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Reading Descartes' *Regulae*: Genealogy, Structure and Fate of the Text

Part II pages (15-38)

Method as Mythic Speech:
A Challenge in the Historiography of Science—Descartes and Beyond

Note: Part II will only be of interest those intending to participate in the second part of this presentation. Those interested solely in the structure and fate of the text of the *Regulae* need to read only the dozen or so pages of Part I.

Part I

Reading Descartes' *Regulae*: Genealogy, Structure and Fate of the Text¹

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1.0 *The Regulae: its Structure and its 'Discontents'*

The *Rules for the direction of the mind* is the only surviving large work of Descartes' younger days; written between mid 1619 and 1628 when it was abandoned, unfinished, at Rule 21. It wasn't published in his life time. It ostensibly deals with his method and with certain early claims about a mechanistic physiology of perception; as well as with issues about how mathematical operations are performed in a justifiable manner. It lacks any formal metaphysics or systematic concern with corpuscular-mechanical natural philosophy, both begun right after abandonment of this text. Until recently these were the known versions: 'A' [Latin] Published Amsterdam 1701; 'H' [Latin] Discovered by Leibniz in Hanover Library; 'N' [Dutch] 1684. The Adam and Tannery standard Descartes edition ignored N, used A with H. While Giovanni Crapulli used A, H and N.³

To make things simple let's say there are two interpretative approaches to the *Regulae*: [1] The philosophical' one which aims to find an overriding unity of doctrine, captured in terms of Descartes' method. That makes the *Regulae* quite important, because a much slimmer version of that method was published in 1637 in Descartes' first publication, at age 41: the *Discourse on Method* and three *Essays*. It is also important to seek a coherent doctrine of method in the *Regulae* because it is often thought Descartes could have used this method in his work, as he claimed. You do not necessarily have to believe in method to seek unity in the *Regulae*, but you might and it certainly is a motive for some. This philosophical approach does not require much contextual or biographical study of Descartes. [2] The other approach is 'historical', meaning it is developmental and contextual: It seeks to understand this text as part of the flow of Descartes' intellectual biography and that biography in context of developments and contestations in natural philosophy, optics, physiology, mathematics and method-talk during his generation. Here it is less important to prove the text coherent in its teaching, and if you are a properly trained historian in the spirit of Koyré, Kuhn and Bachelard, like me, then of course you do not believe in the efficacy of any general methodology, including Descartes'. You attend to rhetorical uses of method-talk; you ask whether Descartes believed in his method, and whether he ever lost faith in it and with what results—for example he may have hidden that fact in public.

I am and have been a member of group [2], taking my initial cues from the work of a renegade philosopher, Jean-Paul Weber, who in 1964 wrote *La Constitution du texte des Regulae*, in which he identified nine to a dozen textual strata, depending upon how one reads his text and his chart at the end of the book. The precise number does not matter at this late date, because Weber adduced no sustained contextual/biographical argument to buttress his archaeology of the text. In contrast I have only three chronological and intentional strata in (and as we shall see, around) the *Regulae*, and I submit that my interpretation is well embedded in a larger biographical and contextual reconstruction of Descartes' early intellectual trajectory. But sometimes we are joined together under the rubric of some kind of 'Weber-Schuster' thesis. That term is used by critics who belong to the first tendency, and whom I have taken to calling 'the discontents' of the *Regulae*.

¹ This is the first part of a two-part discussion about Descartes' method and the problem of method discourse in general, to be presented at the Institute of Philosophy, Czech Academy of Sciences, May 2019. Part II is entitled, 'Method as Mythic Speech: A Challenge in the Historiography of Science—Descartes and Beyond'

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³ Crapulli, G. Ed. (1977) *Rene Descartes Regulae ad directionem ingenii: Texte critique etabli par Giovanni Crapulli avec la version Hollandaise du XVII Siecle*. La Haye, Martinus Nijhoff.

I term them 'Discontents' because the *Regulae* is for them a special kind of text. If you deconstruct it, you are not simply breaking it into temporally and contextually disparate parts, you are blowing up the idea of Cartesian method, and some people—even if only tacitly—do not feel comfortable with that. This gives a special salience to debate about the *Regulae*, because some of those involved are method 'believers', such as I have discussed in various publications. So they are especially touchy about contextual and textual critical analysis of the text.⁴ Their intellectual God father is the semi divine (for French conservative and Catholic Cartesian scholars) Jean-Luc Marion, who is undoubtedly completely wrong about the most important issue in *Regulae* hermeneutics, as we shall see below.

Indeed, there has emerged in philosophy and history of philosophy a bit of an industry, of debunking Weber and Schuster. Some people even attribute Weber-Schuster to Garber and Dear and go off and attack them. I wished they'd correctly name me—I love citations and keep track of them closely.⁵ My concern here is that what philosophers carping about Weber/Schuster really show is not that they are right and we are wrong or vice versa, but that historians and philosophers in this area at least continue to have quite different and to a considerable extent non-overlapping skills, concerns, standards, and aims. I don't say the discontents are wrong per se; only different, but if their claims are taken as historical claims, they are demonstrably wrong.

I have discussed and further developed my views on these matters in several publications stretching from 1980 to 2013.⁶ I shall canvass my findings in a moment. But we have to note a most important recent development which lurks behind all discussions of the *Regulae* these days, but which is far from being resolved, indeed far from even beginning to be addressed. In 2011, just as I was finishing the manuscript of my *Descartes-agonistes*, something astounding happened: Another manuscript of the *Regulae* turned up, the first in three hundred years, discovered in Cambridge by Richard Sergeantson. It appears that it is older than known ones, possibly contemporaneous with Descartes. Another great *Regulae* war is in the offing, and I am delighted: As we shall see, I can easily argue that the new Ms. fits perfectly in my already established larger story about the text and context of the *Regulae* in Descartes' early life. That is I am ready and have the high interpretive ground well prepared in the literature. I was able to insert into *Descartes-agonistes* two long footnotes foreshadowing my response to the discovery. Their content will reappear below. Nothing I have subsequently learned about the still yet to be published Cambridge Ms. substantially changes my original view of the birth, trajectory and fate of the text of the *Regulae* in Descartes' early career.

I hold that there are three chronological and textual strata in (and around) the text of the *Regulae*, which may be noted in brief as follows:

[1] 'Universal Mathematics' in the text that is named Rule 4B (mid 1619).

[2] 'Universal Method' in Rule 4A, Rules 1-3, 5-7, and most of Rules 8, 9 to 11 (can be as early as November 1619, not worked on after 1626/7).

⁴ Here are some of the recent works of the discontents. Jean-Luc Marion (1981) *Sur l'ontologie grise de Descartes*. 2nd ed. Paris; Frederick P. Van de Pitte (1991), 'The Dating of Rule IV-B in Descartes' *Regulae ad directionem ingenii*', *Journal of the History of Philosophy* 29: 375-395. Roger Florka (2004) 'Problems with the Garber-Dear Theory of the Disappearance of Descartes' Method', *Philosophical Studies* 117: 131-141. ; Bret J. L. Doyle (2009) 'How (not) to study Descartes' *Regulae*', *British Journal for the History of Philosophy* 17:3-30; Erico Andrade M. De Oliveira (2010) 'La genèse de la méthode cartésienne: la *mathesis universalis* et la rédaction de la quatrième des *Règles pour la direction de l'esprit*', *Dialogue* 49: 173-198 Nathan Smith (2010) *The Origins of Descartes' Concept of Mind in the Regulae ad Directionem Ingenii*, Ph.D dissertation Boston College.

⁵ Recently one of my esteemed University Sydney colleagues, my old friend Peter Anstey chimed in, claiming that a paper in a Romanian collection has demolished the Weber Schuster view of the *Regulae*. Quite apart from the fact that I am in congenial correspondence with the author of this supposedly devastating attack—an author who admits to deep admiration for my work and its constant influence on him—this remark of Peter's is unjustified, off target and completely misunderstands my project and the evidences for it.

⁶ Schuster (2013) chap. 5 and 7, (1993), (1986), (1980) (1977) The works of the 'discontents' listed above, excluding Marion, have about 37 citations as of this date. The works of mine listed here have in excess of 210 citations to this date.

[3] A 'Parisian/Mersennian' version of Universal Mathematics (1626-28): Parts of Rule 8 (including discussion of the law of refraction, discovered by Descartes 1626/7); Rules 12 to 21 (where the text stops and was given up as a failed project)

There was no work on the text after late 1628 or early 1629. Descartes then moved to Dutch Republic, and start work on his dualist metaphysics and *Le Monde* (his first system of corpuscular-mechanical natural philosophy) mid 1629 ff. I used to assume, with Weber that the text called Rule 4B was always meant to be part of the text. However, I now fully accept the view of Daniel Garber and others that that is a mistake and that 4B is a separate text.⁷ What I continue to insist upon, however, is that 4B is earlier than the other strata. Dealing as it does with something called 'universal mathematics', to be discussed shortly, it served as the template for Descartes' subsequent discussion of method in stratum [2]. The determination of these strata will be discussed below in Sections 3 and 4, but it should be noted that only a miniscule bit of my evidence concerning text and context will be mentioned here. Anyone wishing to dot the i's and cross the t's should look at the these publications.⁸

2.0 The Core of Descartes' Teaching on General Method

Whether we look at the first half of the *Regulae* or at the published *Discourse on Method* (1637) we find the following teaching by Descartes about universal method:

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' 'lattice-work' 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 lattice-work; 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 judgment; 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, while *Regulae* I and II present and develop the notion of the unity of the sciences. Secondly, there is an open ended set of heuristic rules, initially gathered from easy excursions around parts of the lattice-work of knowledge. These are contained in part in rules 2, 3 and 4 of the *Discours* and most of *Regulae* 5 to 11. I say most of these rules because there is other, telling stuff interpolated there, which we will meet in a few moments.⁹

In the *Regulae*, as compared to the *Discours*, we meet an elaborate explication of the vision of the lattice-work of rational truths. Descartes tells us in *Regulae* VI that the logical chains of truths consist in 'absolute' terms linked to a 'series' or 'relative' terms through a greater or smaller number of rationally specifiable 'relations' (*respectūs*). 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 'participate in 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 contact more relations subordinated one to another'.¹⁰

⁷ For details on why this is the case, see below the penultimate paragraph of this Section.

⁸ Schuster (2013), 225-262; 307-346; Schuster (1992); Schuster (1986); Schuster (1980).

⁹ Full details, see Schuster (2013), 252 and note 66 thereto.

¹⁰ Schuster (2013), 253.

It is off this image that Descartes pulls his heuristic rules in Rules 6 through 11: He advises us, Rule 6, to note always the absolute term in question and the order of relations binding the relatives to it. From this sound advice, Rule 6 continues, will flow an ability to classify the problems that can arise about the series, and from that we will develop sagacity in ferreting out the simplest routes of solution to given sorts of problems. Moreover, Rule 7, we must proceed in order, deductively, and review our steps afterwards. More general rules then follow: Rule 9 says you should train yourself to intuit well by starting with simple matters; Rule 10, similarly advises that in order to learn to discern the orderly texture of series, start with simple and to-hand examples. Rule 8 meanwhile told us not to wander where deduction cannot carry us and to recognize when reviews need to be 'complete' and when merely 'sufficient'. All pretty banal, but Descartes actually insists at the end of Rule 7 that virtually the whole of the method consists in these profundities. So much for the main method teaching in the *Regulae*, identical with but much more detailed than that made public later in the *Discourse*.¹¹

But what of Rule 4 which I have barely mentioned? Here is where the fun starts, where Weber started and where I have followed and elaborated on his claims: Rule 4 is entitled '*a method is necessary for finding out the truth*'. It is the bridge between the doctrine of intuition and deduction in rule 3 and the rules of heuristic method in rules 5 to 11. Descartes writes that method consists in 'certain and simple rules' which if followed will prevent our assuming 'what is false as true' and our 'spending effort to no purpose'. Distinguishing true from false is the business of intuition and deduction; preventing wasting of effort is the business of the heuristic rules.

So far so good. But if you look closely at Rule 4 as it has usually been presented in modern editions of the *Regulae* (following Mss. N and A) something very odd emerges. Rule 4 consists of two autonomous sections which have simply been juxtaposed.¹² The two sections are almost identical in structure and flow of argument, but they have two different subject matters. One discusses method as I have just cited it; the other section discusses universal mathematics. In each section Descartes invokes the idea of a hidden wisdom lying behind classical mathematics, and he hints that modern algebra contains traces of the discipline in question. In the first section that discipline is method, in the second go round in the next section it is 'universal mathematics'. These two passages are termed, after Weber as Rule 4A (method) and Rule 4B (universal mathematics).¹³ As mentioned above, it is now quite clear that Rule 4B was not intended to be part of the text. The Cambridge MS does not contain it and Ms. H had relegated it to the end of the text. The texts of 4A and 4B remain very closely related, however, as will now be explained in more detail.

Despite the parallel argument structures, neither section mentions the subject of the other—although the Cottington et al translation very unhelpfully inserts the term 'method' at least six times into the text of Rule 4B—an egregious mistake, misleading to the hordes of Anglophone historians of philosophy who do not bother to read the Latin or check the history of translations to French, German and English (like the Haldane version which gets it right!).¹⁴ Rule 4A on method fits precisely and rationally into the flow of rules 1 to 11, as I have just shown. Rule 4B on universal mathematics finds no echo in these early rules. The method in 4A is universal in scope and is described in wildly enthusiastic terms. The universal mathematics in rule 4B is limited to the properly mathematical disciplines—arithmetic, geometry and mixed mathematical fields, and it is more modestly described. Weber, I and others who agree therefore conclude that Rule 4B on universal mathematics was written prior to Rule 4A: Rule 4B is part of a lost or aborted earlier treatise on universal mathematics whilst Rule 4A is an integral part of the text on method constituted by rules 1 through 11. It may have been added to Rule 4 in the N and A editions, but it exists at the end of the H and is missing in the Cambridge Ms.

¹¹ Schuster (2013), 254-7

¹² Following Weber (1964), 7ff. Rule 4A = AT X, 371 l.1 to 374 l. 15; Rule 4B = AT X, 374 l. 16 to end of rule.

¹³ Schuster (2013), 235-8.

¹⁴ Schuster (2013), 235 Note 24. Cf. Cottington et al. pp.18-19. Haldane and Ross and later Brunschwig in the Alquié edition translated the terms in question correctly as 'art' or 'invention' by simply following the Latin.

3.0 *The dating of Rules 4B and 4A: from 'physico-mathematics', to 'universal mathematics', to method. March to November 1619:*

Leaving the text of the *Regulae* and attending to some early fragments of Descartes, it is possible to go back one step prior to the universal mathematics of text 4B; to understand where that dream came from, and show how Descartes quickly extrapolated from that dream to the dream of method expressed in rule 4A and the opening portions of the *Regulae*. The key here is the idea, and project of 'physico-mathematics'—something Descartes pursued from late 1618 when he met Isaac Beeckman. He continued with it into early 1619 and arguably into 1620, leaving some well-known, but little studied manuscript fragments.¹⁵

Shortly after he met Descartes in 1618 Isaac Beeckman wrote in his diary: '*there are very few physico-mathematicians*', going on to claim that he and Descartes certainly are such. The term was formed in contrast to the Aristotelian understanding of the mixed mathematical sciences, such as optics, mechanics, astronomy or music theory. They used mathematics not in an explanatory way, but instrumentally to represent physical things and processes mathematically. So in geometrical optics, one used geometry, representing light as light rays—this might be useful, but for Aristotelians didn't get at the underlying natural philosophical questions: the physical nature of light and the causes of light phenomena.¹⁶

Now, in physico-mathematics, as Descartes and Beeckman saw it, the old mixed mathematical fields would no longer be subordinate to—but rather become proper domains of—one's favored natural philosophy. In their case the preferred species of natural philosophy was a radical corpuscular-mechanism to which Beeckman had introduced Descartes. Conversely, novel findings in the formerly mixed mathematical sciences would now bespeak new insights into whatever natural philosophy a physico-mathematician favored. Physico-mathematics wasn't about the mathematization of natural philosophy. Physico-mathematical gambits envisioned the *physicalisation* of the mixed mathematical sciences: We're not talking about mathematization of anything, but the physicalisation of parts of mixed mathematics—which of course were already mathematical—whereby some natural philosophers aimed to render the mixed mathematical fields more physical, more about matter and cause discourse within one's favoured natural philosophy.

Stephen Gaukroger and I have written about Descartes and Beeckman's attempt to 'physico-mathematicise' Stevin's hydrostatics.¹⁷ I've also presented the trajectories of Descartes in optics and vortex mechanics as physico-mathematical in tenor. And the entire third chapter of *Descartes-agonistes* is devoted to Descartes' early physico-mathematical work, 1618-20, in hydrostatics, in optics and in regard to the law of falling bodies. The philosophical 'discontents' about the *Regulae* tend to disregard the young Descartes' physico-mathematical initiatives. Nevertheless, they are highly relevant, because, arguably, what Descartes meant by universal mathematics a few months into 1619 was a unified problem solving approach in physico-mathematics *and* pure mathematics—that is, in all the properly mathematical fields.

We can date this early physico-mathematical work. Descartes met Beeckman in late 1618. He worked on hydrostatics and the law of falling bodies in a physico-mathematical way with Beeckman and corresponded with him about these matters into 1619. Another fragment from 1620 dealing with the refraction of light also bespeaks a physico-mathematical approach, as I have demonstrated in detail elsewhere. Now, the correspondence also gives a date before which Descartes could not have hit on universal mathematics. In March 1619 Descartes writes a letter to Beeckman about his mathematical agenda. He hasn't yet conceived of his universal mathematics. He envisions a loose classification or compendium of types of solution techniques for differing classes of mathematical problems.¹⁸ However, in the *Discourse on method* Descartes dated his initial method insights from November 1619. There is no reason to place them before that, but nothing much depends on when he worked out

¹⁵ Full details are contained in Schuster (2013), chapter 3

¹⁶ On physico-mathematics, Schuster (2013), 56-9 and chapter 3.

¹⁷ Gaukroger and Schuster (2002).

¹⁸ Descartes to Beeckman 26 March 1619, AT X 156-7; Cf. Schuster (2013), 242, note 51.

his method—talk after November 1619 and wrote it in the early parts of the *Regulae*, except that it all must have taken place before he discovered the law of refraction of light in 1626/27, because the text takes a strange new jag, with a new, third stratum at that point.¹⁹

Therefore, universal mathematics in text rule 4B must likely after March 1619 and before the explosion of method excitement in November 1619, and it may have some echoes in it about physico-mathematics. So let's go back to the text of Rule 4B which expounds this universal mathematics. 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.²⁰

All of this reflects neo-Platonically inspired ideas about a 'general mathematics' that were current in the late 16th and early 17th centuries.²¹ The text contained in Rule 4B was probably written as part of an early projected treatise on universal mathematics; it predates the surrounding text of the *Regulae* which was composed after November 1619 and deals, of course, with the method. As noted earlier, I now agree with those who hold that rule 4B was never intended to be part of the (later) *Regulae* text and that it has been erroneously located there by various earlier editors. I have argued that Descartes formulated the text we call rule 4B by combining those available notions of a universal mathematics 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 this proportional compass. In particular he was interested in its rough and ready practical use to solve problems. He focused 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 generalize.²² (Fig. 1)

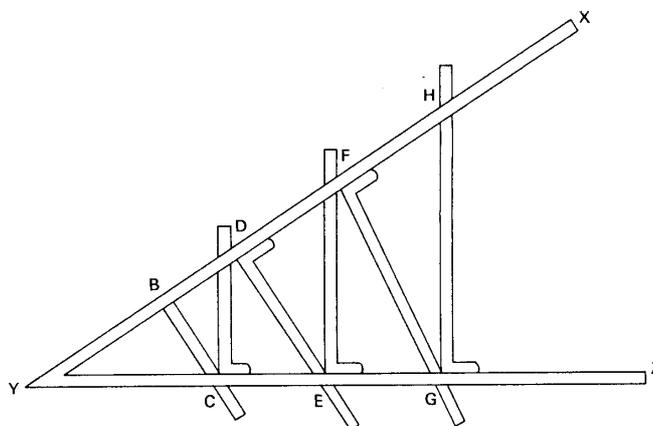


Figure 1. Descartes' Proportional Compass 1619, 1637

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 is 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

¹⁹ For dating of the first glimpses of method from November 1619, Schuster (2013), 238. We can firmly date the discovery of the law of refraction in 1626/27, not earlier or later. See Schuster (2000), 274-5; (2013), chap 4, especially 186-7 and Appendix 1.

²⁰ Schuster (2013), 230-5.

²¹ Schuster (1980), 42-45; (2013), 242-7.

²² For more detail see Schuster (2013), 239-42.

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 the text on universal mathematics now contained in Rule 4B was composed as part of the larger intended treatise on that topic. But it was only a dream, his techniques did not even work for all the algebraic problems he had attempted. Yet, he did have before him the successful special cases and the overblown grand idea.²³

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 overextended*. We have in the *Regulae* some of the fossil traces of this process of extrapolation. Let us recall the peculiar portion of *Regulae* 6 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* 7 through 11. 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* 6 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 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 overexcited young Descartes thought, wrongly, was true of universal mathematics, he daringly extended into the realm of all rational enquiry. The method discourse 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.²⁴

So despite the insistence of the discontents that the texts of rules 4A and 4B do not bespeak anything different from each other, it is clear by textual analysis; contextual evidence, timing and content of correspondence and of physico-mathematics that the text in 4B is about something new—universal mathematics—and that it is earlier than, and different from, the method talk in rule 4A.

²³ Schuster (1977), 116-124; (2013), 239-48.

²⁴ Schuster (2013) 257-60.

4.0 *The Structure of the Later Regulae, 1626-28*

We can now turn to the third and final stratum in the text of the *Regulae*, some of rule 8 and rules 12 to 21 where the text stops. In this stratum, written after he discovered the law of refraction in 1626/7, and before he left to settle in the Netherlands in early 1629, Descartes returns to his idea of universal mathematics, which he is now going to articulate in detail, attempt to ground or legitimate, and broadly set upon the framework of the grand method that the text has been talking about in rules 1-3, 4A and 5 to 11. Rule 8 first reveals the new initiative of the mid and late 1620s. The first two paragraphs of rule 8 advance general heuristic advice in the spirit of rules 5 to 7. Then, all of a sudden, three paragraphs into rule 8 Descartes breaks off his heuristic rule discussion and starts talking about how to discover the law of refraction. This then leads onto further stark novelties of aim and agenda in the remainder of rule 8, which are worked out in rules 12 forward.²⁵

Now, in the methodological story in rule 8 in which Descartes tells about how one 'might' discover the law of refraction of light and anaclastic curve, he uses the subjunctive voice. He is saying that one could perhaps do it this way. I have published extensively to show how Descartes in fact discovered the law of refraction around 1626/27 and how his cover story spins that experience, covering pitfalls and issues that critics could have fatally seized upon. He did not discover the law the way he says one might perhaps do it using the method. Note however that this passage, seemingly inserted later into the smooth flow of banal heuristics opening rule 8, must date from after the discovery of the law of refraction in 1626/7.²⁶

All this is interesting but not nearly so much as what immediately follows in the text of rule 8. Descartes starts talking about another methodological example which he calls 'the most splendid of all'. It initially surfaces as a proposed application of the method to discover the capabilities and limits of the mind itself. But the text of rule 8 rolls on, articulating and shifting this idea, seemingly as Descartes wrote. In the process the project of the later *Regulae*, rules 12 forward, begin to emerge. Eventually Descartes decides in rule 8 that two problems are involved: an inquiry into the nature and limits of the mind, and into the character and status of the objects of knowledge. Here the plan for what we have in rules 12 to 21 first comes into view; this plan in other words governs the shape and aim of the latter part of the text—it was never discussed or even hinted before in the text. The obvious conclusion for the historian is that here we have Descartes, sometime after his optical triumph, that is in 1626/7—and before dropping the text in 1628—reorienting the project of the text.²⁷

What we have in this third and final stratum in the text is this:²⁸ Descartes returning to his early idea of a universal mathematics and trying to explicate it in a way that would follow and improve upon the strategies of his friend Marin Mersenne for combating unorthodox philosophies of nature, those of alchemical, neo-Platonic or 'Hermetic' inspiration, whilst avoiding the threat of a fashionable and corrosive scepticism. Mersenne's strategy was to avoid systematic natural philosophy or metaphysics. He would deploy, piecemeal, supposedly reliable bits of physico-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 physico-mathematical 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 rules 1 to 11 of the *Regulae*. Universal mathematics, carrying out Mersenne's tactics,

²⁵ Schuster (2013) 311-314.

²⁶ Schuster (2013), 312, 215-221; See also Section 8 of Schuster, '*A Guide to Descartes' Optical Work*', prepared for the '*Conference on 'Experience and Reasoning in Scientific Methodology: Between Antiquity and the Early Modern Period*', Institute of Philosophy, Czech Academy of Sciences, Prague, May 2019. This should be available on the Institute website for my visit.

²⁷ Schuster (2013), 311-14.

²⁸ For material in this and next two paragraphs, Schuster (2013), 314-333.

would appear to grow out of the doctrine of method. Rules 12 to 21 were written in Paris for this purpose.

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 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 physico-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.²⁹ We then try to establish mathematical correlations amongst such empirically given and mechanically impressed measures of physical quantities which Descartes terms 'dimensions'. 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. Indeed, 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.

5.0 Reasons for the Collapse of Descartes' *Regulae* Program 1628/9

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: The first two problems are crucial in shaping his post 1628 programs in metaphysics and systematic corpuscular-mechanical natural philosophy, but it is the third problem, clearly visible in the text, which blew apart the entire text and led to its abandonment.

(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 post 1628 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*.³⁰

(2) Insistence that the world is known under geometrico-mechanical schemas focussed the problem of the status and origin of non-geometrical perceptions of what later were called secondary qualities. The solution was to extend and metaphysicalise the incipient systematic dualism of the later *Regulae*, so that one could, in the mature metaphysics, clearly distinguish between purely mental 'ideas' and the

²⁹ It was Jacob Klein (Klein [1968]), 198, 202, 208) who first attained the fundamental insight that Descartes was offering a mathematics expressed in and manipulated through line lengths functioning as operative symbols, or as they may be termed 'extension symbols'. Cf Schuster (2013), 325 note 66. It is exceedingly unwise for anyone to attempt an interpretation of the later rules of the *Regulae* without the prior benefit of having studied Klein's epoch making work.

³⁰ Schuster (2013), 334-9.

corpuscular-mechanical states of affairs that sometimes occasion ideas, but which are not necessarily represented by them.³¹ Descartes starts this in the very first chapter of *Le Monde* composed within a year or so of the collapse of the *Regulae*.³²

(3) The third problem is quite explicit in the text—for those with eyes to see and the mathematical and hermeneutical *nous* to understand. This problem obviously exploded the entire later *Regulae* program and relegated the text to the bottom of one of Descartes trunks. The text breaks off at Rule 21 which exists only in a title indicating Descartes is embarking on a discussion of the solutions of algebraic equations.³³ This break is explicable in terms of what he had said earlier in attempting to ground mathematical operations as clear and obvious manipulations in the imagination of lines and rectangles representing 'dimensions'. Now, the solution of equations calls for the extraction of various sorts of roots. According to the project of the later *Regulae* such operations must be grounded in the imaginative manipulations of lines and rectangles, such as he provides in Rule 18 for the four basic mathematical operations: addition, subtraction, multiplication and division. But root extraction cannot be achieved by means of lines and rectangles.

When speaking in Rule 17 Descartes had lauded algebra as a way of rendering 'indirect' problems into 'direct' form. And that is true. In algebra unknowns are symbolised and expressed in terms of relations between knowns, so that solving of a problem takes on the character of a direct deductive movement of thought. All that is fine in the abstract and as abstractly stated in rule 17.³⁴ But, curiously, in rule 18, when Descartes attempts to construct the operations of universal mathematics, he stumbles after addition, subtraction, multiplication and division, when he turns to root extractions.

Speaking of the determination of mean proportionals, mathematically equivalent to extracting roots, he defers treatment, claiming these operations require 'an indirect and reverse movement on the part of the imagination'.³⁵ That contradicts his Rule 17 statement about solving algebraic equations and marks a concession that root extractions require circles or higher order curves.

As a *matter of actual mathematical practice* Descartes can do that—he can solve equations and extract various sorts of roots. But, what he cannot do, he now realizes, is *legitimate the required mental/imaginative operations according to his machinery of rules 12 through 14*. His doctrine of imaginative grounding and manipulation of very simple symbolic instances of extension, lines and rectangles, cannot cut to higher order problems. Following this debacle, Descartes spends the rest of rule 18 discussing direct operations, laying off lines or constructing or deconstructing rectangles. Rules 19, 20 and 21 consist only of their titles, no more mathematical content is given, and here the text gives out.

Remember rule 21 seems to be ushering in a discussion of equations—but the project of legitimating the needed logistical operations in the manner demanded by the machinery of Rules 12 to 14 has proven to be impossible, hence end of inscription, end of text, end of project. Again I emphasise: the issue in Descartes' mind is not his pragmatic ability as a mathematician to handle equations; it is whether this legitimacy machinery, theory of mind, imagination and perception can carry that weight. This is what was now revealed as impossible. Descartes knew this—he stutters, splutters and gives up; that is, gives up on this mode of justifying mathematical operations and practices. His mature 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

³¹ *ibid.*, 339-43.

³² See Schuster (2013), 341-2 for quote from Chapter 1 of *Le Monde* and discussion. On 15 April 1630 (AT I, 137-8), Descartes wrote Mersenne from the United Provinces signalling the end of work on the *Regulae*, and foreshadowing his new projects in metaphysics and corporeo-mechanical natural philosophy: '*Perhaps you find it strange that I have not persevered with some other treatises I began while I was at Paris. I will tell you the reason; while I was working on them I acquired a little more knowledge than I had when I began them, and when I tried to take account of this I was forced to start a new project rather larger than the first.*'

³³ The title of Rule 21 reads, 'Si plures sint eiusmodi aequationes, sunt omnes ad uniam reducendae, nempe ad illam cuius termini pauciores gradus occupabunt in serie magnitudinum continue proportionalium, secundum quam iidem ordine disponendi.' (AT X 469). For more detail on the material in this and the next four paragraphs, see Schuster (2013), 343-44.

³⁴ Schuster (2013), 344.

³⁵ *ibid.*

could supposedly guarantee intuitions which do not have to depend upon imaginative representation, or geometrical presentation at all.³⁶

In sum, my story, long held and further refined, is that the *Regulae*, this concatenation of three stages, failed, Descartes recognized that. In 1629-30, having moved to the Dutch Republic he started on two new and related projects that initiate his mature career: his dualist metaphysics and his systematic corpuscular-mechanical natural philosophy. Both together I have argued are launched on the basis of trying to resolve problems left by the failure of the *Regulae*, and of course Descartes in later life never tried to revive method (seriously I mean) nor universal mathematics.³⁷

6.0 Descartes' Career Inflection Point: Chandoux, Bérulle and the Cambridge Ms.

We know that before he left to settle in the Netherlands, and just around the time the *Regulae* would have failed, two important events occurred in Descartes' life in Paris. First he appeared a soirée to hear the alchemical natural philosopher Chandoux hold forth. Descartes attacked him, and apparently convinced many of his points. He claimed that his performance was made possible by his natural method. He was then summoned by Cardinal Bérulle, the leader of the French counter reformation, and encouraged to develop his projects. Some, like Richard Popkin, think this triggered his anti-sceptical metaphysics, and some of his mechanistic natural philosophy given its consistency with Catholic theology. But both of those enterprises only come shortly later, in the Netherlands. It is more natural to think Descartes was again nattering on to the Cardinal about his method and its uses. He had nothing else to hand of general cultural value; but—and this is the big 'but'—he had recently realised or was just about to realize that the *Regulae* would not work. If you like, you can think that the Bérulle episode later helped trigger his metaphysical endeavours and or his work on systematic corpuscular mechanism. But if you think that something more immediate and to hand was discussed, then you have to look to the *Regulae*, and to Descartes' at that moment lingering or just disappointed hopes in them.

And it is here that the existence of the Cambridge Ms. comes in possibly to supply a missing piece of the puzzle—the 'Schuster version' of the puzzle that is. The new manuscript is characterized by, amongst other things, the fact that it is about forty per cent shorter than the other versions; does not contain rule 4B; ends at rule 16, rather than with the mere title of rule 21, and omits the discussion in rule 12 of 'simple natures', a sore point and space of endless debate by philosophers; and has the material on the law of refraction, hence is post 1626.

All these reported facts, combined with our overall interpretation to this point, suggest the following conjecture about the Cambridge Ms.: The abridged Cambridge Ms. looks to be a version concocted as a holding action, after the discovery of the law of refraction in 1626 and before the final abandonment of the project in 1628. The Ms. seems to have been sculpted to avoid overt revelation of the obvious difficulties arising from rules 17 to 22, so that it might seem to a reader that the project was still on course. I suspect that it was intended for a friend, Mersenne or Beeckman, or potential patron, like Bérulle, to whom, we have seen, Descartes had probably sounded off about his 'method'.

In the context of Descartes' awareness of the failure of the project, and his growing public profile, the idea behind this shortened and streamlined document would perhaps have been something like this: '*People expect something about method/universal mathematics from me after my recent private and public posturing; but the Regulae are not going to work out, as I have recently discovered; however, until I find my way forward, a streamlined, less confused and confusing version of the Regulae can circulate. (When I see how to get around what stymied it, my new projects and results will drive this out of people's minds anyway.)*' A *Regulae* text, lacking the confusingly redundant rule 4B, and ending with rule 16, could still look like a promising and yet to be fully completed project. Had the remaining rules been present (amounting to about ten pages of mathematical material), smart mathematicians, would have seen Descartes' difficulties and textual squirming, and so suspected something was

³⁶ For detailed explication of these developments, Schuster (2013), 345-72. That there was a major inflection in Descartes' intellectual career involving the suspension of work on the *Regulae* has been clear to some Cartesian scholars for several generations. Readers might care to examine the brilliant analysis of Léon Brunschvicq (1927) and (1922), 106-123.

³⁷ Recall Descartes' key letter to Mersenne 15 April 1630, cited above Note 32, clearly reflecting these developments.

seriously amiss. Judicious cutting made the text look as though it were still representing a living project, and that clear sailing would still be ahead, should Descartes move past the intriguing rule 16. In short, on my textual and contextual reading of the *Regulae*, if the Cambridge Ms. did not exist we would have to make it up, because being an early version it fits perfectly with my account of where Descartes had gotten to by 1628 with the text and in public showing off. So bring on *Regulae* World War II, I say to my history of philosophy friends.³⁸

7.0 Some Untimely Reflections on Historian/Philosopher Relations

To put it ideal typically, and there are exceptions, a philosopher wants to read an important philosophical text for unity of conception and argument, then point out lapses and pitfalls. But the kind of unity sought is what registers in philosophers' conversations; philosophers' grant applications, philosophers' promotion cases and philosophers' general chit chat. For the *Regulae* it is that Descartes, the great *philosopher* in their *professional* sense, is here grappling with some unitary idea of method, around which must cluster the other stuff he talks about, like universal mathematics (which may just be another manifestation or expression of same).

It does not occur in this thought-world that Descartes is a physico-mathematician on the make; at first only moderately successful as he pens the earliest parts of the *Regulae*; then a very successful physico-mathematician in possession of the law of refraction of light, as he returns to the text several years later. It does not register that what Descartes says about mathematics in the text is not about his actual work in that field but about an attempt to rationalize or legitimate its possibility and its logistical procedures; nor *à fortiori* is it of much importance to many philosophers to see that what he tried to do turned out to be impossible on his own knowledge base, and that, tellingly, the text breaks *off right there*.

Nor do the existing letters play much of a role in philosophers' accounts as they do in the historians' genetic reconstructions of the production, modification, alteration and abandonment of the text—after all letters, even Descartes' are not usually philosophical documents. Descartes' correspondence, and his manuscript fragments dealing with physico-mathematics, confirm his emerging ideas about universal mathematics in mid 1618; and confirm that he had overthrown the whole project by the time he settled in the Dutch Republic in 1629. Such letters are not all that important in trying to construct an atemporal, static, ideally unified reading. But they, like other bits of contextual evidence are crucial in trying to reconstruct a course of work, a process, an adventure if you like in drafting, redrafting, cutting, pasting and ultimately giving up and looking for explanations of one's previous expenditure of time. Consider the role of my reconstruction of Descartes' physico-mathematics in this story. One never finds that in the discourse of the 'discontents'. Hmm! If you ignore Descartes' physico-mathematics of 1618 forward, you will surely misunderstand universal mathematics in the text of rule 4B and you are well on your way to misunderstanding the *Regulae*.

Contexts, large and small, and movement, dynamics, process and change are the categorical frames of historians' inquiries, and outputs—narratives-*cum*-explanations of such processes. Such things are not taught to philosophers *qua* philosophers and certainly they are not initiated into the ongoing conversation of historical experts about what types of context and what senses of movement are important to consider in treating any given historical problem or topic.

Let me close with a quote from one Brett Doyle, author of one the anti-Weber Schuster diatribes with the wonderful title: 'How not to read the *Regulae*.' He writes, *if the Regulae is incoherent we can hardly learn much from studying it.* (p.4) Now that is clearly right—on condition that you are convinced that the *Regulae*, as a whole and taken in its entirety, must be a coherent philosophical document, worthy of Descartes spouting off about it in the Oxford Philosophy Faculty tea room. (By the way, have I ever said the *Regulae* are 'incoherent'? They are quite coherent to the historian's eye for diachronicity, dynamics and change.)

³⁸ There is another way of interpreting the Chandoux episode and Descartes' relations with Cardinal Bérulle, which does not essentially affect this view of the place and role of the Cambridge Ms. version of the *Regulae*. See my review essay, Schuster (2019), of Harold Cook (2018), *The Young Descartes: Nobility, Rumor and War*.

So, if you should quote Doyle's pronouncement to a group of philosophers, all but the historically sensitive will sagely nod assent and say, '*Send that paper to the BJHP immediately, old boy! Schuster and Weber must be mad, bad philosophers and hence dangerous.*' If you say that to half way intelligent and well educated historians, not just historians of science, they would have to smile to suppress an outright laugh. Thus, let me be perfectly clear: Study of text and context shows that there are strata in the text of the *Regulae*; chronologically and intentionally distinct strata. Doyle, Smith, Anstey, Florka, even the semi-divine Marion and the others are like dysfunctional archaeologists looking a well presented cross section in a dig and saying, '*nothing to see here, ladies and gents, all these cities and cultures are really the same in the end*'. End of archaeology; end of history. I rest my case, having shown along the way that the Cambridge Ms. is unlikely to prove the unity of the text, but rather take its likely place in the cross section of trench I have dug.

I shall not say the philosophical 'discontents' of the *Regulae* are wrong (although they naively say that Weber and I are wrong). I shall say we are different; we have different jobs. But if they want to keep citing me on how not to read the *Regulae* (as a philosopher) I will accept the addition to my lifetime citation count and Hirsch Index (which is quite healthy at this late date, as it should be).

* * *

We can now proceed to Part II of this exercise, my analysis of Cartesian method as mythic speech. It need only be read by those intending to attend the second hour of my presentation.

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Abstract: The *Regulae* and *Discourse on Method* teach a method that cannot work but which has persuaded many that it can—arguably including Descartes himself, at least up until the collapse of the *Regulae* in 1628. The problem, moreover, is more general, pertaining not just to Descartes' method but to any of the grand methods that have been proclaimed to be universal, efficacious and transferable: methods of Newton, Popper, Lakatos and others. All suffer from the same problem: Such methods consists of a type of discursive structure which [a] prevents the method from actually being efficacious; yet, [b] creates powerful literary effects (illusions) that the method is efficacious, progressive and transferable.

Part II

Method as Mythic Speech: A Challenge in the Historiography of Science—Descartes and Beyond¹

Dr. John A. Schuster, FAHA²

1.0 *The Cult of Method in the History of Science and Cartesian Studies*

Up until the later twentieth century, interpretations of the Scientific Revolution tended to be dominated by heroic tales of the discovery, perfection and application of the 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—singular, transferable and efficacious—so that its further perfection and wider application could insure the future growth of the sciences³. Earlier in the twentieth 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.⁴ 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,⁵ 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.⁶

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. 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.

¹ This is the second part of a two-part discussion about Descartes' method and the problem of method discourse in general, to be presented at the Institute of Philosophy, Czech Academy of Sciences, May 2019. Part I is entitled, 'Reading Descartes' *Regulae*: Genealogy, Structure and Fate of the Text'.

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³ 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.

⁴ 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.

⁵ 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)

⁶ 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.

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 turn⁷.

This paper is motivated by some news which will probably be unwelcome amongst method cultists: we have excellent grounds for being 'atheists' about method. In the past two generations 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.⁸ From this perspective it follows that apologetic scholarship directed to Descartes' method is misguided 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 2 we locate the grounds of modern 'atheism' about method in the history of science in the writings of Koyré, Kuhn and Bachelard. Sections 3 and 4 prepare the ground for my approach to method as mythic speech. Then we examine in Section 5 an example of de-methodologized, 'post-Kuhnian' analysis of Cartesian scientific practice. Sections 6, 7 and 8 then analyze 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 9 turns to the issue of some of the micro-political and rhetorical functions of Cartesian method discourse.

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

It is 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, and since the 1970s 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.⁹ 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*

⁷ 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.

⁸ 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.

⁹ A.Koyré, *Etudes Galiléennes* (Paris: Hermann & Cie, 1939); Koyré, *Galileo Studies*, trans. J. Mephram (Hassocks, Sussex: Harvester, 1978); Koyré, *Metaphysics and Measurement: Essays in Scientific Revolution*, (London: Chapman & Hall, 1969); Koyré, 'The Origins of Modern Science', *Diogenes*, 16 (1956), 1-22.

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.¹⁰

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 forty 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.¹¹ To paraphrase Bachelard, the meaning of a concept must include the technical conditions of its material realization.¹² 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 armory. 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.¹³

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

¹⁰ A. Koyré, 'The Origins of Modern Science', *Diogenes* 16 (1956), pp.1-22.

¹¹ G. Bachelard, *Le Nouvel Esprit Scientifique* (Paris: Presses Universitaires de France, 13e edn, 1975); Bachelard, *Le Rationalisme Appliqué* (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.

¹² G. Bachelard, *La Formation de l'Esprit Scientifique* (Paris: Vrin, 9e edn, 1975), 61.

¹³ 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 11).

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 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. [Fig. 1]

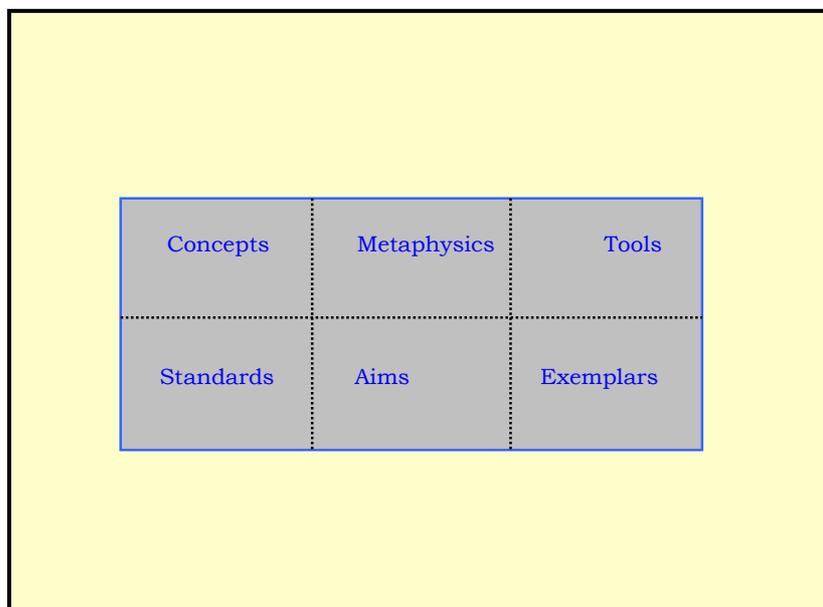


Figure 1. The Kuhnian Disciplinary Matrix of Elements in a Paradigm

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, neighboring 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.

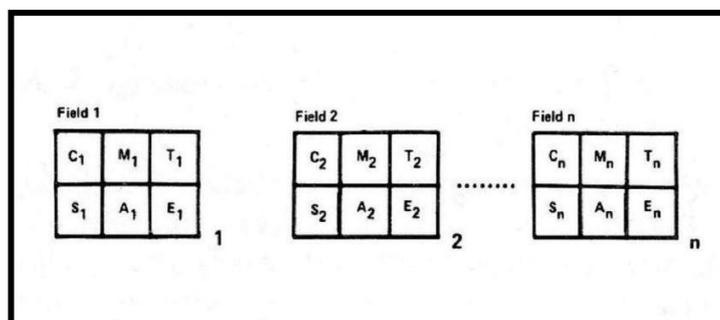


Figure 2. A set of *n* coexistent, *sui generis* paradigms or *n* conceptually and materially *sui generis* research traditions at a given moment of time

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 practiced 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.

Classic work in the sociology of scientific knowledge in the 1970s and 80s 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.¹⁴ 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.¹⁵ 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.

Indeed, the post-Kuhnian case against method does not stop here. Over the past two generations historians and sociologists of science have examined 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 subtly 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 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. 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.

3.0 *The Way Forward: Between Naïve Belief and Pure Debunking*

The Koyré-Bachelard-Kuhn debunking of method obviously marked a step forward in the historiography of the sciences, at least for those who took this crucial work seriously. There was, however, a deeper problem raised by the debunking of grand methods. It is this. If method talk is complete nonsense and of no account whatsoever in the life of traditions of scientific research, we may reasonably ask, 'How, then, can it 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-Koyréans' 'know' cannot have worked?' No historian wishes to accuse his subjects of being fools or mad persons, just because they appear to disagree with him. Therefore, we are obliged to discover just what it is about systematic method doctrines that creates and sustains their plausibility to believers,

¹⁴ J. R. Ravetz, op.cit., (Note 13); 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).

¹⁵ For early 'derivations' of this position from the writings of Kuhn see Ravetz, op. cit. (note 13), and J.A. Schuster, 'Kuhn and Lakatos Revisited', *British Journal for the History of Science*, 12 (1979), 301-17. Later observations along these lines are contained in Schuster (2016) and (2018), 402-7.

past and present. We must, in short, become more like anthropologists of method, seeking to understand how belief in various types of putatively unified, efficacious methods is sustained amongst certain groups and what are the consequences of those beliefs (and differences of opinion about them) for players in living traditions of research, despite the fact that we cannot possibly subscribe to the substance of their beliefs. Debunking of general methodologies is not a sufficient strategy. There is more to learn about the life of method-talk in the dynamics of the sciences, even if general methods do not and cannot work on their own terms. This issue has motivated much of my work since about 1980.

My answer to this question, previously developed not only in relation to Descartes' method, but to any grand method doctrine past or present is this:¹⁶ *All systematic method doctrines belong to a definite species of discourse. The species is characterized 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 easily sustains or creates a set of illusions (in the form of literary effects) to the effect that the method in question can indeed accomplish what it claims to be able to do.* In other words, all grand, 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. This, I suggest, is the way forward in dealing with Descartes' colossal claims about his method—claims he apparently genuinely believed in (at least up to the late 1620s), but claims we should never literally accept as explanations of his technical achievements (let alone their order and trajectory, as he asserted in the *Discours*).

4.0 Descartes' Method as Mythic Speech: Where 'Myth' is not a Colloquial Term of Abuse

I have always used terms like 'myth' or 'mythic speech' to describe the species of discourse described above in italics, characterizing my model of what method-talk is, and how it works.¹⁷ I have always meant such words seriously, but not dismissively. Yes, I concede that in part they were intended to have a certain shock value for those readers who do not share in the not entirely uncommon, but I think largely tacit, post-Koyréan opinion that 'method is myth' in the usual dictionary sense of the term. *More importantly my choice of words intends something more theoretically precise, and not at all dismissive or abusive:* My use of the terms 'myth' and 'mythopoeic' (myth making) derives, at one or two removes, from Roland Barthes' early essay 'Myth Today'.¹⁸ Barthes claimed to identify a peculiarly modern ('bourgeois') form of myth in which the trick is to naturalize values, interests and socially negotiated outcomes, making them appear to be factual, natural and inevitable.

It seemed to me that one characteristic myth, operating in exactly this manner, is myth of scientific method, even though in this case the mythopoeic discourse in question can be traced back to Aristotle and up to our methodological prophets of modernity, such as Bacon and Descartes, through the high medieval and renaissance methodological debates of the Scholastics. After all, if the naturalizing of human commitments of theory, value and aim is the mark of modern forms of myth, then we have to accept that perhaps the first example of a characteristically modern Western myth was indeed the myth of scientific method, and although it did not start with the heroes of the Scientific Revolution, it certainly was given new force and cultural cachet as a result of being attached to the novelties of natural philosophy and the sciences emergent in the seventeenth century. Now, according to Barthes these effects are brought about by the structure of the discourse in question. Although I do not deploy the kind of semiotic techniques which Barthes advocated, I do claim to have identified certain structural levels in any systematic method discourse and I locate the persuasive power and naturalizing force of such discourses in the relations holding generally amongst these levels. In my account grand methodologies are discourses so structured that they necessarily lie about their own powers and capabilities in the interest of turning *culture* (how the natural sciences are actually practiced) into

¹⁶ I began to generalize from Descartes' case to systematic method discourses of any type in Schuster (1984).

¹⁷ Hence the full title of my (1984) 'Methodologies as Mythic Structures: A Preface to the Future Historiography of Method',

¹⁸ Barthes (1973) 109-59. Also relevant here was the work of Claude Lévi-Strauss (1972), 216, 224. Cf Schuster (2013) p. 217 Note 92.

nature (a simple outgrowth of human rationality and nature's amenability to it) In these precise senses, then, methodologies deserve the (Barthian) title of myths.¹⁹

Given all this, we can pose the following question in the case of Descartes and his method. '*How can it be that Descartes and others apparently believed in the reality and efficacy of a method which most post-Koyréan historians of science are convinced cannot have worked?*' My fundamental claim is that the vacuity and sterility of the method *and* its appearance of efficacy are both effects of a common cause. That cause is the way Descartes' method discourse is structured onto several interacting levels. Descartes' method cannot possibly do what it claims to be able to do, because, as discourse, it has a particular structure; and yet it is that very structure which can create and sustain illusions or literary effects about the efficacy, applicability and unity of the method.

The analysis begins from a naive but fundamental premise: In order for the rules of the method to be considered *efficacious* in the practice of a given field of research, the rules have to be applied and deployed within inquiry in the target field in ways adequate to the proclaimed goals and foci of the method.²⁰ Granting this point, it would seem that Descartes must give some arguably adequate account or redescription of the contents and workings of the target field and that this account must be couched in terms supplied by the core of his methodological discourse, his talk of 'absolutes' and 'relatives', of 'relations' and 'series', which are aspects of the latticework, as discussed in Part I, Section 2 of this Guide above and in Schuster (2013,248-52). This is necessary because the heuristic rules are claimed to apply to entities of this type. The heuristic rules of Descartes' method, after all, were formulated directly in terms of, and are clearly 'relevant' and 'applicable' to the discourse about 'absolutes', 'relatives', 'series' and 'relations'. Approaching a target field with his method, Descartes must be able to construe that field in terms relevant to the use and application of his rules. Descartes, as we shall soon see, accepted these conditions and worked within them. He thought his results exemplified the efficacy of the method.

However, following the dictates of Section 2 above, I subscribe to a post-Kuhnian understanding of the dynamics of living traditions of research. Any grand method discourse aims to produce '*arguably adequate accounts of the contents and working of its target fields*'. I hold that such methodological accounts, adequate in the sense required cannot be achieved. The post-Kuhnian understanding of research tradition dynamics sees the conceptual structures and modes of practice of living fields as both *sui generis*, and in constant re-negotiated flux. We saw above that to redescribe, gloss or translate these conceptual structures and modes of practice into some other idiom or discourse, is simply to

¹⁹ My approach also owes much to the anti-methodism of Paul Feyerabend, although in a precise way which requires clarification. Post-Kuhnian debunkers of method can, I think, perceive two rather distinct initiatives in Feyerabend's work. On the one hand there is Feyerabend's historical critique of methodology, consisting mainly in case-study illustrations of the non-binding character of any and all systematic methodologies. [Feyerabend (1975, 1978)]. 'Progress' in science, he persuasively argued, has always broken the pat rules laid down by methodologists, and it has always *had* to do so. New standards are constructed and refined in the act, through the very processes of major scientific change; and, in the cases studied, Feyerabend tended to show that rigid adherence to the rules of contemporary (or later) methodologies would have aborted or obstructed the course of development. Feyerabend's efforts in this direction must seem brilliant and historically revealing to any Koyré- or Kuhn-influenced debunker of method. Nevertheless, this work did not really constitute a great advance in our ability to theorize seriously about the nature of methodological *discourse*. On the other hand, in a small and rather neglected corner of his work, Feyerabend, in my opinion, offered the first sustained demonstration of the structural sources of the mythic character of method discourse. In his important paper, 'Classical Empiricism', Feyerabend laid bare the discursive mechanisms by which Newton's methodological claims in physical optics present a systematically distorted picture of his actual practice, producing a convincing fairy tale about the genesis and status of his claims in that field. [Feyerabend (1970)]. Although Feyerabend did not pursue his analysis in explicitly structural terms, his approach, viewed through my semi-Barthian spectacles, helped to catalyze my own structural schema for methodologies. How Feyerabend's argument maps onto my structural schema for method discourse is shown below, see Note 55. My earliest suspicion that there was a specific mechanism of mystification involved in methodological accounts was aroused in the mid 1970s by reading Bachelard (1949), where he deals with the systematic role of traditional philosophical perspectives (not particularly methodologies) such as empiricism, rationalism, conventionalism, in producing a structured series of illusory pictures of how theory and practice (or 'applied rationalism' and 'technical materialism') interrelate in the constitution of mature mathematico-experimental sciences. (Cf. LeCourt 1975, 41 ff.) I read Barthes and Levi-Strauss at that time as well. Up to that point I was a (Kuhn-trained) Kuhnian debunker of method.

²⁰ This eliminates claims for the efficacy of a method which rest on an arguable misconstrual of its own proclaimed resources and goals, for example, claims that Descartes' method facilitated the discovery of articles of *faith*, or that Popper's method was of use in the *discovery* of a fact or law.

translate them, on paper, for some 'outside' purpose. Research proceeds from within each field's proper, and evolving framework, and not in terms of glosses provided by putative single, transferable methods.²¹ So, in what follows here we are not concerned with further supporting the initial premise that method discourses, such as that of Descartes, must fail adequately to gloss or redescribe target fields; rather we explore the mechanisms which *simultaneously explain both the necessity of that failure and the creation of the illusion or literary effect that no such failure has in fact occurred*—termining such curious and important mechanisms 'mythopoeic' in a precise and considered sense of the term, inspired by Barthes: In short, Descartes' method discourse, like any grand method discourse, produced only literary effects of its own efficacy, applicability and unity, effects that tended to convince Descartes (and other believers) that the method actually possessed these virtues.

5.0 *The Failure of Adequate Redescription: An Example of Descartes Attempting to 'Methodologize' a Field of Inquiry*

Elsewhere I have looked closely at the distance separating, on the one hand, Descartes' actual path of discovery of the law of refraction and his subsequent struggle to find an adequate mechanistic rationale for it, and, on the other hand, his methodological account or redescription in rule 8 of the *Regulae* of how one might accomplish these ends.²² For the moment, however, it will be useful to start our analysis with a new and different example. Our case deals with what we may term for ease of expression 'the science of magnets', which Descartes discusses twice in the *Regulae*.²³ In fact what we are dealing with here is better described as one typical domain of explanation within the larger realm of corpuscular-mechanical natural philosophising as a whole. What we are going to find out about this one domain arguably holds across any and all regions of phenomena one would wish to cover with corpuscular-mechanical explanations. Although Descartes first discussed 'magnet science' in the *Regulae* at a time before he was committed to constructing a system of corpuscular-mechanical natural philosophy, we can still treat this case as involving in the end an attempt at such corpuscular-mechanical explanation. There are two reasons for this: Firstly, as in the case of the explanation of light in the *Regulae*, we know that although Descartes was no system builder in the 1620s, he nevertheless clearly preferred to ground exercises in his brand of physico-mathematics in piecemeal corpuscular-mechanical explanations. Secondly, later in the *Principles of Philosophy*, his second and definitive system of corpuscular-mechanism, magnetism took pride of place as an object of study and example of explanatory success.²⁴

So, let us now consider the fate of the 'science of magnets' when Descartes tries in the *Regulae* to explain how to 'do' this science according to his method. We need to compare Descartes' methodological tale, or redescription of magnet science, with what he in fact had to do to produce corpuscular-mechanical explanations of magnets and their phenomena within the living field of natural philosophising, where actual corpuscular-mechanical explanations had to be thought up and inscribed. Methodologically speaking, Descartes instructs us first to isolate a fixed set of experimental data about

²¹ One reservation must be registered to this claim. It is perfectly true that bits and pieces of 'methodological discourse', including putative glosses of the field in question, can be deployed in practice as resources in debate, negotiation and adjudication of the content and acceptability of knowledge claims. Below in Section 9, we shall identify these as the 'rhetorical' uses of method discourse in debates and negotiations about knowledge claims inside the living fabrics of disciplines and fields. Scientists can appeal to methodological principles to attempt to substantiate or undermine such claims. However, such deployments of method discourses within scientific debates are merely small portions of the total structure of action and belief through which knowledge is made and unmade. Such deployments do not represent clear and accurate meta-level versions of the specific practice of that field. Indeed, the deployment of method claims in scientific debate in no way whatsoever constitutes even prima facie evidence of the efficacy of that method. The issue must be turned on its head—how are such claims, as discursive phenomena, shaped by the resources of method discourses and what literary effects of genuine efficacy are thereby created? Classic sociological studies of methodological discourse in scientific debate and negotiation include: Gilbert and M. Mulkay (1980, 1981); Mulkay and Gilbert (1981, 1982).

²² Schuster (2013), 312, 215-221; See also Section 8 of Schuster, '*A Guide to Descartes' Optical Work*', prepared for the '*Conference on Experience and Reasoning in Scientific Methodology: Between Antiquity and the Early Modern Period*', Institute of Philosophy, Czech Academy of Sciences, Prague, May 2019. This should be available on the Institute website for my visit.

²³ *Regulae*, Rule 12, AT, X, p. 427; Rule 13, AT, X, pp. 430-1.

²⁴ *Principles of Philosophy*, Part IV arts 133-183. See Schuster (2013), Chapter 12, Section 5 on the key role of 'cosmic' magnetism in the systematizing strategy of the *Principles*; also Schuster (2017) and Schuster and Brody (2013).

magnets, in practice the experiments reported in Gilbert's *De magnete* (1600). We are then to inquire into the 'intermixture' of 'simple natures' which will explain the magnet.²⁵ Here the absolute natures or terms surely are primitive geometrico-mechanical elements, corpuscles, with their properties of size, shape, hardness and state of motion or rest. What, then, are the 'relatives' in this case? Descartes terms them 'intermixtures' of absolutes. That conveys the image of some set of complex corpuscular-mechanical models, models for the structure of lodestones, magnetic 'effluvia', magnetizable bodies etc.²⁶ Hence in this case Descartes' methodological gloss or redescription of 'magnet science' does depend in a loose sense upon the required specification of 'absolutes' and their studied 'complexification' into sets of 'relatives'. And these absolutes and relatives are arguably the sorts of entities which did eventually enter into his detailed explanation of magnetism, set forth later in his *Principia philosophiae* (1644). The mythologist of method, however, must ask an embarrassing question: 'Has Descartes provided here an adequate redescription or gloss of what it took to construct corpuscular-mechanical explanations of magnetism (or of anything else for that matter)?'

Modern Cartesian scholarship has given us answers to the question of what really was involved in Descartes' formulating and inscribing of corpuscular-mechanical explanations, and something approaching a consensus has existed in the literature for a considerable time:²⁷ In non-methodological contexts and later in his career, after 1628, Descartes increasingly came to see that although there are some absolutely certain metaphysical principles, for example, that the essence of matter is extension, neither the details of particular corpuscular-mechanical explanatory models, nor the facts to be explained, can be *deduced* in the strict sense from such absolutely certain metaphysical principles. *A fortiori* there is no question of the full details of the corpuscular-mechanical world system being fully deduced from such 'first principles'. Nevertheless, the absolutely certain metaphysical principles do place constraints upon what can and cannot be asserted of any detailed corpuscular model designed to explain a particular class of phenomena. For example, nothing should be asserted in a particular explanatory model that contradicts any of the metaphysical principles. Additionally, available empirical evidence, and in particular, the 'facts' to be explained, also need to be considered in the formulation of the detailed explanatory models. By the time he published the *Principles of Philosophy* in 1644 his position became very clear: We may know with certainty from metaphysical deduction that the essence of matter is extension, as well as certain laws of motion and collision, but we cannot deduce from these truths more detailed explanatory models for such diverse phenomena as gravity, light, magnetism, planetary motion, sensory perception and animal locomotion. The best one can say is that such models should not contradict metaphysically derived certainties and that relevant facts must also be considered in shaping explanatory models. Hence, such lower level models are necessarily hypothetical and can achieve at best only 'moral certainty'. When, in his later works, Descartes spoke of 'deducing' phenomena from his principles, he did not mean the strictly mathematical deduction envisioned in his central methodological texts, but rather 'deduction' in the looser contemporary acceptance of 'plausibly explain'.

Such, then, were Descartes' own later and more considered views about the production of corpuscular-mechanical models and explanations. Although they show that his strict methodological views bore little relation to the procedure, they do not quite do full justice to what we might now term the interpretational complexity and fluidity of his project and the indexical character of virtually every move within it. Imagine a sociologist or anthropologist of science transported back in time to observe Descartes as he attempted to produce and inscribe a piece of corpuscular-mechanical discourse, about magnets for example. Our temporal interloper would probably have identified three interacting moments in Descartes' performance. His field notes might read as follows:

(1) Logically and temporally prior to the construction of any particular explanation, Descartes tries to devise and legitimate his basic metaphysical principles which will constrain and condition the

²⁵ I leave aside the problem, obvious to anyone familiar with the post-Kuhnian sociology of scientific knowledge literature cited in Note 21, of the criteria by which Descartes selects as 'adequate and 'reliable' Gilbert's own selection of a set of experiments, their performance and their glossing in his book.

²⁶ On the complexity of interpreting Descartes' remarks see Buchdahl (1969) 85-8, 126-47 and Schuster (1980) 74-5, and notes 150, 151 thereto.

²⁷ Buchdahl (1969) 97, 118-26; Sabra (1967) 21-45; Clarke (1977) and (2006) 154, 161-68; Schuster (2000a, 2017).

*formulation of specific corpuscular-mechanical models. Such principles include his fundamental definitions of matter and mind, and his basic laws of motion, collision and the behaviour of directional tendencies to motion.*²⁸ Needless to say, the production of Cartesian conclusions in metaphysics and dynamics, and their legitimation, are not amenable to clear, consistent, rule-bound procedural glossing. One might parody M. Descartes' own account of his procedures and say that 'God only knows how he does it'. Nor is it clear how and in precisely what sense the models should be 'constrained' in any given case. That, too, can only be a matter of on the spot interpretation and 'negotiation', if only with himself!

(2) 'Relevant' empirical evidence has to be selected, weighed and 'appropriately' deployed and described. Evidence can include 'facts' needing explanation, or 'facts' lending credibility to the explanatory model offered (including 'facts' purporting to weaken the credibility of competing explanations). It is not clear that M. Descartes has procedures for accomplishing these tasks which are any more rule-bound than those ongoing negotiations and 'constructions' of facts and arguments revealingly studied from the later twentieth century by post-Kuhnians such as Latour, Collins, Pickering, Pinch or Shapin.²⁹

(3) In the light of the 'evidence' and the metaphysical 'constraints' a specific corpuscular-mechanical model for the phenomena in question has to be constructed. Given the un-methodological character of the proceedings under (1) and (2), it is not to be expected that M. Descartes' inscribing of characterizations of particular models is a method-bound activity. Consider, additionally, that in Descartes' usages the meaning of 'deduce' in the phrase 'deduce the phenomena from the model' is fluid and reinterpretible. As if this were not enough, there is also the point that each specific model has ultimately to 'fit' into a 'system' of natural philosophy. This raises a host of additional interpretive challenges which reflect back upon the way in which a model is to be constructed. For example, Descartes, the 'systematizer' always asks, 'To what degree does a particular model 'comport' (itself a fraught word) with other specific models within the system in respect of (a) consistency of mode of metaphysical constraint,³⁰ (b) similarity (or difference) of explananda in view,³¹ and (c) the degree of structural, cosmological 'interplay' intended to hold between these models in the overall system of the world machine.³²

Not to put too fine a point on it, in the field notes of our sociologist or anthropologist, (1), (2) and (3) together constitute a complex undertaking—it's not easy (or method-bound) to think up and write down corpuscular-mechanical explanations of things, let alone under seventeenth century conditions of pursuit of 'systematic' completeness. Each of the three steps involves discursive practices, interpretations, weightings and selections for which no rules were ever given, and from which it is not plausible to imagine any workable and consistent full set of rules could ever be elicited by *post facto* glossing.³³ *A fortiori* these moments elude Descartes' own recommendation in effect to 'find the

²⁸ The principles and laws of Descartes' dynamics are included here, not because there is scholarly agreement that Descartes intended all of them to be deducible from his metaphysics, but rather because they are foundational for all his detailed model building and particular explanations, and because he often gives strong indications that they were meant to be deducible from first principles.

²⁹ Latour and Woolgar (1985); Pinch (1985); Shapin (1982).

³⁰ How, for example, can the behaviour of Descartes' 'first element' comport with the metaphysical principle that extension is the essence of matter, since it seems able to change density instantaneously, as it instantaneously changes shape to fully 'fill' interstitial 'spaces'; or, how does Descartes' account of the internal sensations and passions of the soul comport with his ontological dualism? (Descartes' seems not to have attended to the former but devoted much effort to the latter.)

³¹ Descartes' judgments about these similarity/difference relations would at least in part depend upon already established patterns of interpretation. For example, Descartes thoroughly accepts the broad difference Gilbert had sought to draw between magnetic and electrical phenomena, although he does not accept this as an ontological distinction (between a spiritual and a corporeal cause respectively), and yet *à la* Gilbert he still promotes magnetism as a phenomenon of 'cosmic' scope and importance, contrasting it to the rather trivial role of the known electrical phenomena. (Obviously, our anthropologist of method has taken a position reminiscent of Barry Barnes in these remarks, Barnes (1982), especially chapter 2.)

³² On the logic and tensions/pitfalls of system construction in natural philosophy in the case of Descartes, See Schuster (2017, 2013, 527-40, 576-586); Schuster and Brody (2013).

³³ Cf. Garfinkel (1967); Barnes and Law (1976).

absolutes and the structure of relatives and use my heuristic rules in so doing'. Descartes' methodological discourse on how to 'do' magnet science bears no significant relation to whatever it is he must have been 'doing' in order to accomplish magnet science; that is, to think up and inscribe his typical corpuscular-mechanical texts on the subject. The recommendations embedded in his method-talk could never have led to the construction of this corpuscular-mechanical discourse about magnets. Here, the methodological recommendations which he gives in the *Regulae* simply batten upon the prior accomplishment of (1), (2) and (3), and upon the uncodified and arguably uncodifiable body of discursive practices which underpin them at every step. The methodological version of what it is to do magnet science drains the practice of magnet science of its actual *sui generis* procedural density (whatever that might have been); and, having drained that density, the method discourse poses as the real basis of Descartes' practice.

Nobody should really be surprised by this. Very few serious post-Kuhnian students of the history of scientific practice would expect that Cartesian method discourse (or any systematic method discourse) could adequately gloss the content and practice of a field of research.³⁴ And yet, an important fact presents itself at this point. Descartes' methodological story about doing magnet science has a curious property: *Certainly it fails adequately to redescribe its target; but, inadequate as the story is, the rules of the method do seem to apply to it and mesh within it.* Consider that in the *Regulae* Descartes discusses the problem of explaining magnetism, and his discourse proceeds by using the terms 'absolute', 'relatives', 'relations' and 'series' ('intermixture'). So, as far as 'just discoursing' goes, one can gear the rules of the method right in, so as to facilitate the investigation; that is, to facilitate the continuation of our little methodological story about how we really should 'do' magnet science. Descartes' redescription of magnet science, his little account of what we should do, is just the sort of text into which additional talk about the rules and their use can be inserted 'convincingly' and 'coherently'.

What then should we make of the fact that the rules of the method can be applied to and deployed within Descartes' account of doing magnet science? The answer depends upon whether we are believers in the method, or are post-Kuhnian mythologists trying to demystify it. Believer and mythologist can agree that the rules 'go together' with the methodological account. But, the mythologist sees that the redescription is a phantom, a parody of what must have gone on when Descartes 'did' corpuscular-mechanical natural philosophy. The situation of the believer, however, is very different, especially if the believer is an early modern methodologist, like Descartes, who did not have the benefit of *our* contemporary critical tools through which to articulate what we mean by saying that the dynamics of the inscribing corpuscular-mechanical explanations were very complex, and quite probably ultimately elusive. Our early modern methodologist (or our surviving contemporary methodologists of various ilks) might have easily overlooked the slide between, on the one hand, the practices involved in thinking up and writing down corpuscular-mechanical explanations, and, on the other hand, the methodological glossing of those practices. Or, better expressed, he is unlikely to have overlooked it, because, he probably had no discursive tools through which to thematize it in the first place—except for the resources of a *methodological* discourse of one sort or another, a type of discourse, which enjoyed cultural prestige and was accorded epistemological precedence in attempts to 'account for' intellectual practices. Now, if you overlook the structural slide; or if it is just unthematizable for you, then you are bound to be impressed by Descartes' method story, and in particular, you are bound to be impressed by the wonderful way the rules of his method apply beautifully within the story.

³⁴ 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 data, 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. Moreover, on hitherto little noticed problems, and successes, of this research, see Buchwald (2008), whose brilliant reconstructions further undermine the idea that Cartesian method actually controlled Descartes' course of work in this area.

Any believer who reaches such a level of conviction would then be in a position to engage in a pair of characteristic behaviors, quite reasonable from his viewpoint, which reflect his belief in method and would serve to reinforce it. Having constructed the method story about magnet science, or having consumed it from an 'authoritative' source, the believer could then do the following:

(1) He could practice corpuscular-mechanical natural philosophy—thinking up and writing down explanations—more or less proceeding in the way discovered by our time traveling anthropologist, while monitoring his actions to himself and/or to others in terms of the little method story. This would lend credence to the method by seemingly attaching it to segments of the practice as they occur in the flow described by the anthropologist. Such 'voicing over' or real time monitoring of practice could be institutionalized in pedagogy to entrench both the method in question and the theories embedded in the routines so glossed.³⁵

(2) He could practice corpuscular-mechanical natural philosophy, and then after the fact claim in good faith that, although what he had done might not fully reflect the dictates of the method, he could in principle have accomplished the same things by strictly adhering to it.³⁶

Both (1) and (2) are very easy to accomplish. The method story which the methodologist recites while working, or after the fact (and 'in principle' or 'in fact'), is simple to construct, because it mainly utilizes the core of the method discourse, its generalized statement of rules; and, it is easy to accept, because the tame methodologist (and his tame audiences), almost by definition, either have no alternative critical frameworks available for theorizing about practice, or, if they have them to hand, they are for one reason or another unwilling to deploy them.

6.0 *The Structural Levels and Underlying Metaphors in Descartes' [or Anybody's] Method Discourse*

By reflecting on the example of Descartes' 'magnet science', we are about to uncover the basic structural units of his method discourse, indeed the basic structural units of any systematic method discourse. Knowledge of the structural levels in a method discourse, such as Descartes', will, in turn, allow us to locate and explain the four characteristic illusions or textual effects which method doctrines exercise upon those who believe in them.

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. Typically, a new doctrine is fabricated out of bits of older method

³⁵ Feyerabend (1970) effectively first identified this phenomenon and isolated its mythopoeic character through what amounted to a structural delineation of Newton's methodological discourse. I introduced the term 'voicing over' in this context in Schuster (1984) 21ff in articulating Feyerabend's argument.

³⁶ If this move were accompanied by the claim that the results of research are justified only if such a *post facto* gloss is possible, then we would have the typical modern methodologist's tactic of appealing to the possibility of 'rational reconstruction'. On the view to be advanced here, 'rational reconstructions' guided by systematic methodologies are simply a species of methodological story or account, and share in their mythopoeic character. Existing studies which suggest this criticism of Popperian-Lakatosian conceptions of method include: Feyerabend (1978) 201-2; Schuster (1984) especially Section VIII; and Mulkay, Potter and Yearley (1983) 172-82 where the authors produce a brilliant ethnomethodological critique of a quasi-Lakatosian attempt to offer an historical 'rational reconstruction' of some developments in particle physics). Reasons for the mythic character of Lakatosian reconstructions are also discussed in Schuster (1979) 301-17. As for Descartes' own *post facto* glossing of practice, we have already studied the wonderful example in the *Regulae* (Rule 8, AT, X, pp. 393-5) where he discusses (in the carefully chosen subjunctive mood) how one *might have discovered* the law of refraction of light by following the method. See Schuster (2013), 312, 215-221; See also Section 8 of Schuster, '*A Guide to Descartes' Optical Work*', prepared for the '*Conference on Experience and Reasoning in Scientific Methodology: Between Antiquity and the Early Modern Period*', Institute of Philosophy, Czech Academy of Sciences, Prague, May 2019. This should be available on the Institute website for my visit.

doctrines, as well as pieces of neighboring varieties of discourse—theological, natural philosophical, ethical, mathematical, psychological, and so on.

Let us now turn to the generic structure of method discourses (Figure 3). Reflection upon the discussion of ‘magnet science’ in the previous section shows that we are dealing with three levels of discourse: The systematic ‘core’ of the method discourse, the *sui generis* target fields, and the methodological accounts, glosses, redescrptions and stories which can be manufactured about the latter, using the resources of the former. Figure 3 is a map of these levels and it will also become a guide to the places where the characteristic illusions or literary effects of Descartes' (or anyone else's) method discourse are generated, between and across levels.

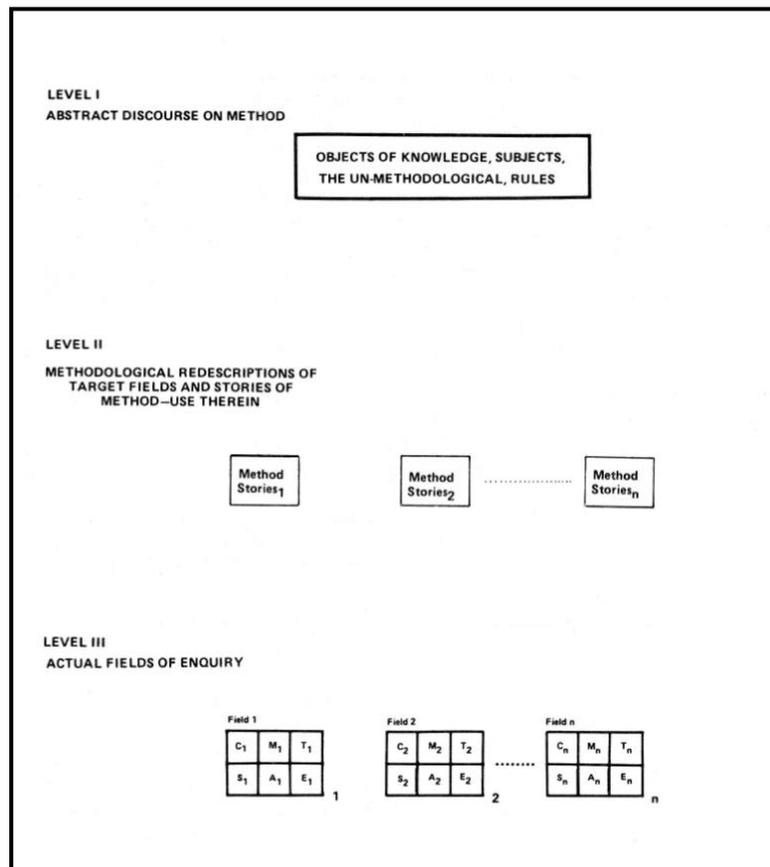


Figure 3. The Structural Levels In Any Grand Method Doctrine of Method

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. In some grand method doctrines Level I is itself packaged within a metaphysical or even theological framework. In Descartes' method we have already discussed this Level I core, as presented in the *Discours* and *Regulae* in Part I of this Guide. It includes his teaching concerning intuition and deduction, the conception of the latticework with its intertwined concepts of absolutes, relatives, series and relations, the statement of the rules of the method, and in addition a discourse on pitfalls and sources of error, which I have elsewhere (2013, Chapter 5, Section 6) termed his 'negative heuristic advice'.³⁷

³⁷ For example, trust not in a authority, nor in unclear, indistinct belief, will or emotion; avoid precipitation and hasty judgment; go only as far as intuition and deduction reveal the truth. Descartes also has, as many methodologists do, a catchall 'saving clause' to the effect (using our terminology) that somebody might indeed point to gaps between methodological stories of practice and what it really is like to pursue the discipline in question. Systematic methodologists, being reasonable

Level III has been met previously. It is a representation of the *sui generis* character of scientific fields, viewed in post-Kuhnian perspective, which was presented above in Section 2.0 and Figure 2, building on Figure 1. What we have here again is the domain of living fields, depicted as a set of matrices, each one schematically representing the existence of a discrete, efficacious field of inquiry. Inside each matrix there are six spaces symbolizing what one might take to be the contents and structure of such a field—its own basic concepts [C]; metaphysics [M]; tools and instruments [T]; standards of relevance and adequacy [S]; goals and aims [A] and exemplars [E]. The aim, as in Section 2 above, is to represent the neo-Kuhnian thesis that the ‘method(s)’ of any such field are *sui generis*, for they are inextricably bound up with the contents and structure of the relevant disciplinary matrix. The point of thus placing Figure 2 here in Figure 3 as Level III of a method doctrine is to show that it is the set of such living fields that are the ‘targets for redescription’ in this method. So, if we were to take Figure 3 to be depicting Descartes’ method, ‘magnet science’ would be one of these target fields on Level III, so would optics as we have seen him presented it in rule 8 of the *Regulae*,³⁸ and we shall soon meet another, analytical mathematics, which Descartes believed could be commanded by and through his method. Indeed, 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.

Finally, *Level II* consists of a set of ‘methodological versions’ of the corresponding fields of inquiry represented on Level III. Here one finds methodological accounts, redescrptions 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 redescrbed 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 ‘magnet science’. Any such Level II story or account is couched in terms of the core methodological terms and rules available on Level I of Descartes’ discourse on method.

7.0 The First Two Structural ‘Effects’: Adequate Redescription’ and ‘Application’

If we return to the case of magnet science, bearing in mind the structure in Figure 3, we can begin to locate the sites at which a method discourse generates illusions or creates literary effects concerning its own efficacy, and we can specify its *modus operandi* at those sites. Thus, we can examine how the structure of a method discourse contributes to its dynamics as a mode of mythic speech; that is, we can explain how method discourses, Descartes’ included, succeed in creating literary effects of their own efficacy whilst in fact being structurally incapable of doing what they literally claim to be able to do.

The key to the mythological operation of Descartes’ (or anybody’s) method discourse resides in getting the audience, potential believers, to operate on Level II, where they bask in the methodological version(s) of the target field(s) with which they are concerned. On Level II the rules of the method do ‘apply to the redescrptions offered’; ongoing work on Level III can be glossed (or ‘voiced over’) in terms of Level II, or *post facto* ‘accounting’ for practice on Level III can be offered in terms of Level II stories. It is crucial that the Level II story both embodies the rules of the method and disastrously

people, do notice gaps between stories and counter-accounts, when these are brought to their attention. But this does not induce them to become mythologists of method. Consider Descartes, writing to Mersenne (27 February 1637) ‘...je n’ai su bien entendre ce que vous objectez touchant le titre; car je ne mets pas *Traité de la Méthode*, mais *Discours de la Méthode*, ce qui est le même que *Préface* ou *Avis touchant le Méthode*, pour montrer que je n’ai pas dessein de l’enseigner, mais seulement d’en parler. Car comme on peut vois de ce que j’en dis, elle consiste plus en pratique qu’en théorie... (Alquie, 1963, t.1, 521-2)

³⁸ See references in Notes 22, 36.

misses all the cognitive and organizational density of actual Level III practice. Indeed these two characteristics of Level II stories may be seen as interacting in the very constitution of such stories: Level II stories eviscerate actual Level III practice because, failing to engage the density of that practice at all, they are episodic, fabular versions of the core methodological terms available on Level I. They are fairy tales of methodological comportment spun out of materials and scenarios available on Level I. But, of course, the rules of the method only mesh into such stories because the stories derive from Level I.

Recall our case study of Descartes' practices for formulating corpuscular-mechanical explanations of magnets and his methodological tale in the *Regulae* purportedly corresponding to that activity. According to our new terminology, there is a Level III field of natural philosophical practice and a corresponding Level II methodological account of this target domain. Descartes' methodological tale about 'magnet science' eviscerates and suppresses the specific content and dynamics of his practice in corpuscular-mechanical explanation of magnets, 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' inscription of his method tale is dependent upon those two processes: (1) the suppression of the real content of 'magnet science'; and (2) the fabular rendition of the core discourse of Level I 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 the early modern culture of method most probably did not, for they, *ex hypothesi*, had virtually no discursive resources for explicating and accounting for successful practice in a discipline other than those offered by some method discourse or other—either a version of the neo-Scholastic discussions of method acquired at university, or some alternative of their own or other's manufacture. 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.³⁹ 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.

One may therefore state that 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 illusions or literary effects characteristic of systematic method discourses. (Figure 4) 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 (target field) 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 possible and sustained by the relationships amongst the three levels of discourse. That structural arrangement also simultaneously explains 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 any accurate and specific understanding of the structure and dynamics 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 redescription or account, is then solidified and underwritten by its 'obvious' congruence with the grandiose, self-proclaimedly authoritative core discourse on Level I. Since the very process of manufacturing, solidifying and underwriting phantoms on Level II is the source of the effects, we may well reassert my central thesis: *Descartes' method discourse is a species of mythic speech whose discursive structure renders it vacuous whilst simultaneously sustaining powerful illusions that it is not.*

³⁹ Even in his activities on Level III, Cf above Note 21 and see below Section 9 on the rhetorical functions of Cartesian method.

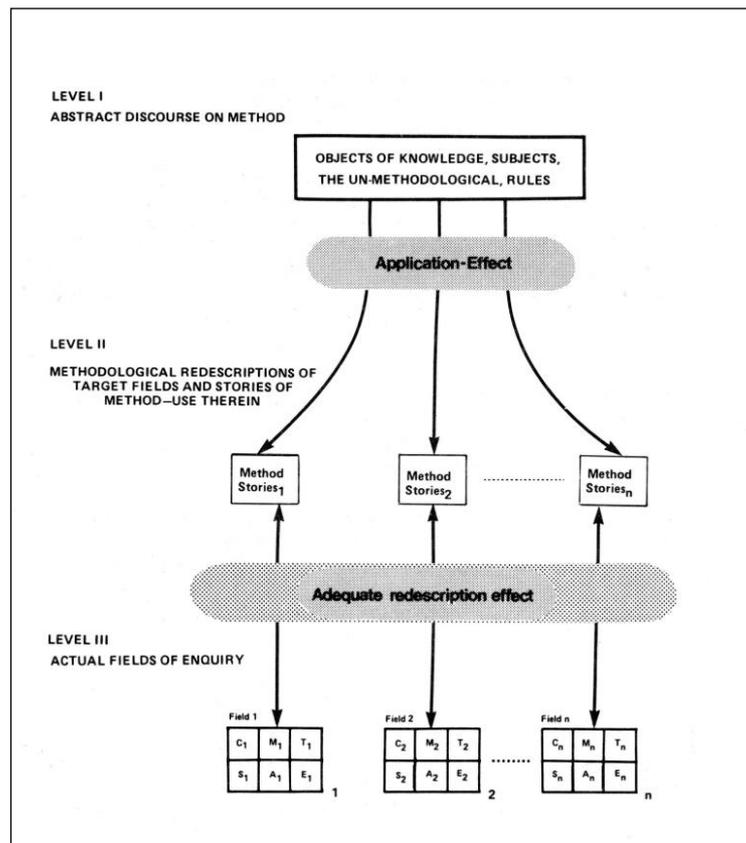


Figure 4 Location of the First Two Structural Effects of a Grand Method Discourse

It may well be asked how a reader/listener ever gets to Level II in the first place; what types of reader/listeners are at risk of falling victim to these effects; and what exactly are the dynamics of their acquiescent reading/listener? These are crucial problems. 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 subordinate sciences was largely unquestioned (except by some sceptics). The task was to devise and enforce *the* 'correct' method. From the historian's standpoint, analytically speaking, there is the historical problem of explaining the election 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.⁴⁰ 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 inquiries taken together.⁴¹

⁴⁰ In some cases the second task is involved in the first. For example, in cases where actors were arguably indoctrinated into a method, part of the account of the indoctrination process has to do with the mythological mechanics of method discourses. Early modern figures, such as Descartes, present a more complex picture. As I just said, their intellectual culture widely accepted the in principle existence of efficacious methods of discovery and proof. The challenge was to devise, select and enforce the 'correct' one.

⁴¹ It must be stressed that two broad lines of inquiry are involved: 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. So, firstly, one can ask in general how stories or accounts of putative matters of fact come to be heard as true even prior to, and possibly completely independently of any attempt at further inquiry. I suggest that ethnomethodological investigations of the strategies used in making sense of everyday accounts and narratives are relevant here. [See Schuster (1984) especially Section V. In this connection I particularly rely on the researches of Harvey Sacks (1972); read against the background of Schutz (1970) and Schutz and Luckman (1974).] Secondly, the general theory of construing of accounts needs to be supplemented in any given historical case by an inquiry into the conditions and determinations which turned the historical actor in question into a particular sort of reader/listener. A believer in a method is someone who has chosen or been led to choose (in some wide

8.0 The Third and Fourth 'Effects': the 'Unity' and 'Progress' of a Method Discourse

Thus far the analysis has concerned the relationship between any given target field on Level III and its phantom redescription on Level II. The first two textual effects are produced by vertical relations holding amongst the three levels of a method discourse. A third and fourth textual effect are created horizontally, across Level II of a method discourse. This is due to the fact that a method discourse, such as Descartes', can generate across Level II a range of redescrptions, each one corresponding to a different target field. (Figure 5) A general method, after all, has to be able to command more than one area of research. In any given systematic method discourse, each and every Level II redescription will be couched in terms of Level I elements. In the case of Descartes these involve reference to 'absolutes', 'relatives', 'series' etc. Hence all the Level II redescrptions in a given method discourse will appear to be 'similar', although we shall see that serious equivocations are introduced as Level I elements are deployed in several Level II redescrptions. In addition, any Level II story then produced about the use of the method will involve an account of the application of the rules of the method to the redescrbed field. Unsurprisingly, the believer in Descartes' or some other method will find that the rules of his favorite method gear into each and every redescrption offered across its Level II. With that realization, the application effect gives birth to what I term the 'unity effect'—the illusion that the rules of the method are efficaciously applicable to some set of discrete fields of inquiry.

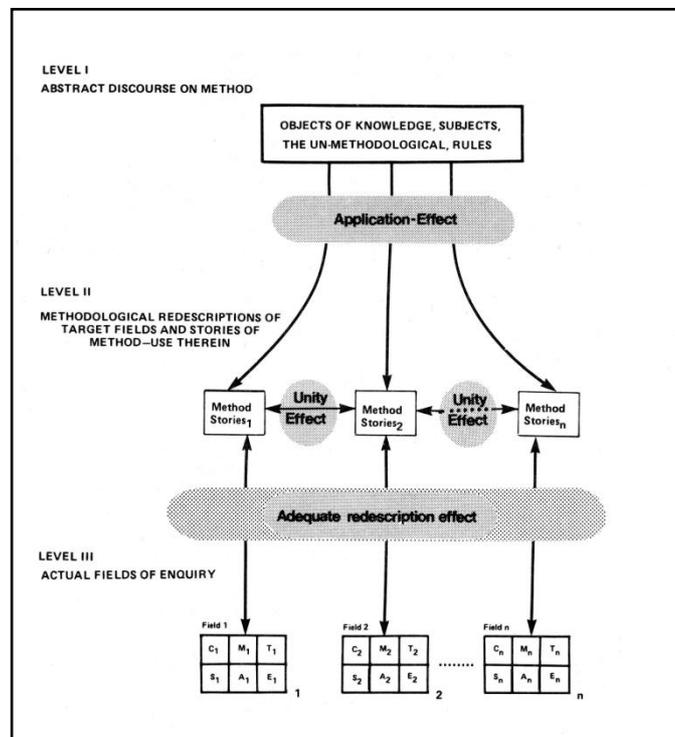


Figure 5. Adding the Unity Effect to the Application and Adequate Redescrption Effects

In the *Regulae* Descartes provides splendid illustrations of these points. He not only offers his methodological version of magnet science, but also his methodological version of mathematical analysis. He writes that in this case the 'absolute' is the straight line, the coordinate, the 'relative' is a curve.⁴² Presumably we are to refer a curve to straight lines by means of equations expressing the nature and degree of its 'relativity' to the coordinates. These clear and distinct algebraic representations of curves then become the vehicles for further inquiry into the properties and relations of curves. The entire discipline is pursued under the aegis of an emerging algebraic theory of equations. Before exploring the unity effect by comparing Level II magnet science with Level II mathematical analysis, we must first consider how this *Regulae* version of mathematical analysis fares

social-historical senses of the term) to have no other resources for explicating scientific practice than those offered by the method of his 'choice'. We must try to supply an account of the historical context and biographical trajectory which made that method available to him and 'preferred' by him.

⁴² AT, X, 381 1.25, 382 1.9.

in relation to the adequate redescription effect (and hence in relation to the application effect). This will further illustrate points made above in Section 7.

Recall Descartes' heuristic rules, such as 'note always the absolute in question and the order of relations binding the relatives to it'; or, 'break down questions into simpler parts and resolve them in due order proceeding from the simpler to the more complex'. These rules not only *seem* to be transcriptions of maxims used in resolving equations, they were indeed elicited from parts of Descartes' mathematical practice. As we have seen, the concepts of the 'lattice-work', of 'absolute' and 'relative' terms, of 'relations' and 'series', were all derived by analogy from the elements of the sort of little mathematical problems informing 'universal mathematics'; that is, from problems about quantities in continued geometrical proportions, such as the one at the end of rule 6, or the ones which so fascinated Descartes in 1619.⁴³ Descartes' heuristic rules of method were likewise constituted as methodological analogues of maxims which were useful and readily at hand in dealing with such little problems. So, mythologists of method can agree with believers in Descartes' method: (1) that the heuristic rules of the method do give a *post facto* summary of *some* aspects of the domain of mathematical analysis as Descartes knew it; and, (2) that Descartes' entire Level I discourse is at least minimally adequate and applicable to the small domain of mathematical problems which formed the basis of 'universal mathematics', because the Level I discourse and the rules are simply further analogical developments of that base.

However, any believer in Descartes' method would surely wish to go much further than this. Echoing Descartes' text, he would issue a stronger claim: *If* coordinates are 'absolutes' and curves are 'relatives' describable by equations which can be solved using the rules of the method, *then* the method adequately constitutes in principle the entire field of geometrical analysis, by, of course, rendering it part of the domain of application of the method. Descartes would have us believe that if one were to set up such a version of geometrical analysis, he could then use the rules of the method to expand the field further (and to account for its previous achievements as well). This, in historical and mathematical point of fact, is not the case; and, as our structural analysis shows, cannot be the case.

The heuristic rules of Descartes' method were simply too vague and too limited to be of use in the further expansion of the domain of geometrical analysis. The heuristic rules did not, and could not help Descartes or anyone else move beyond the resolution of just the sorts of trivial problems which formed the basis of 'universal mathematics' and of Level I of the method discourse. Nor can they explain the prior development of the field in any way other than through a tendentious and implausible Level II account, a methodologist's 'rational reconstruction', of the sort Imre Lakatos and his followers used to advocate.⁴⁴ Consider, for example, that in Book Three of the *Géométrie* (1637) we have Descartes' own mature and considered statement of the procedures and techniques of contemporary algebra and theory of equations. He summarized, rationalized and extended the rapidly growing early seventeenth century 'tool kit' of algebraic techniques. He showed how to evaluate the number and signs of the roots of an equation; discussed real and imaginary roots of an equation; showed how to alter roots by common factors; how to reduce varieties of equations to canonical forms etc.⁴⁵ It is obvious that the heuristic rules of the method are mildly reflective of a small amount of this material. But in themselves they cannot account for the full richness of the theory of equations or its development to that contemporary point of perfection. Algebra, which was increasingly seen as a general theory of equations, was developing within a largely *sui generis* and rapidly changing realm of properly algebraic conception, expression and technique (it was a Level III discourse and practice).⁴⁶ The heuristic rules redundantly echo some few of the findings. However, they furnish no significant insight into how the algebraic art had developed, nor how it could be further extended.⁴⁷

⁴³ See Schuster (2013) Chapter 5 Section 7, or Part I of this Guide, above, p.8.

⁴⁴ Schuster (1979)

⁴⁵ *Géométrie*, Liv. III, AT, VI, pp. 444-61

⁴⁶ Mahoney (1973, 1980).

⁴⁷ Consider a parallel case, Descartes' discovery of a general constructive solution for all cubic and quartic equations, presented in the third book of the *Géométrie* as the solution of all the 'solid' problems of the ancients. I have reconstructed Descartes' path to this construction (Schuster, 1977, 131-49). showing how this achievement grew out of Descartes' early

In short, Descartes did not formulate an adequate Level II version of mathematical analysis. This was despite the fact that his Level I rules were indeed applicable to a small portion of that domain, and the fact that his entire Level I discourse was an analogical transcription of that small portion. So, although Descartes' method-discourse was based upon a sub-domain of mathematical practice, this did not prevent his Level II version of mathematical analysis from suffering from the adequate redescription and application effects, and it has led to much misunderstanding of the real scope and efficacy of his method.⁴⁸

Now we can compare Descartes' Level II version of mathematical analysis to his Level II version of magnet science. Everyone immediately sees that the rules of the method 'apply' to both Level II stories, and that the stories have a certain resemblance: They are both about the 'relations' of 'absolutes' to 'relatives' etc. Mythologists realize, however, that this resemblance is superficial and misleading. It is based on deeply worrying equivocations about the terms 'absolute', 'relatives' and 'series'. The words are the same, but they are badly abused. Depending upon how one uses the term 'absolute', as coordinate or corpuscle, we then suitably frame our 'relatives' as algebraic equations or corpuscular-mechanical models. But coordinates bear to curves or equations vastly different 'relations' than basic corpuscles bear to physical conjunctions thereof.⁴⁹ *Within* each story there is some unity of expression about absolutes, relatives and relations, whilst across versions there is nothing but an almost comical equivocation.⁵⁰

How can it possibly be that a believer does not see these equivocations? How is it that he falls for the unity effect? In Section 6 it was suggested that a believer in a method tends not to see, or at least not to thematize, the slippage between Level III and Level II, what was termed the evisceration of Level III in favor of its Level II redescription. Once located on Level II, and lacking any motive or machinery for removing himself from that comfortable perch, the believer may be confronted with two (or more) Level II redescriptions, corresponding respectively to two (or more) target fields. He is now likely to be impressed by the observation that these Level II versions are 'rather similar' if not identical. To the extent that he can think of the target fields in independent, Level III, terms, he is likely to say that the methodological versions of the fields render them 'more alike' than the non-methodological accounts (which accounts may be further stigmatized for their lack of 'methodological articulation').

compass researches, recounted in part in Schuster (2013) Chapter 5 Section 4, and how the analytical skills involved elude pat generalizations in terms of the simple heuristic rules of the method.

⁴⁸ Let us now consider how this line of analysis affects our understandings of [1] Descartes' posture toward his own method, and [2] the relevant traditions of Descartes scholarship on these issues:

[1] In 1619-20 Descartes might well have believed his method did in fact or could in principle subsume all of the analytical mathematics he knew. The point is that his own work (for example, in the mid 1620s on 'all the solid problems of the ancients'—cubic and quartic equations—or, in the early 1630s, the problem of Pappus which focused the research issuing in the *Géométrie* itself) and that of others would eventually be seen to outstrip the method-talk of 1619-20, elaborated as it had been from tiny bits of analytical practice. These points concern his likely belief in the relevance of his method to his high level mathematical practices.

[2] Considering these issues more generally, I suggest that this line of analysis offers closure to several generations work of heated and contentious speculation amongst Cartesian scholars concerning 'the relation' between his method and his mathematics, particularly his mature analytical geometry and theory of equations.

⁴⁹ The only way out of this problem would be to maintain with Cassirer (and some of his latter day acolytes) that Descartes' (discursive) corpuscularianism was some sort of mistake or illusion, and that Descartes did (or should) have entertained an 'ontology' for physics of coordinates and functions of physical quantities thereof. This, however, would hardly establish the efficacy of Descartes' method. If true it would only show that mathematics provided genuine tools for the pursuit of Descartes' physics. In any case, after 1628 the historical Descartes resolutely stuck to grinding out qualitative corpuscular-mechanical discourse. See Cassirer (1902), Einleitung, 'Descartes' Kritik der mathematischen und naturwissenschaftlichen Erkenntnis'.

⁵⁰ This may well help explain Gerd Buchdahl's acute observation (1969, 126-7) that Descartes seems to use terms such as 'analysis' and 'absolutes' in several contrasting ways; for example, the latter may be (a) elements to which explicanda are to be reduced; (b) elements into which explicanda are decomposed; (c) elements to which explicanda are referred; (d) elements from which they can be physically generated, or (e) logically derived. The cause of this proliferation is not so much a philosophical profundity struggling with inadequate terminology, or underdeveloped conceptual distinctions. Rather, it is the structural product of the way the method discourse devours and denatures domain-specific discourses in the interests of (a pseudo) generality.

Now, it is worth noting that even for mythologists there is some truth in this. The Level II versions are indeed more similar than any serious accounts which might be given of their Level III matrices.⁵¹ Level II accounts do not lie about each other in quite the way they each lie about their respective Level III targets. The Level II accounts are honest (that is both leading and misleading) analogues of each other. They are of the same discursive fabric, Level I, and there are legitimate things to be said about their analogies. All this, of course, greatly aids the unity effect, which in itself is no better, but certainly no worse than any other categorical grouping of instances by 'similarities'.⁵² It is the structural location of this effect which makes it an accomplice of the really delusional two prior effects.

Returning to the believer, we find that he is much impressed by the discoveries that the Level II versions have a wondrous similarity and that the rules of his method gear into each and every one of these accounts. The method truly is 'unitary': For him it reveals the underlying similarity of the fields in its domain, and it empowers him, *via* its widely applicable rules, to command and pursue efficaciously any and all of these fields. To the extent, if any, that such a believer becomes aware of any contrasts or tensions between any Level II version and its independently characterizable target field, he can mobilize a discourse on the 'practical problems of applying the method'; for, as Descartes and other methodologists are wont to say, 'the method consists more in practice than in abstract statement after all'.⁵³

As in the case of the first two structural effects, the reason for this third effect is the obverse of the reason why the method cannot actually work in the ways it claims to work. There is a similarity between Level II versions and a gearing of the rules of the method to them *because* those versions are woven out of the cloth of the Level I discourse; but, for that very reason these Level II versions 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 necessarily equivocate amongst themselves.

Finally, for much the same reasons, there is even a fourth literary effect of method, which I term the 'progress effect'. Reading across Level II of Figure 5 over time, 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, such as I have discovered the young Descartes doing in 1619-20.⁵⁴

In general, then, the literary effects of Cartesian (or any other) methodology relate to each other in this manner: In any systematic method discourse the adequate redescription effect is fundamental, and it ultimately depends upon the plausibility of Level II stories within a context of cultural precedence accorded to the Level I discourse as the way of thematizing practices in the disciplines in question. The application effect depends upon the adequate redescription effect, for it fosters the illusion that the application of the rules within stories 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 inevitable and dangerous equivocations. The progress effect is the unity effect experienced or accounted over time as more Level II stories are added to the collection, or, as Level I rules are explicated, new ones added and 'problems' of the discourse identified and 'resolved'.⁵⁵

⁵¹ 'Serious' here denotes the accounts given by 'properly trained' historians and sociologists of science, of course!

⁵² The allusion here of course is again (cf Note 31) to Barry Barnes, explicating Kuhn, on 'finitism' in regard to the acquisition and ongoing negotiation of meaning and reference of concepts. Barnes (1982), chapter 2.

⁵³ Cf Note 37 above.

⁵⁴ See Schuster (2013), Chapter 5 Section 6.

⁵⁵ In Section 4 Note 19 it was mentioned that my approach to methodology as mythic speech has been heavily influenced by Feyerabend's dissection of the mechanics of Newton's methodological claims (Feyerabend, 1970). We can now examine Feyerabend's argument in an attempt to bring out the implicitly structural character of his analysis, and thereby also to show

One can conclude that any believer seriously engaged in the business of grand prescriptive general methodology will probably stumble into this hall of discursive effects. The believer will then happily expatiate on the unity, applicability, efficacy and progress of his method, or that of his master; 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 at all; 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 understand the trajectory of Descartes in natural philosophy and its subordinate disciplines, 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. Accordingly we have one further item on our agenda before concluding the present study. We must, in the next section, examine more closely the ways the young Descartes himself deployed method stories rhetorically, that is, as tools of self or public persuasion within the Level III practice of given disciplines. Then, ideally, it would be time to consider the issue of whether Descartes ever stopped believing in his method as a guide to his intellectual agenda, and source of his intellectual identity, choosing thereafter to maintain a rather cynical public posture as to its efficacy, and its role in his agenda and identity—a matter we have already foreshadowed in Part 1, Sections 5 and 6, albeit without the benefit of our demystification of method-talk accomplished here in Part 2.⁵⁶

9.0 *The Rhetorical Functions of Cartesian (and Other) Method Discourses*

We have seen that whilst the sceptical historiographies of Koyré and Kuhn effectively debunked method as having no role in the dynamics of the sciences, our proposed discursive model of grand method doctrines entails that methodologies can play some roles in the formation and negotiation of knowledge claims in scientific disciplines, 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.

The work of Feyerabend, discussed above (Note 55) was extended in the literature within the history and sociology of science that capitalized 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 work suggested that method discourses are often deployed as rhetorical

how his analysis can be mapped onto my general schema: Feyerabend is concerned with the ways in which Newton's proclaimed empiricist method enabled him to package and defend his theory of physical optics. In Feyerabend's version of Newton's empiricism, the essential methodological rule is: 'The sole basis for the derivation (induction) of theory is "experience"'. (p.150) 'Experience' is the stable, objective, repeatable, public foundation from which theories may be rigorously induced (hence this is part of Newton's Level I). Experience, moreover, requires no interpretation or justification. A methodological corollary, expressed by Newton in versions of his 'fourth rule of philosophising', states that theories derived from experience are not to be challenged on the basis of 'speculative alternative hypotheses'. (p.159 Note 7) (This is a Level I counter-discourse on the 'unmethodological').

What interests Feyerabend is the essential vacuity of Newton's central methodological rule (the parallel of our first two 'effects'), for the rule itself states nothing about what experience is; what is to count as experience; how it is to be located; how, if located, it is to be interpreted. Newton's (Level I) pronouncements name a privileged basis for theory without being able to specify how that basis is to be produced, recognized, evaluated or inferred from. This produces a situation in which any institutionalized teaching about what is to count as 'experience' can be used by believers to fill the criteriological void. (pp.155, 168-9) Feyerabend then shows how Newton summons 'experience' into existence by a two stage semantic slight of hand. First we have the paradigmatic experimental illustrations of the theory which are, of course, thoroughly theory- and standard-laden. (These being his presentation of the famous elongated prismatic spectrum, and his '*experimentum crucis*'.) The 'results' of these experiments, what Newton or we might perceive in them, are first identified with what Newton calls 'phenomena'. But 'phenomena' in his usages are to us highly generalized, idealized and differentially weighted observation reports. Then Newton claims that such 'phenomena' are the 'experience' which we seek as the sole basis of theory. (pp.161-5) That a theory does seem to follow from such 'experience' is hardly surprising, Feyerabend observes, since such experiences already have the logical form of laws, (p.163 Note 11) being general propositions well loaded with the terms of the very theory to be 'induced'. (This, I would suggest, can be seen as a very neat instance of the application effect, the convincing 'realization' that the rules of one's method do apply in the realm of the 'authoritative' method story which one has spun about one's practice using the resources of Level 1.) (Cf. Feyerabend's remarks at p.165).

⁵⁶ See Schuster (2013), Section 6.9.2 'The Failure of the *Regulae*, the Birth of the System and the Problem of the Cynical *Discours de la méthode*'; also Schuster (1993), 216-20.

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.⁵⁷

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. 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 rigorous 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.⁵⁸

Now, all the various grand doctrines of scientific method, as well as the particular stories derivable from them, form a reservoir of discursive resources available to scientists for use 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 debate, 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 practices; but, they can be deployed by players 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.⁵⁹

René Descartes certainly practiced such rhetorical deployments of method, mobilizing Level II accounts in order partly to constitute knowledge bids he was advancing on Level III—where the Level III fields might be construed as natural philosophy and other cognate or subordinate fields such as mathematics, medicine, optics, mechanics and the like. His methodological account in the *Regulae* of the discovery of the law of refraction and of its mechanistic explanation is just such a gambit. I have shown elsewhere that the story bears no relation to Descartes' 'bench practice';⁶⁰ yet, it structures a presentation of his work and so is partly constitutive of it as a knowledge claim proffered to the intended audience of the *Regulae*.⁶¹

⁵⁷ Schuster and Yeo (1986) and Yeo (1986).

⁵⁸ Perelman (1979); Perelman and L. Olbrechts-Tyteca (1971); J.R.Ravetz (1971); Yearley (1981); Weimar (1977).

⁵⁹ Mulkay and Gilbert (1981); Feyerabend (1975); Miller (1986); LeGrand (1986); Wood (1980); Richards and Schuster (1989).

⁶⁰ Of course, given the fact that Descartes left off the *Regulae* unfinished, the actual audience has consisted not of his natural philosophical and mathematical contemporaries, but mainly of modern historians of philosophy.

⁶¹ See the references in above in Note 22 or 36. Descartes' method story about his optical work also served several subsidiary functions in the overall interest of advancing his claims: First, it occluded the dependence of his actual work upon the traditional image principle rendered dubious by Kepler's findings in his new theory of vision. Second, it provided a (methodological) connection between the geometrical optical and physico-mathematical explanatory stages in his work. Thirdly, the vagueness of Descartes' methodological language about 'natural powers', and his methodological reflections about 'analogy' covered what was, in 1628, hesitation and ambivalence about the best direction to take in articulating a mechanistic model of light. 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. (Schuster, 2013, 197-203) 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.⁶²

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 post-Kuhnian work in the sociology of science which further established 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 in principle be questioned.⁶³ 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, not only did particular claims need to be woven out of the sturdy cloth of method discourse; but, the method itself, the ultimate legitimizing 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 legitimizing the method by the 'evidence' of concrete application and success. (In the *Regulae* 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 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 natural philosophy, its cognate and subordinate disciplines. 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, after 1628 when he began to work on *Le Monde*, the key issue for Descartes was the status of this, his emerging first system of mechanistic natural philosophy. In this regard he now became a symptomatic, indeed leading player in the central dynamic of this stage of the so-called Scientific Revolution, which focused precisely on the clash of opposing systematic visions of natural philosophy.⁶⁴ His method functioned on this peak level of struggle by supposedly underpinning 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

⁶² 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 specialist 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 about 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.

⁶³ Bourdieu (1975); Latour and Woolgar (1979); Callon (1980); Shapin (1982); Mulkay and Gilbert (1981).

⁶⁴ See Schuster (2013) Chapter 2 and Schuster (1990, 2002).

appetizers for the natural philosophical 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 in the *Discours*, 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 may have ceased to believe.

To conclude, making use of the totality of my work on Descartes, I would submit the following: Descartes' natural philosophical, scientific and mathematical work did not emerge from a method and neither did his order of study and biographical trajectory; nor, finally was his mature, metaphysically legitimated system of nature methodologically 'deduced' from first principles. By the same token, and all nostalgia for the good old days of Cartesian studies aside, one cannot take seriously the autobiography in the *Discours* as anything less than a method articulated, post-facto, self-legitimizing narrative. As for the *Regulae*, I would now say the following: the *Regulae* obviously constitute a critically important obstacle and challenge to any 'method-smart' modern scholarship about Descartes. One must ask: What is the text of the *Regulae*; when was it written; what are its subject and aim; and what was the fate of this abandoned project, premising all the answers on the new view of method as discourse which structurally cannot accomplish what it structurally so convincingly says it can accomplish. These are the problems the solutions of which have been attempted in these two parts of my presentation to the Institute of Philosophy, Czech Academy of Science, Prague.

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