of much history of science. The boundaries of science are viewed as constructs and to find out how and where they have been established is an empirical endeavour involving the interrogation of primary sources. One cannot discriminate between the various historical claimants to the truth without violating someone else's

form it may take, it brings with it no surety: abandoning the search for an algorithm to uncover 'scientific truth' has merely highlighted the general relationship between algorithms and methodologies. There can be no guaranteed historical methodology for getting a purchase on the past, be it contextual, ethnomethodological, or what you will. Consequently, no amount of methodological strategies, regardless of the disciplines they are culled from, can provide that guarantee for methodological relativists.

Certainly present-day social history of science has problems. It has problems with its methodological tools and strategies, both individually, as in the case of exclusionary language, and with the circularity attendant upon the use of such tools and strategies in general. It also has problems with its tendency to beg the question of what is to count as history of science knowledge. Certainly it could pay more attention to difficulties arising from the philosophical problem of knowledge and acknowledge a tendency to believe in the existence of a reliable historical methodology. I wish to assert, however, that the central weakness of social history of science is not related to questions about how far one may employ such tools and strategies, or subvert such concepts, or ignore philosophical problems. The central weakness is to be identified as foundational to the question of what the best historical practices are, a question which is not addressed by tinkering with methodological devices or altering practices but leaving them within agreed parameters. It is best addressed by formulating methodologies, criticising them and questioning their validity and (particularly) that of their 'legitimating' origins. Indeed, I cannot but be struck by the conviction that the traditional (almost exclusively male) intellectual history of science scenario, in which the lone, heroic scholar interrogated Nature in the attempt to wrest away her secrets, has become transposed onto the academic historian of science. The historian, eschewing that sort of view of the scientific past, is now the subject of his own heroic scenario, in which he goes to great lengths, employing the sorts of methodological tools and techniques, and strategies already discussed, to divest himself of his social, cultural, and intellectual prejudices. The heroic scenario, it seems to me, is as much a part of the historical endeavour as it ever was. Once it formed part of a consensus concerning how

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boundary. Knowledge claims on all sides have to be explained symmetrically. See, Moore, J., 'Here's History of Science', *A 103: The Arts Foundation Course*, Milton Keynes: The Open University Press, 1998.

historical agents in the scientific enterprise acted, the concept of the 'scientist as hero' being bolstered by such explanatory frameworks as Popperian conjectures and refutations. Now it forms part of a growing consensus as to how historians of science should act, and the concept of the 'historian of science as hero' has as many repercussions for the discipline as did the older scenario. This can be seen in the fact that some sociological/anthropological tools are now used so routinely by such historians of science that any work which does not take them into account runs the risk of not counting as a properly historical engagement with science. Exclusionary language has been mentioned but there is also the capacity for work to be replicated by others, notions of practice, the importance of instruments, and (remembering reference [61] the necessity of operating a level playing field where all historical actors are 'commented' upon, rather than 'refereed'. The result is that present-day social history of science is actually formulated so as to ensure that it itself is the only permissible history of science! It is not simply that it fails to provide prescriptions for anyone interested in forging a different conceptual scheme because, as I have already pointed out, there is no reason why it should. What is reprehensible is that it subtly re-employs the heroic scenario in order to deny the legitimacy of other prescriptions with which such a scheme might be formulated. There is a quote from Shapin which is worth repeating: *'Historians are supposed to want to know* [my underlining] *how actors themselves perceived their circumstances'.*[57] The implication is that historians are not supposed to want to know anything else. If this amounts to a prescriptive element in Shapin's writing, for it might be argued that historians are able and willing to take into consideration much more than the sum total of what was available to all the actors, it nevertheless does get a purchase on the overwhelming problem arising from the present ascendancy of social history of science. A crusade to suppress prejudice in practice functions to severely curtail what is allowed to count as legitimate historical study. Methodologies intended to remove bias - and theorized as legitimate on that account - are actually functioning to eliminate, by methodological fiat, certain areas of inquiry and the kinds of specific historiographies that might be constructed to deal with them. I have already argued that in academic terms questions like 'how does the growth of explanatory knowledge come about?', or 'is it rational to accept X as a contribution to explanatory knowledge?' don't appear to exist: they have become non-questions because the legitimacy of a progressive history of scientific ideas is denied. The reasons for this are now apparent. The endeavour to be even-handed about giving historical actors a voice simultaneously reduces historians of science to one voice.

On the other hand, to reject methodological relativism and instead engage in the problem of whether and in what degree nature has a hand in constructing natural knowledge is to adopt a stance that has little historical or philosophical validity. To construe the nature of the problem <u>in this way</u> is to behave like someone who accepts, to whatever degree of reliability, an inductivist method of discovery, or at the very least believes that facts assemble themselves, atheoretically, into explanations. No-one in philosophy of science believes this any more, it is philosophically discredited and if one examines the seventeenth century one finds that plenty of people, even Newton himself, thought that the connection between discovery and verification was problematic. As we have seen, the absence of an algorithm for getting a purchase on the truth does <u>not</u> entail that a progressive history of scientific knowledge is impossible: rather it entails that a totally infallible methodology for the pursuit of such knowledge is impossible. Consequently, an argument might still be made asserting that a progressive history of scientific knowledge is <u>possible</u>. Then the way to proceed would be to employ a thoroughly imaginative, inquiring and critical attitude, which would encompass different world views, or theories, or entities concerning the specific matter in hand, and then try to decide which of them does the best justice to the historical record. Indeed it might even be argued that the rigid prohibition of twentieth-century ideas and concepts produces the very effect that it is designed to prevent - the impossibility of seeing the past in its own terms. As historians unavoidably working in the twentieth-century the best way to 'lose' our modern notions might not be to hedge them about with methodological devices at all but to talk about them and be critical about them. There may well be occasions when, as historians, we <u>need</u> to refer to current ideas in order to make sure we are not foisting them onto the past: the twentieth-century perspective may be required to highlight differences between 'then' and 'now', to show that current ideas are different from historical ideas. And it need not end there. If consensus-building, of whatever complexion, <u>does</u> lead to the denial of subject areas and their legitimately related historiographies, then the kind of critical enquiry that uncovers this could be extended to show that critical enquiry is, in fact, the function of history. In place of the consensus, why not enlarge our understanding of the scientific past by constructing a range of competing perspectives, social, intellectual, or cultural, so that it becomes, in principle, possible to evaluate their relative strengths? Real illumination might result from the clash of interpretations. Surely it is possible that one of the things which being a historian might entail can be a critical stance towards any perspectives, attitudes, values, or theoretical viewpoints, be they explicitly employed or implicitly involved?

What *IS* Whiggish About Whig History?

In any consideration of the problems inimical to present-day social history of science, this is a question that cannot be ignored. Historians, even those who may acknowledge that there are problems associated with current methodological strategies, such that they cannot on their own provide secure justification for curtailing some areas of study, nevertheless may continue to feel that they are justified in their methodological decisions just as long as the term 'Whiggish' can be applied. Ideas of progress and present-centred perspectives will always be open to accusations of Whiggishness unless and until the true heart of Whig history is dissected for all to see. As I mentioned earlier, criticisms of Whig history crop up all over the place, in the mouths of historians, sociologists of scientific knowledge, and philosophers. Unfortunately, it has been used as a term of historiographical criticism for so long now (Butterfield's *The Whig Interpretation of History* was published in 1931) that there is a widespread, implicit acceptance of Butterfield's definition and only an intermittent debate about whether it is accurate. Such reasoned discussion as there has been has only occurred since Butterfield's death.[38] When, in 1986, Latour wrote that Whig history is,

> **a history that crowns the winners, calling them the best and the brightest and which says the losers like Blondlot lost simply <u>because</u> they were wrong,** [65]

he exemplified this general tendency to use the terminology uncritically.[39]

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[38] **There was no real debate on this subject during Butterfield's lifetime.**

[39] **For example, David Goodman defines it as** *'that old-fashioned and discredited Whig historiography, which used the past to glorify the present'*, **Goodman D. and Russell, C. A.,** *The Rise of Scientific Europe*, 1. **Cunningham and Williams write of,** *'specific 'Whiggish' or 'present-centred' traditions of the history of science which our own generation of historians have inherited, and from which we have been trying to move away for some twenty-five years.'*, **Cunningham, A. and Williams, P.,** 'De-centring the 'big picture', 427.

Ashplant and Wilson, in an attempt to produce a more careful definition of Whig history and a non-Whiggish historiographical method, argue that Butterfield succeeded in grafting the phrase Whig history into the language of professional historians because,

> ***The whig interpretation* offered a ready-made answer to certain historiographic issues, and thus helped historians to avoid "reflecting at length" on those issues....the acceptance of Butterfield's terminology effectively blocked further discussion and debate.** [85]

Having decided that Butterfield's 'whig fallacy' is but special case of the real problem, defined as the unavoidable location of the historian within the perceptual and conceptual categories of the present, they concentrate on providing a methodology which will allow the historians' questions and the 'relics' of the past to make a meaningful engagement. The method, which they term a 'source-generating process', works by means of an explicit investigation of the *modus operandi* by which the historical source under examination was generated. In practice this means that the object or area to be studied undergoes a short-lived but determinate switch from a given set of substantive questions to the nature of the process which generated that body of relics which, the investigator believes, could function as sources to provide answers to the original questions. This is said to obviate the danger of simply trying to extract history from the sources.

> **Thus the historian's initial questions provide the motive force which animates the enquiry. That dimension of historical research which investigates the source-generating process provides the means whereby those substantive questions can be so modified, away from their initial present-centredness, as to mesh with the past through the relics it has left.** [86]

The source-generating process is seen by its originators as, *'a different framework of historiographic critique, and above all a framework of historiographical method.'*,[87] precisely because it *'tackles directly the present-centredness of the historian's observing position'*.[88] Butterfield's mere 'sensibility towards the past' as being something to be studied for its own sake, is contrasted with A. R. Hall's alternative 'sensibility'.

> **The need in the human consciousness to search for ancestry and continuity is deep. ...The question is put, and the questioner will find an answer somewhere; if academic historians are silent he or she will seek an answer in other ways. ... Academia does not exist solely for the sake of delighting and gratifying itself.** [89]

Neither constitutes a framework because neither addresses the problem of the structural position of the historian in studying the past.

Ashplant and Wilson are careful to point out that their new framework of historiographical method does raise problems. Although they argue that Butterfield's stress on value judgements was misplaced (demonstrating that Whig history emerges as long as the structural standpoint is present-centred but that the evaluative standpoint does not have to be 'present-favouring'), they admit that the relationship between value judgements and present-centredness *'remains to be systematically explored'*.[90] Secondly they admit that there are many different ways of construing the generation of a source and use the example of Newton's *Principia*.

> **One historian may see the work as the product of Newton's autonomous ego: another, as the expression of Newton's unconscious psychic contradictions; a third, as the result of the political and religious struggles of the 1680s. Each such choice could conform perfectly well to the methodological prescriptions we have advanced. What, if any, principle of selectivity could there be among those various approaches?** [91]

Thirdly, they admit that although they have been concerned to stress the present as a source of anachronistic misunderstandings of the past, there is still the question of what role the present does play in the past. The historian, whose point of vantage on the past is unavoidably in the here and now, is also,

> **necessarily faced with a choice as to which aspects of the past to study, which people in the past to bring alive, what dimension in the past to "go out and meet". It is only after that choice has been made, and a preliminary object-of-study constituted, that our injunction to investigate the source-generating process comes into play. And there too we are immediately confronted with another choice, of which approach to adopt. Moreover, the making of these choices will be based upon assumptions of the present.** [92]

Ashplant and Wilson criticise Butterfield effectively and offer some interesting prescriptions as to how present and past might be made to meet in non-anachronistic ways. What they offer is designed to make it possible to get a purchase on something that is heavily contextualized <u>without</u> disturbing the context. However, it offers no means of producing a non-Whiggish progress history. Any attempt to construct an argument for progress history must, it seems, focus on modifying substantive questions so as to mesh with a past that consists of a long time-span, not just something localized and short-term as perceived by Ashplant and Wilson. It is hard to see how this might be accomplished when, on their own admission, the *'process which makes it possible for the historian's questions to connect with the real past'* [93] can hardly be implemented before other problems, to which they offer no solutions, have been solved. Although source-generation is intended to connect the historian's questions with the real past, it does not provide a legitimate role for value judgements and it does not provide principles of selectivity to direct the choice of subject area, nor to decide which is the best approach to adopt, nor to delineate which aspects of that area are the ones to study. Ashplant and Wilson don't seem to see this for the problem it really is. How is it even possible to <u>talk</u> about principles of selectivity if there is no recognised way of using value judgements: such principles depend upon value judgements, they can only function from an evaluative standpoint. Such problems are compounded when the case of progress history is considered because another level of value judgements and principles of selectivity is required if the historian is to be able to move legitimately from source to succeeding source. To try and construct a progress history of scientific ideas would require the use of value judgements and principles of selection (which have no legitimate function within the schema) to enable the historian to decide which sources form a series and should therefore have their mode of generation elucidated. How might a perfectly generated source go one to become part of the genesis of another source? This would be a necessary step but, even supposing it could be rendered legitimate within the process, it would not suffice. The explicit investigation of the process by which a given historical source was generated does not offer any tools to investigate how <u>concepts</u> within that source might project forward to a later time. The concepts within a source are defined by the source: context is all and nothing is timeless. This is discrete, compartmentalized history.

This outcome, however, suggests another way of attacking the problem, which is to look at it from the other side and try to understand what a totally anti-Whig position can entail. The important point to make is that if historians jettison absolutely <u>everything</u> that has been associated with Whig history, however loosely, erroneously, or uncritically that association may have been made, all that they are left with, as Ashplant's and Wilson's methodological prescriptions imply, is a very localized, synchronic approach. In this static sort of historiography any changes that take place are only going to occur over a reasonably short time. *Leviathan and the Air Pump* exemplifies this with its examination of the Boylian form of life. The refusal to engage with change over time is not confined to questions of knowledge and rationality because, if Ashplant and Wilson are typical, there seems to be little enthusiasm for doing belief history on a larger time-scale either. What the authors of *Leviathan* do towards the end of the book is to suggest the Latour model as a resource for how one might cope with change and development over time. This entails such social constructs as markets, enrolling interests, and power and the answer to the question why the Royal Society programme became plausible is given in terms of the success of the Society in convincing the public of its utility. The Fellows presented themselves as being competent to solve all problems, ranging from the agricultural to the theological, and thus, in Latour's terminology, enrolled a great many people as allies.[94] The danger of being confined to this sort of localized approach has certainly been recognised as a problem. Andrew Cunningham's and Perry William's paper 'Decentering the "big picture": the *Origins of Modern Science* and the modern origins of science', for example, addresses the need for a historiography to deal with change and development over time. However, they argue that any,

> **new big picture, a general history of ways-of-knowing-the-world across the whole of human history would have to be the history of many different things, rather than of one single thing at different stages of development.** [95]

The origins of science, they argue, must not be thought of as being derived from,

> **some transcendent realm, but** [rather thought of as being] **as a result of particular human activity in response to the local conditions of material life.** [96]

Most significantly of all, from my point of view, is their belief that any new 'big picture' must ensure that it does not *'privilege one particular kind of knowledge'*. [97] The difficulty of constructing a progress history of scientific ideas remains.

In support for my contention that many of the methodological prescriptions of present day history of science function to deny the very legitimacy of a progressive history of scientific ideas, I have found one eminent historian of science who has thought along similar lines. A. R. Hall has argued that the synchronic approach is not only problematic in itself but also denies the possibility of any kind of diachronic history.[98] He has already been mentioned, arguing for the legitimacy of continuous, linear history: Ashplant and Wilson did not think that A. R. Hall's historical 'sensibilities' amounted to a historiographical method but it is nevertheless worth examining what he says. Hall is convinced that a certain degree of Whiggish perspective is unavoidable if we are not to be left in the position of denying the legitimacy of certain areas of study. He urges against historians arguing themselves into the sort of corner where they can only recognise the validity of what he calls 'cross-sectional' history. In such history,

> **ramifications present themselves in endless rows; in what we would now call natural science alone, vital heat relates to all other aspects of heat and all other conceptualization of life, to chemistry and physics in open-ended perspectives, to the theory of matter and correlated views of dynamism in the universe, to the composition of the atmosphere and pneumatics, to animal physiology and nutrition, to disease and the study of medicine in general (since abnormal manifestations of vital heat constitute a pathological condition) in short, pick up any problem, such as vital heat, and the historian is ineluctably led to every other contemporary problem, not forgetting those of methodology and philosophy.**[99]

Hall wishes to establish the credentials of 'linear' intellectual history of science, which he defines as the artificial isolation, from its intellectual and social context, of an idea which is then given a *'coherence and identification through time'* .[100] He is at pains to point out that a certain amount of what is generally thought of as Whiggism is not merely permissible but actually unavoidable in such an endeavour. For example, the historian of science is as entitled to reinterpret a document in terms of later chemical or mathematical analyses as is a philologist to reconstruct a dead language, or an archaeologist to recreate on the computer screen a largely non-existent building. *'History'*, he declares, *'must sometimes step out of context'*. The questions which the historian considers to be significant vary through time, they will not always be contextual questions. Moreover, for an historian to formulate all possible questions at a given time, let alone undertake the task of answering them, would be impossible. Moreover, the history of science presents particular difficulties of its own: if an historian knows less science than the object of the historical study, then it is hard to see how that subject's researches can be properly followed; if the historian knows more, then the problem arises of how to 'forget' what the subject did not know. So by what principles of selectivity are questions formulated? Hall asserts that a contrast between Whiggish linear history and a-Whiggish cross-sectional history demonstrates that the latter is *'no more free from conscious or unconscious tendentiousness'* than the former.[101] The data chosen for both,

> **have to be chosen according to some pattern. For in either case the possible range of data is infinite; selection must be made, and since it would be absurd for selection to be random, it must be made in accord with certain principles, that is a pattern.** [102]

In this he agrees with Ashplant's and Wilson's conclusion that,

> **all historiography rests upon acts of choice. No historiography can ever be a neutral enterprise: an enquiry into any given aspect of the past necessarily derives from some evaluation in the present.** [103]

It is impossible to liberate oneself from one's own age, or the standards that characterize it, and that will be reflected in the choice of subject matter and relevant problems whatever historiography one chooses to pursue. Hall concludes that these aspects of Whiggism are a necessary part of linear history (as of all history) but we may infer that he does not regard it as also sufficient because he goes on to say,

> **if linear history is to be written at all, as I believe it must and should be, then it must be composed with all the scholarly care and sympathy for the remote periods and peoples with which the cross-sectional historian may also work in his way. If (as Butterfield maintained) the objectives of cross-sectional historical study are attainable with some degree of strictness, and those of linear history only much more loosely and speculatively, nevertheless the reader is entitled to expect that the linear history be written as strictly, its speculations be as well founded, and the search for causes and explanations be as prudent, as the limitations of human intellect and endeavour will allow.** [104]

What are we to make of this? Although Ashplant and Wilson wrote their papers partly in response to Hall's, in many ways his paper gives the impression of being an advance on their paper. Perhaps, to an extent, they were merely elucidating what was to a degree already considered to be the best methodological practice in history but what comes out of their collaboration is a set of methodological tools designed to bring the present into a non-Whiggish relationship with the past. The aim is to grasp historical contexts without in any way disturbing them but, as will become clear, the methodology is one in which the necessary value judgements and principles of selectivity have no legitimate place and the possibility of a linear progressive history is denied. Hall, we may assume, feels that this compulsion to eliminate all bias is positively Baconian and unattainable. I think his (temporally prior) arguments represent an advance in that they do not proscribe, by methodological fiat, certain areas of historical study and in addition to considering the difficulties attendant upon making the questions of 'now' connect meaningfully with 'then' they are also prepared to tackle the much more involved question of linear, continuous history. Firstly, value judgements and principles of selectivity are deemed necessary in order to be able to do history, whether localized or linear: therefore on this count all sorts of different types of history, or approaches to the past, are legitimate. It is not, for Hall, a matter of finding just one historiographical framework that is guaranteed to produce a non-Whiggish history but a matter of finding an appropriate methodological framework for the kind of history you wish to do. These are important topics that I shall be returning to shortly. Secondly, Hall is arguing that there are undesirable features connected with Whig history that can be banished, through the medium of well-founded arguments, by a strict and prudent historian who is interested in formulating a methodology for the doing of linear history. This in turn would imply that some aspects of what has traditionally been identified as Whiggish have been mis-attributed and are, in fact, perfectly acceptable in the construction of a history which traces the progress of something through time. It could be that value judgements and principles of selectivity have been thus wrongly defined. It could be that, in certain circumstances, the twentieth-century perspective could be used to highlight differences between current ideas and historical ideas. Hall does not seek to establish these arguments but he opens up a line of inquiry that is worth considering.

I believe that it is now the time to try and put together a putative summary of the entities that comprise the unacceptable face of Whig history. I think it is reasonable to define it as a crude, present-minded perspective with readily-identifiable faults. One such fault is the evaluation of the knowledge of the past as though it dealt with precisely the same subject and concepts that we think it is 'really' about today. A second fault lies in the formalization into (usually) chemical and mathematical terms of statements which were not merely expressed differently but, in their original form, involved conceptual contents that are too far removed from those of modernized translations. Aristotle's law of motion is a case in point.[40] A third fault is the imposition of modern notions of coherence and rationality on the thoughts of historical agents, so that their assumptions are examined with the benefit of the sort of hindsight that not merely fixes the aim, or goal, of science in the present, but tries to demonstrate that historical agents must have, at least partially, articulated it, or understood it, in the same sort of form. Karin Figala, for example, has argued that Newton's ideas, *'Show ... surprising resemblances with the atomic shell model proposed by N. Bohr'* and that Newton himself seems to have suspected that there are metals which have greater density and are more precious than gold.[105] The Whiggishness in this is clear to see: platinum, iridium and osmium were only discovered much later and Newton could have had no concept of either compound atoms resembling planetary systems, or of elements in any post-Daltonian sense of the word.[41] A fourth significant fault is that of anticipation, the kind of interpretation which, in trying to elucidate connections between an alleged forerunner and a later doctrine, ascribes clairvoyant abilities to predecessors, or projects later theories back onto them.[42] All of these characteristics, of course, involve the notion of progress and, simultaneously, serve to undermine that notion by association.

I think few would disagree that any historiography cast in this form will undoubtedly rationalize any attempt at linear history and thereby distort it into a triumphal progress. Such a historiography will display all the faults traditionally associated with Whig history, resulting in serious anachronisms that twist the historical record beyond recognition. I therefore maintain that these four areas constitute the true heart of Whig history and should be avoided at all costs. An interpretation can be said to be Whiggish when contemporary subjects, concepts, techniques, and methods are imposed on the past in the manner just

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[40] **Aristotle's law of motion states that a body moves because it is under the influence of a motive force (*F*). The speed (*v*) is proportional to the force and inversely proportional to the friction (*R*) between the body and the medium in which it moves. This is expressed as $v = k \ F/R$, where *k* is a constant. However, the mathematical form, even the very idea that motion might be expressed quantitively, was foreign to Aristotelian science, and the terms contained in the law refer to the knowledge and concepts of a later age, as did the concept of natural law. The conceptual contents of Aristotelian science are too far removed from those of the modern 'conversions'.**

[41] **Kragh has argued that anachronism creeps in when clarity is judged by modern standards but that the clarification of obscure passages is perfectly acceptable when it can be justified by means of independent evidence. The possibility of the text's being inconsistent and incoherent remains. See, Kragh, H.,** *An Introduction to the History of Science*, Cambridge, Cambridge University Press, 1987, 100.

[42] **Sandler has argued that the concept of anticipation is always context-dependent, that scientists and historians will necessarily interpret it differently because there is no one absolute criterion which always applies when theory X is said to anticipate theory Y. See, Sandler, I.,** 'Some reflections on the protean nature of the scientific precursor', *Hist. Sci.* 17, 1979, 179-190.

described and held to be a legitimate means of linking past and present. This sort of imposition, however, does not negate the possibility of progressive histories as such, even those devised within the framework of present-day perspectives, but it does require of any historiography venturing into such areas that its parameters be carefully defined and that particular care be taken over its execution. A Whig history is necessarily a history of progress but that does not entail that the reverse is automatically the case.

A Possible Route Towards a Solution

Perhaps the best way to introduce this topic is to make the (perhaps contentious) assertion that historians of science, among whose ranks I number myself, generally have a mediocre understanding of philosophy of science. If one concentrates not so much on the content of what we say and write and more upon the implications, or presuppositions contained therein, it becomes evident that historians of science do not always consider philosophical questions rigorously. This is perhaps not so surprising, given that philosophy of science has had such a negative influence on the development of present-day history of science and sociology of scientific knowledge. The actual attitude of historians to philosophy of science varies from those who believe it to be an inappropriate historiography (history and philosophy of science are two separate disciplines), to those who believe it to be harmful (philosophical considerations will necessarily bias history and lead to crude anachronisms), to those who believe it to be irrelevant to everything, including science itself. Unfortunately, however, some philosophical judgements still have to be made and historians of science are in danger of making them badly, if they do not refine their philosophical interests and skills. I mentioned earlier the tendency to believe that a choice has to be made between methodological relativism, or the debate concerning the degree to which nature has a hand in constructing knowledge about itself. To embrace the latter, as we have seen, entails that anyone addressing the question of how far concrete facts might assemble themselves a-theoretically into explanations is taking for granted the existence of a reliable inductivist method of discovery. Academic philosophy of science has long since abandoned pretensions to any such thing. Latour's earlier cited quote about Blondlot, in which he maintains that *'Nature herself discriminates between the bad guys and the good guys'* partakes of this error and falls into the further one of believing that the truth is manifest. Popper argued a long time ago, in *Conjectures and Refutations*, that the truth is not manifest: it can be stumbled upon through following a conventionally 'bad' scientific path, like that of phlogiston theory, because

> **As we learn from our mistakes our knowledge grows, even though we may never know - that is, know for certain. Since our knowledge can grow, there can be no reason here for despair of reason. And since we can never know for certain, there can be no authority here for any claim to authority, for conceit over our knowledge, or for smugness.** [106]

Implicit in social history of science practice is the idea that those who operate frameworks designed to deal with the concept of the progress of scientific ideas must do so because they believe that when a contribution to science is considered to be true, or rational, or justified, or acceptable today, this suffices to explain its acceptance at the time. The above quote from Popper shows that this was wide of the mark as long ago as 1962. There is also a widespread belief that reliable knowledge equals absolutely certain knowledge: the argument which Shapin summarized as *'truth is one and what people have taken to be true is many'*,[41] reflects this in its assumption that academic philosophy's view is that questions of truth are the same as questions of knowledge of truth, which is not at all the case. Wallis also partakes of this error with his argument that scientific truths have to be established with absolute certainty if they are to count as such but they can't be so established because of the absence of logically watertight grounds for any set of beliefs. Another prevalent persuasion is that if there is such a thing as rationality it can only exist in the form of an infallible algorithm: T. S. Kuhn, for example, demonstrated that such an algorimth had never been found, therefore, they conclude, rationality cannot exist. The possibilities inherent in the fact that Kuhn's explanation was not that rationality was defunct but rather that, *'existing theories of rationality are not quite right'* seem to have been largely ignored.[107] Latour certainly believes that the concepts of rationality and scientific method are fixed and immutable, and therefore untenable. Pickering holds that the failure to produce a satisfactory logic of science renders concepts like rationality, objectivity, and scientific realism quite meaningless. Dear openly bases his approach on the assertion that *'notions of the rational have themselves become problematic in recent years.'* [40]

Attempts to produce alternative definitions of rationality, or explain that the mechanism whereby it is produced is rather different to what has been previously imagined, are largely ignored, it seems.[43] It might be concluded that those who explicitly accept methodological relativism as an alternative to the debate concerning the degree to which nature has a hand in constructing knowledge about itself, do so deliberately in order to avoid having to grapple with such philosophical problems. I think this would be a fair assessment of their intentions. However, it is also manifestly clear that they fail in their ambition. The examples considered in the section entitled **A Closer Look: Social History of Science Over Twenty-Five Years** are riddled with the kind of half-baked, even bad philosophical assumptions just considered. Philosophy of science has long since moved on from such ideas and the best way for historians to ensure that they make their few philosophical judgements correctly is not to ignore the discipline but to keep abreast of its developments. For a final example it might be argued that if philosophy of science has not yet explained how and why science is a rational and progressive enterprise, that does not entail that it is not. Indeed one does not have to await the invention of such a philosophy of science to see that this stance is flawed. Just as naive falsificationists were criticised for believing that once falsified meant always falsified, so social historians of science can be criticised for falling into a similar trap in believing that all philosophies of science have been discredited, as historiographies, for all time.

I think that two very important questions need to be articulated and made separate, for I believe at present they are imperfectly expressed and frequently conflated. The first one demonstrates the importance of not ignoring philosophy of science because it takes the form of a philosophical debate: 'is there such a thing as knowledge, or not?'. This is not a question that can be settled by doing history. All that history can do is improve historical knowledge about something which is being presupposed. The fundamental point is, does the subject matter exist: is it legitimate to take the idea of knowledge seriously? The scope and limits of knowledge is a very traditional philosophical question and part of the job of

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[43] **For example, historians of science don't seem to have grasped the implications for their discipline contained in Nick Maxwell's arguments about there being an historical dimension to rationality. See** *From Knowledge to Wisdom*, Oxford: Blackwell, 1984 **and** *The Comprehensibility of the Universe*, Oxford: Oxford University Press, 1998.

academic philosophy and philosophy of science is to say something about it.[44] It has never been the concern of historians of science to argue about problems of knowledge. For example, an age-old epistemological concern has been the problem of how we can get certain knowledge. Popper came up with an answer to this decades ago when he argued that we should be more concerned with the growth of knowledge in the sense of increasing it, rather than with verifying it to the point of certainty. None of these considerations have ever been a part of the historian's remit but it nevertheless behoves the historian of science to be critically aware of them - and of any new developments in them - in case they might be relevant to decisions and judgements the historian might make. The historian, of course, feels on safe ground in assuming that philosophy of science has failed to explain how and why we can have, <u>and</u> progressively increase, our explanatory knowledge of the natural world - and how and why such an enterprise is rational - and therefore assumes that there is no explanation. However, the failure of present-day philosophy of science (if indeed all philosophies of science have failed in this respect) <u>does not mean</u> that there is no such thing as a scientific knowledge that is characterized by entities such as rationality and progress. It is also perfectly possible to develop Popper and argue, as Nick Maxwell has done,[108] that if we have a methodology that shows signs of being empirically progressive in terms of existing criteria about what constitutes knowledge, then this is a good sign we are thinking about the world in the right way. Our ability to improve our knowledge, or have a growth of knowledge in that sense, even though ultimately all knowledge is conjectural, is itself a sign that we should place confidence in the process. Confidence comes through growth rather than growth being an additional matter which one does not consider when talking about certainty. Formulating a proposition and proving it may work in logic but it never has in science. Once again, I must emphasize, <u>these are not historical judgements</u>. However, historians should be aware of such possibilities and avoid making hasty or selective judgements which necessarily involve disciplines other than their own: this is especially vital when it involves them in formulating historiographies in the belief that certain, possibly localized, failures are universal and timeless.

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[44] **As indeed it does in the ongoing debate about instrumentalism versus realism.**

The second question simply concerns a matter of choice: given that the subject matter exists then we can move from demonstrating that arguments for its non-existence are unacceptable to the position where, just as general history recognises that there are histories of many different things, so history of science has to recognise that a perfectly respectable sort of history, which historians of science can choose to do, may be the history of people's long-term attempts to improve knowledge about the natural world. It is simply in the nature of some topics to be concerned with progress. Once this point has been reached the decisions to be taken and the judgements to be made are of a historical or historiographical nature: chief among them is the need to formulate a suitable methodology. I hereby offer, as a working hypothesis to be examined shortly, the notion that a progressive history, including a progressive history of science, is simply a way of describing people's past efforts to achieve some kind of goal: a 'goal-seeking endeavour' is something which can be explained and understood in terms of the successes and failures of those actions which the historian takes to be involved in bringing the endeavour closer to its goal. It does not require that goals be specifically articulated (or imply that they must, necessarily, have been so articulated) and it does not expect that progress must have occurred. These are precisely the things that must not be dictated by methodological fiat but must be left open to investigation.

Generalized Arguments for the Existence of the Subject Matter

There are some very generalized things that can immediately - and with no particular depth of analysis - be said in favour of the existence of the subject matter, which is the question of whether there is such a thing as knowledge and whether or not it can be progressively improved. Indeed, the fundamental point is that the aim, at least, of doing a history of scientific progress is valid just as long as scientific progress is possible. One does not have to assume that it has happened, only that it is possible.[45] Right at the beginning of this chapter I made two assertions. The first was that it is surely still legitimate to take as a fundamental problem for the history of science the question, 'how has science made

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[45] **At this stage of the argument nothing depends on what is actually meant by terms such as 'knowledge' and 'progress'.**

progress?'. The second was that, in such a case, was it not possible to appeal to such entities as truth, reason, rationality, and progress merely in order to state the problems to be addressed, without being drawn into the justification of solutions? At long last it is time to fully justify these two assertions. Of course ultimately the view that we have no knowledge at all about the world is untenable on logical grounds. For logical reasons, even our false beliefs carry within them some truth.[46] With regard to actually developing the sort of knowledge that would be positively useful, however, Popper was correct to say that scepticism has a circumscribed role to play in its generation and development. Nicholas Maxwell has promoted and augmented this, arguing that, just conceivably, radical scepticism about theoretical scientific knowledge might contribute to the improvement of knowledge because theoretical scientific knowledge might be so radically wrong as to not constitute knowledge at all. It is possible that we could discover such a thing and yet continue to live and improve our knowledge just as we did in pre-scientific cultures.[109] Human life is, after all, possible without scientific knowledge. However, if we apply radical scepticism to all our everyday common sense knowledge about our immediate environment, the situation is very different. It is rational to doubt some of our common sense knowledge but to doubt all of it deprives us of the possibility of action, of living, and of updating and improving our common sense knowledge as we live. Hence we are rationally entitled to hold that we do possess some factual knowledge of particular objects and states of affairs in our immediate environment.

What this is leading to is the assertion that there is an inescapable link between all forms of knowledge. Methodological relativists try to deny this link insofar as they undertake to jettison knowledge and its associated entities such as progress, rationality, and knowledge when they are dealing with the history of science, and yet are happy to retain common sense notions of knowledge, progress, and rationality in everyday life. Indeed if they did not retain them it would be very difficult to make sense of those methodological strategies, which have been contrived so as to make sure that such notions do not

[46] Any false proposition will have some true consequences. My belief that I am a human being implies that I am not a hippopotamus or a butterfly. Possibly I am wrong about being a human being, but I am not wrong about not being a hippopotamus (or if I am wrong about that, I am not wrong about not being a butterfly).

contaminate their investigations of the past. I aim to show that the main problem behind what is, in effect a negative input from the philosophy of science (which manifests itself as a ban on so many concepts and ideas) is the traditional problem of the rationality of science, the problem of induction. This is the problem of how we can have knowledge of universal laws and theories granted that we only have knowledge of particular matters of fact. It can take the form of the practical problem of induction and the theoretical problem of induction. These are both problems of justification: the theoretical form needs to justify accepting empirically successful theories, granted that the intention is to secure theoretical knowledge, and the practical form needs to justify acknowledging such theories, admitting that the intention is to utilize the theories for action. There is also, as will be revealed in Chapter Two, a methodological problem of induction. This is not a problem of justification: it is not asking 'how is scientific method to be justified?' but is asking what scientific method consists of.[47] The relative importance of these various facets of the problem of induction will be dealt with later: for now, there are some general points to be made about the existence of the problem and the implications which it has for there being a link between all forms of knowledge.

The first thing to be said about it is that it tallies with what I have just asserted with regard to common sense knowledge and the methodological strategies of the relativists: if you recognise, even if only implicitly, the existence of the problem of induction then you are also assuming that, in spite of scepticism, we do have knowledge of particular matters of (scientific or other) fact, based on experience. If, furthermore, you hold that we must not take that (distorting) knowledge of particular matters of fact into the past, it being impossible to connect ideas of what counted as knowledge then with ideas of what counts as knowledge now (because there is no recognised universal and timeless standard of rationality), you are again acknowledging the problem of induction. **SSK** and social history of science have argued that scientific knowledge is only as fixed as it is taken to be. They

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[47] Most of the literature on the problem of induction tends to home in on the problems concerning justification assuming, by implication, that the nature of scientific method is not problematic. Yet it is well known that there are difficulties associated with formulating and using methods, for example how is it possible to designate methods that give precedence to simple theories when specifying what simplicity is has proved so difficult. How and why can it be rational to accept that there is an unchanging entity called 'the scientific method', when methods appear to change from time to time and even from one branch of science to another?

assert that in any given period or context scientific knowledge is taken on trust, trustworthiness being seen as a necessary, although historically and contextually variable, constituent in its development and maintenance. Thus, there is nothing to be gained by trying to take it into the different contexts of the past because there is no way of linking up those contexts. However, if present knowledge is considered to be every bit as contextual as past knowledge, the lack of a universal and timeless standard of rationality is still implied: social history of science is merely utilizing contexts to explain why this might be. The second thing to be said about the problem of induction is that it is just as much a problem of common sense or practical knowledge (of the kind which helps us to function in our environment) as it is of scientific knowledge. It is ubiquitous and if we do not learn how to solve it, or accommodate it, we can have neither reliable scientific knowledge, nor a reliable basis for practical action in our everyday lives! Social historians of science who hold that the science of any age can only be viewed contextually, and who employ tools and strategies to ensure that they don't import undesirable concepts and notions into the past, must believe that those concepts reside, or function, somewhere. If they are true to the demands of contextuality they should regard notions of scientific rationality and progress as being what the modern scientific consensus holds them to be. As such, they would not translate into past contexts. Therefore the most likely place for these notions is in common sense knowledge. Consequently, it is necessary to appreciate that we cannot just give up scientific knowledge in this way (and with it all hope of ever studying its historical development) in the expectation that that will produce a bias-free history of science, without accepting that we either have to give up common sense knowledge as well, or acknowledge that what we are doing is likely to have quite as many drawbacks as advantages - perhaps more because it introduces an artificial break between legitimately linked forms of knowledge.

The simple answer is that the problem of induction, contingent as it is upon our only ever having knowledge of particular matters of fact, applies equally to all forms of 'scientific' knowledge, right down to the simple living strategies and problem-solving technologies of primitive peoples. It is perfectly true that scientific knowledge and philosophical knowledge are not the same as common sense knowledge - indeed scientific knowledge is often held to be counter-intuitive[110] - and plenty of people would argue that technology is more than just applied science. However, is it possible to hold scientific knowledge (massively) on trust, on the grounds that there is no universal standard of rationality, without holding all knowledge (massively) on trust? Is it possible to maintain a belief in entities like progress and rationality in common sense knowledge, whilst holding that the scientific knowledge of any age, including the present, is only as fixed as the proper, contextually-based consensus allows? Grant that social historians of science are right, and we can only take scientific knowledge on trust: haven't we been taking some things on trust for a very, very long time, so that they have passed through context after context? Maxwell has argued that in <u>all</u> contexts, whenever a law or theory is properly judged to be well-founded, it is inevitable that it will be grounded in more general theoretical, metaphysical, or cosmological suppositions which warrant the rejection of infinitely many, empirically successful, aberrant laws or theories.[111] In the realm of the practical and non-scientific - perhaps in cases where technology and its utilization has developed as practical responses to problems, which was so frequently the case during pre-industrialization - the implicit and underlying suppositions may be, or may at some future date prove to be, some relevant part of scientific knowledge. When Galileo worked out the mathematical analysis of the motion of projectiles he demonstrated the necessary truth of something already known to generations of artillerymen - that the optimum angle of elevation is 45°. In the realm of science outside physics, the social, biological, or chemical, the implicit supposition may still be a part of physics. Where physics deals with the description and classification of phenomena the lurking supposition may well reside in more fundamental theoretical physics. It is only at the level of fundamental theoretical physics that this business of appealing to some area of established scientific knowledge is no longer possible and the inevitable problem of induction must be confronted. It is in this way that almost all scientific knowledge is 'like' everyday practical, common sense knowledge. At all levels metaphysical or cosmological suppositions are there: appealing to established scientific knowledge that can be 'trusted' merely avoids having to confront them. In the light of this the methodological tools and contextual practices adopted by historians to avoid bias at all costs seem harder than ever to justify. What they are doing is (albeit implicitly) identifying the problem of induction, the traditional problem of the rationality of science, as a primary justification for such strategies. However, one feels that they ought at least to admit that by denying that so many forms of knowledge are linked, they are promoting an historiography that involves artificial divisions in the concept of knowledge.

The next thing to be said is that if we do have some common sense knowledge, some genuine factual knowledge of particular things in our immediate environment, <u>which we must be implicitly assuming if we take seriously the problem of induction</u>, we also have at least the possibility of a way around the problem of induction.[112] If we have some genuine piece of common sense knowledge in this sense, we also have some cosmological knowledge about everything, albeit very vague and generalised. A humble statement such as, 'The paperweight on the desk is a lump of slate', carries the implication that the rest of the universe is such that it permits slate in general, and this lump of slate in particular, to exist. Maxwell has argued that cosmological assumptions are implicit in knowledge itself, in any item of factual knowledge whatsoever, however lowly and limited in scope.[113] Moreover, it is not simply the mere existence of knowledge that has a cosmological dimension. Any adequate knowledge - adequate, that is, as a basis for human life - must implicitly contain knowledge of some universal laws. In theoretical physics and in everyday examples of technology we recognise law-like behaviour all the time. It may well be imperfect in that it perhaps only applies to a very limited environment: for example primitive man would have expected all dry wood to burn, having no idea that it will not do so in certain circumstances, such as when in a vacuum. Notwithstanding, such imperfect knowledge of universal laws remains very important because in a mutable environment, in which thing come into and pass out of existence, primitive man needed to be able to predict, with a good degree of accuracy, the properties or behaviour of newly-encountered entities. The assumption that it is - and will continue to be - possible to improve common sense knowledge to the extent of updating our knowledge about our immediate surroundings as we live (which is as true for us as it was for primitive man), also implies something about the nature of the cosmos: it assumes that the cosmos is such that, however chaotic it may be elsewhere, this chaos will not spread to our immediate environment to make it impossible for us to acquire knowledge of it. The universe must be so constituted that the acquisition of explanatory knowledge, sufficient to make adequately successful action possible, is feasible.

The failure of philosophy of science to solve the problem of induction (if indeed it has failed) has not revealed that there is no solution to the problem of induction and the foregoing arguments suggest that the search for one is not hopeless. Consequently, as I asserted at the beginning of this section, the aim of doing a history of how we have progressively increased our explanatory knowledge of the natural world remains a valid one as long as the possibility exists for us to possess and increase knowledge of particular matters of fact about the natural world. Once again, one does not even have to assume that such a thing has happened.

The final thing to consider with regard to whether or not the subject matter exists is the difference between knowledge and belief. Earlier in this chapter, in the section entitled **What Does This Mean for Histories of Scientific Progress**, I asserted that present-day history of science denies the legitimacy of alternative conceptual schemes, most particularly the legitimacy of a progressive intellectual history of science. This is particularly clear when we come to consider belief and knowledge because historians of science who consider the questions I have been addressing as irrelevant are, generally speaking, concerned with belief. They have turned to the study of belief because the study of knowledge appears to be untenable. From such a perspective it isn't clear what function concepts like knowledge, rationality etc. could have, or indeed what analytical work they could perform, in the history of science. They are seen to belong to 'flawed' and 'indefensible' history of scientific knowledge. Of course it might be argued that in history although such notions as rationality, objectivity, and truth are not possible, a notion of progress, at least, is: for example, Boyle and Newton had little in common in their general methodologies and yet it is almost intuitively obvious that both made great contributions to the knowledge of the late seventeenth century - it progressed as a result of their efforts.[48] However, this is really progress in, or the accumulation of, belief: seventeenth-century 'knowledge' is not being presented as having links with, or being part of the evolutionary development of, twentieth-century knowledge. In this sense, Dobb's book, *The Janus Faces of Genius*, is a history of progress, charting the development of Newton's ideas within the context of his attempt to

[48] **Although it has to be said that both Boyle and Newton were radical voluntarists and both tended to separate certain knowledge from probablistic knowledge, ie of causes.**

produce a grand unification of natural and divine principles, a contextual framework quite divorced from anything that would be recognizable to modern physics.[114]

The degree to which 'belief' history of science denies the legitimacy of 'knowledge' history of science becomes apparent <u>when knowledge is considered</u>.[49] Historians of science who deny any notion of rationality are left with a definition of progress as being that which the human race, at any given time, takes to be progress. In other words, they are left with belief history. Due to the refusal of social historians of science to grant a privileged status to scientific knowledge, or any one particular kind of knowledge, the intellectual entities that have been an indispensable adjunct to any understanding of scientific knowledge can have no place in belief history. Entities like scientific method and scientific rationality, objectivity, and progress are, indeed, not relevant to belief history. Notwithstanding, that does not entail that they are *per se* unnecessary and irrelevant to the history of science. The mere fact that belief history denies the legitimacy of alternative conceptual schemes does not, thereby, render such alternatives wholly inadmissible. I submit that in the case of scientific knowledge these entities are fundamental. They are not appealed to in order to justify solutions but because <u>they set the problems that the historian is interested in trying to solve</u>. Without them it is meaningless to be critical of science, either of the past or of the present. For example, it is impossible to discuss the progress of scientific knowledge without using notions of truth, rationality, method, etc. because all of these things stand, or fall, together. To deny rationality a role in the history of science is to remove all grounds for defining what is meant by progress in knowledge and for assessing whether or not it has occurred. <u>Some</u> notion of rationality is necessary to make this sort of assessment possible.[50] For reasons already documented and criticised, many historians of science try to keep entirely separate the differences between seventeenth-century perceptions and twentieth-century perceptions - and yet the very notion of <u>progress</u> depends upon understanding those differences and in understanding that, as a concept, progress is very often something which is <u>assembled</u>

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[49] Historians of science of a sociological persuasion and proponents of SSK would argue that, <u>of course</u>, they are interested in scientific knowledge: they simply take such knowledge to be whatever the contemporary scientific consensus takes it to be. They would probably further argue that such 'knowledge' has the advantage that it can be treated as publicly available behaviours, without reference to inner mental states. This, in fact, has become a relevant part not just of history of science but of all recent historiography.

[50] The nature of the relativist framework, it would appear, is ultimately self-refuting!

afterwards, in so far as that is possible. I therefore submit that it is inevitable that the historian who is concerned with formulating a methodology encapsulating a progressive history of scientific ideas must appeal, as a logical necessity, to some notion of rationality. It is important to emphasize the 'some', for how good or bad such a history of progress is will depend upon that definition of rationality. The point made above about the contributions made by Boyle and Newton to the progress of knowledge in the late seventeenth century implies a belief that the notion of rationality can have no role in the history of science because it is a fixed notion. The implication is that whatever contributes towards progress in knowledge must, by definition, be rational and the example of the different methodologies of Newton and Boyle can be invoked to make nonsense of such an argument. In an ideal scenario it might be demonstrated that progress of knowledge history has come to have a bad name precisely because it hinged upon an unsatisfactory definition of rationality. If that could be accomplished it might offer a means of forging a new conceptual scheme. However, for the moment I simply assert two things. One is that the entities traditionally associated with scientific knowledge are actually fundamental to the very definition of scientific knowledge and, as such, set the problems which historians interested in that sort of knowledge will want to answer. Consequently they cannot be dismissed as 'arbitrary definitions'. The second point is that any notion of progress is particularly dependent upon some notion of rationality: indeed, the extent to which an historian has a notion of progress marks the extent to which he or she has a notion of rationality. A part of what a progress-oriented historian wants to do is to try and understand something of how and why progress happens. It may happen at distinguishable points in time, or gradually, but pinpointing it involves questions of rationality.

There is also a third point to be made, this time concerning the relationship between progress and knowledge. As we will see later, there are grounds for holding that it does not make sense to say that we possess knowledge unless we can also improve the knowledge we possess. If knowledge is to exist, it does not suffice that there exist persisting marks that can be interpreted as true sentences; there must, at least, be beings who can act (or pursue goals) more or less successfully in the world, knowledge being implicit, at the very least, in their actions. Beings that are able to act more or less successfully in the world must be able to update their knowledge about their surroundings as they act; they must be able to improve their knowledge. Thus, we only truly possess knowledge if we can improve it in the sense, at least, of being able to acquire new knowledge. Popper was right when he argued that the fundamental problem of knowledge is the problem of the growth, or the improvement, of knowledge. Here is yet another argument in favour of the assertion that the entities which define knowledge also specify the nature of the kind of problems associated with knowledge. It is simply impossible to proceed without them.

Generalized Arguments Concerning the Pursuit of the Subject Matter

Once the legitimacy of the subject matter has been settled there are two questions to be sorted out. The first is, what are the advantages of doing a history of progress and the second is, what methodological decisions must be taken in order to produce a workable historiography? With regard to the first, perhaps the obvious answer is, "Well, because it is there and at present its legitimacy is being denied".[51] Once it is realised that academic history of science is, by methodological fiat, proscribing whole areas of historical study and when it becomes clear that their reasons for so doing, although well-intentioned, are not as well thought-out as they pretend to be, then it emerges that the whole question of the validity of progress history, for so long deemed to be worthless for a combination of complicated and in some cases inter-related reasons, is worthy of re-examination. A further important issue which is frequently denied consideration is the inter-relationship between past and present. Ashplant and Wilson draw attention to this when they define the role played by the present in understanding the past as an important research topic. However, they do not go far enough because a history of progress is not necessarily a uni-directional activity, moving triumphantly from 'then' to 'now': it could be an activity which works in two directions. There is an interplay between the present and the past - and between the past and the present - which, if handled sensitively, might ensure that each illuminates the other in revealing ways. These points have been excellently made by Helena Cronin. She argues firstly, that modern ideas about evolution, ideas which have emerged from the

[51] **Of course Shapin, for example, rather felt this way about** SSK **but perhaps did not debate how possible it was as an enterprise.**

debates of the past decade or so, provide a powerful tool to sharpen our understanding of the debate between Darwin and Wallace. The recent revolution in neo-Darwinism helps us to perceive strands in the earlier dispute that would not otherwise have been clarified. Secondly, she argues that historical insight can also help us to understand just why Darwin's theory is so explanatory, even though it is often compared unfavourably with the 'paradigmatic' theories of Newton and Einstein.[115] Both Cronin and John Maynard Smith, whom she quotes, understand that there is something about past/present interactions that is profoundly important.

> **Here is the biologist John Maynard Smith, rumbling one of those ritual disavowals of Whiggishness that nowadays tend to precede histories of biology, *'He, it happens to be Ernst Mayr, remarks of the need to avoid writing a Whiggish history of science but that is the kind of history he has written. I cannot imagine how a man, who has striven all his life to understand nature, and who has sought to persuade others of the correctness of his understanding, could write any other kind of history. If Victorian England really had been the highest peak of civilization, and if it really had held in itself the guarantee of continued progress, Macaulay's method of writing history would have much to recommend it, unfashionable as it may be to say so. We really do have a better grasp of biology than any generation before us and if further progress is to be made we'll have to start from where we now stand, so the story of how we got here is surely worth telling.'*** [116]

Maynard Smith offers no definition of Whig history but one gathers that, like Hall, he sees that the practice encompasses something which is potentially useful and fruitful. Historical perspectives which are part of a continuous development, *'the story of how we got here'*, can greatly aid us in sorting out present-day problems. Likewise, the perspective of the present has an important role in the evaluation of the past. Not only can it assist in clarifying what the historical problems actually are but it also functions as a yardstick to help us to decide whether or not a particular goal was ever achieved. It would seem that past-present interaction deserves consideration and certainly does not deserve to be methodologically proscribed.

Now what of the sort of methodological decisions that must be taken by anyone wishing to formulate a workable historiography for the development and progress of scientific ideas. The relevant word here is the one just used, 'goal'. Maxwell has argued that history is always about something more or less specific and can be about an endeavour to achieve a goal over a short, a medium, or even a long time-span.

> **Real history is always history of something; history of government, the struggle for power, international relations, economic developments, patterns of employment, class, marriage, attitudes towards children, towards death, towards sex, fashion, art, science, medicine, travel, and so on. Real history always carves out some temporally extended object or process to study from the amorphous, impossible, almost entirely boring mass of total history.** [117]

Of course temporal extension is rather out of favour with many historians of science, as I have repeatedly demonstrated.[52] This account by Maxwell represents a genuine attempt to provide a definition of temporal extension that answers all the objections of such historians of science. The argument says that the temporally extended object or process can be identified as a goal-oriented enterprise within a temporally extended framework, whose upper limit may even be the present day. More particularly, it could even address the successes and failures of such an endeavour and the reasons for them. Even more particularly, the goal-oriented endeavour to be studied could quite legitimately be defined as the attempt to improve scientific knowledge and understanding.

Before I go any further, I must emphasize that this is not the kind of goal whose study is advocated by Marta Fehér. She identifies these as the historically different goals of what we, misguidedly, have tended to refer to as a single scientific enterprise transcending different periods. She argues that the way to investigate them is to find out what authority has appointed the goal, which interest groups fought for the right to determine what goals are legitimate, and by what means they can be obtained. These are historically-located goals and in the absence of a convincing and transhistorical definition of 'scientific rationality' the only way of defining a historically changing scientific rationality appropriate to each period has to be in relation to the specific goals of the period, which are socially given.[118] Maxwell's goal-oriented history in no way implies that any definition of a 'goal-seeking endeavour' was explicitly pursued all of the time, or even some of the time, nor that it was articulated in any recognisable form at any time, nor indeed that goal-seeking endeavours are always, or even ultimately, successful. These are exactly the questions that ought to be left

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[52] **Although some, like Cunningham and Williams with,** 'De-centring the "big picture"' (1993), **are trying to resurrect a version more suited to social history of science sensibilities.**

open to consideration. This sort of goal-oriented history offers a solution to a central problem generated by contextuality and which now confronts it, that of *'how to interpret the relationship between the local settings in which scientific knowledge is produced and the unique efficiency with which such knowledge seems to travel.'* [119] Maxwell goes so far as to say,

> **Indeed, granted that science is, in many respects, a successful goal-seeking endeavour par excellence, and is consciously and actively understood to be this by scientists and non-scientists alike, we may well hold that there is a certain perversity in treating the history of science in any other fashion.** [120]

If it is fruitful to treat the history of science in this way, then to that extent it is surely justified. The implication that science is widely accepted as a discipline that has made astonishing progress in comprehending and manipulating nature, and is therefore deserving of its privileged status, does not necessitate that every aspect of scientific development has always been rational and progressive in the highest degree, or has utilized the same methodology: on the contrary, the progressive areas of the scientific endeavour are further illuminated if they are studied alongside the 'blind alleys', like the miasma theory of disease, or the phlogiston theory. Indeed blind alleys are essential to the study of the progressive process and, once again, only adherence to bad inductivist philosophy of science might lead anyone to suppose otherwise. If, for example, Popperian conjectures and refutations were (at least) a part of scientific rationality, then with no blind alleys there would be nothing to refute and science would be irrational! It is even possible to deplore what science has lost in pursuit of its goals, such as the philosophical dimension it enjoyed in the seventeenth century. In studying the writings of various of the natural philosophers of that era, Howard Stein concludes,

> **I have found striking not only their intellectual power but even more the extraordinarily high plane of discussion in which they engaged, from which the subsequent generations of scientific-philosophical controversy seem to me a serious falling off.** [121]

The whole business of goal-orientation is bound up with the making of intellectual or value-judgements and typical objections to this practice revolve around the argument that any process of selecting material from the sum of total history in accordance with interests and values cannot help but be biased by those interests and values. Again, Maxwell tries to anticipate and pre-empt such objections by showing that the sphere of the influence of value judgements, although surprisingly wide, is not necessarily all-embracing: it covers no more than the delineation of objects or areas of study and the identification, out of a mass of historical data, of the important, significant problems, the ones that really should be addressed. The argument begins by addressing itself to the influences involved in the choice of the object intended for study,

> **In deciding what one is to write history about - in deciding what the object X of the history is to be - one may be influenced by all sorts of things: interests, values, tradition, career considerations, public interest, fashion. It is of course absolutely inevitable that values should influence the choice of subject matter. This happens throughout inquiry, it happens just as much in physics or astronomy as it does in history or anthropology.** [122]

It is not the case that such influences will necessarily bias history but trends in the history of science have ensured that it has too often been assumed that objectivity and freedom from bias depends on not selecting historical subjects in accordance with interests and values.[53] This position conflates two entirely distinct ways in which interests and values may influence the content of history. Interests and values may, as the quotation implies, influence the choice of the subject matter for an historical work, or they may further operate in identifying, within the chosen area of investigation, the problems which are to be regarded as significant. These are two legitimate areas of operation and constitute the first type of influence. Secondly, interests and values may influence factual judgements, factual statements, made by the historians about the object of study. It is, of course, the second type of influence, which can constitute bias, the distortion of truth, and the degeneration of history into propaganda and hagiology. It is not automatically the case that the first type of influence will creep into (and distort) any factual or explanatory statements that the historian may make: if properly used interests and values can help to sort out what constitutes a valuable area of study and what it is that requires explanation within that area without necessarily being drawn into the form of that explanation. Notwithstanding, there are two things to note about the possible risks that can emerge from the use of the first type of influence. Firstly, if the values that exercise this sort of influence over historical work as a

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[53] **Many proponents of SSK, one suspects, would agree that interests and values are to be avoided, though few of them think about history.**

whole are too circumscribed, the subject matter of history as a whole is likely to be too circumscribed; history may in this way be restricted to monarchies and wars, or to the rise and fall of nations. Secondly, the danger remains that the values which exercise a harmless (first type) influence over a given piece of historical work will also tend to smuggle in an unavoidably detrimental influence of the second type. The way to safeguard against these possible pitfalls is firstly, to encourage histories of as wide a range of historical subjects as possible, so that the problem of history as a whole being too restricted is rendered unlikely and secondly, to ensure that, when relevant, the historian makes it very clear what the object of historical study is, both its subject area and its most pressing problems, and why precisely it was chosen. Value judgements that have been subjected to this second precautionary measure cannot be implicated in bias or distortion and furthermore are involved in identifying progressive histories as goal-oriented histories.

However, the case can be put more strongly than this. It is not just that interests and values can direct the choice of historical subject matter but rather that they must. The scope of value judgements goes even further than has so far been suggested. It should be pointed out that benign interests and values, which influence the choice of subject matter and identify the important problems associated with it, may still exert a pernicious influence when too narrowly applied to historical work as a whole. The consequence can be the restriction of the subject matter, as can be seen from the effects of the widespread belief that history of science is an unavoidably social construct. History, however, is always history of something which has been carefully selected from that inchoate and unstructured body of information that constitutes 'the past': it is surely incontrovertible to state that there exists a legitimate range of perspectives on the past.[54] A consistent relativist might, at this point, argue that no story taken from the amorphous mass of historical data is intrinsically more acceptable than any other. However, that is not really the point at issue. The 'legitimate perspectives' should pick out, from the amorphous mass of historical data, those developments and episodes which are of particular relevance to their given point of view. As long as interests and values

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[54] There are, undoubtedly, 'stories' concerning science and social or cultural history, science and economic history, science and political history, science and religious belief, and science seen as a progressive development of ideas internal to itself, ideas which can cover the physical, conceptual, mathematical, and metaphysical dimensions.

influence the choice of the subject matter, the object of study carved out from total history, then they do not automatically entail the prescription of the facts and the biasing of history. Equally values and interests must be used if what is intellectually significant is ever going to be sorted out from what is not. To refuse to clarify what is intellectually significant is to deny the data and the proscribe the range of possible interpretations.

I submit that the concept of a progressive history of scientific ideas is not, in principle, a hopeless one. What is required for the successful establishment of such a historiography is a legitimate way of dealing with goal-orientation, value judgements, and the traditional problem of the rationality of science, the problem of induction. This last is necessary because if the basic goal for science is that of improving knowledge about natural phenomena then it is vital to specify what it is that counts as knowledge and be able to make rational sense of the notion that progress in such knowledge is possible and does occur. So far we have established the possibility of a goal-oriented endeavour stretched out in time, which can be rationally dealt with if, by means of the legitimate use of value judgements, the goal is clearly elucidated and the developments relevant to our understanding of the episodes under investigation are also involved as liberally, critically, and self-consciously as possible. Such developments, of course, include 'external factors', from metaphysics to economics, where appropriate. The search for a solution to the problem of induction also appears to be worth pursuing, when what has already been demonstrated is considered. Firstly, the problem itself presupposes that we have some genuine factual knowledge of particular entities in our immediate locale. Secondly, such knowledge is invariably explanatory, enabling us to act - and continue to act - successfully in our environment Thirdly, this carries implications for the constitution of the entire cosmos, which must be so arranged as to make it possible for us to acquire this (admittedly vague and generalized) knowledge of it. Any historiography aimed at elucidating a history of the progress of scientific ideas must find a legitimate way of developing a methodology to accommodate goal-orientation and value-judgements in a manner which makes it possible to delineate and address the problems, without being drawn into the justification of solutions. It must also produce a solution to the traditional problem of the rationality of science, the problem of induction. The ideal would perhaps be a historically evolving notion of scientific rationality, appropriate to each period, and defined in relation to a specific goal selected by the historian.

[1] Donovan, A., Laudan, L., and Laudan, R. (eds.), *Scrutinizing Science: Empirical Studies of Scientific Change*, Dordrecht: D. Reidel/Kluwer Academic, 1988, 8.

[2] ibid., 12.

[3] Gooding, D., 'Essay Review', *BJHS*, 1989, 22, 425.

[4] Hoch, P., 'A Historical Philosophy of Science?', *Hist. Sci., xxviii*, 1990, 218-19.

[5] Shapiro, A., *Fits, Passions, and Paroxysms: Physics, Method, and Chemistry and Newton's Theories of Colored Bodies and Fits of Easy Reflection*, Cambridge: Cambridge University Press, 1993.

[6] Levere, T. and Shea, W. (eds.), *Nature, Experiment, and the Sciences: Essays on Galileo and the History of Science in Honour of Stillman Drake*, Dordrecht: Kluwer, 1990.

[7] Goodman, D. A. and Russell, C. A. (eds.), *The Rise of Scientific Europe 1500-1800*, Sevenoaks: Hodder and Stoughton/The Open University, 1991, 3.

[8] Goodman, D. A. and Russell, C. A. (eds.), ibid, 253-276.

[9] Kuhn, T. S., *The Structure of Scientific Revolutions* (2nd edn.), Chicago: University of Chicago Press, 1970.

[10] Kuhn, T. S., 'Objectivity, Value Judgement, and Theory Choice', in *The Essential Tension*, Chicago: University of Chicago Press, 1977.

[11] Quine, W. V., 'Two Dogmas of Empiricism', *From a Logical Point of View*, Cambridge, Mass.: Harvard University Press, 1953, 20; *Word and Object*, Cambridge, Mass.: Harvard University Press, 1960, chapters one and two.

[12] Feyerabend, P., 'Against Method', *Minnesota Studies in the Philosophy of Science*, 4, 1970; 'Consolations for the Specialist', in I. Lakatos and A. Musgrave (eds.), *Criticism and the Growth of Knowledge*, Cambridge: Cambridge University Press, 1970, 219-222.

[13] Hesse, M., *Revolutions and Reconstructions in the Philosophy of Science*, Sussex: Harvester, 1980, 33.

[14] Shapin, S., 'Here and Everywhere: Sociology of Scientific Knowledge', *Annu. Rev. Sociol.1995*. 297.

[15] Barnes B. and Bloor D., 'Relativism, Rationalism and the Sociology of Knowledge', M. Hollis and S. Lukes (eds.), *Rationality and Relativism*, Oxford: Blackwell, 1982, 21.

[16] ibid., 21-22.

[17] Hesse, M., *Revolutions and Reconstructions*, 57.

[18] Hollis, M. and Lukes, S, *Rationality and Relativism*, 4.

[19] Barnes, B. and Bloor D., 'Relativism, Rationalism and the Sociology of Knowledge', 23.

[20] Bloor, D., *Knowledge and Social Imagery*, (2nd. edn.), Chicago: University of Chicago Press, 1991.

[21] Barnes, B., *Interests and the Growth of Knowledge*, London: Routledge and Kegan Paul, 1977, 25.

[22] Kuhn, T. S. 'Objectivity, Value Judgement and Theory Choice', 220-229.

[23] Barnes, B., *T. S. Kuhn and Social Science*, London: Macmillan, 1982.

[24] Barnes, B., *Interests and the Growth of Knowledge*, 123.

[25] Barnes, B., *T. S. Kuhn and Social Science.*

[26] Mannheim, K., *Ideology and Utopia*, New York: Harcourt, Brace and World, 1936.

[27] Hesse, M., 'Changing concepts and stable order', *Social Studies of Science*, iv, 1986, 714-26; 'Socialising epistemology', in E. McMullin (ed.), *Construction and constraint: The shaping of scientific rationality*, Notre Dame: University of Notre Dame Press, 1988. 97-122.

[28] Pickstone, J., 'Past and Present Knowledge in the Practice of the History of Science', *Hist. Sci., xxxiii*, 1995, 214.

[29] Wallis, R. (ed.), *On the Margins of Science: The Social Construction of Rejected Knowledge*, University of Keele, 1979.

[30] Latour, B., *Science in Action: How to Follow Scientists and Engineers through Society*, Cambridge, Mass: Harvard University Press, 1987, 191.

[31] ibid., 184.

[32] ibid., 195.

[33] ibid.

[34] ibid., 196.

[35] ibid.

[36] ibid., 179.

[37] ibid.

[38] Pickering, A., 'Knowledge, Practice and Mere Construction', *Social Studies of Science*, Vol 20, London: Sage, 1990, 682-729.

[39] Dear, C., 'Cultural History of Science: An Overview with Reflections', *Science, Technology, and Human Values*,Vol 20 no. 2, London: Sage, 1995, 150-170.

[40] ibid., 155.

[41] Shapin, S., *A Social History of Truth: Civility and Science in Seventeenth-Century England*, Chicago: University of Chicago Press, 1994, 43.

[42] Gellner, E., 'Relativism and Universals', in M. Hollis and S. Lukes, *Rationality and Relativism*, Oxford: Blackwell, 1982, 181.

[43] Shapin, S., *A Social History of Truth*, 350-1.

[44] ibid., 5-6.

[45] Bloor, D., *Knowledge and Social Imagery* (2nd. edn.), 33.

[46] Shapin, S., 'Here and Everywhere', 303-4.

[47] Garfinkel, H., *Studies in Ethnomethodology*, Cambridge: Polity Press, 1984 (© 1967).

[48] Kuklick, H., 'Mind Over Matter?', *HSPS 25:2*, 1995, 364-65.

[49] Collins, H. and Pinch T., *Frames of Meaning: The Social Construction of Extraordinary Science*, London: Routledge and Kegan Paul, 1982; Collins, H., *Changing Order: Replication and Induction in Scientific Practice*, (2nd edn.) Chicago: University of Chicago Press, 1992.

[50] Kuklick, H., 'Mind Over Matter?', 361.

[51] Latour, B., *Science in Action*, 98.

[52] Hollis, M. and Lukes, S. (eds.), *Rationality and Relativism*, 1.

[53] Hesse, M., *Revolutions and Reconstructions*, 58.

[54] Collins, H., *Changing Order: Replication and Induction in Scientific Practice* (2nd. edn.).

[55] Stewart, L., 'The Selling of Newton', *J. of British Studies,* 25, 1986, 192.

[56] Shapin, S., 'Discipline and Bounding: The History and Sociology of Science as Seen Through The Externalism-Internalism Debate', *Hist. Sci.,*xxx, 1992, 358.

[57] Barnes, B., *Scientific Knowledge and Sociological Theory*, London: Routledge and Kegan Paul, 1974.

[58] Shapin, S., 'Discipline and Bounding', 352.

[59] ibid., 354.

[60] ibid., 352.

[61] Moore, J., 'Here's History of Science', in *A103: The Arts Foundation Course, Block Four,* Milton Keynes: The Open University, 1998, 20.

[62] Forman, P., 'Immanance not transcendence, for the historian of science', *Isis*, 1 xxxii, 1991, 78-9.

[63] Shapin, S., 'Discipline and Bounding', 358.

[64] ibid., 352.

[65] Olby, R. C., 'Rediscovery as an Historical Concept', R. P. W. Visser *et al* (eds.), *New Trends in the History of* Scienc, Amsterdam: Rodopi B. V., 1989, 179.

[66] Shapin, S., 'Here and Everywhere', 292.

[67] Barnes, B., *T. S. Kuhn and Social Science.*

[68] Latour, B., *Science in Action*, 100.

[69] Wilson, A. and Ashplant, T., 'Whig History and Present-Centred History', *Hist. Jour.*, 31, 1, 1988, 1-16; 'Present-Centred History and the Problem of Historical Knowledge', *Hist. Jour.*, 31, 2, 1988, 253-274.

[70] Bachelard, G., *L'actualité de l'histoire des sciences*, Paris: Palais de la decouverte, 1951.

[71] Fichant, M. and Pechaux, M., *Om Vetenskapernas Historia*, Stockhol.: Bo Cavefors, 1971, (English translation in H. Kragh, *An Introduction to the History of Science*, Cambridge: Cambridge University Press, 1987, 131.

[72] ibid.

[73] Lovejoy, A. O., *The Great Chain of Being*, Cambridge, Mass: Harvard University Press, 1976.

[74] ibid., 15.

[75] Sachs, M., 'Maimonides, Spinoza and the field concept in physics', *J. Hist. Ideas*, 37, 1976, 125-131.

[76] Sambursky, S., *The Physical World of the Greeks*, London: Routledge and Kegan Paul, 1963, 137.

[77] Kragh, H., *An Introduction to the History of Science*, Cambridge: Cambridge University Press, 1987, 85-6

[78] ibid., 86.

[79] ibid.

[80] Holton, G., *Thematic Origins of Scientific Thought*, Cambridge, Mass: Harvard University Press, 1973; *The Scientific Imagination: Case Studies*, Cambridge, Mass: Harvard University Press, 1978.

[81] Sargent, R., *The Diffident Naturalist*, Chicago: University of Chicago Press, 1995.

[82] Shapin, S., 'Discipline and Bounding', 358-9.

[83] Shapin, S., 'Here and Everywhere, 315.

[84] ibid., 292.

[85] Ashplant, T. and Wilson, A. , 'Whig History and Present-Centred History', 1.

[86] Ashplant, T. and Wilson, A., 'Present-Centred History and the Problem of Historical Knowledge', 270.

[87] Ashplant, T. and Wilson, A., 'Whig History and Present-Centred History', 10.

[88] Ashplant, T. and Wilson, A., 'Present-Centred History and the Problem of Historical Knowledge', 268.

[89] Hall, A. R., 'On Whiggism', *Hist. Sci.*, xxi, 1983, 53, 54.

[90] Ashplant, T. and Wilson, A., 'Present-Centred History and the Problem of Historical Knowledge', 272.

[91] ibid., 274.

[92] ibid.

[93] ibid.

[94] Shapin, S. and Schaffer, S., *Leviathan and the Air Pump: Hobbes, Boyle and the Experimental Life*, Princeton: Princeton University Press, 1985.

[95] Cunningham, A. and Williams, P., 'Decentring the "big picture": the *Origins of Modern Science* and the modern origins of science', *BJHS,* 1992, 26, 409.

[96] ibid.

[97] ibid., 410.

[98] Hall, A. R., 'On Whiggism', *Hist. Sci.*, xxi, 1983, 45-59.

[99] ibid., 53.

[100] ibid., 52.

[101] ibid.

[102] ibid.

[103] Ashplant, T. and Wilson, A., 'Present-Centred History and the Problem of Historical Knowledge', 274.

[104] Hall, A. R., 'On Whiggism', 60.

[105] Figala, K., 'Newtons rationale System der Alchemie', *Chemie in unserer Zeit 12*, 1978, 101-110, trans. H. Kragh, *An Introduction to the History of Science*, 99.

[106] Popper, K., *Conjectures and Refutations*, (4th edn. rev.) London: Routledge and Kegan Paul, 1972, vii.

[107] Kuhn, T. S., *The Essential Tension*, Chicago: University of Chicago Press, 1977, 334.

[108] Maxwell, N., *From Knowledge to Wisdom*, Oxford: Blackwell, 1984; *The Comprehensibility of the Universe*, Oxford: Oxford University Press, 1998.

[109] Maxwell, N., *The Comprehensibility of the Universe.*

[110] Wolpert, L., *The Unnatural Nature of Science*, London: Faber and Faber, 1992.

[111] Maxwell, N., *The Comprehensibility of the Universe.*

[112] ibid.

[113] ibid.

[114] Dobbs, B., *The Janus Faces of Genius: The Role of Alchemy in Newton's Thought*, Cambridge: Cambridge University Press, 1992.

[115] Cronin, H., *The Ant and the Peacock*, Cambridge: Cambridge University Press, 1991, 4.

[116] ibid., 5.

[117] Maxwell, N., 'The Odd Couple: An Inquiry into the Crisis in History and Philosophy of Science and its Role in Obstructing the Resolution of Global Problems', unpublished, 1992, 6.

[118] Hronsky, I., Fehér, M., and Dajka, B., *Scientific Knowledge Socialized*, Dordrecht: Kluwer, 1988.

[119] Shapin, S., 'Here and Everywhere', 290.

[120] Maxwell, N., 'The Odd Couple', 12-13.

[121] Stein, H., 'Newtonian Space-Time, *The Annus Mirabilis of Sir Isaac Newton*, The Texas Quarterly, Autumn, 1967, Vol X, no. 3, 186-87.

[122] Maxwell, N., 'The Odd Couple', 6.

CHAPTER TWO

AIM-ORIENTED EMPIRICISM: A SUITABLE METHODOLOGICAL FRAMEWORK TO PROVIDE CRITERIA FOR THE EXECUTION OF A PROGRESSIVE HISTORY OF SCIENTIFIC IDEAS

In the previous chapter certain recommendations were made concerning the need to adopt a tolerant stance about what constitute the legitimate objects of study within history in general and, more particularly, the need to take progress histories seriously and formulate methodological frameworks to provide criteria for the doing of progress history. In particular Chapter One argued for <u>progressive histories of science</u> and their concomitant methodologies. In general terms, granted that a legitimate object of historical investigation is the sort of long-term, goal-seeking endeavour that has already been described, a major task of the historian will be to understand the successes and failures of the endeavour from the standpoint of actions tending to bring the endeavour towards its specified goal. The historian does have a certain amount of freedom to choose what he or she considers to be the goal of the endeavour: it is not, as a matter of policy, a good idea to let the historical actors 'make' the decision because that would imply that they knew, in advance, what goal was going to be. However, certain rules do apply and must be adhered to if the thing is to be carried off successfully. First and foremost in this sort of historical study of progressive undertakings, particular care must be taken to specify <u>what the goal is taken to be</u> and then equal care must be taken to adhere to the chosen definition: differently defined goals for the overall endeavour will necessarily lead to different progress histories. For example, the serial adoption of a range of the different meanings which may be given to terms such as 'democracy', or 'totalitarianism' would result in a series of very different histories of endeavours to achieve those two states. Furthermore, a carefully elucidated goal is more likely to preserve the historian from falling into certain presuppositional traps. One is the trap of presupposing that in any goal-pursuing undertaking progress must have been achieved. Whether or not such a thing has happened is an important factual, historical question which is to be established through historical studies: goal-oriented history can be a history of failing to attain a particular goal quite as much as it can be a history of attaining it. Moreover, even if there is good evidence that progress has taken place, that does not imply that progress has been made in every particular and with respect to every justifiable goal. Again, those are things which are to be established. Another presupposition that can be avoided, if goal-articulation is meticulously performed, is the implication that the historical actors either articulated a goal at all, or if they did that they articulated it in exactly the same way that the historian does. Progress may result without ever having been intended, or as the result of a completely different endeavour, which may or may not have been explicitly goal-pursuing. One need only think here of a history of humanity's 'attempts' to live in a balanced and sustainable way with nature. Far from constituting progress, most people would probably regard this as a long decline, the very opposite of progressive. Moreover, few would dispute that our ancestors had any idea of it as a relevant, let alone realisable, goal. The essential point to understand is that the historian of goal-seeking endeavours must, as a crucial part of historical work, endlessly assess historical deeds and episodes from the point of view of their success and failure in tending towards the goal. Therefore the goal needs to be stated explicitly, with no hidden agendas or suppositions.

To bring this argument from the general to the particular (the specific example being a progressive history of science) it should be said that to all appearances science <u>is</u> a goal-seeking endeavour which, since the seventeenth century, has made astounding and unparalleled progress in improving our knowledge of the natural world. As a rule, both scientists and philosophers of science take it for granted that science (at its best) is an instantiation of progress, intellectual worth, and rationality. In their eyes progress in scientific knowledge has been a real, tangible process, even though there might be disagreement as to whether science improves knowledge of unobservable entities such as quarks, or quantum fields. Clearly, the emphasis of the kind of progress-oriented history that would accommodate these concerns would be consistent with what used to be known as intellectual history of science, with its stress on scientific ideas, theories, discoveries, arguments, problems, and the adequacy or otherwise of attempts to judge claims to scientific knowledge from the standpoint of their adequacy to the facts. Towards the end of the last chapter I made a point concerning the ubiquity and seeming intractability of the problem of induction: contemporary philosophy of science may be convinced that science has made dramatic progress in improving knowledge of nature but it has yet to show that it is even possible to acquire theoretical knowledge about the world. What price a progress-oriented intellectual history of science if there is no means of making rational sense of the idea that progress in scientific knowledge is possible, and does occur?[1] However, I also made the point that the aim of doing a history of scientific progress is valid just as long as scientific progress is possible and to this I now add the point that a further trap which careful goal-articulation helps the historian of science to avoid is that of assuming that the advance of science has been a rational affair. Like progress and the other concepts mentioned in the previous paragraph, this is something to be established through investigation and not presupposed. Is it not therefore reasonable to assert that academic history of science can indeed construe the object of its study as a goal-seeking endeavour, perhaps the goal of improving expert knowledge about natural phenomena? In this guise it can be said to belong to that part of general history which studies long-term goal-pursuing endeavours and concentrates on the successes and failures of scientific work, as evaluated from the standpoint of helping science to realize its basic goal. I think the answer is, 'Yes, it can be so construed', but such progress-oriented history of science, although possible, is significantly hindered by the absence of a solution to the problem of induction. There is a real need to tackle the intellectual problem of the rationality of science because there is a real need to understand how science ought to proceed if it is to be ideally rational. If the basic goal for science is that of improving expert knowledge about natural phenomena then it is vital to specify what it is that is to count as knowledge and be able to make rational sense of the notion that progress in scientific knowledge is possible and does occur.

Of course, at this point well-rehearsed prejudices rear their heads. Historians of science tend to see all attempts to solve the intellectual problem of the rationality of science

[1] **It is over twenty years since Kuhn, in the light of this problem, doubted the advisability of approaching the history of science from the perspective of any philosophy of science. He argued that the philosophy of science can never illuminate the historical record and, if used as an interpretative framework, will simply reconstruct history. See Kuhn, T. S.,** 'The History and Philosophy of Science', *The Essential Tension*, Chicago, University of Chicago Press, 1977, 3-20.

as attempts to set up a factual, rationalistic theory of actual scientific development. They know that the most cursory historical study of how science has actually proceeded shows it to have been very different from any such purely rationalistic account: as the previous chapter demonstrates, they tend to dismiss claims on the part of philosophy of science to provide any sort of a historiography of science on the grounds that the rationalistic aspect of science is a chimera. In such a situation, how can it be realistic to move from empirical material from the past to prescriptive statements about the development of science through time? The fact that the problem of induction remains apparently unsolved only serves to reinforce this attitude. However, the historians are not quite right: philosophers attempting to solve the intellectual problem of the rationality of science are actually trying to specify how science ought ideally to be rather than how it in fact is: they are concerned with normative questions. Their failure does not prove that the problem of induction has no solution, but equally it sheds no illumination on the question of how an ideally rational science ought to proceed, the very problem to which progress-oriented history of science needs an answer if it is to construct a historiographic framework. If it were to prove possible to specify the rational ideal then the question of the degree to which that ideal has been put into real scientific practice would be an area amenable to historical investigation.

Expounding Aim-Oriented Empiricism

Bearing in mind the above considerations, now is perhaps the time to unequivocally state that the overall intention of this thesis is twofold: firstly, it aims to try and formulate a methodological framework that provides criteria for implementing a progressive history of scientific ideas and secondly, it intends to test the adequacy of that framework by using it to examine an important area in the history of science. I aim to show that it is possible to study aspects of an important episode in the history of science, the Scientific Revolution, in terms of a methodology culled from a current philosophy of science, aim-oriented empiricism. It will be shown that this can be accomplished in a manner which does not fall into any of the pitfalls so frequently cited by social historians of science (and summarized in Chapter One) but which does build on the general arguments already given as to why a history of scientific progress is possible and even desirable. More specifically the 'aspects' comprise the work of Galileo, who for many years was dubbed 'the father of the Scientific Revolution' and whose actual contribution to it continues to be of perennial interest. Secondly, I shall demonstrate how illuminating it is to conceive of Galileo's work in such terms, how fruitful it is in the solution of historical and philosophical problems which have proved tenacious and, how it makes rational sense of the entire episode.

A Brief Summary of Aim-Oriented Empiricism

Aim-oriented empiricism, a new way of comprehending the scientific enterprise, found its first expression in two papers by Nicholas Maxwell entitled 'The Rationality of Scientific Discovery: Parts One and Two'.[1] It is a subject he has pursued and developed with energy ever since, for example a decade later, his book *From Knowledge to Wisdom* in effect generalized aim-oriented empiricism to aim-oriented rationalism and applied it to the whole of inquiry.[2] Briefly, aim-oriented empiricism was formulated in response to what Maxwell identified as a problematic and largely unchallenged assumption at the heart of the scientific endeavour, which he termed 'standard empiricism', whose chief characteristic is that,

> **when it comes to the assessment of results in science, the assessment of scientific propositions, laws and theories, these results must be accepted and rejected solely with respect to empirical**

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[2] **The themes of this book are painted on a vast canvas. Its aim is to,** *'put into practice a profound and comprehensive intellectual revolution affecting to a greater or lesser extent all branches of scientific and technological research, scholarship and education'*, **a transformation involving,** *'a radical change in the fundamental overall intellectual aims and methods of inquiry'.* **(See the 'Preface) Maxwell diagnoses the present malaise in academic enquiry as being caused by the ubiquitous belief that the only way to promote human welfare is to improve knowledge in as many areas as possible. In his opinion this** *'official basic creed of the whole scientific/academic enterprise'* **is an irrational and unrigorous way of proceeding that can be traced to the development of modern science in the seventeenth century. Science, according to Maxwell, took an unfortunate turning at the beginning of the century with which it is especially associated, when Francis Bacon, in a move against Aristotelianism, aimed to conquer nature through improvements in knowledge, and it has continued in the wrong path ever since infecting the whole of academic enquiry on the way. Whilst there always have been those who have sought to apply science to utilitarian or other social or ethical ends, the primary task for scientists has remained the improvement of knowledge. The present knowledge-oriented orthodoxy is the result of the reciprocal historical development of the scientific enterprise and the doctrine of the necessity of improving knowledge, which Maxwell calls the 'philosophy of knowledge', a reciprocity which may be demonstrated by showing that the paradigmatic heart of the philosophical doctrine of knowledge is a more specific philosophy of science, which he calls 'standard empiricism'. Thus is that the declared or official aims and methods which science has evolved must be reformed first before the all-inclusive intellectual revolution can be instigated. Maxwell calls this new way of comprehending the scientific enterprise 'aim-oriented empiricism' and his exposition ranges not only over the natural sciences but over the social sciences and humanities too, urging that our conception of all three needs to be transformed along with our ideas about their inter-relationships. Above all, he concerns himself with the problem of how there can be life of value in the physicalist universe which he so ardently advocates.**

success and failure, to the justice that they do to observational and experimental evidence, in an impartial way. [2]

However, why would a philosophy which concerns itself with natural science - and which was developed in response to problems associated with too rigidly empirical an approach in the natural sciences - be of any interest to the historian? The answer is that an aspect of aim-oriented empiricism which is on the whole left implicit in Maxwell's work, and therefore underdeveloped, is its application to the history of science, particularly the history of the progress of scientific ideas. It would appear to be, even at the most superficial level, an interesting aspect and one worth pursuing because it deals with the beginnings and subsequent development of modern science. However, at a more fundamental level it becomes clear that aim-oriented empiricism provides a framework for historical analysis that has yet to be properly articulated and implemented. As will be amply illustrated later, it demonstrates that the rational pursuit of knowledge has an historical character to it which implies that the correct way to legitimize or justify scientific method is to examine the search for knowledge from a historical perspective. Without reproducing aim-oriented empiricism in its entirety, it is nevertheless necessary to expound it to a degree sufficient to be able to demonstrate the possibility, the desirability, of applying it to the history of science.

The first crucial point that must be understood before venturing any further is that aim-oriented empiricism and standard empiricism, although similar enough to bear comparison, are based on different intellectual principles. The result is that the former represents a more rigorous conception of science than does the latter. In Chapter One I quoted Pickering's passing remark that all attempts to articulate a logic of science, which he claims have failed, have assumed that theories are evaluated on the basis of a *'static comparison with evidence'*. Maxwell sees the unquestioning dependence on empiricism as the fundamental reason for this general failure. In fact, he identifies two clashing principles, *(a)* the principle of empiricism, which requires that all knowledge be ultimately based on experience, and *(b)* the principle of intellectual integrity, which requires that assumptions that are substantial, influential, and problematic in some way need to be stated unequivocally in order that they might be criticized, and alternatives considered, ideally leading to improvement and development. This includes both empirical considerations and metaphysical considerations concerning the comprehensibility of the universe. Science pursued entirely and solely in accordance with *(a)*, in which only empirical considerations count, may at first sight appear to be a perfectly ingenuous ideal of scientific integrity but in the end proves to be neither ingenuous nor workable. Attempts to do science solely in accordance with the principle of empiricism would result in the enterprise being overwhelmed by infinitely many, empirically successful but aberrant and non-explanatory theories. However well verified a theory may be by a body of evidence, there will always be infinitely many grossly ad hoc rival theories that will fit the evidence just as convincingly - and infinitely many more that are empirically more successful. Progress in knowledge would not be possible under such circumstances. Standard empiricism, as was demonstrated in quotation [2] above, purports to work through the principle of empiricism: in fact it only works at all, as those who defend it will admit, by dint of making persistent assumptions about the nature of the universe. The most obvious examples include criteria that have to do with unity, simplicity, and explanatoriness. Now it is not immediately obvious that this modification to the principle of empiricism poses any problems sufficiently serious to threaten standard empiricism, especially as it has traditionally offered a feasible conception of science and the possibility (and indeed the actuality) of progress in scientific knowledge. However, closer examination reveals that standard empiricism's amended form of *(a)* involves smuggling in assumptions that properly belong to *(b)* and holding them implicitly in order to avoid a direct clash with the principle of empiricism itself. Unfortunately, a direct clash is unavoidable. Persistently rejecting empirically successful but decidedly *ad hoc* rivals to accepted theories, on the grounds that these rivals clash with criteria having to do with simplicity, explanatoriness, or unity, involves implicitly assuming that, at the very least, nature behaves as if simple, or amenable to explanation, or unified. Standard empiricism's attempt to proceed in accordance with a modified version of *(a)* inexorably violates *(b)*, the principle of intellectual integrity, because *(b)* requires that such implicit assumptions be made explicit. This matters because of the two *(b)* is more fundamental and should always prevail over *(a)* in the event of a clash. This can be demonstrated by arguing that the principle of empiricism, whatever form it is employed in, would accept that in order to have something to test against observation and experiment it is first necessary to extricate the expected empirical consequences of the scientific theories under consideration. However this, if one thinks about it, is simply a process of making the implicit content of the theories explicit, in order that the theories may be critically assessed, which is precisely what the principle of intellectual integrity requires: the necessary process of unravelling the (testable) empirical consequences of scientific theories is actually implicit within the principle of intellectual integrity. Standard empiricism's violation of *(b)* means that the substantial, influential, problematic assumptions about the world implicit in the adoption of simplicity criteria for theory-acceptance, implicit in the persistent rejection of empirically successful *ad hoc* theories, cannot be explicitly revised and reconsidered in the light of progress, or the lack of it. It has long been thought a feasible conception of science - and a certain amount of progress in knowledge may be (and indeed has been) achievable within its parameters - but it is not a wholly rational and rigorous one. Aim-oriented empiricism, on the other hand, also makes assumptions about the universe but, unlike standard empiricism, it explicitly articulates and criticises those assumptions with the aim of improving them, thereby recognising the primacy of *(b)* over *(a)* and resulting in the rational evolution of aims-and-methods with evolving knowledge.

Furthermore the principle of intellectual integrity is also superior by virtue of having universal applicability. It must always be necessary to intellectual integrity and rigour, to rationality, to make explicit whatever is implicit but substantial, problematic, and influential in a given intellectual context. This remains the case no matter what the actual intellectual context may be. In the context of the progress of science, standard empiricism demonstrates that explanatory theories are always preferred to aberrant and non-explanatory ones, even when the latter are empirically more successful. This being the case, present-day standard empiricist science is thereby committed to a massive, persuasive and yet problematic assumption, held quite independently of empirical considerations, that the nature of phenomena is such as to be at least partly knowable and explicable. Aim-oriented empiricism makes such assumptions about the nature of the universe but, unlike standard empiricism, it explicitly articulates and criticises those assumptions with the aim of improving them, thus recognising the superiority of the principle of intellectual integrity over the principle of empiricism. Therefore it is more intellectually rigorous for science to be pursued in accordance with aim-oriented empiricism, and hold (as a permanent item of scientific knowledge) the assumption that the universe is at least partly knowable in some way or other, than it is to believe that empirical success and failure alone determines what theories are accepted and rejected. Irrespective of any illuminative strengths or problem-solving capacities which it might possess, aim-oriented empiricism is a more rigorous and rational conception of science.

A Closer Look

Having established the centrality of the principle of intellectual integrity to aim-oriented empiricism, it is now possible to analyse how it works and demonstrate that it does indeed render aim-oriented empiricism a more rational and rigorous conception of science. In Chapter One I remarked that cosmological assumptions are implicit <u>in</u> factual knowledge, so that there is a sense in which any factual knowledge about anything contains or implies some (possibly very small and insignificant) knowledge about everything. However, what is required in order for human beings to act successfully in the world is much more substantial knowledge, something with considerably more content. Aim-oriented empiricism asserts that there can be no such thing as scientific knowledge worthy of the name, without a conjecture to the effect that the natural world is partially knowable. This is a legitimate starting point because to doubt it would do nothing to aid the acquisition of knowledge in any way. In the generalized aim-oriented empiricist framework it is designated **level 10**. From it descends a series of increasingly contentful and increasingly problematic assumptions, always chosen as offering the best hope of facilitating progress in knowledge, until the level is reached where complete comprehensibility is postulated as being the conjecture offering the best help with improving knowledge of the universe. This **level 5** comprehensibility conjecture (**C**) does not say in what precise way the universe is comprehensible, but merely that it is perfectly comprehensible in <u>some</u> way. The logical relationships between the propositions at the various levels is fully demonstrated in the **Appendix**, including the schematic (**Diagram 1**). Aim-oriented empiricism requires that **Level 10** is completely unrevisable and **level 9** is almost completely unrevisable. **Levels 8, 7, 6,** and **5** become <u>possible</u> to revise: if the universe is such that, after every revolution there is, for example, one more force, then **levels 4, 5,** and **6** would require revision. This is not beyond the bounds of possibility and would instigate a radical change in scientific method. Secondly, aim-oriented empiricism requires that from this **level 5** comprehensibility conjecture there be developed at least one blueprint, rather more specific in character, which develops ideas as to the particular way in which the universe is thought to be comprehensible. Aim-oriented empiricism, bearing in mind the tremendous success of modern science, holds that the best such (**level 4**) speculation is that the universe is physically comprehensible. This more or less specific version of the manner in which the universe is held to be comprehensible counts as the metaphysical blueprint. There need to be further (**level 3**) blueprints, of increasing specificity, in order to articulate the best current version of the **level 4** blueprint. From this is developed a legitimately related non-empirical methodology, typically taking the form of increasingly restrictive rules that are used in formulating (**level 2**) and assessing (**level 1**), a series of theories. This series of logically linked cosmological speculations forms a matrix in whose terms everything that is radical and innovative about aim-oriented empiricism will now be explained.

Now to an examination of the sequence or matrix of cosmological suppositions: why should such a sequence really merit consideration when so much of it seems to lie above the level of what is normally thought of as the realm of science? Well, to think about it in fairly general terms, although speculation concerning the ultimate nature of the universe often proves to be epistemologically flawed and ill-founded, it nevertheless in practice plays such an indispensable part in the rational procurement and employment of knowledge that it is vital to recognize it as a legitimate part of knowledge. **Level 10** partial knowability and **level 9** epistemological non-maliciousness - because they are wholly conjectural and non-empirical in character - are upheld as permanent items of scientific knowledge because doubting them cannot help the progress of knowledge in any circumstances whatsoever. **Levels 5 - 8** are upheld as quasi-permanent items of conjectural scientific knowledge, through a mixture of *a priori* and quasi-empirical considerations, because they are also accepted as being more empirically fruitful than any rival. As an examination of the principle of empiricism has already demonstrated it is impossible to do physics in the complete absence of any implicit cosmological assumptions: however well verified a theory may be by a body of evidence, there will always be infinitely many grossly *ad hoc* and aberrant rival theories that will fit the evidence just as convincingly - and infinitely many more that are empirically more successful.[3] It is, after all, the persistent and blanket exclusion, independently of empirical grounds, of the infinitely many *ad hoc*, non-explanatory rivals to successful theories that has made possible present-day, standard empiricist physics. However, it is precisely the exclusion of such aberrant and non-explanatory theories that indicates that physics as we know it makes an enormous, if tacit, supposition concerning the nature of the universe, a supposition which can be inferred from its methods. It assumes that, at the very least, the universe is so constituted that (locally) it behaves as if approximately comprehensible and to a degree adequate enough for science to be empirically successful when it continuously chooses theories that are slanted in the direction of comprehensibility. So it would appear that cosmological assumptions are not just implicit in factual knowledge itself but also in the methods employed in acquiring and using such knowledge.

I have already argued that aim-oriented empiricism is, in principle, more rational and rigorous than standard empiricism because it accords with the principle of intellectual integrity. More particularly, aim-oriented empiricism is, in practice, more rational and rigorous because in accord with the principle of intellectual integrity it requires that the various cosmological assumptions inherent in the methods that incline towards non-*ad hoc* theories be stated unambiguously. Another way of putting this is to say that aim-oriented empiricism holds such assumptions as a legitimate part of scientific knowledge recognising, in accordance with the principle of intellectual integrity and with the aim of aiding the

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[3] **Maxwell has written,** *'There is no end to the way in which physical systems, to which T applies, can be regarded as varying, and no end to the number of physical variables we can employ to distinguish different physical systems. These variables need not be referred to by T: it may simply be presupposed that variation of these variables leaves the applicability and success of T entirely unaffected. Thus the form of the equations of Newtonian theory not only remains invariant as we vary the place or time of a physical system, the mass, relative distance, velocity and acceleration of the bodies, their density and shape: the equations remain invariant as we change substance, temperature, colour, elasticity, smell. In order to formulate endlessly many empirically corroborated aberrant rivals to Newtonian theory, all we need do is specify, in universal terms (in terms of shape, substance, temperature, colour, smell or whatever) a kind of physical system, to which Newtonian theory applies, which has not yet been physically realized (perhaps because of its bizarre character): we then arbitrarily modify the Newtonian equations, in any way we please, for this specific kind of system. Thus we might stipulate: for two bodies, each of mass greater than two tons, each made of gold and shaped like a grand piano, adrift in space, an inverse cube gravitational law applies (but otherwise Newton's inverse square law applies)'.* **N. Maxwell,** *From Knowledge to Wisdom*, Oxford: Blackwell, 1984, 209.

acquisition of knowledge, their indispensable role in the rational procurement and utilisation of knowledge.

Before going any further it is worth enquiring whether some presupposition about the nature of the universe is always built into the methods of acquiring knowledge about the universe. All that has been established is that standard empiricist suppositions concerning the (at least localized and approximate) comprehensibility of the universe have to be inferred from its methods, which permanently incline towards non *ad hoc* theories. However, the **level 4**, more specific version of comprehensibility known as the metaphysical blueprint has not, in all times and societies, postulated the kind of impersonal universe that modern science typically deals with. Perhaps this might make a difference to the relationship between cosmology and methodology. Given a metaphysical blueprint which is a version of personalism, be that monotheistic or multi-theistic, then the rational methodololgy to employ would be one suitable for all-powerful persons and would probably include prayer, ritual and various means of foretelling the future and interpreting signs. On the other hand, should the chosen metaphysical blueprint be physicalism, further refined to postulate that the impersonal universe is governed by some pattern of physical law, then the rational way to proceed would be to propose and test hypothetical (and perhaps non-existent) laws of nature. These two cases at first sight appear to have nothing in common. At second sight, however, the relationship between cosmology and methodology holds. It is simply the case that in order to acquire knowledge in different kinds of universe it is necessary to use different methods: much of the methodology that is required to develop knowledge will vary from one kind of universe to another if the methodology is to meet with success in its given universe. All possible and knowable universes will have a legitimate, associated methodology containing (probably implicit) assumptions about the particular universe to which it applies.

Some problems and some solutions

Unsurprisingly, there are problems to be resolved, not least that we might still be lacking a convincing rationale for the entire family of cosmological speculations, **levels 1** to **10**. Could one not postulate that it is unnecessary to invoke more than perfect comprehensibility, at **level 5**? It could be argued that if the comprehensibility thesis is true then there is nothing to lose - and a great deal to gain - by assuming **C** at **level 5**; if, however, the universe is in reality only partially comprehensible (at **level 7** or **level 6**) there is still nothing to lose and much to gain from assuming **C** at **level 5** because the best hope of increasing theoretical knowledge is to assume perfect comprehensibility and develop increasingly empirically successful theories in accordance with this assumption until all progress ceases. In other words, is it not obvious that the best way to discover that the universe is only partially comprehensible would be to seek perfect comprehensibility and fail to find it? Of course, as Nick Maxwell has shown, this argument is not valid. It certainly justifies the neglect of two possibilities: that the universe is totally incomprehensible and that it is partially comprehensible in such a way that to assume perfect comprehensibility is to best promote the growth of knowledge. However, it does not account for possible, partially comprehensible universes in which to postulate perfect comprehensibility will actually hinder the progress of knowledge. Such universes can be imagined.[4] A second problem arises over the fact that aim-oriented empiricism upholds the principle of intellectual integrity much better than standard empiricism but, once adopted, actually seems to contravene the principle in that it then forbids the criticism of the cosmological assumption that the universe is wholly comprehensible in some way or other. This assumption might still be wrong and yet it cannot be criticised or revised. To put it another way, it might be argued that although aim-oriented empiricism recognises the importance of the principle of intellectual integrity it nevertheless fails to fully satisfy that principle.

However, as long as the matrix of generalized aim-oriented empiricism is properly understood, neither of these questions really pose a problem at all. The solution involves the mental 'inversion' of the process of logically linked cosmological speculations depicted in **Diagram 1**, in the **Appendix**. I have described it as a descending series of increasingly contentful and problematic assumptions but it is also instructive and illuminating to think of

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[4] **For example, it might be the case that after a succession of conceptual or blueprint revolutions the outcome is a disunified collection of N theories, such that N increases by one after each revolution, there being no end to the succession of revolutions that are, in principle, possible. Or one could imagine a universe in which intermittent, random and inexplicable events occur. See Maxwell, N.,** *The Comprehensibility of the Universe*, Oxford: Oxford University Press, 1998 forthcoming.

it from the bottom upwards, as the derivation of an hierarchical gradation of ever more enervated, rarified cosmological assumptions until the appearance of the one which is such that, no matter what the nature of the universe may be, doubting it cannot possibly aid the growth of knowledge. This approach is implied by the discovery, already made, that cosmological assumptions are always present in knowledge-gathering methodologies. Another way of putting this is to say that aim-oriented empiricism must be generalized in order to include increasingly contentless and therefore unproblematic assumptions, until an assumption is reached which is such that doubting its truth cannot help in the acquisition of knowledge in any possible circumstances. From this it can be seen that once formulated in this manner, aim-oriented empiricism solves the problem of integrity precisely because at each level the principle of intellectual integrity is fulfilled and the logical relationships between the levels is assured <u>when</u> from the various rival assumptions the one chosen is that which is conjectured to best facilitate the growth of knowledge. Given that the aim is to acquire and improve knowledge, this provides the necessary rationale for firstly, accepting epistemological non-maliciousness and meta-knowability and secondly, for accepting the assumption of perfect comprehensibility (instead of rough comprehensibility or near comprehensibility) as the best available bet. Far from it being the case that aim-oriented empiricism fails to fully satisfy the principle of intellectual integrity and far from its being possible to simply assume the **level 5** conjecture of comprehensibility, we can now see that once this process of generalization has been undertaken, we are rationally entitled to hold the comprehensibility thesis (and the higher, more generalized levels that support it) as a permanent and unrevisable item of scientific knowledge unless or until we come to know differently.

From this it becomes obvious why, below the level of the comprehensibility conjecture, aim-oriented empiricism requires some further conjecture(s) about the precise nature of ultimate reality. To begin with the establishment of the comprehensibility conjecture as a legitimate part of scientific knowledge has been accomplished through a process of generalization from the lower level of scientific method. There is a legitimate relationship between the levels. Moreover, to have demonstrated the legitimacy of postulating complete comprehensibility in some form or other is necessary for the growth of knowledge - but it is not sufficient. The key point to remember about the comprehensibility thesis is that it postulates that the universe is fully comprehensible in some way or other. The fact that it is vital to articulate more precise (**level 4** and **3**) ways in which the world is conjectured to be comprehensible is most easily demonstrated using Maxwell's definition of the thesis of comprehensibility, or **C**, which is to be understood in the following terms:

> **The universe is such that it has two aspects, U and V. U is present everywhere, at all times and places, throughout all phenomena, in an unchanging form; V varies from place to place, and changes from time to time. Furthermore, the diverse ways in which V can change are precisely determined by U, so that were V to change differently from the way it does, this would mean that U does not exist at all. Given U and the state of V everywhere at any instant, all subsequent states of V are precisely determined (or probablistically determined, given probablism). U and V are dovetailed together, in that V must change in the way in which it does everywhere, for U to exist anywhere!** [3]

This is fairly unspecific and leaves open all sorts of possibilities for the nature of **U**: **U** could be God, or a society of gods, or a unified pattern of physical law and there are all sorts of possibilities as to how it might determine variations in **V**. A more precise metaphysical blueprint, or a hierarchy of them, detailing more specifically the way in which the universe is comprehensible, is unavoidable because it is only from blueprints which postulate concrete states of affairs that there is any hope of drawing non-empirical methodological principles and testable theories.

However, at this point another set of problems emerge: whether or not knowledge is improved may well depend on how good a 'fit' there is between the theories drawn from this level of cosmological assumption and objective reality. If the comprehensibility thesis is true then, on the face of it, the situation looks hopeful because the comprehensibility thesis implies that precise knowledge concerning the evolution of any fragment of the universe is bound to contain some (perhaps very sparse) knowledge of **U**, which is invariant throughout all phenomena. This, in turn, makes it possible to know - in principle - all the ways in which states of affairs can differ and how these different states of affairs can evolve.[5] To this degree the truth of the comprehensibility thesis guarantees that the universe is partially

[5] **Indeed, if there is precise knowledge of the evolution of a bit of the universe then the full nature of U is known!**

knowable to us. Unfortunately, on closer inspection the situation looks unpromising: we are not yet in the position of possessing anything more than partial and fragmentary knowledge of the evolution of any portion of the universe, not excepting our own particular locale. We suffer from an apparently inescapable ignorance about the ultimate nature of the cosmos which seems to render impossible any hopes of our making a good choice of cosmological assumption, or blueprint, concerning the ultimate nature of the universe. The comprehensibility thesis may be the best available bet for the acquisition and improvement of knowledge, given the other options but it cannot, of itself, guarantee that the universe is even partially knowable to us.[6]

There are, notwithstanding, ways of finding out the best possible assumption about the ultimate nature of the universe, whilst remaining ignorant of the nature of ultimate reality. One way might be to create rival cosmologies (or adopt pre-existing ones) which postulate that the world is comprehensible in some way or other. The primary way to chose between such rival cosmologies is to work out the methodology associated with each and then see which will best improve and promote the growth of knowledge, when judged in implicit common sense terms. A better way, when in the position of already possessing a considerable body of scientific knowledge, is to implement aim-oriented empiricism, articulate the cosmological presupposition that is implicit within current scientific methodology and thus invoke the (by now familiar) hierarchy of cosmological assumptions from it. At each level in the 'family tree' the assumption is adopted which holds out the greatest hope for the growth of knowledge and seems best to support it. I earlier made two claims, firstly that cosmological speculation is indispensable to the rational procurement and employment of knowledge, citing the role which it always plays in any methodology which purports to govern the gathering of knowledge about the physical world, and secondly that aim-oriented empiricism is a more rational and rigorous conception of science because it insists that such assumptions be made an explicit and unrevisable part of scientific knowledge. Now it is possible to see how both these claims are vindicated by the process of generalization. The process recognises that there is no absolute guarantee at any level that

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[6] **After all, U might not be knowable, or comprehensible to us! This is still a possibility unless the truth of the comprehensibility thesis, by definition of 'comprehensibility', excludes it.**

the universe is comprehensible to us. Instead the process demonstrates that there is a worthwhile procedure which can show that the comprehensibility thesis is the best bet for the growth of knowledge and which can be further utilized to articulate the best possible **level 4** and **3** assumptions about the ultimate nature of the universe.

So far the aim-oriented empiricist matrix has legitimized the comprehensibility thesis, offered a means of blueprint articulation, and demonstrated that blueprints have legitimately related methodologies. At this juncture it is necessary to understand that although **levels 1** to **10** form a legitimate, logically-related whole, they are not unified in all their aspects. This is because empirical refutation has a role to play at **levels 1** to **3**. Clearly, evidence has a fundamental role at **levels 1** and **2** but historically even **level 3** blueprints have been falsified empirically, as when the corpuscular blueprint, which only permitted repulsive forces by contact, was falsified by the observation of attractive forces at a distance. Part of the reason for accepting cosmological ideas at **level 4** and above is that they are more empirically fruitful than any rival but they are not open in quite the same way to straightforward empirical refutation. It is possible that a decisive empirical proof of the existence of, for example, ESP or telepathy <u>could</u> amount to a refutation of physicalism. The empirical success of a research programme based on some **level** 7 thesis dealing with comprehensibility could even amount to empirical considerations counting <u>against</u> the **level 5** thesis. However, such eventualities, although in principle possible, are considered to be fairly unlikely. It is from the **level 3** blueprint downwards that conjectures, cosmological assumptions and associated methods are <u>changed</u> whenever those that are current either fail to support the growth of knowledge or are less successful in this respect than rival assumptions and methods. From **level 4** upwards the matrix of cosmological speculation is held as a quasi-permanent item of scientific knowledge: the lower level cosmological assumption or metaphysical blueprint which purports to gain a purchase on reality is, with its associated methodology, endlessly revisable.

To put it another way: **Lr** implies **Lr + 1** for **r = 3 ... 10** <u>but the reverse is not the case</u>. **Level 3**, the current best blueprint, has more content that **levels 4 ... 10** and is therefore most likely to be false. It is the highest level which is, in principle, susceptible to empirical falsification. Mostly, however, **level 3** blueprints can be tested with regard to their empirical adequacy and are likely to be false, so that progress in theoretical physics requires a succession of revised versions of the best blueprint.[7] **Level 4** is still less likely to be false (although it still could be) but **levels 5 ... 8** are increasingly contentless and the likelihood of their needing revision becomes increasingly remote, although not completely beyond the bounds of probability. However, **level 10**, and to all intents and purposes **level 9** as well, are completely unrevisable. The top of the hierarchy must be upheld independently of empirical considerations as it is the only means of excluding empirically progressive, *ad hoc* cosmologies. In the absence of this the aim-oriented empiricist framework would collapse.

The Operation of Aim-Oriented Empiricist Science

If we now look at the situation in more detail we will see that at the point where <u>any</u> **level 4** metaphysical blueprint is accepted, it is possible to see that a particular set of conditions obtains. We have already covered the necessity of articulating the best version of the metaphysical blueprint in order to obtain a sufficiently precise methodology and this applies for all **level 4** blueprints. For example, a primitive people, only too aware of their own mortality and believing in a **level 4** multi-theistic version of personalism, would have to decide on the best means of approaching and interacting with all-powerful, unpredictable, immortal beings on whom they believe themselves to be so dependent. In this matter the dead might have a role to play: perhaps their souls, on leaving their bodies, go to live with the gods, or perhaps they go to some intermediate place between the earth and the abode of the immortals. Either way they would be in a better position to interact with deities than are mere humans and, having been human themselves, are naturally more amenable to

[7] **Maxwell has argued the point thus:** *'Each of these blueprints can, however, be interpreted as <u>generalizing</u> its predecessor (somewhat as Riemannian geometry generalizes Euclidean geometry), as long as they are all interpreted as specific versions of physicalism. Thus the corpuscle idea that there is an infinitely repulsive force located on the closed, rigid <u>surface</u> of each corpuscle is generalized by Boscovich into the idea that there is an alternatively repulsive and attractive force which varies in a fixed way throughout a <u>volume</u> about a central point-particle. This blueprint requires that changes be transmitted instantaneously from point-particle to point-particle through space. More generally, the velocity of such transmission may be <u>finite</u>, which means the state of the force-field around each particle will vary (depending on the past motions of the particle). In order to take this case into account, we may coalesce all the diverse force-fields of distinct particles together to form <u>one</u> force-field, created by, and acting on, point-particles. This point-particle/field blueprint, associated with Faraday, Maxwell and Lorentz, may in turn be modified by eliminating the particles and insisting that the field <u>interacts</u> with itself, small, intense regions of the field standing in for point-particles. This, in essence, is Einstein's unified field blueprint.'* **See Maxwell, N.,** *From Knowledge to Wisdom*, 239.

approaches from humans. Consequently, a more specific version of the blueprint could be formulated delineating, with the greatest possible precision, the relationships between the corporeal and ephemeral beings, anxious to understand and (as far as possible) control their environment, and the all-powerful, governing hierarchy of sempiternal beings of which the lowest level comprises human ancestors. For a society holding some **level 4** version of physicalisim the same principles apply. A universe in which, for example, **U** is impersonal and unchanging but possessed of dispositional properties which are responsible for all diversity and change, needs to be made more precise. Perhaps **U** might be characterized by means of some physically interpreted mathematics from which (in unison with initial conditions), descriptions of that which varies can be deduced. In both cases it is clear that the best version of the metaphysical blueprint determines the current non-empirical methods of knowledge-gathering. This, by now, is probably instinctively obvious: it is the correlative of the fact that one can discover implicit cosmological assumptions in any methodology which is used to acquire knowledge about the world of nature. In the system of knowledge-gathering which we know as science, the non-empirical methods are those methodological rules used in formulating and assessing a series of theories.

A new blueprint also clarifies the aim of the endeavour and illuminates the nature of the relationship between aims and methods. The aim of any scientific endeavour within an aim-oriented empiricist framework is the elucidation of the basic blueprint: the relationship between the aim and the method is that the method, by trying to formulate and confirm a series of physical theories, which successively apply to ever-widening ranges of phenomena with ever-increasing accuracy, tries to move more closely to the aim of predicting and explaining phenomena in terms of the governing blueprint. In other words, aim-oriented empiricism transforms the notion of relationships into a concept of evolving interplay between metaphysical problems and developments, methodological problems and developments, and empirical problems and developments. This is the mechanism by which revision takes place at blueprint **level 4** and below. The careful elucidation and delineation of these levels recognises that if the blueprint is critically assessed and reformed in some way, then this must cause the aims, the methods, and the scientific knowledge all to evolve in their turn. Equally, if there is reassessment at, say, the conceptual level, then the blueprint too must be involved in the resulting adjustments. Even alterations at the level of observation and experiment can, as it were, percolate upwards and effect changes right up to the highest level of the metaphysical blueprint. In fact as an evolving metaphysical blueprint/theoretical/empirical framework (rather than one which either ignores metaphysical considerations or adopts a fixed metaphysical conjecture and fixed methods) aim-oriented empiricism performs a unique function: it offers the possibility of improving knowledge about how to improve knowledge, which is another way of saying that it provides the means to improve methods for improving knowledge. So although **levels 1 ... 5** are arranged in a hierarchy which recognises the supreme importance of the blueprint, it is emphatically not the hierarchy familiar from *a priori* deductivist systems, with their uni-directional, step-wise descent and their (often doubtful) resemblance to Euclidean axioms and postulates. Nor, indeed, is it a circular system, nor one which holds that the only rational justification for accepting a theory is empirical success. In fact it is an evolving framework, governed by meta-rules which specify,

> **how we should seek to modify each component to bring it into better accord with the others in such a way as to improve our knowledge and understanding of the world.** [4]

What such a framework produces is not an algorithm, a set of rules to be mechanically applied in order to determine rational choice, but a fallible and yet entirely rational method of discovery. It is a consequence of the more rational and rigorous principle of intellectual integrity, and of the generalized version of aim-oriented empiricism which it occasions, that such rules are impossible: within the matrix decisive proofs of the chosen assumptions at **levels 4 ... 10** are unattainable and in practice this generally extends to levels **3 ... 10**. Intellectual thoroughness requires that the highly fallible and conjectural character of all the assumptions be recognized and the implementation of the principle of integrity ensures that this happens. Simultaneously, in conjunction with the aim of improving knowledge, the principle shows how one of these fallible assumptions may nevertheless be chosen as a part of knowledge. The rationality of science therefore does not demand the rule-governed determination of scientific choice but rather the acknowledgement of the genuine fallibility of the choices that have been made. Scientific rationality in an aim-oriented empiricist framework is what emerges from the explicit articulation and constant critical assessment of all the interlocking components of the framework and it is precisely this kind of progressive, evolutionary assessment that turns the whole framework into a method of discovery. Indeed, in aim-oriented empiricist terms it becomes necessary to think not of the rationality of scientific justification but of the rationality of scientific discovery.

Going Beyond Popper

There are certain Popperian principles involved in the claim that an aim-oriented empiricist framework is necessary for the production of a *'rational, even if fallible and non-mechanical, method of discovery'*.[5] This is hardly surprising because aim-oriented empiricism was developed as a series of revisions to Popper and the result is a new conception of rationality and scientific method. The first major revision was the inclusion of metaphysics as a part of scientific knowledge and the linking up of metaphysics and methodology. Aim-oriented empiricism goes to great lengths to demonstrate that the reason why cosmological assumptions are implicit in any knowledge-gathering methodology - but particularly in scientific methodologies - is because they play such a seminal role in the acquisition and use of knowledge. An examination of the generalized aim-oriented empiricist matrix shows that such assumptions, once they have been carefully worked through according to the principle of intellectual integrity, can be held as a legitimate and quasi-permanent part of scientific knowledge. Popper considers metaphysics to be unscientific and would certainly exclude it from the methodology, or anything to do with theory assessment. The second major revision was to make the metaphysics and the methodology into an evolving framework, whose interactions produced a meta-methodology for assessing rival possible modifications of aims and methods in the light of evolving knowledge. What emerges from the interplay between aims, methods, and new knowledge is an evolutionary process which, in essence, produces a methodology for improving methods for the improvement of knowledge and also represents a new concept of rationality. Aim-oriented empiricism holds that if rationality is to be fundamentally critical then all aspects of the framework, from the requirement of partial knowability downwards, must be explicitly formulated as a part of scientific knowledge and that from the **level 3** metaphysical blueprint downwards the interlocking parts of the framework must be constantly criticized too. The resulting state of intellectual rigour provides the motive power for the evolutionary framework. Clearly this is not Popper's conception of rationality as criticism, with falsification representing the most reliable way of being critical, and so it obviously counts as a revision. However, it still contains a surviving Popperian principle: it shares with Popper the notion that the role of reason is a fundamentally critical role, that criticism really does lie at the heart of rationality. It also shares the notion that we need to be critical of criticism. The whole point of criticism is to promote the growth of knowledge: where criticism cannot perform this task, at **levels 9** and **10**, it ceases to be rational. The second Popperian principle survives in aim-oriented empiricism's commitment to the problem of the growth of knowledge, which Popper saw as the real problem of knowledge, exemplified by its concern to improve methods for improving knowledge. The third Popperian principle may be discerned in the aim-oriented empiricist emphasis on conjecture and the necessity of criticising the blueprint, which implies that at any given stage the current blueprint is almost certainly false. Popper acknowledges that science, the most rational activity in the history of human intellectual endeavour, has been wrong all the time. His archetype of rationality has dealt endlessly with error. Aim-oriented empiricism accepts this view through its commitment to the belief that ultimately all knowledge, even the assumption that the universe is partially knowable, is conjectural in character. Lastly, an entirely un-Popperian element in aim-oriented empiricism is that justification is both legitimate and necessary: the chief justification for acceptance of **levels 3** to **10** can be summed up as, 'what holds out the best promise for the growth of knowledge'. Justification is something which comes about via growth, or the promise of growth, or through being necessary for growth. This is completely the opposite to Popper's notion of rejecting justification in favour of growth. The generalized aim-oriented empiricist matrix shows exactly why we are entitled to hold the comprehensibility conjecture if we want the growth of knowledge to be a rational possibility. The extent of the importance of these revisions to Popper - and the fact that they represent a new departure which negates the possibility of aim-oriented empiricism's being dismissed as simply quasi-Popperian, will become apparent in the next section, particularly after the discussion on the problem of induction.

The Illuminative Strengths of Aim-Oriented Empiricism

The important point to establish, so far as this chapter is concerned, is that aim-oriented empiricism can justify its role as an interpretative framework for the history of science. So far arguments have been put forward to the effect that there is no one historiography of science. The idea that there is entails that there is but one meaningful way of interacting with the past, a single methodology, or set of methods, for getting the one purchase on the subject that can count as meaningful. However, intellectual integrity alone requires that there are a range of possible perspectives on the past, including those that are concerned with the progress and development of scientific ideas. It has been demonstrated, quite simply, that different basic goals for science will naturally lead to different progress histories of science, An examination of the role of evaluation, in choosing the goal and assessing the success or failure of the endeavour in tending towards that goal, does not entail imposing anything on the past or being drawn into the form of the explanation. Progress history, it would seem, is entirely possible without aim-oriented empiricism. However, where a progress history of scientific ideas is concerned (and whilst not going so far as to claim that it is the only route to formulating a satisfactory history of scientific progress) aim-oriented empiricism does hold out some distinct possibilities. The claim that will be substantiated in the remainder of this chapter is that aim-oriented empiricism has an inherently historical character which is such that it can make rational sense of a progressive history of scientific ideas, without indulging in rational reconstruction or in any of the historiographic sins traditionally associated with Whig or progress history. We are not yet in the position of being able to ratify any of these objectives. So far we have made two major affirmations. Firstly, as a result of making explicit, and so criticizable and improvable, problematic metaphysical assumptions about the (more or less precise) unity or comprehensibility of the universe, aim-oriented empiricist science is intellectually more rigorous than any rival conception of science that is primarily shaped by an attempt to implement the principle of empiricism. Consequently, aim-oriented empiricism represents an improvement over conceptions which leave problematic metaphysical assumptions implicit, unacknowledged and undiscussed within science. Secondly, we have demonstrated, although not thus far attempted to evaluate, the significant fact that the generalized aim-oriented empiricist matrix generates a new theory of rationality, which emerges as the result of the repeated application, as required, of the principle of intellectual integrity to all of the interlocking components of the evolving framework. Aim-oriented empiricism shows not how science is but how it must be if it is to be ideally rational, such that, if put into practice, it would be always conducive to producing scientific progress. In order to move the investigation forward to the point of being able to fulfil the main aim of the chapter, which requires the affirmation of the claim that the rational pursuit of knowledge has a historical dimension (which in turn implies that a perfectly correct way to legitimize or justify scientific method is to examine the search for scientific knowledge from a historical perspective), it is necessary to show that aim-oriented empiricism suffices to solve the major problem of the rationality of science.

This, of course, is the problem of induction. In the most general terms this is the problem of how we can be certain that the future will continue to represent the past, or the problem of how it is possible to be sure that a theory verified or corroborated yesterday will also be verified or corroborated tomorrow. Every instance of observational evidence, once we have obtained it, is of necessity evidence about the past. In Hume's classic formulation it is the problem of the inferential gap between the particularity of inductive evidence and the generality of inductive conclusions: a denial, in other words, that there exists a process of reasoning which enables general truths to be established from particular instances. The inductive principle - and the form of argument it engenders and the sort of moves allowed by those arguments - is of an entirely different species to the deductive principle and its arguments and moves.[8] Deductively valid inferences move infallibly from the truth of the premises to the truth of the conclusion but it is a process in which the conclusion cannot assert more than is asserted in the premises: deductive logic produces true conclusions but it cannot increase the content of what is known. In inductive reasoning the conclusion does assert more than is asserted in the premises, thus increasing the content of what is known,

[8] **Deductive reasoning, which is what Hume called 'reasoning about relations of ideas', is, of course, concerned with the connections between concepts such that the truth of the premises is enough to provide a logical guarantee of the truth of the conclusions. Given a statement such as, 'a cricket ball is a solid sphere', there can be no difficulty in seeing that it is possible to deduce from it the statement, 'a cricket ball is not a cube'. To assert, simultaneously, that it is both a cube and a sphere, would be to assert a contradiction.**

but the truth of the premises is not sufficient to provide a logical guarantee of the truth of the conclusion. Hume called this 'reasoning about matters of fact'.[9] Logically speaking any event can follow any event. Traditionally, the end result of all this has been taken to be a frustrating one. Firstly, the empirical record for the generalizations accepted by science is solely concerned with evidence about the past. Secondly, deductive logic, with its guarantee that a true conclusion will be produced, is not content-increasing. It cannot follow deductively, from any amount of evidence, that accepted generalizations will continue to hold in the future: it is impossible, using deductive means, to make a content-increasing inference from a statement describing one event (or series of events) to a statement describing another. Such a move is possible using an inductive inference but that form of reasoning does not provide a logical guarantee of the truth of the conclusion, it does not bridge the deductive space which exists between the past evidence gathered about a given theory and whatever future evidence may reveal. Following Hume, it has been generally concluded that the problem of induction is the problem of how we can justify making inductive arguments at all, given their general invalidity. Strictly speaking, this was not how Hume saw the problem, for he was more interested in seeing how inductive arguments worked, rather than with whether or not they could be justified, but it is the problem of justification which has preoccupied his successors. The ramifications which this has for judgements about the rationality of science, and the responsibility of using scientific knowledge as a basis for action whether it be the progress of science through history or the state of science today, are well-known.

It is probably true to say that most philosophers of science hold that the problem of induction will not admit of a solution. We may recall, however, that in Chapter One it was established that cosmological assumptions are implicit in factual knowledge such that there is a sense in which any factual knowledge about anything contains or implies some knowledge about <u>everything</u>. This may be fairly insubstantial knowledge, of the order, for

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[9] From the observed fact that the spherical cricket ball is now heading towards the greenhouse at a high speed and on an uninterrupted trajectory, it is possible to reason to the conclusion that at least one pane of glass in the greenhouse will be shattered <u>but it is impossible to deduce it</u>. There is no logical incompatibility between the present airborne state of the ball and the conclusion that no glass will be broken because there is no logical reason why, for example, the world should not disappear and be replaced by a new one (in which different physical laws obtain) before the ball completes its journey.

example, that the existence of wood implies that the universe is so constituted that it permits the existence of wood, but it nevertheless is cosmological knowledge. In the early part of the present chapter, utilizing this notion of cosmological knowledge, it was asserted that the generalized aim-oriented empiricist framework, composed of increasingly contentful metaphysical assertions from **level 10** down to **level 5** which are selected according to certain methodological principles, supports the **level 4 P** concept that there exists something, U, which determines (deterministically or probablistically) the way in which events unfold. It was also established that some presupposition about the nature of the universe is always built into the methods of acquiring knowledge about nature, which not only implies a reliance on causal connections but also implies that the methods rely on the inherent completeness of the causal system. Do not these assertions contradict the established, Humean position on lawful regularities? Maxwell has argued,

> **unless we can make sense of the idea that there is something *that actually exists* which, in some sense, determines or is responsible for lawfulness, it is difficult to see how the lawfulness that physics has revealed in the world around us can possibly be intelligible. It would become one gigantic, wildly implausible cosmic accident. The more physics explains, by showing how restricted regularities are consequences of more widely obtaining regularities, the more *inexplicable* the existence of these regularities becomes.** [6]

By employing the idea of a property determining change, Maxwell has shown how there can be necessary connections between successive events, thus refuting Hume. He argues that almost all physical properties are dispositional, or necessitating, and therefore determine how things change.[10] This is true of common sense properties like rigidity, elasticity, solidity, and inflammability and of theoretical properties like gravitational charge, mass, or electric charge. Of course, a necessitating property like inflammability, used in an explanation for the fact that a given body burns when in the presence of a naked flame, is both restricted and inadequate. A more adequate explanation would need to invoke more fundamental, possibly theoretical, necessitating properties common to a much wider range of physical entities, to explain what constitutes the property of inflammability. Such

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[10] **This position is called 'conjectural essentialism', which maintains that it is legitimate to interpret (appropriate) physical theories as attributing necessitating properties to postulated physical entities, as a result of which the entities must, necessarily, obey the laws of the theory.**

necessitating properties are the charge and spin attributed to electrons and nuclei. Theoretical quantum mechanical explanations are based on very few necessitating properties being attributed to different kinds of entity, but they can be applied to a vast range of phenomena. When quantum theory is applied to chemistry, inflammability proves to reside in the possession of a molecular structure, so composed that at the concurrence of sufficient oxygen and a high enough temperature, oxidation progresses speedily and in a self-sustaining manner, emitting gas, heat and light in the process.[11] The general conclusion is that conjecturally essential physics is able to explain why regularities exist in nature: they are simply the necessary outcome of physical entities possessing appropriate necessitating properties.[12]

The Principles of Uniformity

Before we proceed any further we should perhaps be clear in our own minds that an acceptable solution to the problem of induction must not involve an appeal to the principle of the uniformity of nature as it is ordinarily understood, i.e., the idea that nature is uniform and subject to universal and unchanging laws. There are decisive arguments which can be levelled against any appeals to such a principle. Firstly, there is the problem of circularity: the principle of uniformity is justified by an appeal to the success of science and the success of science, in its turn, is justified by an appeal to the principle of uniformity. This was recognised by Hume, who pointed out that, if we assert that nature is uniform, we cannot argue that the uniformity of nature is verified by experience without assuming the very principle we are seeking to verify.[7] Secondly, even were we to somehow come to know that nature is indeed uniform, the problem remains that given any empirically successful

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[11] **Of course it is still a logical possibility that the world is so constituted that all the disparate array of observational necessitating properties that appear to us, nevertheless exist in the absence of an underlying mechanism. In other words there could still be nothing in whose terms a theoretical explanation of the necessitating properties might be set. Therefore, to ascribe the property of inflammability to an entity is merely to leave open the possibility of an explanation couched in terms of more fundamental and widely possessed properties. If the possession of a necessitating property is dependent on there being a theoretical explanation to be discovered then it is not possible to interpret fundamental physical theories in terms of attributing essentialistic properties to postulated entities.**

[12] **Nick Maxwell has written upon this subject for the past thirty years. The earliest publication was** 'Can There be Necessary Connections Between Successive Events?', *BJPS 19*, 1968, 1-25. **The most recent publication is** *The Comprehensibility of the Universe*, Oxford: Oxford University Press, 1998.

theory **T**, there will always be infinitely many rivals, $\mathbf{T_1}$-$\mathbf{T_n}$, which will fit all the available data just as well - in some cases perhaps better - but which disagree for some as yet unobserved phenomena. These rivals will also be compatible with the principle that the same set of physical laws **(L)**, invariant with respect to space and time, govern the way events unfold at all times and places. This will be true whether we seek to verify theories, or falsify them: the scope for fabricating divergent variants of any empirically fruitful theory is endless. Thirdly, it is not the case that such scientific knowledge as we have so far accumulated would not be possible unless all phenomena were governed by the same laws everywhere. One can postulate universes in which scientific knowledge could still be possible, even though the uniformity principle might, in any ordinarily understood sense, be false. Such a universe might be comprehensible because a deity (or Deity) is in ultimate control: in this sense there would be something both uniform and invariant - God's Will or the will of the gods - which is some sense is responsible for all the change and diversity in nature but which does not manifest itself in the sort of uniform laws which are normally taken to be indicative of the uniformity of nature.

These arguments appear, on the face of it, to destroy the idea that an appeal to the uniformity of nature will solve the problem of induction. If the aim-oriented empiricist solution to the problem of induction is to be taken seriously, it must demonstrate that it does not depend on this sort of strategy. Aim-oriented empiricism's solution to the problem of induction rests in that hierarchy of increasingly contentless assumptions about the nature of the universe which has already been shown to be (as a consequence of satisfying fully the principle of intellectual integrity in the criticism of all substantial, influential, problematic assumptions) the solution to the 'problem of integrity'. This generalization of aim-oriented empiricism, we may remember, progresses through increasingly contentless (and thereby increasingly less problematic) assumptions as to how the universe is, although not fully comprehensible, at least partially knowable (**levels 6 - 10**). At each level all rival assumptions must be considered but the one to be adopted is the one which, we conjecture, offers the best chance of promoting progress in knowledge. It must hold out greater hope of promoting the growth of knowledge than any rival (whether true or false), or than any rival (if true). Moreover, it must do better justice to apparent scientific progress than any rival and it must be inherently more likely to be true than any rival, even if only because it has less content.[13] We may further remember that this process involves the recognition of the fallible character of all assumptions, particularly the lack of decisive proof at **levels 4 - 10**, and the recognition that rationality requires us to accept the genuine fallibility of the choices that we make. This is important because any solution to the problem of induction must include the unavoidable lack of decisive empirical proofs, the possibility that the universe might not be comprehensible in quite the way we think it to be, and even the possibility that the universe might not, after all, be comprehensible. The ten levels of generalized aim-oriented empiricism can then be used to formulate the induction theorem: label the levels so that **H1** represents evidence, **H2** represents fundamental physical theories, **H3** represents **level 3** blueprints, **H4** represents alternative ways in which the world might be comprehensible, all the way up to level **H10**, or partial knowability. Having done this we are justified in accepting **Hr**, given **H1** and **Hr+1**, for **r = 2, 3 ...9**, as at least the best available bet, given the other options at this level, and our (seeming) successes and failures so far, even though it is by no means a certain bet and the danger remains that it might prove wrong and turn us in the wrong direction. Yet this formulation does not involve the 'top and bottom' of the theorem, so is there any way in which they can justifiably be included? Our justification for accepting **H1** is that it is presupposed by the problem of induction: the problem would not arise if we did not accept that we do have knowledge of particular matters of fact. Our justification for accepting **H10** is that in making this assumption we have nothing to lose, whatever the truth of the matter may be, and we may stand to gain a great deal. Having established this we can see that we are, in fact, justified in accepting **Hr, r = 1 ... 10**.

Now before we consider the problems associated with the principle of uniformity it should, to begin with, be pointed out that the principle itself is not without its own problems. An aim-oriented empiricist perspective indicates this through its central tenet that we need

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[13] **Of course, there is the immediate possibility of conflict here. The more an assumption indicates its capacity (if true) with regard to the growth of knowledge the more it is likely to assert, and *vice versa*. However, the assumption which indicates its capacity to help the growth of knowledge, if true, over all rivals and which is still inherently more plausible than all rivals, is an extremely strong assumption and doubly worthy of adoption. It might even be argued that the comprehensibility thesis gains much of its *a priori* appeal precisely because it is possible to formulate from it this kind of extremely strong assumption.**

to learn in what way the world is comprehensible as we proceed. In the light of this consideration a uniformity principle which, for example, considers lawful regulations but ignores whatever it is that lies behind such regulations, is too narrow and restricting. Equally, if the uniformity principle is defined as 'uniform with respect to space and time', then it can be too loose a definition; it is in principle possible to have a theory that, whilst satisfying translational invariance, violates rotational invariance. Thirdly, the argument that the uniformity principle is not vital for the acquisition of knowledge indicates that the principle could, in some way, be involved in the notion of comprehensibility. Yet it cannot just be that the idea of the uniformity of nature is a special case of the **level 5** notion of comprehensibility, that the universe is comprehensible in some, as yet unspecified, way. **C** itself is part of (and dependent upon) a hierarchy of increasingly contentless assumptions: to suddenly introduce the uniformity principle as a special case of **level 5** would be an arbitrary move. Finally, the principle of intellectual integrity, the principle which aim-oriented empiricism asserts needs to be put into practice if we are to give ourselves the best chances of making progress, demonstrates that a kind of uniformity principle is actually implicated in the methods of science itself. We have seen that the principle has to be implemented if science is to make progress because it requires that that which is substantial, influential, problematic and implicit be made absolutely explicit. The assumption routinely made about the nature of phenomena which fulfils these criteria - and in whose absence science performed in accordance with the principle of empiricism is doomed to drown in a great tidal wash of infinitely many aberrant theories - is that the phenomena are non-aberrant. The presupposition about the nature of the universe that is always built into the methods of acquiring knowledge about nature, which is why science always proceeds as if reliant on the system being causally connected, is that nature is non-aberrant. The methods of science makes a persistent and massive assumption about the nature of the universe, quite independently of empirical evidence, that also functions as a sort of uniformity principle. Aim-oriented empiricism suggests possibilities which tend to suggest that it is not the principle as such which is flawed but the manner in which it has been traditionally understood. It might be the case that the aim-oriented empiricist framework can encompass the principle of uniformity in a new way which avoids problems and takes account of implicit suppositions.

In fact, it does precisely that. Aim-oriented empiricism's solution, as is only to be expected, lies in the hierarchical perspective: the principle of the uniformity of nature can be formulated as a multi-levelled principle, an ordered set of principles which correspond to the steps in the generalized aim-oriented empiricist framework and consequently increase in content from **level 10** descending to **level 3**. Each of the (by now) familiar levels strengthens the claims made and each can be legitimately assigned a different justification. Once the possibility of a gradation of uniformity principles is entertained it becomes easier to see that, as traditionally formulated, the principle of uniformity has been required to perform an impossible task. It has needed to be strong enough to exclude empirically successful aberrant theories of the type particularly associated with **Level 3** *and* sufficiently weak to be justified on the grounds that otherwise knowledge is impossible. It has needed to avoid the fallacy of circularity and also find some means of bridging the deductive gap between the past and present performance of a given theory.

Aim-oriented empiricism, with its eight graded uniformity principles, can offer a solution to all of these problems. Uniformity principles accepted at **levels 9** and **10** constitute the weakest versions. At **level 10** the principle of uniformity is highly restrictive and qualified, merely stating that the universe is so constituted that the acquisition of explanatory knowledge, sufficient to make partially successful action feasible, is possible. This allows that we have a very meagre amount of knowledge about the entire cosmos, to wit that the universe is such that it makes our particular knowledge possible. At **level 9** the idea of non-maliciousness functions as a weak uniformity principle insofar as it holds that whatever aspect it is that makes our immediate environment partially knowable extends throughout the whole cosmos. Therefore any aberrant phenomena in the cosmos must be discoverable by us in our immediate environment. These principles are justifiable on the grounds that, given that the aim is to acquire explanatory knowledge, there is nothing to be lost by assuming that they are true. In accepting them it becomes possible to solve two of the three problems stated above. Firstly, these principles have nothing to say about what kinds of possible universes there might be in which knowledge can be acquired. They do not assert that all phenomena are governed by the same laws everywhere, or even that approximately lawful phenomena occur everywhere. They are sufficiently weak to be justified in terms of its being impossible to acquire any knowledge without them. Secondly, the fallacy of circularity has been avoided because in justifying these highest level uniformity principles, no appeal has been made to the success of science. Any such appeal would demand the truth of non-maliciousness and would thus become circular. Of course, the success, or apparent success, of science is invoked at **levels 3** to **7** because at these levels the choice of uniformity principle is made from the standpoint of promoting the growth of empirical knowledge. However, that is never the only consideration because the **levels 3** to **7** methodological and metaphysical principles must also be fulfilled. This can be brought out with regard to the aim-oriented empiricist solution to the third problem, that of infinitely many rival theories which will fit all the available data. Aim-oriented empiricism upholds, at **levels 3** and **4**, uniformity principles which are more rigorous than the traditionally formulated requirement that laws should be uniform in space and time. At these levels the uniformity principle is strong enough to select, whenever necessary, those fundamental dynamical theories that best serve both the evidence and the metaphysical and methodological requirements at **level 3** or **4** - it is capable, in other words, of excluding empirically successful, aberrant theories. At **level 5**, aim-oriented empiricism upholds a uniformity principle which is strong enough to justify the assertion that the universe is sufficiently unified for there to be a knowable, underlying something, which is ultimately responsible for all change and diversity. This uniformity principle is weaker than the traditional formulation because the postulated entity need not be physical and need not manifest itself in uniform laws. The inescapable conclusion is that a single uniformity principle cannot simultaneously perform all the tasks that aim-oriented empiricism reveals are required of it. The eight distinct uniformity principles associated with the generalized aim-oriented empiricist framework can and that is why they are not susceptible to the arguments which undermine all attempts to solve the problem of induction by an appeal to a single principle of uniformity.

The Solution to the Problems of Induction

So the basic aim-oriented empiricist idea, that we are justified in assuming that it is possible for us to acquire knowledge <u>and</u> improve it by improving methods within a framework of fixed meta-methods, does not entail an appeal to the principle of uniformity and renders the problem of induction solvable. There are at least two parts to the problem of induction, the methodological problem and the justificational problem. The methodological problem is prior in so far as its solution requires that the methods in terms of which science selects theories in the light of evidence should be specified. The justificational problem not only requires that the methods be specified but also needs to justify the claim that theories chosen in this way do indeed constitute knowledge (in some acceptable sense of 'knowledge'). It should be pointed out that all arguments concerning this solution to the problem of induction are fully developed by Nick Maxwell in chapter five of his book, *The Comprehensibility of the Universe*, to which the reader is, of course, referred.

The Methodological Problem of Induction

To begin with the methodological problem of induction, which qualifies as being problematic on two fronts. Firstly, there is the well-documented problem that the methods of science change over time, within individual sciences, and are clearly different between different disciplines such as the life sciences and the physical sciences. There is no such entity as '<u>the</u> scientific method'. Secondly, because of the complications inherent in attempts to specify what simplicity or explanatoriness is, there is the difficulty of specifying those methods which select the simple and explanatory in preference to the complex and non-explanatory. The first problem is easily solvable in terms of the generalized aim-oriented empiricist framework. By postulating a link between the aims and methods of science and the fallible metaphysical assumptions concerning the comprehensibility of the phenomena (more or less specific in both cases), aim-oriented empiricism asserts that aims and methods must evolve with evolving concepts about comprehensibility. Aim-oriented empiricism both predicts and requires, as being essential for rationality and for scientific progress, that <u>methods do change</u> over time and between disciplines. The comprehensibility of the universe is something about which we progressively, if slowly, improve our knowledge and understanding as science makes progress, it being something of which we are originally ignorant. When ideas about comprehensibility develop, associated methodological ideas must likewise develop if they are to fulfil the primary aim of elucidating the best versions of that developing comprehensibility. Furthermore, the link established between aims, methods, and metaphysical assumptions, implies that disciplines which seek to explain and understand things in different sorts of ways must have different methods if they are to be rational.[14] Physics will have a particular way in which it must seek to explain and understand things, for which it will use appropriate methods, and psychology will have a different way. It may well be that the psychologists' body of knowledge is ultimately reducible to the physicists' more fundamental knowledge but each individual way of explaining and understanding things needs must have its own methods, if it is to be rational.[15]

As for the second facet of this problem, concerning the difficulty of specifying those methods which select the simple and explanatory in preference to the complex and non-explanatory, this too is solvable within the parameters of aim-oriented empiricism. An authentic, legitimate physical theory must, if it is the genuine article, have the ability to make predictions about infinitely many possible physical states of affairs. It must have this characteristic of encompassing factual and counterfactual phenomena if it is to count as a properly law-like statement and if it is to be used for practical, technological purposes. It is in the latter instance that it becomes peculiarly important. Genuine theories must be correct for a range of possible phenomena which may never occur in actuality: with regard to technological innovation they must apply to circumstances which would not have occurred but for our mediation. However, this same characteristic entails that a theory can make predictions for an infinite number of possible phenomena, infinitely variable in quite arbitrary and *ad hoc* ways. It is impossible to test the predictions for an infinity of possible

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[14] **The only fixed, universal methods which aim-oriented empiricism allows are those that operate at the metamethodological level. These govern the choice of metaphysical assertions in the generalized framework, including the choice of the much more specific, changeable methods governing theory-choice and linked to changeable** level 3 **ideas.**

[15] **If we stop considering the narrow scientific aims of the pursuit of knowledge and understanding and widen our deliberations to include the drive to increase control over nature by means of technology, it becomes obvious that the applied sciences, having somewhat different aims from pure, or more theoretical science, should have different methods.**

phenomena: no matter how many versions are experimentally refuted, infinitely many will always remain. It must, if this part of the methodological problem is to be solved, be at least possible to specify methods which lead to the exclusion of the infinity of these sort of aberrant, although quite possibly empirically successful, theories. The aim-oriented empiricist explanation is that preference for simple, explanatory theories in any science, at any given time, equates to a predilection for theories which conform with the best possible (so far articulated) metaphysical blueprint for that science. Criteria of simplicity and explanatoriness are linked to - and must change with - metaphysical assertions concerning comprehensibility. In this case too, because it is so closely related to the first one, aim-oriented empiricism predicts and requires for rationality that ideals of simplicity and explanatoriness, which are associated with metaphysical assumptions concerning the comprehensibility of the phenomena, change, in line with those assumptions, from time to time and between disciplines. This, of course, contributes to bringing about concomitant changes in the methods.

The justificational problem of induction has traditionally been formulated as the theoretical problem and the practical problem. The theoretical problem of induction is concerned with how to justify the acceptance of empirically successful theories, granted that the purpose is to acquire theoretical knowledge and understanding which is no more than speculative, or conjectural. The practical problem of induction arises because of the further requirement that theoretical knowledge should form a rational basis for action. It is concerned with the need to justify the acceptance of the scientific theories that, generally speaking, illuminate and imbue our technological praxis. This concern extends from the predictions of well-corroborated, high-level theories, of the type which, like Newton's laws, give every appearance of being a rational basis for action, but which the history of science has so often shown to be strictly false, to the predictions of commonly accepted, low-level generalizations that obtain at the level of applied science, technology, and even common sense. Both versions, of course, hinge on the need to justify the claim that theories chosen through the agency of (specified) methods do indeed constitute knowledge.

The Justificational Problem of Induction

Almost all attempts to solve the problem of induction regarding theoretical knowledge, in this century, have belonged to the empiricist tradition. Historically there were thought to have been two possible routes to a solution to the problem of induction, the other one being empirico-rationalism, which demands that some metaphysical principles are 'proved' by reason alone. However, empirico-rationalism, in so far as it appeals to synthetic *a priori* knowledge, proved inadmissible on general grounds.[16] Notwithstanding, the failure of the empirico-rationalist route does not, of itself, guarantee the success of the empiricist route: the latter is just as much open to criticism as the former. Furthermore it could be argued that depending upon what is meant by 'theoretical knowledge', a range of possibilities exists as to how far the problem of induction may admit of a solution even <u>within</u> the confines of empiricism. If, to qualify as theoretical knowledge something has to be <u>empirically proved beyond all possible doubt</u>, then the problem of induction <u>is</u> insoluble and there are no prizes for saying so. However, although this formulation is common, is it the only possible one? If we take Popper's conception of theoretical knowledge, which is that which has <u>survived</u> severe falsificationist testing, then although Popper failed to solve the problem of induction in these terms, it nevertheless is an easier version of the problem than the first one. If theoretical knowledge is defined as that which is <u>selected</u> according to Popper's falsificationist methodological rules for science, then (in so far as such rules are sufficiently restrictive and strictly applied) the problem is solvable. If theoretical knowledge is simply that which is <u>chosen</u> by the effective (implicit or explicit) methods of science, the methods that are current in scientific practice with regard to selecting theories as scientifically established, then the problem is obviously solvable. Unfortunately it hinges upon a tautology: theoretical knowledge already means 'current scientifically accepted theories'. This version of the problem is therefore trivially solvable and, again, there are no prizes on offer. From the fact that it is possible to formulate versions of the problem of induction in terms of the relative strengths of what 'theoretical knowledge' means, and

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[16] It proved inadmissible as it gradually became apparent that the quest to prove any substantial proposition about the world by reason alone appeared to be unattainable. In addition, nominees for such knowledge (for example, that space is Euclidean) were shown by the advance of science itself to be false.

prizes on offer. From the fact that it is possible to formulate versions of the problem of induction in terms of the relative strengths of what 'theoretical knowledge' means, and thereby demonstrate that weak versions can certainly, if trivially, be solved, we may infer that a solution to the justificational problem of induction is possible. The reputation which the problem of induction has for being insoluble stems, in all probability, from the widespread (if implicit) belief that a solution can only be had within the (untenable) framework of standard empiricism, which holds the empirical principle above all others. A further tendency to formulate the problem in the strongest possible standard empiricist terms, such that 'theoretical knowledge' is defined as 'that which is empirically proved beyond all possible doubt', reinforces this propensity. What is perhaps required is that the strongest solvable version of the problem be defined and a solution worked out, all stronger versions of the problem being shown to be unsolvable. Such a version of the problem would have to reconcile the objectives of needing to be as close as is feasible to being a requirement for <u>any</u> <u>knowledge</u> <u>at</u> <u>all</u> to be possible, with being as heuristically and methodologically fruitful as possible, to give us the greatest hope of making real progress in knowledge.[17]

This is where the aim-oriented empiricist interpretation can be invoked. If the brief exposition already given of its solution to the problem of induction is re-examined it can be seen to have formulated just such a version of the problem, doing justice to these competing aims and fully implementing the principle of intellectual integrity. The justificational problem of induction becomes the problem of justifying the metamethodology of aim-oriented empiricism, granted that what we are seeking is knowledge. When the family of increasingly contentless metaphysical assumptions depicted in the **Appendix** is clearly articulated, the methodological undertaking of selecting the best possible assumption to facilitate the growth of knowledge at each level is thereby given the greatest possible help. This satisfies the principle of intellectual integrity. It also satisfies the two seemingly contradictory aims in that the desire for a cosmological assumption, which is as fruitful as possible with regard to

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[17] The problem being, of course, that these two are likely to be on a collision course. Any metaphysical assumption that is heuristically and methodologically fruitful will, more probably than not, make quite contentful, precise assertions about the universe: the more contentful and precise an such an assertion is, the more likely it is to be strictly false.

the growth of knowledge, is satisfied by the current **level 3** blueprint and the desire for an assumption least likely to be false and in need of revision is satisfied at **levels 9** and **10**. As with the case of the principle of uniformity, just one assumption cannot satisfy all that needs to be satisfied but a hierarchy of increasingly contentless assumptions <u>can</u>. A scientific enterprise which adopts a conjectural and presuppositional approach, which allows a definition of 'theoretical knowledge' which is not wholly dependent upon the empirical principle, and at every stage offers alternatives and the methodological means to judge between them, is one which is sufficiently flexible to learn in what way the universe is comprehensible <u>as it proceeds</u> and thereby avoid the traditional problem of the rationality of science. It is, in other words, one which offers a solution to the justificational problem of induction and it does so by providing a rational means of selecting the best possible cosmological speculations, at each level, out of the totality of all possible speculations.[18] It also, by recognising the fallible character of all the assumptions, fulfils that requirement of rationality which demands that we accept the genuine fallibility of our choices. Although aim-oriented empiricism seeks to prove that science has met with massive and astonishing success at **level 2**, this is something which can never be assumed. However, what can be accepted is that once a particular **level 4** assertion, such as physicalism, is agreed upon, the **level 4** methodological rule which constrains us to chose whichever theory does the best justice to physicalism becomes fixed but the **level 3** rule, which concerns choosing that theory which does the best justice to the best blueprint, ensures that theories and methods will always change with changing blueprints. We have already seen that it is a requirement for rationality that when metaphysical assumptions concerning the comprehensibility of the phenomena change, then the methods which select theories must reflect this if they are to fulfil the primary aim of elucidating the best versions of that developing comprehensibility. They must take account of the way in which the world is held to be comprehensible and they must reflect the legitimately associated criteria of simplicity and explanatoriness.

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[18] **Given this increasingly attenuated hierarchy about the nature of the universe, it is also possible to consider infinitely many further such principles, [Q_3 - Q_8], such that, for** $r = 3, \ldots 8$:-

a) Q_r implies P_{r+1} **but is not implied by** $P_{r=1}$;

b) Q_r is incompatible with P_r

In view of a) and b) each Q can be regarded as a possible rival to P_r. Solving the justificational problem of induction involves justifying preference for P_r over rivals.

Consequently it almost goes without saying that **level 3** metaphysical blueprints will evolve with evolving knowledge because this is the mechanism by which means an improvement in knowledge brings about an improvement in methods. This productive process of modification and development is the very core of aim-oriented empiricism and serves once more to illustrate how much more rational aim-oriented empiricism is than any rival conception of science which elevates the principle of empiricism.

Aim-oriented Empiricism and the History of Science

I appear to have made a great many claims on behalf of the generalized aim-oriented empiricist framework. Now is the moment to investigate the further claim that the framework has the additional advantage of being peculiarly appropriate as an interpretative framework for a progressive history of scientific ideas. However, before we examine this in more detail we must recap on what has been established so far with regard to histories of progress.

Progress History of Science *is* Possible

A number of general points were made in Chapter One about progress-oriented history of science, which it is necessary to review before going any further. Firstly, in the matter of deciding what is to be taken as the basic goal of science, any progress historian must employ legitimate interests and value judgements, which do not influence the form of the explanation, in order to facilitate making a selection from among the alternative possibilities and their attendant problems. There is a certain amount of freedom of choice involved: the historian cannot simply allow the historical agents to decide the goal of a given endeavour. If the aim is to improve knowledge then the question arises as to how knowledge is to be defined: is it that which is verified, that which has so far evaded falsification, that which is probable, or that which has been empirically confirmed? Is knowledge essentially personal or social in character, or is it construed in an impersonal way? If the latter, how can it possibly be of use to human beings? Is it part of the goal of science to help promote human welfare, the idea being that scientific progress should contribute to social progress? Conversely, is it better to keep the goal of improving scientific knowledge (which belongs to the intellectual domain and is construed in impersonal, intellectual terms) quite separate from the goal of contributing to human welfare (which belongs to the social domain)? The historian may choose the most realizable goal, or the most loosely defined, but two things are clear. It is obvious that different basic goals of science will lead to different progress-oriented histories of science and it is equally obvious that evaluation not only plays a role in goal selection but must also operate from the standpoint of the success or failure of a given endeavour in tending towards its goal.

Secondly, the progress-oriented historian does not, inevitably, presuppose scientific progress, or define it as an indubitable truth. Yet again I must reiterate that to appeal to such entities as progress, rationality, and truth is not to draw them into the form of the explanation and use them in order to justify solutions. Progress, for example, can never aspire to more than conjectural status because ensuing scientific developments may alter notions about the degree to which past scientific discoveries did, and did not, contribute towards progress. However, it is a conjecture which is made in the context of a chosen goal and whether or not it can be said to have occurred is an historical question and therefore to be discovered. Even when the progress historian judges that with respect to a particular goal science has, by and large, made progress, this still does not entail unavoidable presuppositions. For example, the historian is not thereby committed to the notion that it has made progress in every respect. Indeed criticism, where relevant, of the myth that contemporary science must always be better than past science, is an important task for progress historians. Similarly, a progress-oriented historian does not automatically think that the advance of science is always a rational affair: it is perfectly possible to be undogmatic about whether a scientific development has come to pass in a rational, non-rational, or even irrational way. Progress may even have been achieved without progress having ever been intended by the historical actors involved. Again, the progress-oriented historian does not inevitably impose concepts of scientific progress and rationality on the past, or on historical agents, be they personally-held ideas, or current scientific ideas. The job of such a historian is to uncover and evaluate historical agents' clearly stated and tacit ideas concerning the aims and methods of science, which is the nature of scientific rationality, and evaluate these things with regard to a clearly defined goal. It is not even the case that the historian is automatically restricted to considering success: a good progress historian is obliged to attend to research projects, theories, and ideas that ultimately made no sort of contribution to scientific progress because such entities play an indispensable role in that progress. To suppose that a tolerable grasp of scientific progress is possible without paying any attention to such blind alleys and useless conjectures is to assume that science itself provides a perfect method of discovery, which infallibly defines the path of progress and all those who have travelled by it! In short, none of these errors is contingent upon a progress-oriented historical work which is also defined as a goal-seeking endeavour: it is possible to continuously evaluate the actions, events, and processes of the scientific past from the standpoint of their success and failure in tending towards the goal of the endeavour, as legitimately construed, without presupposing, or predetermining anything.

Towards a Historiography of Science

What can be legitimately claimed for aim-oriented empiricism as a progress-oriented historiography of science? Such generalities as those of the previous section allow us to argue for the reality of scientific progress, which we all implicitly do every time we entrust our lives to science-based technology. Tactical methodological relativism, although specifically intended not to, nevertheless does strictly rule out our common sense belief in entities like realism, rationality, and progress. As Richard Dawkins has said,

> **Show me a cultural relativist at thirty thousand feet and I'll show you a hypocrite. Airplanes built according to scientific principles work. They stay aloft, and they get you to a chosen destination. Airplanes built to tribal or mythological specifications, such as the sunny planes of the cargo cults in jungle clearings or the beeswaxed wings of Icarus, don't. If you are flying to an international congress of anthropologists or literary critics, the reason you will probably get there - the reason you don't plummet into a ploughed field - is that a lot of Western scientifically trained engineers have got their sums right. [8]**

As a response to analytical relativism, this is perhaps a little harsh: it can still be allowed a role - but those who utilize it ought to be explicit about the manner in which it introduces boundaries or discontinuities into the very concept of knowledge. The removal of those boundaries indicates that all of our knowledge has to be taken on trust. The generalities of the previous section also establish that a progress-oriented history of scientific ideas needs philosophy of science in order to formulate its historical problems about how and why progress has come about: such history is inevitably affected by the working-out of ideas which originate with the philosophy of science. However, what such generalities do not do is tell us how to make rational sense of scientific progress. They do not explain how science ought to proceed if it is to be ideally rational but if the basic goal of science is conceived of in impersonal, asocial, intellectual terms then the problem that has to be solved is precisely this intellectual problem of the rationality of science, otherwise known as the problem of induction. What is required from this solution is what philosophy of science is generally perceived to have failed to produce, to whit some universally applicable definition of scientific rationality, such that it would always be conducive to making scientific progress were it to be put into scientific practice, and in terms of which scientific progress, past or present, may always be evaluated. The aim-oriented empiricist solution to the problem of induction provides that universal definition and makes rational sense of scientific progress. It accomplishes this by producing a framework in which there are good grounds for accepting the methodological principles of **levels 5 - 10**, as metaphysical principles true of the actual universe.

In addition, aim-oriented empiricism shows that the fact that science has no one fixed method is no longer a problem. The concept of methods changing as aims and knowledge change is shown to be a requirement of rationality: there is disunity and diversity of methods at **level 3** but, unless there are scientists who reject **level 5** comprehensibility, there is unity of methods at **level 5**. Furthermore, by accepting that science is the paradigmatic case of progressive knowledge, and trying to determine the character of the methodology responsible for this, in so far as that methodology has been implemented in scientific practice, aim-oriented empiricism ensures that determination of the extent to which a given methodology has been realised in the past, in terms of the relative empirical progressiveness or sterility of some past research programme, is a factual, historical question. More particularly, in the assertion that the rationality of science depends upon evolving aims and methods within a fixed, meta-methodological framework, aim-oriented empiricism indicates that there is an historical dimension to the intellectual problem of the rationality of science itself. It has important features which are only explicable historically, in terms of the relative empirical progressiveness or sterility of some past or present research programme, some more of less specific **level 3** or **4** view as to what the aims and methods of (some part of) science ought to be. Thus aim-oriented empiricism's historiographic strength is that it can be accepted as a process which maximizes the possibility of improving knowledge and then used to judge the work of certain historical events or characters. Indeed it provides a mechanism for fulfilling another instinctive belief, articulated long ago by Conant [9] to the effect that the historical approach, which helps us to appraise a theory relative to its background, also helps us to see it as progress.

This claim can be justified if we recall that the central idea of aim-oriented empiricism is that science must discover in what way the universe is comprehensible as it proceeds and that this is accomplished by means of a positive feedback between improving scientific knowledge and improving scientific aims and methods. The fixed meta-methodological rules of aim-oriented empiricism require that each of the postulated components be modified to bring it into better agreement with the others. This is an essential feature of scientific rationality, which also helps to explain the tremendous growth (since the early modern period) of scientific knowledge, because it results in theories, methodologies, metaphysical blueprints, and empirical practices evolving through time as a part of the whole framework. The process can also be articulated in terms of goal-orientation. The aim of science of discovering in what way the universe is comprehensible, which can be thought of as a goal towards which the scientific endeavour can legitimately tend, can be construed in increasingly more precise ways, from **level 5** to **level 3**, with the result that the presupposition of the truth of some idea at the lower level perhaps that the universe is a self-interacting field, produces associated methods that are both more restrictive and (potentially) heuristically more fruitful, than at the higher levels. This is particularly true from the standpoint of creating new theories. The payoff is that the aim becomes more problematic: even granted **levels 4** and **5**, almost certainly the universe is not comprehensible in this presupposed **level 3** way. Consequently, as scientific knowledge improves, science must seek to improve its more or less specific aim and associated methods within the framework of the fixed but more imprecise aim (of discovering in which way the universe is comprehensible) and its associated meta-methodological methods. The first consequence of this is that the activity of proposing and assessing rival possible aims-and-methods for science (or philosophy of science, as it is more properly known) becomes an integral, indispensable part of science itself. The second consequence is that the much-used argument that even intellectual history of science has no use for philosophy of science, because history of science is concerned with the emergence of scientific ideas from facts and philosophy of science is concerned with whether scientific ideas really do emerge from facts, is simply not applicable. In the aim-oriented empiricist framework the emergence of scientific ideas is something to be studied historically because the intellectual problem of the rationality of science has significant features which can only be explained historically. It is impossible to understand why **level 3** and **level 4** aims and methods are accepted at any given stage in the development of science without considering that they are cosmologically related and will change as cosmological ideas change. Indeed **level 10** partial knowability of the universe depends on there being some level at which cosmologically-dependent methods are implicit and can also be regarded as being at least implicit in the history of the acquisition of knowledge up to now. Equally it is necessary to consider the successes and failures of contemporaneous rival research programmes based on rival aims and methods - and ask the degree to which they are cosmologically-dependent and related. Criticism is not synonymous with condemnation, rather it is the rational thing to do.

To sum up: aim-oriented empiricism offers a framework in which it becomes credible to make judgements as to the rationality, or otherwise, of past scientific developments, or a progression of past scientific developments, which can be assessed according to how well they have performed as part of an evolutionary framework. In this it fulfils the requirement stated in Chapter One of showing that a history of the progress of scientific ideas is possible as a result of scientific progress being possible: the latter is accomplished by specifying what it is that is to count as knowledge and then demonstrating that it is feasible to make rational sense of the notion that progress in scientific knowledge is realizable and does occur. It demonstrates what is intuitively felt, that progress in knowledge is no chimera. Using aim-oriented empiricism as an historiographical framework is not, of course, to use it as an empirical theory of scientific practice in history. If it were then the further historical figures depart from what aim-oriented empiricism expects, the more impoverished aim-oriented empiricism would be as a descriptive account of the historical record. The aim-oriented empiricist framework can function as a valid historiography of science, in which the more or less specific aims and methods of **level 3** are improved within the structure of the much more imprecise but fixed aims and associated methods of **levels 4** and **5**, without presupposing any conscious implementation of aim-oriented empiricism, by any historical agents, in any past age or period. Arguments which venture no higher than **levels 2** and **3**, so that there is nowhere, as it were, to pigeonhole the many theoretical and even metaphysical revolutions that the advance of physics has produced, might well conclude that theoretical knowledge <u>cannot</u> persist through such changes. The study of precise **level 2** theories - and even of less precise blueprint ideas at **level 3** - can quite easily seem to prove that. But include the **level 4** notion of physicalism, which holds that the cosmos is such that the kind of explanation sought by theoretical physics exists to be found, and it becomes obvious that physicalism is implicit in the aim and the methodology of modern physics insofar as the only theories under consideration are restricted to those that bring about increasing theoretical unity and ignore the empirically successful, *ad hoc*, disunited theories. When **level 4** is included it is possible to conclude that physics is a body of knowledge which deals with some definite view of the universe, in which the only change is one of degree as the pictures become more detailed, more precise, more certain. Physicalism has a unique capacity to be empirically progressive, among all possible **level 4** versions of **C**. This occurs if, at **level 2**, physicalism in realized in the form of a theory, **T**, which becomes both more inclusive, more accurately predictive of phenomena, and more unified. In this case there <u>is</u> progress in theoretical knowledge, as **T** is becoming an ever more specific, more clearly realized version of **P**. Obviously, the aim-oriented empiricist framework has important consequences for the validity of using present-day perspectives as frameworks for studying the past. Not only is it a present-day perspective, it is also one which demonstrates that the historian's unavoidable position in the conceptual and perceptual categories of the present does not necessitate their imposition upon the historical record. Aim-oriented empiricism equals Ashplant's and Wilson's source-generating methodology in providing the means whereby the past and historians' questions about it can make a meaningful engagement. It surpasses it in also making possible progress histories of scientific ideas.

Ashplant and Wilson drew attention to the existence of a relationship between present-centredness and value judgements, so it should occasion no surprise that aim-oriented empiricism has important consequences for the nature of this relationship. Some assertions have already been made about value judgements, that they are necessary to delineate subject areas and their significant problems and, more generally, to ensure that those subject areas are not too limited in number and scope. The general conclusion reached in Chapter One was that 'total history' is impossible to handle: historical methods can only be of limited application but they themselves need not be severely limited in number. It should always be possible to work out ways of investigating interesting areas which lie outside the domain of conventional methodologies. Aim-oriented empiricism ratifies this in two ways. Firstly, the critical-rational approach that lies at its core encompasses the idea that there can be many different but valid approaches to the history of science. Secondly, the evolutionary nature of the framework offers a possible model for sorting out legitimate 'principles of selectivity', the problem which Ashplant and Wilson admit they have not solved. Quite generally, <u>any</u> subject area can be defined in terms of a goal, or an aim, and the strategies employed to attain it. It need not be a long-term goal. For example, there is the particular argument to the effect that Newtonianism spread quickly in England because, through such means as the Boyle Lectures and similar propaganda, it was being deliberately used as an instrument of social control.[10] The goal of such an endeavour can be defined as an attempt to uncover the socio-political purpose behind the outpourings of natural theology in early eighteenth-century England. The strategies are those methods which can be identified as having been designed to attain the goal, for example the Boyle Lectures, coffee house meetings, and the popularizing of the Newtonian system through edited and simplified publications. The historian should, of course, offer some sort of support for the choice of goal and strategy. This could include the emergence of many atheistic sects, seen as a threat to the national Church after the recent instability of the Glorious Revolution of 1688. In this light the unchanging, law-governed order of the natural world <u>could</u> look like a desirable model for society. Moreover, all the most vocal proponents of natural theology belonged to the party of Anglican clergy who had supported the deposition of James II and rejected the Stuart obsession with 'the divine right of kings'. Lawfulness and stability in society and a role for Divine intervention, such as the fortunate easterly wind that sped Mary and William safely into Plymouth, would be congenial to such Churchmen. Finally, the historian should demonstrate the rationality of this chosen enterprise in terms of the closeness of the relationship between the goal and the strategies. Principles of selectivity work to pick out a realizable goal and a set of strategies that are the best available from the standpoint of attaining the goal: the worth of the enterprise can be judged in terms of how successfully all the necessary steps have been implemented in the attainment of the goal. Anyone in disagreement with a given enterprise can attack its rationale, or show that all the necessary steps weren't, in fact, implemented, or argue that they were not the best possible strategies, or challenge the statement that the success of the goal was solely due to the implementation of the strategies. The various approaches to Newton cited by Ashplant and Wilson (see Chapter One, reference [91]) can be judged in precisely these terms.

All of this is quite general, concerning the rational generation and implementation of principles of selectivity, or value judgements, in the attempt to formulate enterprises that will get a meaningful purchase on the past. The form of purchase will, naturally, depend upon the way in which the basic goal is construed but a realizable goal and a related set of strategies should be picked out by means of principles of selectivity. Thirdly, however, aim-oriented empiricism validates the kind of progress-oriented history of science which has generally been known as 'intellectual history of science', with its emphasis on ideas, theories, discoveries, arguments, scientific problems, the adequacy or inadequacy of attempts to solve them, and how good or how bad the theories and arguments are. In Chapter One it was also concluded that <u>given</u> that a history of scientific progress is possible, there must be a means of working out the methodological decisions that would produce a functioning historiography for the development and progress of scientific ideas. Aim-oriented empiricism accomplishes this by means of the methodological prescription that the best available choice at any level is the one that best facilitates the <u>primary aim of the growth of knowledge</u>. This then functions as a mechanism for identifying how these entities may be expressed in terms of an aim and a method for realising that aim, and for judging how good/bad, adequate/inadequate they are in terms of fulfilling that aim. The first thing that becomes clear is that the aim-oriented empiricist historiographic framework does not turn the historian of science into 'a historiographer of truth'. With Bachelard and his school, for example, a historiography of truth is the unavoidable result of their definition of sanctioned history as *'the history of thoughts that are always topical or can be made topical if they are evaluated in terms of the science of the day'*.[11] Bachelard's values are those of modern science and therefore partake of all the problems perceived to follow from all such present-day frames of reference. Secondly, it becomes clear that in no circumstances does the aim-oriented empiricist framework invalidate other historiographies. Providing a framework for a history of scientific ideas involves a mechanism for the proper functioning of the intellectual or value judgements necessary to that sort of historiography, but it is merely a special case of the more general mechanism (in its turn ratified by the critical-rationalist heart of the enterprise) for generating principles of selectivity for any perspective. In the light of this discovery, the suggestion in Chapter One that it might be advisable to enlarge our understanding of the scientific past by constructing a range of competing, even clashing perspectives, social, intellectual, or cultural, becomes feasible.

There are also points to be made about contextualism because in legitimizing intellectual histories of progress aim-oriented empiricism also grants a certain role to contexts. In Chapter One, I discussed the possibility that instead of historians employing methodological tools in order to 'lose' their modern notions they ought perhaps to be critical about them, which requires that they be articulated and kept in view. The twentieth-century perspective may be required to highlight differences between 'then' and 'now', to show that current ideas are different from historical ideas. The critical-rational heart of aim-oriented empiricism, of course, provides a legitimation for this in that it implies that, as a matter of intellectual integrity, the historical development of scientific rationality be assessed in terms of its contextual ideas (both explicitly articulated and implicit in the decisions and judgements of the historical actors) and the ideas which comprise the goal. The former are contextual matters and should be kept separate from, in order that they can be compared to and contrasted with, the goal or the enterprise as the historian defines it. However, this requires particularly delicate handling on the part of the progress historian of science, in order to absolutely avoid the appearance of imposing anything onto the past. It is necessary to be completely open-minded about what presuppositions, metaphysical and otherwise, natural philosophers may have had, and how rational they may have been, whilst considering them relative to a framework encapsulating entities like progress and rationality which, by their very nature, may only have been assembled much later. These problems may be illuminated by recalling that aim-oriented empiricism favours physicalism as the best way in which the universe can be said to be comprehensible. It judges it to have an associated methodology best able to promote the growth of knowledge. Physicalism is the independent, constant, and quasi-permanent quantity in whose terms ever more specific and restrictive cosmologies can be formed. According to Maxwell, it was first recognisably formulated in the seventeenth century and subsequent science has developed increasingly more restrictive physicalist conceptions of comprehensibility, which have led to more restrictive cosmologies and methodologies. From the work of,

> **Kepler, Galileo, Newton, Dalton, Fresnel, Faraday, Maxwell, Darwin, Boltzmann and Planck to Einstein, Schrodinger, Watson, Crick, Salem, Weinberg and Gell-Mann, there is the gradual clarification and development of one basic idea, physicalism: ... All major theoretical developments of science can be interpreted as enabling us to understand, in ever greater detail and with ever greater precision, how more and more apparently diverse phenomena are the outcome of relatively few different sorts of entities interacting by means of ever fewer different sorts of invariant forces (at present described by the three or four fundamental dynamical theories of modern physics).** [12]

However, there speaks the philosopher of science, with a different agenda to that of the historian of science. The philosopher of science, as I said earlier, is interested in normative questions, which includes questions of how science ought ideally to proceed if it is to be rational. The historian of science has to remain open-minded about the metaphysical and other presuppositions that natural philosophers have actually had.[19] Physicalism is not something that can be presupposed. Perhaps even comprehensibility cannot be assumed beyond the unexceptional fact that almost all prolonged attempts to improve knowledge and

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[19] **The historian has to recognise that there are a superfluity of different and innovative ontological positions associated with the scientists listed above, in addition to physicalism.**

understanding of nature, even pre-scientific ones, have surmised that explanations exist: that why things happen as they do can be explained. The historian of science would point out, for example, that Kepler, Galileo, and Newton cannot possibly be characterized as having cosmologies that were physicalist in the terms of the dynamical theories of modern physics, or even in the sense of explicitly setting out to explain many diverse phenomena in terms of the fewest possible number of fundamental physical entities and actions. Kepler was a metaphysicist with mystical, not to say animist, tendencies. He saw the sun as the embodiment of the world soul, controlling the motion of the planets by a direct influence that owed much to the Aristotelian concept that motion requires a force. He had a Platonic conception of geometric archetypes in the mind of both the Creator and the created being. His third law was discovered as a part of his search for the harmony of the world system, something which could be intuited from the divine plan.[20] This is the background against which his Platonic-Pythagorean vision of a mathematical universe must be judged. This vision led to the theory that all knowledge is ultimately mathematical and all things are bound together by proportions. He even went so far as to apply this theory to observed facts. When he applied Tycho's data on planetary positions to his hypothesis that the planetary orbits inscribe and circumscribe the five regular solids, he discovered his first two laws.[21] Gradually the combination of Tycho's highly accurate observations and his own training as a mathematical astronomer encouraged Kepler to think less in animist terms and more in terms of mechanical or physical explanations for planetary paths. However, as for imposing physicalism on him, it cannot be done. All that might be attempted is some judgement as to the degree to which Kepler's belief in mathematical proportions, and the laws he produced as a result of applying the concomitant theory to observed facts, approximates basic physicalist requirements. Do his mathematical view of the world and the three mathematical laws of astronomical motion that resulted encapsulate anything that is fundamental about physicalism? What happens if the goal is redefined rather more broadly, into a form such as, 'is anything approaching aim-oriented empiricist science found in the

[20] The third law states that of any two planets orbiting the sun, the squares of their times of revolution are proportional to the cubes of their mean distances from the sun:
[21] The first law states that the orbit of a planet is in the shape of an ellipse, with the sun at one focus. The second, or equal area law, states that a line from the planet to the sun will sweep out equal areas in equal times.

work of Kepler?'. If aim-oriented empiricism is accepted on the strength that it is a process which maximises the possibility of improving knowledge then the questions to be considered would reflect this. They would include whether or not there was any evidence of a new metaphysical blueprint being generalized from an earlier one, or any evidence of interplay between the various levels, experimental, theoretical, metaphysical, and mathematical, and any evidence of whether or not the methodology was flexible when necessary and precise when required. From this perspective, how is the historian to understand the relationship between Kepler's belief that the universe has a simple, comprehensible mathematical structure, created by God, and his methodological practice of trying out any number of geometric possibilities and testing them by means of Tycho's observations?[22]

Newton is even more of a problem. In purely physicalist terms, Newton's universe can be thought of as being composed of three-dimensional Euclidean space and continuous time, which together form an infinite framework for the containment of rigid corpuscles having mass and interacting by means of centrally directed forces. If one is viewing Newton as a physicalist, one would expect that a physicalist kind of comprehensibility would be built into the developing methodological structure of his science However, he produced an extraordinary theologico-metaphysical blueprint in which the something which is present at all times and places (and which is itself both unchanging and responsible for all change) was not a physical entity at all but a totally spiritual one, God.[23] The general **level 5** idea of comprehensibility, invoking the concept of a thing which is invariant, includes non-physical entities like God - but **level 4** physicalism does not. It is quite widely accepted that Newton would most probably have denied the possibility that the entities and forces associated with physicalist explanations were physical at all: space and time, matter and force, were all, for him, dependent upon the existence and active will of God. Indeed, it has been argued that a quest for evidence of divine activity in the universe prompted and shaped all of Newton's

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[22] **See, for example, Gary Hatfield,** 'Metaphysics and the New Science', **in D. Lindberg and R.. Westman, (eds.),** *Reappraisals of the Scientific Revolution*, **Cambridge: Cambridge University Press, 1990, 102-110.**

[23] **This is a different case from that of Descartes. His use of God within the Cartesian blueprint obliged the Creator to become an entity to be understood through that aspect of himself that was physical. This was because He created the material universe and the laws that govern it but became, in the process, an entity to be understood through that aspect of himself that was physical and constrained by the regularities of His own creation.**

work in natural philosophy, alchemy and theology.[24] Again, if the goal is broadened into a form such as, 'is anything approaching aim-oriented empiricist science found in the work of Newton?', the problem immediately arises that although the Newtonian blueprint can be seen as part of a progression, as aim-oriented empiricism expects, it nevertheless disappoints the expectation that the best articulated blueprint will be most likely to spawn the really successful theory.[25] The blueprint progression in question runs from Cartesian corpuscles acting by contact, to Newtonian corpuscles interacting by means of centrally directed forces, to Boscovichian point-particles about which attractive and repulsive forces vary in a fixed way. The really successful mathematized theory, of course, is the force of gravitational attraction, which operates within an inverse square relation and falls away inversely as the distance between the two attracting bodies. The corpuscular action by contact blueprint was highly problematical and one of the trouble spots was that of collisions between infinitely rigid particles. It was well understood in the seventeenth century that when two infinitely rigid particles collide they sustain an infinite acceleration/deceleration as a result of an infinitely repulsive force being achieved when the two surfaces come into contact. At the point of collision the velocities of two colliding particles are obliged to change their directions instantaneously, suggesting that there is a point of discontinuity which is not altogether understood or accommodated. Moreover, it is difficult to account for attraction and cohesion is frankly puzzling, necessitating Cartesian 'hooked' corpuscles and the like. However, it is possible to regard this corpuscular action by contact metaphysical blueprint as a special case of something rather more general, namely the idea that the force, instead of being infinitely repulsive and dependent for its existence on the centres of two spherical, rigid corpuscles being a given distance apart, now varies continuously with distance, possibly tending to the infinite at the centre. This generalized blueprint is much more flexible: it retains certain good features of corpuscularianism, such as rigidity and symmetry, so that asymmetries like hooked corpuscles are no longer needed. Attraction is just as

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[24] **See, for example, J. E. McGuire,** 'Existence, Actuality, and Necessity: Newton on Space and Time', *Annals of Sci. 35*, **1978, 463-508;** 'Space, Infinity, and Indivisibility: Newton on the Creation of Matter', **in Z. Bechler (ed.)** *Contemporary Newtonian Research*, **Dordrecht: D. Reidel, 1982, 140-166; B. Dobbs,** *The Janus Faces of Genius: The Role of Alchemy in Newton's Thought*, **Cambridge: Cambridge University Press, 1992.**

[25] **It can be seen as part of a progression in which a given blueprint is generalized in order to develop a better one which answers all the problems thrown up by the first version**

understandable as repulsion: it becomes possible to imagine that there are certain distances at which particles tend to congregate and form a solid body and certain others at which repulsive forces take over from the attractive ones, as illustrated by the difficulty of compressing solid bodies. The original corpuscular blueprint has been successfully generalized to try and accommodate the problems that were inherent in it: the earlier blueprint comes within the scope of the new one as a special, arbitrary case. The inverse square relation of gravitational attraction derives quite comfortably from this blueprint. Unfortunately what aim-oriented empiricism would naturally identify as the clearly articulated 'generator' of the most successful theory of the age was not Newton's blueprint of corpuscles having mass and interacting by means of centrally directed forces, but rather Boscovich's point-particle blueprint formulated in the eighteenth century. Anyone attempting to do more than view from an aim-oriented empiricist persective, to perhaps try to actually establish that Newton was a physicalist, is faced with a seeming conundrum. The strength of the aim-oriented empiricist historiography, as was asserted above, is as a process which maximizes the possibility of improving knowledge because it operates through the methodological prescription that the best available choice at any of the **levels** is the one that best facilitates the primary aim of the growth of knowledge. This, however, implies that the individual who developed Newtonian universal gravitation and mechanics ought also to have been the formulator of Boscovich's blueprint!

However, the development that took place between Kepler and Newton can still be interpreted in aim-oriented empiricist terms and even seen as the first (tentative and mostly inadvertent) flowering of a modern physicalist cosmology. This is possible as long physicalism defines the parameters of what is to count as knowledge and enables us to make rational sense of the notion that progress in scientific knowledge is possible and does occur. It fulfils this latter requirement if it is construed as having the methodology best adapted to fulfilling the goal of improving expert knowledge about natural phenomena. The assessment of historical deeds and episodes, concerning what was both explicit and implicit in the decisions and judgements of historical actors, therefore can be made from the point of view of their success and/or failure in tending towards this goal, rather than in any contextual, historicist terms. It is in this sense that the changes from the corpuscle idea of the seventeenth century, to Boscovich's eighteenth-century point-particle, to the nineteenth-century concept of the point particle/field, associated with Faraday, Clerk Maxwell, and Lorentz to Einstein's unified field blueprint of the early twentieth century, can all be interpreted as specific versions of physicalism, each of which generalizes the one that preceded it.[26] In no sense does this presuppose that the historical actors concerned articulated the goal, or were even engaged in an explicitly goal-pursuing endeavour: it does not in any way impose upon the historical context. It would be a mammoth task to keep the context and the articulated goal separate over such a time-span involving so many significant individuals and it is not the intention of this thesis to attempt this. However, it would, in principle, be possible.

Granted all this, it also needs to be stated that all previous methods intended to reconcile the conflict between the demands of histories of intellectual progress and contextual, non-anachronical history, regardless of the degree to which a present-day perspective is involved in the formulation of that method, have tended to be standard empiricist in form and function. As a result, those parts of the history of science which do not conform to the edict that only empirical considerations can determine what is to count as scientific knowledge, for example metaphysics and theology, may well be regarded as influential in the development of science but they will always be firmly extra-scientific and non-rational. Holton's thematic analysis is a good case in point: it specifically embraces the standard empiricist definition by asserting that *themata* function as underlying or even subconscious motives during scientific research and they can be drawn from practically anywhere, from metaphysics, politics, economics, or sociology. However, this actually means that a particularly subtle form of Whiggism has been at work, one that accepts the standard empiricist account of rationality as the correct account. Conversely, the aim-oriented empiricist framework is powered by a range of components, of which observation and experiment form but one aspect: it includes the category of the totality of testable theories and laws, the best (explicitly articulated, criticised, and heuristically fruitful)

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[26] **Refer to Footnote 26 for the complete exposition of this argument.**

conjecture as to how the universe is comprehensible, and the currently accepted best methods of scientific enquiry. The extra-scientific and non-rational are transformed into legitimate components in the rational scientific process, and a devious form of Whiggism is exposed and thereby avoided.

Finally, one important issue which is frequently denied consideration is the inter-relationship between past and present. Cronin was perfectly correct in her argument that a history of progress is not necessarily a uni-directional activity, moving truimphantly from 'then' to 'now', but can be an activity which works in two directions. There is indeed an interplay between the present and the past - and between the past and the present - which, if handled sensitively, can ensure that each illuminates the other in revealing ways. I have already argued for the validity of a perspective which can reconcile progressive, goal-oriented intellectual histories with a degree of appropriate historical contextualism. If we wish to then extend that historical progress right up to the present day, the aim-oriented empiricist evolutionary framework permits us to interpret the ideas of the present as the culmination of all the developments which have occurred since the historical episodes under consideration - provided that all those developments are seen as being relevant to our understanding of those episodes. If all of these requirements are fulfilled, then a present perspective can illuminate the past without automatically rationalizing it. Not only can it assist in clarifying what the historical problems actually are but it also functions as a yardstick to help us to decide whether or not a particular goal was ever achieved. The very notion of progress depends upon understanding the differences between then and now. Conversely, continuities with the past can help us to understand present-day problems and difficulties. Historical perspectives which are part of a continuous development, 'the story of how we got here', can greatly aid us in sorting out present-day problems. Aim-oriented empiricism facilitates these processes by virtue of interpreting science as a progression of evolving frameworks, so that the relatively seamless progression from past to present that supports the idea that histories of progress are possible also expedites this two-way current. Intellectual history of science would scarcely be possible without making this sort of use of current scientific knowledge and in doing so it merely partakes of the character of any history of progress, always vulnerable to new discoveries pertaining to the parent academic discipline but not totally governed by them. There are echoes here of Bachelard's non-teleological, active consideration of past science in the light of contemporary science, in so far as history can be rewritten under both systems. However, as aim-oriented empiricism does not take the rather crude position of simply judging the science of the past in terms of contemporary scientific knowledge but provides instead an evolving link between the two, certain benefits accrue. One is the shifting of exposition towards the direction of improving the understanding, or illuminating the object of study, and away from blunt explanation. Consequently, although the role of current scientific knowledge in evaluating the comparative success or failure of a past goal-oriented endeavour may be subject to problems caused by new scientific discoveries, the fact that the aim is to improve the understanding makes it less of a problem that it would be if the aim were to definitively explain. More significantly, it does not partake of the major faults associated with Bachelard's recurrent history, which were earlier listed as an historiography of the truth tending towards a Whig interpretation.

Aim-Oriented Empiricism and Whig Historiography

I have already argued the case for aim-oriented empiricist parameters but I will just spell out what these mean for the avoidance of the true and undesirable heart of Whig history, defined in Chapter One as the imposition, held to be a legitimate means of linking past and present, of contemporary subjects, concepts, techniques and methods onto the past. The evolutionary framework avoids the problem of the subjects and concepts of today being transposed onto those of the past because instead of demanding that the latter are about precisely the same things as the former, they merely ask that they be shown to have partaken of the same evolutionary process. *Australopithecus australis* is not *Homo sapiens*, after all, but it is generally accepted that they are located on the same evolutionary line of development. This same process also demonstrates how a goal or aim-oriented history, properly defined, can obviate the problems concerned with modern notions of coherence and rationality. Again, an historical agent does not have to have worked in precisely what modern science or modern philosophy would identify as a coherent and rational way for the evolutionary links between past and present, from which the rationality and coherence of the enterprise springs, to be maintained. The goal can be identified and traced through time without any of the historical figures having actually articulated, or even consciously understood it, and without its ever having been ultimately attained. The problems connected with formalization and anticipation likewise fade away in the face of the evolutionary, goal-oriented process. It is certainly possible to give modernized translations of chemical and mathematical statements, as long as they can be convincingly shown to be on the same evolutionary time-line. Newton did not formulate **f = ma** but his **Second Law** can be written as such without doing violence to his intentions.[27] Theories can also be shown to have antecedents without incurring the accusations that the alleged forerunner must have been formulated by a clairvoyant, or that the product of a later age is being projected back onto an earlier age. If, for example, a series of theories can be interpreted as a series of blueprints, each one dealing with the ultimate constituents of the cosmos and generalizing its predecessor, then anticipation can be a legitimate historiographical tool. As I argued earlier, it is possible, if all the acts in the drama are interpreted as specific versions of physicalism, to trace the relationship from the seventeenth century to the twentieth. Moreover, the legitimization of intellectual or value judgements through the medium of serious and sustained criticism, which is necessary for the validation of all the levels in the framework, produces the kind of judgements that Hall was hinting at when he outlined the processes of data selection, including subject areas and their significant problems, that historians are obliged to employ. These definitions of progressive histories and perspectives of the present, with all their many ramifications, are not to be confused with the concepts of progress and the present which are truly Whiggish. These are largely located in discredited inductivist historiography with its tendency to transcribe, retranscribe, streamline, and generally render the history of science as it was into the history of science as it should have

[27] **For an example of this see Cohen's account of universal gravitation in, Cohen. I. B.,** *The Birth of a New Physics*, Harmondsworth: Pelican Books, 1986, 164-174.

been. Since Whig historiography was stripped of its positivist/inductivist clothes, it has wandered about without visible means of identification.[28]

A decent historiography should be able to explain the reasoning behind its choice of area and explain its criteria of selection. Aim-oriented empiricist parameters demand that in the complicated processes which the historian of progress indulges in the episodes and events under consideration really do have relevance to the goal the historian has identified; this is facilitated by the sanctioning of the use of value judgements. Concepts belonging to the present, if suitably handled, <u>can</u> offer a subtle and illuminating perspective on the past which does not distort. If the ideas of the science of the present are seen as the culmination of all the developments, up to and including the present, are recognised as being of relevance in helping us to decide what was significant during those episodes, then the present state of scientific knowledge <u>does</u> have a role to play that will rise above the simple rationalization of the history of science. In addition, the past will also be able to illuminate the present firstly, in the sense of being a continually history-making force acting right up to the present and secondly, in the sense that historical insight into, say, attitudes towards a theory that belongs to an earlier age, can help us to understand attitudes towards it in its present form.

Aim-Oriented Empiricism and Present-Day Social History of Science

In Chapter One present-day history of science was presented as a discipline which has increasingly come to exclude questions concerning rationality and scientific progress. From this perspective, the intellectual aspects of science are, at best, taken to be the province of philosophy of science and at the worst are taken to be a chimera - because philosophers of science don't seem able to explain how scientific progress is possible. This is not to deny the mutual interests and concerns which exist between historians who remain primarily

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[28] Indeed, logical positivism might almost have been designed with the intention of elevating the concept of the superiority of the present because of its doctrine that the scientific knowledge of today is, in an absolute epistemological sense, superior to that of earlier times. By reinforcing Whiggish tendencies in the history of science, it managed to provide an instantiation of all that was bad about letting a philosophy of science perform such an expository role.

interested in scientific ideas, and philosophers of science with an interest in history. Notwithstanding, it is the case that many historians of science treat their subject matter as purely social phenomenon and too often fall into a fresh set of errors, many of which are connected with relativist doctrines[29]. Those who are not wholly committed to relativist doctrines - but equate progress history with Whiggishness or bias of various sorts, be that intellectual or cultural imperialism, the systematic disregard of social factors, or an uncritical support for modern science - might be persuaded by the arguments that were introduced in Chapter One and summarized in the present chapter under the heading **Progress History *is* Possible**. Many of the objections to the general legitimacy of progress-oriented history can be seen to be wide of the mark. However, upholders of versions of cultural relativism need to be persuaded of a more serious shortcoming in their practice. It is not simply that it can lead to a failure to communicate outside disciplinary boundaries and to the subversion of the notion of knowledge, which quickly leads to contradiction, or that there are problems with the increasingly sophisticated use of methodological and historiographical tools. Furthermore, it is not simply the case that social history of science argues against the sort of position I am advocating, or tries to show that it is flawed. Such things, after all, are natural in academic enquiry and only to be expected. Academics may attack one another with impunity, whilst allowing one another both the right to develop opinions and the space in which to propagate them. The problem, as was stated in the section entitled **Problems Associated With Social History and Historiography of Science**, is that methodologies intended to remove bias, which are theorized as legitimate on that score, actually function to eliminate certain sectors of historical inquiry, so that, in institutional terms, various areas of study and their related historical methodologies no longer exist. There is no space in which they might be developed. If there is to be space it has to be created anew. The overall outcome is that social history of science, committed as it is to contextualism and concomitant relativist methodologies, is increasingly able to define itself as the only permissible history of science.[30] As such it is in no danger of being cross-examined by any

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[29] **What this means for progress histories of scientific knowledge has been considered at some length.**

[30] **Again, reading The Open University's introduction to the history of science in the Humanities Foundation Course, A 103, and aimed at first-year undergraduates, it is striking how far it reflects the position I have here outlined. History of science,** *'specialises in showing how science is historical, how it has been made'*, **and historians**

really sophisticated arguments concerned with what the best historical practice might be. The discussions on historical practice that do take place do so within the existing constructivist/relativist framework. The framework itself is not investigated nor, indeed, is the question of whether a range of competing historical practices might not be the most illuminating way of dealing with the scientific past.

Most of the reasons why relativism in its various manifestations is so influential in the social history of science have been covered in Chapter One, **Influences on the Development of a Social History of Science** and **A Closer Look**. Social history of science was formulated in response to four interacting areas but received particular support from **SSK** arguments, themselves somewhat influenced by the failure of philosophers of science, at that time, to reconstruct science as rational and progressive.

> **SSK has strongly engaged the attention of historians and philosophers (e.g. Shapin 1982, Shapin and Schaffer 1985, Rudwick 1985, Golinski, 1990, Dear 1995, Fuller 1988, 1992, Rouse 1987, Toulmin 1990, and the boundary lines between what counts as historical or philosophical and what as sociological practice in the area have been blurred to the point of invisibility.**[13]

A further contributory factor is that there is a wide spectrum of relativist methodologies, some of which are sufficiently extreme to make others look moderate and acceptable. Extreme examples are the sort of deeply anthropological cultural relativism which holds that science is simply the tribal mythology of the modern Western tradition and has no more claim to truth than any other such myth. More restrained versions, which were defined in Chapter One, **Methodological Relativism,** as a conscious decision not to interpret the beliefs of a given culture, past or present, in terms of your own culture, are usually acknowledged to be the acceptable form of cultural relativism. It seems, on the face of it and in the absence of the sort of arguments I have summarized in the section entitled **Towards a Historiography of Science**, intuitively obvious that such doctrines are sound and liberal. This divide has been clearly demonstrated by Richard Dawkins, whose stirring

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need to have, *'a historical outlook, an ability to understand the past as far as possible on its own terms'*. **The conclusion is that,** *'Only contextual history can deal fairly with all aspects of* [historical study] ... *I want you to study it this way - to practise what historians of science today preach'*. **See Moore J.,** 'Here's History of Science', 15, 17, 20.

riposte to the anthropological form, in the **1996 Dimbleby Lecture**, was that any of his audience could give Aristotle a lecture on astronomy, or biology that would thrill that great intellect of the ancient world to the very core of his being.[31] Scientific beliefs, he asserted, are cumulative, they are supported by carefully gathered and tested evidence, it is possible to base predictions upon them, and they do produce results. Their only resemblance to myths is that, like myths, they too seek the answers to fundamental questions about the origins of the universe. The similarity between them extends no further than that. It is a subject he pursued in *River Out of Eden*:

> **Western science, acting on good evidence that the moon orbits the Earth a quarter of a million miles away, using Western-designed computers and rockets, has succeeded in placing people on its surface. Tribal science, believing that the moon is just above the treetops, will never touch it outside of dreams.** [14]

Then in a footnote on the same page he states,

> **There are others who, confusingly, also call themselves cultural relativists. ... To them, cultural relativism just means that you cannot understand a culture if you try to interpret its beliefs in terms of your own culture. You have to see each of the culture's beliefs in the context of the culture's other beliefs. I suspect that this sensible form of cultural relativism is the original one ... Sensible relativists should work harder at distancing themselves from the fatuous kind.** [15]

Dawkins here exemplifies a significant misconception and demonstrates just why relativists and non-relativists persist in talking past each other. On the one hand he is persuaded that natural science is the paradigmatic example of progressive learning and cumulative, reliable knowledge. On the other he states that what he calls 'sensible' cultural relativism, at least in the field of anthropology, is acceptable. However, he produces no arguments to persuade even 'sensible' cultural relativists that science is exempt from the dictates of their inherent contextualism. In their terms the past is a different culture and <u>there is no way</u> in which past/earlier knowledge/belief can be connected to present/later

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[31] Of course, one of the historical conditions of historical enquiry, in the absence of reliable time-travel, is that such an eventuality is out of the question. Aristotle is irrevocably dead. However, this does not interfere with the point that I am making, which concerns the extent of the non-communication between relativists and non-relativists.

knowledge/belief. Scientific ideas may well persist but the contexts in which they inhere and in whose terms they display their predictive powers and produce their concomitant results, change. The past is another country and they do do things differently there. Cultural relativists have made much of the fact that often, in the pursuit of the history of scientific ideas, historians and philosophers of science have apparently forgotten this. Lakatos, for example, urged that non-rational explanations could be allowed only when there was a complete failure to find a rational explanation [16] and Laudan has argued on similar lines for his 'arationality principle'.[17] Such arguments imply that science is and always has been a rational endeavour. If the weight is to be taken out of the objections raised against the likes of Lakatos and Laudan, then it is necessary to demonstrate that the problem lies in the fact that their approach biases science in terms of rational explanation when the question of whether or not it is rational ought to be left open. If the kind of assertions articulated by Dawkins are to rest on foundations more convincing than a dislike of the idea that our paradigmatic case has come about through a perpetual, inexplicable, improbable miracle, then they require that some earlier knowledge is going to be relevant to some later knowledge. There remains a gap which can only be bridged by showing that the rational evolution of knowledge is possible.

What is true for Dawkins is true for anyone who aims to reveal the inherent weaknesses in the arguments of the representative sample of social history of science apologists quoted in Chapter One. Consider again some of the assumptions upon which their edifice is built. Scientific truths are held to be objectively real, they are eternal, immutable entities. Scientific knowledge, if it is to be worthy of the title, must deal with such truths. In order to be sure that scientific knowledge has a correct purchase on these timeless and unchanging entities, its statements and beliefs must be established with absolute certainty. The means of establishing truth judgements are, in the last analysis, solely empirical. Scientific rationality emerges when a fixed methodology, or set of rules, is correctly applied to ensure that empirical means get a secure purchase on objective reality. The validity of these assumptions is never questioned, they are simply utilized to prove that the history of science needs none of them and is much better off without them. The proof runs along these lines. There has never yet been a satisfactory elucidation of a logic of science that can answer the philosophical problem of knowledge and rationality. Consequently, there is no algorithm to ensure that knowledge statements really incorporate scientific objectivity and scientific realism and without this purchase on objective reality there can be no criteria for distinguishing truth from error. This explains why scientific truth and scientific methodology have changed over time and between communities, and it also explains the lack of a progressive, non-anachronical historical methodology. Social historians of science and sociologists of scientific knowledge, and some philosophers of science too, have turned these arguments into a justification for their own positions. As was seen in Chapter One, the precise focus varies between individuals within the same discipline, as well as between disciplines. The immediate subject may be Whig historiography, or the impossibility of past truths projecting forward to the present, or the debate about how far nature constructs knowledge about itself, or the idea that the entire pattern of reality is necessarily our own construct. However, the almost universal escape routes from the various perceived horrors are methodological relativism and a thorough historicism. To put it as succinctly as possible, traditional historiographies of science were dependent upon an apparently <u>inescapably</u> problematic philosophy of science and partook of all the concomitant flaws. Context-dependent, sociology of knowledge approaches to the history of science are not subject to any of these problems.

The generalized aim-oriented empiricist framework, of course, undermines the social history of science position because it challenges the assumptions whose negation acts as a foundation for that discipline. By questioning <u>their</u> validity, aim-oriented empiricism robs them of much of the status which made them such a tempting Aunt Sally to those with a sociology of knowledge bent. It suggests that scientific knowledge, far from needing to be <u>guaranteed</u> to correspond to objective reality, can be admitted to be <u>conjectural</u>, and still fulfil the requirement of rationality. As such it can be incorporated into a progressive, interpretative framework that is neither irrational nor relativistic, and which provides criteria for distinguishing scientific knowledge which is reliable from that which is not. The framework does not accept that rationality is something fixed but demonstrates that it is possible to solve the intellectual problem of the rationality of change, which is the normative question of how science ought to proceed if ideally rational in character. A definition of rationality which is not dependent on universal algorithms, and which simultaneously offers a route through the apparent incommensurabilities of relativism and a rationale for the fact that scientific methodologies change over time and between disciplines, destroys the seemingly compelling combination of relativist framework and social unit, which was described in Chapter One, **A Closer Look**. A central aspect of this new definition of rationality is its motivational, intellectual integrity. This provides a rationale for the argument that a rigid relativism does not merely run the risk of 'privacy' (Shapin) but might actually prevent the historian from seeing the past in its own terms. To be critical of the ideas of the past in their own context and to be critical of the ideas in the goal which the historian has formulated, is surely a more reliable way of avoiding any conflation between the two than the implementation of the heroic scenario of the methodological relativist. Another advantage is that the principle of intellectual integrity allows for the possibility that science might not be rational and might not be progressive. Finally, the principle ensures that aim-oriented empiricism provides a progressive historiography of science which is not a factual theory of actual scientific change: rather it allows the question of the extent to which the rational ideal, once established, has been put into actual scientific practice to be amenable to historical investigation. Consequently, in providing strong counter-arguments to the idea that relativism reigns because rationality is defunct it proves that it is not the sort of rationalistic theory which is opposed by the non-rationalistic, relativist accounts of social historians of science. In short, the aim-oriented empiricist generalized framework thoroughly undermines the entire foundation on which rests the social history of science's critique of the intellectual dimension.

However, as has already been established, aim-oriented empiricism is not biased against social considerations in the way that social history of science is biased against progressive histories of scientific ideas. For one thing, it highlights the advisability of tolerance about what constitute legitimate objects of historical study: consequently, the aim of studying the history of science in a social, or contextual manner, is a perfectly proper one.

It all depends on what the goal of the enterprise is taken to be. More significantly, the aforementioned motivational intellectual integrity also demands that we recognise that there could be a whole range of inputs into any given definition of rationality, which could include human motivations and social interconnections. Science is not irrational by virtue of being a part of the social (and therefore only amenable to interpretations in terms of social considerations) but it is irrational if it doesn't produce a good methodology, itself a fusion of the social and the intellectual. Indeed, rationality demands that the current methodology (carefully produced under conditions of the strictest intellectual integrity) is taken seriously as part of the social fabric. All of the social world, including the scientific, is composed of human beings pursuing goals for a multitude of reasons, including intellectual reasons. Making sense of people engaged in science is a special case of the more general case of understanding people engaged in any social activity. The intellectual and the rational, which govern the sphere of knowledge, ideas, arguments, hypotheses, theories, methodologies, problems, and even the gathering of empirical data and the production of scientific texts, are social dimensions. The scientific community is a social unit interacting socially by means of a shared language, a common currency which takes the form of the intellectual dimension just described. The social character of the intellectual dimension is not in question. However,

> **The social world exists in the physical universe. Claims to knowledge, though personal or social, may be considered from the standpoint of truth, their adequacy to the facts. Arguments, though social in character ... may nevertheless be assessed from the standpoint of their validity or invalidity. The social character of science is not at odds with its intellectual and rationalistic character at all.** [18]

The intellectual dimension, when construed in such social terms, does not lose its traditional characteristics and strengths, its evaluative role, or its function in aim or goal articulation. Intellectual issues can still be evaluated over time and in respect to their validity, or the absence of it. To judge something intellectually is not to exclude external factors, which may well range from the psychological to the political, but is to bring them in where relevant. A history of science concerned with the progress of scientific ideas is an example of the sort of social history that covers long-standing, goal-oriented enterprises of which it is legitimate to make evaluative judgements with regard to whether or not that goal was achieved. The social dimension of the intellectual does not render the history of science less of an intellectual history, nor does it invalidate its methods, which involve the making of value judgements. Intellectual or value judgements, as Hall indicates, are an indispensable part of <u>any</u> historical analysis and not merely those areas that can be subsumed under the category of the history of ideas. They are just as necessary for the delineation of areas of study, which are not too limited in variety and scope, and the identification of important problems in social, political, and economic history.

Consequently, working from an aim-oriented empiricist perspective does not mean that an historian is being rationalistically intellectual. Aim-oriented empiricism, of course, is not <u>unique</u> in suggesting some sort of a reconciliation between external social history of science and internal intellectual history of science. Popper, who would certainly never qualify as an aim-oriented empiricist, also sees science and rationality as essentially social in character. Aim-oriented rationality does, however, guarantee that an historian works within an intellectual history that is actually an aspect of the social and from which the old dichotomies of science and metaphysics, and scientific/intellectual and social considerations, have vanished. The end result is that the aim-oriented empiricist evolving framework avoids being a relativist framework and develops, as a central aspect, a motivational intellectual integrity, a new concept of rationality that facilitates the inclusion of the intellectual dimension into the social fabric and legitimates the role of intellectual or value judgements in all forms of history. The history of science, I would submit, is much improved as a result.

The overriding conclusion is that in aim-oriented empiricism there does exist a philosophy of science which interprets science as rational, evolutionary, rule-governed (in a <u>carefully</u> defined sense), and definitely non-relativistic and which <u>can</u> provide a legitimate, enlightening viewpoint from which to study the history of science. It reinstates the possibility that the history of science can encompass a history of intellectual progress. It also demonstrates just why the social fabric, which includes the intellectual, is rationally handled only when the sort of evaluative methodology traditionally associated with the intellectual dimension is employed. This general position is supported by a careful redefinition of what constitutes the true core of Whig history and the result is that the concepts of 'perspectives of the present' and 'histories of progress', suitably defined, can be shown to have a legitimate role in the history of science. Finally, as will be seen in the next chapter, it shows the extent to which Galileo's traditional title of 'the father of the Scientific Revolution' can be reinstated.[32] By extension it suggests, as a topic for further research, that the Scientific Revolution itself did, in fact, exist [33] as a singularly important episode in the history of humanity; an episode which can be meaningfully interpreted as an intellectual metamorphosis involving metaphysical systems.

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[32] **For examples of the view that Galileo can no longer be called the 'father of the Scientific Revolution', see Hall, A. R.,** 'Was Galileo a Metaphysicist?' **and MacLachlan, J.,** 'Drake Against the Philosophers', **in Levere, T. H. and Shea, W. R. (eds.),** *Nature, Experiment, and the Sciences*, Dordrecht: Kluwer, 1990, 105-121 and 123-144.

[33] **For a collection of papers questioning the validity of the Scientific Revolution as a narrative category see the** ***BJHS,*** 1993, **26.**

[1] Maxwell, N., 'The Rationality of Scientific Discovery: Parts One and Two', *Phil. Sci.* 41, 1974, 123-53 and 247-95.

[2] Maxwell, N., *From Knowledge to Wisdom*, Oxford: Blackwell, 1984, 205.

[3] Maxwell, N., *The Comprehensibility of the Universe,* Oxford: Oxford University Press, 1998, forthcoming.

[4] Maxwell, N., *From Knowledge to Wisdom,* 237.

[5] ibid.

[6] Maxwell, N., *The Comprehensibility of the Universe*, forthcoming.

[7] Hume, D., *A Treatise on Human Nature*, Book 1: Fontana/Collins, 1962, part III, section XII.

[8] Dawkins, R., *River Out of Eden*, London: Weidenfeld and Nicolson, 1995, 31-32.

[9] Conant J. B., *On Understanding Science: A Historical Approach,* New York: New American Library, 1951.

[10] Jacob, M. C., *The Newtonians and the English Revolution 1689-1720*, Hassocks: Harvester, 1976.

[11]Kragh, H. (trans.), *An Introduction to the History of Science,* Cambridge: Cambridge University Press, 1987, 131.

[12] Maxwell, N., *From Knowledge to Wisdom*, 238.

[13] Shapin, S., 'Here and Everywhere', 291.

[14] Dawkins, R., *River Out of Eden*, 32.

[15] ibid.

[16] Lakatos, I., *The Methodology of Scientific Research Programmes*, Cambridge: Cambridge University Press, 1978.

[17] Laudan, L., *Progress and Its Problems: Towards a Theory of Scientific Growth,* London: Routledge and Kegan Paul, 1977, 202.

[18] Maxwell N., 'The Odd Couple', (unpublished).

CHAPTER THREE

GALILEO AND THE AIM-ORIENTED EMPIRICIST PERSPECTIVE

Introduction

This chapter proposes to take the bull by the horns and consider what Galileo would look like from an aim-oriented empiricist perspective. This, of course, will not imply that this perspective is the only one from which it is meaningful to examine Galileo, nor that it is necessarily superior to all others, but simply that it is possible and brings its own illuminating strengths to the subject area. It might be argued that as aim-oriented empiricism has been presented as a system which makes rational sense of science, and is therefore held to be superior to rival philosophies of science, it cannot be put forward as an arbitrary alternative as if it were a system of measurement. However, that is to rather miss the point, which is that social history of science can, by all means, explain knowledge claims symmetrically, without reference to who was, or is now, thought to be orthodox, heretical, right or wrong. They can, if they so wish, chose to see the boundaries of science as a series of constructs whose origins and means of construction are to be inveigled from a close study of the primary sources. What they should not do is base the legitimacy of the symmetry principle upon the assumption that historiography must not 'know in advance' what should count as science. Progress history of science needs to know, at least, what would constitute an ideally rational science if it is to be possible to judge how far science measures up to this ideal. The demands of Chapters One and Two require that solutions be found to certain problems and mostly these solutions can only be fully demonstrated through the medium of the case study. It is necessary to show that it is possible to draw out the progressive aspects of historical work, those parts which legitimately comprise a progressive history of scientific ideas. This endeavour hinges on the aim-oriented empiricist assertion that the development of rationality has a historical dimension to it. That is to say it depicts a historical dimension up to, roughly speaking, **level 4** and from **levels 5-10** it perhaps depicts an ahistorical dimension. The fact that it is so constituted allows it to avoid historical relativism and form the bridge between paradigms or research programmes, or whatever is designated to invoke discontinuity and reinforce the superiority of context or the ineluctable ascendancy of social causes. Such approaches may satisfy the demands of historicism but they do not permit a history of the development, implementation, and growth of scientific ideas.

So far this thesis has demonstrated that such a historiography is desirable: if history is confined to interpreting historical actions in historical actors' terms then certain perspectives on the past are being proscribed in an unacceptable manner. Furthermore, the historical dimension displayed by the aim-oriented empiricist solution to the problem of induction offers one means of formulating such a historiography. It will be remembered that the generalized aim-oriented empiricist framework chooses metaphysical theses at **level 3** to **level 10** according to the role they have in fostering empirical knowledge. This is true even of **levels 9** and **10** which, although not amenable to empirical support, need never be revised because making such assumptions can only help the growth of knowledge. From **level 8** to **level 3**, the framework provides science with a matrix of metaphysical assumptions and affiliated methods, which are increasingly likely to require revision as one descends the scale. This is because, as they become increasingly specific, such metaphysical assumptions about the nature of the universe are also increasingly likely to be <u>incorrect</u>: the fundamentally problematic nature of such assumptions makes this inevitable. However, should they nevertheless prove to be increasingly productive heuristically and methodologically - and capable of being cultivated and evaluated from the viewpoint of their genuine competency in advancing the growth of empirical knowledge - then they form a framework in which they remain capable of being improved in the light of empirical success or failure. In this way a dilemma is solved, science being able to select metaphysical assumptions which, although probably strictly false, are sufficiently precise and productive to be heuristically and methodologically rewarding with regard to the empirical fecundity of the research programmes they engender.

An unavoidable consequence of this descent from **level 8** to **level 3** is that the correlative assumptions and associated methods are progressively dependent on <u>history</u>, on the way in which the search for knowledge has developed in the past right up to the present. If the universe were constituted differently, perhaps being such that a Paracelsian cosmology most nearly approached it, then the metaphysical assumptions and affiliated methods that would have developed from the sixteenth century onwards would be very different from those that have developed. Such assumptions would need to encompass the idea that knowledge exists within things as their essence, or virtue, all parts of Creation being linked by their shared virtues. They would also have to accommodate the further level of organisation which states that man is the centre of Creation and a small replica of it, uniting in himself all the powers of its constituents. In other words, **level 4** and **3** ideas would be radically different from those now current and the Paracelsian methodological requirement that knowledge be intuited, so foreign to us, would be quite acceptable, given the existence of a mutual sympathy between individuals and natural objects. **Level 5 C** would remain but its status would not be what aim-oriented empiricism expects. The Paracelsian macro-cosmos is comprehensible to man because the microcosmic nature of man renders it so: the macrocosom and the microcosom are the visible reflection of the unseen work of the Deity and that work is conjoined in such a manner as to ensure that the former is fully comprehensible to any member of the latter who works at it in the approved manner.[1]

This is a fanciful example but it does not exhaust the limits of what is conceivable. It is just possible that the universe may in the future turn out to be so very different from our current understanding of it that revisions at **level 5 C** might be forced upon us. There might be a **level 5** rival to the comprehensibility thesis which does not postulate comprehensibility in any form, yet fulfils the necessary requirements of being compatible with the **level 9** thesis and of engendering an empirical research programme even more successful in terms of finding explanatory theories than the one produced by modern science! However, although in principle possible, this is a highly implausible scenario and one which anyway still assumes some degree of comprehensibility, to the extent that explanations exist to be found. The rejection of comprehensibility at **levels 5, 6** and **7** entails that knowledge must be acquired through the medium of assumptions and methods that do not involve the development of explanatory theories. History shows that almost all prolonged attempts to improve knowledge and understanding of nature, even

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[1] Paracelsus thought that man was the centre of creation as well as being a small replica of it, uniting in himself all the powers of the constituents of the surrounding cosmos. Thus the organs of the human body, plants, minerals, even the planets were all linked by their shared virtues and it was therefore fruitful to seek out their mutual sympathies by means of a kind of intuitive trance rather than by studying books and employing logical deduction. The individual who achieved this transcendental knowledge was nothing less than the Magus - which is how Paracelsus saw himself.

pre-scientific ones, have at the very least assumed that explanations of some sort exist as to why things happen as they do. Frequently they have tried to develop explanatory theories, as the Paracelsian example shows. Aim-oriented empiricism interprets this struggle to improve knowledge as evidence of the implicit acceptance, throughout much of history, of the **level 5** assumption that the universe is comprehensible, or at least the **level 6** assumption of near-comprehensibility. Furthermore it asserts that given the generality and unrivalled empirical efficacy of the comprehensibility thesis, in terms of the tremendous (apparent) success that the search for explanatory theories has met with so far, it is fairly unlikely to be in need of revision at any time.

However even such distinctions as being possible and desirable are perhaps not sufficient to wholeheartedly recommend a historiography. In performing its function it ought also to illuminate the historical record in some fruitful way, perhaps by solving long-standing historical puzzles, or by making a positive contribution to an established argument, or by indicating something which no previous interpretation has alighted upon. The degree to which all of these aims can be accomplished will now be substantiated by examining Galileo's work in terms of various of the levels of the generalized aim-oriented empiricist framework, **levels 1** to **5** in particular. This will be done with the minimum of apology and qualification, in order to minimize disruptions to the flow of the analysis and give as clear and succinct a picture as possible of how Galileo's work appears from such a perspective. The purpose of the exercise is to demonstrate the degree to which Galileo can be fitted into a progressive historiography. Such an exercise has little to do with Galileo in context but a lot to do with how his ideas and work form a part of 'the story of how we got here'. During the exercise it will become clear that the aim-oriented empiricist framework really <u>is</u> an option for exegesis and does not produce rational reconstructions. The scene will be set by commencing with **levels 5** to **3**. This will provide the skeleton which will then be fleshed out using Galileo's actual work at **levels 1** and **2**. The methodological strategies which he utilized to direct this work will be examined for the (usually) implicit support that they give to the higher levels and the frequency with which they are marshalled to destructively clash with, or neutralize, alternative blueprints.

Level 5: the Comprehensibility Thesis

The generalized framework of aim-oriented empiricism might appear complicated at first sight but the basic idea of aim-oriented empiricism, that science makes a hierarchy of increasingly attenuated assumptions concerning the comprehensibility and knowability of the universe, is exceptionally simple. The particular way in which it is knowable can only be uncovered as we proceed but the best bet is enshrined in the **level 5** comprehensibility thesis, that it is fully comprehensible. Thus **level 5 C** is to be held as a legitimate, if conjectural, part of scientific knowledge because it maximizes the possibility of our obtaining knowledge of nature. It makes no concrete claim for the way in which the universe is to be defined but confines itself to the assertion that the universe is fully comprehensible in that there exists something which is unchanging and universally common to all phenomena, in an invariant form, and in whose terms all phenomena can, in principle, be explained. There are a number of ways of postulating the way in which the universe might be comprehensible, as the Paracelsus example reminds us.[2] However, aim-oriented empiricism holds that the best version is **level 4 P**, that the universe is physically comprehensible: it is **level 5 C** and **level 4 P** that, although usually implicitly held, have sustained the hugely successful empirical research programme of modern science.[3]

Given all of the above requirements, to what degree does Galileo measure up to them? To begin with **level 5** there are two striking and related ways in which he totally fails to conform. Firstly, all the evidence indicates that Galileo was thinking in ontological terms, but rather than construct the sort of progressive but conjectural knowledge that is associated with aim-oriented empiricism he aimed to construct a body of certain, provable knowledge. In other words there is no way in which he can be interpreted as holding the comprehensibility of the universe as a legitimate, if conjectural,

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[2] **It could be that events are unfolding in accordance with some kind of cosmic computer programme, or in accordance with some form of overall cosmic goal. In such cases events would be explained in terms of the programme or the goal. It might be that God exists and all natural phenomena are the outcome of the Divine Will. All of these possibilities fulfil the** level 5 **requirement of the existence of an ultimate, invariant entity which, in some sense, determines all change and diversity in the universe.**

[3] **An example of this, utilizing the idea of an invariant and universal something, in whose terms all phenomena can be explained, is the seventeenth-century discovery that phenomena as seemingly diverse as astronomical and terrestrial motions are really all facets of the one kind of phenomenon, that of objects moving and interacting in accordance with Newtonian Theory.**

part of scientific knowledge. To begin with there is no doubt that Nature is prior to man: the human mind is not the measure of what can occur in nature and to argue as though,

> **nature had first made the brain of man and then arranged everything to conform to the capacity of his intellect.** [1]

is to fall into error. The universe exists to be discovered. Secondly, the mathematical or rather geometrical structure that Galileo postulated for this universe indicates his desire for a transcendent reality which specified what was essential about bodies and also brought certainty into physics. Galileo was always inclined to arrange things according to the ideal of geometry: it is evident from the discussions about friction and air resistance in *De Motu* that he was thinking about the possibility of simple mathematical laws of motion as early as 1590.[2] When, some fourteen years later, he did discover the progression that constituted the odd numbers law and from thence deduced the law of free fall [3] he had uncovered something so elegant and simple that, for him, it just had to represent the essential 'disembodied' reality of things behind the misleading and changing appearances of the world. He could not interpret it differently because geometry simply was the eternal reality, an expression of necessary truths. Understanding natural phenomena had to be a more general case of understanding the nature of Euclidean space.

There is a threefold argument to be made in support of this. The first part depends upon Galileo's predilection, recognised by both Maier and Barbour, for intuitively regarding motion as something that occurs relative to an independent and real space, rather than to other bodies. Although he gives no general discussion on the nature of motion, geometry, for Galileo, was never simply a calculating device, a means of pinpointing the relations existing between bodies contingent upon a constantly changing world. It can, of course, be argued that his celebrated thought experiments, especially the one concerning the investigations conducted in the ship's cabin,[4] imply the relativity of motion, *'the unoperative nature of motion common to a collection of bodies'*.[5] A relational concept is also implicit in his idea of motion going towards the centre of the earth in a straight line, in order to reach its proper place, and then moving around the earth in a circle so as to remain in that place. Galileo himself even wrote, *'Motion, in so far as it is and acts as motion, to that extent exists relative to things that lack it.'* With respect to the example of the artist drawing a picture on the deck of a ship, in which the true path of the pen emerges as the minutest deviations appearing around a long line stretching from Venice to Aleppo, Galileo remarked, *'You are not the first to feel a great repugnance towards recognizing this inoperative quality of motion among the things which share it in common.'*.[6] However, that same quotation then states, *'and among things which all share equally in any motion, it does not act and it is as if it did not exist.'* This final phrase implies that motion is indeed an actual objective fact, which takes place in real, independent space, rather than relative to other objects. That body X still moves relative to body Y is as much an objective fact for relativists as for absolutists but the important point is that when thinking about motion Galileo seemed to abandon the relational concepts of position which typified his thinking about the instantaneous configuration of bodies in the world and assumed that motion takes place in space. Barbour cites some credible evidence in favour of this.[7] Galileo makes no sustained and convincing assertion to the effect that motion is something which is purely relative, nor even shows any interest in defining position by means of something material and visible. When, in the First Day of the *Dialogue*, he discusses primordial chaos where everything is in a state of motion, the only way to make sense of the straight motions *'which nature would have very properly used'* to impose order is to regard them as occurring with respect to space. His constant reiteration of the fundamental belief that *'Eternal motion and permanent rest are such important events in nature and so very different from each other'*, particularly with regard to the motion of the earth which is either moving or stationary demands that, once again, there has to be some ultimate standard.[8] It is only when motion is thought of as taking place relative to space that it becomes possible to state that any given body is at rest or in motion. If motion is purely relative then it can only be considered in terms of any number of bodies up to an entire system of bodies.

> **Indeed, without it velocity as a unitary concept dissolves - a single velocity in space is replaced by a huge and completely indeterminate number of relations to other objects in the universe. Had Galileo stopped to consider that daunting prospect, he would surely never have made any progress at all. Thus motionics rested on space, strengthening an already strong geometrical predisposition.** [9]

The same point is also made with regard to Galileo's attitude to the fixed stars and his Theory of the Tides. The *Dialogue* and the *Two New Sciences* are full of passages in which the concepts that Galileo is struggling to formulate depend upon space as an underlying reality. The stars, distributed over a finite region but at an immense distance from the sun, *'the fixed stars, which are so many suns, agree with our sun enjoying perpetual rest'*.[10] In other words, they were fixed relative to one another and in an overall state of rest. With regard to the argument that the tides might be the result of the earth's motion being accelerated and decelerated, it is not a foregone conclusion that Galileo referred this motion to the fixed stars. Although Mach famously wrote,

> **It is noteworthy that Galileo in his theory of the tides treats the first dynamic problem of space without troubling himself about the new system of co-ordinates. In the most naive manner he considers the fixed stars as the new system of reference.** [11]

it might be the case that Galileo did not regard the stars at all.

> **It must happen that in coupling the diurnal motion with the annual, there results an absolute motion of the parts of the surface which is at one time very much accelerated and at another retarded by the same amount.** [12]

Bearing in mind his reaction to Kepler's 'occult' suggestion that the moon affected the tides, would Galileo really have wished to postulate a causal connection between the tides and the speed of the earth relative to the stars? It is much more likely that the parts of the earth are accelerated and decelerated relative to <u>space</u> - which happens to contain stars. So, bearing in mind that space, to Galileo, meant the space of Euclidean geometry, and given that his concept of bodies in motion cannot involve the disposition of objects relative to one another, it seems highly unlikely that the geometrical figures he used to characterize it are not every bit as real as space itself. Following in that mathematical sciences tradition of antiquity and the Medieval period, which identified Euclidean with real space, Galileo regarded the geometric figures that he was able to ascribe to physical events as truly real.

The second part of the argument turns on the realisation that this reality is a <u>transcendent</u> reality, which had to be cast in the form of mathematically <u>precise</u> hypotheses which could only be approximately glimpsed through empirically 'interfering' with everyday transitory events. Galileo's experiments have to the precise theory the same relationship as exists between the diagram in Euclidean geometry and the precise theorem. Just as the theorem doesn't precisely hold true for the diagram, so the accurate, exact, geometrical relationships intrinsic to motions are only approximated in reality. All the pages of 'proofs' in the *Discourses* are explicitly presented as being that which would obtain in the ideal case, with ideal objects on ideal planes, where *'there are* *no chance or outside resistances'*.[13] The third part, utilizing the combination of transcendence and reality, undermines any suggestion that the mathematical properties of bodies are not truly essential but merely those (perfectly real) properties upon which it is possible to gain a purchase. Of course Galileo explicitly abjures the search for Scholastic essences, for example in the *Letter on Sunspots* where he asserts that he is only interested in the properties of things and observed events: however, this was surely just a part of the general repudiation of Aristotelianism, of the futile logic-chopping and obscurantism, or what Locke later called the *'curious and inexplicable web of perplexed words'*.[4] Galileo discovered laws of such apparent geometrical perfection that, by their very nature, they surely indicated to him that the essential thing about motion was the mathematics which caused it to operate in the way that it did. Barbour has suggested that Galileo's theory of motions is foundational in the same sense as atomic, or corpuscular theory. The argument hinges on the use Galileo made of the individual motions themselves. He introduced the hitherto missing quantitative element and he demonstrated what could be done with the motions. Although he couldn't explain the world in terms of fundamental, material, geometric entities, what he actually produced entailed primordial natural motions fulfilling the role of fundamental entities.[14] Gravity, of course, presented something of a problem but, as will become clear in a later section, Galileo simply managed without any concrete representation of the way in which motion and acceleration are produced, whilst all the while leaving the door open to the possibility of future mathematization. Meanwhile, the discovery that translating geometrical figures into the physics of motion produced solutions that agree with the way those things are experienced was powerful support for the belief that nothing could be more essential about the phenomena than the geometrical regularities which governed it.

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[4] Aristotle devised a considerable body of technical terms like substance, subject, predicate, matter, and form which were everyday, ancient Greek words to which he attached particularly specific but related meanings. Latin lacked many of the words needed to correspond well with the Greek ones, forcing many early translators and commentators to distort the meanings of some Latin words and completely invent others. In addition, both languages have an amazing ability to create syntactical variants on existing words, e.g. the Latin for 'what' (quid), plus a suffix to turn it into a noun (itas) becomes quidditas, the Scholastic translation of the Greek noun-phrase, the 'that which is to be' of something. The same exercise in English would produce 'whatness'! The seventeenth century heavily criticized the schools for their obscurantism and futile logic-chopping and as early as the sixteenth century an important part of the Scholastic curriculum, the trio of grammar, rhetorica and logic known as the trivium, had given rise to our words 'trivia' and 'trivial'.

Galileo was unable to mathematize any further but the primary/secondary distinction made in *The Assayer*, implying that geometry is the essence of all physical phenomena, both supports and extends this argument. Bearing in mind that he had succeeded in explaining a restricted area of phenomena by means of geometrical motions analogous to an atomic theory, it seems impossible that bodies, which are only allowed to consist of *'shapes, numbers, sizes and slow or rapid movements'* and whose configurations are responsible for all sensible properties, can in essence be anything other than geometrical shapes in motion.[15] As Hall has argued,

> **On Galileo's position whatever is not mathematical in nature is not really there at all; it is merely a sensation, a response of the human nervous system - or the imagination - to stimuli which are mathematical.** [16]

This makes it unlikely that his rejection of Aristotelian 'essences' was also a rejection of the concept of fundamental defining properties as such.[5] Geometrical properties have to be the essence of matter because nothing else exists to fulfil that role. For the same reason they are the only means, through the powers of local arrangements of matter, of our being able to experience bodies at all. Galileo had no way of capturing these ideas in mathematical formulae but it is clear that he had framed the concept in terms amenable to mathematization. As well as being the most obvious way of trying to picture the fundamental mathematical character of the world, atomism is also a doctrine perfectly adapted to explaining change in terms of motion. Its not a great step from here to postulating that there is something, be it a property of space or of the objects themselves, which is in some sense responsible for the way things behave and move and interact. It is not possible to state the degree to which Galileo saw this as a legitimate task for natural philosophy but what is certain is that for him mathematics expressed the very essence of physical reality.

> **Philosophy is written in this grand book the universe, which stands continually open to our gaze. But the book cannot be understood unless one first learns to comprehend the language and read the alphabet in which it is composed. It is written in the language of mathematics and its characters are triangles, circles, and other geometric figures, without which it is humanly impossible to understand a single world of it; without these one wanders about in a dark labyrinth.** [17]

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[5] **Indeed, the primary/secondary distinction actually connects with the Aristotelian 'essence' in so far as that entity constituted the necessary characteristic which, when possessed by a substance, or distinctive species, determined its unique make-up.**

The 'language of mathematics' can be interpreted as the language of the foundational, 'atomic' theory of geometric motions. The geometric figures that Galileo ascribed to these events were as truly real as they were transcendent, the natural outcome of the fundamental belief that understanding natural phenomena could only be a more general case of understanding the nature of Euclidean space. We will be returning to this subject in due course but in the meantime let there be no doubt that Galileo believed that to mathematize nature was to gain a purchase upon what was essential, rather than simply capturable, about phenomena.

What Galileo did recognize as problematic was the formulation and application of the correct interpretative abstract entity. Simplicio objects that although a sphere may well touch a plane in one point in the abstract, in reality *'Material spheres are subject to many accidents'* so that *'it is hard to find the perfect sphere'*.[18] Salviati draws the clever conclusion that nevertheless things in the abstract have the same requirements as in reality,

> **But I tell you even in the abstract, an immaterial sphere which is not a perfect sphere can touch an immaterial plane which is not perfectly flat in not one point, but over a part of its surface, so that what happens in the concrete up to this point happens the same way in the abstract.** [19]

Imperfect material entities correspond to imperfect abstract entities rather than perfect ones: the problem is always to find out the transcendent, mathematical reality that governs the everyday physical process. The shortcomings of the human intellect might make this a difficult task because

> **with regard to the multitude of intelligibles, which are infinite, the human understanding is as nothing even if it understands a thousand propositions; for a thousand in relation to infinity is zero** .[20]

but notwithstanding it is the only viable methodology if certain knowledge of that which is essential is the goal.[6] Scholars who maintain that with regard to motion Galileo was ignoring what was essential and concentrating on what he could actually get a purchase on - mathematical regularities - have to also maintain that his metaphysical musings concerning mechanical theory are firmly 'non-scientific'. A perspective which begins

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[6] It is a particular characteristic of the *Dialogue* that the arguments both for and against Copernican heliocentrism are investigated by means of thorough mathematical analysis. Empirical considerations are only brought in after such a process has been undertaken, when they can be ordered by mathematics.

with the premise that metaphysics is a part of science demonstrates how the idea that transcendent mathematics is the essence of reality runs, like a thread, through the physical and the metaphysical alike. Assuredly, Galileo does not conform to the model of progressive but conjectural knowledge that is associated with aim-oriented empiricism.

Secondly, in requiring certain knowledge in this way, he also violates the aim-oriented empiricist requirement that at **level 5** there should be no considerations which might influence the formulation, at **level 4**, of any one particular way of articulating the way in which the universe is held to be comprehensible. Galileo can be interpreted as utilizing God at something like **level 5**, to try and ensure that the necessity with which mathematical truths are known is translated into the **level 4** and **level 3** blueprints that govern the study of natural phenomena, thus guaranteeing the growth of objectively certain empirical knowledge. Of course, the idea that translating natural phenomena into geometrical figures will render them comprehensible to the human understanding as objective certainties is a fallacious one. However, Galileo would hardly have recognised that he had, in effect, conflated fruitfulness with certainty.[7] To begin with he addressed the higher question of why mathematical truths such as those of Euclidean geometry should be accessible to us. The answer is that the one being whose comprehension of nature is total is God: the divine mind is infinitely superior to the mortal mind and so can contemplate infinitely many propositions in geometry and arithmetic simultaneously and in their entirety even though they can only be known by a step-wise process. Those who understand such propositions succeed *'in understanding necessity beyond which there can be no greater sureness'*.[21] Of course, the *'book of Nature'* derives from God, its Creator, just as much as does the Bible but both were independent repositories of divine truth, the former accessible to us through certain faculties bestowed upon us, who are also part of that same Creation.

> **But I do not feel obliged to believe that that same God who has endowed us with senses, reason, and intellect has intended to forgo their use and by some other means to give us knowledge *which we can attain by them*. He would not require us to deny sense and reason**

[7] Although in its earliest days Euclidean geometry had sprung from empirical considerations it had, after Euclid, been presented as a deductive system of certain truths comprising relatively few, easy axioms from which were derived some quite startling theorems about the nature of physical objects. A good example is the theory of the five regular solids, which Kepler wove into his cosmology as a means of explaining the number of planets, and the known proportions of their orbits, as a function of his Euclidean model.

in physical matters which are set before our eyes and minds by direct experience or necessary demonstrations. [22] (my italics)

Thus it emerges that there is one particular branch of knowledge in which God has ensured that human beings are equipped to be particularly competent, in which they may aspire to the level of the divine paradigm of comprehension and understanding. Add to this the conviction that the physical universe has a mathematical structure and it emerges that Galileo's physical world is knowable by virtue of being written (by God) in the one language from which (God has ensured that) we can competently gather certain knowledge of it.

It is clear that the role of God does exist at something like **level 5** because there is no suggestion of a lower **level** methodological role: the comprehensibility of the universe is not dependent upon presence and activity of God but rather on our being a part of that same Creation and endowed by the Creator with the one ability that is needful for us to have the means of fully understanding it. However, the drive to ensure that mathematical truths are necessarily translated into the **level 4** and **level 3** blueprints that govern the study of natural phenomena means that the universe's having an inherently mathematical structure has become a necessary condition of the very possibility of obtaining this kind of strong, provable knowledge about it. Contrary to what aim-oriented empiricism would expect, Galileo went to great lengths to influence the formulation, at **level 4**, of a particular version of the comprehensibility of the universe. His manner of so doing was *a prioristic* in so far as it ensured that proper knowledge can only take a particular form.[8] He turned the mathematization of nature into a prerequisite for obtaining objectively certain knowledge by making it a function of his definition of the way in which the universe is to be understood. When all the evidence given for **level 5** is taken together there can be no suggestion that Galileo thought about the comprehensibility of the universe in any strict aim-oriented empiricist

[8] Frequently Galileo has been taken to have been an *a priorist* when what he was really requiring of us was that we should think about familiar sensory experiences, or extrapolate from commonplace objects, or processes, in the correct, geometrical way. Such everyday experiences included the stone dropped from the mast of a uniformly moving ship, rolling a ball down or up an incline, throwing a heavy ball and a wad of cotton, observing how flies keep up with a moving horse, shooting arrows from moving carriages, firing cannons, and the experiment in the cabin of the ship.

sense, although an aim-oriented empiricist interpretation does throw new light on his work by showing the precise nature of its metaphysical dimension.

The Articulation of the Level 4 P Blueprint

Having arrived at **level 4** we once again find that Galileo does not totally conform to aim-oriented empiricist requirements. The above might suggest that Galileo's system can be called, in some sense, <u>physicalist</u>: the effort that went into ensuring the mathematical character of natural phenomena implies this. The question can be illuminated by continuing to focus on Galileo's concept of the relationship between God and the natural world. Galileo regarded God very much as the Creator, who could be glorified through the study of His works. So if Galileo's universe is comprehensible because a beneficent God exists, the material world <u>conforming in some way to physicalism because of some manifestation of the will of God</u>, then it is clear that the generalized aim-oriented empiricist step **r = 4** (given **level 5 C** the best available **level 4** conjecture is **P**), is something else that does not strictly apply in Galileo's system. However, it is not immediately clear whether or not this matters overmuch because it is not obvious that physicalism is an <u>inherently</u> more rational, preferable view to the idea of a beneficent God. This is because all views which hold that the universe is comprehensible, be they monotheistic, multi-theistic, anthropomorphic, or physicalist, <u>have</u> to confront two problems. Firstly, they have to confront the problem of formulating the nature of that which persists unchanged throughout all change and diversity, and which determines the way in which things do change.[9] Secondly, they need to demonstrate how it is possible to capture this basic idea in terms of a mathematically precise, testable, dynamic theory, such as Newtonian theory.[10] The God thesis fulfils the requirement of the existence of an ultimate, invariant entity which, <u>in some sense</u>,

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[9] **Maxwell has postulated many possible universes in which** level 5 C **does not obtain: these include 'local physicalism', in which the universe only behaves as if physicalism is true in our particular space-time region; 'accidental physicalism', in which events occur purely at random; 'physicalism with intermittent miracles', and 'asymptotic physicalism', in which the universe requires infinitely many theoretical revolutions before its ultimate structure can be correctly and completely captured by means of a true, unified, 'theory of everything'. In none of them does the problem arise of having to decide what it is that is unchanging but which also determines change.**

[10] **This, of course, was the requirement which gave Descartes such trouble. His corpuscular theory fulfils the first requirement but not in a configuration that is amenable to formulating a satisfactory response to the second requirement.**

determines all change and diversity in the universe. It also allows for the possibility that all such change could exhibit stable and precise regularities that prove to be amenable to mathematization. It even explains why the physical universe is comprehensible to us.[11] All in all it appears to satisfy points (i) to (v) below quite as well as **level 4 P**.

(i) They both hold out an equally greater hope of promoting the growth of knowledge than any other rival (at that level) whether true or false.

(ii) They both hold out an equally greater hope of promoting the growth of knowledge than any other rival (at that level) if true.

(iii) They both do equally better justice to apparent scientific progress than any other rival (at that level). the material world conforming in some way to physicalism because of some manifestation of the will of God,

(iv) They both are inherently more plausible, more likely to be true, than any other rival (if only because they have less content).

(v) Accepting either of them does less damage to the growth of knowledge, if they are false, than any other rival at that level.

In fact the only criteria which the 'beneficent God' thesis seems to fail is that of acknowledging, at least as a possibility, that the universe is impersonal in character.[12] All those who engaged in the generation of modern physics in the early modern period held some version of the God thesis: natural philosophy was always a deeply religious undertaking.[13] Consequently, progress in physical science, whilst linked with the

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[11] **Galileo provides an answer to the question that so puzzled Einstein, although not one that either he or aim-oriented empiricism could accept. Einstein felt that the most incomprehensible thing about the universe is that it is comprehensible to us. Galileo's system enables us to understand why the universe is comprehensible to us: it is comprehensible because God exists and this provides a much stronger guarantee for the veracity of the knowledge thereby obtained than a scheme that can do no more that conjecture that the universe is comprehensible in order to maximize the possibility of our obtaining knowledge of nature.**

[12] **Of course, the universe might just not be impersonal in character: it is the top three** levels **(at most) in the generalized aim-oriented empiricist framework (and perhaps only ever** levels 9 and 10**), plus whatever may be assumed at lower levels given the state of knowledge at the time. Notwithstanding physicalism alone has to be considered an improvement on physicalism plus God, unless and until the latter begins to support an (empirically) more successful research programme which would indicate that physicalism on its own is not quite right. This assumes that** P **can solve problems like why the universe should be comprehensible and also explain its lawfulness.**

[13] **The mechanistic ideas of Thomas Hobbes were greeted with outright hostility. Critics of Hobbes such as Seth Ward and Edward Hyde, whilst adhering to the mechanical philosophy in respect of natural phenomena, would never countenance its application to ethics and politics. Boyle, totally seduced by the mechanical philosophy, nevertheless exerted himself to reaffirm the reality of God's Providence and guidance, producing a myriad of arguments to support the position that God is constantly active in supervising and preserving His Creation. See** 'A Free Enquiry into the Vulgarly Received Notion of Nature', **in** M. B. Hall (ed.), *Robert Boyle on Natural Philosophy*, Indiana University Press, 1965. **Hobbes took the materialist view that no other being, including God, existed that was not material and argued that the laws of nature, far from revealing underlying rationality in the structure of the universe, were simply theorems**

characterization in some way of the ubiquitous abiding entity and mathematically capturing the manner of its functioning, has not also depended upon accepting that the universe is impersonal in character. Galileo's belief in God at **level 5** and something close to **P** at **level 4** can count, it would seem, as a <u>version</u> of aim-oriented empiricism.

The Presocratics offer an illuminating illustration of this point. Insofar as they endeavoured to define the constitution of a single variety of universal 'something' and postulate its role in change, they can be interpreted as having been engaged in the process of postulating an <u>impersonal</u>, unchanging, ultimate entity In executing this aim they usually utilized (analogously) one kind of naturally occurring process as a model for all processes or change.[14] Of course the Presocratics were no more than partially successful, particularly with the idea that the universe is impersonal, and their systems retained some decidedly anthropomorphic concepts.[15] More significantly, however, they failed to capture their basic ideas in mathematical terms: indeed faced with the difficulty of realizing the need to develop something of which no one had a clear idea it is not surprising that they failed to realise that it was necessary.[16] It would be fair to say that the Presocratics and the natural philosophers of the seventeenth century both scored two out the necessary three for the formulation of a physicalist universe - but that on each occasion it was a different pair. The significant point, however, is that the seventeenth-

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which people agreed upon in an accordant spirit. See Hobbes, T. *Leviathan* (1651), Harmondsworth: Penguin Books, 1980.

[14] **Hence we get Thales and the basic omnipresent entity of water, which presumably becomes earth and air rather as ordinary water reacts to temperature changes by becoming steam or ice. Anaxanemes thought that the basic entity, air, became water and then earth through a process of condensation or compression. Heraclitus held that the basic process was that of fire, which could either proceed quickly, or slowly and invisibly. Empedocles held that there were four different basic kinds of stuff, earth, air, fire, and water and the process was one of mixing these entities in different amounts. Democritus postulated that the basic building blocks were atoms and that the processes were analogous to particles of sand flying around in a wind, or remaining stacked up like sand particles in a dune. This was a particularly good theory, which was to prove profoundly significant for the early modern period, particularly in that it implies that the study of motion is fundamental to natural philosophy. As all phenomena are reducible to the motion of atoms in the void, this especially applies to motion in the vacuum.**

[15] **For example Thales believed that everything is full of Gods and that magnets are alive; Heraclitus held that fire is a kind of god; Empedocles asserted that the two basic principles which combined the four elements were love and strife, and Anaxagoras thought that mind was the original force behind the universe. Even Democritus couldn't quite sustain the basic concept of impersonality and postulated special mind atoms, which constitute our minds.**

[16] **The vital problem is that of indicating precisely, quantitatively, how the unchanging can determine the way in which change occurs. Atomism solves the conceptual and metaphysical problems of change in that it demonstrates how an entity can retains its identity through change and it offers a credible account of the nature of the ultimate entity. However, without such aids as the real number system, algebra and geometry, the notion of a function, and the differential calculus the dynamic problem of change remains unapproachable. The unexpected discovery of irrational numbers, like $\sqrt{2}$, which cannot be represented as whole number fractions, demolished the Pythagorean belief that integers are fundamental to the nature of the cosmos. Consequently, Greek mathematicians developed mathematics as geometry.**

century version, with its various manifestations the God thesis, was frequently close enough to **P** in the significant, knowledge-developing areas as to allow the burgeoning scientific enterprise to operate as though it were in accordance with the best available **level 4** conjecture's being **P**. That is not to assert that anyone came close to conceiving of physicalism[17] but rather that **level 3** ideas like the corpuscularian hypothesis were consistent with some of the requirements of **level 4 P**.

This was the case with Galileo. Although we have seen that Galileo utilized God to ensure *a priori* that humans are particularly talented in the one area which is a prerequisite for obtaining objectively certain knowledge about nature, he nevertheless proscribed the relationship between God and natural philosophy so carefully that it is only too easy to interpret his universe as an autonomous, physical system commensurate with the requirements of **level 4 P**. From about 1611 onwards, Galileo became increasingly embroiled in a violent controversy over the right of a scientist to teach and defend his scientific beliefs, the culmination of which was the infamous trial of April 1633. It is possible, of course, to debate what his primary purpose may have been during this prolonged struggle.[18] Whatever his motives, an important consequence is that the created physical world becomes comprehensible in purely physical terms. In his *Letter to the Grand Duchess Christina* [23] he argued that the Bible contained the revealed word of God and was the ultimate authority on moral matters, whilst the study of the physical universe would reveal truths about God's Creation, provided that men investigated it using their God-given senses and powers of reasoning. The fact that Christianized Aristotelianism was full of inconsistencies (and often incoherent) did not entail that the blueprint which replaced it would have to be theology-free but Galileo was careful to circumscribe God's role. Authority, be it that of Aristotle (as in the *Dialogue*) or that of

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[17] **With the possible exception of Newton's** *Opticks.*

[18] **Perhaps he sought to detach God from natural philosophy in order to rid the discipline of tiresome theological dogma. Perhaps he aimed to remove the dead hand of Authority in any form from theorizing. Perhaps he was principally guided by Augustine's centuries old warning to the effect that Christians must avoid firm statements on inexactly comprehended matters to do with the natural world. In his** *Letter to the Grand Duchess Christina* **he quotes Augustine on the nature of animals, the properties of the earth, and the distance of the stars: he concentrates particularly on Augustine's concern that should such assertions prove false, non-believers would associate Christian doctrine with error and reject fundamentals like the resurrection of the dead or belief in the Kingdom of Heaven.** *'And in St. Augustine we read: "If anyone shall set the authority of Holy Writ against clear and manifest reason, he who does this knows not what he has undertaken; for he opposes to the truth not the meaning of the Bible, which is beyond his comprehension, but rather his own interpretation; not what is in the Bible, but what he has found in himself and imagines to be there"'*, **in Drake, S.,** *Discoveries and Opinions of Galileo*, New York: Doubleday, 1957, 186.

God, via the Holy Fathers and Biblical Scribes, (in the *Letter to the Grand Duchess Christina*) could never serve a methodological function in natural philosophy. God was not, as Descartes and Newton were later to assert, involved in the continuing existence or functioning of natural phenomena. He is even constrained to abide by the rules of His own Creation. Near the beginning of the *Dialogue*, Galileo postulates that God, in the beginning, let the planets fall linearly into their orbits because He created the linear motion of fall as the only natural means of producing (natural) circular motion.[24] Galileo was tactful enough not to directly question God's omnipotence: God could create motion directly but did not, for that would be an unnecessary miracle and the motion created would not be natural.[25] When he came to the argument from the tides on the Fourth Day, Galileo apparently reneged and allowed that a necessary demonstration purporting to show that there was only one possible way to produce the phenomenon concerned would deny God's omnipotence. Simplicio objects as follows:

> **I know that if asked whether God in His infinite power and wisdom could have conferred upon the watery element its observed reciprocating motion using some other means than moving its containing vessels, both of you would reply that He could have and that He would have known how to do this in many ways which are unthinkable to our minds. From this I forthwith conclude that, this being so, it would be excessive boldness for anyone to limit and restrict the Divine power and wisdom to some particular fancy of his own.**

Salviati responds,

> **An admirable and angelic doctrine, and well in accord with another one, also Divine, which, while it grants to us the right to argue about the constitution of the universe (perhaps in order that the working of the human mind shall not be curtailed or made lazy) adds that we cannot discover the work of His hands. Let us, then exercise these activities permitted to us and ordained by God, that we may recognize and thereby so much the more admire His greatness, however much less fit we may find ourselves to penetrate the profound depths of His infinite wisdom.** [26]

However, taken verbatim this simply denies things which Galileo has argued for passionately, such as the realistic approach of the astronomer, as opposed to the position consistently taken by Rome that astronomy dealt with mathematical hypotheses, and the conviction that the human intellect was given by God in order to discern the laws of the created universe. The clause in parentheses comes to the rescue: the implication that the aim of human intellectual endeavour is to guard against metal sloth indicates sarcasm and a position adopted under pressure, to be precise the baleful glare of Pope Urban, whose argument regarding God's omnipotence is put into Simplicio's mouth. Galileo's concept of the relationship between God and the natural world implies that the most obvious way to understand natural phenomena independently of the being or entity who created them is by means of something inherent in the physical universe itself. Given this and the evidence cited earlier regarding the attempt to translate the necessity with which mathematical truths are known into **levels 4** and **3**, the fundamental mathematical character of geometrical motions and atomism, and the unrivalled suitability of the latter for explaining change in terms of motion, and the picture that emerges is of a created physical system in which mathematical properties are the essence of physical reality, and which functions autonomously.

Does this mean that Galileo went on to fulfil all the requirements of a physicalist cosmology? Advocated by aim-oriented empiricism as the best **level 4** option, physicalism refines the notion that nature is fundamentally mathematical into the idea that nature has some definite, simple, unified, mathematical structure. Ideally, this takes the form of some invariant which is postulated to be such that, although unchanging itself, is capable of being characterized by means of some physically interpreted mathematics from which, together with initial conditions, descriptions of that which varies can (in principle) be deduced. In terms of the **U** and **V** definition of the Comprehensibility Thesis quoted in Chapter Two this simply means that the dispositional property/properties **V** of invariant **U** are being captured through some physically interpreted mathematics and conjoined with the appropriate initial conditions. In this way that which is essential is also mathematizable. Two historical examples of how this has been attempted are the corpuscular hypothesis and Newtonian theory. If physicalism is articulated as corpuscular philosophy, the invariant comprises the corpuscles themselves, infinitely rigid and possessing inertial mass: these are the fundamental physical entities which are responsible for change insofar as their own physical properties determine the way in which they interact. However, such a model is an unsatisfactory version of physicalism because it has proved impossible to mathematize. The capacity to modify motion and instigate change, which is associated with the corpuscle, depends upon the same properties which constitute its definition and offer no evidence of law-like regularities.[19] If physicalism is articulated as the more sophisticated Newtonian theory, then the invariant, fundamental, physical entities have the additional property of gravitational force, which determines the way in which the said entities interact, and has the added advantage of being mathematizable. This fulfils all the basic requirements of physicalism, although it is not without problems of its own and can greatly benefit from being further refined.[20]

Once again, Galileo does not seem to belong to this sort of scenario: his most successful attempt at mathematization, that of motion, occurs at the macroscopic level and does not encompass fundamental physical entities with dispositional properties like gravitational force. Now if one wished to press the aim-oriented empiricist viewpoint after the manner of a rational reconstructionist, one could argue that in Galileo's case the invariant is uniformly accelerated motion which, consisting as it does of equal increments of motion in equal times, is mathematizable and also determines the simple, regular changes that comprise the motions of terrestrial bodies. If a law is about change, as Galileo's *times-squared law* is, then its form remains invariant through change. This gives credence to the notion that Galileo was at least thinking about what varied and what did not. It is even possible to indicate the existence of limited occasions upon which it is possible to vary the initial conditions. Drake discusses a manuscript account of an early experiment which, by rolling balls down incline planes of different heights, simultaneously served to (indirectly) verify Galileo's 'law of the persistence of horizontal

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[19] **If the universe is made up of infinitely rigid corpuscles of different sizes and shapes, which have inertial mass, and which interact only by contact then certain problems arise. Attractive forces are difficult to account for and cohesive forces require corpuscles to have the addition of hooks. Infinite rigidity introduces discontinuity with the instantaneous infinitely repulsive forces which are produced when corpuscles collide. Also, such a blueprint makes an invalid appeal to experience in the macroscopic world. A physicalistically correct formulation would be that associated with each corpuscle there is a closed surface of some definite invariant shape and size, which is such that the instant two such surfaces touch, the two corpuscles experience an infinitely repulsive force.**

[20] **Associated with each corpuscle there is a force which varies with distance from some central point in an invariant fashion and which affects acceleration in accordance with the equation $F = ma$. No longer is there the restriction of an infinitely repulsive force located on some closed surface: now the force is allowed to vary, in some fixed way, throughout a volume of space. The internal homogeneity of the corpuscle is replaced by a central point of infinite mass density. Thus two point-particles will experience alternatively attractive and repulsive forces as they approach each other and because the force tends towards the infinitely repulsive as the distance between them tends to zero, the previously problematic point of discontinuity now simply means that no two corpuscles ever occupy the same spatial point. Attractive and cohesive forces between particles can be accounted for by the alternating regions of attractive and repulsive forces. It is even possible to introduce the concept of positive and negative charge. However, this blueprint cannot assess the relative simplicity of theories which specify different ways in which force varies with distance from each point-atom. Continuity and spherical symmetry are the only constraints imposed.**

motion', which was his formulation of the law of inertia, and directly verify the law of parabolic motion.[21] [27]

However, to fully interpret Galileo's work in terms of invariants responsible for change is to go beyond what is to be found in Galileo's own writings and to reconstruct his work too thoroughly for comfort. One can realistically claim no more than that a fundamental, central idea of physicalism is present in his work: Galileo certainly had the generalized **level 4** idea that nature has some definite, simple, unified, mathematical structure in the form of some kind of mathematically precise, physical law governing all natural processes. The most successful formulation he was able to give it was that simple, mathematically formulable laws govern the motion of bodies everywhere. What varies and what does not is, as we have seen, implicit in the form of *the times-squared theorem* but there is no evidence that Galileo had worked out the nature of the relationship between them. The existence of initial conditions, in certain prescribed circumstances, is not sufficient to confirm that he completely understood, as a general principle, the relationship between physically interpreted mathematics and initial conditions in the process of formulating descriptions of that which varies. Our first conclusion was that Galileo produced a system of the world which, although Divinely created, could be interpreted as if it were autonomous and physical. Our second conclusion must be that insofar as he was able to demonstrate, through the *times-squared theorem* and the *law of parabolic motion*, that motion and change are governed by simple mathematical relationships, he was implying a key idea of physicalism and making use of, although without realising their future significance, concepts which became central to more sophisticated versions of physicalism. Finally, insofar as it is implicit in so much of his methodology, in his almost instinctive feel for when flexibility was appropriate and when precision was called for, in his observational and experimental work, and in his theorizing, it will become clear that the underlying unity of Galileo's work is to be found in this commitment to a key idea of physicalism. The unity of Galileo's work resides at the level of metaphysics and if this is ignored it is only too easy to view Galileo's achievement as fragmentary, a scattering of observations, a few neat

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[21] By rolling balls down inclined planes of different heights onto a horizontal table top - and then allowing them to roll with their acquired speed right off the edge - Galileo was indirectly testing the relationship between the speed acquired in the descent and the horizontal distance travelled by the ball after leaving the table.

experiments, some clever theorizing. A clear concept of where the unity lies, as will become clear, makes it possible to draw together the empirical and experimental with the *a priori*, the theoretical and the metaphysical.

The Development of the Level 3 Blueprint

In developing the theme of Galileo's most successful interpretation of the central **level 4** concept of physicalism in more detail, it is possible to argue that he visualized the mathematical, or rather the geometric structure of the universe in terms of the nature of the shapes made by the trajectories of relatively moving bodies. These shapes were mathematically generated <u>curves</u>, traced out in space and time. He must have been encouraged in this belief when he succeeded in realising, at **level 2**, the actual motion of a real body by the simultaneous action of the universal law governing fall and the specific initial conditions. It proved to have the geometric structure of a <u>parabola</u>, emerging mathematically as a result of combining uniform horizontal motion and uniformly accelerating vertical motion. This can be seen as his most sophisticated formulation of the basic physicalist idea that simple, mathematically formulated laws govern the motion of bodies everywhere, not least because it was the one where the experimental set-up produces, but does not impinge on, the natural form of the motion.[22] Later on we will be discussing the **level 3** implications of the theorems and proofs concerning naturally accelerated motion on the inclined plane. However, for now we must concentrate on the concept of curves traced out in space and time and starting point was, of course, Euclidean geometry.[23] At the beginning of the *Dialogue* Galileo states that space has three dimensions, that there are at most three mutually perpendicular lines through any

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[22] For one thing, the experimental set-up obtrudes rather. Certainly one specific initial condition can be varied to demonstrate that each specific height gives a specific horizontal motion and a resulting proportional relationship with the horizontal distance traversed after leaving the table. However, this variability of the initial velocity could not free Galileo from the limitations of a uniform horizontal motion that could only produce a semi-parabola, commencing at the apex, as it left the edge of the table.

[23] In the sixteenth and seventeenth centuries mathematicians did not hold identical conceptions of geometry, for it was a period of development and change in the discipline. Kepler's notion, in so far as it was applicable to physical reality, required the construction of figures by ruler and compass operations and ignored recent and relevant developments in algebra, of which Vieta was the prime originator. These innovations were adopted by one of the founders of analytical geometry, Descartes, but he still persisted in clinging to ruler and compass operations.

given point. *'And thus by three perpendiculars you will have determined the three dimensions.'* [28]

He was also heir to an old problem, that of how to represent mathematically motion involving changes in velocity. Ironically it existed because of that failure within the tradition which produced Euclidean theory, although struggling towards explanations couched in terms of something completely general underlying all change and diversity, to produce more precise, specific, limiting hypotheses which would be testable. Arithmetic, which depends upon notions of number and was the correct candidate for developing into a mathematics that could accommodate change and produce a dynamical theory, was challenged by the then apparently insuperable problem caused by the discovery of irrational numbers.[24] The line of development which ultimately led to algebra, functions, differentiation, and integration was severely curtailed.[29] The Greek technique of kinematic geometry, which actually generated curves by the composition of different motions, could still only be utilized in the geometrical study of unchanging physical space.[25] The ellipse, that most important of curves for characterizing moving objects and the variations in their speed had been defined and studied by Menaechmus, Euclid and Appolonius but only as a section of a circular cone, cut so that the vertex angle was acute.[26] Plato too was a contributory factor: for him change equalled degeneration and there could be no true knowledge of it. Real knowledge existed in the unchanging world of Ideal Forms: it was about unchanging things, so the obvious basis for it was Euclidean geometry. Interest in motion did not cease: Aristotle considered it at length but gave no mathematical representation of it. Even *'the divine, the superhuman Archimedes'*, who in a manner of speaking managed to invent the integral calculus (insofar as he was able to calculate the areas of curvilinear figures using Eudoxus' method of exhaustion) only managed to characterize motion as a change from the state of equilibrium. The

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[24] In this situation, Zeno's paradoxes, in which motion becomes impossible in the face of the infinite subdivisibility of space and time, or in which the subdivisibility of space and time terminates in indivisibles, making motion impossible, proved to be an important contributory factor to the Greek inability to cope with motion involving changing velocity.

[25] It can, of course, be argued that the whole Euclidean programme, which synthesized a lot of earlier work, was really intended as a cosmology because the building blocks of the world were geometrical in character and the five regular plane solids encompassed all the necessary shapes. However, this was not the form in which it became finalized.

[26] A parabola could be derived when the vertex angle was right and a hyperbola when the vertex angle was obtuse.

succeeding centuries therefore inherited the general problem of how to mathematically represent motion involving changes in velocity. Galileo, inheritor of these two traditions, developed the kinematic techniques of the one, three dimensional Euclidean geometry and thereby augmented it so that it could be utilized to offer a limited solution to the other, motion involving changes in velocity. He couldn't tackle the general problem of how to reduce motion involving changes in velocity to mathematics because that would have required him to be fully conversant with infinitesimals, irrationals, functions, differentiation, and integration. These make it possible to gain a purchase on the relationships between time, distance, velocity, and acceleration by representing them as curves of various kinds, thus producing a precise and accurate mathematical description of motion. Galileo did not have these skills but when he formulated at **level 2** the mathematical law that governs one particular type of motion involving changing velocity, that of freely-falling bodies, he made possible the graphic representation of one fixed curve associated with one particular motion. This was the only occasion in which he was able to represent an accelerated motion as a path in physical space. The *law of free fall*, together with the postulate that in uniform motion equal spaces are traversed in equal times, allowed him, in a restricted sense, to produce a mathematical description of a projectile motion by means of its characteristic parabolic curve, a curve generated on the ancient kinematic principle of the composition of different motions and containing information about acceleration and velocity.

For three centuries it had been assumed that the speed of a falling body must be proportional either to the distance it had fallen from rest, or to the time elapsed - and even these distinctions were not always fully understood. In 1604 Galileo clearly believed that distance was the important factor.[27] By 1611 he had decided that it was time elapsed that was significant in uniformly accelerated motion, a principle which had been accepted by Leonardo da Vinci a century earlier.[28] In certain sorts of motion, such as harmonic

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[27] **In 1604 Galileo wrote to Paolo Sarpi a letter containing the first formulation of the first law of classical physics,** *'that spaces traversed in natural motion are in the squared proportion of the times and consequently the spaces traversed in equal times are as the odd numbers* ab unitate.*'* **However, he went on to erroneously assert that the** *'utterly indubitable principle'* **on which he based this claim was** *'that the natural moving body increases its speed in the proportion that it is distant from the beginning of its motion'* **See Koyré, A.,** *Galileo Studies*, Sussex: The Harvester Press, 1978, 67.

[28] **In harmonic motion, in which a string is plucked, the acceleration is proportional to the distance the string was moved before being released. Galileo saw the swinging of a pendulum as an example of harmonic motion and showed how the musical harmonies between the vibrating strings could be represented by the oscillation of the pendulums whose chords were as 16, 9, and 4 units. However, he also produced a law-like statement that the time of the swing of any pendulum is independent of its amplitude, being proportional to**

motion, the velocity of the vibrating string is proportional to distance but in the case of free-fall distance becomes a mere consequence of what is essentially a temporal reality. Accelerated motion, which occurs in relation to time, could only be understood as a function of time.[29] Actually there is little to be gleaned from studying Galileo's transcripts for direct references to the geometrizing of time, or the effects of incorporating time into three-dimensional Euclidean geometry. Certainly, according to the final page of the Third Day of the *Two New Sciences*, he believed that he had enlarged Euclidean geometry and was confidant that it would prove to have new axioms and postulates that would be as unassailable as those of the established system.[30] However, what was it that he was really developing? There are important and complicated considerations concerning the conceptual framework that was available to an original thinker who has also contributed to our present understanding of things. The following discussion demonstrates the assistance that can be forthcoming to the historian if later ideas are clarified in order to establish how we would instinctively formulate the problem. Having established this it is then possible to judge the extent to which such means were open to Galileo. Blueprint formulation is a sustained problem for physics and if Galileo's central problem was how mathematical laws could describe observed phenomena, in particular motions, then it is clear that the problem of blueprint articulation was closely allied to this. It is necessary therefore to have some means of bringing into focus what he did and did not know and understand.

The concept of curves traced out in space and time might suggest that Galileo, if not exactly developing four-dimensional Euclidean geometry, was at least toying with that fragment of it which deals with space-time world-lines as traced out by particles in the real world.

> **Imagine any particle projected along a horizontal plane without friction; then we know, from what has been more fully explained in the preceding pages, that this particle will move along this same plane with a motion which is uniform and elevated, then the moving particle, which we imagine to be a heavy one, will on passing over the edge of the plane acquire, in addition to its previous uniform and perpetual motion, a downward propensity**

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the square root of its length. This linked distance (pendulum length) to time (intervals of swing) and invoked the notion of velocity as being proportional to the time elapsed.

[29] **In the *Two New Sciences* Galileo first published his assertion that in uniformly accelerated motion the instantaneous velocity v is proportional to the time elapsed t; the distance traversed s is proportional to t^2 and vt=2s.**

due to its own weight; so that the resulting motion which I call projection (*projectio*), is compounded of one which is uniform and horizontal and of another which is vertical and naturally accelerated. We now proceed to demonstrate some of its properties. the first of which is as follows: A projectile which is carried by a uniform horizontal motion compounded with a naturally accelerated vertical motion describes a path which is a semi-parabola. [31]

In three-dimensional Euclidean geometry the position of a point in space can be captured by means of three co-ordinates. An event, however, which is what has to be captured when motion is under consideration, is something which occurs at a particular time and at a particular point in space and so is specified by three spatial co-ordinates and a measure of time. A particle occupies one point of space at each instant of time and so its progress can be represented by a line in space-time. It can also be formulated in terms of functional relationships, linked by means of differentiation and integration. Galileo was crucially concerned with velocity and acceleration (which is the change of rate of velocity) and a four-dimensional space-time view, which represents actual motion as it actually occurs, makes it possible to represent both. The three-dimensional view, on the other hand, can give no such purchase on how a motion is actually executed. The best it can offer is to show that insofar as there was a change of position, it happened along such and such a path, so that *x*, at some point, moved from *A* to *B*. By confining his considerations to objects which were in a state of uniform motion in at least one direction, Galileo caused the *x*-axis of space to also double up for time, enabling the observer to read off equal times over equal horizontal distances, whilst the actual distance of the falling body varies as the square of the time elapsed.[30] The mere introduction of the fourth dimension of time, even though it is unable to function separately from the spatial dimension measuring distance, is sufficient to transform the motion into a changeless, fixed curve, producing one example of a four-dimensional geometry of motion [32] The parabola stretched out along a time-scale becomes a unique, four-dimensional parabola. However this augmentation of the *x*-axis suggests that what he had really produced was an expansion of the three-dimensional view that does not amount to our four-dimensional view of trajectories in space-time because it is

[30] **It is only with variable horizontal velocity, or with infinitely variable velocity in the vertical direction and constant velocity in the horizontal direction, that infinitely many different curves in four-dimensional space-time become possible.**

only possible to gain some understanding of how the motion was executed after its completion: the changeless curve remains an entity that has to be considered in its entirety as existing in 3- D space at a given instant. Having, therefore, the function of a geometrical object stretched out in time it may serve the purpose of a four-dimensional view but it cannot, in itself, constitute such a view. Galileo was correct in believing that what dynamic physics, *'the property of acceleration and the manner in which it varies with slope'* amounted to was an extension of Euclidean geometry to produce *'a new method fraught with numerous and wonderful results which in future years will command the attention of other minds'*.[33] However, the fact that three dimensions had to be extended to four is precisely the point that he remained unclear about: he failed to think of time in an entirely geometrical way and consequently cannot be said to have had our modern conception of trajectories in space-time. Thus the aim-oriented empiricist perspective, which suggests that the **level 3** blueprint concerned mathematically generated curves traced out in space and time, does not automatically reconstruct history. It comprehends the difficulties involved in blueprint formulation and, by allowing later discoveries to be brought to bear, it delineates a relationship which is conceptually quite close without its being possible to confuse Galileo's trajectory in space and time with our modern space-time trajectory in four-dimensional space. It also demonstrates that in order for the blueprint to be developed in fruitful ways it is necessary to either utilize, or create, new mathematics.

The Blueprint Progression

Now it is time to consider the extent to which Galileo's blueprint developments at **levels 4** and **3** can be interpreted as belonging to a tradition of blueprint progression. The Presocratics, although they created the idea, failed to make much headway with the profoundly problematic idea of developing **P** as the best conjecture as to how the universe is comprehensible because they could never capture their ideas in precise, testable forms.[31] However, with atomism they hit upon a blueprint that at least offered

[31] In aim-oriented empiricist terms they never created a research programme which took P as the basic blueprint and simultaneously accommodated the best available rival versions of P (so far proposed) as their rival blueprints.

solutions to the conceptual and metaphysical problems of change[32] and was to prove very fruitful from the seventeenth century onwards. Yet not in Galileo's hands - his musings on atomic theory are largely confined to *The Assayer* and the possibility of empirical support for the idea of ultimate, indivisible particles in the First Day of the *Two New Sciences.* [34] So if he did not work in what might be called the obvious tradition, to what can we ascribe his grasp of the central concept of physicalism and the manner in which he was able to develop and refine it, and even imply that it was applicable at the level of fundamental particles? There was, of course, another tradition, stretching right back to the Presocratics, to which Galileo was the undoubted heir. Although this too did not create a research programme incorporating both **P** and the best available rival versions of **P**, it was nevertheless guided by a basic blueprint idea, it was amenable to mathematization, and it exhibited remarkable predictive success. It was that strikingly successful empirical research programme known as the theory of astronomical motion from the Pythagoreans to Ptolemy and from Ptolemy to Kepler. Ptolemy's final theory, *c.* AD 140, was possibly the first full-grown, empirically fruitful physical theory that dealt with motion.

In the earlier period of development the theory of astronomical motion was grounded in ideas like those of Anaximander, who held that the earth is suspended in space without any support because, being equally distant from all other things, it is in a state of equilibrium. Then from generally accepted notions about revolving spheres in the heavens, Plato (427-347 BC) formulated, in a form which captures the fundamental physicalist notion that nature has some definite, simple, unified, mathematical structure, the basic blueprint idea that the celestial bodies move uniformly in circles about the earth. This was to be the guiding principle for centuries. His younger contemporary, Eudoxus of Cnidos (408?-335?), formulated a physical rationale for the motion of the heavenly bodies: they move in this way because they are attached to crystalline spheres. He increased the number of spheres to four with each one rotating on a different axis and each having its own period of rotation. However, the physical rationale became increasingly hard to sustain as the theory, whilst adhering to the basic idea of uniform motion in a circle, strove to do justice to heavenly motions whose complexities were

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[31] The metaphysical problem requires that the nature of the persisting underlying stuff be understood. The conceptual problem requires an understanding of how something fundamental can retain its identity through change.

revealed by ever more careful observations. Appolonius of Perga (262?-190? BC) produced an epicyclic system for the superior planets and Hipparchus of Nicaea (*c.* 180-*c.* 125 BC) apparently produced the first trigonometric table for use in astronomy. With the advent of Ptolemy in the second century AD, various combinations of circular motion, particularly the *eccentric*, the *equant*, and the *epicycle*, proved the extent to which the theory of astronomical motion had become a collection of mathematical devices aimed at saving the phenomena. In aim-oriented empiricist terms the research programme had come to this pass through not being bold enough, through not considering sufficiently radical alternatives to the basic Platonic idea of the circularity of celestial motions, which would have enabled it to perhaps resolve the problems that surfaced.[33] Indeed, radical alternatives had been present throughout, as may be seen from the work of Philolaus, Heraclides, and Aristarchus. Such radical ideas, however, were not considered worthy of development, not even as geometrical alternatives to the geocentric, geostatic view.

With respect to the second phase of the development of the theory of astronomical motion Copernicus, concerned by its increasing internal inconsistencies and incapacity to measure the astronomical year, returned to Plato's original blueprint, that heavenly bodies move uniformly in circles centred on the earth, with its concomitant simple, unifying mathematical structure. His heliocentric alternative can be interpreted as a generalisation of Plato's blueprint into the notion that heavenly bodies, including the earth, are either stationary or move around some one heavenly body (possibly not the earth) whilst also rotating about their own axes. Heliocentrism is a possible, special case of this scenario. Copernicus hoped that Plato's idea of uniform circular motion would be more simply and perfectly realised. He didn't altogether succeed in this aim, although managing with fewer Ptolemaic devices, but he did produce something elegant and austere, with a new unity and intellectual integrity. It thoroughly focused attention on the problem of how to describe the motions of the celestial bodies and, more importantly,

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[33] Philolaus, a Pythagorean of the fifth century BC assumed that the spheres, including one carrying the earth and one carrying the Sun, revolved about an enormous Central Fire. Obviously this was not a heliocentric system but it did involve a moving earth. Heraclides (fourth century BC) postulated that while the Sun circled the earth, two of the planets, Venus and Mercury, actually revolved about the Sun. He also suggested that the earth rotated daily on its own axis. According to his contemporary Archimedes, in a work entitled the *Sand-reckoner,* Aristarchus (third century BC) put forward the theory that the fixed stars and the Sun are stationary whilst the earth revolves around the Sun circularly, the Sun lying at the centre of the orbit.

the motions of the earth and the motions that occur upon the surface of the earth. Kepler went one better and produced a geometrical generalisation of Plato's original blueprint idea. The hypothesis is that the planets move in elliptical motions, with the sun at one focus of the ellipse. The requirement for uniform motion is apositely generalized in terms of the planet sweeping out equal areas in equal times. Galileo ignored Kepler's contribution and concentrated on what might be called a proper simplified approximation of Copernican circularity. He adopted the simple, unifying, although slightly false concept that heavenly bodies, including the earth, move around the sun in uniform circular motion. Such motion occurs in a basic, regular manner: it is the most elementary dynamic system in existence, being capturable in terms of equal spaces passed in equal times, and was therefore commensurate with the **level 4 P** concept of simple, mathematically formulable laws governing all natural processes. It is further implicit in his work on terrestrial motion that he then generalized this simplified approximation of natural motion to include the **level 3** assertion that bodies moving on, or near, the surface of the earth execute motions which change in some simple and regular fashion, along conic sections.[34]

Thus Galileo's big innovation was part of a genuine blueprint progression for two reasons. Firstly, it concentrated on something which is foundational to blueprint formation and development, the simple, unifying concept, which in this case is that of uniform circular motion. This most useful and proper concept is also an appropriate, simplified approximation of Kepler's development, insofar as it is a case of highest symmetry in which the equal areas law of the ellipse reduces to uniform motion in a circle.[35] Secondly, it accepted that the act of raising the earth into the heavens meant that the ancient dichotomy between circular celestial and linear terrestrial motions could no longer exist. The progression here is particularly marked. Aristotle's motions had belonged to different realms and could never interact. Copernicus elevated the earth to the status of a heavenly body and thereby obliged the motions to become associated in some way or other. Galileo took the simple, unifying concept of natural motion and

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[34] **During the Fourth Day of the *Two New Sciences*, Salviati ensures that his two companions are acquainted with those properties of the parabola necessary for an understanding of the proof of** Theorem 1 Proposition 1 **by demonstrating that a parabola is a section of a circular cone, according as the vertex angle is right. See Galileo,** *Two New Sciences,* trans. Crew and de Salvio, New York: Dover, 1954, 246.

[35] **Kepler's blueprint idea reduces to Galileo's as the eccentricity of the elliptical orbit reduces to zero. Kepler is a proper, simplified approximation of Newton in that Newton's idea reduces to Kepler's as the masses of the planets tend to zero.**

argued that the motion of the planets orbiting the sun corresponds to the uniform motion of a ball rolling around the earth on a smooth, frictionless surface. Thus he had combined two false, but simple and approximately correct notions, that of the heliocentric view and that of the law of circular inertia, produced a unified motion for the earth and the heavens, and taken the first step towards a unified dynamics.[36] This particular concept, being mathematically capturable in the form of *'equal spaces passed in equal times'* functioned to unify Copernican heliocentrism with the physico-mathematical language of mechanics, which in aim-oriented empiricist terms marks the point at which Galileo developed the generalized **level 4** idea[37] into the more specific **level 3** idea that simple, mathematically formulable laws govern the motion of bodies everywhere. However, the progression does not end there. The linear dimension was not lost, it was retained in vertical free-fall and the essentially Aristotelian concept of these distinct, primordial, and mutually exclusive motions was utilized in terms of further **level 3** assertions. The individual identities remained but the simple motions themselves now combined to form compound motions without mutual interference. Galileo's clear articulation, the earliest ever recorded, of the superposition of motions that themselves have an independent physical existence, and the subsequent demonstration that they could combine to represent a kind of accelerated motion as a parabolic path in physical space, is the direct result of the progressive development of the blueprint of astronomical motions. Overall two things become clear. The first is that it is possible to see that there is, if not always straightforwardly progressive development, evidence that each new blueprint is either a slight generalisation of what went before, or a reinterpretation which counts as a proper, simplified approximation of more complicated, versions. The discontinuities with which the history of science has become so familiar, over the last thirty years, do not occur as high as **level 3**. The second is that these generalizations and reinterpretations involve various attempts to articulate, clarify, and even develop lower levels from, the basic **level 4** concept that nature has some definite, simple, unified, mathematical structure: in this blueprint progression it states that the celestial bodies move uniformly in circles about some one other celestial body, a motion that can be

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[36] In so doing he had arrived at yet another approximate, simple, special case - this time one that was yet to come. For appropriate, special cases gravitation, plus the Newtonian form of the law of inertia, gives the same orbits as Galileo's version of the law of inertia.

[37] The idea that nature has some definite, simple, unified, mathematical structure in the form of some kind of mathematically precise, physical law governing all natural processes.

mathematically captured in terms of equal spaces moving in equal times and generalized to include motions on or near to the earth.

Blueprint Clashes

There is a section during the First Day of the *Dialogue* which not only supports this contention that Galileo innovated within a blueprint tradition but also indicates that he had some consciousness of that fact. Firstly, he can be interpreted as asserting that Aristotle's theory of simple motions and his own theory of universal, uniform circular motion, are concepts which are foundational to the formulation of their respective blueprints. Aristotle's theory can lay claim to the title because it is the conceptual foundation of his earth-heaven distinction, *'This is the cornerstone, basis, and foundation of the entire structure of the Aristotelian universe'.* [35] Galileo's own theory qualifies by virtue of being the metaphysical foundation of the unified cosmos, where all moveable bodies must move with uniform circular motion.[36] It is satisfying to see this, given the analysis at the end of the previous section concerning the formulation of **level 4 P** within the blueprint tradition. Secondly, it is clear that Galileo's main aim in defining these two totally different versions of how the cosmos can be comprehended is to introduce the reader to the idea that they are in contention. Galileo quickly attacked Aristotle for, *'pulling cards out of his sleeve and trying to accommodate the architecture to the building.'* [37] He himself was concerned to construct a blueprint *'with sounder architectural precepts'*. He contrasts his theory of universal circular motion favourably with Aristotle's theory of simple motions because it had been formulated independently of empirical considerations and fulfils the requirement that,

> **basic principles and fundamentals must be secure, firm and well established, so that one may build confidently upon them** [and thus avoid] **the many and grave difficulties** [inherent in the Aristotelian universe]. [38]

The battle lines between,

> **the partisans of the Aristotelian and Ptolemaic position on the one hand, and by the followers of the Copernican system on the other.**[39]

are drawn up early in the *Dialogue*. Thirdly, because in practice Galileo appeared to be well-versed in the art of what might be called the 'blueprint clash', this contest could, at its most rigorous, be to the death. Such an assertion, unlike the ones concerning the idea of blueprints in contention, must be drawn from Galileo's methodology rather than from his stated views. Galileo stated that the ideal case would consist of the formulation of an hypothesis postulating certain observable consequences, the detection of those consequences, and the failure to find any other explanation for the observed phenomena.[40] However, in practice he frequently exceeded his third criterion insofar as his arguments assembled observations which were frequently accurate and sometimes quantified, and also compatible with his own hypothesis, but which simultaneously clashed destructively with the rival hypotheses of Scholastic Aristotelianism. This amounts to Galileo's fulfilling the aim-oriented empiricist requirement of creating a research programme taking **level 4 P** as the basic blueprint, and occasionally involving the more specific **level 3** blueprint as well. Testing the basic blueprint against the best available rival versions so far articulated involves, at the methodological level, a comparison of the empirical progress of two rival research programmes. Ideally, the same evidence which renders the one programme the most empirically progressive will simultaneously deny the assertions of the other.

For over twenty years prior to the publication of the *Dialogue*, Galileo had not merely been thinking about rival blueprints, he had been practising the art of the blueprint clash with great success. The early clashes depended very much on observations and mostly served to undermine the earth/heaven dichotomy and all that that meant for terrestrial motion.[38] In *Sidereus Nuncius* (1610) he witnessed imperfections on the Moon that disproved the celestial perfection assumed by Aristotelian cosmology. He observed Jupiter's orbiting satellites, a straightforward falsification of the assumption of Ptolemaic astronomy that all celestial bodies rotated about the earth and, simultaneously, a plausible real-life model of his simplified Copernican system. The following year the discovery of the phases of Venus amounted to a falsification of the established Ptolemaic astronomy: the only explanation of Venus's phase-like behaviour, was that it was orbiting the sun. Of course it did not establish the veracity of Sun-centred astronomy because the appearances could also be explained by Tycho's geostatic theory. However, these developments did, as it were, draw up the battle lines. In 1613, the *Letters To*

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[38] The certainties of ancient astronomy had already been badly shaken by the appearance of the supernova in Cassiopeia in 1572 and the comet of 1577. Tycho used a new sextant to locate the supernova in relation to nine of its neighbours and placed it well beyond the sphere of the planets, in Aristotle's 'incorruptible' heavens. He published his results in *De Nova Stella* (1573). The comet too displayed no parallax and had to belong to the regions above the moon. Moreover it discredited the notion of crystalline spheres, having travelled across that region of space where they were supposed to be.

Mark Welser produced a particularly destructive clash which further discredited the Aristotelian view, in this case promulgated by the Jesuit Scheiner, that heavenly bodies are perfect and unchanging. Observing that the spots travelled across the disc of the sun at the specific measure to correlate with the uniform rotational motion of the sun's surface, Galileo concluded that they are in contact with the surface of the sun and thereby partake of that rotational motion. The evidence gathered and the calculations made simply contradicted all rival Scholastic efforts aimed at preserving their *a priori* theory concerning the incorruptibility of all heavenly bodies. Galileo's observations, measurements and calculations ensured that none of Scheiner's attempts to explain away the observational data could succeed. The spots could not be located in the air, they could not be a short distance above the solar globe, and they could not be stars or other permanent bodies. In a final flourish, Galileo declared,

> **spots observed simultaneously from widely separated positions on earth are nevertheless arranged in the same order and in the same places on the sun.** [41]

and therefore they could not be on a little sphere, equal in diameter to the sun, lying between us and it, our eyes being both in line with its centre and the centre of the sun. All of this evidence renders Galileo's programme empirically progressive and denies the assertions of the Scholastic Aristotelian programme. It amounts to the testing, at methodological level, of one blueprint against another.

Later, in the *Dialogue*, there is an abundance of a slightly different type of blueprint clash, one in which the blueprint which triumphs is the one whose concepts best survive careful and critical examination, or which proves to be the most flexible and accommodating within its own parameters. A particularly concentrated example is to be found in the arguments for the diurnal and annual rotations of the earth on the Third Day, where it is demonstrated that celestial phenomena are much better accounted for in a heliocentric system. The clash manifests itself in such discussions as the one concerning the ball falling from the orbit of the moon to the earth,[42] during which Simplicio's objections are shown to originate in his Scholastic adherence to the motion of the *primum mobile*[43] and the idea that action and motion are dependent on the essential form of a thing.[44] During the Second Day, Simplicio's Aristotelian beliefs force him to conclude that the Copernican system requires that the motion of the earth depends upon three simultaneous, diverse motions. He declares this to be a nonsensical idea in view of the earth's being a simple body for, as such, it could not *'contain within itself three principles of natural motion besides the part moved'.* [45] Sagredo complains,

> **He wants to condemn Copernicus if I cannot on the spot resolve all doubts ... as if from my ignorance it necessarily followed that the doctrine were false.** [46]

Fortunately - and to no-one's surprise except Simplicio's - the doubts are resolvable in half a dozen pages. In essence Simplicio has not grasped the principle of the blueprint clash and still desires that all difficulties be solved in a manner which will confirm his own intellectual constructs. It is only within the Aristotelian conceptual framework that a 'simple body' requires a single simple motion in order for it to make sense. However, careful examination of the comprehensibility of the concepts of motion within the context of their Aristotelian and Copernican blueprints demonstrates the falsity of the Peripatetic notion that a single moving principle is the cause of a single motion - *'a single principle can cause more than one motion of the earth'.* [47] Salviati's sustained, critical investigations of all concepts are very much in the tradition of aim-oriented empiricism, for they amply demonstrate that when two blueprints collide it is the one whose concepts stand up best to criticism - and which is itself the most flexible and accommodating with its own parameters - that will triumph, because of the two it will make the most empirical progress.

Finally there is a strategy related to the **blueprint clash**, which might be termed the **blueprint neutralizer**. This is also noticeable in the destructive encounter with Aristotelianism, most particularly in that brilliant series of thought experiments in the *Dialogue* demonstrating that moving with uniform motion is experimentally equivalent to being at rest. All objects moving near the earth's surface will be unaffected in their motions, whether the earth itself moves or not. With regard to the inertial frames like carriages, ships, and horses, their motion, or lack of it, can observed from outside, or by looking to the outside, and the very existence of primordial or natural motions and the reason why the one common to the frame should be unobservable can be thereby empirically confirmed. However, if the earth is the inertial frame then the overall motion of it, and all its parts, remains quite imperceptible when viewed from its surface. One only needs to remove the cover and look out of the porthole to see if the ship is moving but in the case of the earth the porthole of stellar parallax was to remain covered for a very long time indeed. We are intended to extrapolate from the example of the ship's cabin and assert that we share a common circular motion about the centre of the earth, so that other motions like bird flight, or vertical free-fall, or the journeys of ships and carriages are superimposed on to it. What we cannot do is leave the terrestrial 'ship' and experience this directly.[39] In fact with stellar parallax, Galileo would have had the blueprint clash he so favoured: evidence of an apparent motion of the stars relative to one another, from a series of observations spaced over a year, would have confirmed his hypothesis of a moving earth and negated all rival geostatic hypotheses. One feels that the thought experiments, although they implied Galilean invariance, cited as being *'among the most profound scientific insights ever achieved'*, began life as a compromise on Galileo's part. [48]

In interactions between rival blueprints there are, therefore, various strengths of encounter. The most telling is when evidence supporting one position simply negates the theories presented by the alternative position. That is the strongest form of clash and is also incontrovertible in those telescopic observations which disproved and/or falsified Aristotelian and Ptolemaic astronomy. Less bruising confrontations, as demonstrated above, are won by flexibility and by responding better to criticism than any rival. In both cases a comparison of the empirical progress of two rival research programmes implies testing one blueprint against another. However, in the case of **neutralization**, experimental evidence interpreted in terms of both blueprints is shown to have equal status and so cannot be used to decide between them. Galileo referred to this as *'the nullity of the experiments brought forth'*. [49]

A Brief Résumé

So far some substantial claims have been made with respect to how Galileo's work looks when viewed from an aim-oriented empiricist perspective. Firstly, it was discovered that he seriously violates **level 5** requirements: in asserting that the universe has a mathematical structure he demanded certain knowledge and invoked the 'God thesis' to try to suggest that this structure was guaranteed. This turned the mathematization of nature into a prerequisite for obtaining objectively certain knowledge, an epistemological error which he was not in a position to recognise.

[39] When the stone falls from the tower observers on earth will never see that its true path is a curve, the result of the superposition of a section of uniform circular motion (in this case small enough to be regarded as uniform horizontal motion) and uniformly accelerating free fall.

Secondly, consideration was given to the role of the 'God thesis' as used by Galileo and it was decided that in practice it corresponded well enough with the requirements of **Level 4 P** to have fulfilled that sort of role in the growth of scientific knowledge. The conceptual and metaphysical problems of change have to be explicated and it is necessary that the basic idea can be formulated as a mathematically precise, testable, dynamic theory, as if the universe were an autonomous, physical system. These requirements are to be found in Galileo's work. Then it was decided that Galileo's definition of **level 4 P** was 'nature has some simple, unified mathematical structure governing all natural processes, which are everywhere governed by simple, mathematically formulable laws' and it was firmly asserted that the unity of his work resides at this level. In the case of **level 3** uniform circular motion of heavenly bodies this translated into 'equal spaces passed in equal times', a mathematical purchase on a regularly changing entity.[40] Thirdly, it was argued that of motions executed on or near to the surface of the earth the combination of a segment of uniform circular motion and accelerated free-fall produced a fixed curve which mathematically captured a constantly changing entity. This was his best **level 2** version of the fundamental tenet of physicalism just stated. Fourthly, the subject of blueprint progression was examined and Galileo was shown to be a player in one such tradition, that of the ancient theory of astronomical motion.[41] His contribution was to generalize the basic blueprint idea that the celestial bodies move uniformly in circles about the sun in order to encompass terrestrial motion. He thereby also partook of the tradition of clarifying the basic **level 4** concept. To this could be added the fact that in implying the key idea of physicalism in its modern form he also made use of, although without realising their significance, concepts which are central to more sophisticated versions of physicalism as manifested in the corpuscular philosophy that developed later on in the seventeenth century. Fifthly, blueprint clashes, very significant in the aim-oriented empiricist schema, were discussed. Evidence was presented to suggest that Galileo not only employed this strategy to great effect a lot of the time but also had considerable understanding of how it operates. Now it is time to examine Galileo's work and see to what extent it implies **level 4** and (to a lesser extent) **level 3**, as

[40] Although the discovery that the sunspots were moving at the precise rate associated with the uniform rotational motion of a sphere necessitated that they were tracing out a curved path in space and time, it was a state of affairs dictated by the shape of the sun itself.
[41] Another way of looking at it is to say that, together with Kepler, Galileo transformed the ancient tradition in astronomy of 'saving the phenomena' into the earliest successes in physics.

explicated here. We will begin with the best examples of his work but later on we will consider the extent to which the less obviously mathematized natural phenomena fit in. The overwhelming conclusion will be the extent to which Galileo's work is unifed at **level 4.** Finally, we will test the strength of aim-oriented empiricism as a historiography by comparing it with some recent methods of interpreting Galileo and demonstrating that it can illuminate his work in significant ways.

A Survey of Levels 1 and 2

Galileo created and actively pursued a research programme aimed at discovering the precise mathematical relationships governing phenomena. In the event, he succeeded with just a few, very simple phenomena but the methodological strategies that he employed in his most successful endeavours imply what may be called his version of physicalism at all times. This is because they operate within the framework adopted to confront the problems for terrestrial motion created by the Copernican hypothesis. We know that this involved the combination of two false, but simple and approximately correct notions, that of the heliocentric view and that of the law of inertia. They shared the concept of uniform circular motion, which functioned as the **level 3** metaphysical foundation of the unified cosmos and complied with **level 4** in that it was mathematizable. Galileo seems to have appreciated that it was to his advantage to adopt the false but simple hypothesis, rather than the empirically more accurate but complicated and inconsistent Copernican schema, not least because the proper, simple, and exact geometric relationships inherent in motions only manifested themselves approximately in the physical world. With respect to acquiring knowledge about the physical dynamics of the universe Galileo took a determinedly progressive stance: it was better to grasp the dynamic patterns relevant to simple systems first.

Finally, before we come to an examination of Galileo's actual work, it is necessary to clarify the sort of things that exist at **level 1** and the sort of things that exist at **level 2**, when judged in terms of the higher-level implications present in the methodology employed. Typical **level 1** things include the following. The altering of the relationship between the observer and nature, for example by magnification, or by interfering with existing physical conditions to make manifest the underlying mathematical regularities in phenomena such as free-fall. The implication of the former is that Galileo initially hoped to substantiate the simplified Copernican view and certainly many of his significant, **level 1** empirical discoveries belong in the realm of astronomy. The purpose of the latter was surely to give an empirical demonstration of the basic **level 4** requirement that Nature has some simple, unified mathematical structure, by producing a clear example of a simple, mathematically formulated law describing the motion of freely falling bodies everywhere. The question of initial conditions and the possibility of varying them also resides at this level and also implies **level 4 P**, although Galileo did not work out, as a general principle, the relationship between the invariant and that which varies.

The sorts of things which belong to **level 2** include testable laws and theories, often mathematically rather than empirically derived. With regard to the inclined plane experiment, the real work at **level 2** is the mathematical derivation of the *times-squared law* from its consequence, the empirically established *odd numbers law*, and then from this single principle the deduction of mathematical proofs for all the subsequent theorems on Day Three of the *Two New Sciences*. In projectile motion can be counted the geometric derivation of the characteristic parabola, the calculations from the 'sublimity' or height of fall to produce the mathematical relationship between the motion of the ball crossing the table and the distance it traverses between leaving the table and hitting the ground, and even the calculations that proved to artillerymen what they already knew from experience - that the ideal angle of inclination for firing is 45°.[50] Both the inclined plane and the projectile calculations are in the spirit of the requisite **level 3** concepts about motions being governed by simple mathematical laws. Moreover, projectile motion is also the classic example of the further **level 3** blueprint idea in which curves represent the trajectories of objects: the assumption of uniform horizontal motion enables the three-dimensional parabola to specify a single, unique four-dimensional parabola. Indeed, if Galileo's work is taken as a whole and questions of order and priority are ignored, it could be argued that accelerated free-fall itself is simply a special case of projectile motion, with the initial velocity set at zero, and therefore exemplifies the **level 3** blueprint as well In fact if the defining criterion of **level 3** is taken to be the specification in mathematical terms of the paths, representing distance travelled and time taken, that objects pursue, then inertial circular motion, parabolic motion, accelerated free-fall, and pendulum motion are all included. In these terms 'sunspots', which is concerned with quantifying the shortening distances travelled by the spots in a given time as they approach the edge of the sun, and to that end utilizes the *versed sines of equal arcs*, also belongs. However, it remains the case that all but uniform circular motion involve a constantly changing entity and of the remaining three, only projectile motion involves a superposition of motions combining an initial velocity and unconstrained free-fall. It is perhaps more accurate to say that only projectile motion is the unequivocal derivation of the **level 3** blueprint but that we get some idea of the relationship between all the phenomena if they are subsumed under the idea of <u>mathematically</u> specifying a path in which the fourth dimension of time functions along the spatial dimension measuring distance. Similarly, the **level 3** idea is commensurate with the propositions, theorems, and problems of the Third Day, insofar as the deliberations concerning incline planes and the chords of circles also require the two variables of time and distance to function as one.

Following the chronology of events it can be seen that Galileo was very much influenced throughout his life by the need to make a living, by the stimulus of new ideas, by technological developments, and perhaps most of all by the need to deal with the opposition.[42] However, for the purposes of this argument it is not necessary to adhere to

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[42] His acceptance of Copernican heliocentrism, *c* 1595, forced him to face the difficulties this caused for terrestrial motions. Between 1602 and 1608 he worked fairly steadily at these problems but in 1609, when busy compiling the treatise on natural motions that three decades later was to appear as the *Two New Sciences*, he was deflected from this path by the invention of the spyglass. By the beginning of December he had constructed a twenty-power telescope, which he proceeded to turn on the heavens. The results, published in March 1610 as *Sidereus Nuncius*, provoked some violent reactions. In June the following year yet another dispute prompted Galileo to begin an essay on hydrostatics. While he was writing this a pseudonomously published book on sunspots, by the German Jesuit Christopher Scheiner, caused him to refocus his attention yet again. With the *Treatise on Floating Bodies* safely launched he began a reply, in the form of letters, to Mark Welser of Augsburg. *Letters on Sunspots*, insofar as it declared science to be divorced from philosophy and declared its author to be an unambiguous supporter of heliocentrism, served to concentrate minds on the relationship between developing science and the Church. Enemies made during the controversy about floating bodies now combined into a league at Florence, intent upon refuting anything and everything Galileo said. In a 'Letter to Castelli', which in 1615 was worked up into the longer *Letter to the Grand Duchess Christina*, Galileo argued that the Bible and Nature were two separate paths to God, although both dependent upon Him. Consequently there could be no contradiction between the two. The Bible was open to interpretation, always the job of theologians, but Nature should be allowed to speak freely for herself. The resulting furore ended, in 1620, in Galileo being effectively silenced, for the time being, on the subject of Copernicus. Being greatly stimulated by the appearance of three comets in the autumn of 1618 - and being urged by the Lincean Academy to reply to Grassi's vitriolic attack on his own account of the apparent curvature of cometary paths, which Grassi put down to a clandestine Copernicanism - Galileo published, in 1623, *The Assayer*. This statement of his method of scientific reasoning required, simultaneously, 'sensible experiences and necessary demonstrations'. He explicitly asserted that mathematics was central to this process as the language necessary for comprehending nature and, with his explication of what later became known as the primary/secondary distinction, he implied that it was an essential property of bodies also. Ultimately his misreading of the religious and political situation lead him to publish the *Dialogue* (1632), his defence of Copernican heliocentrism, with the result that his very last

a chronological order. The point is to discover the degree to which Galileo's various areas of investigation imply, through their methodological strategies, the immediately superior levels in the aim-oriented empiricist framework, thus proving that a unifying strand runs through apparently different procedures. As such, they can be taken as more or less autonomous entities or processes and the arguments contained in the *Letters to Mark Welser* [51] concerning sunspots is a good place to begin.

In this instance, Galileo proposed an elegant mathematical model, concerning the sphericity and rotation of the sun. From this hypothesis, which was formulated with the help of empirical data and could therefore reasonably claim to represent reality, he rigorously deduced testable conclusions which were then verified through quantifiable observations. He proposed that all the evidence could be interpreted in terms of,

> **the spots being contiguous to the surface of the sun, and with this surface being spherical rather than any other shape, and with their being carried around by the rotation of the sun itself.** [52]

Therefore it can be plausibly stated that there is a judicious mixture of **level 1** and **level 2** concepts at work here. Galileo declared,

> **the distance passed by the same spot in equal times becomes always less as the spot is situated nearer the edge of the sun. Careful observation shows also that these increases and decreases of travel are quite in proportion to the versed sines of equal arcs, as would happen only in circular motion contiguous to the sun itself.** [53]

The **level 1** observations and measurements indicated that the distance passed by the same spot in equal times decreases as it approaches the edge of the disk of the sun. A combination of **level 1** observations and **level 2** calculations proved that these decreases and increases of travel are in proportion to the *versed sines of equal arcs*. This strongly suggests that the sun is spherical, that it rotates, and that it carries the spots with it. Further investigations conducted at both **levels** allowed Galileo to quantify the constantly varying space between the sunspots and thereby reinforce his conclusion.

> **Accurate observations of the ratios of these separations and approaches shows that they can occur only upon the very surface of the solar globe.** [54]

The guiding framework of **Level 4** is clearly present in the methodology which steered Galileo towards the recognition that the spots were moving at the precise rate associated

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book, the long-overdue treatise on natural motions, was finally produced as he languished under house arrest for the final decade of his life.

with the uniform rotational motion of a sphere.[43] Furthermore, as we have already mentioned in the section on **Blueprint Clashes**, the observations and calculations which are compatible with Galileo's own hypothesis, clash destructively with rival, Aristotelian hypotheses as articulated by Scheiner. The Theory of Sunspots does not merely employ Galileo's **level 4 P** and imply certain, restricted connections with his **level 3** blueprint, it is also very close to aim-oriented empiricist methodology <u>in</u> <u>practice</u>. It involves the comparison of the empirical progress of two rival research programmes in which the same evidence which renders the one programme the most empirically progressive of the two simultaneously denies the existence of the assertions (and the foundational cosmology) of the other.

It has been pointed out that Galileo was canny enough not to claim that he had irrevocably and unarguably demonstrated the truth of his theory.[44] However, what is important is that, from the point of view of attempting to locate Galileo's work in a progressive tradition, it is now regarded as a classic example of scientific method, typical of the very best of the physical sciences through history. Such a method involves the drawing out of the observable consequences of a theory, such that certain, specified, quantifiable things are required to change in a very explicit way. It is possible to extract from such a theory the sort of predictions that lead to that which is quantifiably testable: if the consequences persistently turn out to be other than as predicted then there has to be something wrong with the theory. In the case of 'sunspots' the predictions that as the sunspots approached the edge of the sun the distances travelled in a given time would get shorter were confirmed.

With regard to the treatise on motion, it is unquestionably the case that the inceptive advance was empirical. According to Drake it is quite likely that Galileo's interest in accelerations, particularly those acquired continuously, resulted from the

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[43] Uniform circular motion, as already stated, functioned as the level 3 **metaphysical foundation of the unified cosmos and was mathematically capturable in the form of *'equal spaces passed in equal times'*. This motion, which changes in the simplest and most regular fashion of all, clearly implies Galileo's simplified approximation of Copernican circularity. As noted earlier, 'sunspots' partakes of the notion which links all of the phenomena of motion, that of specifying, in mathematical terms, the path representing the distance travelled and the time taken. It is a simpler *modus operandi* than that employed in projectile motion because the curve which is generated is dictated by the shape of the sun itself, rather than by the composition of two different motions, one of which falls unconstrainedly. Nevertheless there are two variables, the distance travelled and the time it takes to do so, and they are represented by the same path.**

[44] He knew that his evidence did not have the status of the phases of Venus, from which it simply has to follow that Venus is orbiting the sun. His conclusion was attained by reasoning from both the (unstated) definition of uniform circular motion <u>and</u> the quantified ratios between the observed 'increases and decreases of travel' and then verifying the lot by further observation.

experiments he performed with a long and heavy pendulum in 1602.[55] With such an instrument, the steady increase and decrease in speed becomes discernible, almost tangible, indicating the persistence of motion once attained and the importance of accelerations in downward motions. Barbour considers that a remark of Copernicus, in his *Chap. 8* of *Book 1*, might also have had some import,

> **Furthermore, bodies that are carried upward and downward, even when deprived of circular motion, do not execute a simple, constant, and uniform motion. ... Whatever falls moves slowly at first, but increases its speed as it drops.** [56]

In the event, Galileo made a law of nature 'visible' by slowing down the rate of acceleration of a falling body and slightly improving his timekeeping procedures. Drake gives the following account,

> **In 1604 he devised a way to measure actual speeds in acceleration. For this purpose he let a ball roll from rest down a very gently sloping plane (less than 2°) and marked its positions after a series of equal times, judging by musical beats of about a half-second. These distances were then measured in units of about one millimeter.** [57]

However, whatever it was that stimulated him to design this **level 1** experiment, it remains the case that he first recognised the possibility of quantifying an elementary motion. He was quickly rewarded by the discovery of the simple and elegant *odd-numbers law*, to the effect that if the distance travelled in the first unit of time is taken as unity, then the distance covered in the second unit of time is equal to three, that in the next to five, that in the following to seven, and so on. Consequently, if *s(t)* is the distance traversed in time *t*, then

$$\begin{aligned} s(1) - s(0) &= 1 \\ s(2) - s(1) &= 3 \\ s(3) - s(2) &= 5 \\ &\dots \\ s(n) - s(n-1) &= 2n - 1 \end{aligned} \qquad \text{(Fig. 3.1)}$$

The *odd-numbers law*, which Barbour has designated the very first law in a four-dimensional geometry of motion, was therefore derived at **level 1**. [58] It may well be that for Galileo this was a *'law of miraculous Pythagorean harmony'* [59] but he did not stumble upon it by accident. The methodology used throughout implies a belief that there exists an inner mathematical harmony <u>to</u> <u>be</u> <u>found</u> behind the unreliable and misleading appearances of natural phenomena, even though that undefiled state can only be approximated during the empirical process. Without the underlying **level 4** blueprint idea that simple, mathematically formulable laws govern the motion of bodies everywhere, expressed in the forms such as figure 3.1, certain of Galileo's achievements would be impossible. For instance, the recognition that the physical medium, in the shape of things like friction and air resistance, could affect the motions of bodies was not new to Galileo. However, his methodology demonstrates that he possessed a basal theoretical conception of <u>just what can be meaningfully evaluated</u>, which informed all of his actions. How else would he have seen that the correct procedure was to utilize an unequivocal mathematical law firstly, to distinguish between the various physical properties which characterized the concrete example in question and secondly, so to speak, to disconnect the distorting elements that, by definition, belonged to the physical realm. Without admitting the existence of the underlying **level 4 P** concept, how can <u>we</u> make sense of his idea of a <u>standard</u>, a perfect mathematical law, which a body would follow in the absence of the perturbations contingent upon the physical medium?

Another **level 1** interference was perpetrated by the table-top experiment which, as we have already noted, served to verify not one but <u>two</u> of Galileo's laws. There was the indirect verification of the *law of the persistence of horizontal motion*, which was his *law of (circular) inertia*, and the direct verification of the *law of parabolic motion.* The former, which we have already identified as one of those limited occasions upon which it is possible to vary the initial conditions, was indirectly tested through establishing that the speed acquired in descent is dependent upon the starting height and also determines the magnitude of the uniform horizontal motion on the surface of the table. This is achieved by means of testing the relationship between height from which the descent was made and the horizontal distance travelled by the ball after leaving the table and before hitting the ground. Again there is an almost tangible demonstration of what is going on. This is even more true of the phenomenon which is simultaneously produced, that of parabolic motion. The perturbations introduced by the contingent world and the confusions perpetrated upon the senses are eliminated to demonstrate the superposition of inertial horizontal motion and free-fall and turn it into something measurable and quantifiable. The **level 4** assumption that simple laws govern natural phenomena is implicit in all Galileo's table top experiments.

The parabolic motion part of the table-top experiment obviously exemplifies the key **level 3** blueprint, insofar as the mathematical, or rather the geometric structure of the universe is captured in terms of the nature of the curves, traced out in space and time, by relatively moving bodies. This is best demonstrated at **level 2** where the real work involves mathematics and involves the derivation of the *times-squared law*, or *law of free-fall* from its consequence, the **level 1** *odd-numbers law*. According to Drake, Galileo took something of a tortuous route to accomplish the task of deducing from the integrated form of his law the correct law that governs the increase of speed with time.[60] With hindsight, elementary addition in figure 3.1 demonstrates that the distance s of descent increases as the square of the time.

$$\begin{aligned} s\,(0) &= 0 \\ s\,(1) &= 1 \\ s\,(2) &= 4 \\ &\dots \\ s\,(\mathrm{n}) &= \mathrm{n}^2 \end{aligned} \qquad \text{(Fig. 3.2)}$$

Or, to give it the modern formulation,

$$s = \tfrac{1}{2}at^2$$

where, in the units adopted for the purposes of this account, a, the acceleration, has the value of 2. In the account in Day 3 of the *Two New Sciences* he presents the steps by which he arrived at the law more or less in reverse, with a definition of uniformly accelerated motion (equal increments of speed added in equal increments of time), followed by the geometrical proof of the *times-squared law* and, finally, the derivation of its consequence the *odd-numbers law*. We saw, earlier, the degree to which the *times-squared law*, viewed as a special case of projectile motion with the initial velocity set at zero, could also be said to exemplify the **level 3** blueprint. In addition it is again clear that, as in the 'sunspots' example, here is another classic case of a scientific method which tests the consequences of a theory. Had Galileo been wrong about the notion of equal increments in equal times, then the spaces described by a body in free-fall would not have been to each other *'as the squares of the time-intervals employed in traversing these distances'* [61] and the *'increments in the distances traversed during these equal time-intervals'* would not have been to one another *'as the odd numbers beginning with unity'*. [62] However, once he had the *law of free-fall* he was able to turn his attention to the simplest problem in dynamics, the determination of the path of a projectile. By the simultaneous action of the universal law governing fall and the specific initial conditions, the actual motion of a real body was realised in the form of a curve, traced out in space and time. It proved to have the geometric structure of a parabola, emerging mathematically as a result of combining uniform horizontal motion and uniformly accelerating vertical motion. Although Galileo did not create dynamics, having no reference to force or mass, nevertheless in the laws just discussed and particularly in the incorporation of time, he produced some of the cornerstones upon which it was raised.

The thought experiments in the *Dialogue*, devised to show that moving with uniform motion is experimentally equivalent to being at rest, are qualitative rather than quantitative but nevertheless are consistent with **level 4 P** and the more specific **level 3** blueprint. This is because they encompass, without explicitly formulating the concepts, the key elements of what became known as Galilean invariance, which was only formulated qualitatively but was very successful as an explanation for the stability of our immediate environment and for the apparent motions observed upon it.[45] It presupposes physicalism because it is actually the product of that part of the argument where Galileo first demonstrated to the outside world that he was endeavouring to create a unified dynamics. As such it belongs within the before-mentioned framework adopted to confront the problems created by the Copernican hypothesis and encapsulating the basic **level 4** requirement that simple mathematically formulated laws govern the motion of bodies everywhere. Furthermore, two key elements of Galilean invariance, the principle of primordial or elemental motions and the principle of the composition of motions, are intimately related to the more specific **level 3** blueprint in that in one particular empirical situation, the one that demonstrates their very existence, they function to derive a curve traced out by a body, near to the surface of the earth, in extended three-dimensional space and time.

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[45] **The first element maintains that there is a** class of distinguished frames of reference, **moving at uniform speed on the surface of the earth. The most significant property of these frames is captured is that for observations made strictly within them it is impossible to establish the existence, or otherwise, of the frame. Then there is a physical principle, the concept of** *primordial motions*, **which explains why it is impossible to assert the motion or rest of a frame simply by making observations from within it. All parts of a frame partake of a primordial motion which, being common to all, is unobservable. Finally there is the** principle of the composition of motions. **This allows for the superposition of different elemental motions without any mutual interference. This concept of the superposition of primordial motions brings out the relativity of perceived motion among the parts sharing the common motion. The *'flies, butterflies, and other small flying animals'* in the ship's cabin share a common motion with the ship but superimpose upon this certain relative motions among themselves.**

At this point, it is worth considering again just what is - and what is not - being asserted. What is not being asserted is that Galileo provided a philosophical justification for mathematical realism. Although he believed in it, he never proved its unequivocal truth.[46] For a long time it seems that Galileo aspired to derive his propositions on motion from evident principles. In earlier works, such as *De motu* and *The Treatise on Floating Bodies*, he was wont to rely on such immediately evident principles as, 'the heavier cannot be raised by the less heavy' and 'absolutely equal weights moving with equal speeds are of equal force and moment in their operation'. However, the principles concerning accelerated motion, upon which the whole subsequent body of propositions depends, do not present the sort of situation in which anything might be made immediately evident and utilized to function as a fundamental first principle. Direct empirical demonstration, another route to proving the evident truth of a proposition, only succeeds in the case of the *times-squared theorem* and the *odd-numbers law*, which is anyway a consequence of the *times-squared theorem*. An interpretation which holds that the worth of a mathematical approach to nature can only be established in such a manner has,

> **to show what appears to be counter-intuitive, namely that experimental observables are numbers or geometrical shapes, that the very experiences that experiments bring about are mathematical experiences.** [63]

Moreover, it seems that because physically interpreted geometrical theorems do not apply exactly to perceived physical objects, geometry is therefore the best way of characterizing them. The only way that Galileo had of demonstrating that experimental experiences are mathematical experiences, given this paradox, involved deciding which of the material hindrances ought properly to be deduced from the concrete example, in order that it might approximate the abstract case. The only way that this question could be resolved meant employing the very geometrized physics whose validity he was fighting so hard to establish. There is certainly evidence that in the Third Day of the *Two New Sciences* Galileo hoped to prove the truth of mathematical realism in the case of moving bodies. Drake has argued that ultimately Galileo abandoned the attempt to experimentally demonstrate, or derive from more fundamental principles, the postulates upon which the work on motion is constructed. He compromised and made the

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[46] **Any interpretation which maintains that his methodological ideal was to deduce mathematical propositions from true and evident principles can only conclude that this ideal eluded him.**

postulate of accelerated free-fall dependent upon the *times-squared theorem* and the awkward postulate of equal speeds dependent upon the postulate of accelerated free-fall.[64] However, Galileo's position is shot through with ambiguity. During the Fourth Day of the *Two New Sciences* he provided a proof that firing at an elevation of forty-five degrees will give a projectile its maximum range [65] but he did not display it as confirmation of the principles from which it was derived. Instead he used it to reinforce his belief that the chief virtue of holding a mathematical view of reality is the way in which the mathematical demonstration of physical phenomena, which is the result of this view, *'opens the mind to the understanding and certainty of other effects without need of recourse to experiments'*.[66] When admitting that the *'single assumption'* of equal speeds can be demonstrated for acceleration along the arc of a circle but not for the all-important case of planes, he urges,

> **Let us then, for the present, take this as a *postulate*,** (my italics) **the absolute truth of which will be established when we find that the inferences from it correspond to and agree perfectly with experiment.** [67]

Indeed, after his successes with the mathematization of motion, Galileo, seemed to move gradually away from the ideal of working from indubitable first principles in mechanics until in 1639 he confessed, in a letter to Baliani, that there did not seem to be the sort of evidence which is necessary for principles that are assumed to be known. He promised to produce it but never did. [68]

What <u>is</u> being asserted, on the other hand, is that Galileo can be <u>interpreted</u> as implying a version of physicalism in much, if not all, of his work. By this I do not mean that he made these assumptions consciously but implicitly, as it might be in the context of discovery, but having done so did not then explicitly refer to them or employ them in arguments. I mean that it is an aim-oriented empiricist perspective that illuminates these perspectives in his work and although not explicitly referred to they are employed in his arguments and methods. In repudiating Aristotelianism and instigating what might be called 'Galileanism', he was responsible for transforming **level 4** and **level 3** ideas in the explicitly stated interest of promoting progress in knowledge. This, as will be seen, was done because he felt that the existing cosmology and associated methodology produced no knowledge worth having. Aristotelian natural philosophy was a complete body of conclusions about astronomy and physics, marshalled by logical procedures within a framework of metaphysical principles. Galileo castigated it for attracting

the natural curiosity of men without ever offering them one single sample of that sharpness of true proof by which the taste may be awakened to know how insipid is its ordinary fare. [69]

It is in this sense that **level 4 P** and the more specific **level 3** assumptions permeate the work carried out at the lower **levels,** an assertion best established by means of an examination of the methods which Galileo utilized in performing that work. It is possible to grasp the unity underlying what appears on the surface to be fragmentary once the work is interpreted in terms of transformations at **levels 3** and **4**. The blueprint engenders the methodology and is reflected in its practices: utilizing the geometrized physics, far from leading to the circularity characteristic of attempts to <u>prove</u> <u>the</u> <u>truth</u> of mathematical realism, is precisely the means to establish its validity. If it, *'opens the mind to the understanding and certainty of other effects'* then it is instrumental in progressively building up a body of knowledge, the empirical fruitfulness of which confirms the wisdom of having originally chosen the associated metaphysical theses. It also open up the possibility that future empirical fruitfulness will govern the <u>choice</u> of metaphysical theses. It is anyway questionable how serious Galileo's attempts at *a priori* demonstration really were: after all, there is an epistemological error that can be laid to his charge which resulted from his trying to import the certainty of mathematics into mathematized physics. Furthermore, the widely-held view that the areas of Galileo's work which have some physico-mathematics in common have also rather different methodologies, is demonstrated to be erroneous.[47] In astronomical calculations, in mathematical models, and in geometrical demonstrations the common factor is the extent to which **level 4 P** is implied in the methodology employed. Superficially different procedures are, in fact, derivatives of the same blueprint family. Galileo provides an early example of the fundamental truth that scientific methods are not homogeneous wholes to be applied to given lumps of scientific investigation, rather they are composed of strands that have to be woven according to the requirements of the blueprint and the

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[47] In mechanics he is often interpreted to have been struggling to produce a rigorous mathematical treatise, which employed the deductive method to develop a body of propositions derived from (what he considered to be) true and evident principles. In astronomy he is often said to have mixed sound scientific reasoning with sometimes suspect arguments and employed a method demanding the empirical verification of hypotheses and the falsification of all likely alternatives. Sometimes the mode of expression is a mathematical model, as in the discussions on sunspots, often it is a geometrical demonstration, as in the treatise on motion, and in astronomy it takes the form of quantified observation. In some cases more than one will apply.

circumstances of the individual investigation. It is this that directs which of the mathematically-engendered methodological tools are appropriate in the specific case.

For example, to pursue in more detail the propositions upon which the edifice of accelerated and projectile motion is built is to observe the same mingling of **levels 1** and **2** - and the same dependence on the higher **levels** leading to a productive interplay - as has already been considered in the examples so far discussed. Most of the propositions that deal with uniformly accelerated motion are derived from the *postulate of accelerated free-fall*, the *times-squared theorem*, the *'single principle'* which Sagredo delightedly hails as the starting point for the *'proofs of so many theorems'*, [70] and the *postulate of equal speeds*. The propositions based on projectile motion also required the *double-distance law*. The *postulate of accelerated free-fall* is expressed as follows:

> **A motion is said to be equally or uniformly accelerated when, starting from rest, its momentum receives equal increments in equal times.** [71]

The *postulate of equal speeds* assumes,

> **The speeds acquired by one and the same body moving down planes of different inclinations are equal when the heights of these planes are equal.** [72]

The *times-squared theorem*, which was mentioned earlier in the discussion about how far Galileo can be said to have had the concept of what varies, what does not and the relationship between them, states,

> **The spaces described by a body falling from rest with a uniformly accelerated motion are to each other as the squares of the time-intervals employed in traversing those distances.** [73]

Finally, the *double-distance law* maintains,

> **You must therefore know that the falling body, ever acquiring new speed according to the ratios already mentioned, wherever it may be in the line of its motion it will have such a degree of velocity that were it to continue to move uniformly with this, then in a second time equal to its previous descent it would traverse twice the distance already passed over.** [74]

The **level 1** elements within these propositions are to be found in the inclined plane experiment and in the pendulum experiment. This showed that in spite of obstructions, in the form of a nail hammered in, the bob will always (provided that the obstruction does not shorten the string too much) rise up to the same height from which it descended.[75] I have already mentioned that the only proposition with straightforward **level 1** support is the *times-squared theorem*. Although it originated from the discovery of its own consequence, the *odd numbers law*, in the *Two New Sciences* Galileo describes his method of verifying it as follows:

> **we rolled the ball, as I was just saying, along the channel, noting, in a manner presently to be described, the time required to make the descent. We repeated this experiment more than once in order to measure the time with an accuracy such that the deviation between two observations never exceeded one-tenth of a pulse-beat. Having performed this operation, and having assured ourselves of its reliability, we now rolled the ball only one-quarter the length of the channel; and having measured the time of its descent, we found it precisely one-half of the former. Next we tried other distances, comparing the time for the whole length with that for the half, or with that for two-thirds, or indeed for any fraction; in such experiments, repeated a full hundred times, we always found that the spaces traversed were to each other as the squares of the times, and this was true for all inclinations of the plane.** [76]

The postulate of accelerated free-fall which, to borrow Sagredo's phrase, functions as *'arbitrary definition'* [77] is simply assumed [78] and then indirectly supported by the inclined place experiment, as is the *double-distance law.* The *postulate of equal speeds* is also assumed but then underwritten to a degree by the pendulum and nail experiment in so far as that established a relationship between speed at the end of the fall with height at the beginning. The important point, however, is that the **level 4** concepts which have been interpreted as constituting Galileo's version of physicalism run through the whole like a thread. All the postulates that are foundational to accelerated and projectile motion actively accept the idea that that there is a standard, exemplified by a perfect mathematical law which a body would follow in the absence of the physical medium, a standard which allows one to uncouple, from the various physical properties which characterized the concrete example in question, the distorting components. There are explicit statements to that effect. Following the presentation of the postulate of accelerated free-fall, Sagredo exclaims that

> **provided of course there are no chance or outside resistances ... my reason tells me at once that a heavy and perfectly round ball descending along the lines CA, CD, CB would reach the terminal points A, D, A, with equal momenta.** [79]

A little later, Salviati acknowledges that *'resistance and opposition'* was the begetter of the pendulum and nail experiment. Planes would form an angle which would present an obstacle to the ball and prevent it from rising according to the ideal motion once it had reached the lowest point. [80] Galileo asserted that an ideal object on an ideal inclined plane would behave in an analogous manner to the pendulum but he was well aware that acceleration along the arc of a circle, *'varies in a manner greatly different from that which we have assumed for planes'.* [81] Consequently, all postulates are entirely commensurate with the **level 4** idea of a perfect mathematical law governing the motion of bodies everywhere and, by extension, so are the many theorems and problems whose proofs are thereby deduced. Moreover, the idea which is related to **level 3**, of mathematically specifying a path in which the fourth dimension of time functions along the spatial dimension measuring distance, is commensurate with the propositions, theorems, and problems of the Third Day as well as with those of the Fourth, which deal with parabolic motion. The first phenomena involving a change of velocity to be transformed into fruitful geometry, in which straight lines were employed to illuminate a dynamical situation, was illustrated by an experimental set-up which obliged the fourth dimension of time to function along the spatial dimension measuring distance. (fig. 3.3 and fig 3.4) Finally, the arguments which are assembled, and shown to be compatible with Galileo's own hypothesis, simultaneously clash destructively with rival hypotheses. The rival hypothesis in this case is the one which substitutes distance for time.

> **If the velocities are in proportion to the spaces traversed, or to be traversed, then these spaces are traversed in equal intervals of time; if, therefore, the velocity with which the falling body traverses a space of eight feet were double that with which it covered the first four feet (just as the one distance is double the other) then the time-intervals required for these passages would be equal. But for one and the same body to fall eight feet and four feet in the same time is possible only in the case of instantaneous (discontinuous) motion; but observation shows us that the motion of a falling body occupies time, and less of it in covering a distance of four feet than of eight feet; therefore it is not true that its velocity increases in proportion to the space.** [82]

What follows is a series of geometrical calculations working out the proofs of theorems dealing with the length of slope, height of fall, inclination of slope, speed of fall, time of fall, etc., moving through small and necessary steps to evident conclusions. No experiments are offered for these further consequences of the foundational principles and they do not act as recognisable first principles: the higher **level 3** and **4** blueprint ideas have directed that the appropriate methodological tool in this case is that of the geometrical demonstration.

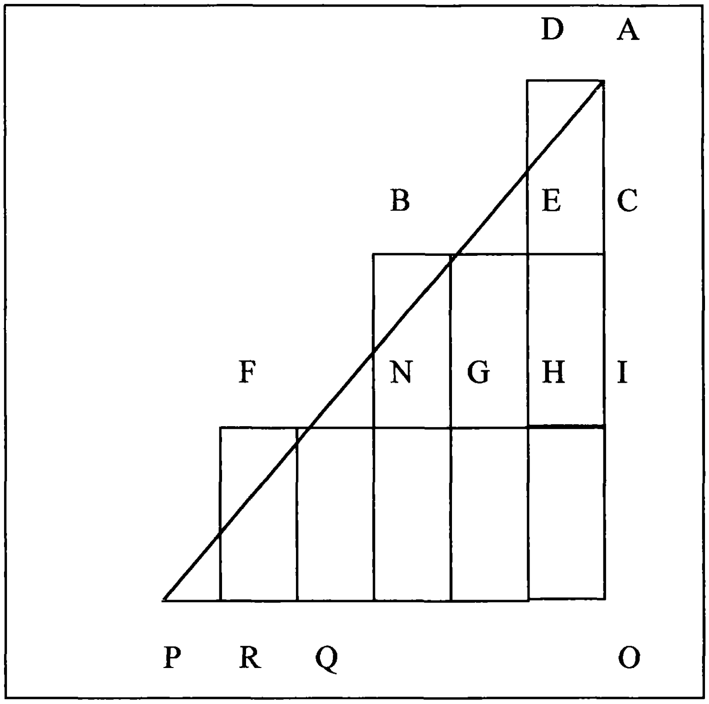

Fig. 3.3

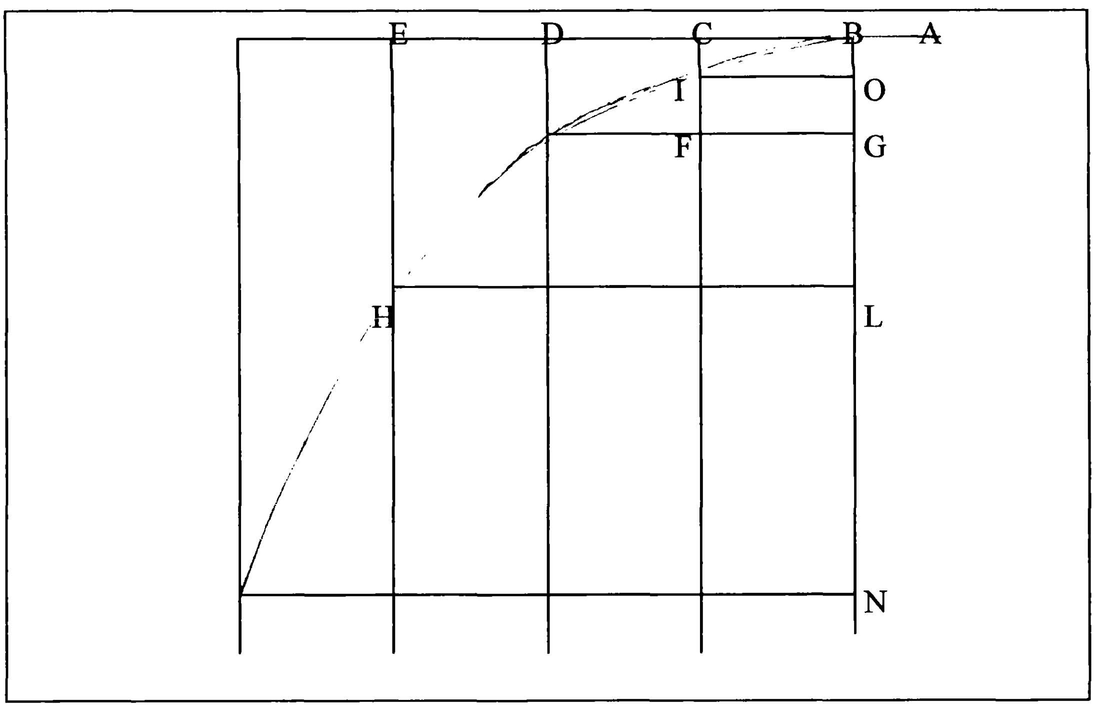

Fig 3.4

The Loop of Positive Feedback

There is more, however, to Galileo's achievements in astronomy and the science of motion than can be encapsulated by the evidence that the Galilean methodology consistently implies a version of **level 4 P** and involves the comparison of the empirical progress of two rival research programmes. Both of these characteristics of Galilean science fulfil important aim-oriented empiricist requirements but does Galilean science fulfil the supreme requirement, which involves a positive feedback between improving knowledge and improving aims and methods? If the description of the aim-oriented empiricist hierarchy of assumptions is recalled to mind, it will be remembered that there is a methodological rule, operative at each of the **levels**, which asserts that preference should be given to those theories which best agree with the chosen cosmological assumption. In other words, the empirical fruitfulness of a research programme governs the choice and acceptance of its associated metaphysical theses at **levels 3** to **7**. Further, the full implementation of the principle of intellectual integrity demands that whenever current cosmological (and allied methodological) suppositions fail to generate the growth of knowledge, or are less successful in this respect than rival assumptions, then the former must be exchanged for the latter at whichever of the **levels** is deemed appropriate. From this practice certain benefits are said to accrue. Firstly, it offers the best possible chance of making progress in acquiring authentic knowledge, whilst reducing to a minimum the likelihood of becoming trapped in a cosmological and methodological dead-end that blocks the growth of knowledge. Even such a seemingly fecund conjecture as **level 4 P** may one day prove false and yet this system means that it can be fully explored and exploited, via the **level 4** methodological injunction to chose the theory which does the best justice to physicalism, <u>without</u> permanently trapping science within physicalist parameters. Secondly, it offers the expectation that increasing knowledge will lead to the improvement in the cosmological assumptions implicit in current methods, thereby leading to improvements in those methods. This positive feedback is a vital feature of scientific rationality and begins with the selection, at metamethodological level, of a metaphysics (and related methodology) which seems better founded with regard to the actual nature of the universe than any rival because of the empirical success of its associated research programme.[48] It is obvious that

[48] This is not to argue that empirical success somehow <u>guarantees</u> a purchase on the ultimate truth.

persistent failure can only be accommodated by invoking the principle of intellectual integrity and adopting new metaphysical assumptions and related methods. However, empirical success, in time, will also require the application of the principle, by introducing pressures and tensions that demand the adaptation of the existing metaphysics and methodology, to the improvement of the latter and the further enhancement of knowledge, understanding, and technological skill. Indeed, in this system it is a requirement for rationality that the **level 4** blueprint can change and the **level 3** blueprint does change, as its legitimately related methods evolve with evolving knowledge: it is precisely that which constitutes the mechanism whereby improvements in our knowledge progressively enhance our capacity to further improve knowledge. It is the mechanism by which science can continuously adjust its own nature in the light of what it discovers about the nature of the universe itself.

Having argued thus far, now is the time to enquire into the degree to which Galileo's work encompasses this vital characteristic by which improving knowledge leads to an improvement in our knowledge about how to improve knowledge. To try to cast the metal of Galileo's endeavours and achievements into a die comprising **levels 3** to **10** would be to reconstruct beyond anything that the historical record could support. However, it is possible to make some claims without insinuating that he had anything like the generalized aim-oriented empiricist framework in mind. Certainly it is safe to say that built into his entire enterprise is a belief which takes the form of the idea that the universe is so constructed that better explanations (than are currently accepted for natural phenomena) do exist, potentially, to be discovered.[49] To begin with there is explicit evidence that Galileo actively sought to improve explanatory knowledge. In the *Dialogue* the conjunction of the formulation of an hypothesis postulating certain observable consequences and the detection of those consequences was held to explain the observed phenomena. Failure to detect the consequences need not result in the abandonment of the hypothesis as long as an adequate explanation could be found for the

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[49] **It could be argued that his entire life's work is cast around this concept. From his earliest days he was dissatisfied with the prevailing Aristotelian blueprint and wished to replace it with something else. In** *De Motu* **(c. 1590) no less than six chapters open with the bullish phrase** *'in quo contra Aristotelem concluditur'*. **See Drake, S. and Drabkin, I.,** *Galileo on Motion and Mechanics,* Madison: University of Wisconsin Press, 1960. **He complained that Aristotle did not grasp the significance of mathematics and placed too much uncritical reliance on everyday, sensory experience. He was simply wrong in practically everything he wrote concerning local motion.**

failure. In the case of rival theories which both explain the relevant facts, Galileo preferred that which is the least *ad hoc* and displays the greatest explanatory power.[83] There is ample, and perhaps more convincing, evidence of his general desire for explanatory knowledge in the methodological practices so far considered[50] and in his more explicit pronouncements on methodology too. An example of this can be found in his argument that Aristotle <u>would</u> have embraced heliocentrism, had he been in possession of the necessary evidence.[51] [84] According to Galileo, a decent scientific method should be able to meet and accommodate the challenge of novel situations - and go on to produce better explanations than it could have before - without the whole edifice tumbling down.[85] A good method, properly applied would lead not only to the re-ordering of propositions but to the actual growth of knowledge. We have already seen evidence of this in the section on **Blueprint Clashes,** where it is demonstrated that celestial phenomena are much better accounted for in a heliocentric system. The concept built into such beliefs and procedures as these corresponds to the **level 8** thesis of meta-knowability, which asserts that the universe is so constructed that there is some discoverable assumption that can be made about the nature of the universe which aids the growth of explanatory knowledge.

In **Chapter One** I defined knowledge as explanatory if it is significant for what actually exists or occurs and also for an entire spectrum of counterfactual circumstances. In such a state it facilitates our understanding of why thing must, in a sense, occur as they do.[52] The most basic scientific law must have this facility to make predictions about counterfactual situations. The *times-squared law* fulfils this requirement insofar as it holds for any freely falling body anywhere. The same is true for *projectile motion*: the trajectory of any body, anywhere, under the simultaneous constraint of uniform horizontal motion and uniformly accelerating free-fall, will be a segment of a parabola.

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[50] **Insofar as the Galilean methodology consistently implies a version of** level 4 P, **it consistently implies, in accordance with the principle of intellectual integrity, that the existing Scholastic cosmology and associated methodology produced no explanatory knowledge worth having.**

[51] **Galileo made the point that, contrary to what his Peripatetic followers feared, there <u>is</u> strength enough in Aristotle's method to meet the challenge of new situations: when there is good observational evidence that it is actually the sun which lies at the centre of the celestial revolutions, Aristotle's method allows that what is fundamental, and therefore to be preferred, is the proposition that the centre of the universe is also the centre of the celestial revolutions. Even Simplicio is forced to agree,** *Dialogue*, 321.

[52] **The examples discussed were of very <u>primitive</u> explanatory knowledge, especially when compared to the explanatory power of scientific knowledge, but for <u>any</u> physical law or theory, it is still true that there are implications both for actual occurrences and for an infinity of possible states of affairs.**

Insofar as these things occur as they do, in all circumstances, because they are mathematically generated, they also demonstrate the fruitfulness inherent in the basic physicalist blueprint which Galileo can be seen to have adopted, and the manner in which its powerful and restraining heuristics and methodology aided the improvement of explanatory knowledge. In aim-oriented empiricist terms it is an instantiation of the tenet that physicalism has, historically, almost always existed in such a form as to make it possible to draw out the essential notion in a range of ways, adopt an appropriate methodology, and see if knowledge improves. Here is an example of this simple, knowledge-improving characteristic that, throughout history, has made it possible to judge between a physicalist blueprint and any rival blueprints.

However, Galileo does not conform so well to theses at all **levels**. Nothing he produced, for example, suggests that he further believed that the world is such that at <u>any</u> given stage in the development of explanatory knowledge better explanations for phenomena can be discovered. His proud boast of a *'new method fraught with numerous and wonderful results'* [86] waiting to be discovered does not amount more than a belief that many other phenomena would be mathematized as successfully as he had mathematized free-fall and projectile motion. In so far as his method embodies conclusions they were partial and fragmentary and likely, Galileo thought, to remain so no matter how far scientific knowledge progresses.

> **To put aside hints and speak plainly, and dealing with science as a method of demonstration and reasoning capable of human pursuit, I hold that the more this partakes of perfection, the smaller the number of propositions it will promise to teach, and the fewer yet it will conclusively prove.** [87]

The problem was that events of Nature, which belong to the province of physics and astronomy, will always elude perfect understanding.

> **There is not a single effect in Nature, not even the least that exists, such that the most ingenious theorists can ever arrive at a complete understanding of it. This vain presumption of understanding everything can have no other basis than never understanding anything. For anyone who had experienced just once the perfect understanding of one single thing, and had truly tasted how knowledge is attained, would understand that of the infinity of other truths he understands nothing.** [88]

The perfect understanding, of course, *'is of the mathematical sciences alone'*, in which the human intellect *'has as much absolute certainty as Nature herself'* and in this knowledge at least *'equals the Divine in objective certainty'.* [89] As we have seen, Galileo found no sure means of bringing out the underlying mathematics without abstracting from the material impediments. Scientific knowledge would progress, he felt:

> **if our conceptions prove true, new achievements will have been made ... if false, their refutation will further confirm the original doctrines ... As to science, it can only improve.** [90]

However, his belief that he had discovered better explanations for phenomena does not amount to a belief that better explanations can always be discovered. Taken to the end point, this turns into a justification for believing the **level 5** thesis that the universe is comprehensible in some way or other. Galileo, as we have seen, never questioned that the world is comprehensible as such. His simple desire to alter what aim-oriented empiricism defines as the presiding **level 4** view about the way in which the universe <u>is</u> comprehensible is a reflection of his frequently stated belief that the current (Scholastic) cosmology and associated methodology were failing to generate reliable knowledge and needed to be replaced. In this he acted in conformity with the principle of intellectual integrity. The higher aim-oriented empiricist **levels 8** and **5** - and by implication the intervening **levels** too - are implicated in the pursuit of that desire.

Moreover, an examination of what is involved in any improvement in the explanatory aspect of scientific knowledge again demonstrates that Galileo had only begun to address the problem and did not fully appreciate that it entails approaching the ideal of a body of knowledge which, in principle, entails that nothing remains to be explained. In aim-oriented empiricist terms this involves the demonstration, at **level 3**, of the **level 4 P** requirement that there is just one invariant, with one type of unchanging property determining the form of change of the changeable feature(s).[53] The postulated existence of more than one entity, with different properties determining their evolution and interaction, will always suggest the existence of a more fundamental entity in whose

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[53] **This, as was discussed in Chapter Two, takes the form of some invariant which is postulated to be such that, although unchanging itself, is capable of being characterized by means of some physically interpreted mathematics from which, together with initial conditions, descriptions of that which varies can (in principle) be deduced. In terms of the** U **and** V **definition of the Comprehensibility Thesis quoted in** Chapter Two **this simply means that the dispositional property/properties** V **of invariant** U **are being captured through some physically interpreted mathematics and conjoined with the appropriate initial conditions.**

terms all other entities can be explained. Thus is the explanatory aspect of knowledge improved by trying to discover the increasing unity foundational to all phenomena at **levels 4** and **3**. Of course, Galileo made some moves towards unity. His stated preference, when confronted with two theories, for the most explanatory and least *ad hoc* theory entails that he preferred to adopt the most unified theory.[91] More significantly, the framework he adopted to confront the problems for terrestrial motion created by the Copernican hypothesis produced a unified motion for the earth and the heavens that could be captured by the physico-mathematical language of mechanics.[54] Thus where there had previously been two different regions, each with its distinct motion and explanation for that motion, there was now one universal region with one kind of (mathematical) explanation for the motions that occurred within it. By postulating, at **levels 4** and **3**, that phenomena of motion were unified in as much as they partook of some simple, unified, mathematical structure, Galileo increased the unity foundational to a restricted range of phenomena. By demonstrating that this was the case in specific instances, he also proved that he had advanced the explanatory aspect of knowledge. However, these were, in aim-oriented empiricist terms, modest steps. Although the various phenomena of motion are united conceptually and mathematically they have not been proved to be different aspects of one fundamental, invariant entity. In Galileo's work this concept is, at best, only implied in an extremely embryonic form. As was seen earlier, Galileo had one of the key ideas of physicalism[55] and his most successful formulation of it[56], *the times-squared law* and the *law of parabolic motion*, implied a relationship between what varies and what does not. This, of course, does not constitute physicalism: there is no firm evidence that Galileo had worked out the nature of the relationship between them. Moreover, the undeniable improvement in knowledge and understanding did not lead Galileo to an obvious further improvement in the basic aims and methods.[57] He felt that there was *'many another more remarkable result'* to be

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[54] **This framework involved the combination of two false, but simple and approximately correct notions, that of the heliocentric view and that of the law of inertia. Through the agency of a shared concept, that of uniform circular motion, the first step was taken towards the creation of a unified dynamics for the universe. This functioned as the metaphysical foundation of the unified cosmos and, being mathematically capturable in the form of *'equal spaces passed in equal times'*, also functioned to unify Copernican heliocentrism with the physico-mathematical language of mechanics.**

[55] **The** level 4 **idea that nature has some definite, simple, unified mathematical structure.**

[56] **This was that simple, mathematically formulable laws govern the motion of bodies everywhere.**

[57] **The idea that developments at the empirical level can percolate upwards and cause the metaphysics to develop is one that Bacon would recognise: one is reminded of his inductivist pyramid, with its broad empirical base and its metaphysical apex. The important thing about aim-oriented empiricism is that it**

found by other *'speculative minds'* from *'the principles which are set forth in this little treatise'* but this served confirm to him the wisdom of his basic choices rather than prompt him to further refinements.[92]

What we have is, if you like, the first (unconscious) step in the aim-oriented empiricist evolutionary process but they are firm, decisive steps. Galileo did select, at **level 4**, a metaphysics, plus associated methods which he then judged, in the light of the empirical success of the associated research programme, to be an improved basic view of the universe. It should be emphasised that this change in **level 4** ideas and methods in the interests of promoting knowledge was considerable. He made a contribution to the development of a particular blueprint tradition because he felt that the existing cosmology and associated methodology produced no explanatory knowledge worth having. In so doing he acted in accordance with the principle of intellectual integrity and met the requirement for rationality that in such circumstances the **level 4** and the **level 3** blueprints <u>should</u> change. At **level 4** he chose a basically factual but indeterminate thesis about the universe, which captured a fundamental tenet of physicalism. Then he met the further requirement for rationality that the adoption of a new metaphysics must also lead to the adoption of a related methodology. He developed the blueprint at **level 3** to produce something from which it was possible to draw precise, mathematical, testable laws and theories and the means of testing them.[58] The *'new method fraught with numerous and wonderful results'* [93] was, therefore, an early example of the powerful and restraining heuristics and methodology of a physicalist cosmology and so far extends improvement in explanatory knowledge and understanding as to encompass technological ingenuity.[59] This relationship may be inferred from his methodological

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works from the top downwards, from genuine cases of blueprint development, in order to be able to work back up again.

[58] **In the generalized aim-oriented empiricist framework the concept of 'metaphysical' is not sharply demarcated from the 'scientific'. Unlike the Popperian definition, it does not equal 'unfalsifiable' because the** level 3 **concepts have been falsified: for example, action by contact entails that there are only repulsive forces in the cosmos but this idea was refuted by the observation of attractive and cohesive forces.**

[59] **Examples of this are to be found in the tables of the Fourth Day of the** *Two New Sciences*, 284-288, **giving the altitudes and sublimities of parabolas and demonstrating to artillerymen the mathematical proof of why what they already knew to be the case <u>was</u> the case. There are many more examples in the First and Second Days, which are concerned not with motion but with** *'the resistance which all solids offer to fracture'*, 152. **There are pages of careful geometrical demonstration which range over such subjects as rope-making, ship-building, architecture, testing the specific gravity of mineral waters, the properties of levers and balances, and my particular favourite,** *'a simple but clever device, invented by a young kinsman of mine, for the purpose of descending from a window by means of a rope without lacerating the palms of his hands, as had happened to him shortly before and greatly to his discomfort.'*, 10.

practices particularly as they imply the belief that transcendent mathematical properties are the essence of physical reality, although somewhat imperfectly realised due to the perturbations introduced by the contingent world and the confusions perpetrated upon the senses. The methods utilized in the inclined plane and table top experiments consistently imply the basic **level 4** requirement that motions are everywhere governed by simple, mathematical laws. This can be seen in the motivating concept of just what, in natural phenomena, can be meaningfully evaluated and what elements can be identified as distorting. It entailed a procedure involving the experimental interference with natural phenomena and processes so that the unequivocal mathematical law (which a body would follow in the absence of the physical medium) might be isolated from the various, characterizing physical properties. **Level 4** can also be seen in the deduction of many mathematical proofs from the single, empirically established *odd numbers law*, in the geometric derivation of the characteristic parabola, in the calculations to produce the mathematical relationship between the magnitude of motion and the distance traversed between leaving the table and hitting the ground, and even in the calculations concerning the angles of inclination for firing a cannon. The **level 3** blueprint idea, in which mathematically-generated curves represent the trajectories of objects, is made manifest in the generation of a three-dimensional parabola which specifies a single, unique four-dimensional parabola. Of course the **level 3** idea is also commensurate with the aforementioned propositions, theorems, and problems following the *odd numbers law* insofar as the deliberations concerning inclined planes and the chords of circles require the fourth dimension of time to function along the spatial dimension measuring distance. In the investigations into sunspots the measurements undertaken, and the utilization of the *versed sines of equal arcs*, imply both the basic **level 4** and **level 3** requirements. By employing a new metaphysics and legitimately related methods to produce an exciting body of explanatory knowledge, Galileo had indeed taken the first step in the evolutionary process. He <u>thought</u>, correctly, that he had extended Euclidean geometry to include motion and had established a body of *'principles'* which were capable of further development in that *'subject, which is superior to any other in nature'*.[94] He felt vindicated, in the light of what it had uncovered about the nature of the universe itself, in his decision to alter the basic idea of what was <u>essential</u> about natural phenomena. In so doing he had (unconsciously) put in place the mechanism by which natural philosophy (in the light of what any reinterpretation concerning ontological categories might subsequently disclose about the nature of the universe itself) could continuously adjust its own nature. He had inaugurated the components necessary for the process of positive feedback that aim-oriented empiricism has designated so vital a feature of scientific rationality.

Methodolgy Implies Blueprint Even in Unexpected Cases

It has been argued that Galileo, who as a matter of historical fact didn't mathematize all his areas of investigation, in truth did not intend to impose mathematics upon every aspect of the physical world.[60] With reference to that early passage in the *Dialogue* which argues so carefully for the three-fold dimensions of space - and to which Simplicio objects, interpreting it as a claim for the supremacy of mathematics - James Maclachlan has argued,

> **You may want to make something of the fact that it is Sagredo who grants that a mathematical demonstration is not required in all cases, but Salviati enters no demurrer - no dogmatic assertion of mathematical primacy. [95]**

In answer to Simplicio's assertion that physical matters do not always require a mathematical description, Sagredo merely says,

> **Granted, where none is to be had; but when there is one at hand, why do you not wish to use it? [96]**

Now it has already been established in this chapter that the central **level 4** concepts bind together superficially different procedures. Methodological diversity exists because scientific methods are composed of strands to be woven according to the requirements of the blueprint and the circumstances of the individual investigation. What is common to all of those so far investigated is the extent to which **level 4 P** is implied. Galileo's particular **level 4** view, that nature has some simple, unified, mathematical structure insofar as simple mathematically-formulated laws govern the motion of bodies everywhere, can be interpreted as a special case of a more general view. This view designates the simple, unified mathematical structure as some kind of pattern of

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[60] It is further quite frequently argued, as a matter of historical fact, that those areas which do have a certain amount of physico-mathematics in common, such as astronomy and mechanics, have rather different methodologies.

mathematically precise physical law governing all natural processes. Now it is time to demonstrate that in the case of those phenomena where there was no motion, and which Galileo was unable to mathematize, he nevertheless remained committed to a metaphysical position commensurate with the more general view. As will now be illustrated, even Galileo's least successful methodological efforts, those which rely heavily on qualitative arguments, those which lack geometry, those which display a paucity of quantified experimental results, still imply the central tenet of the more general view of **level 4 P**. It is this that directs which of the mathematically-engendered methodological tools are appropriate in the specific case. It is particularly striking that Galileo always aimed for the best warp and woof he could achieve in every piece of science he turned his attention towards.

An example of one of these strands is to be found in Winifred Lovell Wisan's reconstruction of Galileo's discussion on William Gilbert and magnetism. She regards this as the true form of the Galilean methodology, which she defines as being methodologically modelled on mathematics, a superior form of reasoning in whose form the natural sciences were cast in order that they might enjoy a similar logical certainty.[61] [97] I, of course, do not accept that Galileo always assumed this mathematical model in order to try and make basic principles evident and that there was no other relationship between his mathematics and his method. Far from being the pure form of his method, unadulterated by attempts to mathematize phenomena (which merely served to demonstrate how hopeless it was to ever attain clear and evident first principles), I see Wisan's mathematically-modelled methodology as being but one strand in a more complex web of relationships and one that Galileo resorted to only when he was unable to put forward mathematically formulated and quantifiably testable hypotheses. In the case of Gilbert he argued that the discipline of geometry would have,

> **rendered him less rash about accepting as rigorous proofs those reasons which he puts forward as *verae causae* for the correct conclusions he himself has observed.**[98]

simply because he realized that the best he himself could offer was an argument that would apparently lead necessarily to Gilbert's own conclusions. For Galileo had no idea

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[61] Hence its strength lay in its logical structure. Wisan means that the structure which comprises clearly defined terms and explicit assumptions - and then moves through small and necessary steps to evident conclusions - was transferred wholesale into the practice of inferring causes from observed effects that was characteristic of the physical sciences.

how to come to grips with the problem which his own **level 4** required him to get a purchase on: he had no idea how to mathematize the mysterious force of magnetic attraction. Had he been able to put forward a mathematically formulated hypothesis, then he would have had something to test. As it is, the only argument that so much as flirts with the necessary quantitative approach is the one centred on the armatured loadstone.[62] Although it gives the respective weights of the loadstone itself and the material it can support, it is but a more precise version of what was already common knowledge, that a loadstone in an iron jacket is capable of sustaining more weight that a naked loadstone.[99] All that has been achieved is the negation of Gilbert's assertion that a loadstone could never hold as much as four times its own weight. The carefully quantified measurements have nothing to offer in the way of explanation and are not significant in the production of the correct conclusion that the force produced by the armatured loadstone does not, in addition to its ability to sustain greater weights of iron, also attract them over a greater distance. Galileo did begin to address the central problem when he decided that the difference in effect is due to the change in contact (because where the object originally touched loadstone it now touches iron) and offered the hypothesis that then the object touches the armatured loadstone there are a greater number of <u>points</u> <u>of</u> <u>contact</u>. He even identified these points of contact as areas of impurity within the pure substance of the polished stone, towards which iron filings were clearly attracted.[63] However, the argument is in no way quantified, not even to the extent of making a simple statement about proportionality linking the strength of the force of attraction to the number of points of contact: mathematics is nowhere evident in the establishment of the conclusion. Galileo was unable to fulfil the major terms of his blueprint: he could not formulate a testable mathematical hypothesis and he could not quantify his experimental findings.[64] Being convinced of the truth of Gilbert's conclusions, the best he could do was tighten up the arguments by a careful, stepwise

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[62] **Salviati declares that his favourite loadstone,** *'being not over six ounces in weight and sustaining no more than two ounces unarmatured, supports one hundred sixty ounces when so equipped. Thus it bears eighty times as much with an armature as without, and holds up to twenty-six times its own weight.'* **See Galileo,** *Dialogue*,, 405.

[63] **Consequently if the iron armature is explained as being a much denser body of this 'impure' substance then the obvious conclusion is that it offers more points of contact for the iron filings to adhere to, points of contact which in virtue of their greater number will sustain greater weights of iron.**

[64] **Working out a simple ratio between the area of the armature and the calculated 'impure' area of the loadstone would, again, have provided little in the way of explanation and actually working out the iron content of the loadstone would probably have been very tricky, bearing in mind that even in a polished specimen,** *'those (spots) which were scarcely visible were almost innumerable'*. **See Galileo,** *Dialogue*, 409.

procedure, a mode of reasoning that approached the logical certainty of that superior form of reasoning, the mathematical demonstration.[65]

The discussion of magnetism does not represent a total departure from the principle that all phenomena are capable of being explored mathematically. Careful study reveals that where Galileo did not attempt to mathematize he can still be seen to be holding open the door to the possibility of mathematization. Even where his attempts at revealing the mathematics underlying natural phenomena simply don't work, as with his Theory of the Tides, they neverthless remain just that.[66] He never abandoned the guiding concept that the essence of reality is mathematical, which is what renders it intelligible to us. Natural phenomena should never be explained in terms which might compromise this principle or make it difficult to realise in the future. The case of gravity, which was discussed in the early part of this chapter, provides another example of Galileo's characteristic refusal to commit himself whenever he did not have a firm, mathematical purchase on a phenomenon. Koyré has argued that Galileo, although impressed by Gilbert's concept that the force of gravity is something like magnetic attraction, had no idea how to mathematize so obscure an idea as that of an attractive force and so,

> **he was able, or knew how, to do without any concrete representation of the way in which motion and acceleration are produced.** [100]

The position which he took in this case has been variously attributed to his empiricism, his positivism, his anarchism, and his commitment to the question 'how?' rather than the question 'why?'. In truth, it is no more than another manifestation of his determination to leave natural phenomena open to the possibility of mathematical interpretation.

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[65] **Towards Peripatetic explanations couched in terms of sympathy and antipathy he was contemptuously dismissive, declaring that they contributed no more to the understanding of physical effects than did the artist to a painting whose title and layout he had provided but whose execution he had left to his students.**

[66] **Mach, in** *The Science of Mechanics* **argued that Galileo considered the motion of the earth relative to the sun but the motion of the water relative to the earth. In 1633, on behalf of a group of French natural philosophers, Jean-Jacques Bouchard wrote to Galileo concerning the mixing up of the two different frames of reference.** *'They draw attention to a difficulty raised by several members about the proposition you make that the tides are caused by the unevenness of the motion of different parts of the earth. They admit that these parts move with greater speed when they descend along the line of direction of the annual motion than when they move in the opposite direction. But this acceleration is only relative to the annual motion, relative to the body of the earth as well as to the water, the parts always move with the same speed. They say, therefore, that it is hard to understand how the parts of the earth, which always move in the same way relative to themselves and to the water, can impress varying motions to the water. They entreat me to try to obtain from you some solution to their difficulty.'* 'Letter to Galileo, 5 September 1633', **cited in** W. Shea, *Galileo's Intellectual Revolution'*, London: Macmillan, 1972.

Again, the conjunction in *The Assayer* of the celebrated primary/secondary distinction and the assertion that nature is written in the language of mathematics,

> **its characters are triangles, circles, and other geometrical figures, without which it is humanly impossible to understand a single word of it.** [101]

suggests another instantiation of the same commitment. It would perhaps be more accurate to say that as the only legal explanation of a given natural phenomenon is a mathematical one, then an explanation couched in any other terms is inadmissible. Aristotelian-type verbal posturing was to be avoided because it so often involved a pretence to knowledge: mathematical explanations, on the other hand, would produce genuine knowledge. So far as Galileo was concerned something caused unimpeded bodies to fall with accelerated motion, something which is constant in its effects and also universal. If he could not comprehend the cause of accelerated free-fall, nevertheless his refusal to allow that the incomprehensible must be false and his insistence that the only form of understanding which was possible was mathematical, combined to keep open the possibility that at some point in the future gravity would be captured in mathematical terms. Likewise, it might one day prove possible to mathematize magnetism and the mechanical philosophy. There was nothing to be gained in the meantime by muddying the water with unmathematizable substitutes.

The Father of Modern Science?

It is an old adage that Galileo is 'the father of modern science' and it used to take the form of the belief that Galileo's scientific method, which was held to be the method that ensured the success of the Scientific Revolution and the ensuing scientific enterprise, was therefore the paradigmatic model of scientific rationality. The definition of that method was extremely wide, extending from Mach's characterization to Koyré's, with Duhem's view somewhere in between.[67] These traditions persisted, in one form or

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[67] **Mach characterized Galileo as the genius who broke with authoritarian Scholastic philosophy and invented a new method which could objectively wring fundamental scientific truths from observation and experiment See Mach, E.,** *The Science of* Mechanics, La Salle, Ill. Open Court, 1960. **Koyré believed that Galileo's scientific work arose out of a mathematical Platonism which utterly rejected common-sense, empirical Aristotelianism. See Koyré, A.,** *Galileo Studies*, Sussex: Harvester Press, 1978. **Duhem's view was that Galileo's work merely marked a stage in a largely uninterrupted process, dating back to ancient times, in which new developments arose as a result of critically analysing already established scientific and metaphysical ideas. See Duhem, P.,** *The Aim and Structure of Physical Theory* (2nd. edn.), trans P. P. Weiner, New York: Atheneum, 1962.

another, until the early nineteen-eighties, as may be seen in the work of Wallace, Settle, Drake, Feyerabend, and Machlauchlin.[68] Notwithstanding, as early as 1956 Crombie noted that scholars,

> **looking for some historical precedent for some interpretation or reform of science, which they themselves are advocating, have all, however much they have differed from each other, been able to find in Galileo their heart's desire.** [102]

It began to be noted that there is evidence of rationalism, empiricism, Aristotelianism, Platonism, inductivism, hypothetico-deductivism, and even counter-inductivism in Galileo's work. There are also Archimedean influences, anarchical tendencies, rhetorical practices, and propagandist traditions: to pick out any one of these attributes or traits and disregard the importance, perhaps even the existence, of the others, was to risk falling into error.[69] Finocchiaro recognised this danger when he wrote,

> **One may find useful in this context the cliché that each interpretation is right in what it asserts and wrong in what it denies.** [103]

True to the trends analysed in Chapter One, doubts were increasingly cast on the possibility of maintaining Galileo's traditional, patriarchal status and concomitant links with present-day science. It began to be argued that Galileo worked in a time of transition. If he was indeed the formulator of a new intellectual framework then he could never quite be a part of that framework. Unlike Descartes, he was not a systematic thinker who began with a small number of unified basic principles, in whose terms all the

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experiment See **Mach, E.,** *The Science of Mechanics*, La Salle, Ill. Open Court, 1960. **Koyré believed that Galileo's scientific work arose out of a mathematical Platonism which utterly rejected common-sense, empirical Aristotelianism. See Koyré, A.,** *Galileo Studies*, Sussex: Harvester Press, 1978. **Duhem's view was that Galileo's work merely marked a stage in a largely uninterrupted process, dating back to ancient times, in which new developments arose as a result of critically analysing already established scientific and metaphysical ideas. See Duhem, P.,** *The Aim and Structure of Physical Theory* (2nd. edn.), trans P. P. Weiner, New York: Atheneum, 1962.

68 **Wallace has asserted that Galileo was really a progressive, somewhat eclectic, Scholastic, Thomistic Aristotelian, who reasoned** ***ex suppositione*****,. See Wallace, W.,** *Galileo and his Sources*, Princeton: Princeton University Press, 1984. **Historians of an empiricist persuasion have continued to flourish: Drake, S.,** 'The role of music in Galileo's experiments', *Scientific American*, 232, June 1975, 98-104; *Galileo Against the Philosophers*, Los Angeles, Zeitlin and Ver Brugge, 1976; *Galileo at Work*, Chicago: University of Chicago Press, 1978: **MacLachlan, J,** 'A Test of an 'imaginary' experiment of Galileo's', *Isis*, 64, 1973, 375-379; 'Drake Against the Philosophers', in Levere, T. H. and Shea, W. R. (eds.), *Nature, Experiment and the Sciences;* Kluwer Academic, 1990, 123-144. **have all produced work claiming that experimentation was centrally important in the foundation of modern science. An earlier extension of the empiricist school was hypothetico-deductivism, a reaction against** ***a priorism*** **which allowed an hypothesis to be 'prior' to any experimental testing without being the product of pure reason. Feyerabend, of course, advocated the theory of methodological anarchy, see Feyerabend, P.,** *Against Method*, London: New Left Books, 1975.

69 **Shapere is good at debunking the various schools of Galilean interpretation and highlighting the way in which history can be distorted by monothematic approaches. See Shapere, D.,** *Galileo: A Philosophical Study*, Chicago: University of Chicago Press, 1974.

facts to which they refer can, in principle, be understood. Few historians of science would argue with the view that Galileo brought about a profound change in the way that we view nature, or that he explicitly attempted to overthrow the Aristotelian world-view. The Aristotelian cosmology, which had existed quite comfortably with Ptolemaic astronomy could not long survive a world which was itself in motion.

> **Between 1605 and 1644, a series of books appeared in rapid succession in England, Italy, and France which laid waste the Aristotelian natural philosophy of the universities. The authors were Francis Bacon, Galileo, and René Descartes. The only conspicuous matter of agreement among them was that Aristotelian natural philosophy was not good science.** [104]

McLachlan has written that the Dialogue *'is an Anti-Aristotelian tract, beginning with the crack in the preface about "professed Peripatetics"* [105] and Hall is of the opinion that,

> **Galileo's opposition to the conventional natural philosophy of his day, with its exaggerated deference to Aristotle, as expressed by Orazio Grassi, Fortunio Liceti, Cesare Cremonini *et alii* is a principle theme.** [106]

Even those who subsume his application of mathematics to nature under the Aristotelian notion of the mixed sciences have also to admit the role of Archimedean mathematical treatises.[70] It is also largely agreed that he did not produce a philosophy of science, in the sense of formulating a theory of scientific method and knowledge. He left unexplored the problems of how to derive the first principles of a mathematical science and how to guarantee the articulation of its formal structure with the reality of nature. If his 'metaphysics' consisted of the doctrine that nature is fundamentally mathematical, a mathematical ontology reminiscent of the geometrical atomism of Plato's *Timaeus*, then it lacks the expected framework giving the relationships between God, nature, and the human mind. Perhaps he simply wanted reliable knowledge and invented mathematical physics as a means of getting it. We will shortly consider the ways in which historians of science still discuss the extent to which Galileo can be called a philosopher, a metaphysicist, and an epistemologist.

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[70] **See** Crombie, 'Sources', **and** A. Carugo and A. C. Crombie, 'The Jesuits and Galileo's Ideas of Science and Nature', *Annali dell'Instituto e museo di storia della scienza di Firenze, 8 (1983), 3-68; Wallace, Galileo and His Sources; The Heritage of the Collegio Romano in Galileo's Science*, Princeton: Princeton University Press, 1984, 91-6, 219-20, 338-49.

It is vanity to imagine that one can introduce a new philosophy by refuting one author or another, It is necessary first to teach reform of the human mind and render it capable of distinguishing truth from falsehood, which only God can do. [107]

The real issue is twofold. Firstly, it concerns the degree to which this transformation may be attributed to Galileo's holding a metaphysical position: the idea that his great achievement rested on the adoption of a Platonic or mathematical metaphysics has, in recent years, been largely overturned in favour of the notion that he was inventing a metaphysics-free mathematical physics. Secondly, it concerns the relationship between Galileo and modern science. Gradually the old certainties concerning the form and role of Galileo's method have disappeared but the idea that he was still, in some sense, the father of modern science persists.[108] When he is now judged to be 'modern' it is in less problematic ways. He is modern, for example, because of the fervent vitality with which he sought to apply Euclidean geometry to the problem of describing and understanding the world, thereby adding axioms to expand that science of properties and relations of magnitudes in space. We continue to describe and understand the cosmos in mathematical terms, still expanding and developing the mathematics in the process, <u>and we expect to</u>. Galileo himself prescribed our path for us,

There have been opened up to this vast and most excellent science (of motion), of which my work is merely the beginning, ways and means by which other minds more acute than mine will explore its remote corners. [109]

Another argument is concerned with the fact that up to the time of Galileo the theory of terrestrial motions had been dominated by the Aristotelian concept of cause. This led to a search for qualitative explanations as to why any given body should move in the way that it did. Such explanations would be couched in terms of entities like 'essential natures'. Galileo, although he retained various residual parts of Aristotelianism, some of which were positively helpful to him, stopped seeking causes and concentrated on describing actually observed motions.[71] Thus, he is held to be modern insofar as he

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[71] Aristotle actually possessed the concept of primordial motions but they belonged to different realms, the circular to the heavens (because spherical bodies have an intrinsic propensity to circular motion) and the rectilinear to the earth. Galileo realised that these two motions must apply to the whole cosmos and, retaining the Aristotelian concepts of 'up' and 'down' and 'proper place', produced rectilinear free fall and circular inertia. These <u>are</u> universal motions - near the beginning of the *Dialogue* Galileo argues that God, in the beginning, let the planets fall linearly (and therefore naturally) into their orbits (in which they occupy their proper place). He also retained the Aristotelian concept of natural and violent motions, again dependent upon a sense of 'proper place', and produces examples, of stones cast from slings and bullets shot from guns, which demonstrate the correct <u>rectilinear</u> definition of inertial motion. See Galileo. *Dialogue*, 190ff. However, the sling and the cannon force their projectiles into <u>unnatural violent motion</u>, which cannot

eschewed the 'why' question for the 'how' question, a strategy which to this day has proved itself to be extremely productive.[110] It is also asserted that in the process of creating a science of the actual motions of real bodies, Galileo produced some results that later became genuine constituents of the modern science of dynamics. These are the *law of free fall*, a restricted form of *the law of inertia*, the *parabolic motion of projectiles*, and the principle of Galilean invariance. Although he did not provide a framework for dynamics - and indeed could not as the concept of force had no place in his work - nevertheless,

> **What Galileo provided was assured quantitative results and proved techniques that Newton was able to incorporate into a quite new but only qualitative framework that Descartes supplied.** [111]

Certainly there is no mention of metaphysics and his methodology is not cited as evidence of his patriarchal status. However, as I have just given a lengthy exposition of the role of metaphysics in Galileo's work, also outlining its role in producing a definition of rationality that does, in a profound sense, identify Galileo as the 'father of the Scientific Revolution', I must now defend it in the light of recent scholarship.

To begin with the question of metaphysics. The view that Galileo was induced by a Platonic or Pythagorean metaphysical presupposition to develop a mathematical ontology, such that the material world is composed of bodies which are fundamentally mathematical, belongs to the tradition most associated with Burtt and Koyré.[72] This was by no means a unified school of thought, indeed Koyré argued that Burtt failed to distinguish between the Neoplatonism of the Florentine Academy, *'a mixture of mysticism, numerology, and magic'* and the Platonism of the mathematicians, *'which is a commitment to the role of mathematics in science'*.[112] Subsequent scholarship has divided the Platonist/Pythagorean influence into three main areas of influence. Firstly, the Neoplatonism which emphasises animism, natural magic, hermeticism, and the need to present knowledge about nature as a secret language - a code for the initiated.

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be regarded as an intrinsic property of a body because it has not been produced through speed acquired by a body falling before being deflected into everlasting uniform circular motion and thereby attaining its proper place. Fortunately this did not matter because with projectile motion the horizontal component is such a small segment of circular inertia that it is, to all intents and purposes, rectilinear.

72 **Cassirer and Whitehead have also been influential in this tradition.** See Cassirer, E., *Das Erkenntnisproblem in der Philosophie und Wissenschaft der neueren Zeit*, 2nd. edn., 2 vols., Berlin: Cassirer, 1911 **and** *The Individual and the Cosmos in Renaissance Philosophy*, trans. M. Domandi, New York: Harper, 1963; Whitehead, A. N., *Science and the Modern World*, New York: Macmillan, 1925.

Secondly, there is the Platonic doctrine that there is a relationship between God and nature such that God is a geometer and the universe has a geometrical structure reflecting the *archai* in His mind. Thirdly, there is the Platonic concept that Galileo's mathematics was metaphysical, that nature's parts and their order are fundamentally, essentially mathematical in character.

The first one, I believe, is not in contention. As long ago as 1936 Strong argued that mathematicians in the Renaissance and early modern period could be divided into two groups. The 'metaphysical' or 'metamathematical', in which he included Bartelomeo Zambarti, Giovanni Pico della Mirandola, D. Hendrion, and John Dee were the continuation of an *'archaic and scientifically divorced position'* that had originated with Nicomachus of Gerasa, Theon of Smyrna, and Proclus Diadochus, who had developed the discussion in Book VII of Plato's *Republic* by endorsing mathematics as a preparatory study for the contemplation of the Divine, or by presenting mathematical objects as being of mystical significance.[73] Into the second group, which he termed mathematic or scientific, Strong put Tartaglia, Cataneo, and Galileo, whom he saw as the inheritors of an autonomous tradition of mathematical science dating back to Euclid, Archimedes, and Ptolemy.[113] Much more recently, Hall has asserted,

> **But there is a great difference between apprenticeship to mathematical or experimental philosophy and apprenticeship to Hermeticism, magic and numerology. The former teaches intellectual and manual capacities; it develops reason in the mind, not mystical apprehensions in the soul. It does not breed familiarity with spiritual and demonic forces apart from Nature, but the forces that operate within Nature. Plato banned the innumerate from his Academy but he was far from requiring his pupils to subscribe to the beliefs and practises introduced by the neo-Platonists centuries later.** [114]

Most convincingly of all, Galileo himself admitted that one belief he shared with Kepler was that his account of the mathematical nature of natural phenomena was very different from the Neoplatonic views of Dee, Fludd, and Campanella.[115] That Campanella

[73] In this usage, mathematics was supposed to train the senses away from the ephemeral and corporeal and towards the intellection of the unchanging, eternal, and immaterial. The process was deemed to lead the scholar towards the contemplation of the Form of the Good or, in Christian Neoplatonic terms, to the contemplation of God.

accepted heliocentricity and Dee made use of it[74] does not alter the fact that Galileo saw himself as related rather to Archimedes and Copernicus.

The second area of possible Platonic or Neoplatonic influence concerns the relationship between God and the *archai* and the structure of natural phenomena, to be understood by a process of *a priori* reasoning. Again, there is general agreement that

> **it would be a mistake to fasten such an epistemology onto Galileo. Although Galileo left indefinite two critical points in his published writings - how we should derive the first principles of a mathematical science, and how the articulation of its formal structure with the reality of Nature is to be guaranteed - he certainly was not content with 'Thus because not otherwise', any more than Newton could be. ... Galileo was well aware that the parameters of thought have to be set by ordered, if artificial, explorations of the properties of natural things.** [116]

Hatfield has contrasted the Platonic doctrine of anamnesis, with it direct apprehension of eternal Forms, with Galileo's rather different practice of using *'common sense* [as the] *means for achieving those conclusions that are based upon recollection of specific instances* [which is further] *guided by geometrical reasoning and applied to images, figures, or diagrams.*[117] He argues that Galileo puts a distance between himself and the doctrine of reminiscence, that his bouts of apparently *a priori* reasoning are merely requests for the imagination to exercise itself on commonplace objects and processes, and that, in short, he gives no consistent, coherent indication of having a Platonic conception of knowledge of nature. [118] To be honest, there is little to be gained by arguing otherwise. There are plenty of Platonic aspects to Galileo's work, which can be either balanced out by allusions to Aristotelianism or perhaps interpreted as the use, for his own distinctive purposes, of an existing genre. Against his many references to Plato, his preference for the dialogue form of argumentation, his examples of *a priori* scientific method, even his use of an pseudo-Platonic cosmological myth concerning the formation of planetary orbits[75], one might cite his many uses of terms associated with Aristotle's method, his retention of certain Aristotelian concepts, and his copying of Aristotelian treatises.[76] Even his assertion that when it comes to mathematical propositions the

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[74] **Dee based his** *Ephemeris annis, 1557* **on the** *Prutenic Tabulae* **of Reinhold, which were drawn directly from those of Copernicus but enlarged and corrected.**
[75] **All of these lines of evidence are presented by Burtt,** *Metaphysical Foundations*, 64-72 **and Koyré,** *Galileo Studies*, 159, 166-7, 204-9.
[76] **These lines of evidence are presented by Crombie,** 'Sources' **and Carugo and Crombie,** 'The Jesuits and Galileo's Ideas'.

human intellect can equal the Divine in objective certainly can, on this reading, look like one more reference amongst a host of others, this time to the Platonic theory of knowledge. It is this regretable restriction of what can legitimately count as metaphysics that allows historians like MacLachlan to argue that there is no real evidence of a mathematical ontology in Galileo's work: on his reading, even the celebrated 'book of nature' quote fails as a major statement of deep metaphysical convictions because to poke fun at Orazio Grassi was its primary purpose.[77][119] MacLachlan further asserts that Galileo *'didn't ride mathematics roughshod across every field in nature'* and actually

> **defined the limitation he placed on the applicability of mathematics to nature, namely that it applied only to circumstances that are constant and unchanging.** [120]

Since Galileo also held that mathematics strictly applies to abstract perfections - and bearing in mind those occasions when he chose not to be dogmatic about mathematical primacy - MacLachlan concludes that Galileo was really,

> **searching for ways to apply abstract mathematics to concrete nature - he was inventing mathematical physics.** [121]

If Galileo was a metaphysicist, it is difficult to see how it could be in any sense which requires that the structure of natural phenomena depends upon intuiting Divine ideas.

The underlying problem for anyone trying to assimilate Galileo to an existing metaphysical tradition is that he does not slot easily into any orthodox conception of historically-located metaphysics or metaphysical forms of argumentation. Hatfield has argued that there are two ways in which metaphysics could have provided a basis for emergent science.

> **Metaphysics as the science of first principles might be thought to provide the first principles of the special sciences. Aristotle raised the question of whether first philosophy could provide the principles of the special sciences, such as physics or geometry, and seems to have decided in the negative. Yet his doctrine that the special sciences cannot establish their own first principles is suggestive of the idea that first philosophy might fill that role. During the Middle Ages and into the seventeenth century there was considerable discussion of this question among scholastic interpreters of Aristotle. Both conceptions of metaphysics had medieval and early modern adherents. In the seventeenth century,**

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[77] **Grassi was the chairman of the department of mathematics in the Collegio Romano who attacked Galileo's analysis of cometary paths in a work called** *The Astronomical Balance*.

Descartes and Bacon conceived metaphysics as providing (at least some of) the principles of physics. [122]

Hatfield argues that Galileo was not a metaphysicist in any sense of the term but rather a philosopher, mathematician, and scientific realist. He identifies historically-located metaphysics as comprising the most fundamental science, functioning as the repository, which transcended the physical, of the consciously-held first principles of the individual sciences.[78] In this role it was prior with respect to nature and aimed to give an account of ontological categories or, at the very least, *'functioned in relation to the new science* [insofar as] *the key to understanding our world lies in the transphysical'.* [123] Secondly, it comprised the associated methodology of *a priori* intellection, and thirdly, it was an account of knowledge and the knowing mind.[79] Granted such definitions, Galileo's theorizing is not metaphysical: he did not attempt a general account of mind and its relation to nature; there was no *a priori* insight into the plan of a geometrising God that could be extended into a rational, intellectualist account of knowledge in general. Even mathematics, his model of knowledge, was not presented as a codified set of precepts but taught through the examination of instances of its application. In asserting that the book of nature is written in the language of mathematics he was saying no more than that it is possible to know some of the 'properties' of bodies and these properties just happen to be the mathematical ones.[80] Although approaching Galileo from a rather different

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[78] **Descartes' claims that metaphysics can ground physics are too well known to need reiteration but it should not be forgotten that much less obvious candidates, like Bacon, also expressed this concept, even if only in random and untypical utterances. See** Jardine, L., *Francis Bacon: Discovery and the Art of Discourse*, Cambridge: Cambridge University Press, 1974, 100-1.

[79] **The burden of Hatfield's argument is that historians like Burtt and Koyré worked with a positivist definition of metaphysics as something presupposed. The key presupposition was to be found in the Neoplatonic assumption that the Creator is a geometer, who has fashioned nature after geometrical ideas. Koyré, for example, apportioned to Galileo a Platonic metaphysics, as a means of explaining his mathematical approach to nature. Hatfield concedes that Galileo may have made occasional metaphysical suppositions but to argue, for example, that his heliocentrism betrayed a metaphysical commitment to the presumed correctness of Euclidean geometry as a description of physical space, is to fail to use the word historically. Galileo can have an occasional metaphysical presupposition of this type and yet still not qualify as a metaphysician who argues metaphysically.**

[80] **Hatfield asserts that to argue for the foundational role of the Neoplatonic concept of God the geometer is to ignore that there was more than one role for mathematics within Platonic thought and also other, influential, non-Platonic traditions that approved of the application of mathematics to nature on a non-metaphysical basis. Within the Platonic canon mathematics could be used as an introductory study to meditations upon the divine, as in Book VII of the *Republic*, which utilized mathematics to focus the mind upon the contemplation of the immaterial form of the Good. In Christian Neoplatonic terms this became the contemplation of God. It could also be cast in the role of geometrical atomism, the creating demiurge of the *Timmaeus* being transformed into the Christian Neoplatonic ideal of God the geometer, who had given the universe a geometrical structure that reflects the *archai*, or Ideas in His mind. Amongst non-Platonic traditions of thought may be numbered Aristotle's belief that mathematics could be used in such 'mixed sciences' as optics, music, and astronomy. Scholastic Aristotelianism held that the object of mathematics (extension) could be considered in isolation from the sensible properties of material bodies, even though the**

perspective to that of MacLachlan, who cheerfully owns up to being a *'historian biographical'*, Hatfield nevertheless comes to a similar conclusion.

Notwithstanding, that is not an exhaustive list; it does not exclude Galileo from all the metaphysical possibilities raised by Hatfield's two conceptions.[81] In what follows I will disagree with arguments put forward to remove all metaphysical dimensions from Galileo's work, on the grounds that the aim-oriented empiricist framework offers an interpretation of Galileo which highlights the change in metaphysics brought about by the emerging science. On those selfsame grounds I will also bring out some points of contact between my position and that of the opposition. So firstly, I agree that that *'the rise of the new science itself altered the conception of metaphysics'* but argue that this goes well beyond the fairly orthodox view that *'each singularly great theoretician is a metaphysician for such persons change the categories in which we think'*.[124] This view limits the metaphysical to the single, unified theory for all of nature which provides a set of basic categories that shape philosophical and scientific thought for many decades.[82] Secondly, I shall maintain that an aim-oriented empiricist interpretation of Galileo is still commensurate with the idea that the key to understanding our world lies in the investigation of the transphysical. In that sense, at least, it can claim to be historically-located. Thirdly, there is the concept; that when making judgements about the metaphysical status of assertions or assumptions, *'one should be careful to see how it is functioning in the thought of the figure who subscribes to it'*. [125] This I retain but only on condition that 'thought' includes metaphysical assumptions present in the methodology of any given historical actor, which need not be couched in obviously metaphysical forms of argumentation. Historical actors of the status of Galileo may well

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two could not actually exist apart. There was also a tradition, descended from Islam, of technical astronomy grounded in mathematical and empirical practice, which involved developing and altering the mathematical models of Ptolemy's *Almagest*. All of these traditions were freely disseminated in the sixteenth and seventeenth centuries and some had been available throughout the Middle Ages. See Strong's account of mathematical and empirical practice and Hatfield's account of the practical and philosophical separation of metaphysics and mathematics. See Strong, E. W., *Procedures and Metaphysics*, Berkeley and Los Angeles: University of California Press, 1936; Hatfield, G., 'Metaphysics and the New Science', in D. Lindberg and R. S. Westman (eds.), *Reappraisals of the Scientific Revolution*, Cambridge: Cambridge University Press, 1990, 100.

[81] **Both of these conceptions of metaphysics were the subject of a fair amount of debate and discussion on the part of Scholastic Aristotelians throughout the Middle Ages and into the seventeenth century. Both Bacon and Descartes thought metaphysics capable of providing at least some of the principles of physics.**

[82] **Newton is the archetype of this sort of metaphysicist, for he fitted a unified mathematical structure to a wide and previously disunified set of phenomena and introduced basic categories such as mass, force, absolute space and time, which provided a framework for physics and metaphysics that lasted over two centuries. However, in aim-oriented empiricist terms he was continuing and extending the process of blueprint articulation and the development of a related methodology begun by Galileo.**

absorb background influences and partake of contemporary practices but they are also the generators of original and personal paths of influence.

Essence, Measurement, or Something Else?

Having discussed the type of metaphysicist that Galileo was not, bearing in mind the types of metaphysical assertion and argumentation that are generally considered to be historically-situated, that still leaves the possibility of a mathematical ontology. Is Galileo's belief that knowledge of nature equates with a knowledge of geometrical propositions a metaphysical position? One way of looking at the current state of Galilean Studies, or at least that portion of it which resides outside the environs of the social history of science, is to assert that it pivots upon a distinction between the metaphysics of substance, or essence, and the non-metaphysics of measurement, maintaining that getting a purchase on *'the quantifiable physical properties of things that numbers can represent'* marks the *'move from Pythagoreanism to mathematical physics'*. [126] The argument to establish the non-metaphysical nature of mathematics, considered as measurement, makes much of Galileo's failure to develop either a metaphysical account of mathematical certainty or an account of why mathematical constructions should fit nature. The position is further reinforced by his explicit rejection of the search for essences. The first two are seen as errors of omission:

> **It is true that Galileo offers no coherent body of doctrine on the relation of the human intellect to physical reality comparable to that of Descartes, or on the relation between the mind and God's geometrical plan for the universe comparable to that of Kepler. Despite his frequent appeals to geometrical demonstration as the standard of certainty, he develops no theory of geometrical demonstration. In fact he presents no theory of knowledge at all, in the sense that he gives no systematic account of the knowing activity of the mind or of the mind's relation to the objects known.** [127]

Furthermore, although

> **he needed to justify his expansion of the boundaries of mathematical science, or of the domain in which geometry is considered to 'fit' nature ... it was not his strategy to attempt an across-the-board- justification.** [128]

Even in the area of methodology, the one area where there is general agreement that Galileo made a decisive contribution, there is no systematic theory of method. However, when it comes to the question of Aristotelian essences as principles of qualitative change there is no need to base judgements upon what Galileo omitted to do. He explicitly contrasted his own aims with the search for active essences in a manner which, in view of the lack of an explicit theory of being, might indeed suggest that his true interest was restricted to those properties of bodies which do not amount to a definition of material substance but which can be handled geometrically. In the *Letters on Sunspots* he makes the contrast as follows:

> **In our speculating we either seek to penetrate the true and intrinsic essence (essenza) of natural substances or content ourselves with a knowledge of some of their properties (affezione).** [129]

The search for essences is

> **as impossible an undertaking with regard to the closest elemental substances as with the more remote celestial things. ... If what we wish to fix in our minds is the apprehension of some properties of things, then it seems to me that we need not despair of our ability to acquire this respecting distant bodies just as those close at hand.** [130]

However, although we may not know the *'true substance'* of sunspots yet,

> **still it does not follow that we cannot know some properties of them, such as their location, motion, shape, size, opacity, mutability, generation, and dissolution.** [131]

These are all changes in shape and size and they are not presented as a definition of material substance. It is as if, with this rejection, the last hope for pinning a mathematical ontology onto Galileo is gone.

Granted an aim-oriented empiricist interpretation, the position looks rather different. If it does not provide an orthodox metaphysical account of certainty, or of the fit between mathematics and nature, it does at least provide a metaphysical framework in which the fit that mathematical constructions have with natural phenomena, and the certainty with which human beings can come to comprehend this, receives a form of guarantee which also takes the sting out of the rejection of essences. It is Galileo's use of God within the framework that makes all this possible. I have argued that Galileo's physical universe is knowable by virtue of being written by God in the one language with which (God has ensured that) we are competent to gather certain knowledge of it. We may recall that according to aim-oriented empiricism the basic **level 5** assumption is that the universe is comprehensible in some way or other, one **level 4** possibility being that it is physicalistically comprehensible. Galileo went so far as to utilize God at something like **level 5** in order to ensure that by transforming natural phenomena into geometrical figures he was also ensuring that the mathematical character of nature was both certain and within the grasp of humans. The end-result was that the mathematization of nature turned into a prerequisite for obtaining objectively certain knowledge of nature. In the *Letter to the Grand Duchess Christina* Galileo speaks of physical nature, *'the observant executrix of God's commands'*, as being

> **inexorable and immutable; she never transgresses the laws imposed upon her, or cares a whit whether her abstruse reasons and methods of operation are understandable to men.** [132]

Later he writes,

> **but it is not in the power of any created being to make things true or false, for this belongs to their own nature and to the fact.** [133]

God, on the other hand, the Creator of all natures, including human natures, does care and to that end has given us,

> **senses, reason and intellect [and] would not require us to deny sense or reason in physical matters which are set before our eyes and minds by direct experience or necessary demonstration.** [134]

The *'use of reason* [is a] *divine gift of Providence'*.[135] The Divine mind can contemplate infinitely many propositions in geometry and arithmetic simultaneously and in their entirety. Humans equal the objective certainty of the Divine with each mathematical proposition they comprehend and also when they comprehend aspects of the mathematics underlying *'the observant executrix of God's commands'*.

It is necessary to be quite clear about the role of metaphysics when viewed from an aim-oriented empiricist perspective if one is ever to deal with the opposing arguments anchored in the necessity for evidence of historically-situated metaphysics and forms of argumentation. Galileo, after all, attributes geometry to the physical world in a variety of ways which have been interpreted as different examples of calculating the fit between mathematics and nature. He calculates the geometry governing some of its motions, such as free-fall, projectile motion, the motion of sunspots and other celestial bodies like the moon and comets. He produced numerous accounts, in a wide range of publications and unpublished works of various descriptions, of the geometrical character of matter. Most famously of all he affirmed, on at least two occasions, that *'the book of nature is written in the language of mathematics'*.[83] What is the status of this celebrated assertion if all the examples are interpreted to suggest that the only things in the universe written in the language of mathematics are those of its properties that constitute the objects of scientific knowledge? Notwithstanding, an aim-oriented empiricist perspective, which illuminates the considerable degree of unity in Galileo's work at the **levels** concerned with metaphysical assumptions, also demonstrates the pitfalls attendant upon interpretations which concentrate on the methodology and make no allowances for the metaphysical assertions therein implied. Just how did Galileo justify a geometrical approach to nature? I have already argued that a favourite strategy of his was to abstract irregularities from concrete examples. That is what he did with motion, making allowances for friction and air resistance in the pages of proofs in the Third and Fourth Days of *Two New Sciences.* He demonstrated the existence of such phenomena by observing bodies as they fell through different fluid media and calculating that they acquire speeds inversely proportional to the resistance of those media. This lead to the conclusion that in the absence of <u>any</u> sort of medium,

> **a heavy body has an inherent tendency to move with a constantly and uniformly accelerated motion towards the common centre of gravity ... so that during equal intervals of time it receives equal intervals of momentum and velocity.** [136]

Earlier in that same book he defends the rigour of Archimedes' argument that the parts of the horizontal arm of a balance are equidistant from the centre of gravity, defined as a point at the centre of the earth, even whilst conceding that in actual instances it will be necessary to discount small deviations.

> **We must find and demonstrate conclusions abstracted from the impediments, in order to make use of them in practice under those limitations that experience will teach us.** [137]

This recalls a much cited passage from the *Dialogue*, in which the mathematical philosopher is likened to the market trader who must subtract the packaging from the weight of the goods sold:

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[83] **This fundamental belief was expressed in both** *The Assayer* **and in a** 'Letter to Liceti', **quoted in S. Drake,** *Galileo at Work*, 412.

when he wants to recognise in the concrete the effects which he has proved in the abstract, he must deduct the material hindrances. ... the error lies not in the abstractness or concreteness, not in geometry or physics, but in a calculator who does not know how to make a true accounting. [138]

In the case of the circle and plane argument in the *Dialogue*, which was quoted earlier, I wish to argue that despite the explicit, localised point that Galileo was making, the most important lesson to be learned from the argument that all shapes are geometrical is still that the best practice is always to abstract the imperfections.[84] The big mistake would be to take it as an example of a different attitude towards the fit between geometry and nature, to interpret it as a discontinuity at the methodological level postulated according to the specifics of the individual case. An even bigger mistake would be to take it as evidence that there can therefore be no comprehensive account of why nature's parts and their order are fundamentally mathematical. The example of the two very irregular, jagged-edged bodies, with which Galileo makes his point, would pose great problems if one really had to formulate their correct interpretative entities. However, the only irregular example for which he gives a geometrical prediction is that of the slightly non-spherical (because physically real) globe and the less than planar surface. In such cases, he argues, geometry predicts that the former will touch the latter, *'over a part of its surface'*, rather than in a single point.[139] In the pages of the *Two New Sciences* Galileo continues to work with the simplest geometrical shapes in order to make his geometrical predictions.

Prisms and cylinders which differ both in length and thickness offer resistance to fracture [i.e., can support at their end loads] which are directly proportional to the cubes of the diameters of their bases and inversely proportional to their lengths. [140]

He continues to base his arguments around the simplest ideal geometrical propositions, developing them, where necessary, to identify and then include physical characteristics which, although not part of the geometrical ideal, can be quantified and therefore usefully included in the calculations governing any given instances.

Let us agree to distinguish between these two points of view; when we consider an instrument in the abstract, i.e., apart from the weight of its own material, we shall speak of 'taking it in an absolute sense'; but if we fill one of these simple and absolute figures

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[84] The primary point of the example is to demonstrate that in so far as matter has a determinate shape then, no matter how irregular it is, that shape is geometrical.

> **with matter and thus give it weight, we shall refer to such a material figure as a 'moment', or 'compound force'.** [141]

He takes *'the properties that belong to figures that are merely geometrical and non-material'* and in an example of a lever on a fulcrum being used to move a heavy stone he predicts a ratio involving the force at the extreme end of the lever, B, the resistance offered by the stone at the other end of the lever, D, and the respective lengths of the lever on either side of the fulcrum, AC and CB. He then writes,

> **But if we take into account the weight of the lever itself ... it is manifest that when this weight has been added to the force at B, the ratio will be changed and must therefore be expressed in different terms.** [142]

The same is true of the stone. The *'force applied at the extremity of the lever at B'* and the force exerted by the stone at the other end of A, *'is always less than the total weight of the stone [or lever] and varies with its shape and elevation'.* [143]

There are various lessons to be learned from all of this. One is that there is a considerable degree of unity at the methodological level of the fit between mathematics and nature. Sometimes this becomes slightly obscured when Galileo is seeking to make a particular point, as with the circle and plane argument, but abstracting the impediments remains his favoured strategy across the range of his published works. An intriguing instance, in which Salviati apparently rejects Sagredo's appeal to 'material hindrances' occurs right at the beginning of Day One of the Two New Sciences. However, closer inspection indicates that he is simply warning against the invocation of material hindrances at the <u>expense</u> of the underlying geometrical reality, which must be prior.

> **the larger the machine, the greater its weakness ... it is clear we are no less able to treat this constant and invariable property in a rigid manner than if it belonged to simple and pure mathematics ... For we can demonstrate by geometry that the large machine is not proportionately stronger than the small.** [144]

The truth of the matter is,

> **imperfections in the material, even those which are great enough to invalidate the clearest mathematical proof, are not sufficient to explain the deviations observed.** [145]

Another lesson is that this unity in Galileo's justification of his geometric approach to nature is a reflection of a higher unity at metaphysical level. The 'calculator' in Galileo's homely example offers a reason why: he knows in advance what constitutes 'packaging', or material hindrances, and what constitutes the 'goods', or the pure form. The same must be true of the philosopher with a mathematical ontology who, as I argued earlier, identifies the various physical properties which characterize a given concrete example, the goods and the packaging, because he already knows the goods in the form of a simple, unequivocal mathematical law or geometrical prediction. This has to be done before any that are distorting, or simply too complicated, can be removed: it has to be done before the decision is made to include, in a given instance, any mathematizable physical characteristic that will legitimately alter the nature of the geometrical prediction. Perhaps what to discount and what to recognise as a disconfirmation only emerges at the empirical level but the idea that a true accounting must be made, and the form that it must take, are already known. The unavoidable conclusion is that Galileo had a prior metaphysical preconception of just what can be meaningfully evaluated which, in aim-oriented empiricist terms, means identifying the underlying **level 4 P** and **level 3** concepts. These, in turn, offer a justification for mathematical certainty and for the question of why mathematical constructions should fit nature which, if not metaphysical in any of the traditionally defined terms, nevertheless involve the metaphysical assumptions that the universe is both comprehensible and comprehensible in a particular way. If the account of mathematical certainty does not take the form of the intellectual apprehension of Platonic *archa*i, if we do not actually intuit straight from the Divine mind, we do at least have access to a privileged area of knowledge which, by God's authority, coincides with Divine ideas. If the fit between mathematics and nature isn't presented in terms of the Platonic separateness of mathematical entities from the sensible world, at least the mathematical realm is not reproduced exactly in the physical world and in that sense can be called 'transphysical'. God exists at **level 5**, or something like it, Creator of all things, including mathematics, nature, and human beings with the gift of understanding the necessity of mathematical proofs. God's existence at **level 5** also ensures that a necessary condition, at **level 4**, of obtaining about the universe the only strong, provable knowledge that is possible, is that the universe has an inherently mathematical structure. Galileo was very careful to ensure the mathematical character of nature.

By using aim-oriented empiricism as an interpretative framework, an option for exegesis that postulates how science must be if it is to be ideally rational and progressive, several developments have become possible. The idea that Galileo was consciously trying to construct a body of certain, provable knowledge is fleshed out by the demonstration that although it is in violation of strict aim-oriented empiricist requirements, which would predict the development of progressive but conjectural knowledge, it is still capable of being illuminated by the aim-oriented empiricist process. The widespread acceptance that Galileo provided no metaphysical framework in which to base his many examples of the links between geometry and the natural world has been shown to take insufficient notice of the changes that were wrought upon conceptions of metaphysics by the rise of the new science. However, this is not to imply a complete discontinuity. At least one requirement of the orthodox definition of historically-located metaphysics has been upheld: the key to understanding the physical still lies in the transphysical, in the ideal abstract law or figure. The certainty of mathematics and the certainty that it is the basic language of nature is given metaphysical support, if it is interpreted in terms of an aim-oriented empiricist framework. Its just that the basis is not a purely rational intuition but an assertion, made in the belief that better explanations exist to be found, about the comprehensibility of the universe.

Galileo's work, therefore, is not lacking in unifying metaphysical principles: the 'book of nature' quotation in *The Assayer*, later reiterated in a letter to Liceti, is a metaphysical principle.

> **I truly believe the book of philosophy to be that which stands perpetually open before our eyes, though since it is written in characters different from our own alphabet it cannot be read by everyone; and the characters of such a book are triangles, squares, circles, spheres, cones, pyramids, and other mathematical figures, most apt for such reading.** [146]

Why, therefore, is this so little recognised, nowadays? MacLachlan does not see this as *'a major statement of Galileo's deepest philosophical convictions'* [147] but - and particularly in its earlier form in *The Assayer* - as an encouragement to natural philosophers to become numerate, spiced up by the ridicule it directs towards Orazio Grassi. Drake sees it as an expression of Galileo's conception of scientific reasoning, as a *'language necessary for understanding nature, not as an end in itself'*. [148] Hall, who used to define it as an expression of a deeply-felt metaphysical position,[149] has more recently interpreted it as Galileo's attempt to make *'a choice between perceptions of reality accessible to him [rather than] creating a new one of his own'*. He accepts that there may well have been a difference in the metaphysical positions of Aristotelian natural philosophers and the emerging discipline of mathematical philosophy but does not believe that all who partook of this latter movement had also to partake of the metaphysical assumptions as well.[150] The *'perception of reality'* which Hall attributes to Galileo,

> **is that of which Apollonius, Archimedes, Ptolemy, Copernicus and many others had sketched fragmentary paragraphs in draft.** [151]

Hatfield interprets it as a statement that,

> **philosophical knowledge of the universe is exclusively geometrical, with the implication that such knowledge extends only to the geometrical properties of natural things ... It is not the universe that is mathematical but those of its properties that constitute the objects of scientific knowledge.** [152]

However, once it becomes clear where the crux of Hatfield's argument lies, the advantages of the aim-oriented empiricist framework become apparent..

> **There is, however, a world of difference between holding with the *Timaeus* that material bodies are constituted by geometrical figures, or with Descartes that the essence of matter is extension, and maintaining with Galileo that material objects possess a determinate size and shape and that the scientific knowledge we possess of such bodies is of those properties. The former amounts to saying what bodies are; the latter amounts to saying which of their properties can be the object of scientific knowledge.** [153]

All of the above hinges on the central theme, already identified, that when mathematics is applied to the physical world it becomes measurement and is therefore non-metaphysical. In the absence of traditional forms of metaphysical belief and argument, in the presence of the rejection of essences and the sheer amount of empirical and mathematical justification involved in Galileo's work, the favoured interpretation is that the objects of natural philosophy have determinate geometrical properties, whose justification lies wholly in the realm of experiment and calculation. Underpinning this is the erroneous belief which is at the heart of the conclusion that Galileo was a scientific realist. It is the belief that metaphysical assertions have no content and, conversely, assertions which do have content cannot be considered metaphysical.

> **it might be argued that the very claim to determine the configuration of the solar system involves the metaphysical presupposition of an absolute spatial framework. One might classify this supposition as metaphysical on the grounds that it makes no *observational***

sense to assign the sun a location at the center of the solar system prior to specifying the inertial frame of the observer, and that putting the sun in the middle has no *physical* content prior to the specification of the sun as the center of gravity of the solar system. Since, before Newton, neither of these specifications could be made, any attempt to give content to heliocentrism may seem, of necessity, to have been pure metaphysics. [154]

Hall asserts that Galileo can only be seen as a metaphysicist in so far as he was a pioneer of the mechanical philosophy of nature. A change in metaphysics means a change in the idea *'of what the real nature of the universe may be imagined to be'*.[155] However, once a concept has physical content, once testable theories have been drawn from it, once it no longer lies wholly in the realm of what can be 'imagined', all of the scholars under consideration appear to divorce it completely from any metaphysical aspects it may have been thought to have had up to that time. Galileo's manifest achievements in establishing a mathematical view of nature, demonstrating its power in natural philosophy and going some way towards establishing that axiomatic mathematical science discloses a true account of nature, is simply interpreted as an early stage in the development of mathematical physics. His pronouncements on matter in motion cannot be subsumed under the axiomatic mathematical science, they cannot be cast into a testable form, and so they can, perhaps, be described as 'metaphysics'[85].

Aim-oriented empiricism, on the other hand, sees the scientific enterprise as an increasingly attenuated hierarchy of assertions in which the physical slides into the metaphysical without immediately losing the justificational role of the empirical. Given the evidence and the comprehensibility thesis **C**, physicalism is the best **level 4** conjecture to adopt: this is primarily because it does far better justice to the immense empirical success of physics than any rival **level 4** conjecture which is also a version of **C**. Given the evidence and **level 4 P**, the best **level 3** theory must accord with **P** and be empirically adequate with respect to the accepted body of **level 2** theory. When we drew the metaphysical assumption out of Galileo's work on motion we discovered that its most likely form was that simple, mathematically formulated laws govern the motion of bodies everywhere, an assumption that proved to be empirically adequate with respect to the **level 2** theories which he formulated concerning motion and simultaneously consistent

[85] **See, for example,** Hall, A. R., 'Was Galileo a Metaphysicist?', 112 **and** Hatfield, G., 'Metaphysics and the New Science', 133, in which he concedes that if Galileo was, in this instance, a metaphysicist, he was but a *'spotty and uneven one'*.

with the **level 4** requirement that nature has some definite, simple, mathematical structure. These are contentful assertions: the **level 3** one is certainly revisable in the light of empirical considerations and the **level 4** one is too, in principle, although given the huge success of the scientific enterprise built on this assumption it seems a remote possibility. The lower and more contentful the assertion, the more methodologically and heuristically fruitful it is and the more likely it is to need revision. The higher and less contentful the assertion, the less fruitful it is and the less likely to ever need revision. It is not the case that, for example, the law of parabolic motion loses all contact with metaphysics simply because it has reached the stage of having quantifiable, physical content. It is rather that the law of parabolic motion acts in conformity with, and thereby provides confirmation for, **levels 3** and **4**. Thus it can be seen that although Galileo retained the notion that the means of comprehending the physical were to be discovered in the study of the transphysical, he did not draw a sharp demarcation between the two states.

It has been amply demonstrated that Galileo did hold the former notion. His methodology bears eloquent testimony to this through its procedures. Methods which rest on the assumption that the resulting, approximate mathematical law was the product of the various physical properties which characterized the concrete example in question imply that the transcendent, physically unrealisable, unequivocal form of the law must be the correct one because it functions to disconnect the distorting elements in the physical situation. The latter notion can also be ascribed to Galileo. The aim-oriented empiricist definition of a 'metaphysical' thesis at **Levels 4** and **3** is a general thesis about the world which, although factual, does not encompass the precision, and concomitant empirical prediction-making facility, of a physical law or theory. However, it does have the characteristic of being open to the formulation of infinitely many more precise theses with this prediction-making quality. Thus a vague, metaphysical doctrine becomes testable by metamorphosing into a scientific theory as it is given greater precision. Galileo formulated a metaphysical thesis at **level 4** which was factual, and further capable of being rendered sufficiently explicit for the production of testable predictions concerning constant change. He then fulfilled the requirement of **level 4** by producing a more precise and testable version, at **level 3**, concerning the curves produced by two types of motion in combination. Although he did not employ any orthodox metaphysical arguments to demonstrate why it would follow that the book of nature is written in the language of mathematics, in practice he made no sharp demarcation between untestable metaphysics and that which is testable. It is this which, in the place of more obviously historically-located metaphysical argumentation, functions to define the relationship between the transcendent and the physical. He moved, as we have seen, from the transcendent, physically unattainable form of the law to the more precise, physically realisable form in such a way as to demonstrate the legitimate growth of a metaphysical thesis into a testable theory. It is in this sense that both aim-oriented empiricism and the metaphysics of the early seventeenth century begin to overlap. The aim-oriented empiricist idea that it is legitimate to make untestable claims, with the potential to be developed into more precise forms, about the ultimate configuration of the universe found early expression in the work of Galileo.

Galileo did not need to pursue historically-located forms of metaphysical assertion and metaphysical argumentation in order to be engaging in metaphysics. He did not need to pursue essences. His refusal to take essences seriously amounts to no more than a refusal to consider the subject of material substance in the manner of more orthodox metaphysicists like Aristotle and, later, Descartes. It amounts to a disavowal of a thoroughly tiresome and unproductive form of argument. He railed against,

> **the 'sympathy', antipathy', 'occult properties', 'influences' and other terms employed by the philosophers as a cloak for the correct reply which would be, 'I do not know'. That reply is as much more tolerable than the others as candid honesty is more beautiful than deceitful duplicity. [156]**

Words, he felt, did not have the special powers with which philosophers credited them:

> **If their opinions and their voices have the power to call into existence the things they name, then I beg them to do me the favour of naming a lot of old hardware I have about my house 'gold'. [157]**

Away with all these foolish notions that there is no metaphysical dimension to Galileo's work!

Matter in Motion

So the aim-oriented empiricist framework allows us to examine the sort of evidence which has quite commonly been presented in favour of Galileo's being a mathematical physicist who eschewed metaphysics, and to draw out the metaphysical assumptions underpinning it. It demonstrates that Galileo was not a metaphysicist in most of the generally-accepted definitions of historically-located metaphysics (apart from a carefully redefined role for the transphysical) but rather in the sense that he thought about the way in which the world is comprehensible, to the extent of utilizing the authority of God to ensure that it was comprehensible in a particular way. He can be interpreted as having developed a central concept of **level 4 P** and, if we recall that he worked to develop an existing blueprint of astronomical motion to include terrestrial motion, from which he was able to formulate a restricted number of spectacularly successful **level 2** theories, we have a good example of what an aim-oriented empiricist **level 3** blueprint with its associated methodology and **level 2** theories looks like. All of which brings us to the question of Galileo's contribution to the mechanical theory which, during his lifetime, was beginning to become independent from the ideas of Greek atomism. For those who maintain that Galileo was a mathematical physicist, this area of his work causes the most problems. They must either categorise Galileo as a mathematical physicist who rejected metaphysics in all his most important work, and yet still enjoyed a bit of metaphysical speculation on the side, or they must deny that his mechanical theory is in any way metaphysical. Hatfield takes this latter course, interpreting the celebrated primary/secondary distinction in *The Assayer* as, *'a sketch of a theory of the senses along mechanistic lines'* rather than *'a metaphysical argument for a metaphysical distinction'*, and therefore *'no more metaphysical than Newton's theory of refrangible rays'*.[158] Notwithstanding, as Galileo introduced the subject, it certainly looks like an acceptable instantiation of historically-located metaphysics, if only because of the apparent evidence of intuition.

> **I do not believe that for exciting in us tastes, odors, and sounds there are required in external bodies anything but sizes, shapes, number, and slow or fast movement... Therefore I say that upon conceiving a material or corporeal substance I immediately feel the need to conceive simultaneously that it is bounded and has this or that shape [size, position, and number].** [159]

The primary quality of shape includes the qualities of solidity and extension[86] and the whole concept introduces the idea that secondary qualities, odours, tastes, and sounds are sensations caused in us by the primary qualities of shape, number and motion in the ultimate indivisible constituents of bodies:

> **but that it must be white or red, bitter or sweet, noisy or silent, of sweet or foul odor my mind feels no compulsion to understand as necessary accompaniments [of a material or corporeal substance]; indeed, without the senses to guide us, reason or imagination would perhaps never arrive at such qualities.** [160]

However, that the primary/secondary distinction has the appearance of being historically-located is of no real help to an aim-oriented empiricist interpretation. What is necessary is an examination of the degree to which Galileo's forays into the subvisible realm are metaphysical in terms of blueprint progress, or in terms of exemplifying the basic version of **level 4 P** and then deriving from it the best **level 3** version and further **level 2** testable theories. Galileo's pronouncements on the mechanical theory do not fare as well as his ideas about motion. It was earlier mentioned that the ancient Greeks failed to develop good versions of **level 4 P,** defined as the ultimate invariant which is somehow responsible for all variety and change, in spite of having originated the concept. Hence they were never really in a position to produce law-like statements or universal generalizations. Galileo never succeeded in this either: his great successes came from developing a different metaphysical blueprint. An aim-oriented empiricist framework bids us concentrate on how corpuscles or atoms in seventeenth-century ideas about the mechanical theory, interpreted as a **level 3** formulation, might exemplify the basic idea of **level 4 P.** Even in its most basic form this must at least explicate the idea that the physical properties of fundamental physical entities are necessarily mathematical properties which also determine the way these entities interact. The corpuscular theory that Newton inherited, for example, can be interpreted as having consisted of infinitely rigid corpuscles with inertial mass which interacted by contact. Granted this blueprint the task of physical theory is to give precise specifications concerning the size, shape, etc. of the different basic corpuscles and explain in detail how all natural phenomena emerge as a result of their combinations in groups or in relative motion. Once these

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[86] **John Locke drew out the distinction in his exposition of the primary/secondary distinction in his** *Essay Concerning Human Understanding*, Book II, ch. xxiii, sec. 11.

kinds of **level 2** theories are formulated the task is then to assess their relative simplicity. The criteria for doing which, bearing in mind that **level 4 P** requires unity, can take the form of choosing the theory postulating the fewest possible number of different basic corpuscles, or it could encompass the theory postulating the unity of the individual corpuscle, perhaps that it is eternal and unchanging. This might further entail the homogeneity of the corpuscle and its geometrical symmetry.[87] Finally comes the task of trying to put the theories into a testable form. It is with these guiding principles in mind that we must look to Galileo's other pronouncements on atomism and the mechanical theory.

In *The Assayer* Galileo produced an emissive theory of heat:[88]

> **Those materials which produce heat in us and make us feel warmth which are known by the general name of fire, would then be a multitude of minute particles having certain shapes and moving with certain velocities ... Since the presence of fire-corpuscles alone does not suffice to excite heat, but their motion is needed also, it seems to me that one may very reasonably say that motion is the cause of heat.** [161]

Years later, in the First Day of the *Two New Sciences*, following a discussion of the concept that *'substances become fluid in virtue of being resolved into their infinitely small, indivisible components'*,[162] we find him speculating that gold and silver,

> **do not become fluids until the finest particles** [gl'indivisibili] **of fire or of the rays of the sun dissolve them, as I think, into their ultimate, indivisible, and infinitely small components.** [163]

In other words, he postulates that metals are resolved into their most basic constituents by other infinitely small indivisible components, *'accompanied by motion, and that the most rapid'*.[164] He compares and contrasts material substances which, although ground into dust, has not been resolved into its ultimate and indivisible components, with a material substance which has been so resolved. Of the former he says that its particles remain *'finite in size, possess shape and capability of being counted'* and that,

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[87] Homogeneity requires that each internal point of the particle must be identical and as symmetrical entities they can only be changed, and continue to possess symmetry, if they change as a whole.

[88] Emissive, or 'stream of particles' theories were another form of corpuscular action, as was vibratory action. In Galileo's theory fire corpuscles were ejected by the process of combustion and on penetrating human skin caused the sensation of heat.

once heaped up they remain in a heap; and if an excavation be made within limits the cavity will remain and the surrounding particles will not rush in to fill it; if shaken the particles come to rest immediately after the external disturbing agent is removed. [165]

His conclusion is:

Seeing that water has less firmness [consistenza] **than the finest of powder, in fact no consistence whatever, we may, it seem to me, very reasonably conclude that the smallest particles into which it can be resolved are quite different from finite and divisible particles; indeed the only difference I am able to discover is that the former are indivisible. The exquisite transparency of water also favors this view, for the most transparent crystal, when broken and ground and reduced to powder loses its transparency; the finer the grinding the greater the loss, but in the case of water where the attrition is of the highest degree we have extreme transparency.** [166]

What kind of blueprint, or metaphysical assertion does this amount to? To begin with one would not expect to find in Galileo's work the concept of inertial mass that only emerged when Newton finally distinguished mass from weight with his Second Law.[89] Galileo, however, approaches the basic **level 4 P** requirement in so far as he asserts that ultimate indivisible particles, no less than divisible ones, have physical properties that are necessarily mathematical: his ideas about the physical properties of corpuscles exemplify the basic idea of a simple, fundamental, mathematical structure. In as much as they are ultimate and indivisible, the corpuscles also have unity. However he never managed to explain how the physical properties determine the way in which the fundamental physical entities interact and so his corpuscular theory did not accord fully with **level 4 P**. It is possible to derive from what he has to say about corpuscles a couple of theories. From the properties of liquids it is possible to propose the theory that, in the liquid state, matter is resolved into its smallest indivisible particles. From the fire corpuscles it may be inferred that, as they are capable of resolving precious metals into their ultimate indivisible particles, they must be smaller and in more rapid motion. Nothing firm is said about such elementary moves as the basic number of corpuscles, or whether some have more complicated shapes than others. There is little that would allow one to draw out decent **level 2** theories that could be ordered with respect to symmetry, or cast into

[89] **Newton introduced the concept of force into rational mechanics with his Second Law. F=Δmv approaches F=ma as a limit when Δt approaches zero. The definition of mass, clearly distinguished from weight for the first time, was involved with the definition of force. See** Westfall, R. S., *The Construction of Modern Science*, Cambridge: Cambridge University Press, 1977, 152.

testable form. Galileo, nonetheless, did develop his theory of fire corpuscles, at least in so far as he began to investigate the rapidity of their motions as manifested in the speed of light. From the action of the sun in melting metals, when concentrated by a burning glass, Salviati concludes that the particles in solar rays are in very rapid motion.[167] He then recounts an ideal experimental set-up, although one which is in principle quite possible, to measure whether this speed was instantaneous or *'extraordinarily rapid'*. Whilst inclining to the latter opinion, Galileo has him confess he had never been able to recreate the set-up in reality, having never found sufficiently favourable geographical circumstances.[90] Galileo, we may conclude, knew that to derive **level 2** theories from the corpuscular blueprint was the right thing to do but could not effect the developments to the **level 3** blueprint that were necessary before this could become possible. In the aim-oriented empiricist framework, Galileo's conjectures about corpuscular theory appear as a genuine metaphysical assertion which neither fully exemplified the basic idea of physicalism, nor fulfilled the requirement of being empirically adequate with respect to a body of legitimately-derived **level 2** theories.

Unity at the Metaphysical Level

There is, in Galileo's work, a great, underlying unity at **level 4**. The orbit of Jupiter's moons, accelerated free-fall, corpuscular theory, even the true configuration of the entire solar system, are all subsumed under the striking metaphor concerning the book of nature from which aim-oriented empiricism draws out a **level 4 P** metaphysical assertion to the effect that simple, mathematically formulated laws govern all natural processes and events. Even areas of his work which remain unmathematized, such as magnetism and gravity, are so constructed as to leave open the possibility of mathematization. The crowning glory, of course, is terrestrial motion, with its development of an existing blueprint at **level 3** and its empirical and theoretical successes at **levels 1** and **2** , but that should not blind us to the overall unity which, in Galileo's scheme, can be seen to lie at **levels 4** and **5**. Granted this underlying unity it becomes relatively straightforward to make rational sense of Galileo's work.

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[90] For this experiment Galileo required two good-sized hills with an unimpeded view of each other and set some three miles apart. The best that he could manage was a distance of about one mile.

In Chapter One I remarked that the present climate of historiography largely ensures that historians never theorize the beliefs and actions of historical actors as rational, or even as irrational, for fear of transgressing some boundary or other. The concept underlying this is that if we work to a standard of rationality then past actors cannot possibly measure up to it and will therefore be found to be irrational. It entails that there can be no legitimate link between 'then' and 'now', no circumstances in which the facts that are known about a historical agent can be compared against twentieth-century expectations. Of course, by now it should be apparent that if we adopt an aim-oriented empiricist perspective we have an ideal case in whose terms it becomes possible to do justice to the <u>genuine</u> <u>rationality</u> of past thinkers. This, as we have seen, is because rationality is dependent upon the current blueprint, the associated framework, and the proper evolution of the whole. Indeed, it does Galileo a grave injustice to disallow comparisons with the present in this way because it becomes impossible to categorize what is so truly original and significant about his contribution. Consider the modern, orthodoxy that God can have played no rational role in the formulation of earlier cosmologies. Historians have, of course, always accepted the role played by theology and religious considerations during the Scientific Revolution and acknowledged the importance of, for example, Newton's voluntarism in the development of his natural philosophy. However, years ago Burtt set the parameters in which all subsequent discussions have taken place. When he posed the question as to whether the mathematical structure of the world is something ultimate or something which can be further explained, his answer was,

> **if a religious basis be a further explication, the latter would appear to be the answer for Galileo, as for Kepler.** [168]

However, he emphasises as more significant, Galileo's *'touch of agnosticism'*, which was enough to *'save science her opportunity for further stupendous victories in the mathematical interpretation of the world'*. [169] In Burtt's view, God is not so much the guarantor of certain knowledge as a threat, being likely to sanction mankind's *'animistic weakness at the expense of the rigorous mathematical character of reality.'* [170] Hence when the role of the ultimate explanation as to why certain knowledge of nature is attainable is allotted to God, it is not permitted to function as a part of the rational character of science. One can further add that who, among twentieth-century historians of science, would have, or would now, consider otherwise?

Aim-oriented empiricism, on the other hand, sees such theological considerations as genuine attempts to guarantee that the universe is such that we can possess and increase our knowledge and understanding of it. In the light of this the careful prescriptions of historicism, described in Chapter One, look somewhat condescending, not to say perverse and anachronistic. In their terms if historical actors acted as if religious considerations properly belonged to science, then historians must not treat them as an external factor. Of course, there must be no appeal to any such 'transcendent reality' in the explanations of specific pieces of scientific knowledge: we must not treat historically-located religious considerations as if they properly belonged to science. However, adopt aim-oriented empiricism and the fact that in the early modern period God was maintained as an explicit, central and permanent item of scientific knowledge, can be interpreted as rational. In aim-oriented empiricist terms Galileo's ultimately religious explanation for the structure of the world fulfils the aim-oriented empiricist criteria of being analogous to the comprehensibility conjecture.[91] Contrary to Burtt's opinion, the role of God in Galileo's work can be construed as being the foundation for the *'rigorous mathematical character of reality'*, rather than the threatening *'animistic weakness'*. It was not Galileo's *'touch of agnosticism'* which saved the day but the fact that he allowed the Divine a pivotal role at **level 5**, which ensued the derivation of a further metaphysical assertion that fulfilled one of the requirements of **level 4 P** and set in motion the evolving framework.

Aim-oriented empiricism, therefore, provides a new explanation for the combination, at the methodological level, of the sustained flexibility and precision with which Galileo employs his experimental, theoretical, and mathematical reasoning. Finocchiaro sums it up thus:

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[91] **A particularly clear example is Kepler's explicit expression of the relationship between the physical system of the planets and a geometrical archetype in the mind of God. Although an example of historically-located metaphysics, and therefore not an obvious instantiation of how the emergence of the new science changed the form of metaphysical assertions, it is still possible to interpret it from an aim-oriented empiricist perspective and see how it looks with respect to the hierarchy of** levels. **Newton's towering intellectual and religious synthesis went even further, in that he theorized forces and other agents within nature as manifestations of God's direct activity in the world. For example, see** 'De Gravitatione aequipondio fluidorum et solidorum', trans. A. R. and M. B. Hall, *The Unpublished Scientific Papers of Sir Isaac Newton*, London: Macmillan, 1962.

> **What characterizes such unsystematic ... philosophy is a constant and sustained willingness to reflect on one's activities in order to understand and justify them, as the need arises in a concrete situation, [a] skilful synthesis of scientific practice and philosophical theorising about it.** [171]

In aim-oriented empiricist terms, however, this 'synthesis' simply follows from the requirements of the metaphysical blueprint which, taken in conjunction with the circumstances of the individual investigation, determine which mathematically engendered method is appropriate in any given instance. It is the interplay between the different **levels** which produces the methodological means that have engendered the unrivalled ongoing success of modern science. It is the legitimate link between 'then' and 'now' which highlights the superiority of seventeenth-century rational thought when compared with twentieth-century thought. It is the ideal case against which the facts that are known about a historical agent may be compared, an option for exegesis which does not reconstruct the past. The developments Galileo made within an existing blueprint tradition, the reciprocity between the metaphysics and the method, the flexibility therein employed, and the construction of a framework which carried within itself the possibility of the 'loop of positive feedback', all suggest rational thought of the highest order. Insofar as twentieth-century science eschews the explicit inclusion of the sorts of assumptions at **levels 4** and **5** that make this evolutionary process possible, it is less rational than its seventeenth-century progenitor.

Stein, whom I quoted at the end of Chapter One, was right. The 'subsequent generations of scientific-philosophical controversy' have indeed been 'a serious falling off' and an aim-oriented empiricist interpretation demonstrates why. Galileo's combination of experimental, empirical work and theoretical, metaphysical thinking can be interpreted as the very beginning of that process of positive feedback which aim-oriented empiricism asserts to be the mechanism whereby the rational development and progress of scientific ideas is inaugurated. It is in his methods, implicit in his observational, experimental and theoretical work, that we can trace the links between Galileo and modern science because, in the final analysis, those methods correspond, <u>in an appropriate aim-oriented empiricist way</u>, to two important things. Firstly, they correspond to a dispassionate commitment to a central concept of physicalism which, although not as rigorously defined as even preSocratic versions, did encapsulate a basic requirement. Secondly, they correspond to the first step in the evolutionary process of positive feedback (that methodological key to scientific rationality), insofar as they develop a mode of thought in which there is a degree of interaction between metaphysical ideas, more specific scientific theories, methodological thinking, and empirical research. Taken together they can be interpreted as the start of the gradual clarification and development of the single, fundamental idea of physicalism, a course of action which all major theoretical developments in science have utilized to produce an ever more detailed, precise, and secure picture of the universe, recognising continuity at the highest theoretical level.[92] To refuse to take seriously the business of the rational assessment of so significant a figure as Galileo is to succumb to a kind of unintentional Whiggishness. It involves the presupposition that if there <u>is</u> such a thing as an ideal for scientific rationality, which is increasingly held to be unlikely, then the only <u>possible</u> model is something like modern, standard empiricist science, combined with the recognition that standard empiricist standards don't apply to seventeenth-century figures. To avoid this sort of Whiggish error it is necessary to recognise that, at the very least, there might be more than one model of rationality.

So there is a certain irony in the fact that an aim-oriented empiricist framework reverts to the position that it is Galileo's <u>methodology</u> that is surprisingly characteristic of modern science. However, it is a position quite unencumbered by the problems which bedevilled the accounts of earlier generations of historians of science: it argues that Galileo's method partakes of some of those features which aim-oriented empiricism defines as providing a solution to the problem of what a rational and progressive scientific method <u>is</u>, holding within itself the possibility of improving knowledge, aims, and methods, whilst fully recognising that this was not, on Galileo's part, a conscious, explicit process. It maintains that what Galileo <u>did</u> was sufficiently close to aim-oriented empiricism to ensure that, whilst he was putting his own beliefs into practice, he can also be interpreted as having implemented a surprising amount of the aim-oriented empiricist framework, with some of the results that might be expected to follow from such an

[92] **Nick Maxwell writes,** *'All major theoretical developments of science can be interpreted as enabling us to understand, in ever greater detail and with ever greater precision, how more and more apparently diverse phenomena are the outcome of relatively few different sorts of entities interacting by means of ever fewer different sorts of invariant forces (at present described by the three or four fundamental dynamical theories of modern physics)'.* **See Maxwell, N.,** *From Knowledge to Wisdom*, 184.

action. His decision to replace the current Scholastic cosmology and methodology with one better suited to the generation of knowledge is in accordance with the principle of intellectual integrity and, in its implication that better explanations exist to be found, it also corresponds with the requirements of **level 8** meta-knowability. He produced a framework which was itself a development of an ancient tradition of blueprint progression. It functioned as the foundation of the Galilean cosmos insofar as he managed to combine a central concept of **level 4 P**, that there are simple, mathematically-formulated laws governing all natural processes, everywhere, with the **level 3** assertion of uniform circular motion, which was further generalized to include bodies moving on or near the surface of the earth. This more successful **level 3** metaphysical assertion, which combined uniform circular motion and uniformly accelerated motion, made possible predictions which are quantifiably testable. The framework therefore qualifies as a research programme whose empirical progress can be measured. It is clear that Galileo treated it in this way because he fulfilled the further aim-oriented empiricist requirement of comparing the empirical progress of this new research programme with the rival Scholastic Aristotelian one. All of this is facilitated by the fact that in his hands the 'God thesis', interpreted as the idea that the material world conforms to at least some aspects of physicalism because of the will of God, is so handled as to make it possible to consider the universe as autonomous, and impersonal, as an entity to be explained by reference to something within itself. His methodological practices reflected his blueprint commitments at **levels 3** and **4** in their experimental set-up, their geometrical demonstrations, and their quantified observations. In their combination of sustained flexibility and precision they also mirror the aim-oriented empiricist expectation that all components in a framework will evolve into a closer line with each other. By the same token they provide a rationale for the amount of philosophical practice that is frequently attributed to Galileo.[93] They can even provide a rationale for Galileo's reliance upon strictly false hypotheses: he was <u>methodologically correct</u> to form his framework from the simple, slightly false version of the Copernican hypothesis and the false, but simple and approximately correct law of circular inertia. Retaining the empirically more accurate but composite, complicated and arbitrary

[93] **Finocchiaro, Hatfield, and Hall, to name but three, have all commented upon the philosophical dimension to Galileo's work. See Finocchiaro, M.,** *Galileo and the Art of Reasoning*, **Hatfield,** 'Metaphysics and the New Science', **Hall,** 'Was Galileo a Metaphysicist?'

assumptions of Copernicus, or recognising the force of the Keplerian point that the sun is at the focus of every elliptical planetary orbit, would never have allowed him to achieve his ambition of adding axioms to the Euclidean system. He fulfilled this ambition precisely because he utilized the best method, the one involving the simplest unifying metaphysical assumptions.[94] The method was so well-suited to the task that even those cases where the assumption might have proved to be an impediment, in the event did not. Circular inertia was the product of that correspondence which he asserted to exist between the motion of the planets orbiting the sun and the uniform motion of a ball rolling around the earth. In the table top situation the curvature of the earth could be ignored and the segment of circular inertial motion produced, uniform horizontal motion, could be treated as linear. When his theory of universal, uniform circular motion, is interpreted as a **level 3** blueprint it becomes clear that it need only be falsified if, over a long period of time, it failed to produce any advances in empirical knowledge in the shape of instances of mathematized physics. Observational falsification of the Copernican part of the framework was not fatal to it, as long as the best **level 3** theory continued to accord with **level 4 P** and was empirically adequate with respect to **level 2** theory. Exploring natural phenomena through the medium of these simple but approximately correct laws was the scientifically proper thing to do and transformed the enterprise of obtaining knowledge of the physical dynamics of the universe into a progressive one: it entails that one must commence with the dynamic patterns that characterize simple systems before moving on to those of more complex phenomena. In effect, Galileo constructed methodological practices in accordance with the requirements of both the metaphysical assumptions at **levels 3** and **4** and the individual circumstances under investigation and he was methodologically correct to do so.

In conclusion, then, the Galilean methodology can be said to represent the very beginnings of the aim-oriented empiricist evolutionary process which, although it has never been explicitly put into practice, has nevertheless seen some of its central tenets subsumed into the standard empiricist model generally held to be responsible for the success of the modern scientific enterprise. Central to this framework is the belief that

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[94] Certainly, Keplerian ellipses were only a real improvement on circles when their associated laws were used to produce the dynamical justification of heliocentrism but circularity was nevertheless easily falsifiable by observation.

metaphysical assertions should be held as an explicit part of scientific knowledge and not, as they tend to be in the standard empiricist model, merely implied in the methodology. Galileo was actually, in aim-oriented empiricist terms, quite open about his metaphysical commitments and it is by no means difficult to understand the tremendous unifying function that they perform in that body of work nor, indeed, the rationale which they offer for Galileo's traditional patriarchal status. It is surely this that explains the fact that,

> **One of the most striking things about Galileo is the confidence and surety with which he writes and formulates principles of scientific method that are as valid now as in the seventeenth century.** [172]

[1] Galileo, *Two New Sciences* (1638), trans. H. Crew and A. de Salvio (1914), New York: Dover Publications, 1954, 264.

[2] Drake, S. and Drabkin, I. (eds.), *Galileo on Motion and Mechanics*, Madison: University of Wisconsin Press, 1960, 65-74.

[3] Galileo, *Dialogue Concerning Two Chief World Systems* (1632), trans. by S. Drake, Berkeley: University of California Press, 1953, 186-7.

[4] ibid., 327.

[5] Barbour, J., *Absolute or Relative Motion Vol 1*, Cambridge: Cambridge University Press, 1989, 397.

[6] Galileo, *Dialogue*, 238.

[7] Barbour, J., *Absolute or Relative Motion Vol 1*, 396-402.

[8] Galileo, *Dialogue*, 130.

[9] Barbour, J., *Absolute or Relative Motion, Vol 1*, 140.

[10] Galileo, *Dialogue*, 327.

[11] Mach, E., *The Science of Mechanics*, La Salle, Ill.: Open Court, 1960, 264.

[12] Galileo, *Dialogue*, 427.

[13] Galileo, *Two New Sciences*, 170.

[14] Barbour, I., *Absolute or Relative Motion, Vol 1*, 365.

[15] Drake, S., *Discoveries and Opinions of Galileo*, New York: Doubleday Anchor Books, 1957, 241.

[16] Hall, A. R., *From Galileo to Newton 1630--1720* (2nd edn.), New York: Dover Publications, 1983, 86-7.

[17] Galileo, *The Assayer* (1623), in S. Drake, Discoveries and Opinions of Galileo, 237-8 .

[18] Galileo, *Dialogue*, 206.

[19] ibid., 207.

[20] Galileo, *Two New Sciences*, 103.

[21] ibid.

[22] Galileo, 'Letter to the Grand Duchess Christina', in S. Drake, *Discoveries and Opinions*, 183-4.

[23] ibid., 175-216.

[24] Galileo, *Dialogue*, 21

[25] Koyré, A., *Galileo Studies*, Susses: Harvester Press, 1978, 131

[26] Galileo, *Dialogue*, 460.

[27] Drake, S., *Galileo at Work: His Scientific Biography*, Chicago: University of Chicago Press, 1978, 128.

[28] Galileo, *Dialogue*, 13.

[29] Boyer, C. B. and Merzbach, U. T., *A History of Mathematics* (rev. edn.), New York: Wiley, 1989, 175-76.

[30] Galileo, *Two New Sciences*, 242-43.

[31] ibid., 244-45.

[32] Barbour, J., *Absolute or Relative Motion Vol. 1*, 372.

[33] Galileo, *Two New Sciences*, 243.

[34] Galileo, *Two New Sciences*, 40-1.

[35] Galileo, *Dialogue*, 18.

[36] ibid., 32.

[37] ibid., 16.

[38] ibid., 18-19.

[39] ibid., 20.

[40] ibid., 76-118.

[41] Galileo, *Letters on Sunspots*, in S. Drake, *Discoveries and Opinions of Galileo*, 110.

[42] Galileo, *Dialogue*, 233.

[43] ibid., 261.

[44] ibid., 264.

[45] ibid., 257.

[46] ibid., 263.

[47] ibid., 260.

[48] Barbour, J., *Absolute or Relative Motion Vol 1*, 395.

[49] Galileo, *Dialogue*, 186-7.

[50] Galileo, *Two New Sciences*, 284-89.

[51] Galileo, *Letters on Sunspots*, 89-114.

[52] ibid., 112.

[53] ibid., 108.

[54] ibid., 109.

[55] Drake, S., *Galileo at Work*, 32-4.

[56] Barbour, J., *Absolute or Relative Motion Vol 1*, 371.

[57] Drake, S., *Galileo at Work*, 33.

[58] Barbour, J., *Absolute or Relative Motion Vol 1*, 372.

[59] ibid., 371.

[60] Drake, S., *Discoveries and Opinions of Galileo*, 57.

[61] Galileo, *Two New Sciences*, 174.

[62] ibid., 176.

[63] Butts, R. E., 'Some Tactics in Galileo's Propaganda', in R. E. Butts and J. C. Pitt (eds.), *New Perspectives on Galileo*, Dordrecht: Reidel, 1978, 63.

[64] Drake, S., 'The Role of Music in Galileo's Experiments', *Scientific American 232*, June 1975, 98-104.

[65] Galileo, *Two New Sciences*, 276.

[66] ibid.

[67] ibid., 172.

[68] Galileo, 'Letter to Baliani, 1 August 1639', *Opere di Galileo 18*, 77.

[69] Galileo, *The Assayer*, trans. S. Drake, *Discoveries and Opinions of Galileo*, 240.

[70] Galileo, *Two New Sciences*, 242.

[71] ibid., 169.

[72] ibid.

[73] ibid., 174.

[74] Galileo, *Dialogue*, 225.

[75] Galileo, *Two New Sciences*, 171.

[76] ibid., 179.

[77] ibid., 162.

[78] ibid., 169.

[79] ibid., 170.

[80] ibid., 172

[81] ibid., 72.

[82] ibid., 167-68

[83] Galileo, *Dialogue*, 76-118.

[84] ibid., 320-21.

[85] Galileo, 'Letters on Sunspots', trans. S. Drake, *Discoveries and Opinions of Galileo* 59-144.

[86] Galileo, *Two New Sciences*, 243.

[87] Galileo, *The Assayer*, trans. S. Drake, *Discoveries and Opinions of Galileo* 239.

[88] Galileo, *Dialogue*, 103.

[89] ibid.

[90] ibid., 37-8.

[91] Galileo, *Dialogue*, 76-118.

[92] Galileo, *Two New Sciences*, 243

[93] ibid.

[94] ibid.

[95] MacLauchlan, J., 'Drake Against The Philosophers', in T. H. Levere and W. R. Shea (eds.), *Nature, Experiment, and the Sciences*, Kluwer Academic Publishers, 1990, 141.

[96] Galileo, *Dialogue*, 14.

[97] Wisan, W., 'Galileo's Scientific Method: A Re-examination', in R. E. Butts and J. C. Pitts (eds.), *New Perspectives on Galileo*, Dordrecht: D. Reidel, 1978, 1-57.

[98] Galileo, *Dialogue*, 406

[99] ibid., 405.

[100] Koyré, A., *Galileo Studies, 101.*

[101] Galileo, *The Assayer* (1623), trans. S. Drake in, *The controversy on the Comets of 1618*, Philadelphia: University of Pennsylvania Press, 1960, 183-4.

[102] Crombie, A. C., 'Galileo: A Philosophical Symbol', *Actes VIII, Congrés Internationale d'Histoire des Science 3*, 1956, 1090.

[103] Finocchiaro, M., *Galileo and the Art of Reasoning*, Dordrecht: D. Reidel, 1980.

[104] Drake, S., *Galileo*, 10.

[105] MacLachlan, J., 'Drake Against the Philosophers', 136.

[106] Hall, A. R., 'Was Galileo a Metaphysicist?', in T. H. Levere and W. R. Shea (eds.), *Nature, Experiment, and the Sciences*, Kluwer Academic Publishers, 1990, 109.

[107] Galileo, *Dialogue*, 57.

[108] Barbour, J., *Absolute or Relative Motion, Vol. 1*, 356-57.

[109] Galileo, *Two New Sciences*, 153-54.

[110] Barbour, J., *Absolute or Relative Motion*, 357.

[111] ibid., 356.

[112] Koyré, A., *Galileo Studies*, 223, n. 123.

[113] Strong, E. W., *Procedures and Metaphysics: A Study of the Philosophy of Mathematical-Physical Science in the Sixteenth and Seventeenth Centuries*, Berkeley and Los Angeles: University of California Press, 1936, the first chapter.

[114] Hall, A. R., 'Was Galileo a Metaphysicist?', 115.

[115] ibid., 113.

[116] ibid., 116.

[117] Hatfield, G., 'Metaphysics and the New Science', in D. Lindberg and R. S. Westman (eds.), *Reappraisals of the Scientific Revolution*, Cambridge: Cambridge University Press, 1990, 123.

[118] Hatfield, G., 'Metaphysics and the New Science', 121-3.

[119] MacLachlan, J., 'Drake Against the Philosophers', 140.

[120] ibid., 141.

[121] ibid.

[122] Hatfield, G., 'Metaphysics and the New Science', 97.

[123] ibid., 97-8.

[124] ibid., 96.

[125] ibid., 95.

[126] Hall, A. R. , 'Was Galileo a Metaphysicist?', 114.

[127] Hatfield, G., 'Metaphysics and the New Science', 128

[128] ibid., 135.

[129] Drake, S., *Discoveries and Opinions*, 123.

[130] ibid., 124.

[131] ibid.

[132] ibid., 182.

[133] ibid., 210.

[134] ibid., 183.

[135] ibid.

[136]Galileo, *Two New Sciences*, 75.

[137] ibid., 12-13.

[138] Galileo, *Dialogue*, 207-8.

[139] ibid., 207.

[140] Galileo, *Two New Sciences*, 123.

[141] Galileo, *Two New Sciences*, 113.

[142] ibid., 112-13.

[143] ibid.

[144] Galileo, *Two New Sciences*, 2-3.

[145] ibid., 3.

[146] Drake, S., *Galileo at Work*, 412.

[147] MacLachlan, J., 'Drake Against the Philosophers', 141.

[148] Drake, S., *Galileo*, 70.

[149] Hall, A. R., *Galileo to Newton*, 84.

[150] Hall, A. R., 'Was Galileo a Metaphysicist?', 113.

[151] ibid.

[152] Hatfield, G., 'Metaphysics and the New Science', 131.

[153] ibid.

[154] ibid.

[155] Hall, A. R. 'Was Galileo a Metaphysicist?', 112.

[156] Drake, S., *Discoveries and Opinions*, 241.

[157] ibid., 253.

[158] Hatfield, G., 'Metaphysics and the New Science', 133.

[159] Drake, S., *The Controversy on the Comets of 1618*, Philadelphia: University of Pennsylvania Press, 1960, 183.

[160] ibid., 184.

[161] Drake, S., *Discoveries and Opinions*, 277-8

[162] Galileo, *Two New Sciences*, 40.

[163] ibid., 41.

[164] ibid., 42.

[165] ibid., 40.

[166] ibid., 41.

[167] ibid., 43.

[168] Burtt, *The Metaphysical Foundations of Modern Physical Science*, 71.

[169] ibid.

[170] ibid.

[171] Finocchiaro, M., *Galileo and the Art of Reasoning*, 165-64.

[172] Barbour, J., *Absolute or Relative Motion Vol. 1*, 376.

CHAPTER FOUR

THE BEST OF BOTH WORLDS?

In the first three chapters of this thesis I have tried to demonstrate, at some length, that the mission of constructing a history of the growth and development of scientific ideas, of understanding how and why scientific learning has come about, is a philosophico-historical task. I have also demonstrated why, even in the face of the ever-accelerating progress of natural science through the past four centuries, such a historiography might seem impossible in the present academic climate. Social history of science concerns itself with contexts, which entails that any historical context in which knowledge of nature has been made is grist to the historians' mill. This could range from the court culture and patronage in which Galileo worked, to the several forms of institution in which scientific work has been done, to the multifarious instruments, from the marine quadrant to the particle accelerator, which have been an indispensable part of the whole operation. It can also include what scientists do in their scientific work, such as theorizing, calculating, calibrating, evaluating, recording, writing up, lecturing and chasing after finance. Social history of science accepts that our knowledge of nature has changed but believes that the process has been - and will continue to be - a cultural one. The cerebral dimension can only be explained by reference to everyday human processes: even the discovery of truly epochal theories cannot be characterized, as Bronowski characterized them, as the moment when, *'you hear God thinking'* [1] This, of course, ignores the extent to which scientists themselves think in these terms. Stephen Hawking concluded *A Brief History of Time* with these words:

> **However, if we do discover a complete theory of everything ...** [we should] **be able to take part in the discussion of the question of why it is that we and the universe exist. If we find the answer to that, it would be the ultimate triumph of human reason - for then we would know the mind of God.** [2]

However, for social historians of science the suspicion surrounding claims that nature has a hand in the ways in which scientists come to a true understanding of it entails that apparent flashes of inspiration - and even 'the ultimate triumph of reason' - have to be explained in natural terms. In the event that may well necessitate investigating the full context of someone's entire career in order to account for why they came to that particular decision at that precise moment.

However, I see present-day social historians of science as being deeply misguided, not in their wish to pursue contextual history, which is a perfectly respectable and rewarding vocation, but rather in their conviction that in order to do history of science at all one must adopt this sort of professional relativism, refusing to make intellectual evaluations about the historically-situated science one studies. The emphasis on the need for symmetrical explanation, the growing orthodoxy which maintains that critical observation and impartiality can only come through anthropologically-inspired relativist techniques, means that only ordinary historical causes and effects can explain the beliefs that we come to hold. One cannot consider the intellectual content of science, whether it is good or bad, rational or irrational, and whether or not it contributed to the progress of scientific knowledge. One cannot, if one accepts the dictates of social historiography of science, forge a conceptual scheme that ignores these core beliefs, because those same dictates deny the validity of history of science done in any other way. For history of science to be done 'properly', and in a 'non-Whiggish' fashion, all assumptions about the rationality and the progressive character of science must be abandoned.

My response to this has been to insist that there are important parts of the history of science which demand that intellectual judgements and evaluations are a necessary part of the endeavour. To prohibit their use is to exclude from the discipline a fundamental and important area of research. I argued, in general terms, for the use of interest or intellectual, or value judgements in the choice of subject matter, for any kind of historical research, and in the selection, from within that area, of the significant problems and questions. The role of such judgements should be made crystal clear: the historian should leave no doubt as to what the object of historical study is, nor why it is judged to be interesting and worthwhile. I pointed out that, if properly handled, such judgements do not automatically become drawn into the form of the explanation which the historian puts forward. The danger that interests and values may be too narrowly defined, so that areas of historical research become too restricted, may be avoided by encouraging research into as wide a range of historical subjects as possible. One such historical object or process that the historian may be legitimately interested in is a progress, or goal-oriented history: the history of people's efforts to attain some kind of long-term goal. Granted that such a long-term goal-seeking, progressive endeavour is a proper object of study, several things become clear. Firstly, the historical account will depend upon how the goal for the progressive endeavour is specified: differently specified goals will lead to different historical accounts of the progress towards that goal. The choice of goal must rest with the historian: it is not something can be inferred from the accounts of historical actors because that would presuppose that they knew what was going to count as the goal in advance. The historian must use interest and value judgements, among other skills, to select and justify a proper, goal-oriented object or area of study. Equally it is the task of the historian to describe, analyse and come to some explanation of the successes and failures of actions conducive to bringing that endeavour towards its designated goal. That involves making intellectual judgements about the significant questions and problems within the defined area. What it does not entail is that progress was ever achieved, or that it was even articulated or explicitly pursued by the historical agents involved. These are important factual, historical questions which can only be established if the historian continually evaluates historical deeds from the perspective of their success or failure in tending towards the goal as specified. Those deeds also involve the interpretations and evaluations which the agents themselves put upon their own actions but such interpretations and actions do not dictate the form of the historical methodology to be applied.

Finally, of course, I extrapolated all of this to the context of science, which can also be construed as having various aims which will lead to (sometimes surprisingly different) progress-oriented histories of science.[1] The conclusion was that the choice of the historical

[1] If the basic goal of science is characterized as being the promotion of human welfare, science might be deemed to have had limited success and quite a lot of failures. If the basic goal, on the other hand, is to develop and improve expert knowledge about natural phenomena, then it would probably be judged to have been a roaring success.

development of scientific ideas, conceived as a goal-seeking endeavour, was entirely warranted even though it corresponds in some degree to what used to be known as 'internalist, intellectual history of science', the very historiography of science that spurred the formulation of the present-day, antithetical social history of science approach. I argued that in this case too there is no inbuilt presupposition that science has made progress, or been a rational affair: discoveries can be made and subsequently accepted by scientific communities for all sorts of reasons, so that the progress-historian should not prejudge the extent to which a development in science was rational, irrational, or even a-rational. Consequently there is no automatic imposition upon historical actors of the progress-oriented historian's own ideas about scientific progress and scientific rationality: a good progress-historian of science should be concerned with the ideas of historical figures, both explicit and implicit, particularly those concerning their aims and methods and what they understood the nature of science to be. It is necessary to have a good grasp of how individuals made science, and drew their own boundaries in order to evaluate their work in terms of the parameters of scientific progress set by the progress-oriented historiography. These parameters require the historian to pay attention to both 'success', or ideas that led to somewhere and 'failure', or ideas that led nowhere at all, both being defined in terms of the parameters. Progress-history of science is over-run with blind alleys, false dawns, aborted beginnings, useless conjectures that spawned serviceable ideas, useful conjectures that subsequently turned out to be wrong, excellent arguments in support of ideas that sank without trace, and bad arguments in support of ideas that subsequently triumphed. A sufficient account of scientific progress must take account of all of these because they form an indispensable adjunct to scientific progress. To imagine otherwise is to suppose that science provides an infallible method of discovery, with only 'bad' scientists failing to keep on the correct, progressive path.

Social historians of science are correct in their estimation that historical actors did not know what was going to count as 'science' in advance but wrong in their conclusion that as a result historians must follow the social relativist/contextual path. The twin thrusts of their justification rest in their commitment to natural causes and symmetrical explanations. Anthropological techniques are intended to ensure that all doctrines or beliefs are explained in terms of ordinary, historical causes and effects: a professional relativist stance is seen as a guarantee against the problems associated with any historiography of science which tries to make intellectual judgements and evaluations of science. If the old internalist model assumed today's parameters and judged the past in accordance with them, then the way to avoid all the concomitant disadvantages of such an approach is for historians to avoid boundaries: they must not invoke existing boundaries and they must not construct their own. As they see it there will be no danger then of being in thrall to the expectation that nature, as the only reality that exists, imposes the answers upon our understanding; there will be no ignoring the degree to which what counts as the truth is the outcome of natural human processes rather than the cause of that outcome; there will be no chance that the problems of achieving consent within a scientific community will not be recognised as, broadly speaking, political problems. After all, to remove human agency from the history of science, which in <u>their</u> terms is the unavoidable result of upholding the belief that nature determines the ways in which nature is to be understood, is to make it <u>unhistorical</u>. Just to make sure there are no present-centred perspectives working undercover, as it were, influencing the form of the explanations given, historians must go on to explain all knowledge claims symmetrically, without reference to who was, or is now, judged to be right or wrong.

Notwithstanding, most of the arguments made against intellectual history of science are answered by the kind of progress-oriented historiography outlined here. Impartiality and a critical attitude <u>are</u> a part of such a historiography, a vital part, and must be employed rigorously in specifying what the goal of the studied endeavour is. Simply in order to be able to identify historical problems, the historian must make assessments about what constitutes scientific progress. So far as trying to discover if progress did happen and, in that event, how and why it happened, it is necessary to evaluate the successes and failures of anything that was conducive to bringing that endeavour towards its specified goal. This demands intellectual judgements and evaluations of the science being studied: it means laying emphasis on discoveries, ideas, arguments and theories, all the while judging their relative merits and demerits, and on scientific problems and the adequacy or inadequacy of attempts to solve them. It does not entail that there is asymmetry in the explanations: that which does not tend to bring the endeavour towards its goal needs to be explained just as much as that which does. Elements which are regarded as progressive in one kind of long-term endeavour may well, given a different goal-oriented endeavour, be categorized as non-progressive. The parameters of scientific progress set by the progress-oriented historiography of science are what the work of historical actors is measured against, not what it is reconstructed into. Nothing in this schema implies that historical actors had to know, in advance, what was going to count as 'truth' and what as 'error'. Whiggishness is actually avoided when both the specified goal and the history which is being studied as contributing towards its attainment are dealt with in this way:[2] if that goal is characterized in terms of modern science then this sort of progress history is capable of being critical of modern science in a way not open to any relativist approach. It is capable of criticising Hawking's ontology, of evaluating its role within his work, of judging whether or not his definition of rationality is workable, in just the same way as it can do all these things for the role of theological considerations in seventeenth-century natural philosophy. The relativistic social history of science approach abandons any attempt to be critical of modern science, even though the original intention was perhaps to have been just the opposite. It is an option for exegesis that avoids many of the things that symmetrical explanations are designed to avoid, like Whiggism, bias, and intellectual imperialism and provides many of the things that symmetrical explanations are intended to provide, like objectivity, impartiality, and critical rigour. Moreover, progress-oriented historiographies, as defined here, do not proscribe whole areas of historiographic study in the way that social history of science does, with its imperative to abjure making intellectual judgements about the science being studied. One can quite freely and properly chose the contextual approach, with its anthropological techniques and its professional relativism, if one wishes to. The choice of historical subject must not, by methodological fiat, be restricted and trying to understand how science is historical, trying to comprehend the processes by which historical actors came into agreement and the means by which those agreements became conventions, is an intellectually

[2] **As I mentioned earlier, a historian could describe the deeply-felt philosophical, metaphysical and epistemological debates of natural philosophers in the seventeenth century with the intention of illustrating that a whole area of serious philosophical discussion is no longer a part of science and that science is the poorer as a result.**

proper study. Equally, it is intellectually proper to chose, as an object of study, science as a progress-achieving endeavour. Finally, it is still possible to construe science as wholly social in character whilst still demanding that intellectual judgements and evaluations of the science under scrutiny are made as a fundamental part of the enterprise. The intellectual is still a social endeavour and its elements, such as ideas, theories, arguments, scientific problems, calculations, observations, and experiments are essentially social in character. Arguments are clearly social in character because they take place between people in languages which they have constructed - but they may still be assessed from the standpoint of their validity or invalidity. The same may be said of claims to knowledge, which may be considered with respect to their adequacy to the facts. The social world, after all, still exists in the physical universe.

Thus far present-day social history of science has missed the mark. There are, however, two arguments which the above definition of progress-oriented history does not deal with and which social history of science might well employ to some effect. The outline given above appears, firstly, to rest on the assumption that it is possible to make rational sense of scientific progress: indeed the progress-oriented history of scientific ideas cannot get underway without such an assumption. Notwithstanding, social historians of science would immediately argue that this is impossible, there being no universally acknowledged solution to the problem of induction or to major unsolved problems in related areas such as the problem of verisimilitude and the problem of simplicity. These, after all, are some of the difficulties in the philosophy of science that fed into social historiography of science as it has developed over the last thirty years: without solutions to them, how is it possible to claim that science makes progress? Secondly, a progress history of science, which is a goal-oriented endeavour, needs as its goal some universally applicable definition of scientific rationality: it needs to specify a rational ideal <u>before</u> historical investigation can begin to evaluate whether, or how far, that ideal was put into practice. To construe this as a proper, goal-oriented endeavour would not be to presuppose its active utilization at all times and in all places, nor would it be to automatically reconstruct the past. I have argued for this cogently. What it does require is that to put this universal conception of scientific rationality into scientific practice would always be conducive to making scientific progress. Given the schema I have outlined, there are two unavoidable corollaries. One is that the extent to which scientific progress is actually achieved may always be judged in terms of this universal conception of scientific rationality. The other is that the extent to which this universal conception has been put into scientific practice remains open and can only be decided through the medium of historical investigation. Again, a considerable encouragement to the development of the social history of science position has been the apparent lack of a universal definition of scientific rationality. If the history of science showed anything at all it showed that methodologies change over time and between different branches of science. If no solution can be found to these problems then it might be the case that, although progress histories of science are still possible, a progress-oriented history of scientific ideas is not.

Of course, I then went on to argue that solutions to these problems have been found in the generalized framework of aim-oriented empiricism. It does offer a solution to the problem of induction, based on two fundamental claims. The first is the claim that we are rationally justified in accepting the increasingly attenuated levels in the aim-oriented empiricist hierarchy as items of standard conjectural scientific knowledge, given that our overall aim is to improve scientific knowledge of the universe.[3] The second is that not only will **levels 1** and **2** evolve with evolving knowledge but that **level 3** metaphysical blueprints will evolve too: this is the mechanism whereby an improvement in knowledge brings about an improvement in methods. It also offers a rationale for the problem of methods changing over time and between disciplines: changing methods become a requirement of rationality because although there is apparent methodological disunity and diversity, at the meta-methodological levels there is unity. When metaphysical assumptions concerning the comprehensibility of the phenomena change, then the methods which select theories must reflect this if they are to fulfil the primary aim of elucidating the best versions of that developing comprehensibility. This productive process of modification and development is the very core of rationality and completely destroys those definitions of rationality which in the past proved vulnerable to social history of science scholarship, and thereby gave the history of scientific ideas a bad name. It is a definition which does not depend on a (non-

[3] **I must reiterate once more that this does not constitute an argument for the truth of the thesis that the hierarchy of** levels **is a part of standard scientific knowledge.**

existent) algorithm being applied in order to wring the truth out of nature; it does not depend upon scientists having a rational grasp of 'reality'. It accepts that all knowledge is conjectural: part of the process involves the recognition of the fallible character of all assumptions, particularly the lack of decisive proofs at **levels 4 - 10**. As well as acknowledging the unavoidable lack of decisive empirical proofs, it also acknowledges the possibility that the universe might not be comprehensible in quite the way we imagine, and even that it might not be comprehensible at all. There might, in the end, be no 'reality' that we could ever be in a position to conjecture about. Notwithstanding, given that our aim is to improve scientific knowledge of the universe, the generalized aim-oriented empiricist framework, its levels of conjectural knowledge as to how the universe is comprehensible, and its motor of adjustment, improvement, and growth produce a definition of rationality far removed from that derided by social historians of science. It is a definition which emerges from the continuous and positive feedback between improving knowledge and improving aims and methods which helps to explain the growth of scientific knowledge.

Most significantly of all, from my point of view, in the aim-oriented empiricist framework the intellectual problem of the rationality of science has significant features which can only be explained historically. As cosmological ideas change so must cosmologically-related **level 3** and **level 4** aims and methods. In the absence of this concept it is impossible to even give a rationale for why particular aims and methods might be accepted at any given stage in the development of science. For a fuller understanding it is also necessary to consider, in terms of their cosmological dependence and inter-relationships, the successes and failures of any contemporaneous rival research programmes based on rival aims and methods. In this way the historical dimension of scientific rationality is revealed in terms of the relative empirical progressiveness (or sterility) of some past research programme or, more particularly, in some (more or less specific) view as to what the aims and methods ought to be. In the end it is about seeing how rationality is historical, how it has evolved through time. Granted aim-oriented empiricism it is possible to see a reciprocal relationship developing between intellectual history of science and philosophy of science. A solution to the intellectual problem of the rationality of science, the problem of induction, has repercussions for the history of scientific ideas in that it specifies an ideal rational methodology which has a historical dimension to it. This gives progress-oriented historians of science the ability to formulate their historical problems about how and why scientific progress has come about. It is only then that determination of the extent to which this may have happened becomes a factual, historical question. Historians have to make decisions as to what the historical actors themselves thought they were doing. Did they really implement the rational ideal or were they working with rather different methods? Does the work they did now appear close to the ideal in spite of their best intentions? Did they misrepresent to themselves what they were about? In so doing they will be helping philosophers of science to specify as precisely as possible the nature of the methodology which has been responsible for the considerable success of the modern scientific enterprise.

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Aim-oriented empiricism can, through the link between scientific rationality and science history, simultaneously introduce new advantages <u>and</u> reinstate old beliefs. The former includes the realisation that aim-oriented empiricism does not place methodological constraints on <u>any</u> perspective: as long as a perspective is properly delineated and directed it has a legitimate role. It is perfectly acceptable, to use an example already cited, to argue that the spread of Newtonianism in England was due to the socio-economic advantages that accrued from the application of Newtonian principles. It is equally acceptable to argue that Newtonianism was chiefly employed in the reformation of physical science and was accepted on that basis. All perspectives can be judged in terms of the relationship between a goal, the strategies employed to attain it, and whether the closeness of the 'fit' between the two has any bearing on the power of the perspective to illuminate the past. Indeed, the possibility exists that it is the play of competing perspectives that will most enlarge our understanding of the scientific past, that the real gains might come from fruitful dialogue or even the clash of interpretations. No one approach should thoroughly dominate the history of science. Many more sorts of history are legitimate than is generally realised. It is ironic that the viewpoint that reinstates intellectual history of science is also the one that increases the range of historiographic studies and offers a rationale for the view that what might be termed a collectivist vision could be more enlightening (because it requires a more critical style of enquiry) than what I have termed the 'heroic vision' of the cultural relativists. The old beliefs which are given a new legitimacy include a sense of the value of the progressive intellectual tradition, the acceptability of a philosophical interpretative framework for the history of science, the particular suitability of the history of science as a means of illustrating and explaining the growth of scientific knowledge, with all the advantages that holds for historians and present-day scientific workers, and the old idea of the value of the historical method of exposition, which is enlarged so that not only can the historical dimension be perceived as important to the understanding of any present-day science, the present can also be shown to be a significant aid in interpreting the past.

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[1] Bronowski, J., *The Ascent of Man*, London: BBC Publications, 1974 256.

[2] Hawking, S., *A Brief History of Time*, London: Bantam Press, 1988, 175.

APPENDIX

THE PROOF OF THE INDUCTION THEOREM

The word 'proof' in the title of this appendix must be taken with a pinch of salt. It is a part of the import of the principle of integrity, and the generalized version of aim-oriented empiricism to which it gives rise, that there cannot be decisive proofs of the chosen assumption at all **levels**, **3** to **10**. Intellectual rigour demands, indeed, that we recognize the highly fallible, conjectural character of the assumption that is made at each **level**. In seeking to solve the problem of induction, we may be led to try to give a decisive proof of the induction theorem; it may seem that the rationality of science demands this. The problem of integrity, however, demands that we acknowledge the genuine fallibility of the choices that we have made.

At each level, the chosen assumption is to be justified on the grounds that it satisfies the following criteria better than any rival assumption at this level:

(a) It holds out greater hope of promoting the growth of knowledge than any rival whether true or false.

(b) It holds out greater hope of promoting the growth of knowledge than any rival if true.

(c) It does better justice to apparent scientific progress than any rival.

(d) It is inherently more plausible, more likely to be true, than any rival (if only because it has less content).

As was explained in Chapter Two, it is to be expected that (b) and (d) will clash. The more an assumption promises to help the growth of knowledge, if true, so the more the assumption is likely to assert, and *vice versa* The more an assumption satisfies both (b) and (d), the better it is.

The logical relationship between the propositions at the various levels is as follows. Let us suppose, initially, that the universe really is physically comprehensible, and the true theory of everything, **T**, at **level 2**, has been discovered. In this case, ideally, $\mathbf{H_2}$ would entail $\mathbf{H_1}$, and $\mathbf{H_r}$ would entail $\mathbf{H_{r+1}}$ for $\mathbf{r = 2, \ldots 8}$. $\mathbf{H_9}$ does not entail $\mathbf{H_{10}}$, for reasons that will be made clear. For $\mathbf{2 \leq r \leq 8}$, we may think of $\mathbf{H_{r+1}}$ as consisting of a statement of the form '$\mathbf{H_r}$ **or** $\mathbf{H_r^*}$ or $\mathbf{H_r^{**}}$ or ...', where $\mathbf{H_r^*}$, $\mathbf{H_r^{**}}$, etc., are rival cosmological theses to $\mathbf{H_r}$. In moving down from **level r + 1** to **level r** we adopt the factual conjecture that $\mathbf{H_r^*}$, $\mathbf{H_r^{**}}$, etc., are all false and $\mathbf{H_r}$ is true. It is this that needs to be justified by an appeal to (a) and to (d).

For $\mathbf{5 \leq r \leq 8}$, the above does not represent an idealization; in our present state of knowledge, $\mathbf{H_r}$ entails $\mathbf{H_{r+1}}$. However, for $\mathbf{r < 5}$ the above is an idealization in many ways. Granted that we have discovered the true unified theory of everything, **T**, there should be no problem with $\mathbf{H_2}$ implying $\mathbf{H_3}$, $\mathbf{H_3}$ in turn implying $\mathbf{H_4}$, and $\mathbf{H_4}$ implying $\mathbf{H_5}$. Unfortunately, we have not discovered **T** and may never do so (physicalism, perhaps, being false). At present we have at least two very different, even clashing, fundamental physical theories - the standard model (SM) and general relativity (GR). This means $\mathbf{H_2}$ conflicts with $\mathbf{H_3}$. Even taken individually, currently accepted theories belonging to $\mathbf{H_2}$ may clash with $\mathbf{H_3}$ (as when Newtonian theory clashes with the corpuscular blueprint). Furthermore, in trying to formulate $\mathbf{H_3}$ in such a way that it does as much justice as possible to the theories of $\mathbf{H_2}$, $\mathbf{H_3}$ may well conflict with $\mathbf{H_4}$.

The clashes between **levels** for $\mathbf{r \geq 5}$, and clashes within **levels**, especially within $\mathbf{H_2}$, serve to drive theoretical physics forward. These pose the problems that physicists try to solve. They are symptomatic of our ignorance. Progress in theoretical physics is to be assessed in terms of the extent to which a contribution promises to bring physics closer to the ideal state of affairs in which $\mathbf{H_2}$ implies both $\mathbf{H_1}$ and $\mathbf{H_3}$, $\mathbf{H_2}$ being a candidate for the true, unified theory of everything.

Here is a very brief consideration of the stages in turn.[1]

r = 2 To prove: given the evidence and the best available **level 3** blueprint, the best **level 2** conjecture is encapsulated by the current fundamental physical theories, the standard model (SM) and general relativity (GR). This is the case even though (SM) and (GR) are two very different sorts of theories and there is no universal agreement as to

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[1] **For a full exposition of the proof of the induction theorem, the reader is referred to Maxwell, A. N.,** ***The Comprehensibility of the Universe***, Oxford: Oxford University Press, 1998, forthcoming.

what sort of **level 3** blueprint might do justice to such very different sorts of theories.[2] This, however, is not a problem for the proof of the induction theory.

r = 3 To prove: given the evidence and the **level 4** thesis of physical comprehensibility, the best **level 3** theory is the one that will accord with, or be a special case of, physicalism, and also render the accepted body of **level 2** theory as unified as possible. Given two blueprints which clash with physicalism, that one is to be preferred which clashes the least with **(P)**, assuming that the comparison can be made. The more the accepted theory is unified with respect to a blueprint, so the more empirically adequate the blueprint is.

r = 4 To prove: given the evidence and the comprehensibility thesis **(C)**, physicalism is the best **level 4** conjecture to adopt. **(P)** is to be preferred to any other **level 4** conjecture primarily because it does far better justice to the immense apparent empirical success of physics than any rival **level 4** conjecture that is a version of **(C)**. In other words, **(P)** is vastly more empirically fruitful than any comparable, rival **(C)**-type conjecture. It is a basic task of theory in physics to unify disparate phenomena. Unity is sought as an essential part of the search for explanation and as long as some disunity remains, there remains something to explain which can only be eliminated with the development of a better explanatory theory which depicts underlying unity. Physicalism merely postulates that the universe is such that the kind of explanation sought by theoretical physics, even when pushed to the limit, exists to be discovered.

r = 5 To prove: given the evidence and the **level 6** thesis that the universe is nearly comprehensible, the best **level 5** conjecture is perfect comprehensibility, **(C)**. This step is almost a tautology. Given that the universe is nearly comprehensible it follows, by definition, that the universe is such that the growth of knowledge is better promoted by acceptance of **(C)** rather than any rival partial comprehensibility thesis. **(C)**, in the

[2] **Maxwell has suggested that such a** level 3 **blueprint might be formulated as follows: the universe is such that all phenomena evolve in accordance with Hamilton's principle of least action, formulated in terms of some unified Lagrangian, L. At present L is the sum of two or more distinct Lagrangians, with distinct physical interpretations and symmetries, for example, one for the electroweak force, one for the strong force, and one for gravitation. What is required is that L should have a single physical interpretation, and its symmetries should have an appropriate group structure. In addition, (SM) and (GR) must emerge when appropriate limits are taken.**

specific form of (**P**) has apparently been spectacularly successful in promoting the growth of knowledge and is therefore to be preferred.

r = 6 To prove: given the evidence and the **level 7** thesis that the universe is roughly comprehensible, the best **level 6** conjecture is near comprehensibility. In certain very specific circumstances, which would lead to some roughly comprehensible version of (**P**) being preferred to the perfectly comprehensible version of (**P**), we could have rational grounds for favouring some conjecture of partial comprehensibility rather than perfect comprehensibility. Given that (**P**) and (**C**) have so far been more fruitful empirically, then near comprehensibility is the best **level 6** conjecture to adopt.

r = 7 To prove: given the evidence and the **level 8** thesis that the universe is meta-knowable, the best **level 7** thesis is that the universe is roughly comprehensible. In asserting that the universe is meta-knowable we are asserting that it is such that there is some discoverable assumption that can be made about the nature of the universe which aids, and does not hinder, the growth of knowledge. Meta-knowability guarantees that generalized aim-oriented empiricism is an appropriate methodology to adopt: the universe is such that, as we acquire new knowledge we can improve the cosmological conjectures presupposed by our methods, this in turn enabling us to improve our methods. Just as in the case of comprehensibility, it only makes sense to say that the universe is (or is not) meta-knowable with respect to some specific body of knowledge together with its methods for improving knowledge (and possibly implicit meta-methods for improving existing methods). As no methods for acquiring knowledge have so far matched up to the extraordinary (apparent) success that science has met with, in its quest to explain and understand, the greater empirical fruitfulness of rough comprehensibility over any rival meta-knowable conjecture justifies us in preferring it.

r = 8 To prove: given the evidence, the **level 9** thesis that the universe is non-malicious, and the **level 10** thesis that the universe is partially knowable, the best **level 8** thesis is that the universe is meta-knowable. The basic idea of this step in the argument is: the **level 10** thesis of partial knowability if true, guarantees that we have some genuine factual knowledge of local phenomena, and some capacity to improve this local knowledge. The **level 9** thesis of epistemological non-maliciousness, if true, then guarantees that this local knowledge applies universally, to all possible phenomena at all times and places. Granted this we can conclude that the universe is partly knowable, in principle, universally. We cannot conclude, however, that existing methods that have proved successful locally would be equally successful globally: at best we can conclude that there is some discoverable development of existing methods which would be successful globally. In other words, we can conclude that the universe is meta-knowable in some way, which is what we sought to establish as the best **level 8** conjecture.

r = 9 To prove: given the evidence and the **level 10** thesis that the universe is partially knowable, the best **level 9** thesis is that the universe is non-malicious. We are justified in making this assumption because in so doing we have nothing to lose by it. If the universe is epistemologically malicious so that, despite the immense apparent empirical success of science, in ten minutes time we will abruptly find ourselves living in an entirely different kind of world with no possibility of having any advanced warning of the change, it could not conceivably help to take such an eventuality seriously, prior to the event. Even if, by some miracle, we guessed correctly, prior to the event, when the change will occur, and what the new world will be like, nevertheless this could not form a rational basis for action as there would be an infinity of equally plausible (or implausible) conjectures which would postulate different changes at different times.

r = 10 To prove: given the evidence, we are justified in accepting that the universe is partially knowable. We are justified in holding that we have some genuine knowledge of particular things and processes in our immediate environment because this follows from our having knowledge of evidence. Even our most experientially primitive, least theoretical empirical knowledge consists of, amongst other things, knowledge of macroscopic objects: scientific evidence presupposes knowledge of objects with various sorts of physical properties - bits of apparatus with the usual macroscopic properties. In so far as the problem of induction presupposes knowledge of evidence, it presupposes that we possess knowledge of this type. We are further justified in assuming that we have some capacity to improve this knowledge of particular things in our immediate environment, since in making this assumption we have nothing to lose. Doubt is rational only in so far as it can contribute to the improvement of knowledge. Granted that we have some factual knowledge of things and events of our immediate environment, it follows immediately that we have some meagre knowledge of the entire cosmos. We know that the entire cosmos is such that it makes our particular knowledge possible, that nowhere is there an explosion of chaos which will travel at near infinite speed to engulf our corner of it. This is meagre enough but in knowing it we do genuinely know something about the entire universe, about ultimate reality, even though it is knowledge without a great deal of content.

As we descend from **level 10**, via **levels 9**, **8**, ... to **level 2**, so our claims to cosmological knowledge become increasingly contentful and, to that extent, increasingly likely to be false.

Generalized Aim-Oriented Empiricism

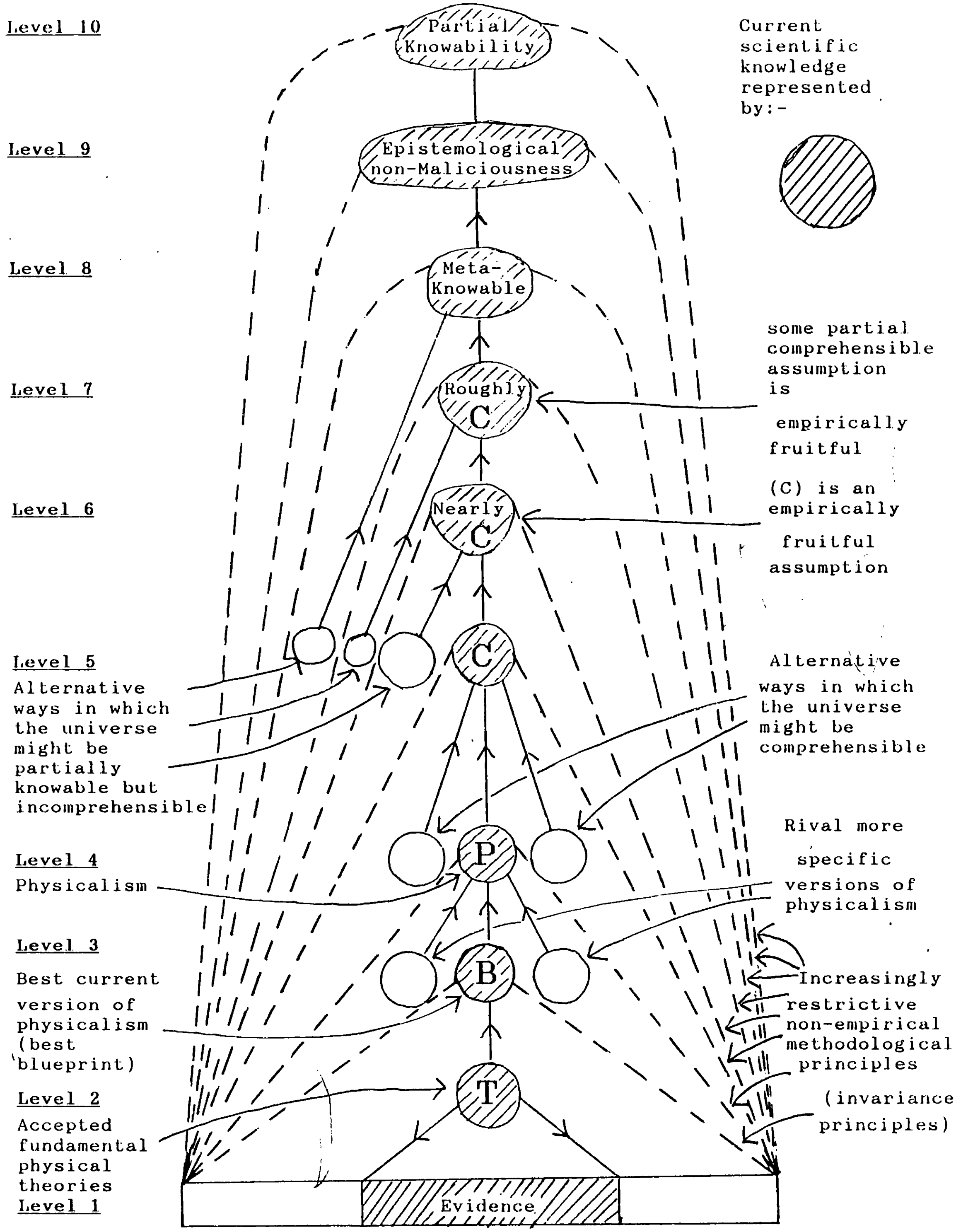

Diagram I

BIBLIOGRAPHY

Agassi, J., *Towards an Historiography of Science*, Middletown, Connec.: Wesleyan University Press, 1963.
Aiton, E. J., 'Galileo and the Theory of Tides', *Isis* 55, 1965, 56-61.
Bachelard, G., *L'actualité de l'histoire des sciences*, Paris: Palais de la decouverte, 1951.
Barbour, J., *Absolute or Relative Motion Volume One*, Cambridge: Cambridge University Press, 1989.
Barnes, B., *Interests and the Growth of Knowledge*, London: Routledge and Kegan Paul, 1977.
Barnes, B., *T. S. Kuhn and Social Science*, London: Macmillan, 1982.
Bagioli, M., *Galileo, Courtier: The Practice of Science in the Culture of Absolutism*, Chicago, Chicago University Press, 1993.
Bloor, D., *Knowledge and Social Imagery*, (2nd. edn.) Chicago: University of Chicago Press, 1991.
Boyer, C. B. and Merzbach, U. C., *A History of Mathematics* (2nd edn.), New York: John Wiley, 1989.
Brannigan, A., *The Social Basis of Scientific Discoveries*, Cambridge: Cambridge University Press, 1981.
Bronowski, J., *The Ascent of Man*, London: BBC Publications, 1974.
Brown, J. R., (ed.) *Scientific Rationality: The Sociological Turn*, Dordrecht: D. Reidel, 1984.
Buchdahl, G., *Metaphysics and the Philosophy of Science: The Classical Origins, Descartes to Kant*, Oxford, Blackwell, 1969.
Burtt, E. A., *The Metaphysical Foundations of Modern Physical Science* (rev. edn.), London: Routledge and Kegan Paul, 1932.
Butterfield, H., *The Origins of Modern Science* (2nd edn.), New York: Macmillan, 1957.
Butterfield, H., *The Whig Interpretation of History*, London: G. Bell and Sons Ltd., 1931.
Butts, R. E. and Pitt, J. C. (eds.), *New Perspectives on Galileo*, Dordrecht: D. Reidel, 1978.
Carugo, A., and Crombie, A. C., 'The Jesuits and Galileo's Ideas of Science and Nature', *Annali dell'Instituto e museo di storia della scienza di Firenze, 8,* 1983, 3-68.
Cassirer, E., *Das Erkenntnisproblem in der Philosophie und Wissenschaft der neueren Zeit*, 2nd. edn., 2 vols., Berlin: Cassirer, 1911.
Cassirer, E., *The Individual and the Cosmos in Renaissance Philosophy*, trans, M. Domandi, New York: Harper, 1963.
Clavelin, M., *The Natural Philosophy of Galileo*, Mass.: MIT Press, 1974.
Cohen, I. B., *The Birth of a New Physics* (rev. edn.), Middx.: Penguin Books, 1987.
Cohen, I. B., *Revolution in Science*, Camb., Mass., and London: Harvard University Press, 1985.
Collins, H., *Changing Order: Replication and Induction in Scientific Practice* (2nd edn.), Chicago: University of Chicago Press, 1992.
Collins H. and Pinch T., *Frames of Meaning: The Social Construction of Extraordinary Science* (2nd edn.), Chicago: University of Chicago Press, 1982.
Conant, J. B., *On Understanding Science: A Historical Approach*, New York: New American Library, 1951.
Crombie, A. C., 'Galileo: A Philosophical Symbol', *Actes V111, Congres Internationale d'Histoire des Science*, 3, 1956.
Cronin, H., *The Ant and the Peacock*, Cambridge: Cambridge University Press, 1991.
Cunningham, A. and Williams, P., 'Decentring the "big picture": the *Origins of Modern Science* and the modern origins of science', *BJHS*, 1992, 26.
Dawkins, R., *River Out of Eden*, London: Weidenfeld and Nicholson, 1995.
Debus, A. G., *Man and Nature in the Renaissance*, Cambridge: Cambridge University Press, 1978.
Debus, A. G. (ed.), *Science, Medicine and Society* (2 vols), New York: Science History Publications, 1972.
Descartes, R., *Oeurvres de Descartes*, C. Adam and P. Tannery, (eds.), Paris, 1891-1912.
Dijksterhuis, E. J., *The Mechanization of the World Picture*, Oxford: Oxford University Press, 1961.
Douglas, M., *Purity and Danger: An Analysis of Concepts of Pollution and Taboo*,London: Routledge and Kegan Paul, 1966.
Douglas, M., *Natural Symbols*, London: Barrie Jenkins, 1970.
Douglas, M., *Implicit Meanings*, London: Routledge and Kegan Paul, 1975.
Douglas, M., *Cultural Bias*, London: Royal Anthropological Institute, 1978.

Drake, S., *Discoveries and Opinions of Galileo*, New York: Doubleday, 1957.
Drake, S., *The Controversy on the Comets of 1618*, Philadelphia: University of Pennsylvania Press, 1960.
Drake, S., *Galileo*, Oxford: Oxford University Press, 1980.
Drake, S., *Galileo Against the Philosophers*, Los Angeles: Zeitlin and Ver Brugge, 1976.
Drake, S., *Galileo at Work: His Scientific Biography*, Chicago: University of Chicago Press, 1978.
Drake, S., *Galileo Studies*, Ann Arbour: University of Michegan Press, 1970.
Drake, S., 'The role of music in Galileo's Experiements', *Scientific American,* 232, June 1975, 98-104.
Drake, S. and Drabkin, I. (eds.), *Galileo on Motion and Mechanics*, Madison: University of Wisconsin Press, 1960.
Dobbs, B. J. T., *The Janus Faces of Genius: The Role of Alchemy in Newton's Thought,* Cambridge: Cambridge University Press, 1992.
Donovan, A., Laudan, L. and Laudan, R. (eds.), *Scrutinizing Science: Empirical Studies of Scientific Change*, Dordrecht: D. Reidel/Kluwer Academic, 1988.
Duhem, P., *The Aim and Structure of Physical Theory* (2nd edn. 1914), trans. by P. P. Wiener, New York: Atheneum, 1962.
Durbin, P. T. (ed.), *A Guide to the Culture of Science, Technology and Medicine*, New York: Knopf, 1988.
Durkheim, E., *Selected Writings* (1899), trans. and ed. A. Giddens, Cambridge: Cambridge University Press, 1972.
Farley, J. and Geison, G., 'Science, politics and spontaneous generation in nineteenth-century France: The Pasteur-Pouchet debate', *Bull. of the Hist. Medicine*, 48, 1974, 161-198.
Feyerabend, P., *Against Method: Outline of an Anarchistic Theory of Knowledge*, London: New Left Books, 1975.
Feyerabend P., 'Problems of Empiricism', *University of Pittsburgh Series in Philosophy of Science*, Englewood Cliffs, N. J., vol. ii, 1965, and vol. iv, 1970.
Finocchiaro, M., *Galileo and the Art of Reasoning*, Dordrecht: D. Reidel, 1980.
Forman, P., 'Immanence not transcendence for the historian of science', *Isis*, 1 xxxii, 1991.
Forman, P. 'Weimar culture, causality and quantum theory 1918-27: Adaption by German physicists to a hostile environment', in R. McCormach (ed.), *Hist. Studies in the Phys. Sci.* No. 3, Philadelphia, Penn.: University of Pennsylvania Press, 1971.
Galileo, *Dialogue Concerning the Two Chief World Systems* (1632), trans. by S. Drake, Berkeley: University of California Press, 1953.
Galileo, *Le Opere di Galileo* (20 vols), ed. by A. Favaro, Florence: G. Barbera, 1899-1909. Reprinted 1929-39, 1964-66 and 1968.
Galileo, *Two New Sciences* (1638), trans. by H. Crew and A. de Salvio (1914), New York: Dover Publications, 1954.
Galileo, *Two New Sciences* (1638), trans. by S. Drake, Madison: University of Wisconsin Press, 1974.
Garfinkel, H., *Studies in Ethnomethodology*, Cambridge: Polity Press, 1984 (© 1967).
Grosholz, E., 'Geometry, Time and Force in the Diagrams of Descartes, Galileo, Toricelli and Newton', *PSA* Vol 2, 1988, 237-48.
Gutting, G., *Paradigms and Revolutions*, Notre Dame, Ind.: University of Notre Dame Press, 1980.
Hacking, I. (ed.), *Scientific Revolutions*, Oxford: Oxford University Press, 1981.
Hall, A. R., *From Galileo to Newton 1630-1720* (2nd edn.), New York: Dover Publications, 1983.
Hall, A. R. *The Revolution in Science, 1500-1700*, London: Longman, 1983.
Hall, A. R. and M. B., *The Unpublished Scientific Papers of Sir Isaac Newton,* Cambridge: Cambridge University Press, 1962.
Hall, M. B. (ed.), *Robert Boyle on Natural Philosophy*, Indiana: Indiana University Press, 1965.
Hanson, N. R., *Patterns of Discovery*, Cambridge: Cambridge University Press, 1958.
Hanson, N. R., *Constellations and Conjectures*, Dordrecht: Reidel, 1973.
Harré, R., *Matter and Method*, London, Macmillan, 1964.
Harré, R., *The Philosophies of Science* (2nd edn.), Oxford: Oxford University Press, 1985.
Harré, R. (ed.), *The Physical Sciences Since Antiquity*, London: Croom and Helm, 1986.
Hobbes, T., *Leviathan* (1651), Harmondsworth: Penguin Books, 1980.

Hesse, M., *Forces and Fields: The Concept of Action at a Distance in the History of Physics*, Totowa, New Jersey: Littlefield, Adams and Co., 1965.
Hesse, M., 'Changing concepts and stable order', *Social Studies of Science*, iv, 1986 14-26.
Hesse, M., *Revolution and Reconstruction in the History of Science*, Sussex: Harvester, 1980.
Hoch, P., 'A Historical Philosophy of Science?', *Hist. Sci.*, xxviii, 1990, 218-19.
Hollis, M. and Lukes, S. (eds.), *Rationality and Relativism*, Oxford: Blackwell, 1982.
Holton, G., *Thematic Origins of Scientific Thought*, Cambridge (Mass.): Harvard University Press, 1973.
Hooykaas, R., *Religion and the Rise of Modern Science*, Edinburgh: Scottish Academic Press, 1972.
Hume, D., *A Treatise of Human Nature* Book 1 (1739), ed. by D. G. C. McNabb, London: Fontana/Collins, 1962.
Jacob, M. C., *The Cultural Meaning of the Scientific Revolution*, New York, Knopf, 1988.
Jacob, M. C., *The Newtonians and the English Revolution 1689-1720*, Hassocks: Harvester Press, 1976.
Jardine, L., *Francis Bacon: Discovery and the Art of Discourse*, Cambridge: Cambridge University Press, 1974.
Kline, M., *Mathematics in Western Culture*, New York: Oxford University Press, 1953.
Koyré, A., *From The Closed World to The Infinite Universe*, Baltimore: John Hopkins Press, 1968.
Koyré, A., 'Galileo and Plato', in P. P. Wiener and A. Noland (eds.), *Roots of Scientific Thought*, New York: Basic Books, 1957.
Koyré, A., *Galileo Studies*, Sussex: Harvester Press, 1978.
Kragh, H., *An Introduction to the Historiography of Science*, Cambridge: Cambridge University Press, 1987.
Krige, J., *Science, Revolution and Discontinuity*, Sussex: Harvester Press, 1980.
Kuklick, H., 'Mind Over Matter', *HSPS* 26:2, 1995.
Kuhn, T. S. *The Essential Tension*, Chicago: University of Chicago Press, 1977.
Kuhn, T. S., *The Structure of Scientific Revolutions* (2nd edn.), Chicago: University of Chicago Press, 1970.
Lakatos, I., *The Methodology of Scientific Research Programmes*, Cambridge: Cambridge University Press, 1978.
Lakatos, I. and Musgrave A., *Criticism and the Growth of Knowledge*, Cambridge, Cambridge University Press, 1965.
Latour, B., *Science in Action: How to Follow Scientists and Engineers through Society*, Cambridge (Mass.): Harvard University Press, 1987.
Latour, B. and Woolgar, S., *Laboratory Life: The Social Construction of Scientific Facts*, London: Sage, 1979.
Laudan, L., *Progress and its Problems*, London: Routledge and Kegan Paul, 1977.
Laudan, L., *Science and Hypothesis*, Dordrecht: D. Reidel, 1981.
Laudan, L., *Science and Value*, Berkeley: University of California Press, 1984.
Levere, T. H. and Shea, W. R., *Nature, Experiment, and the Sciences*, Dordrecht: Kluwer Academic Publications, 1990.
Lindberg, D. and Westman R. S. (eds.), *Reappraisals of the Scientific Revolution*, Cambridge: Cambridge University Press, 1990.
Lovejoy, A. O., *The Great Chain of Being*, Cambridge (Mass): Harvard University Press, 1976.
Lynch, M., *Scientific Practice and Ordinary Actions: Ethnomethodological and Social Studies of Science*, Cambridge: Cambridge University Press, 1993.
Mach, E., *The Science of Mechanics*, La Salle, Ill.: Open Court, 1960.
Machlachlan, J., 'A Test of an 'imaginary' experiment of Galileo's', *Isis*, 64, 1973, 375-79.
Mannheim, K., *Ideology and Utopia*, New York: Harcourt, Brace and World, 1936.
Maxwell, N., *The Comprehensibility of the Universe*, Oxford: Oxford University Press, 1998, forthcoming.
Maxwell, N., *From Knowledge to Wisdom*, Oxford: Blackwell, 1984.
Maxwell, N., 'Induction and Scientific Realism: Einstein Versus van Fraassen Part One: How to Solve the Problem of Induction', *Brit. J. Phil. Sci.* 44, 1993, 61-79 and 'Part Two: Aim-Oriented Empiricism and Scientific Essentialism', 81-101.
Maxwell, N., 'The Odd Couple: An Enquiry into the Crisis in the History and Philosophy of Science and its Role in Obstructing the Resolution of Global Problems', unpublished.

Maxwell, N. 'The Rationality of Scientific Discovery: Parts One and Two', *Phil. Sci.* 41, 1974, 124-53 and 247-95.
McGuire, J. E. and Rattansi, P. M., 'Newton and the Pipes of Pan', *Notes and Records of the Royal Society,* 21, 1966, 108-114.
McMullin, E. (ed.), *Construction and constraint: The shaping of scientific rationality*, Notre Dame, 1988.
McMullin, E. (ed.), *Galileo, Man of Science*, New York: Basic Books, 1967.
Meghill, A. (ed.), *Rethinking Objectivity*, Durham NC: Duke University Press, 1994.
Merton, R. K., 'Science, Technology and Society in Seventeenth-Century England', *Osiris* 1V, 1938. 332-360.
Nickles, T. (ed.), *Scientific Discovery, Logic and Rationality*, Dordrecht: D. Reidel, 1980.
Nickles, T. (ed.), *Scientific Discovery: Case Studies*, Dordrecht: D. Reidel, 1980.
Newton-Smith, W. H., *The Rationality of Science*, London: Routledge and Kegan Paul, 1981.
Pagel, W., *Paracelsus: An Introduction to Philosophical Medicine in the Era of the Renaissance*, Basel and New York: Karger, 1958.
Pickering, A., 'Knowledge, Practice and Mere Construction', *Social Studies of Science*, Vol. 20, London: Sage, 1990.
Pickering, A., (ed.), *Science as Practice and Culture*, Chicago: University of Chicago Press, 1992.
Pickstone, J., 'Past and Present Knowledge in the Practice of the History of Science', *Hist. Sci.*, xxxiii, 1995.
Pitt, J. C., 'Galileo, Rationality and Explanation', *Phil. Sci.* 55, 1988, 87-103.
Polyani, M., *Personal Knowledge: Towarda a Post-Critical Philosophy*, London: Routledge and Kegan Paul, 1958.
Polyani, M., *The Scientific Imagination*, London, Routledge and Kegan Paul, 1966.
Popper, K., *Conjectures and Refutations* (4th edn.), London: Routledge and Kegan Paul, 1972.
Popper, K., *The Logic of Scientific Discovery* (3rd rev. edn.), London: Hutchinson, 1983.
Popper, K., *The Poverty of Historicism*, London: Routledge and Kegan Paul, 1960.
Porter, R. and Teich, M. (eds.), *The Scientific Revolution in National Context*, Cambridge: Cambridge University Press, 1992.
Quine, W. V., *From a Logical Point of View*, Cambridge (Mass.): Harvard University Press, 1953.
Quine, W. V., *Word and Object*, Cambridge (Mass.): Harvard University Press, 1960.
Randall, J. H., *The Career of Philosophy* Vol 1, New York and London: Columbia University Press, 1962.
Raven, D. T. *et al* (eds.), *Cognitive Relativism and Social Science*, New Brunswick: Transaction, 1992.
Righini, B. and Shea, W., *Reason, Experiment and Mysticism in the Scientific Revolution*, New York: Science History Publications, 1975.
Russell, C. A., *Science and Social Change, 1700-1900*, London: Macmillan, 1983.
Sachs, M., 'Maimonides, Spinoza and the field concept in physics', *J. Hist. Ideas*, 37, 1976, 125-131.
Sambursky, S., *The Physical World of the Greeks*, London: Routledge and Kegan Paul, 1963.
Sargent, R., *The Diffident Naturalist,* Chicago: University of Chicago Press, 1995.
Shapere, D., *Galileo: A Philosophical Study*, Chicago: University of Chicago Press, 1974.
Shapin, S., 'Discipline and Bounding: The History and Sociology of Science as Seen Through the Externalism-Internalism Debate', *Hist. Sci.*, xxx, 1992.
Shapin, S., 'Here and Everywhere: Sociology of Scientific Knowledge', *Annu. Rev. Sociol.* 1995.
Shapin, S., 'The House of Experiment in Seventeenth-Century England', *Isis* 79, 1988, 373-404.
Shapin, S., *A Social History of Truth: Civility and Science in Seventeenth-Century England*, Chicago: University of Chicago Press, 1994.
Shapin, S. and Schaffer, S., *Leviathan and the Air Pump: Hobbes, Boyle and the Experimental Life*, Princeton: Princeton University Press, 1985.
Shapiro, A., *Fits, Passions, and Paroxysms: Physics, Method, and Chemistry and Newton's Theories of Colored Bodies and Fits of Easy Reflection*, Cambridge: Cambridge University Press, 1993.
Shea, W., *Galileo's Intellectual Revolution*, London: Macmillan, 1972.
Singleton, C. S., *Art, Science and History in the Renaissance*, Baltimore, 1968.
Stein, H., 'Newtonian Space-Time', *The Annus Mirabilis of Sir Isaac Newton, The Texas Quarterly*, Autumn 1967, Vol. X, no. 3.
Stewart, L., 'The Selling of Newton', *J. of Brit. Studies*, 25, 1986, 178-92.

Strong, E. W., *Procedures and Metaphysics*, Berkeley and Los Angeles: University of California Press, 1936.
Stuewer, R. H., *Historical and Philosophical Perspectives of Science*, New York: Gordon and Breach, 1989.
Sturchio, J., 'Artifact and Experiment', *Isis* 79. 1988, 367-72.
Taylor, A. E., *Aristotle* (rev. edn. 1919), New York: Dover Publications, 1955.
Tribby, J., 'Cooking (with) Clio and Cleo: Eloquence and Experiment in 17th Century Florence', *Jour. Hist. Ideas*, 52, 1991.
Tribby, J., 'Club Medici: Natural experiment and the imagineering of Tuscany', *Configurations,* 2, 1994.
Visser, R. P. W., *et al* (eds.), *New Trends in the History of Science*, Amsterdam: Rodopi B. V., 1989.
Wallace, W., *Galileo and his Sources: The Heritage of the Collegio Romano in Galileo's Science*, Princeton: Princeton University Press, 1984.
Wallis, R., *On The Margins of Science: The Social Construction of Rejected Knowledge*, University of Keel, 1979.`
Webster, C., *The Great Instauration*, London: Duckworth, 1975.
Westfall, R. S., *Construction of Modern Science*, New York: John Wiley, 1971.
Whitehead, A. N., *Science and the Modern World*, New York: Macmillan, 1925.
Wilson, H. T., *The American Ideology: Science, Technonlgy, and Organisation as Modes of Rationality in Advanced Industrial Societies*, London: Routledge and Kegan Paul, 1977.
Wilson, A. and Ashplant, T., 'Whig History and Present-Centred History' and 'Present-Centred History and the Problem of Historical Knowledge', *Hist. Jour.*, 31, 1, 1988, 1-16 and 253-74.
Winch, P., *The Idea of a Social Science and its Relation to Philosophy*, London: Routledge and Kegan Paul, 1958.
Wisan, W., 'Galileo and the Process of Scientific Creation', *Isis* 75, 1984, 269-86.
Wisan, W., 'The New Science of Motion: A Study of Galileo's *De motu locali*', *Arch. Hist. Exact Sci.* 13, 1974 103-306.
Wittgenstein, L., *Philosophical Investigations*, New York: Macmillan, 1953.
Woolgar, S., 'Interests and explanation in the social study of science', *Soc. Studies Sci.* 11, 1981, 365-94.
Wolpert, L., *The Unnatural Nature of Science*, London: Faber and Faber, 1992.
Yates, F., *Giordano Bruno and the Hermetic Tradition*, London: Routledge and Kegan Paul, 1964.
Yearley, S., 'The relationship between epistemological and sociological cognitive interests: some ambiguities underlying the use of interest theory in the study of scientific knowledge', *Studies Hist. Philos. Sci.* 13, 1982, 353-88.