Whatever Should We Do With Cartesian Method? —Reclaiming Descartes for the History of Science

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I. The Cult of Method in the History of Science and Cartesian Studies

Until relatively recently, interpretations of the Scientific Revolution have tended to be dominated by heroic tales of the discovery, perfection and application of the scientific method. Descartes, Bacon, Galileo, Harvey, Huygens and Newton were singularly successful in persuading posterity, historians of science included, that they contributed to the invention of a single, transferable and efficacious scientific method. The earliest systematic studies of the history and philosophy of science, the writings of d'Alembert, Priestley, Whewell and Comte, attempted to distil from the historical progress of science a sense of that method, so that its further perfection and wider application could insure the future growth of the sciences[1]. Earlier in this century, pioneer professional historians of science, such as George Sarton and Charles Singer, saw the elucidation of the scientific method as one of the chief functions of the study of the history of science.[2] Subsequently, a thriving sub-discipline of the history of science concerned itself with the history of methodological ideas in (supposed) relation to the larger course of the history of science,[3] and later, Karl Popper, Imre Lakatos and their followers sought to revive the link between theorizing about the purported scientific method and re-writing a 'method-centric' history of science.[4]

The treatment of Descartes' method by historians of science and historians of philosophy has been no exception to this pattern. The Discours de la méthode has been seen as one of the most important methodological treatises in the Western intellectual tradition, and Cartesian method has been viewed as doubly successful and significant within that tradition. Firstly, Descartes' method has been taken to mark an early stage in that long maturation of the scientific method resulting from interaction between application of method in scientific work and critical reflection about method carried out by great methodologists, from Bacon and Descartes down to Popper and Lakatos. [195] Secondly, Descartes' own considerable achievements in the sciences and in mathematics during a crucial stage of the Scientific Revolution of the Seventeenth Century have been taken to have depended upon his method.

The aim of much research on Cartesian method is serious, scholarly, 'apologetic' exegesis: the analysis and explanation of how and why Descartes' well-omened methodological enterprise came to pass. Just as all Christian apologists believe in God, so apologists for Cartesian method agree on the basic aim of elucidating, historically and philosophically, what was in principle and in practice a triumph of an efficacious method. To be sure, differences over minor points of interpretation and emphasis have arisen. Just as Christian apologists differ over points of biblical exegesis, so, as I have argued elsewhere, apologists for Cartesian method fall into broad camps: there are naive literalists, sophisticated hermeneutical exegetes, and those whose belief takes a dry sceptical turn [5].

This paper is motivated by some news which will probably be unwelcome amongst method cultists: we now have excellent grounds for being 'atheists' about method. Although the message has not yet spread very widely, some historians, philosophers and sociologists of science have established that no doctrine of method, whether Descartes' or anybody else's, ever has guided and constituted the actualities of scientific practice -- conceptual and material --in the literal ways that such methods proclaim for themselves.[6] From this perspective it follows that apologetic scholarship directed to Descartes' method is misguided, not so much in its separable scholarly detail, but certainly in its view of science, of method, and of their intertwined histories. And it further follows that in so far as biographical writing about Descartes is a function of the larger historiographies of method and of science, it too requires reformation.

As an historian of science of this peculiarly atheistical bent, my intention is to reclaim Descartes as a de-mystified object of study in my field. Since the cult of method and the apologetic Cartesian scholarship block that possibility, I seek the tools of demystification within those developments in the historiography of science and the related field of sociology of scientific knowledge just mentioned.

My argument will proceed as follows: In section II we locate the grounds of modern 'atheism' about method in the history of science in the writings of Koyré, Kuhn and Bachelard. Following a brief outline in Section III of the 'core' of Descartes' methodological claims, we examine in Section IV an example of de-methodologized, 'post-Kuhnian' analysis of Cartesian scientific practice. Sections V and VI then analyse method doctrines, Descartes' included, utilizing the previous example and demonstrating how the discursive structures of method theories guarantee their lack of efficacy and their creation of literary illusions of that very efficacy. With that model of discursive structure and dynamics in hand, Section VII turns to the issue of some of the micro-political and rhetorical functions of Cartesian method discourse, and Section VIII sketches an account of Descartes' early career, premised upon our post-Kuhnian findings about the nature of method and its roles in the history of science. [196]

II. Toward Methodological Atheism: Koyré, Bachelard and Kuhn on the History of Science

It is becoming increasingly clear to some historians and sociologists of science that the traditional belief in the existence of a single, transferable, efficacious scientific method is highly dubious. The work of Alexandre Koyré, Gaston Bachelard and Thomas S. Kuhn especially pointed in this direction, although only lately have their insights been followed up in attempts to revise the 'believer's' historiography of method.

Although Koyré firmly believed in scientific progress, he did not consider it the product of applying a general scientific method. Rather, for Koyré, progress depended upon the adoption of appropriate metaphysical presuppositions and the pursuit of science within them. His classic example was Galileo's mechanics, which, he argued, owed nothing to any methodological achievement, but issued from Galileo's brilliance in working and arguing his case within the framework of a loosely 'Platonic', mathematical metaphysics. Similarly, Aristotelian physics had not failed for lack of a method, but largely because it had had the wrong conceptual presuppositions, ones too close to untutored commonsense about motion.[7] The point for Koyré was that a general, transferable method is neither necessary nor sufficient for the pursuit of science. "No

science has ever started with a treatise on method and progressed by the application of such an abstractly derived method," Koyré intoned, commenting on the Discours, and at least some historians of science have tended, correctly, to agree.[8]

Bachelard's early work slightly pre-dated that of Koyré, and seems to have been subtly refracted in the thinking of both Koyré and Kuhn. In this process Bachelard's scepticism about method was not brought to the fore, and even with the wider dissemination of his writings over the past twenty years, the implications of his work for undermining the cult of method have not been sufficiently articulated. However, those implications are quite clear in the core of his work.

For Bachelard, each field of science consists in a set of interlinked, mathematicized concepts which interact dialectically with the instrumentalities through which the concepts are objectified and materialized.[9] To paraphrase Bachelard, the meaning of a concept must include the technical conditions of its material realization.[10] When a science is created, an artificial technical realm comes into being, in which phenomena are literally manufactured under the joint guidance of the system of mathematicized concepts and the instruments and experimental hardware in which those concepts have been realized. In an ironic jibe at positivist dogma, Bachelard termed any such realm of theoretically dominated artificial experience a 'phénoméno-technique', thus signifying that the phenomena of science are not discovered but made, not natural but artificial, being created and commanded in the light of theory and theory-loaded instruments. In Bachelard's view, therefore, each science is unique and self-contained; each has its own specific system of concepts and related instrumental [197] armoury. No single, transferable, general scientific method can explain the genesis of any science or its contents and dynamics.

Kuhn, too, can hardly be said to have focused upon the demystification of method in his theoretical or historical writings. But, as with Koyré and Bachelard, there is in Kuhn a clear denial of the role traditionally ascribed to method, and that denial relates directly to the major premises of his position. In effect, Kuhn's approach vastly strengthened Koyré's assertion that grand set-piece doctrines of method are irrelevant to the practice of the sciences. The key point resided not in Kuhn's conception of 'scientific revolutions', but rather was implicit in his view of routine, 'normal', 'puzzle-solving' research within a 'paradigm'.

As is well known, a Kuhnian paradigm is that entire discipline-specific culture which at a given time governs cognition, action and evaluation within a given mature field of scientific inquiry. For Kuhn a paradigm consists first of all in a 'metaphysics', a set of deep conceptual presuppositions, which need not be of Koyré's Platonic type. A paradigm also contains the central concepts and law sketches of the field, and all the instrumental hardware and experimental procedures considered relevant to the posing and solving of problems within the paradigm. Kuhn stresses the theory-loading, or, more precisely, the paradigm-loading of the instruments and procedures. Standards and norms for the adequate use of instruments and procedures are also part of the paradigm, being inherent in the theoretical and craft training necessary to become proficient in paradigm-based research. One learns these and other parts of the paradigm through a course of practice on piecemeal, already solved problems--'paradigms' in the narrow sense (later designated 'exemplars'), bearing some relation to Bachelard's phénoméno-techniques. There is also a negotiable pecking order of unsolved problems and their correspondingly negotiable degrees of 'significance' or 'anomalousness', which forms a resource for selecting, shaping and evaluating courses of research and their results. [11]

Assuming that such paradigms, or anything like them, guide normal research in the various sciences, it then becomes highly unlikely that some single method guides the history of the sciences, individually or collectively. The elements making up a particular paradigm, and hence making possible a particular tradition of research, are unique to that field and are a sufficient basis for its practice. Moreover, if each field has such a unique and self-contained conceptual fabric and associated mode of practice, then it is irrelevant to our understanding of its cognitive dynamics to re-describe, gloss or otherwise 'account' for them by the use of heroic tales of method. This point also holds for *all* the sciences existing at any moment: Each has its own particular paradigm, and whilst neighbouring or cognate fields might share certain paradigm elements in common, there is no reason to assume, as methodological accounts must, that there is some identity or long term convergence among paradigms.

The radical anti-methodism which can be extracted from Kuhn's position is illustrated in Figure 1. Any given field of science has at any given moment [198] its own paradigm, its own versions of the generic elements displayed in the matrix: (a) basic concepts and law sketches; (b) metaphysics; (c) tools and instrumentalities (including the theories and standards thereof); (d) standards of relevance and of adequacy for the selection of problems and for the formulation and evaluation of knowledge claims; (e) disciplinary goals of any internally or externally generated sort; (f) concrete achievements, exemplars, instantiating laws, concepts and standards. [**Figure 1**]

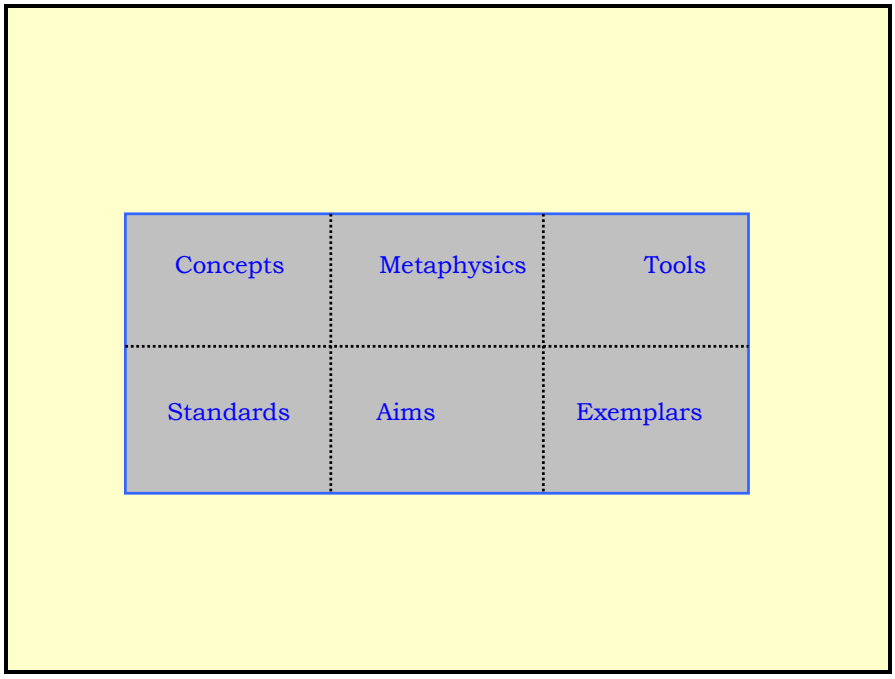

Figure 1 The Kuhnian Disciplinary Matrix of Elements in a Paradigm

At any given moment the domain of the sciences may then be represented as in **Figure 2**, where we have n sui generis fields, each with its own particular constellation of matrix elements, constituting for the time being its own paradigm. The sciences are thus many, not one. True, neighbouring and cognate fields may share certain elements in common; concepts in one field may be taken up (under translation) as tools in another; or, groups of fields may have emerged under the aegis of a common

metaphysical umbrella. But none of this argues the identity or even the long term convergence among paradigms.

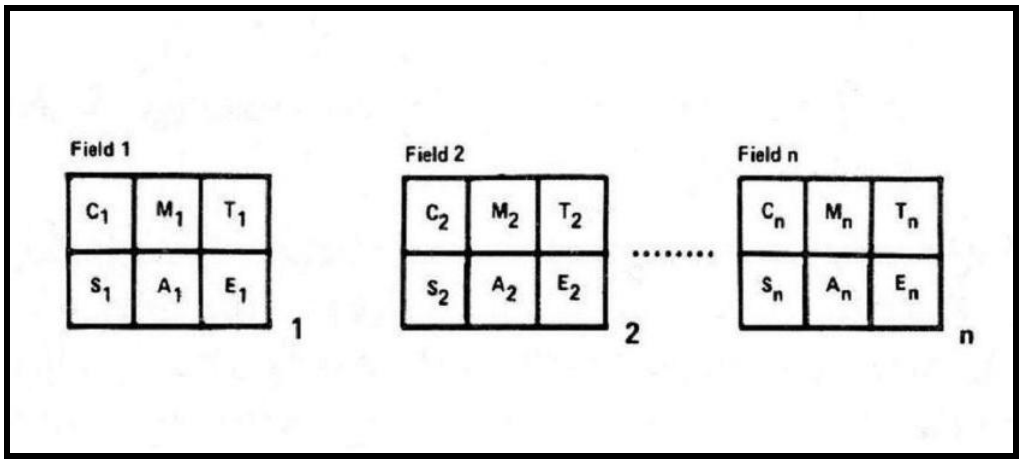

Figure 2 A set of n coexistent, sui generis paradigms or n conceptually and materially sui generis research traditions at a given moment of time

In Kuhnian terms each field has its own 'method(s)', inextricable from the contents and dynamics of its paradigm at that moment. But, to speak of some putatively common, transferable, efficacious scientific method or epistemology--Baconian, Cartesian, Newtonian, Popperian--is merely to float above the lived, thought and practised life of each of the sciences, and fallaciously to substitute an externally prompted discourse for the dense cultures of the several paradigms. There are, in short, no unified and literally applicable methods. No method discourse corresponds to or maps onto any given domain of scientific practice, let alone a number of such domains.

Recent work in the sociology of scientific knowledge has deepened all these claims, by in effect unfreezing Kuhn's metaphor of routine 'puzzle solving', and suggesting that even in normal research there is a constant, subtle revision and negotiation of the elements in the paradigm.[12] This is because normal research always involves bids to make small, but significant, alterations in the prevailing disciplinary objects of inquiry. Such bids exert feedback effects on some of the elements of the paradigm--conceptual, instrumental, evaluative--if they are successful. So, normal science may be 'puzzle solving', but it is a peculiar version of that activity, because the pieces, the rules of assembly and the ultimate 'picture' keep changing as the players play and negotiate.[13] And, if disciplinary 'method' is inextricable from a particular paradigm, now it is also in flux, inextricable from the socio-cognitive dynamics of the field. Again, no general doctrine of method can command or describe this situation. [199]

Indeed, the post-Kuhnian case against method does not stop here. Increasingly historians and sociologists of science are examining the social and political organization of normal fields and communities. If a field is not in the grip of a total and immobilizing consensus (until the next 'revolution'), and if 'significant' research is always a negotiated outcome subtlely altering the state of disciplinary play, then a normal field must have a social and political life sufficient for the carrying out of these knowledge-making and knowledge-breaking manoeuvres, and for keeping them, most of the time, within the accounted realm of the 'non-revolutionary' (hence acceptable and 'non-cranky'). Accordingly, attention has shifted to the micro-sociology and micro-politics of scientific specialty groups to see how they manage, negotiate, refine, accept

and reject bids to modify the paradigm, i.e. bids to have accomplished 'significant' results. In this view the 'method' of a discipline is not simply identified with its own particular paradigm, but further with the political and social structure and dynamics of the specialist community. The construction of scientific knowledge cannot be explained apart from the social processes in and through which that activity takes place.[14] So, again, no invocation of a general method can explain the manufacture and transformation of knowledge by paradigm-bearing and paradigm-negotiating communities, including the historically contingent socio-political structures of those communities. Method discourse abstracts from and floats above the proper cognitive and social complexity of scientific fields, and so it misses everything that now appears to be of importance in understanding the dynamics of the sciences.

The Koyré-Bachelard-Kuhn debunking of method offers both opportunities and pitfalls to the historian of science, including the historian of Cartesian science. Before we can explore these we need to establish what, in the remainder of this paper, will be meant by the term 'Cartesian method'.

III. Understanding the Core of Descartes' Method Discourse

The core of Descartes' method, his central methodological claims, is to be found in the Discours and in the Regulae, where Descartes offers formal, systematized versions of his method. As I have argued elsewhere, this core of Descartes' method discourse, offered in Regulae II through XI and in the Discours, consists in three fundamental premises.[15] [200]

> 1. All rationally obtainable truths subsist in a network of deductive linkages, and this is the meaning of the unity of the sciences. [This will henceforth be termed Descartes' 'latticework' vision of the unity of the sciences].
>
> 2. As rational beings, humans possess two divinely given faculties for the attainment of truth; the power of intuiting individual truths, and the power of deducing valid links between them.
>
> 3. A single mind, exercising intuition and deduction, could in principle traverse the entire latticework; but, some help is required in the form of practical hints or suggestions, heuristic rules, to aid in the preparation of inquiries, the ordering of inquiries, and the checking up after inquiries.

Therefore, there are two complementary moments or aspects within the statement of the rules of the method. Firstly there is a doctrine of truth. On the one hand, it informs us of what we presumably already know--that we can intuit and deduce truths. On the other hand, it adduces some negative heuristic advice from this fact: trust not in any authority, nor in unclear, indistinct belief, will or emotion; avoid precipitation and hasty judgement; go only as far as intuition and deduction reveal the truth. All this is essentially contained in rule 1 of the Discours and Regulae III and IV. Secondly, there is an open ended set of heuristic rules, initially gathered from easy excursions around parts of the latticework of knowledge. These are contained in part in rules 2, 3 and 4 of the Discours and Regulae V to XI. We shall meet some of these later.

In the Regulae, as compared to the Discours, we meet an elaborate explication of the vision of the latticework of rational truths, an explication that is crucial in understanding Descartes' method claims about specific cases of scientific and

mathematical practice. Descartes tells us in Regulae VI that the logical chains of truths consist in 'absolute' terms linked to a 'series' of 'relative' terms through a greater or smaller number of rationally specifiable 'relations' (respectûs). Absolute terms are the initial terms in particular deductive series, and they are themselves relative to a small set of what might be termed 'absolutely absolute terms'. Relative terms, properly so called, are those occurring further down deductive series. In some degree they 'share the same nature' as their antecedents, the absolutes; but, they also involve complex conditioning factors or 'relations'. Relatives are distanced from their absolute to the degree that they contain more 'mutually dependent relations subordinated one to another'.[16]

This sketch of the core will aid our analysis of the discursive structure and function of Descartes' method discourse in sections V and VI below.

IV. An Example of post-Kuhnian Historiography of Science: Descartes' Construction of the Law of Refraction and its Methodological Fairy Tale

Koyré, Bachelard, Kuhn and the post-Kuhnian sociologists of scientific knowledge offer historians of science the opportunity to reconstruct courses of scientific practice free from overriding fairy tales about the literal efficacy [201] of scientific method. Certainly no progress can be made in understanding the natural philosophical career of Descartes and his place in the Scientific Revolution, unless we learn to explicate his science independently of his fable of method. In this section we examine an example of the distance between Cartesian method rhetoric and an arguably quite plausible reconstruction of one course of his scientific practice. This exercise in reconstruction will later serve as a building block for our attempt to grasp the structure and dynamics of Descartes' (or anyone else's) method discourse.

Our example concerns Descartes' discovery or construction of the law of refraction of light, Snel's law, and his attempt to explain the law by means of a mechanical theory of light. That theory states that light is a mechanical impulse of variable 'force' transmitted instantaneously through continuous optical media. This greatest of Descartes' scientific achievements has long been the subject of mythical explanation, starting predictably with Descartes himself. In Regulae VIII he gives an account of how both the law and the theory could have been discovered using the method, although he uses the subjunctive mood and does not actually claim to have done it this way.[17] In Descartes' story application of the method amounts to the sagacious posing of a series of heuristic questions for research against the background of the core method doctrines outlined above. The answers to the questions unfold the best course of research to be followed, surely a sound sense of method as heuristic aid.

The first step is to see that the discovery of the law will depend upon the relationship between the angle of incidence and the angle of refraction. At this point, Descartes observes, a mathematician must give up the search, for all he can do is assume some relation and work out the consequences. The method shows that the problem depends upon knowledge of physics as well, for the relation of the two angles depends in some way upon the manner in which light actually passes through media. But the answer to that question is seen to depend on the more general issue of "what is the action of light?", and the answer to that in turn supposedly depends on the answer to the ultimate question of "what is a natural power?" We must, by a "mental intuition", determine what this 'absolute nature' is. (In fact, we know it will turn out to be mechanical action-

-impulse or pressure; but, God, literally, knows how we arrive at that intuition.) In any case, having intuited this basic "nature", we then proceed back down the chain of questions, "deducing" the more relative natures from the less relative ones. We may stall somewhere along this route, for example, in trying to deduce the nature of light from the nature of natural powers in general. In that case, we proceed by analogy; but, again, we know that we are not to worry, for the analogies can only be to other forms of mechanical action, and that narrows the field quite a bit. Ultimately this synthesis leads, through a theory of light, to the law of refraction. In the Dioptrics of 1637 Descartes presents a model for the theory of light: he talks of tennis balls rather than impulses, and he pretends to deduce the law of refraction from the central elements of the model and his principles of motion.

Leaving this story aside, what can the sceptical post-Kuhnian historian of [202] science say about how Descartes may have come to construct the law of refraction and devise the model and the theory? My view of this is that he did it by being in the first instance a good practitioner of traditional geometrical optics, by working, that is, well within the 'paradigm' of that field. The law was constructed by using data and principles available within the traditions of the field. The key principle used concerned the location of images of bodies seen under refraction. It assumed that the images of point sources could be determined on the basis of the behaviour of one ray only. Interestingly, this assumption had been seriously threatened in the new theory of vision of Kepler, published in 1604, and which Descartes had read, at least in parts. So, according to my reconstruction, in 1626/7 Descartes, using nothing better than the traditional (cooked) data and this very possibly obsolete principle, constructed the law in a trigonometric form somewhat different from that we use today. With the law to hand, he then moved to cover it with a mechanical theory.[18]

To understand how he constructed his mechanical theory, we have to recall first of all that since 1619 Descartes had been largely committed to a corpuscular-mechanical ontology, and that he had dabbled with a qualitative mechanical theory of light at that time. This early qualitative theory contained assumptions which would have hindered rather than facilitated the search for the law. What seems to have happened was this: having found the law by traditional means, Descartes acted in the light of his most basic scientific commitments, to wit, that the world is basically micro-mechanical, and, that macroscopic mathematical regularities must bespeak underlying micro-mechanical causes. He therefore took the ray diagrams in which the law had first been discerned and he literally read into their parameters manifestations of underlying mechanical causes. (Back in 1619/20 he had done the same thing with hydrostatics diagrams from Stevin and optics diagrams from Kepler.) This permitted him to reformulate his old ideas about the mechanical cause of light and to mould his new ideas precisely to fit the diagrams. To cap it all off, he wrote the methodological 'cover story' to Regulae VIII.[19]

This case illustrates the redundancy of employing methodological stories in attempting to reconstruct courses of scientific practice according to the standards of our post-Kuhnian understanding of science: If we credit the post-Kuhnian story or anything like it, we see that the banalities of Descartes' method story batten upon the prior existence of a dense local disciplinary culture of concepts, techniques, goals and standards--in and through which Descartes worked. Needless to say, in a post-Kuhnian universe of historical discourse, the same points should apply to the reconstruction of every passage of scientific practice in which Descartes engaged.

V. The Structure of Method Discourses (Descartes' Included)

The sort of post-Kuhnian debunking of method just illustrated certainly helps liberate us from the myth of method in the history of science. However, it [203] entails certain difficulties. For example, it suggests that methodologists must be cynics or fools: cynics for advocating doctrines of method which they know are ineffective; or, fools for failing to grasp the wisdom of the post-Kuhnian critique of method. But this clearly will not do. It hardly suits historians of science weaned on Butterfield's The Whig Interpretation of History uncharitably to invent a new whiggish hagiography/demonology of their own discipline. So, the question becomes 'how can we understand the existence of honest and rational believers in method?' This problem first stimulated my own work on method, centring on the question of how it can possibly be that throughout the history of science, methodologists and their audiences have often genuinely believed in the efficacy of method doctrines which we post-Kuhnian 'know' cannot have worked. In short, what is it about systematic method doctrines that sustains their plausibility to believers?

My answer, developed initially in an attempt to facilitate historical research on Descartes, is this:[20] All systematic method doctrines are examples of a determinate species of discourse. The species is characterised by the presence of a certain discursive structure common to all instances of the type. This structure is such that it necessarily defeats the ability of any methodology to accomplish what it literally announces itself to be able to accomplish. At the same time, this same discursive structure sustains a set of literary effects tending to create the illusion that the method in question can indeed accomplish what it claims to be able to do. In other words, all set-piece method doctrines have the same underlying discursive structure which explains their lack of efficacy as well as their ability to create the literary effect that they are efficacious.

Before we look at this structure and its characteristic effects, we must, however, remind ourselves of the fact that all method doctrines encountered in the Western tradition from Aristotle to Popper and beyond are structured around two intertwined metaphors: (1) to acquire knowledge is a matter of establishing a correct subjective grasp, or more typically, vision, of independently existing, objective objects of knowledge; (2) method, drawing on the literal Greek meaning of the term, is the subject's 'way through' to the objects of knowledge, a set of prescriptions as to the path to be followed by the subject in the pursuit of knowledge. All particular method doctrines are attempts to explicate the key metaphors. Indeed, the history of method doctrines is in large measure the history of various and competing attempts to dress these notions in conceptual vestments deemed appropriate to each methodologist's perception of the context of debate and structure of socio-cognitive relevances holding in his time and place.[21] Typically, a new doctrine is fabricated out of bits of older method doctrines, as well as pieces of neighbouring varieties of discourse--theological, natural philosophical, ethical, mathematical, psychological, etc.

Let us now turn to the generic structure of method discourses (**Figure 3a**). [204] Any and all systematic method doctrines consist of and operate upon three interacting levels of discourse. Level I is that of explicit, 'systematized' discourse about the core of any given method doctrine. In any particular method doctrine Level I will consist in (1) generalized [non-discipline specific] statements of the rules of that method, and (2) explicit, more or less systematized, abstract and generalized discourse concerning the canonical themes, 'knowing subjects' and 'objects of knowledge', and how the rules help

them to get together. There is typically present also (3) some discourse on the 'pitfalls', 'obstacles' and 'sources of error' which can deflect a subject, mask or distort the objects or lead to misapplication of the rules. Sometimes Level I is itself packaged within a metaphysical or even theological framework.

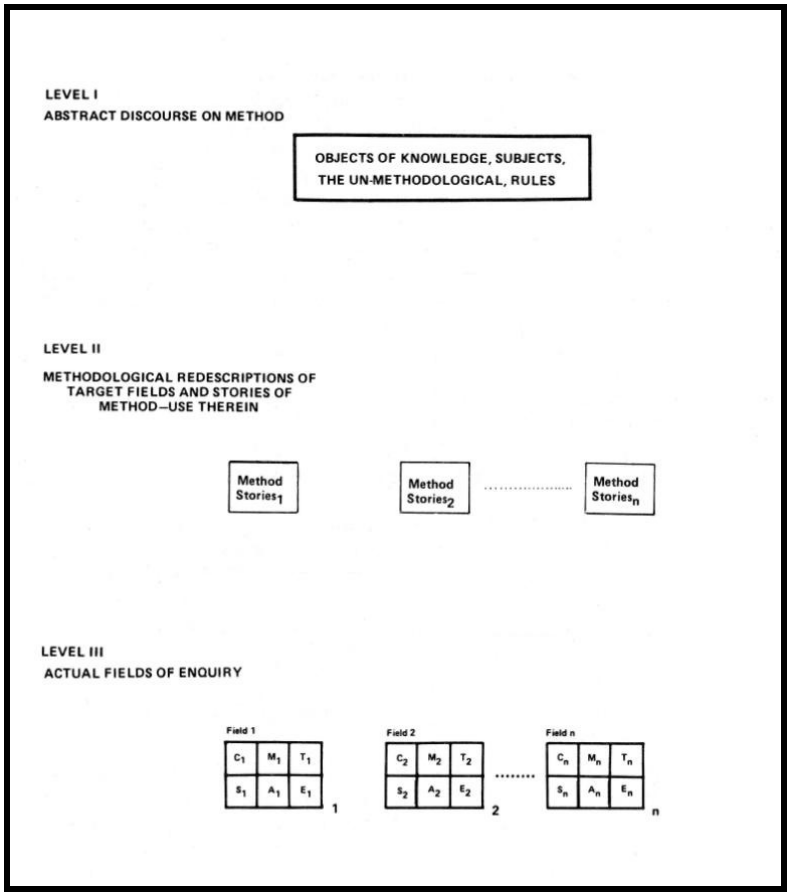

Figure 3a. The Structural Levels In Any Grand Method Doctrine of Method

In Descartes' method we have already discussed this Level I core, as presented in the Discours and Regulae. It includes his teaching concerning intuition and deduction, the conception of the latticework with its intertwined concepts of absolutes, relatives, series and relations, and, in addition, the statement of the rules of the method, [*and a discourse on the causes of error and obstacles to proper procedure*] [22] [205]

Level III consists in the domain of scientific fields and specialties that the method in question claims to command. In Figure 3a this domain, Level III, is represented by inserting Figure 2, which, we recall, is a representation of the domain of scientific fields at any given time, each field viewed in post-Kuhnian perspective, with its unique paradigm signified by a matrix of paradigm contents.

In the case of Descartes' method, Level III should be thought to contain all the scientific traditions, fields or disciplines because they all fall within the claimed scope of the method, along with all mathematical disciplines, and, indeed, all domains of rational inquiry, as opposed to those controlled by faith. We have already looked at one such target field, optics.

Level II consists of a set of 'methodological versions' of the corresponding fields of inquiry represented on Level III. Here one finds methodological accounts or stories

which purport to describe or capture the essence of the practice of the corresponding Level III fields. These stories or accounts are structured in terms of the elements provided by Level I, by the core discourse on 'subjects', 'objects' and rules characteristic of the particular method discourse in question. Such stories or accounts analytically proceed as follows: the 'target' field, the corresponding Level III field, is redescribed or glossed in terms of the elements provided by Level I of this particular method discourse, and an account or story of practice is woven by reference to a subject (conceived in Level I terms) applying the rules within the glossed field. Hence Level II stories and accounts can only exist in so far as they are shaped by deployment of the conceptual resources of Level I of that method. In any given method, the stories on Level II are specific, episodic unfoldings of the conceptual resources provided on Level I as elements in the core discourse of the method.

In the case of Descartes' method we have just met one example of a Level II methodological story corresponding to the target field of optics. This story is couched in terms of the core methodological terms and rules available on Level I of Descartes' discourse on method.[23]

VI. The Literary Effects of Method Discourses (Descartes' Included)

We can now attack the problem of explaining how method discourses, Descartes' included, succeed in creating literary effects of efficacy whilst in fact being structurally incapable of doing what they literally claim to be able to do. The first point to grasp is that the seduction of an historical actor is greatly facilitated if he or she is a member of a culture in which 'scientific method' is generally believed to exist, in practice or in principle. Early Modern figures, such as Bacon and Descartes, moved in an intellectual culture permeated by this belief: the in principle existence of efficacious methods of discovery and proof in mathematics and the sciences was largely unquestioned (except by some sceptics). The task was to devise and enforce the 'correct' general [206] method. In my view, the structural study of the dynamics of method discourse always must be joined to social historical and biographical enquiry into the expectations, aims and discursive resources concerning method available to and/or enforced upon actors in their particular historical circumstances. Analytically speaking, there is the historical problem of explaining the construction or selection of a method by an actor, and then there is the general problem of explaining how a method discourse functions upon an actor, once he is 'inside' it. In the case of Descartes, how and why he formulated his particular method is an historical problem; how his method could be sterile and yet appear not to be is a structural problem it shares with other method doctrines. To explain how and why Descartes could believe in such a method is a function of both enquiries taken together.

Let us now examine the reasonable appearance of efficacy generated by method discourses. The key to the mythological operation of Descartes' or anybody else's method discourse resides in getting the audience, potential reasonable believers, operating on Level II. Recall our case study of Cartesian optical practice and its corresponding methodological tale. According to our new terminology, there is a Level III field of scientific practice and a corresponding Level II methodological account of this target field. Descartes' methodological tale about optics eviscerates and suppresses the specific content and dynamics of his practice in optics, the target field, while the tale itself is spun out of the Level I cloth of core discourse about rules, series, absolutes,

relatives, etc. In fact Descartes' method tale is inscribed by those two processes: (1) the suppression of the real content of optics; and (2) the fabular rendition of the core discourse as a Level II story to replace that content as the methodologically sound 'essence' of the target field. However, whilst post-Kuhnian mythologists of method know all this, historical actors living in a culture of method most probably do not, for they, ex hypothesi, most likely have virtually no discursive resources for explicating and accounting for successful practice in a discipline other than those offered by some method discourse or other. Such a 'believer' is likely to miss the slide between Level III and the method accounts on Level II; indeed, he might not even be aware of it since 'method talk' is his preferred (or only) way of thematizing practice.[24] Once on Level II, however, he is likely to be impressed by the way the Level II account (1) 'applies' the rules of the method (and generally articulates the core concepts of the method), whilst (2) (apparently) constituting an adequate account of what the disciplinary practice is about.

Therefore, [**Figure 3b**] when a reader or listener is confronted with a Level II redescription or story of rule-following, he is in danger of succumbing to two structurally produced literary effects characteristic of systematic method discourses. Firstly, he may be taken in by the 'adequate redescription effect', producing the illusion that Level II redescriptions are in any sense adequate to Level III contents and practices. Secondly, he may be taken in by the 'application effect', producing the illusion that the application of the rules in the Level II story is (or could be) the application of the rules to the practice of the target field (Level III). These effects are structural in the sense that they are made [207] possible and are sustained by the relationships amongst the three levels of discourse. That structural arrangement also explains, as we have seen, why a method discourse, such as Descartes', must be inadequate and ineffective in real practice. In an appropriate cultural environment its upper two levels marginalize or displace the discursive thematizing of the Level III field as such, and pose in its place a desiccated phantom of its actual structure and practice. That phantom, the Level II account, is solidified and underwritten by its 'obvious' congruence with the grandiose, self-proclaimedly authoritative core discourse on Level I.

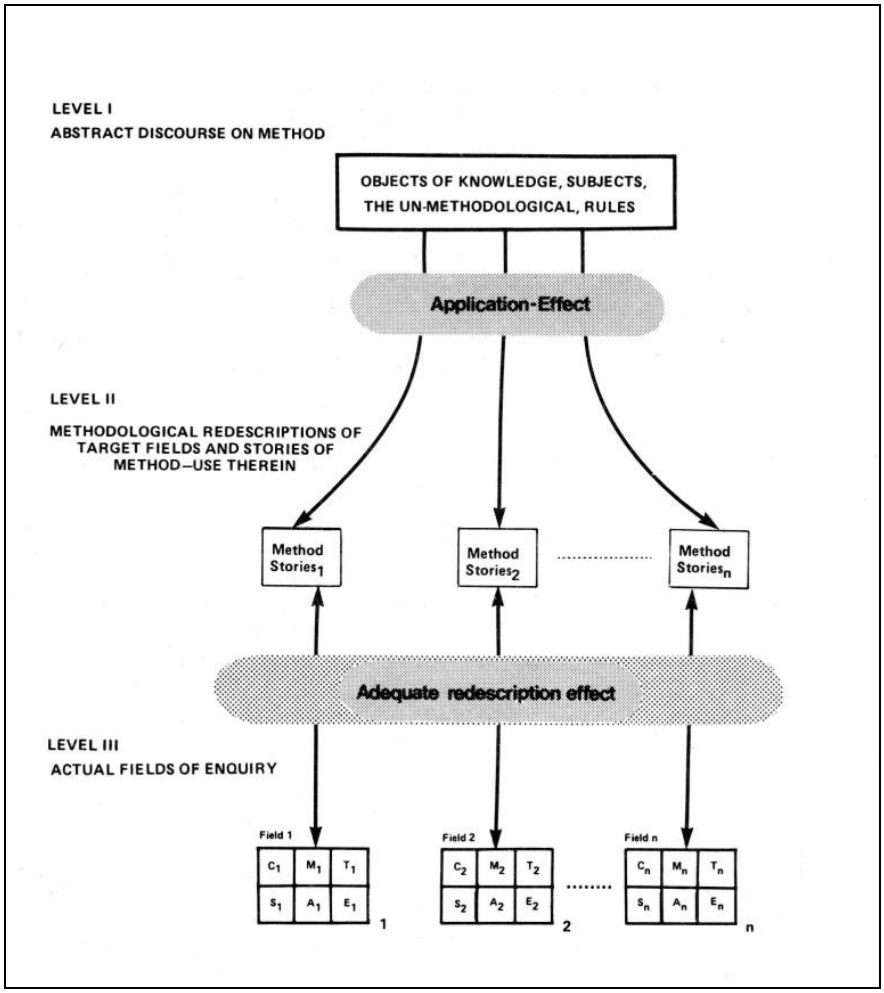

Figure 3b. Location of the First Two Structural Effects of a Grand Method Discourse

These two literary effects are produced by the vertical relations holding amongst the levels of a method discourse. A third literary effect is created horizontally across Level II. This I term the 'unity effect'. [**Figure 3c**] Although there is no space to elaborate here, it is obvious that a method discourse such as Descartes' can generate across Level II a range of redescriptions and stories, each one corresponding to a specific target field (cf. figure 3). For example, I have elsewhere examined Level II method stories corresponding respectively to Descartes' practice of analytical mathematics and the corpuscular-mechanical explanation of magnetism.[25] Each Level II redescription or story will of course be couched in terms of Level I elements, and will involve an account of the application of the rules of the method to the redescribed field. Hence, within a given method discourse all such Level II accounts will appear to be similar. For example, Descartes' three Level II stories about optics, magnetism and mathematics all involve tales of absolutes, relatives, series and the rules. I have argued that this pleasing resemblance amongst Level II stories is productive of the unity effect, the illusion that the terms and rules of the method are applicable across some set of fields of inquiry. But, where a believer is impressed by the fact that all of Descartes' Level II stories articulate and use the same core method concepts and the same rules, a post-Kuhnian mythologist of method is unimpressed for two very good reasons. Firstly, as we have seen, this unity is a unity in vacuity, for each Level II account floats loose of its target field (whilst appearing to appropriate it and grasp its essence). Secondly, the various Level II accounts often generate quite devastating equivocations. For example, I have

shown that Descartes' use of the terms 'absolutes' and 'relatives' in his mathematics story bears no relation whatsoever to their respective denotations in the optics story.[26]

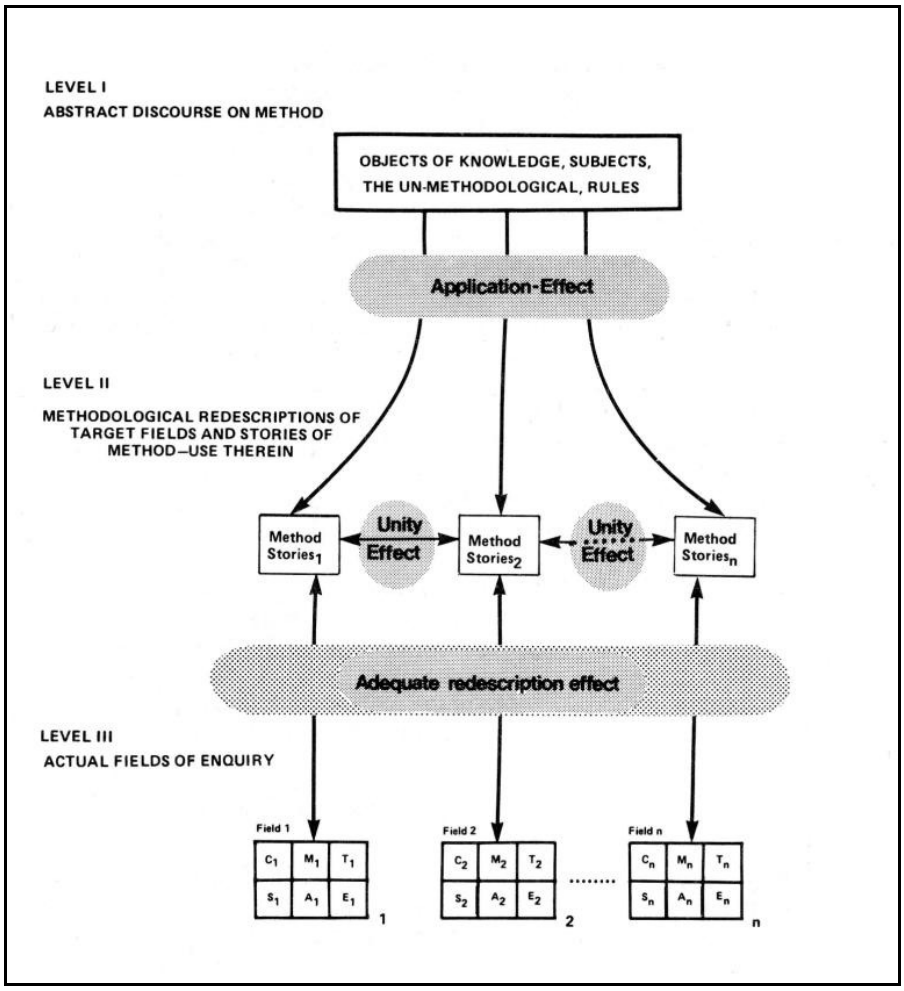

Figure 3c. Adding the Unity Effect to the Application and Adequate Redescription Effects

As in the case of the first two structural effects, the mechanism constituting this third effect also explains why the method cannot actually work in the ways it claims to work: The Level II accounts are similar and the rules of the method gear into them because these accounts are woven out of Level I discourse; but, for that very reason the Level II accounts cannot hope to be adequate glosses of the structure and dynamics of living, Level III fields; they eviscerate those fields in the interests of Level I and still equivocate amongst themselves.

In general, then, the three literary effects of any methodology relate to each other in this manner: In any method discourse the adequate redescription effect is fundamental, and it ultimately depends upon the plausibility of [208] Level II stories within a cultural context according precedence to the Level I discourse as the way of thematizing scientific practice. The application effect depends upon the adequate redescription effect, for it fosters the illusion that the application of the rules on Level II is the application of the rules in actual practice. The unity effect results from the iteration of the application effect across the spectrum of fields thought to be commanded by the method in question, and it is facilitated by the fact that Level II entities bear some analogical relations to each other, despite possible equivocations.

There is even a fourth literary effect of method, which I term the 'progress effect'. Methodologists can proudly point to 'progress' as the method is 'extended'; that is, as new Level II accounts of new target domains are added. Methodologists can also label as 'progress' the revising of existing Level II accounts of old domains in order to grasp and 'explain' new developments in those already methodologized fields. 'Progress' can also be discerned in the discovery and resolution of certain internal problems set in train by the very structure of the method discourse. Often this takes the form of adding to or revising the rules.

One can conclude that any believer seriously engaged in the business of prescriptive methodology will probably stumble into this hall of discursive effects. The believer will then happily expatiate on the unity, applicability, efficacy and progress of this method; he will refine and explicate Level I, his Level II stories and the rules; he will castigate other methodologists, and those who do not believe in methodology; and, he will comment upon all these matters at ever higher levels of meta-discourse. Like other believers, René Descartes got lost in this hall of discursive effects, only to be followed there by many of his loyal scholars. In order to write the scientific biography of Descartes and to understand his role in the Scientific Revolution, one must leave the hall of effects and subject it to the sort of critique begun here.

VII. The Rhetorical Functions of Cartesian and Other Method Discourses

The sceptical historiographies of Koyré and Kuhn effectively debunk method as having no role in the dynamics of the sciences. We shall now see that our discursive model of method entails that methodologies can play some roles in the formation and negotiation of knowledge claims in science, although they cannot play the definitive roles they claim for themselves. Methods do not capture the (non-existent) essences of their target fields; but, they are certainly rather useful resources in the rhetorical combats and political struggles through which knowledge claims come into being, prosper and/or die. This section explores these political and rhetorical functions of method and suggests some ways in which they apply to Descartes' work, in the interest of reclaiming him, and his method, for an historiography of science which neither merely debunks method, nor falls victim to its literary effects.

The work of Paul Feyerabend on the rhetorical and propaganda functions [209] of Galileo's and Newton's methodological pronouncements began to point toward the political functions of method discourse in the life of the sciences.[27] His initiative has been extended in an emerging literature within the history and sociology of science that is beginning to capitalize on the 'post-Kuhnian' challenge to explain what method discourse does in the sciences, if it does not and cannot do what had traditionally been claimed for it. Broadly speaking, this new work suggests that method discourses are often deployed as rhetorical weapons in those negotiations and struggles over the framing and evaluation of knowledge claims which go on at all levels of scientific activity, from the laboratory bench, through published texts, to disciplinary debate and its necessarily associated micro-politics of groups and institutions.[28]

Let us first consider what the 'rhetorical' function of method discourse means at the level of the formulation of technical arguments and knowledge claims. Some historians of science and sociologists of scientific knowledge plausibly claim that technical scientific arguments, even in published form, are pieces of practical, rather than formal reasoning, more akin to legal briefs than to chains of strictly valid inferences. The

burden of a scientific argument is, typically, to promote some novel, or revised, claim about the 'objects of inquiry' within a given field.[29] To that end various resources may be deployed: Appeals are made to theory- and standard-laden data; claims are made about the objects, tools and techniques currently accepted in the field; and, implicitly, at least, field-specific standards of adequacy and relevance guide the assemblage of these resources into a 'compelling' but not rigourous argument. Hence scientific argument, as essentially persuasive argument, may rightly be termed 'rhetorical' in the sense defined by students of 'the new rhetoric', denoting the entire field of discursive structures and strategies used to render arguments persuasive in given situations.[30]

Now, all the various doctrines of scientific method, as well as the particular stories derivable from them, form a reservoir of discursive resources available to scientists in the formulation of such essentially rhetorical arguments. Hence to this extent it is correct to say that methodological doctrines can be partially constitutive of knowledge claims in the sciences; that is, in terms of our model, Level I and II method discourse, especially Level II stories, can be deployed on Level III in the cut and thrust of scientific practice, and hence in that sense can be said to be partially constitutive of socially negotiated outcomes within the Level III matrices. Methods do not command, explain or grasp the essence of Level III practice; but, they can be deployed on that level as resources in the struggle to establish claims. Historians and sociologists of science have observed that all such rhetorical deployments of method discourses are highly flexible and context dependent, scientists sometimes giving different methodological accounts in different argumentative contexts, and sometimes even contradicting themselves by offering contradictory interpretations of their own methods or those of famous methodologists.[31]

Descartes certainly practised such rhetorical deployments of method, mobilizing Level II accounts in order partly to constitute knowledge bids he was [210] advancing on Level III. His methodological account in the Regulae of the discovery of the law of refraction and of its mechanistic explanation is just such a gambit. The story bears no relation to his 'bench practice'; yet, it structures a presentation of his work and so is partly constitutive of it as a knowledge claim proffered to his audience. Moreover, Descartes' method story about his optical work served other subordinate functions in the overall interest of facilitating the acceptance of his claims. First, it occluded the dependence of his actual work upon the traditional image principle made dubious by Kepler's findings. Second, it provided a (method-)logical connection between the geometrical-optical and mechanistic- explanatory stages in his work. Thirdly, the vagueness of Descartes' methodological language about "natural powers", and his methodological reflections about 'analogy' covered what I would contend was, in 1628, real hesitation and ambivalence about the best direction to take in articulating a mechanistic model of light.[32] The method story was a very valuable way of framing, constituting and presenting his knowledge claims while finessing these secondary problems. When one additionally considers that Descartes probably believed that the work could have been done the way the story tells, the power and utility of the method become very clear. Descartes, one suspects, was probably getting the benefit of his own 'just so' story (by virtue of the literary effects), just as his readers were (honestly, rather than cynically) intended to do.

All the foregoing points are based upon our model of method discourse. Taken together, they also reinforce and articulate that model, because they allow us to see additional reasons why actors quite reasonably fall for the apparent efficacy and applicability of any method doctrine: For believers in a particular method, any

deployment on Level III of its Level II stories will be highly privileged and impressive. These stories will probably be the only resources in play on Level III which label themselves as 'methodological'. Participants debating and negotiating claims on Level III will generate and hear these method stories as the only elements in the cluttered landscape of debate which are of a 'methodological' character. Hence believers will see method-talk "in action" as a crucial, or the crucial element in the debate. This will lend more support to the truth of the Level II stories. The stories say "practice proceeds just thus and so", and here is "practice", that is the social world of the laboratory, conference, published debate etc., in which method discourse is a crucial resource in the fray.[33]

Method claims on Level III need not be consensually accepted by all parties and can of course be contested. This can be understood in terms of the recent work in the sociology of science which further establishes that the evaluation and negotiation of knowledge claims is a social and political process, and that any and all of the tools or weapons used in constructing or evaluating a claim can be questioned.[34] The recourse to methodological discourse on Level III is simply one possible tactic in this knowledge-making/knowledge-breaking game, and so deployments of method discourse can become objects of contention within it. Hence for a contestant like Descartes, [211] not only did particular claims need to be woven out of the sturdy cloth of method discourse; but, the method itself, the ultimate legitimating weapon, required support and justification. So, when Descartes presented his optics in terms of his method, he not only tried to legitimate the optics in the ways we have indicated; he was also legitimating the method by the 'evidence' of concrete application and success. (The optics case illustrated a text on method, not vice versa.)

All this was particularly important, because the method in turn was going to have to bear the weight of legitimating any and all of his projects. Descartes, like others contending for scientific and natural philosophical pre-eminence, was not concerned simply with particular claims and arguments. He wanted to group together and package a certain family of results ranging over a spectrum of specialties, from mathematics to medicine. So, when Descartes grouped together otherwise widely disparate pieces of research as products of his method, he was staking out a series of political claims in the economy of the sciences. Not only was he endorsing his results individually, he was also linking them under the claim that they were all to be accepted as a piece, because they all fell within and followed from his method, the method. He was claiming methodological hegemony over these and other fields, positioning himself in relation to practitioners within and across those fields. The literary effects of method, especially those of unity and progress, probably provided him with a great deal of honestly held confidence about taking this posture.

In the final analysis the key issue for Descartes was the status of his system of mechanistic natural philosophy. Indeed, the central issue in the period of the so-called Scientific Revolution was precisely the clash of opposing systematic visions of natural philosophy, a clash which climaxed during the lifetime of Descartes.[35] His method functions on this peak level of struggle by supposedly underpining his entire project in natural philosophy, underwriting, that is, his claim to pre-eminence in resolving the clash of natural philosophies of his day. This is intimated in the way the Essais of 1637, themselves appetizers for the system, are subordinated to the overarching tale of the method in the Discours; and in the way the metaphysical grounding for his natural philosophy is also offered as a triumph of method. Descartes even carried this method-rhetorical shaping of his claim to cognitive dominance to a higher, more personal,

heroic, indeed Baroque level, when he claimed that his life as a natural philosopher, mathematician and metaphysician had itself been shaped and lived, in order, according to the method.

But whether Descartes himself believed these wider claims, especially after he abandoned the Regulae in 1628, is another matter. Method discourses may systematically delude believers, but there may also be particular circumstances, social and biographical, in which actors cynically exploit the rhetorical power of a method discourse in which they have cause not to believe. In the next section we will examine the possibility that Descartes' career in methodology conforms to a melodramatic plot in which the honest delusion of youth later gave way to cynical opportunism. [212]

VIII. Rethinking Method and the Career of Descartes

1. The Original Inscription of Descartes' Method: Bricolage and Self-Deception

Our sceptical, post-Kuhnian view of method implies that the grand tradition of theorizing about method that extends from Bacon (indeed from Aristotle) to Popper is not a "whiggish" progression toward ever more clear insights into the 'truth' about method. Each methodologist has operated with (and against) the available formal discourses on method; but each new methodology has been constructed by its author in the light of problems and goals which might relate to the tradition itself, to the perceived state of one or more of the contemporary sciences, or to other discourses believed to be relevant, such as natural theology, political theory, and moral philosophy. The perception and weighting of such concerns by a methodologist is a complex function of his biography, social location, institutional affiliations and perceived interests. Moreover, it seems that a certain biographically and contextually conditioned bricolage of available cultural resources governs the manufacture of any particular 'great' methodologist's brand of method.[36] The task of a new historiography of method is to abandon an heroic, 'whig' history of spuriously efficacious methodological ideas in favour of a social and political history of theorizing about method, a history that also takes seriously the structure and literary effects of method discourses. By way of illustration, let me sketch some points about Descartes' early decisive experience as an understandably deluded bricoleur of method.

I have argued elsewhere in detail that the core of Descartes' method doctrine was constructed in late 1619 and early 1620; that his enthusiastically constructed method doctrine marked the third and final step in a series of youthfully over-ambitious and under-articulated enterprises, each one more grandiose and general than the previous one, each one inscribed partly by means of unjustifiable analogical extension of its predecessor. My argument was based on dating the earliest parts of the Regulae from this period, on the basis of internal evidence and its relation to datable fragments of Descartes' mathematical and natural philosophical work.[37]. I shall not enter into the details here, but merely sketch the story of Descartes' enthusiastic methodological bricolage.

In November 1618 Descartes met Isaac Beeckman and fell in with his dream of a natural philosophy that would be both corpuscular-mechanical and properly (rather than metaphorically) 'mathematical', in the sense of depending upon mathematical argument, analysis and demonstration. They termed this project 'physico-mathematics'. Descartes and Beeckman were youthful, enthusiastic and badly confused about the difference

between aspiration and performance. They produced no convincing examples of this physico-mathematics, although in one or two special problem cases it is clear that they thought they had hit upon real instances of it. Wild aspiration masked a tissue of ad hoc ontology and post facto pseudo-geometrization. But what Descartes could see [213] was that in principle physico-mathematics demanded a general way of mathematically stating and solving problems in physics.

In mid 1619 Descartes' dream of physico-mathematics, as well as his own recent researches in mathematics, were subsumed under an even more grandiose project of 'universal mathematics'. I have argued elsewhere that universal mathematics can be properly understood only if we correctly date and interpret that curious portion of Regulae IV now termed rule IVB, and place it in the context of Descartes' recent mathematical researches.[38] Rule IVB was probably written as part of a projected treatise on universal mathematics; it predates the surrounding text of the Regulae which was composed shortly after November 1619 and deals, of course, with the method. Rule IVB tells us that universal mathematics embraces the axioms, principles and methods common to all properly mathematical fields, that it is the science of "order" and "measure" wherever they appear in the various mathematical disciplines. "Measure" plausibly denotes here "quantity in general", the abstract object with which one deals after one has abstracted from the particular mathematical objects of the particular mathematical disciplines. "Order" seems to connote a concern with finding general schemas of analysis for problems, once they have been stated in abstract terms. All of this reflects neo-Platonically inspired ideas about a "general mathematics" that were current in the late 16th and early 17th centuries. I have argued that Descartes formulated rule IVB combining these available notions with a daring extrapolation of certain aspects of his mathematical and physico-mathematical researches of 1619.

At that time Descartes was very much interested in the proportional compass represented in **figure 4**. In particular he was interested in its rough and ready practical use to solve problems. He focussed not upon the curves it drew (as he did later in the Géométrie), but rather upon the way many problems in algebra or geometry could be modeled on the compass, provided the terms of the problems could be reduced to the finding of relations amongst proportional magnitudes. There were very real limits to the value of the compass in this respect; but Descartes ran directly over them in his haste to generalise.

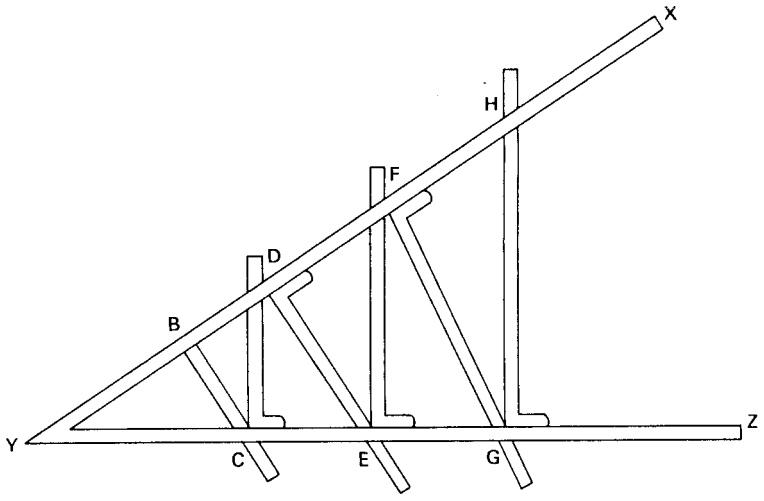

Figure 4. Descartes' Proportional Compass 1619, 1637

The compass, is described in Books II and III of the *Geometry* in terms corresponding to the more crude figures and implied mode of use in the *Cogitationes privatae*. The lettering in the figure is

> based on that in the *Geometry*. The compass consists of two main branches, YX and YZ, pivoted at Y. Set inside the branches are a series of rulers, of which BC, DE and FG are set at right angles to YX, while CD, EF and GH are set at right angles to YZ. BC is fixed to YX at B, but the bases of the rest of the rulers can slide along the inner side of the branch to which they are set. *As* the compass is opened BC pushes CD along YZ, and CD in turn pushes the base of DE along YX and so on. The compass is `a machine for generating series of magnitudes (line lengths) in continued geometrical proportion' for, by similar right triangles CYB, DYC, EYD, FYE, GYF, and HYG it is the case that:
>
> $$\frac{YB}{YC} = \frac{YC}{YD} = \frac{YD}{YE} = \frac{YE}{YF} = \frac{YF}{YG} = \frac{YG}{YH}$$

It did not matter whether the problem was arithmetical, algebraical or geometrical: one could abstract from the particular numerical, symbolic or figurate setting of the problem, translating the numbers or magnitudes into line lengths representable on the limbs and branches of the compass. Solving the problem so abstracted simply involved unfolding a set of proportions holding amongst these abstracted quantities. In other words, 'quantity in general' was represented by limb lengths; 'schemas of solution' could be examined by looking at the structure of relations amongst the quantities thus represented. Used in this way, the compass was a veritable exemplar for the idea that the various mathematical disciplines could be subordinated to a universal mathematics. When, between March and November 1619, Descartes further realized that 'physico-mathematical' problems, as well, would or should boil down to problems about structures of ratios and proportions holding amongst representative quantities, the dream of universal mathematics was born and rule IVB composed as part of the larger intended treatise. But it was only a [214] dream, his techniques did not even work for all the algebraic problems he had attempted; and such treatment of physico-mathematical problems was, of course, a non-starter. Yet, he did have before him the successful special cases and the overblown grand idea. Soon, however, the whole undertaking was swamped by the grandiose vision of the method, which was, in fact, a vast analogical extrapolation of notions embodied in universal mathematics, notions themselves half-baked and over-extended.

We have in the Regulae some of the fossil traces of this process of extrapolation. Let us recall the peculiar portion of Regulae VI which elaborates the concepts of series, absolutes, relatives and relations, notions which I have also argued served as the template for the elaboration of the heuristic rules of the method in Regulae VII through XI.[39] My contention is that the entire abstract and high flown language of absolutes and relatives, of series and relations, and the portentous heuristic rules that go along with it, are nothing more than vast analogical extensions of a set of ideas fundamental to the as yet not fully constituted discipline of universal mathematics.

At the end of Regulae VI there is a little mathematical example about a series of numbers in a continued geometrical proportion. Such a series is, of course, typical of the sort of entities to be treated in universal mathematics, as I have unpacked it above. Descartes uses the series to illustrate some of the general heuristic rules, but although the example poses as an illustration, everything we have seen powerfully suggests that this is the sort of example in universal mathematics from which the central portions of the method discourse were analogically derived. Consider that for the methodological concept of the 'absolute term', we can read 'defining ratio applied to an initial unit'; for 'relative terms', we can read 'numbers subsequently generated in a continued [215] geometrical proportion'; for the grandiose latticework of rational truths, we can read the orderly interlinked series of numbers in continued geometrical proportions; and, finally, for each of those heuristic rules of method 'illustrated' by the series, we can read a

concrete but fairly trivial piece of advice about the solution of problems arising about series of magnitudes in continued proportions. In other words, there is very good reason to think that what the over-excited young Descartes thought, wrongly, was true of universal mathematics, he daringly extended into the realm of all rational enquiry. The method discourse was not abstracted from successful practice in some genuine area of mathematics; it was produced by a megalomaniac performance of operations of analogical extension upon the terms of a discourse, universal mathematics, which itself could not do very much of what it was purported to do.

What we seem to have, therefore, in Descartes' path to his initial inscription of his method is a trail of somewhat confused and over-enthusiastic bricolage. Bits of his own work are assembled with elements of culturally available discourse on 'general mathematics', and then 'method', in a series of analogical extensions and subsumptions of previous discourse, issuing in the manufacture of the method. And, as we know from our study of the enticing discursive dynamics of method discourses, Descartes was probably beginning to fall for the literary effects of his discourse. Yet, from Descartes' perspective his path to the method would have seemed a marvellous and triumphal progress. Recalled to study in 1619 by the vision of Beeckmanian physico-mathematics, he had, by mid 1619, merged that project with his work in mathematics to formulate the intoxicating dream of universal mathematics. Then, musing in the late autumn of 1619, he had seen how to conquer all rationally obtainable knowledge by generalizing his earlier revelations. No wonder, then, that on St. Martin's eve 1619, Descartes, enthused by his skill in thus transforming one discourse into another, dreamt that the project he had glimpsed had been consecrated by God himself.

2. The Failure of the *Regulae*, The Birth of the System and the Problem of the Cynical *Discours de la méthode*

I have been suggesting all along that Descartes' project of method is crucial to understanding his career as a mathematician and natural philosopher, but not in the senses that he (or approving scholars) claim. Nowhere is this more apparent than in the decisive fifteen years following the methodological frenzies of 1619/20. Unless we maintain a cool, sceptical approach to method, we are likely to get hopelessly lost in Descartes' own mystifications, and so lose the key to reclaiming him as a realistically conceived actor in the history of science. To this end a proper, demystified understanding is required of Descartes' activities in the 1620s, in particular his attempt in the later portions of the Regulae to flesh out and partially redirect his method project of 1619.[40]

In 1620 Descartes was neither a builder of systems of natural philosophy, nor a systematic metaphysician; he was--following Beeckman--a practicing, piecemeal mechanical philosopher and mathematician, as well as a self-appointed [216] methodological prophet. We can understand how he was convinced that he possessed a method; that it subsumed universal mathematics; that it was efficacious; and that it could guide his researches in every field of rational inquiry.

By the time he settled in Paris in the mid 1620s, Descartes had produced a genuine mathematical triumph with his construction of all the 'solid' problems of the ancients, using only a circle and parabola (equivalent to a general construction for all cubic and quartic equations).[41] In Paris, sometime in 1626 or 1627 he produced his master stroke in physics: the construction of the law of refraction, followed by the development of a theory of lenses and the attempt to subsume the law under a mechanistic theory of light.

Working in the circles around Marin Mersenne, he was very much a rising figure in the emerging community of French mathematical savants. Although these achievements had not been produced by application of the method, Descartes no doubt conceived the method to be relevant to his triumphs, and they in turn reinforced his belief in his method, according to the mechanisms we have described.

Descartes also became enmeshed in the wider cultural life of the capital, which was then such a hot house of political, religious, literary and philosophical debate that some historians have been moved to speak of some sort of intellectual or religious/ideological 'crisis' of the 1620s. Like his friend Mersenne, Descartes became convinced of the need to combat unorthodox philosophies of nature, those of alchemical, neo-Platonic or 'Hermetic' inspiration, whilst avoiding the threat of a fashionable and corrosive scepticism. Eager to exploit his technical achievements in optics and mathematics, and to win public recognition for his personal methodological illuminations of 1619/20, Descartes entered the fray. As I have argued at length elsewhere, his tactics were modeled in part upon those pursued by Mersenne: avoiding systematic natural philosophy or metaphysics, he would deploy, piecemeal, supposedly reliable bits of mathematical and natural philosophical knowledge in order to show, on the one hand, that unorthodox natural philosophies lack valid scientific foundations, whilst, on the other hand, showing that scepticism can be sidestepped, if not refuted, by the mere ostension of achievements whose practical efficacy could not reasonably be denied. Descartes, however, conceived that he had resources for these tasks far superior to those of Mersenne, for he had a method, and some outstanding results in mathematics and optics. Descartes' project took the form of returning to his universal mathematics of 1619, which he now tried to articulate in detail, under the guise of extending his 1619/20 text on method, roughly Regulae I to XI. Universal mathematics, carrying out Mersenne's tactics, would appear to grow out of the doctrine of method. Regulae XII to XI were written in Paris for this purpose.[42]

Taking up bits and pieces of his own theories of mechanistic optics and physiology, Descartes worked them into a sketch of a mechanistic theory of nervous function and sensation. Combining this with a reformulation of elements of scholastic discourse on psychology, he produced an idiosyncratic [217] mechanistic account of perception and cognition, meant to underwrite universal mathematics and show how its logistical machinery was to work. The nub of this doctrine was that the spiritual or intellectual component of our human make-up is a vis cognoscens, a thinking power, which literally sees and inspects patterns and figures mechanically impressed in various brain loci. The vis cognoscens obviously is the conceptual resource out of which the 'thinking substance' of the later metaphysics was fabricated, after the collapse of the Regulae.

A Mersennian mathematical science thus became possible in the following way: We limit ourselves to quantifiable, measurable properties, such as size, shape, weight (sic), speed, density, etc. Lines or figures representing the measures of quantities are directly impressible into appropriate brain loci. We then try to establish mathematical correlations amongst such empirically given and mechanically impressed measures of physical quantities. No sceptic can reasonably question the validity of such procedures, for the vis cognoscens has a direct validating vision of precisely what we are doing with and to these lines and figures. Unorthodox natural philosophies are also in trouble, for they clearly deal with fantasies; the only aspects of reality with which we can rationally and methodologically come to grips are measurable physical quantities. Number mysticism, immaterial agencies, occult causes, are epistemologically irrelevant, if not exactly shown not to exist.

The doctrine of the later Regulae was Mersennian in overall design and goal; but it was worked out in epistemological, psychological, physiological and methodological detail undreamt of by Mersenne. There was only one thing wrong with this newly articulated universal mathematics--it did not work, and Descartes, I have demonstrated, realised this by late 1628, when he abruptly abandoned composition of the Regulae and moved to the United Provinces, there to work on the metaphysics and systematic mechanistic natural philosophy which could answer and transcend the difficulties upon which the Regulae had foundered.

Close textual analysis shows that three related problems crippled the project of the later Regulae, opening new and unintended difficulties and creating the problematic in which the subsequent metaphysics and systematic corpuscular-mechanism were to move: (1) The newly articulated universal mathematics dealt with macroscopic 'dimensions' directly known and certified. But Descartes' corpuscular-mechanical leanings in natural philosophy dealt with an invisible realm of micro-particles. The answer was to elaborate a fully ontological doctrine of matter-extension which could license macro-microscopic analogies, and ground a systematic corpuscular mechanism, but at the cost of giving up claims to proper mathematization, envisioned in the later Regulae. (2) Insistence that the world is known under geometrico-mechanical schemas focused the problem of the status and origin of non-geometrical perceptions. The solution was to extend and metaphysicalize the incipient systematic dualism of the later Regulae, so that one could, in the mature metaphysics, clearly distinguish between purely mental 'ideas' and the corpuscular-mechanical states of affairs that sometimes occasion ideas, [218] but which are not necessarily represented by them. (3) Not all mathematical operations and objects lend themselves to justification via the excessively simple procedures of imaginative representation, manipulation and inspection of line lengths advocated in the later Regulae. The text breaks off at precisely the point this would have become clear to Descartes. The answer was to retreat from justification of mathematics by intuition of geometrical representations to a more abstract-relational view of the grounds of mathematical truth, and to erect a metaphysics that could supposedly guarantee intuitions which do not have to depend upon imaginative representation, or geometrical presentation at all.

In sum, the very failure of the later Regulae structured problems and opportunities that Descartes then began to pursue through the elaboration of his mature metaphysics and systematic natural philosophy. The problem is that he was still to write the Discours; still to claim that the method guided his life and work; still to claim, indeed, that none of the messy history just outlined ever happened.

Nevertheless, these claims need not disarm (or impress) any cool, sceptical, post-Kuhnian mythologist of method. Virtually everything Descartes states in the Discours about the provenance, use and development of the method, and its role in his career, is a fiction. It should be patently obvious by now that Descartes did not elicit his method by abstracting out and synthesizing the best aspects of scholastic logic, Greek geometrical analysis and algebra (his construction being more fraught and opportunistic);[43] that he did not develop his universal mathematics with logistic of line lengths in 1620, nor did he do it by applying his method;[44] that applying his method did not generate an ever enlarging collection of rules for mathematical analysis;[45] that, after 1618, the method in no way offered a full account of "everything that gives the rules of arithmetic their certainty";[46] and, finally, that the method, in 1619, did not dictate the course of his career, the preparatory years spent in lower studies before he was ready to assay metaphysics after 1628.[47]

We can, I think, conclude that down until the collapse of the renewed project of universal mathematics in the later Regulae in 1628, Descartes was probably under the sway of his method discourse, generally believing, for the reasons already discussed, in its de facto or in principle relevance to his scientific and mathematical projects. After 1628, one cannot be so confident that Descartes was so firmly in the grip of the discursive dynamics of method, nor, accordingly can one be so charitable about his likely beliefs and intentions. It would seem likely that when he used the method to articulate his autobiography in the Discours, he was largely covering the tracks of his abortive enterprise of the late 1620s and was cynically exploiting the method as a rhetorical device in the traditional perjorative sense. Similarly, in other contexts of crude methodological assertion, it becomes increasingly difficult to believe that Descartes genuinely believed what he was saying. And yet, the discursive mechanisms of method are such that no amount of experience must dissuade a believer; and the fact that Descartes was probably both a cynical manipulator of the method and the first of its many victims may explain the air of ambiguous ambivalence [219] that seems to surround many of his later methodological pronouncements. He may have feared that the method did not work, and feared and resisted coming to grips with that suspicion. The psychology of a crisis of belief in a method may bear similarities to the better known contours of crises of religious belief, especially if methods are indeed powerful species of mythic speech.

Whatever one makes of these problems, it should at least be clear that the sorting out of Descartes' method discourse, the reconstruction of its genesis and the identification of its discursive structure and dynamics, are all necessary conditions for our recovery (literary manufacture) of an historical rather than mythological Descartes. Although Descartes posed behind his method as a lone prophet of a new science, in reality--as an exponent of mechanism, practitioner of the mathematical sciences and advocate of new values in natural philosophy--he was a figure highly symptomatic of the contextual forces in play and opportunities at hand at this crucial moment in the process of the Scientific Revolution. His method explains neither his manner of work, his achievements nor the course of his symptomatic career. Rather, his absorption in method, his succumbing to its effects, and even his later suspected manipulation of it, are simply a part, an essential part, of that very contextual weave, a weave the method deceivingly claims to command and explain. [220]

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[1] Cf. J. Priestley, The History and Present State of Electricity with Original Experiments (London, 1767), pp.v-vi; W.Whewell, History of the Inductive Sciences (London,1837), vol.I., p.5; W. Whewell, The Philosophy of the Inductive Sciences (London, 1980), pp.3-4.

[2] C. Singer (ed.), Studies in the History and Method of Science, vol.I (Oxford, 1917-21) p.vi; G. Sarton, 'Introduction to the History and Philosophy of Science', Isis 4 (1921-22), 23-31 at p.25; G. Sarton, "The New Humanism', Isis 6 (1924), 9-34 at p.26.

[3] For example, A.C.Crombie, Robert Grosseteste and the Origins of Experimental Science, 1100-1700 (Oxford, 1953); J.H.Randall, The School of Padua and the Emergence of Modern Science (Padua,1961)

[4] K. R. Popper, The Logic of Scientific Discovery (London, 1959); I. Lakatos, 'Falsification and the Methodology of Scientific Research Programmes', in J. Worrall and G. Currie (eds.), Imre Lakatos: Philosophical Papers, vol.I (C.U.P., 1978), pp.8-101.

[5] J. A. Schuster, 'Cartesian Method as Mythic Speech: A Diachronic and Structural Analysis', in J. A. Schuster and R. Yeo (eds.), The Politics and Rhetoric of Scientific Method: Historical Studies (Dordrecht, 1986), pp.33-95, at p.38-40.

[6] J.A.Schuster and R.Yeo, "Introduction" to Schuster and Yeo (eds.] The Politics and Rhetoric of Scientific Method: Historical Studies [Dordrecht, 1986], pp.ix-xxxvii.

[7] A.Koyre, Etudes Galileennes (Paris: Hermann & Cie, 1939); Koyre, Galileo Studies, trans. J. Mepham (Hassocks, Sussex: Harvester, 1978); Koyre, Metaphysics and Measurement: Essays in Scientific Revolution, (London: Chapman & Hall, 1969); Koyre, 'The Origins of Modern Science', Diogenes, 16 (1959), 1-22.

[8] A. Koyre, 'The Origins of Modern Science', Diogenes 16 (1956), pp.1-22.

[9]G.Bachelard, Le Nouvel Esprit Scientifique (Paris: Presses Universitaires de France, 13e edn, 1975); Bachelard, Le Rationalisme Applique (Paris: Presses Universitaires de France, 1949); D.Lecourt, Marxism and Epistemology: Bachelard, Canguilhem, Foucault, trans. B.Brewster (London: New Left Books, 1975), 40-47, 60-70.

[10] G. Bachelard, La Formation de l'Esprit Scientifique (Paris: Vrin, 9e edn, 1975), 61.

[11] Presumably none of this surprises readers of T.S.Kuhn, The Structure of Scientific Revolutions (Chicago,IL: The University of Chicago Press, 2nd edn,1970), especially the 'Postscript'; Kuhn, The Essential Tension: Selected Studies in Scientific Tradition and Change (Chicago,IL: The University of Chicago Press, 1977), Chapter 13; J.R.Ravetz, Scientific Knowledge and Its Social Problems (Oxford: Clarendon Press, 1971), 71-240; B.Barnes, T.S.Kuhn and Social Science (London: MacMillan, 1982); Bachelard (1975), op. cit. (note 10).

[12] J. R. Ravetz, op.cit., (Note 11); M. Mulkay, Science and the Sociology of Knowledge (London, 1979); B. Latour and S. Woolgar, Laboratory Life, The Social Construction of Scientific Facts (London, 1979); K. Knorr-Cetina, The Manufacture of Knowledge: An Essay on the Constructivist and Conventional Character of Knowledge and Cognition (Oxford, 1981); H. Collins, Changing Order (London, 1985).

[13] For early 'derivations' of this position from the writings of Kuhn see Ravetz, op. cit. (note 11), and J.A. Schuster, 'Kuhn and Lakatos Revisited', British Journal for the History of Science, 12 (1979), 301-17.

[14] These points are perhaps best brought out in full-scale contextualist studies in the history of science, for example, M.J.S. Rudwick, The Great Devonian Controversy: The Shaping of Scientific Knowledge among Gentlemanly Specialists (Chicago,IL: The University of Chicago Press,1985); S. Shapin and S. Schaffer, Leviathan and the Air-Pump: Hobbes, Boyle and the Experimental Life (Princeton, NJ: Princeton University Press,1985); A.Desmond, Archetypes and Ancestors: Paleontology in Victorian London, 1850-1875 (London: Blond & Briggs, 1982).

[15] Cf. J. A. Schuster, op.cit., (Note 5), pp.40-47.

[16] A.T.X, p.382; CSM . I. pp.21-22.

[17] Regulae VIII AT X p.393 l.22 to 396 l.25.CSM. I. p.29.

[18] J. A. Schuster, Descartes and the Scientific Revolution--1618-34: An Interpretation, (Ph.D., Princeton, 1977), pp.268-368.

[19] Analogous remarks apply to that supposed case of application of the method, the discovery of the explanation of the formation and geometrical properties of the rainbow. This was indeed an exceedingly good piece of normal science, the solution to a classic puzzle in geometrical optics. But it was also highly traditional, conditioned by the aims, concepts, tools and standards of the discipline. Descartes' recourse to a water filled flask as a model rain drop was not novel, and even had it been, it could be interpreted as having been mediated by a very commonsensical, rather than methodological rationale. Descartes' sole advantage over others was possession of an exact law of refraction, which now served, as laws often do, as a tool in facilitating further research. An exact tool, a standard model, some sufficiently accurate date, and laborious calculation resolved the problem. To invoke the rules of the method here is to glide over the rich, tradition bound dynamics of the research.

[20] J.A.Schuster, 'Methodologies as Mythic Structures: A Preface to the Future Historiography of Method', Metascience: Annual Review of the Australasian Association for the History, Philosophy and Social Studies of Science, 1/2 (1984), pp.15-36; J.A.Schuster, op. cit. (Note 5).

[21] below Section VIII, p19.

[22] Descartes also has a discourse on Level I concerning errors and pitfalls. See Schuster, op. cit. (Note 5) n119 to p.79. [*clause in italics added March 2010*] Published version had one figure 3. This has been

dissected into three related figures here 3a, 3b 3c to better illustrate the points about textual effects of a method discourse.

[23]ibid. pp.62-5, 71-4. Here not only the case of optics is examined, but also those of analytical mathematics and magnetism (treated as a case study in corpuscular-mechanical explanation). The method stories corresponding to Descartes' Level I practice in these areas are discussed.

[24]even in his activities on Level III, see section VII below, pp 16-17.

[25] Schuster op. cit. (Note 5), pp.62-65, 71-74

[26]In the former case these terms denote respectively coordinates and algebraically expressed curves; in the latter case respectively corpuscular-mechanical ontological primitives and compounds.

[27] P.K.Feyerabend, Against Method (London, 1975) and Science in a Free Society (London 1978) ; Cf. Schuster , op. cit. (Note 5), pp.36-37, 79-80.

[28] Schuster & Yeo, op.cit. (Note 6); R. Yeo, 'Scientific Method and the Rhetoric of Science in Britain, 1830-1917', in ibid., 259-97.

[29]see above Section II pp.5-6

[30]C. Perelman, The New Rhetoric and the Humanities (Dordrecht, 1979); C. Perelman and L. Olbrechts-Tyteca, The New Rhetoric: A Treatise on Argumentation (London, 1971); J.R.Ravetz, op.cit., (Note 11); S. Yearley, 'Textual Persuasion: The Role of Social Accounting in the Construction of Scientific Arguments', Philosophy of the Social Sciences 11 (1981) pp.409-35; W. Weimar 'Science as Rhetorical Transaction: Toward a Nonjustificational Conception of Rhetoric', Philosophy and Rhetoric 10 (1977),pp.1-29.

[31] M.Mulkay and G.N.Gilbert, 'Putting Philosophy to Work: Sir Karl Popper's Influence on Scientific Practice', Philosophy of the Social Sciences, 11 (1981), 389-407; P.K. Feyerabend, Against Method (London: New Left Books, 1975); D.P.Miller, 'Method and the "Micropolitics" of Science: The Early Years of the Geological and Astronomical Societies of London', in Schuster & Yeo, op.cit. (Note 6), 227-57; H.E. LeGrand, 'Steady as a Rock: Methodology and Moving Continents', ibid., 97-138; P.Wood, 'Methodology and Apologetics: Thomas Sprat's *History of the Royal Society'*, British Journal for the History of Science, 13 (1980), 1-26; E.Richards & J.A. Schuster, The Feminine Method as Myth and Accounting Resource: A Challenge to Gender Studies and Social Studies of Science, Social Studies of Science, 19, (1989), 697-720.

[32] Descartes was then probably playing with models of light involving bent arm balances, balls, as well as crude versions of his ontological model--mechanical disturbance in a medium; see Schuster, op. cit. (Note 18), pp.346-52.

[33] From all this we can derive two laws in the anthropology of method which help to explain why method-talk is deployed in certain ways in scientific debate. Consider a scientific community engaged in debate over two divergent knowledge claims:

(1) To the extent that all debaters share elements of the same method discourse, their debate will tend to take the form 'to which claim does the method story attach' not 'how can one credit stories generated in our method discourse?'

(2) If there are differences in preferred method discourse, debates about method will take centre stage away from debate about the divergent claims per se. That is, debate about the claims will be carried on to a large extent by means of debate about which method is to be followed. In either case all sides will still share the method believer's view that the crucial element in debate is method.

[34]P. Bourdieu, 'The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason', Social Science Information 6 (1975), pp.19-47; B. Latour and S.Woolgar, op.cit., (Note 12); M. Callon, 'Struggles and Negotiations to Define What is Problematical and What is Not: The Sociologic Translation', in K.D.Knorr et.al. (eds.) The Social Process of Scientific Investigation [Sociology of the Sciences Yearbook IV (Dordrecht, 1980), pp.197-219; S.Shapin, 'The History of Science and its Sociological Reconstructions', History of Science, 20 (1982), p.157-211; M.Mulkay and G.N.Gilbert, 'Putting Philosophy to Work: Sir Karl Popper's Influence on Scientific Practice', Philosophy of the Social Sciences 11 (1981), pp.389-407.

[35] J.A.Schuster, 'The Scientific Revolution', in R. Olby et al (eds.) The Companion to the History of Modern Science (London 1990), 217-242.

[36] see e.g. R. Yeo,'William Whewell, Natural Theology and the Philosophy of Science in Mid-Nineteenth Century Britain, Annals of Science 36 (1979), 493-516, and Richards and Schuster op. cit. (Note 31).

[37]Details in J.A. Schuster, 'Descartes' *Mathesis Universalis*: 1619-28' in S. Gaukroger (ed.) Descartes: Philosophy, Mathematics and Physics (Brighton, 1980), pp.41-96, esp.pp.42-55; and, Schuster, op.cit. (Note 5), pp.47-59.

[38] Schuster, op. cit. (Note 37), pp42-47; The delineation of portions 4B and 4A within Regulae IV was owing to J.-P Weber, La constitution du texte des Regulae (Paris 1964),pp5-7; Rule 4B runs from AT X p.374 l.16 to the end of rule 4; Rule 4A opens rule 4 at AT X p.371 l.1 to 374 l.15.

[39] Schuster, op.cit., (Note 5), pp.44-47.

[40]Material in the next eight paragraphs is dealt with in detail in Schuster, op. cit. (Note 37) pp.55-80;

[41]Schuster op. cit. (Note 18) pp.127-149. Needless to say, this reconstruction of Descartes' discovery owes nothing to invoking the rules of his method.

[42]Schuster op. cit. (note 37) pp.55-64.

[43]AT. VI.p.18; CSM. I. pp.119-20.

[44] AT. VI. p.20; CSM I. pp.120-21.

[45] AT I. pp.20-21; CSM I. p.121.

[46] AT.VI. p.21; CSM I. p.121.

[47] AT. VI. pp.21-22; CSM I. p.121-22.