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SCIENCE AND SKEPTICISM IN THE SEVENTEENTH CENTURY:
THE ATOMISM AND SCIENTIFIC METHOD OF PIERRE GASSENDI

by

SAUL FISHER

A dissertation submitted to the Graduate Faculty in Philosophy
in partial fulfillment of the requirements for the degree of
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Abstract

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Adviser: Professor Martin Tamny

In this account of the philosophical and scientific pursuits of Pierre Gassendi (1592-1655), I challenge a traditional view which says that the inspiration, motivation, and demonstrative grounds for his physical atomism consist not in his empiricism but in his historicist commitments. Indeed, Gassendi suggests that it's a consequence of our best theory of knowledge and sound scientific method that we get evidence which warrants his microphysical theory.

The primary novelty of his theory of empirical knowledge is his proposal, against the Stoics and Descartes, that it is not a necessary condition of our knowing some claim that we are certain of it. This move immediately broadens the scope of what we can know through the senses, as does his suggestion that we include among such knowledge claims those assertions about what is hidden to the senses which we legitimately infer from the evidence of the perceptually given.

On the basis of these epistemological positions, Gassendi develops his views on

scientific method according to which (1) we attain and justify our best empirical claims by deduction yet, he believes, these claims are at root probabilistic, and (2) we may maintain hypotheses as the basis of our scientific reasoning so long as there is some empirical evidence for them, however broadly construed.

Hence Gassendi suggests that his atomism constitutes a 'most likely hypothesis' for which we have empirical evidence--which of necessity is found indirectly in 'indicative' signs, those surface level phenomena for which he takes the existence of atoms to be a *sine qua non* condition. Perhaps the paramount case of such evidence consists in those microscopic observations of crystalline formation and dissolution that he takes to demonstrate the molecular structure of matter--a key and innovative consequence of his atomism.

The robust character of Gassendi's method emerges in his appeals to indirect evidence for claims about the unobservable and his willingness to count this as adequate empirical grounds for maintaining an atomist hypothesis. Remarkably, he stands at the dawn of the modern era with at least a proposal as to how to resolve one of empiricism's more vexing difficulties, still with us in some form today.

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PREFACE: WHO WAS PIERRE GASSENDI?

A. Gassendi's Different Careers

1. Savant, Scientist, and Priest

Though he was born in 1592 to a rather undistinguished family in the equally undistinguished Haute-Provence¹ town of Champtercier, Pierre Gassendi² was to become the greatest Provençal scholar of his day, a member of the preeminent French intellectual group of his times (the Mersenne circle), and a professor of mathematics at the College Royale. His path to these heights began when he was recognized by his instructors early on as an intellect of great potential, and sent at a tender age to Aix-en-Provence for studies of further sophistication than his local schooling could provide. In these first years of scholarly pursuit, Gassendi shuttled between Aix and Digne (his provincial capital) and so set a life-long pattern of not staying still in one place for very long. His extensive and early education enabled him to win an appointment as Professor at Aix while still in his late adolescence. He was shuttling

¹Or Basse-Alpes, depending on one's temperament.

²There is an entirely inconsequential controversy over whether his name is properly 'Gassendi' or 'Gassend'. The answer, in short, is *both*: the usual account is that his family name is 'Gassend'--and this is how he signs his correspondence--but that he was known widely (in his day, and thereafter) as 'Gassendi' because a contemporary savant incorrectly interpreted his name back from the Latinized 'Gassendus' into French with the supplementary 'i' at the end. It is further possible that this mistake could have corrected another error, in the not implausible case that his family name was rendered by Church officials in French from the Provenço language--in which it would undoubtedly have been 'Gassendi'.

between careers as well, attaining success and station not only in academic circles but in the Catholic Church as well.

Gassendi's career as a priest is an undoubtedly crucial facet of his intellectual constitution: his writings reflect an unbending allegiance to holy scripture and Church teachings, though not necessarily in orthodox doctrinal lights. He was ordained at an early age and, while there is no question of the strength of his faith, one motivation for his career in the Church appears to be its life-long provision of a sinecure. Thus Gassendi rose to the rank of a low-level local official in Digne, but never any higher. Though he clearly pursued a studied relationship with his higher authority, all the while he wrote letters of support to the Church-embattled Galileo, sought appointment to the secular College Royale, and cultivated deep personal and intellectual ties with his non-Church patrons, the Provençal savant Nicole-Pierre Fabri de Peiresc, the local count of Alais--Louis Emmanuel de Valois, and the Parisian Habert de Montmor.

After some years of instructing pupils in philosophy and theology, Gassendi distanced himself from what he believed to be rigid teachings of the Scholastics in his Exercitationes Paradoxicae of 1624. Thereafter he began a formative partnership in physiological, astronomical, and historical studies with the wise and wealthy Peiresc, summarized in a glowing biography written upon his death in 1637. By this time, Gassendi had also developed his early interests in restoring the philosophy of Epicurus--in the way that Thomas had restored Aristotle, integrating his thought with what he believed theologically viable--and in a variety of questions in basic physics. His writings on these last interests brought him to the attention of the Minim priest, Marin Mersenne.

Gassendi spent the last two decades of his life travelling back and forth between Provence and Paris because of his periodic involvement with the group of philosophers and scientists brought together through correspondence with Mersenne. In the Mersenne circle, debates ranged over numerous topics central to the dismantling of Aristotelian and Scholastic world-views, and Mersenne often used his role as facilitator to bring together opponents on these issues. Notably, his negotiations enabled the entry of Gassendi's criticisms of Descartes's Meditations into the Objections and Replies (Gassendi subsequently published his rebuttals in his Disquistio Metaphysica of 1646). Members of this circle regularly reported on each other's experiments and proposed new challenges, such as Poysson's well-known puzzling over whether physical indivisibles could or should be identified with mathematical points. Gassendi used this forum to learn about other empirical studies as well as to work out a good many of his views.

His scientific career was varied and complex. Some historians consider his greatest accomplishment in this sphere to be his contribution to the revival of ancient atomism (which I discuss at length in chapter three), but this represents only one end of his anti-Aristotelian physics, and a small part altogether of his scientific interests. His other work in physics includes the study of bodies in free fall (closely modelled on Galileo's work), a mature enunciation of the principle of inertia, and an early and reasonably accurate interpretation of the Pascalian barometry experiments of the late 1640s. He also ventured into experimental science: he attempted to measure the speed of sound by cannon fire, arranged to have weights dropped from the mast of a moving ship off Marseille to enact Galileo's thought experiment (and so dispel doubts about the

motion of the Earth), and conducted various chemical trials involving, among other things, the dissolution of salts and formation of crystals (to bolster his views about the molecular nature of matter), to mention some of his better-known exploits. In addition, he offered a wide variety of speculations on the earth sciences based partly on geological field work and biological and physiological observations and as shaped by his atomist hypothesis. Finally, Gassendi devoted much of his time to astronomical pursuits. He made regular observations of the skies for decades, in an effort to lend credence to Kepler's views, and in so doing he observed the rings of Saturn and the passage of Mercury before the Sun (1631) and successfully predicted an eclipse in 1654. Moreover, he commissioned the first map of the moon (drawn on the basis of his observations), defended the Copernican view as plausible save for its conflicts with Church teachings (he opted for the Tychean view, at least publicly, as a result), and offered many disparaging words on what he considered the scurrilous practice of astrology.

In his final years, Gassendi consented to the pressures of his friends and released a major portion of his Epicurean studies to the public, publishing his Latin translation of Diogenes Laertius's Book X on Epicurus, along with ample commentary, in his Animadversiones of 1649. He continued to work on this interpretive material, however, steadily incorporating his philosophical and scientific insights, until his death in the Paris apartment of Montmor in 1655. Montmor, acting as executor, collected this material in manuscript form and with Gassendi's other Parisian friends arranged to have it published as the posthumous Syntagma Philosophicum. The Syntagma is a far more systematic book than the Animadversiones, largely eschewing the sometimes philological character

of the earlier commentary and principally discussing logic, the natural sciences, psychology, and ethics from the perspective of what he deems philosophically, historically, and theologically supportable. His interests in Epicurus are ever-present, not least in the structure of the work, which is divided into a Logic (including his textbook-format *Institutio Logica*), Physics, and Ethics. Happily, we have a rather well-rounded picture of Gassendi's intellectual pursuits, on top of his Epicurean projects. Montmor and company had the good sense to bundle the Syntagma together with the better part of Gassendi's writings in six volumes of collected works, including as well his earlier letters on optics and the free fall of bodies, a portion of his voluminous astronomical observations, and a great deal of his correspondence.

2. *Philosophical Highlights*

In this study I explore in detail several central facets of Gassendi's thought, namely, his atomist hypothesis, and his theories of empirical knowledge and scientific method (and the relations between them). For many commentators, however, his paramount contributions to philosophy include not only his epistemological views--comprising an empiricist theory of knowledge and a set of theses which have been dubbed "constructive skepticism"--but also his objections to Descartes's Meditations, in which he seeks to refute the criterion of clarity and distinctness and strives to undermine the reasoning behind the *cogito*. Here I review some of the chief elements of these views.

While I will discuss Gassendi's theory of empirical knowledge at length in chapter one, in this context I shall simply stress how his view that all knowledge comes from the

senses contrasts with Aristotelian and cartesian perspectives. The central claim of Aristotle's theory of empirical knowledge is that we know universal truths about natural kinds in the world, through a combination of observational experience, intellectual intuition, and logical demonstration. Our principal means of gaining all knowledge (including what we know of the external world) is through demonstration and use of the syllogism. Aristotle and like-minded scholastics take it as evident that a body of knowledge built solely on syllogisms requires well-grounded initial premises in the form of fundamental principles and so they propose knowledge begins with what they consider to be universal and necessary principles such as a principle of non-contradiction. Empirical knowledge accordingly begins with universal principles relating the essences of forms of objects in the world, such as "all A's are B's". But we can only know principles like "all A's are B's" about worldly objects if we have 'real' definitions giving the essences of such objects; otherwise we could not be certain about the necessity of any A and B having those properties f that would make all A's B's. The proposal is that we get such real definitions by induction on our particular experiences (observations of individuals of a given kind having properties f) plus acts of the intellect where we intuit, as a real definition of individuals of some kind y , the essence x from which it follows that y s have f essentially.

The goal of science according to this view is the organization of real definitions in a branched hierarchy ('tree of porphery') which, giving the relations of sorts of things relative to their essential properties, maps the natural kinds. This grand picture of empirical knowledge (and science in particular) as an account of immutable and

underlying natures rests on the suggestion that we can get from induction on particulars to universal claims of a necessary nature, and the justification for this move seems to be that the 'right' induction is somehow persuasive and our powers of intuition enable us to spot a real definition when we are presented with the right inductive arguments.³ Hence we attain empirical knowledge by employing (universal) principles of reasoning to calculate the relations of universal empirical principles and our beliefs about particular specimens or instances, each of which we base ultimately on individual empirical experiences.

Underlying this view of empirical knowledge are at least two metaphysical assumptions, that the world is carved into the sort of neat substances Aristotle posits, and that the kind of substance a thing is, is determined by some immutable essence.⁴ In addition, Aristotle presumes we have some epistemic access to those essences such that we can determine the real definitions of substances. Given these sizable assumptions, the principal problem Aristotle leaves us with concerning empirical knowledge is how to connect up our experiential data with his demonstrative picture of the sciences, that is, how to lend precision to his suggestion that we 'intuit' universals on the basis of inductions on our data.

In the cartesian picture of empirical knowledge we find an attempt to work out something like this last problem of Aristotle's. Descartes agrees that we know about the

³Though the connection between the universal and the necessary is of course tenuous, Aristotle does not talk about the possibility of intuiting contingent universal truths, no doubt because he is a firm believer in natural kinds.

⁴In what follows, I employ the term 'essence' to refer, roughly, to the Aristotelian underlying defining character of a thing; 'essential characteristics' accordingly refer to the elements which comprise that character.

world through reason: necessary laws govern the relations between objects and the necessary features of substances tell us about the underlying reality of the world, but perceptual knowledge concerning objects cannot give us a true picture of these necessary laws or features, so we must rely on knowledge from reason alone to arrive at these truths and reveal their relations, through deductive logic. At the same time, Descartes like Aristotle recognizes the value of attaining empirical knowledge through the senses. There is a body of claims about the world we may strive to improve regarding appearances, not underlying reality *per se*, and any such science of appearances (and our collection of justified empirical beliefs generally) relies on information from the senses. Our sets of beliefs about appearances and about their underlying reality come together when we structure the empirically-derived beliefs as theoretical claims related by deductive proof. His suggestion is that geometry models our empirical beliefs concerning shape and motion in just this way, and that the rest of our empirical beliefs can either be subsumed under geometric argument or else under similarly deductive arguments with the support of a few central first metaphysical principles--a project he tries to sketch in the Principles of Philosophy. This approach at once widens and bridges the gap between an inductivist view of perceptually-based empirical judgements and any deductivist edifice of beliefs about the way the world 'really is'. On one hand, Descartes suggests there is no necessary connection between knowledge of appearances and knowledge of physical or metaphysical 'reality': our sensory-based knowledge of appearances is particular to our perspectives whereas our reason-based knowledge is not. Yet he also proposes how we may fit sensory based beliefs into the deductivist structure of our metaphysical and

physical theories. In contrast with Aristotle, Descartes tries to minimize his reliance on intellectual intuition for putting together the two kinds of beliefs; instead, he places faith in the incontestability of geometric and other deductive reasoning, and the particular basic metaphysical principles he ordains. Both sets of beliefs meet his proposed truth-criteria⁵ of being clear and distinct, Descartes argues, and thereby yield a rational framework for those truths we arrive at through observation and experiment.

Naturally, Descartes's views here also rest on important assumptions, and some of these bear a surprising affinity to suggestions made by Aristotle--for example, that there are necessary laws and features in the world just waiting to be discovered by us, and that we can arrive at certain knowledge about such necessary aspects of the world given our pure *apriori* reasoning about the matter. In addition, Descartes proposes that knowledge of appearances helps guide our choice of the right deductive account of necessity in nature, and this proposal rests on (among other things) his particular deductivist account of the sciences and background truth-criteria.

Gassendi, too, worries about problems in the Aristotelian view, but he does not believe they are resolved by Descartes. For one, Gassendi is not outwardly impressed by the notion that there is anything necessary about the way the world is. God, he proposes, could have made the world work in any number of ways, and the contingent history and character of Creation means, among other things, that there is nothing immutable about

⁵It's not entirely clear that Descartes intends his clarity and distinctness 'criterion' so much as a criterion of truth as a standard for epistemic warrant, or what John Pollock calls 'justificatory norms'. I say a bit more about this below when I get to Gassendi's rejection of the cartesian criterion.

the essence of a material thing.⁶ For another, Gassendi maintains that, regardless of whether there are any essences and whether they might be mutable, there are none to which we have any epistemic access. The sole originating source of our knowledge is the information our senses provide, and so what we know is closely linked to what we can perceive. But--as Descartes notes--we can perceive only appearances, and Gassendi draws from this point the very unc cartesian lesson that this is all we can know about, too, and this rules out knowledge of unperceivable essences. One line of reasoning which brings him to this conclusion can be found in his discussion of classical skeptical tropes concerning the relativity of evidence from the senses to individual experience--that honey tastes sweet to me, though bitter to you; and that fire seems hot to us, though not so to insects that live near fire.⁷ Since different people have different experiences, our knowledge of honey's taste or fire's heat differs from person to person and thus is not a reliable guide to invariable characteristics of, for example, the honey or fire. In cases like these we know a thing's qualities only as we record them on a subjective basis, and such sensory information where our experiences vary intersubjectively cannot yield judgements about the thing's qualities which do not vary in that (or any other) way. Hence we lack knowledge of the thing's essence (if there is one), and the broader point is

⁶That a 'substance', in either the Aristotelian or cartesian sense, might have an immutable essence, is a different matter, and insofar as Gassendi has such a notion (for example, with respect to space, time, matter, and void) he agrees that such things feature unchangeable *sine qua non* characteristics.

⁷DM, Q III 388b; R 535.

that, generally speaking, we know only how things appear to us.⁸

Moreover, Gassendi throws out the entire Aristotelian package of views concerning knowledge of universals on the grounds that we cannot perceive anything more than particulars in the world—it follows, he claims, that neither can there can be any universals we know about, at least with anything like certainty. Indeed, his insistence that all knowledge comes from the senses leads him to oppose the suggestion of Descartes and Aristotle that there are *any* propositions we can know with certainty. Since all propositions are judged as true or false on an empirical basis, none can be deemed indubitable, save those of theology and theologically-derived cosmology. There are, it turns out anyway, Aristotelian and cartesian overtones to Gassendi's scientific method (see chapter two): there is clearly a place for logical demonstration, based on induction and deduction alike, in our attainment of knowledge about the external world and our organization of the same. Yet he believes there is nothing certain about such demonstration except the limits imposed upon it by the frailty of human intellectual capacities. The natural bounds of our epistemic grasp in the sciences or most any other field of study is just what our senses tell us, plus any correctives our powers of reason supply on the basis of the sensory-based knowledge we have already accrued.

That Gassendi thinks there is a need for correctives on sensory information makes clear the depths of his rejection of the sort of epistemic foundationalism we find in

⁸Gassendi proposes that our knowledge of an object's essence requires a "perfect interior examination" of the object, and until we complete such an inquiry we cannot know that essence (*DM*, *Q* III 311b-312a); elsewhere he says we cannot know the "profound nature" of things though we can know their appearances.

Aristotle or Descartes: we cannot even trust information from the senses to give us a failsafe picture of the world. In this he embraces the ancient Skeptical judgement that no source of our beliefs can provide us with certain knowledge. Yet he cannot accept their wholesale dismissal of sensory-derived evidence for our beliefs, and suggests instead that we indeed have knowledge from the senses just in case we have warrant for our judgements about appearances, though not necessarily warrant for certainty about those judgements. The source of this warrant, he asserts, is the very reliability of our sensory apparatuses (which he describes in his Epicurean-inspired account of perception). In sum, Gassendi proposes a 'constructive skepticist' response to the skeptics who say no knowledge is possible and to those 'dogmatic' thinkers (like Aristotle and Descartes) who say knowledge involves the attainment of certain belief--and is readily attainable. His response is that while it may not be possible to have certain beliefs it is quite possible to have knowledge given that we construe it as less-than-certain justified belief.⁹

Fortunately for posterity's sake, Gassendi did not merely object to Descartes in the abstract but engaged him in written debate, the fruits of which appear in his *Objections to the Meditations*. In his initial criticisms and subsequent response to Descartes's replies, Gassendi outlines some of the most crucial points levelled by any contemporaries against the cartesian claims. Thus, for example, he rejects the proposed standard for judging ideas, their clarity and distinctness by the light of reason on the grounds that reason, which comprises our intellectual judgements and interpretations of sensory information,

⁹Naturally, it must have a veridical character as well, and Gassendi thinks it sufficient that such beliefs are at least truth-resembling; see chapter one.

is itself prone to error:

...although deception, or falsity, is not to be found in the senses themselves, which merely behave passively and only report things as they appear and as they must appear given their causes, it is to be found in the judgement, or mind, when it does not act with enough circumspection and does not perceive that things which are distant...appear more indistinct and smaller than they do close up....¹⁰

If for all their faults claims based on sensory information are more reliable than those based on reasoning (about sensory information or anything else), then we ought not appeal to the latter as a basis for judging among claims of the former. One reason he thinks claims based on sensory information are the more reliable of the two is that he thinks the senses exhibit passive (hence steady) behavior in collecting information, as opposed to the active (hence irregular) behavior our mental judgement exhibits in organizing or relating information.¹¹

Further, Gassendi argues that Descartes's criterion is irrelevant to judging cases of empirical knowledge, and were this criterion not irrelevant, then Descartes's own claims to knowledge should fail. First, in the case of empirical knowledge, Gassendi counters that the cartesian criterion fails on classical skeptical grounds: it is possible for us to have ideas from the senses we take to be clear and distinct which are nevertheless not the basis for warranted claims to empirical knowledge of a general nature. Though we may have clear and distinct ideas about the appearances of, for example, the color of the sky we

¹⁰DM, Q III 388a; R 532; B 266-267.

¹¹He does not deny that we may have problems gauging the warrant for claims based on sensory information as we collect it, but he insists we will have even more problems when the claims we gauge are based on ideas about sensory information after we have removed it from a direct and perspicuous relationship to the world as experienced, by organizing and relating it in various ways to other ideas.

perceive, we cannot infer from this clarity and distinctness that we know what color the sky is. As the skeptics warn, the sky could appear to different persons in different colors, and as Gassendi adds, our ideas of such an appearance could each be clear and distinct.¹² Second, knowledge claims Descartes takes to be demonstrated by reason alone Gassendi dismisses as anyway failing the proposed criterion; they are partial and confused, he suggests, because they lack the immediacy characteristic of judgements we attain by strictly empirical means. Whereas ideas we gain from the senses directly represent worldly objects and events, ideas we attain by deductive proof are but hypothetical analogues to such sensory-derived ideas:

...it is not the same thing for us to conceive something by a veritable idea or a true image, and to conceive that thing by a conclusion that follows necessarily from an anterior hypothesis. In the first case in effect we conceive of the thing as absolutely so; in the second, that it should be some such thing; and also in the first case we know the thing distinctly and as it is in itself, and in the second case we know it only in a confused manner and by analogy, that is, in referring to it as something that must be known by way of some Idea.¹³

In brief, our reasoned claims are less than fully clear or distinct because we arrive at them through the mediation of ideas about our empirical knowledge. Hence Descartes's most cherished claims about essential properties of mind, God, and matter fail his own criterion for lack of direct foundation in our ideas from the senses.

Not all of Gassendi's criticisms of the clarity and distinctness criterion are rooted in his defense of knowledge from the senses. Thus, Gassendi suggests one other problem

¹²DM, Q III 314b; R 198-199.

¹³DM, Q 322b III; R 234. In this passage Gassendi contrasts the ideas or images of an object we attain directly from the senses with those ideas or images of the same that we infer from the ideas we attain directly.

with the cartesian criterion is that it cannot make good on its foundationalist promise. Just in case some of our clear and distinct ideas turn out to be wrong, we will require some further criterion to distinguish them from the correct ideas. And if the new criterion is simply something like 'more clear and more distinct', then we are on the road to infinitely many higher-order criteria, which defeats the aims of the foundationalist project altogether. Finally, Gassendi offers what Descartes himself came to call "the objection of objections." Our ideas are clear and distinct ideas is perfectly consonant with a solipsistic perspective, but then the only thing we can know with certainty is our own thoughts. Yet a viable criterion should (a) also distinguish our knowledge claims about the external world, and (b) mark solipsistic claims as dubitable to begin with. On top of these objections, Gassendi proposes Descartes has put the cart before the horse by suggesting we can readily recognize what is clear and distinct and use this as a guide to what is indubitable when what we need in the first place is some guide as to what is clear and distinct.¹⁴

Gassendi also raises objections against Descartes's views on mind and body, and these, too, have attained some renown. One such objection is a natural prelude to the famous puzzle over how mind and body could be thought to interact, posed by Princess Elizabeth of Bohemia. Gassendi wonders how Descartes could take the mind to be thinking substance which lacks extension yet claim that it is somehow attached to the

¹⁴"...the difficulty is not, as it seems, to know if one must conceive things clearly and distinctly so as not to be mistaken at all, but to know by what way or method it is possible to recognize that one has a conception so clear and so distinct that it is true and impossible that we are mistaken." (DM, Q III 372b; R 458)

body, given that anything joined to a body must itself be extended. Descartes's answer is that he does not take mind and body to be attached, and this response immediately invites Princess Elizabeth's puzzle.

A second objection is levelled directly against Descartes's *cogito* reasoning. Gassendi understands this reasoning as one person's inference from his or her undoubtable recognition of cognitive activity, to the claim that he or she exists as the selfsame seat of such activity. Against this inference he points out that the recognition that one has a set of thoughts does not imply that one is a particular thinker or another. Were we to move from the observation that there is thinking occurring to the attribution of this thinking to a particular agent, we would simply assume what we set out to prove, namely, that there exists a particular person endowed with the capacity for thought. Descartes is in dangerous waters at this point, for if he is indeed entitled to calling indubitable no more than the agent-independent claim that there is cognitive activity present, then he can be fairly associated with Averroist panpsychism, which bears a considerable taint in his day. From Gassendi's perspective, this is further evidence that Descartes places altogether too much faith in his criterion and the work he thinks it can do. That Gassendi is primarily concerned here with the character of Descartes's reasoning is plain enough, for in the end and broadly speaking, the two are close in their views about mind and body: they are both dualists of one stripe or another, and the particulars of Gassendi's physically-rooted psychology are reasonably similar to those of Descartes's

model.¹⁵

3. Gassendi at the Cusp of Modern Thought

Does Gassendi belong to the Renaissance or the early Modern era? While commentators and historians conventionally consider Gassendi as an early Modern thinker, there is good reason to see him as a thinker in the Renaissance mold, or at least a borderline figure, and much recent scholarship reflects this characterization. One facet of his work which seems like a page out of Renaissance philosophy and science is the historical focus of his method of inquiry. For just about each philosophical issue Gassendi deems worthy of discussion, he first introduces a wide range of previous competing views, beginning with the antique schools, which he considers as 'live' options. The history of philosophy is for him a source of vitally important reasoning, the generally correct way to frame our questions, and more occasionally, the answers to those questions. Thus, as I argue in chapter three, one principal attraction of atomism for Gassendi is that it suggests a way to think about causation among material objects which he finds an attractive alternative to Aristotelian views on one hand and non-physicalist views on the other. In his theory of knowledge we find one more instance of using ancient frameworks to model contemporary problems: no criterion of truth, for example, is satisfactory unless it satisfies the classical skeptics. Ironically, some might see this as

¹⁵On the other hand, for some of the differences which separate them, see Emily Michael and Fred S. Michael, "Two early modern concepts of mind: reflecting substance vs. thinking substance," *Journal of the History of Philosophy* 27 (1989): 29-48, and "Hierarchy and Early Empiricism," in *Antifoundationalism Old and New*, ed. Beth Singer and Tom Rockmore (Philadelphia: Temple University Press, 1991).

evidence of Gassendi's modernism, but he is certainly among good Renaissance company in his concerns with skeptical thought.

Another Renaissance-type element of Gassendi's writings is his stylistic obsession with antiquity. His Latin is learned if a bit stultified and this embroidered, ornate quality marks his French, too. He quotes frequently and liberally from classical sources, usually in Latin though sometimes in Greek as well, and his translation of Diogenes Laertius's Book X on the life of Epicurus is the centerpiece of his 1649 Animadversiones. Gassendi does not, as does Descartes, suggest his work is so modern as to have been invented *de novo*; rather, he constantly refers the reader to a wide variety of other, generally classical writers as sources of both agreeing and differing perspectives. Finally, the single most time-consuming project of his career consists in his prodigious efforts towards reviving the works and reputation of one particular classical figure, Epicurus. Thus it is hardly surprising that much of the recent Gassendi literature (some of which I briefly discuss below) highlights his classical interests.

I agree that these Renaissance trappings are a key facet of Gassendi's work, but it seems to me that there are at least two compelling reasons to think of Gassendi as very much among the moderns. For one, he embraces the empiricist's assessment of the old science: what is wrong with the Aristotelian's physics is its fruition as well as grounding in *apriori* theoretical claims. Galileo is among the first to distinguish a contrasting science of motion that, in principle at least, makes use of observation and experiment, and Bacon and Descartes also counsel (to varying degrees) a scientific method which builds on experiential knowledge. But among the *savants* of the early seventeenth century, only

Gassendi, whose awareness of empiricism's roots and implications is evident across his intellectual pursuits, integrates philosophy and science on what he believes to be strictly empiricist grounds. This integration is a natural consequence of his suggestion that we gain all knowledge--outside of the theological--from the senses.

A range of equally modern views follow from his proposed empiricist foundations for the new science and its methods. Thus he suggests a probabilist notion of what counts as warranted empirical belief, and insists that we may license beliefs about the sub- and supra-perceivable but only if they are well-grounded in our beliefs about the perceptually given. Further, he devises rules for accepting or rejecting hypotheses, and guidelines for directing empirical discoveries and our judgements of the same, which he holds to the higher-order rule that all such claims must pass experimental or observational muster. It is far from true that all (or even most) of his own claims about the nature of the physical world meet this test, but the modernity of Gassendi's philosophy and science lies in his proposal that we set this as a goal.

B. A New, Integrated Picture of Gassendi

1. Some Recent Literature

Whether Gassendi is to be viewed as a modern in these or other ways is not, however, a problem posed or answered by much recent Gassendi scholarship, generally because of that difficulty which afflicts the blind man relative to the elephant: we get starkly different and exaggerated notions of the creature from the perception of his vastly different parts, and so we miss his character as seen in its entirety. The situation is all the

worse because a number of contextualist historians portray Gassendi principally as an opponent of one or another contemporary, to the neglect of his positive theses. While the idea in those contextualist studies is to demonstrate that he is a key participant in numerous important debates of the early modern era, the picture that emerges from some deliberation on these commentaries is rather Gassendi's relative eclipsing by other figures, notably Descartes. For example, Barry Brundell highlights a strong anti-Aristotelian strain which guides Gassendi's earlier criticisms of the scholastics as well as his later Epicurean works, and a consequence of this view (though not one Brundell explicitly advocates) is that in effect Gassendi may be seen as fighting some of the same battles as Descartes.¹⁶ Gassendi inevitably loses in any such comparison because he engages his foes with generally lesser flash and apparently less sophisticated, or at least less novel, weaponry. Lynn Joy, for her part, emphasizes the historicist elements of Gassendi's method, as employed in his conversations and correspondence with the members of the Mersenne circle. Though Joy argues for Gassendi's central role in this circle, and the centrality of that circle in early modern scientific debate, it is important to remember that the most important figure to this group was not actually an active participant in it, and this was Descartes. Mersenne, of course, is the other core personality in this context, and

¹⁶The literature I discuss here includes: Barry Brundell, Pierre Gassendi: from Aristotelianism to a new natural philosophy (Dordrecht; Boston: D. Reidel, 1987); Lynn S. Joy, Gassendi the Atomist (Cambridge: Cambridge University Press, 1987); Margaret J. Osler, Divine Will and the Mechanical Philosophy (Cambridge and New York: Cambridge University Press, 1994); Thomas M. Lennon, The battle of the gods and giants: the legacies of Descartes and Gassendi, 1655-1715 (Princeton: Princeton University Press, 1993); Olivier René Bloch, La philosophie de Gassendi: nominalisme, materialisme, et métaphysique (La Haye: Martinus Nijhoff, 1971), and Richard Popkin, "Gassendi, Pierre," in The Encyclopedia of Philosophy, ed. Paul Edwards (New York: Macmillan and the Free Press, 1967), Volume 3, 269-73.

so Gassendi by default is at best a third.¹⁷ Yet there is no better place to see Gassendi poorly comparing with Descartes than in the recent literature on their direct conflict over the Meditations. In their respective contributions to this literature, Margaret Osler and Thomas Lennon each focus on Gassendi's role as the premier contemporary critic of Descartes and accordingly present his anti-Cartesian views as a prism through which we may best perceive the spectrum of his views. While there is undoubtedly a good case to be made for the claim that he represents the most prominent alternative to Descartes in his times, putting a spotlight on this facet of Gassendi's career encourages the view that he deserves no more than footnote status and emphasizes, at least historically speaking, his role as the *losing* alternative. Indeed, as I suggest below, there is manifestly a great deal more to be said about Gassendi than what he believes to be wrong about the *cogito*. It is an indication of the work which remains to be done on Gassendi that, by comparison, nobody could possibly make a similar mistake about Descartes.

An even less promising fashion in the literature has it that Gassendi's doctrinal beliefs form the systematic foundation of his philosophical views. Thus Olivier Bloch, in an extensive and masterful study, maintains that Gassendi's spiritual concerns and materialist ontology jointly shape the character of his metaphysics--leading to irresolvable internal conflict--while Margaret Osler proposes that his theological views alone, and specifically his voluntarism, lead him to his empiricism. These analyses suggest Gassendi is best understood as laboring in service of his theological beliefs, or at least as

¹⁷It is far from clear, in any case, that what excited other members of Mersenne's circle about Gassendi's work was the historicizing aspect of his writings.

motivating his philosophical views by appeal to one or another theological view. It seems to me that such a suggestion is off the mark. While he indeed tailors his Epicurean views to meet theological constraints and endorses the only astronomical world-view he believes acceptable to the Church (the Tychean view), Gassendi's definitive criterion for any physical, metaphysical, or epistemological thesis is its approximation to the truth, which he generally views as empirically-determined. It is also the case that some theologically-inspired claims are woven deeply into the fabric of his metaphysics and psychology. For example, he holds that there are two souls, one rational and the other not, and he intends the latter to satisfy religious demands for an immortal unity attached to, but not susceptible to the fate of, the material body. But in all such cases Gassendi is quite clear about his non-philosophical motives in introducing such corrections or additions to his reasoned or empirically-demonstrated views as are necessary by the dictates of faith or Scripture. I argue, then, that to suggest that he comes to the main tenets of his metaphysics or epistemology to draw out the ultimate consequences of his theology, thereby misconstrues not only his broad philosophical motives but his particular reflective and investigative strategies as well.

By way of contrast, some recent scholarship highlights Gassendi's philosophical motivations and strategies *in strictu sensu*, and so follows in the footsteps of Bernard Rochot who, in the years during and following the second World War, brought to light numerous lesser-known texts as well as the manuscript background to Gassendist atomism. Among the more important work in this tradition is a series of articles by Fred and Emily Michael which call attention to the empiricist sources and character of

Gassendi's psychology and epistemology. But it is Richard Popkin who has most elegantly posed the global character of the empiricism which links Gassendist philosophy and science. Popkin ties together two central facets of Gassendi's thought, proposing that the constructive skepticism which lies at the core of his epistemological views is an attempt (among other things) to show how to have an atomist science, that is, through inferences based on our data concerning appearances:

Gassendi did not claim we could discover the real nature of objects by experiential reasoning....Rather, he claimed that we could find scientific explanations of the causes of our experience... [and that such explanations constitute knowledge which]... results from a most careful examination of appearances and a most cautious rational evaluation of the data derived from them. This evaluation is not based on knowing the real nature of things, but on a consideration of the conditions that would make our experience both possible and intelligible. For Gassendi, the best explanatory system was atomism, which can account for the sense qualities we experience and can provide a model for the known data about the observable world. Gassendi's atomism, derived from a study of the classical Epicurean texts, was not advanced as a metaphysical theory about the true nature of things. The atomic world is inferred from experiential indicative signs. It is confirmed by verifying predictions about atomic effects in the observable world. Gassendi limited his descriptions of the characteristics of atoms to sensory qualities found in experience.¹⁸

Thus the starting point of Gassendi's philosophy, in Popkin's view, is his skepticism about knowledge of essences, which is mitigated by allowance for warranted beliefs about appearances and causal knowledge to which we are entitled just because it helps us to make sense of our beliefs about appearances. There are a few small difficulties here yet the picture is, I believe, largely correct. One problem is the suggestion that Gassendi rules out knowledge of essences as the basis for accepting atomism and instead embraces atomism solely on the basis of something that looks like inference to the best explanation

¹⁸Popkin, "Gassendi, Pierre," 271.

(IBE). As I suggest in chapter three, it is not clear that Gassendi must reject the former even if he accepts the latter, nor is it so that he actually relies on IBE arguments *alone* to defend atomist claims. Another apparent problem in this vein is that, given that we infer our beliefs about atoms from experience and confirm those beliefs on the basis of their predictive value, it seems we land on atomism in virtue of the supporting evidence and not the explanatory value of the thesis: hence there should be a diminished (though not necessarily empty) role for IBE arguments. However, I think Popkin's assessment anyway hints at a correct understanding of Gassendi as well as an interesting point about IBE--and why this is not a problem after all. When Gassendi defends atomism by appealing to its explanatory value, he cannot grant that the evidence is equally compelling for all competing theses, just because he takes it that atomism is the physical thesis which best makes intelligible our experiential data to begin with. In brief, the way we view data about appearances depends on which physical thesis we are entertaining.¹⁹ Finally, Popkin is wrong to suggest that characteristics of atoms simply resemble the features found in experience; as I point out in chapter three, the way atoms move in Gassendi's view is quite distinct from the sensible motion of supra-atomic bodies. Despite these small faults, it seems to me that Popkin nearly pinpoints the project which binds together the different principal themes in Gassendi's corpus, to wit, showing how to be an atomist if you're a thoroughgoing empiricist. Naturally, given the brevity of his remarks, Popkin spells out neither how constructive skepticist principles lend support to a scientific method, nor how our views of what constitutes empirical knowledge or scientific method

¹⁹I return to this issue in my concluding chapter.

might licence the particular hypothesis of atomism.

2. Aims of this Thesis

My thesis is an attempt to present a fuller picture of Gassendi than that offered by commentators on his negative theses, and an intense exploration of Popkin's suggestion concerning the relations between his philosophy and science.²⁰ To fill out the conceptual geography of Gassendi's epistemology, metaphysics, and physics, I argue for several new views of his thinking: that he anticipates the crucial elements of contemporary reliabilist theories of warrant, that he adapts the Renaissance *regressus* method to suit his probabilism about empirical knowledge, and that aspects of his atomism fit poorly with the mechanical philosophy it is supposed to exemplify, to name some important claims of chapters one, two, and three, respectively. But the main thrust of my more complete and refined picture of Gassendi is to depict his philosophical and scientific pursuits as part of one and the same project. This contrasts with a traditional view which says that despite his empiricism, the inspiration, motivation, and demonstrative grounds for his atomism is instead purely historical. That, however, is only part of the story, for Gassendi also argues for atomism as a consequence of his empiricism. He thinks it's an outgrowth of our best theory of knowledge and sound scientific method that we get evidence which

²⁰Other such attempts in this direction include Wolfgang Detel, Scientia rerum natura occultarum: Methodologische Studien zur Physik Pierre Gassendis (Berlin; New York: De Gruyter, 1978); Joy, Gassendi the Atomist; W. Makin, "The Philosophy of Pierre Gassendi: Science and Belief in 17th Century Paris and Provence" (Ph.D. diss., Open University, 1986); Marco Messeri Causa e Spiegazione: la Fisica di Pierre Gassendi (Milan: F. Angeli, 1985); Osler, Divine Will; and Lillian Unger Pancheri, "The Atomism of Pierre Gassendi: Ontology for the New Physics" (Ph. D. diss., Tulane University, 1972). Either because of historiographical commitments (e.g. Joy and Osler) or not, all these works save Detel's, however, pay scant attention to the particulars of Gassendi's arguments.

warrants his microphysical theory. It is the central claim of my thesis that Gassendi relies on his empiricism and corresponding theory of method to provide sufficient, and perhaps even necessary, conditions for a scientifically viable atomism.

In arguing for this particular historical claim, I address this timeless question: what can we expect to know about unobservables given the epistemological stance that all knowledge is from the senses? The answer Gassendi offers, I propose, is that we can know about unobservables like atoms just in case we construe our empiricism in the 'right' way.

To see what this entails, in my first chapter, I outline the character of Gassendi's theory of empirical knowledge. Thus, against the Stoics and Descartes, Gassendi argues that it is not a necessary condition of our knowing some claim that we be certain of it. This move immediately broadens the sense of what we can know through the senses, as does his suggestion that we include among such claims to knowledge those assertions about what is hidden to the senses which we legitimately infer from the evidence of the perceptually given. This is his theory of sign-based inference, which suggests such inferences are legitimate just in case they would be agreed to by a great enough number of experts giving testimony or else would be false only on pain of contradiction.

On the basis of these epistemological claims, Gassendi develops his views on scientific method according to which, as I explain in chapter two, (1) we attain and justify our best empirical claims by deduction yet, he believes, these claims are at root probabilistic, and (2) we may maintain hypotheses as the basis of our scientific reasoning so long as there is some empirical evidence for them, however broadly construed.

Hence, as I suggest in chapter three, Gassendi suggests that his atomist views constitute a 'most likely hypothesis', for which we have empirical evidence--though of necessity it is found indirectly in 'indicative' signs, those surface level phenomena for which he takes the existence of atoms to be a *sine qua non* condition. Perhaps the paramount instance of such evidence in Gassendi's view is what we may see through the microscope. He argues that the microscopic observations of crystalline formation and dissolution demonstrate the molecular structure of matter--a key and innovative consequence of his atomism.

In my concluding chapter, I suggest that the robust character of Gassendi's method emerges in his appeals to indirect evidence for claims about the unobservable and his willingness to count this as adequate empirical grounds for maintaining an atomist hypothesis. One of the remarkable features of Gassendi's philosophy, then, is that he stands at the dawn of the modern era with at least a proposal as to how to resolve one of empiricism's more vexing questions, still with us in some form today.

C. Methodology

In general, I view the history of philosophy as notes on an ongoing conversation (what Edwin Curley has called a "dialogue with the dead"), such that writing that history entails first and foremost grasping the details of arguments which the conversants propose (what is sometimes called the "arguments of the philosophers" approach, after the book series of the same name)--and this study reflects an effort in that direction. It is true, to be perfectly fair, that the context of Gassendi's times is an invaluable guide to his thought,

since much of his outlook is shaped in response to those of his fellow early modern thinkers. Accordingly, I incorporate such material where I believe it is of the utmost importance for following the train of Gassendi's thought. But two factors prevented me from pursuing more of a contextual approach. For one, a contingent feature of the currently available literature is that there are no adequate overviews of Gassendi's philosophy and science written from a non-contextualist, argument-focused approach, and I therefore present this work in the hopes of providing one. For another, Gassendi's Renaissance manners of scholarship lead him to a style of exposition which involves review of and response to significant chunks of the history of science and philosophy--so much so that any discussion of his own thought which is contextualist in the classical sense (that is, in the sense of focusing on his immediate, contemporary context) runs the risk of missing the broad intentions Gassendi has for his work, in which he addresses various metaphysical, epistemological, and methodological traditions in the grand sweep of Western thought. To fairly identify and explain his own perspectives, then, it seems we owe him a 'grand sweep' analysis, a view of his thinking at least partly abstracted away from the thinking of his contemporary fellow intellectual travellers. Beyond the assessment of Gassendi's thought, I believe such an abstract focus on what a given philosopher sees as his or her contribution to the tradition marks a necessary first step in writing the history of philosophy. In this way, we do not give short shrift to the greater historical intentions that a philosopher may have (which are rather close to the surface in a philosopher who, like Gassendi, expends so much effort discussing the history of various philosophical issues). We also gain a better sense of the overall place of that

writer's reasoning in the immense spectrum of thought that is Western philosophical history. Without the benefit of this abstract focus, the contextualist approach is really a different project, aiming instead at a conceptual map of coarser grain and with broader intellectual connections. That is rather the map drawn in the history of ideas.

CHAPTER I

GASSENDI'S THEORY OF EMPIRICAL KNOWLEDGE

In this chapter I sketch the contours of Gassendi's theory of empirical knowledge. I suggest we best understand this theory--and his entire epistemological project--as an attempt to resolve skeptical worries by advancing a set of truth-criteria and elements of epistemic warrant that he describes in the physicalist terms of his account of sensory perception and the formation of ideas. Gassendi is ever the historian of philosophy and he deploys this theory in a bid to reject an Aristotelian picture of essentialist empirical belief, and counter Descartes's cognitive and epistemological views. Yet an account of that and how we gain all our knowledge through the senses is not just a foil Gassendi wields against adversaries past and present; it is the product of a body of considered views including his theories of perception, ideas, truth, and justification. He clearly uses these views to respond to Skeptics, Aristotelians, and Cartesians alike that we can have warranted empirical beliefs without certainty, based on trustworthy evidence from the senses. This last, broad claim stands or falls, however, not so much as it fares as a response to Gassendi's adversaries, but as fare his arguments for its constituent reliabilist and probabilist claims and the background principles on which it rests. Thus the central claims of his theory of empirical knowledge include the proposals that our empirical beliefs are warranted just in case the evidence for them is gathered in a generally reliable fashion and does not conflict with accepted evidentiary experiences, and that we can maintain empirical beliefs about which we cannot be wholly certain. For Gassendi, these

positions follow naturally from some of his key views on cognition and the truth and justification of our beliefs. These include the suggestion that the mind receives sensory information through a process we can describe in wholly physical terms, that some of our perceptual givens serve as tools of inference that enable us to make warranted claims about physical phenomena to which we have no direct sensory access, and that we can use our cognitive faculties to judge the truth of our empirical beliefs because those faculties dependably allow us to detect the truth properties of the objects of knowledge.

A. A Conceptual Map: The Background Skeptical Challenge

We typically and intuitively accept that our perceptions of appearances tell us *something* about the world, if only how the world appears to us. A question that has engaged many a thinker is whether such perceptions yield warranted claims about the way the world actually is, beyond appearances. If we accept that knowledge minimally comprises beliefs that are justified and true, then the primary puzzles of perceptual knowledge include the search for justification of beliefs we base on perception and the criteria for the truth of such beliefs. These challenges are outlined early in the history of Western philosophy by the ancient Skeptics who propose that we face great obstacles in trying to identify either a truth-criterion or the proper warrant for taking any of our beliefs (empirical or otherwise) as instances of knowledge. Indeed, they suggest our inability to find warrant for our beliefs is a result of our inability to locate a truth-criterion; having such a criterion is understood as a necessary component of coming to beliefs for which we have good reasons, one good reason apparently being that the belief is judged to be

true.¹ Lacking a means of distinguishing true and false claims, we ought not invest belief in any of them but instead suspend our judgement as a means of attaining mental composure and tranquility (*ataraxia*).

Thus the basic problem posed by the Skeptics with respect to empirical judgements in particular is how and why we can call our claims about the world to be certain knowledge. Whereas some classical philosophers such as Aristotle and the Stoics argue that evidence from the senses lends sufficient warrant to our empirical beliefs, the Skeptics view that affirmation as sheer dogma. They counter that we lack a criterion for differentiating true and false claims based on evidence from the senses and that even if we had such a criterion, we would still lack absolute assurance that any of these claims are certain.² The most we can say about sensory evidence, they suggest, is that it gives us justified beliefs about the way things appear to us.

Possible responses to the challenge posed by the Skeptics include those that define the epistemic judgements to which we are entitled,

- (a) lacking truth-criteria, justification, or both, for our beliefs.

¹It is not clear why they include knowing that *p* is true as a necessary condition for taking *p* to be justified (that is, as a necessary reason for *p*) given that we frequently have beliefs we take to be justified that turn out to be false though we remain justified in retrospect, for example, given our background information at the time. A response here might be that, just as we recognize what we previously took to be true as false, we should recognize what we previously took to be justified was in fact not so. A problem with this response is that we can be genuinely wrong about beliefs being true, but given that we meet the relevant justification standards it is hard to see how we could be wrong about being justified.

²The Pyrrhonians who precede the Skeptics of the Academy and the Post-Academics (including Sextus Empiricus) point out that if we have no justification for saying that we know something with certainty, then we can no more justify our claims against the possibility of certain knowledge than we can the claims for it. They conclude that we should suspend judgement even about the question of whether or not we can attain knowledge.

- (b) lacking absolute justification, but not some more limited warrant, for our beliefs (so that we may not know them with certainty), or
- (c) given some truth-criterion and absolute justification for our beliefs (so that we know them if and only if we are certain of them),

and how it is we are entitled to such beliefs.

The first possibility entails working wholly within the skeptical framework—that is, we do not dismiss the Skeptic's doubts about the plausible strength of our beliefs, even if we take them to be either justifiable or plausibly identified as true. The last possibility, called 'dogmatism' by the Skeptics, represents the utter rejection of systematic doubt and, correspondingly, the assertion that we have within our grasp the capacity to satisfy the primary conditions for knowledge with certainty. We can find this latter view in Aristotle and in Descartes, for whom the justification of our empirical beliefs is a special problem generated by maintaining an essentialist metaphysics in combination with faith in our ability, partly through experience, to know with certainty the ultimate structure of the world. For at least some empirical beliefs, they suggest, it is not only the case that we can be certain of them, but these beliefs also provide us with a sure grasp of the essential nature of things. The difficulty lies in providing the warrant for such beliefs. It was clear to Skeptics like Sextus Empiricus that Aristotle's theory of empirical knowledge failed in this regard, and Descartes took it to be at least one of his tasks to show the skeptics wrong for thinking such warrant unfindable.

By contrast, the second possible kind of response represents the view that, against the Skeptics (but also against the Aristotelian or Cartesian responses), our beliefs may be justified independent of whether the warrant for them is absolute, that is, entitles us to believe them with certainty. One instance of this kind of response, developed in the wake

of the Reformation, contests the way the Skeptics conceive of the problem of empirical knowledge, though the view is often thought of as a species of skepticism itself.³ This is 'constructive' or 'mitigated' skepticism, the view that ancient Skeptics were *right* to claim that our senses may not yield certain knowledge but were *wrong* to suggest that our knowledge of the world depends on its certainty and that knowing appearances cannot yield knowledge of the world. To the contrary, the constructive skeptic says the very character of sensory evidence such as we perceive it warrants our empirical judgements. To support this claim, they first divide the ancient Skeptic's quest for epistemic warrant into the search for justification for taking our beliefs to be certain on one hand, and reliable on the other. A traditional skeptic takes beliefs about which we are certain to be the only reliable kind and concludes that the search in either case is futile. By distinguishing between epistemic certainty and reliability, the constructive skeptic is able to agree with his/her traditional counterpart that we may not find warrant for certain knowledge and yet affirm, against the traditional skeptic, that we can attain reliable knowledge. Indeed, whereas ancient Skeptics doubt we can identify a fixed or stable criterion for identifying true empirical beliefs in individual instances, much less a means of appraising their reliability (across instances), constructive skeptics hold out the possibility of a feasible truth criterion and defend our perceptual grasp of the world as

³Though texts of the ancient Skeptics were generally lost or not read during the middle ages, interest in skepticism was revived in the West in various forms during the Renaissance. From works of al-Ghazali and Yehuda ha-Levi, we see the disappearance of Greek and Roman texts in Latin Europe did not mean an absence of skeptical thought among all medieval thinkers. See Richard Popkin, The History of Scepticism from Erasmus to Spinoza (Berkeley: University of California Press, 1979).

stable and regular enough for us to build, and gauge the merits of, our empirical understanding. This is not likely to satisfy adherents of that ancient brand of skepticism which says that both faculty- and belief-based truth-criteria fail because they rely on our fallible senses and that, since information about appearances depends greatly on personal and contextual factors, it can yield neither reliable nor consistent empirical claims (even though we rely on such appearances to record our individual sensations of material phenomena). Against such epistemological pessimism, the principal task of a constructive skeptic is to explain what it is about our sensory faculties, acts of sensation, and beliefs we base on those acts, that lends a discernable character of truth to such beliefs and renders them sufficiently reliable to build an abstract understanding of the world beyond the particulars of individual sensory perceptions.

This is where Gassendi comes in. In his Exercitationes Paradoxicæ (1624), he endorses the Pyrrhonian proposal that we cannot know with certainty about the world thus we are justified only in suspending our empirical judgements. Yet in the later Disquisitio Metaphysica (1644) and Syntagma Philosophicum (1658) he mitigates his skeptical view by differentiating between certain knowledge and probable knowledge, and claiming that each is attainable given trustworthy sensory evidence. Though all our beliefs about the world are subject to revision, we can have a dependable (and in some cases, even certain) understanding of sensible phenomena because the physical characteristics of perception guarantee that our sensory data approximate the apparent features of the world they represent, which in turn lends verisimilitude and reliability to beliefs we found on their basis. Further, we can identify the cognitive faculties which, by

their constitution, enable us to pick out our true and false empirical beliefs. While Gassendi maintains principled skeptical doubts about knowing the innermost structure of reality—he thinks we cannot know with certainty about the basic and constituent elements and relations of physical things—yet he rejects the anti-empiricist bent of classical Skepticism. This balance of doubts about certain knowledge of essences and arguments for cognitive truth-criteria and reliabilist warrant for empirical beliefs is the hallmark of his theory of knowledge.

B. Gassendi's Constructive Skepticism

We find Gassendi's mature response to skepticism and his developed theory of empirical knowledge principally in the Disquisitio and his vast and comprehensive Syntagma. Gassendi agrees with the ancient Skeptics that we can have neither reasoned knowledge with certainty nor knowledge of essences. Yet against those Skeptics he maintains that we can have credible beliefs about the world--and so knowledge of appearances (though not of essences)--because of the reliability of sensory data upon which we base such beliefs. We take appearances to reliably represent features of worldly objects and phenomena given that our senses provide us with what are in principle flawless bits of perceptual information in the form of corpuscular transmissions emitted from or reflected by those objects or phenomena.

Gassendi did not always hold that we have a reliable knowledge of appearances which yields a grasp of worldly phenomena. He moved to this position from an earlier, Pyrrhonian skepticism in the Exercitationes where he doubts we can have either truth-

criteria or belief-justification. Those doubts lead him, as Popkin notes, to reject even knowledge of appearances and to suspend judgement about the nature and reality of objects.⁴ In later works, Gassendi retains his doubts about knowing essences but claims we can know at least the reality of appearances. As he states in the Disquisitio,

...of a certain thing one may say that it is or appears in such a way, in the sense that it is endowed with such a mass, shape, movement, or position; and at the same time that it presents some analogy with respect to such and so organs [or] with respect to such and so animal or human being [or] with respect to such and so temperament or way of being, and other similar circumstances; but not that it [the thing] would be for that [its appearance] such [a thing] in itself or in its deep nature.⁵

That we can know appearances albeit with uncertainty was accepted by many ancient Skeptics as well; what marks Gassendi's view as distinct is the further claim that those appearances somehow refer to objects in the world and that they are 'true' appearances.⁶ He proposes, against the traditional Skeptical view, that we can form justified beliefs about 'actual', or at least intersubjectively agreed-upon, phenomenal qualities of objects based on their appearances, though we have no grounds for knowing their essences. For the Skeptics, we cannot assent to "the honey tastes bitter" because we cannot know if honey has an essentially bitter taste; yet they allow that we can assent to the relativized claim "the honey tastes bitter to me." Gassendi goes a step further, suggesting that we can assent to claims like "the honey has a contingently bitter taste" on

⁴Popkin, The History of Scepticism, 144-46.

⁵DM, Q III 286b; R 70.

⁶Below I examine what he takes to make appearances 'true' or 'truth-like'; cf. also DM, Q III 286b; R 70.

the grounds that our beliefs about appearances are at least in some cases acceptable on a broad intersubjective basis, in accordance with physiological norms of human perception. While we might not know the unchanging, essential qualities of objects, we do not *need* such knowledge to arrive at warranted beliefs about their inessential qualities. One result of this proposal, which I explore in the following chapters, is the suggestion that the goal of science is to explain apparent phenomena, and that this goal is satisfied by those accounts of material phenomena which are warranted by no more than experience. The pressing question in this context, though, is how Gassendi departs from earlier skepticist reservations and defends this robust knowledge of appearances.

In what follows I examine Gassendi's view that we can have true and justified beliefs about not only appearances but the actual qualities we take those appearances to represent. The central claims comprising this view are that we base our empirical knowledge--indeed, virtually all knowledge--primarily on information from the senses, which we attain both directly and through the medium of signs, and that we can identify truth-criteria and means of justifying empirical beliefs. *How* we have such justification is explained by Gassendi's account of knowledge acquisition--an account which I suggest should be counted among the first attempts at a reliabilist theory. The fact *that* such justification is possible altogether follows from his probabilist picture of empirical judgements. I conclude this picture of Gassendi's theory of empirical knowledge by indicating a special consequence for our beliefs about the ultimate nature of the world, that empirical belief cannot yield knowledge of essences.

1. We Base our Knowledge Primarily on Information from the Senses

We find one clue to Gassendi's views on the sensorial source of empirical knowledge in the role he assigns the senses in his model of mental operations. According to this model our cognitive faculties include the senses, the imagination, and the understanding, but it is the senses which provide our most reliable data about the world by passively receiving physical bits of information from the outside environment. By contrast, the other cognitive faculties are at least one step removed from the information retrieval process. The imagination, or *phantasia*, directs our will and contains images from imprinted 'species' or from sense-derived ideas combined in novel ways (e.g. the image of an Hippocentaurus),⁷ and the understanding, or *intellectus*, makes abstractions and generalizations and reflects on itself (its own past activity).⁸ Moreover, we form universal claims with the understanding, by induction on particular images of the imagination.⁹ Whereas the imagination and the intellect store and process the information which composes the content of our beliefs, the senses constitute the frontline in knowledge-acquisition, which suggests that Gassendi takes the foundational elements of our empirical claims to be bits of sensory information. Of course, this frontline status does not guarantee that the senses must be the true base of our empirical knowledge, for it still would be open to Gassendi to hold that we gain empirical beliefs indirectly through

⁷SP, Q II 409f.

⁸This requires powers of retention for, and contributes to our repository of, memory.

⁹SP, Q II 460f. The understanding also employs a correction mechanism, namely, the criterion of further sensory information, which I discuss below.

reason or directly through revelation.¹⁰ But in those cases he would have to admit, as he does not, that the acquisition role he assigns the senses is a waste of mental resources or that the senses function in a generally inadequate and unreliable fashion.

On the contrary, he argues that empirical knowledge is possible through the senses precisely because they are reliable. Moreover, he holds that we base nearly all knowledge on information from the senses, where the hard cases include knowledge of necessary truths like those of mathematics and logic,¹¹ and even knowledge of select religious truths including claims as to the existence of God. Thus practically the entire edifice of knowledge rests on the reliability of sensory information which, Gassendi claims, is largely independent of the cognitive contribution of reason and surpassed only by that of revelation. To establish that sensory information is reliable, his strategy is to argue that we cannot possibly doubt two kinds of foundational sensory experiences: first, sensations of the particular things which appear to us (*tout court*), and second, sensations of such things appearing to us with so-and-so particular qualities. These sorts of information about appearances we perceive are likely to fail, Gassendi proposes, only if our senses themselves fail.

It might seem that this strategy is ill-founded because these cases cannot definitively establish the reliability of sensory information; even if we cannot doubt these

¹⁰For example, among Gassendi's contemporaries, Descartes holds that reason provides the basic principles which guide our empirical beliefs, and Marin Mersenne suggests that reason is the ultimate source of our empirical beliefs. See in particular his *Vérité des Sciences*, and Susan James, "Certain and Less Certain Knowledge," *Proceedings of the Aristotelian Society* 87 (1987): 227-242.

¹¹DM, O III 398a; R 484.

fundamental sensations, there would be other kinds of sensations that should remain doubtable, for example, our sensations of the particular qualities of things.¹² Thus the classical Skeptic's cases of round towers that appear square to us at close range or straight oars that appear bent in water (Sextus Empiricus, Pyrrhoniae Hypotoses I. 36-163) are intended as examples of sensations we could, if not should, doubt. Yet Gassendi proposes that we can doubt neither the sensation of the towers and oars appearing to us nor the sensation of their appearance to us, respectively, as round or bent, and so the only thing we might doubt is that we may properly gather from those sensations that the towers are indeed round or that the oars are indeed bent.¹³ But that, he contends, is a matter of judgement made by the intellect, not by the senses. He further proposes that when we pull the oar out of the water and it appears straight, we not only cannot doubt our sensations of a quality of straightness (which in this instance is contingently associated with the oar), we also cannot doubt our sensations of the oar in particular being straight and we can be sure of this sensation: "And when it would be permitted to doubt all the rest, at least we cannot doubt that such things appear to us, and it is not possible that it not be quite true that they appear to us in such a way."¹⁴ The claim here is that we can form certain beliefs about appearances where the perceptual basis of those beliefs fixes

¹²There is another potential difficulty here which echoes a standard complaint against reliabilism in particular and externalism in general; see Lawrence BonJour, The Structure of Empirical Knowledge (Cambridge, Mass.: Harvard University Press, 1985). The senses could provide us with undoubtable information yet be unreliable. That is, we could be mistaken to rely upon such information simply because, for whatever contingent reasons, it was such that we happened to be incapable of doubting it.

¹³DM, O III 388a-b; R 532-34; B 266-68.

¹⁴DM, O III 388b; R 534-35; B 267.

particular qualities to particular objects. This is a bit puzzling, for without any other background information it seems we can only be sure of our sensations of the oar being straight in the same way (if not to the very same degree) we are sure of sensations of it being bent, or else we are more sure in one case or the other because something distinguishes the more reliable of our sensations, and in particular, relative to properties of the objects. Yet there is no suggestion, at least in this context, as to any such distinguishing mark of reliability. Another difficulty is that Gassendi fails to tell us what exactly makes these two types of sensations foundational. On the other hand, one merit of this strategy for addressing the Skeptic is the innovative attempt Gassendi proposes to license our empirical beliefs by appealing to the indubitability of our sensations of appearances *and* their accuracy (that is, resemblance to the truth).

Before we look at the details of this strategy, it bears stressing that such a response to the Skeptics is supposed to account for the viability of virtually *all* our knowledge, because Gassendi holds that what we typically take to be non-empirical knowledge is ultimately attained through the senses. Consider one of the hard cases, that we know the classic 'truths of reason'--truths of mathematics and logic--by evidence from the senses. He suggests we arrive at exemplars of such truths not by reason after all but by induction upon information we gain from the senses. For example, we standardly take it that we know the basic qualities of a triangle (e.g. that they have three sides, the internal angles of which add up to 180 degrees) with certainty and that this knowledge is derived from reason alone. Yet, according to Gassendi, every person learns about the universal qualities of triangles, such as their number of sides, through inductive reasoning about all

the triangles viewed in the past.¹⁵ Likewise, he proposes that the proof of a claim such as "the whole is greater than the sum of its parts" is by enumeration. Only after we have gathered information from our senses concerning a class of particulars can we attribute characteristics to members of that class on a general basis. Thus we come to know such 'truths of reason' by drawing universal claims from 'empirical' research regarding abstract items like triangles, numbers, and propositions. Though we tend to take those claims as certain, we should view them instead as tentative since they are no more secure than the results of our similar previous or future 'empirical' reports. One consequence of this account is the suggestion that we can designate no universal qualities of the abstract objects of mathematics and logic since that would entail our drawing a certain conclusion from an inductive argument.¹⁶

This view faces some apparent objections. One immediate difficulty is that truths of reason such as those concerning triangles are not dependent on past events; these truths should hold not only if, for example, triangles were never sensed, but even if triangles never existed. Thus Gassendi's view would seem strange if, by his suggestion that we know such truths about triangles because past reports of triangles uniformly describe them with three sides, we understood him to mean that those truths are dependent on

¹⁵DM, Q III 378b-79a; R 486-89; B 256-58.

¹⁶DM, Q III 375; R 470-73; B 246-47. Gassendi's view here strongly anticipates that of J. S. Mill. As for the second hard case--religious truths--Margaret Osler (in her *Divine Will*) has suggested that this might be demonstrated just in case we could show that truths we typically take as known through revelation are in fact known on the basis of induction upon information from the senses. This seems a fair enough suggestion yet Gassendi actually makes a special case of revealed knowledge, which he views as fundamentally non-empirical and instead known through Holy Scripture and the writ of the Church.

sightings of triangles past. This difficulty dissolves, though, if we take Gassendi as suggesting that our knowledge of triangle-truths depends on past observations, so that we come to know such truths only if we have prior experiences that gives us relevant knowledge about whichever triangles, past, present, or future. But then his point is an element of a descriptive educational psychology of mathematics, and not a normative characterization of the nature of these objects of knowledge.

Another difficulty is that Gassendi's account fails to acknowledge that some reasoned truths may be stipulated or conventional, and not discovered or learned. Descartes anticipates this difficulty in his Fifth Meditation, where he points out that we don't ever need to encounter a triangle to come up with its essential features. Whether or not we think geometric features are essential in any interesting way, it remains the case that if, before we discover particular instances of *x*-figures we stipulate that they are those figures with *y* sides, containing *y* angles adding up to *z* degrees, then we can pick out the conforming shapes, and our knowledge of what an *x*-figure is precedes our having seen one. It is not clear how we are supposed to reconcile any such *apriori* knowledge of a stipulative truth of reason with Gassendi's claim that we require some empirical input.¹⁷

An additional problem may follow from the feature of truths of reason that they

¹⁷Such stipulative or conventional truths of reason are one class of the claims standardly considered as 'analytic' yet for Gassendi, as Ralph Walker has noted, there aren't any analytic claims (save in theology and cosmology). This suggests that Gassendi need not worry that such truths might be known *apriori*, unless he anticipates the suggestion that there could be a synthetic *apriori*. Since he considers no such possibility, his views on this matter are at least consistent. They cannot be correct, however, for independent of whether there are genuine analytic claims (or whether there is a synthetic *apriori*), there are apparently claims such as the ones Descartes considers which can be known by reason without the benefit of experience.

often require our entertaining properties of a thing or class of things which are somehow necessary to defining such things--and which consequently have been thought of in some circles as 'essential' properties. Thus, that "prime numbers are divisible by themselves and 1 only" suggests that whatever would have such a property would be a prime number and any number without it would not be prime. For Gassendi, given that we know the putative truths of reason by evidence from the senses, we should be able to acquire knowledge of such necessary properties by empirical means. But for those who consider these as essential properties, there is an added problem: Gassendi holds that, whatever essential properties there may be, we have no epistemic access to them. If these are construed as essential properties, then--particularly with respect to abstract items--how could we come to know about them through the senses? Further, if the senses cannot bring us such knowledge, what else could? Gassendi can well deny that these properties in particular are, after all, essential, but he does not deny that there are essential properties altogether. He is most comfortable talking about such properties in the context of theological concerns--and here he falls back on the suggestion that we know about such properties, if at all, through revelation and not experience. On the other hand, he never rules out the possibility that there are essential features to material or abstract objects; he simply suggests we have no epistemic access to them.

If these are indeed instances of claims we come to know *apriori*, Gassendi faces some difficulties in subsuming the traditional candidates for truths of reason under

empirical belief.¹⁸ But of more immediate concern than this issue (or the difficulties he faces in that regard) are the mechanics of his account of knowledge through the senses. How does perception result in knowledge acquisition? Given the breadth of our empirical knowledge what enables us to form perception-based empirical beliefs though we perceive only a limited portion of the world that is the subject of such beliefs? Gassendi offers a physicalist account of how we gain knowledge through the senses, where the information that forms the contents of our beliefs bears a physical form, and the cognitive processes of receiving and storing (and, perhaps, manipulating) information turn out to be physical processes. Such a physicalist account can only cover part of his knowledge acquisition story, though, because he also maintains that we come to warranted beliefs about the world by taking our actual perceptions to have surplus significance, beyond the representation of features of objects they directly and physically report. This last claim follows from his theory of signs, according to which we are able to attain empirical beliefs on the basis of perceptions which provide us with information about objects or phenomena we do not actually perceive.

To begin with, let's consider what he takes to be the primary element of his physicalist accounts of perception and empirical belief formation: the story of visual perception and our concomitant beliefs. The central claims here are that (1) we can tell a corpuscularian story about the physics of light and images and about the physiology of

¹⁸There are, naturally, other difficulties which arise in this regard though they are not immediately pertinent to the sort of cases in geometry and arithmetic which Gassendi discusses. The greater the sophistication of the mathematical endeavor, the greater one must strain to suggest that the properties of the objects in question might be known empirically.

light and image perception, and (2) ideas are perceptual images, so that all that it means to get an idea is to physically perceive an image. If sensory-based ideas are understood as made of physical stuff and as the primary elements of our empirical beliefs, then this account of perception is also a physicalist account of at least a first step in empirical belief formation.¹⁹

Our first move, then, is to see how Gassendi tells a corpuscularian story about the physics of light and images, and about the physiology of perception. It is appropriate to begin with his account of how we see light because he holds that what we see generally is a function of how light is reflected off or transmitted by distant bodies. He takes light as composed of small, corpuscular bodies which travel from those distant bodies to our eyes, where they hit the back of our retinas and are transformed into sensory data about relevant worldly phenomena such as the character or behavior of the bodies that either emit or reflect the light--or the light itself, e.g. its color or intensity. We are entitled to this account, of course, only if we have a corpuscular account of light, which should follow

¹⁹One way this view could be wrong is if there's more to the initial steps of belief-formation than simply the reception of ideas. Along these lines, the Michaels claim that since Gassendi holds that 'idea' means no more than 'image', even though he may think our sense-derived images are our ideas, this does not commit him to the view that the ultimate source of our beliefs is in all cases sensory information; see Emily Michael and Fred S. Michael, "Gassendi on sensation and reflection: a non-cartesian dualism," *History of European Ideas* 9 (1988): 583-95. They suggest that in Gassendi's mental model the images or ideas we collect through perception are merely stored by the *phantasia* until we form beliefs through a process of reflective judgement conducted by the higher-order and *non-physical* cognitive faculty of the understanding.

There is no question that he considers judgement to be the special province of the immaterial understanding, and so whatever claims or beliefs are subject to 'judgement' in his sense should be reasonably taken to be immaterial, too. But, I submit, it is also clear that he views ideas from perception as providing the content of whatever claims or beliefs we arrive at concerning qualities we perceive and the objects that bear those qualities (more on this below). This suggests that, whatever else we can say about the role of the understanding in Gassendi's mental model, the reception of ideas truly is the first step requisite for the formation of empirical beliefs.

from a corpuscularian matter theory just in case we take light to be matter—a natural move for Gassendi to make. In addition to simply stating this view, though, he offers a thought experiment in its defense. A ray of light passes through a dark room, where the passage entails that the ray enters through a hole on one side and exits out a hole on the other side. Although the light passes through the room, it cannot be seen from the perspective of somebody actually in the room but outside the ray's path. This suggests to Gassendi that all there is to the ray is a set of light corpuscles travelling in a straight line from one hole to the other, such that, lacking physical contact with the observer in the room, the light passes directly through, undisturbed and also unseen. On the other hand, if light was not composed of material corpuscles, it might well diffuse (hence be seen) though no obstacles are present in its path.²⁰ One clever aspect of this thought experiment is the proposal, well before the very idea of lasers, that we could cast light in rays with no divergent streams. Yet the experiment is flawed, for the observer would see the light ray passing through the room because the ray must be diffused since it is not passing through a vacuum.²¹

Flawed or not, the corpuscular accounts of light transmission and perception provide Gassendi with the basis of a physical account of perception generally, because he takes it that all material objects and not just bodies of light are capable of emitting or

²⁰SP, Q II 340a. See Bloch, *La philosophie de Gassendi*, 10.

²¹Further, even if light is corpuscular and we force it into a ray there is in principle nothing to prevent it from subsequently falling apart into distinct corpuscular bodies, the likes of which we could observe (by Gassendi's own reasoning). As we see in chapter three, Gassendi's particular corpuscularian hypothesis helps him out in this regard; he stipulates that corpuscles can be held together by hook-like protrusions.

reflecting light corpuscles which bear images somehow representative of the emitting or reflecting objects. This is, as Bloch has suggested, a materialist version of the Scholastic thesis that what we perceive are 'simulacra' or immaterial 'intentional species' of worldly objects.²² By taking the unit of perception to be an image composed of light corpuscles arranged in particular ways and transmitted by or reflected off the objects of perception, Gassendi lays the groundwork for the view that perception entails receiving those material images with our sensory organs and fashioning them into images suitable for scanning by whatever counts in our minds as internal image readers. Indeed, he develops a perceptual theory along these lines, on the basis of conclusions he draws from a series of optics-related observations he conducted together with his longtime patron Fabri de Peiresc in 1634, towards an understanding of the eye's structure and function.²³ These observations, largely an exercise in speculative morphology, consisted in dissection of eyes from animals of a great many species, including birds, fish, and 'quadrupeds'. The two experimentalists concluded from their data that visual perception amounts to reception of information about the external world in the form of physically-constituted

²²Bloch, *La philosophie de Gassendi*. Bloch points out that Gassendi takes material species to be objects of visual cognitive acts (that is, what we perceive), in contrast to the scholastic view of their intentional 'species' as the cognitive but immaterial objects of perception. Gassendi views these intentional species as the *contents* of visual cognitions. Given the scholastic view, vision turns out to be an act of mentally intuiting the immaterial cognitive contents of sensation. On the other hand, given Gassendi's view that the species are material representations through which we gain a mental image of the species-emitting object, vision turns out to be a physical process.

²³Brundell (1986, 89) suggests that Peiresc and Gassendi primarily set out to judge Kepler's proposal that an image appears on the retina; cf. Peiresc's letter to Dupuy of September 5, 1634 in Philippe Tamizey de Larroque, ed., *Lettres de Peiresc* (Paris: Imprimerie Nationale, 1893), volume III, 97-101. But in his Peiresc biography Gassendi describes these experiments as motivated by more wide-ranging naturalist pursuits.

images transmitted by external objects and collected at the retina—in keeping with Gassendi's proposal that we perceive the information borne by light corpuscles when the rays they constitute strike the back of our eyes.²⁴

Specifically, Peiresc and Gassendi suggest rays of light corpuscles carry image-bearing information to our optical apparatuses in the form of light corpuscles arranged in particular ways, or what they call 'species', after the medieval unit of perceptual data. The rays of light carry these material species across the cristalline humour, where the rays are reflected against the retina and thus focused against the external (back) side of the vitreous humour—where this information is turned into mental data. The convex cristalline humour in the front of the eye turns images carried by these rays upside down and the concave retina reverses them, so that when they hit the back of the vitreous humour we are able to visualize them, and thus the world, the right-side up.²⁵ Thus, Gassendi suggests, it must be that vision occurs *after* the rays pass through the center of the retina so that our mental data accurately reflects the way the world is.²⁶

²⁴Gassendi saw this proposal as a challenge to the ancient view that vision occurs in the cristalline humour and to the contemporary view that it occurs in the retinal membrane; he and Peiresc proposed instead that it occurs in the rear of the vitreous humour.

²⁵They concluded from observations of eye shapes in a wide variety of specimens that the back of the eye is always concave and therefore capable of righting an inverse image. Such an image would not be righted unless the back of the eye was also mirror-like, or else the inverse image might just diffuse straight through the retina. This mirror they claimed to have found in the form of a metallic-like buff on the surface of the choroid membrane.

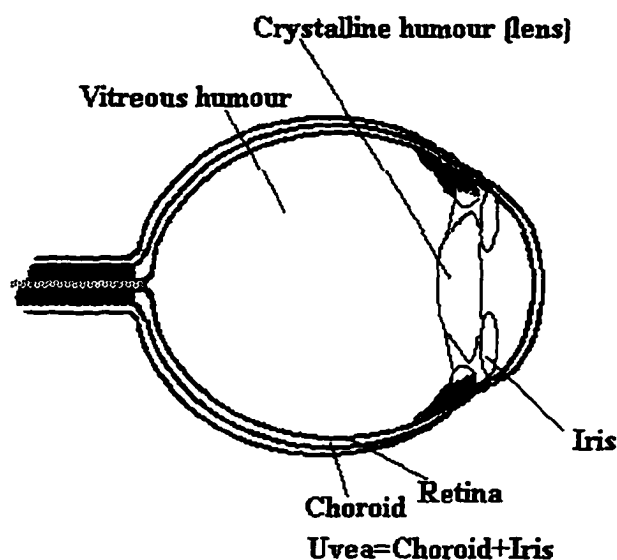
²⁶Gassendi writes that "...the faculty in question must reside in the center of this concavity, [the point] from which vision could gaze upon the reflected and righted image leaving the retina, and thus the object in its natural position." (VP) While in his biography of his patron Gassendi attributes this optical picture solely to Peiresc (with reservations), the correspondance shows that the experiments and conclusions drawn were more or less the result of a mutual effort.

Thus, in a letter to Elia Diodati of August 29, 1634, Gassendi writes: "I will content

It cannot be too surprising that these gentlemen-scientists of Provence dissected a host of eyes and claimed to find the right apparati for turning the material species into an image our minds can retain, when by hypothesis they assure us that the images we perceive and our sensory and image-retaining organs share a physical character. The

myself to tell you that generally we have discovered that the concavity of the eye, that is to say that which embraces the crystalline, aqueous, and vitreous humours, is a true concave mirror and, that alone representing the objects turned upside down, represents them in their natural position after having been turned upside down by the crystalline humour. Now that which is principally the effect of the mirror is the membrane we call *choroid*, or the posterior part of the uvea, which is commonly colored by the color that appears in the iris of the eye and a metallic polish having at its back a black sediment in the manner of a lead that one puts behind a mirror. And being moreover the rear concavity of the eye--apart from the place of the pupil, shaded of a similar blackness for making that the least glimmer that enters in the capacity of the eye, is rendered there more brightly--this choroid is all covered by the cloth or membrane that we call *retina*--which is such that, being conserved together with the choroid, seems one and the same in color, lustre, and representation; indeed it [the retina] represents the things all the better because its surface is rendered alive by the moistening it receives from the vitreous humour which is more immediate to it than to the choroid. Thus it appears close to the truth of the matter that vision is made in the retina because, moreover, proceeding or being formed by, and being only a production and dilation of, the interior and soft part of the optic nerve, it [the retina] receives, or if you like forms, in itself the eyesight, the expression of the animated image of the thing which to it is opposed." (*Mersenne* IV 335-341)

If their visual story was somewhat off, at least their physiology of the eye was reasonably close to the mark, as the following diagram illustrates:



striking aspect of this physicalist account is rather that Gassendi (and Peiresc) unabashedly claim to begin with that what we perceive—the unit of perception—is a physically-constituted image somehow borne by light corpuscles which are in turn carried by rays transmitted from or reflected by whatever object we profess to perceive. Gassendi even reports experimental evidence for the representative nature of such images and their preservation as representations through the optical process:

...Peiresc prodigiously exulted when, after extracting all the humours, and the cristalline having been next recovered and reestablished nearly in its place, he saw the image of the candle painted on the interior, on the retina, not in a reversed position but correctly; and otherwise, when the background was lit in a way that the cristalline could only receive light from it, it was established that the image, reversed through the retina, was righted on the cristalline that received it.²⁷

We cannot place much stock in these experimental reports, especially since he frequently gets the facts wrong—for example, images which appear on the retina should be reversed. But this does not stop Gassendi from attempting to develop his account in order to provide a wide range of explanations of visual distortions and so address topics as diverse as microscopic and telescopic observations on one hand, and skeptical tropes regarding perceptual relativity on the other.²⁸ The central point of his optics account, though, is that perception consists in the physiological reception of physically-constituted information that represents to us the external world, and the representation of which is preserved in

²⁷VP 226. Here Gassendi spells out a couple of differences between himself and Peiresc relative to interpreting these observations: the former locates the faculty of vision in the complex of optical membranes including the retina and holds that this faculty consists of the interior perception of images as transmitted from the external objects; the latter locates vision in the retina alone, and suggests it is the perception of external objects *per se*.

²⁸In my review of Gassendi's account of epistemic warrant, I look at one such case—the tower appearing square or round, relative to one's distance from it.

our cognitive-reception process.

From this retinal image story it seems Gassendi takes material species to be straightforwardly representational. But later on in the Syntagma, he writes:

The species is not a picture...and is not itself looked at. It is simply the ground for knowing the object by which the species is impressed. In the same way the species impressed on the eye is not what is seen but is simply the ground for seeing the thing which emitted it.²⁹

One way to understand this passage is as a bit of a turn-around on Gassendi's part, taking the species as a non-representational stimulus which provokes the imagination into having the right image for the right external object, without the species itself presenting any image. A difficulty with this suggestion, quite apart from its interpretive plausibility, is that we might then wonder how he could think, in the absence of any transmitted images, our mental images are mapped onto the right external objects. There might be any number of plausible proposals to make here but Gassendi offers none, raising at least the possibility that such a non-representational view is not quite the perspective he intends. Another possible reading, in any case, makes better sense of earlier claims about the representative character of species. In the passage just quoted Gassendi rules out not that material species have a representational character altogether but that they present us with pictures *per se*, presumably of external objects. It is possible to read him here as allowing that species present us with semblances of the *qualities* of external objects. This suggestion accommodates his denying that material species are pictures yet captures his earlier proposal that we perceive likenesses of external objects: we perceive an object's

²⁹SP, Q II 405b (trans. Brundell). The first phrase Brundell translates as 'ground' is *ratio*; the second is *solum ratio*.

apparent qualities when species impress qualitative semblances upon our senses. Further, this reading permits us to retain the representationalism we earlier attributed to his account of perception. Thus, we arrive at perceptual beliefs through our reception of the 'images' of material species which bear the qualities of their originating objects, and our consequent grasp of the likeness of those objects from the qualities so transmitted.³⁰

One caveat regarding such an account is raised by Gassendi himself, in the form of a qualia problem. Even if we know how light excites the retina, he points out, we will not know the relationship between such stimulation and our sensing particular phenomenal qualities such as colors, because we will still lack an account that ties individual sensory qualia to the mechanical operations of vision.³¹ The idea is that, since we don't receive sensations of the properties of objects directly—rather we sense their representational corpuscular emissions in the form of material species—we receive in perception representations of qualities which are *somehow* translated into qualia we associate with particular objects or their properties. Exactly how such a translation could occur is a mystery to Gassendi and, a different physicalist story notwithstanding, remains largely obscure by the lights of at least some contemporary philosophers of mind.³²

³⁰At any rate, there are two reasons to take the passage at II 405b as something of an afterthought on Gassendi's part. For one, he simply asserts here without argument that species are not pictures. Further, as Brundell (*Pierre Gassendi*, 96) points out, Aquinas suggests that the species are no more than stimuli for provoking the right images (*Summa Theologiae* Ia q. 85 art. 2) hence Gassendi could have been motivated by doctrinal concerns alone to modify his view of species.

³¹*DA*, Q III 472a. See Bloch, *La philosophie de Gassendi*, 20, n. 54; Carpentras ms. 1832, folio 244 v^o.

³²For a contemporary discussion, see, for example, Frank Jackson, "Epiphenomenal Qualia," *Philosophical Quarterly* 32 (1982): 127-36.

Though Gassendi appears to recognize the profundity of this problem, he never admits that it is a potentially principled obstacle to his perceptual theory, and takes it as a technical difficulty to be resolved, perhaps at some later date.

The second important point here for Gassendi's account of perceptual belief, that getting an idea consists in getting an image, is mentioned in the Institutio Logica³³ and spelled out in the Disquisitio. Again, this is an important point because, given Gassendi's view that perception is the reception of images of the external world and the proposal that ideas are just images, then--barring any other primary source of empirical images--it follows that perception is the primary source of our empirical ideas. In the Disquisitio, he offers exactly this proposal in reply to what he takes (mistakenly) to be the cartesian view that whatever is an idea is a mental image. Gassendi argues that, in addition, whatever is a mental image is an idea. Moreover, he rejects Descartes's attributing to him the view that ideas are only images in the imagination, and insists rather that ideas are also images in the understanding, on the grounds that whatever ideas are in the understanding must be images.³⁴ Thus Gassendi believes himself and Descartes to agree on the claim that the objects of thought (our ideas) are mental images. An untidy consequence for Descartes, Gassendi maintains, is that this claim commits us to the material origin of at least some mental objects, a view that Descartes strenuously rejects.

³³IL 83-84.

³⁴To claim that ideas in the imagination are images, Gassendi points out, does not preclude us from considering those or any other ideas that the understanding processes as images, too. Gassendi proposes that (i) all images, from the imagination or not, are mental items and thus objects of thought, so whatever images not from the imagination are still ideas (and so all images are ideas); and (ii) all ideas, from the imagination or not, are images present to the mind (thus all ideas are images). DM, Q III 333b-34a; R 282-85.

Gassendi comes to this view by embracing Descartes's view that whatever we form by thought--namely, ideas--are objects of thought, and what he takes to be Descartes's view, that such objects are given to thinking minds *qua* images, so that whatever ideas are given to minds just are images.³⁵ Yet for Descartes this should be problematic, Gassendi contends, for if ideas given to the mind are images, and at least one subset of these images are material (because we form them as a result of perceiving material species), then any such idea or object of thought must be represented to the mind as a corporeal thing. But Descartes thinks all mental images are incorporeal.³⁶

Gassendi does not face this putative problem because he does not think all our mental images are incorporeal: his perceptual theory suggests that we gain corporeal, imagistic objects of thought from sensory input. But he faces a different kind of problem than the one he assigns to Descartes: how is it that the understanding, which he takes to

³⁵Gassendi writes: "Now I ask you, that which we form by thought, whatever it be, isn't it the object of thought? And the object in question, isn't it put together [juxtaposed] with the thinking mind--and as given to it? And that, isn't it presented and given under the aspect of some image which, if it isn't fantasied [emerging first from the fantasy], is at least mental? And would not such an image be an Idea? And reciprocally, would not the Idea thus obtained be an image present to the mind? And yourself, as I have noted, haven't you said that only the things thought as images are properly spoken of as ideas? If there are then among all the images some of them that I have not admitted to the set of ideas; if there are those that I have reduced to the imaginative fantasy; if there are those that I have not extended to the entire mind, or to all that we form by thought, then give the proof of it!" (DM, Q III 333b-34a; R 284.)

³⁶Gassendi further contends that Descartes is aware of this supposed difficulty yet insists anyway that we don't depend on material things (such as bodily sensations) for knowledge of immaterial things (notably, what Gassendi thinks of as cartesian mental images). Accordingly, Descartes is not entitled to 'representative' mental images of immaterial things (say, angels), because the existence of such images would suggest that knowledge of those things comes from perceptual acquaintance with something material; see DM, Q III 333b-34a; R 284. Now, Descartes does not think that all ideas are mental images, nor could he, for more or less the reasons Gassendi outlines. It is hard to see why Gassendi should have misinterpreted Descartes's position, but perhaps he simply confused the latter's notion of idea-production (through pure thought) with his own notion of idea-production (through the gathering of images from experience).

be incorporeal, can process corporeal images? Naturally, it is open for him to suggest that we *also* form images in the understanding, which would perforce be incorporeal, but that wouldn't explain how the corporeal images are handled. At any rate, it is *not* open for Gassendi to suggest that the understanding is the primary place for forming images with empirical content, because the perceptual theory says we introduce those images to our cognitive apparatus through sensory contact with the external world. To sum up, when we put together this perceptual basis of our empirical ideas with their physical origin and character, Gassendi's view of the origination of the mental images or ideas that are the objects of our empirical beliefs turns out to be a thoroughly physicalist account.

2. The Theory of Signs: Inferences Beyond our Sense-Experiences

The account of knowledge through the senses presented thusfar is supposed by Gassendi to explain how we attain ideas or beliefs about what is presented to us directly--the perceptually 'given'. He further proposes that we attain beliefs about the external world through the senses *indirectly* with the aid of inferential tools he calls 'signs', or that "which, once it is known, leads us to the knowledge of something else."³⁷ That we employ such tools of inference is an ancient view recommended by Aristotle and the Stoics and rejected by, among others, Sextus Empiricus. Against the Skeptical view, Gassendi claims information about what we perceive may carry surplus significance regarding what we do not perceive, just in case we are aware of special conditions under which we perceive some phenomena where those conditions justify further beliefs about

³⁷B 330.

some other phenomena not presented to us directly. So what is the nature of those special conditions?

Following Sextus Empiricus, Gassendi divides phenomena into the manifest (evident) and the hidden (non-evident), where the hidden are either totally (in-principle), temporarily, or naturally (contingently but indefinitely) non-evident.³⁸ We can infer justified claims about the temporarily and naturally non-evident from the evidence of perceivable phenomena we take as signs that indicate the character of the external world beyond appearances. Gassendi's primary justification for employing this kind of inferential tool, broadly speaking, is that nothing else accounts for the viability of a wide range of judgements we regularly make about that world of the non-evident. The idea is that, since sensory information is the source of the constitutive ideas of empirical judgements, the intellect can only render judgements about hidden things we do not directly perceive if it can interpret something we can perceive as evidence of the hidden.³⁹

There are two plausible ways the perceived constitutes evidence of the unperceived, Gassendi says, adopting the classification of the Stoics and Sextus Empiricus. A *commemorative* sign is evidence of the temporarily non-evident in the sense that it "...had always been observed to be connected with a thing which we could see clearly, so that as soon as we perceive it at a later date we are reminded of the thing which must accompany it." Thus smoke is a commemorative sign of fire, breast lactation

³⁸B 289-90.

³⁹B 333.

of pregnancy, and scars of wounds. In each case we judge from the evidence that the latter exist though we perceive only the former because we infer as much from our prior grouping of cotermporal though distinct phenomena. By contrast, an *indicative* sign is evidence of the naturally non-evident in the sense that "...it is of such a nature that it could not exist unless the thing exists, and therefore whenever it exists, the thing also exists."⁴⁰ Thus motion is an indicative sign of the void, sweat of pores, vital action of the soul, and the universe's wondrous qualities of God the creator.⁴¹ In each case we infer the presence of something we cannot see and perhaps never will, but which the need of a plausible explanation requires us to posit. If we could not refer to the signified thing, we should have no better way to explain the appearance of the indicative sign-phenomena. For example, Gassendi proposes, before the age of microscopy there was no better account of how sweat appears on the skin's outer surface than postulating pores in the skin. Hence it is a matter of an explanation-guiding rule--if you cannot best explain *x* without *y*, posit *y*--not any additional empirical information, that we judge an unperceived thing exists just in case we perceive something else we take to be an indicative sign.⁴²

⁴⁰B 332.

⁴¹B 332-33. Both the pores and soul examples first appear in the Stoic theory of signs.

⁴²Gassendi recalls the suggestions in the *Prior Analytics* and from later Stoic writings that signs be classified as either necessary ('*tekmeria*') or probable ('*semeia*'). In the first case, as these perceived and unperceived phenomena are conjoined without known exception, we may take the former as indubitable evidence of the latter; in the second case, as these perceived and unperceived phenomena are typically conjoined, we may take the former as reasonable or weighty, but not indubitable, evidence of the latter (B 331). According to the distinction between commemorative and indicative signs in *De Fine Logica* (in the *Logic* of the *Syntagma*), we might think Gassendi sees commemorative signs as *tekmeria* and indicative signs as *semeia*. This is plausible enough but (i) Gassendi does not himself draw this parallel, and (ii) he gives us no reason to suggest that inferences from commemorative signs have the force of necessity whereas those from indicative

One of the reasons we should accept the evidence of indicative signs, Gassendi suggests, is that advances in telescropy and microscopy have proven worthy various instances of reasoning from such signs. So for example, we might have taken tiny insects such as mites to have legs before we could actually see them, from the indicative sign of their motion, and now such reasoning would be vindicated by microscopic enhancement of the perceivable realm. Democritus took the 'filmy white' nature of the distant light in the night sky as an indicative sign that the sky contains innumerable many stars whose light forms an undifferentiated blur when seen from a great distance, and his reasoning from an indicative sign is vindicated by astronomical sightings through the telescope.⁴³ Of course, these inferences could easily have gone awry: we might have taken mites to have wheels, and Democritus could have thought there was just one big, gooey star in the sky. Moreover, for every vindicated inference from indicative signs, there are surely many cases of proven failure. But what Gassendi seems to be proposing is that the success of those several instances means we have no grounds for ruling out indicative signs generally as a means of suggesting what lies beyond experience.⁴⁴

signs do not. If anything, he seems to suggest quite the opposite; cf. section three.

⁴³B 334-35.

⁴⁴If the success rate was important here, as Gassendi seems to think, we might still cast aspersions upon reasoning from indicative signs if it turned out that the pattern of vindication was insignificant or random. In chapter two, I explore his aspirations for exploiting inference from indicative signs in the sciences. Here I briefly note some of his attempts to identify viable instances of such inference:

i. *the void*. Following Epicurus, Gassendi suggests we can infer that the void exists between particles of matter from the motion of those particles. Without such a void, there could be no motion of matter, since matter in any voidless world would constitute a plenum where everything is locked into its original position, as a "single totally rigid mass, utterly inflexible...with all its places occupied in every direction, so there would be nothing that could

But Gassendi derives his principal defense of sign-based inference from his view that accounts of at least some basic epistemic notions (also including a viable notion of truth) are satisfied by showing that such notions fulfill two conditions, that they are satisfactory to reasonable persons and--what he apparently takes to be an equivalent condition--undeniable on pain of contradiction.⁴⁵ His primary interest lies in the second condition. Thus, for example, he asks us to consider the particular inference of the existence of pores in the skin from the sign of sweat. It is evident from such signs that there must be pores, he proposes, for if there were not, we would be lead into a contradiction, namely, "...two bodies would have to be in the same place at the same time." When we reject this as a contradiction, we end up with a claim--that there are

yield its place into which something else could succeed it." Thus motion is an indicative sign of the void. (B 338)

ii. *astronomy*. (a) We take the changes of the lunar quarters as a sign of the spherical shape of the moon: without such a shape the moon would not reflect the light of the sun in its quarterly pattern, and given that it has such a shape, that pattern must result. (b) We infer from lunar states at times of eclipse that eclipses are caused by the positioning of heavenly bodies in the path of the sun's light. The only lunar eclipses occur at full moon, when Earth blocks the light of the sun projected in the direction of the moon, and the only solar eclipses occur in a new moon, when the moon blocks the light of the sun projected in the direction of Earth. (c) We infer from the appearance of Venus (as a horn and larger in retrograde motion, and as full and smaller in forward motion) that Venus is either beyond the sun or between the Earth and the sun. (d) Copernicus inferred that the Earth moves from the apparent growth of Mars when moving away from the sun and its apparent shrinking when moving towards the sun, on the grounds that we could only see Mars as larger in this case if we were approaching it, and as smaller in this case if we were moving away from it (namely, on the opposite side of the sun). (B 339)

iii. *geometry*. The truths of geometry are not self-evident in the sense that we require a proof before we assent to such claims as "the volume of a cone is one-third that of the cylinder," and we prove their truth by inferences from axioms or "...the definitions by which a point is said to have no parts, a line is length without width, a surface is width without depth"--which Gassendi somehow construes as signs. (B 339-40) It is not clear, however, what we might take them to be signs of.

⁴⁵In this he rejects both demonstration of the account's viability along Aristotelian lines (e.g. utilizing definition by division or syllogism) as well as foundations of such accounts in the distinct accounts we develop for related epistemic notions.

pores--the truth of which we can be certain. The first condition is sufficiently less interesting to him that he does not even bother to spell out how it is satisfied here. Yet we might suppose that he intends something like this: we need to take such phenomena as sweat appearing on skin as signs which permit such inferences in order to generally explain the assuredness of reasonable persons about the non-evident on the basis of ideas about the evident. According to this suggestion, we accept such sign-based inferences, not only because they allow us to avoid unpalatable claims, but also because they thereby conform with the thinking of persons who reason well.⁴⁶

At any rate, the second condition is not without problems, either. One problem is that it is physically absurd though not a logical contradiction to believe that two bodies are in the same place at the same time.⁴⁷ Why does Gassendi think descriptions of physically impossible events are contradictory? In the Logic of the Syntagma he elevates certain principles of physical science to the status of necessary or indubitable, so perhaps he mistakenly concludes from this that whatever contradicts the indubitable is in itself contradictory. Whatever the reason, though, it seems this is just a slip on his part, and we might better understand the motivation for accepting indicative signs and their corresponding inferences to empirical claims as the need to avoid physically absurd claims.

Ralph Walker proposes another, rather distinct difficulty with Gassendi's defense

⁴⁶B 346-47. Note a hint of reliabilism here: we have warrant for our beliefs if we employ such inferences as reasonable persons accept. What else accounts for the proposed warrant except the regularity of successful inferential performances by reasonable persons?

⁴⁷Thanks to Mark Kulstad for this point.

of sign-based inference. He understands Gassendi's view as the proposal that such inferences are warranted if *self-evident*--perhaps because Gassendi does not entertain a foundationalist defense of sign-based inference. Walker argues that he trips up by proposing self-evidence as the grounds for any beliefs we so infer, since the structure of his own argument against the cartesian grounds for epistemic warrant can easily be turned against justification by self-evidence. Gassendi, let us recall, criticizes Descartes for arguing that the warrant for our knowledge is clarity and distinctness, on premises we can only claim to know because they are clear and distinct. Similarly, Walker proposes, any argument for the claim that our inferences are permissible if and only if self-evident will rest on premises we accept only because they are self-evident.⁴⁸ Moreover, whereas Gassendi says in the Logic that the intellect or understanding cannot reject such self-evident premises,⁴⁹ in the Disquisitio he states that we have no reason to place absolute trust in intellectual judgements.⁵⁰ It is no doubt true that there are many problems with defending the viability of beliefs by their self-evidence--though the problems outlined here are not necessarily the worst ones there are (if indeed they represent problems at all⁵¹). In any case, I think Walker misconstrues the guideline Gassendi actually urges for

⁴⁸Ralph Walker, "Gassendi and Skepticism," in The Skeptical Tradition, ed. Miles Burnyeat (Berkeley: University of California Press, 1983), 328.

⁴⁹SP, Q I 85b; B 347.

⁵⁰DM, Q III 280a; B 167.

⁵¹It is not at all obvious, for example, that we could not defend the self-evidence standard on self-evident premises, since the nature of the standard for admitting these inferences need not have anything to do with the nature of the premises in question--unless the argument for that standard relied on inferences of just that kind. But in that case there would be a circularity difficulty whether the premises were self-evident or not.

proper inference from indicative signs, namely, that we licence such inferences when alternative accounts of the sign-phenomena constitute or generate physically impossible claims. For Gassendi, this guideline lends the kinds of inferences we can make the warrant of a reason-derived rule, quite apart from any self-evidentiary nature they may feature.

Indeed, Walker recognizes the role of reason in Gassendi's defense of sign-based inference, and accordingly accuses him of trying to license, against better empiricist principles, inferences that go beyond the perceptually given.⁵² It is problematic enough that a strict empiricist like Gassendi proposes an appeal to experience to show how to get to beliefs based on inferences *beyond* experience.⁵³ Worse still, his proposed means of justifying sign-based inference entails that the understanding accepts such inferences (on pain of contradiction) such that the warrant for beliefs so inferred appeals to something outside experience altogether, namely, the reasoned judgement of the immaterial understanding--which itself is opaque to empirical study. Walker contrasts Gassendi's project with that of Quine, who says we should not bother trying to find *apriori* grounds for justifying beliefs about inferences beyond experience. Rather, we should turn to a naturalized epistemology in which we describe the ways we actually and normally come to maintain such beliefs. If by the lights of this approach we can still talk of right and wrong inferences, the mark of aptness lies in their empirically testable consequences. The naturalizing epistemologist answers the skeptic's demand for justificatory grounds by

⁵²Walker, "Gassendi and Skepticism," 329.

⁵³Ibid, 331.

providing descriptive, non-normative accounts, for example, that some x actually knows that p on the grounds that some y knows that p and y told x 'p' and x believes y , or x read 'p' in a book, and so forth.

In brief, then, Walker claims Gassendi's empiricism alone rules out his appeal to our right reasoning as warrant for our empirical beliefs because the relevant reasons (and background cognitive account) are at root not empirical. However, Gassendi has an empiricist story to tell about how such guarantees are provided: when he says we justify sign-based inferences just in case they are, among other things, "obvious to all rational men,"⁵⁴ he appeals to an empirical claim or sorts, to wit, that those inferences are justified in the sense that they are evident (because indubitable?) to a plurality of rational thinkers. We might even say this is the kind of proposal plausibly made by a naturalizing epistemologist. Indeed, Gassendi anticipates another part of the naturalized epistemological programme in the sense that he thinks his defense of sign-based inference by appeal to the judgement of the intellect yields *aposteriori* knowledge of the warrant for beliefs we so infer. We locate this warrant (as well as viable truth-criteria, he thinks) through the detection of our cognitive capacities, and not by reflection on the nature of justificatory norms, as for example Descartes pursues with respect to clarity and distinctness. This is, admittedly, hardly the stuff of mature empirical psychology, the ostensible route of the contemporary naturalizing epistemologist. But it is a crude, semi-speculative kind of cognitive psychology, crucially supported in this case by Gassendi's contention that the acceptable kinds of sign-based inferential beliefs are the ones that are

⁵⁴Ibid, 333.

not doubtable under normal circumstances by the average rational person. Thus there is no inconsistency in Gassendi's empiricist proposal that there is no *apriori* knowledge (outside of revealed truths) and the suggestion that we know (or judge) that the justification of some beliefs may consist in so-and-so reasons determined by the understanding, in accordance with so-and-so 'facts' about cognition. In this respect Gassendi approaches the position Walker attributes to Quine, that we can defend beliefs about inferences beyond experience through appeal to experience just in case the defense is descriptive in nature.

Finally, the objection to the indicative signs account with which Gassendi himself is most concerned is the skeptical charge that there are no criteria by which we could recognize phenomena as signs yielding justified veridical judgements about the non-evident. Sextus, for one, argues that we cannot even hope to discover a criterion for judging claims about the naturally non-evident since we never experience such phenomena and it is therefore unintelligible by the standard that "every intelligible thing derives its origin and source of confirmation from sensation."⁵⁵ Gassendi counters that we recognize the sign phenomena themselves--since they are evident--by the criterion of the senses or test of experience, and we can reason from such signs to the non-evident just in case our reasoned judgements meet the criterion of the intellect, or rational constraints on our thinking--and principally (as we have seen) the constraint that we

⁵⁵SE II 429. The original Stoic proposal, against which Sextus argues, is that appearances may function as indicative signs if they are sufficiently 'apprehensive and gripping'; Sextus replies that these characteristics yield neither regularity nor viable existence claims. Gassendi's response, in turn, seems to be an attempt to flesh out the Stoic view.

accept incontrovertible claims. Thus indicative signs give us viable evidence of the imperceptible and the basis for warranted claims about the same if we can (a) form empirically viable beliefs about the sign-phenomena and (b) accept only those claims about the hidden that are incontrovertible--which include, in his view, the judgements that particular signs could not be present unless some indicated imperceptible were, too.⁵⁶ The force of this reply to the Skeptics, then, lies in Gassendi's defense of the view that we can identify criteria for judging the veridical nature of such claims about that part of the world which is nonevident.

3. Truth-Criteria and Means of Justifying Empirical Beliefs

More broadly, Gassendi argues against the Skeptics that we can have warrant for a wide range of empirical judgements we hold to be veridical. One key element of this view is the suggestion that our beliefs about appearance are generally truthful, or at least bear a resemblance to the truth, about the appearances we apprehend.⁵⁷ Accepting this degree of accuracy among appearance-beliefs entails that there is some means by which we can gauge their truth-likeness, so that (contrary to the Skeptics) there must be at least one epistemic criterion. But Gassendi actually goes further, suggesting that the same source of this criterion also yields the grounds for justifying judgements about such appearance-beliefs. He proposes, in short, that the reliability of our faculties of sensation provides warrant for our empirical claims.

⁵⁶B 333.

⁵⁷B 329.

What is the nature of these criteria--are they standards of truth or of justifiability?

Typically, we take the search for criteria in epistemology to be a search for standards of gauging the truth of propositions.⁵⁸ How can the mark of truth provide the warrant for our knowledge as well, particularly given the possibility of accidentally holding true beliefs? Gassendi should be aware of the dangers of conflating truth and epistemic warrant, as he takes Descartes to task for doing just that.⁵⁹ Yet the approach Gassendi offers to the problem of the criterion also conflates, or at least combines, these two elements of knowledge. In brief, he proposes that we take both appearances and claims about appearances to be reliable because there are criteria by which we can, with great regularity and dependability, detect that such appearances and claims bear something very near the mark of truth--and this reliability provides in turn just the warrant we need to reasonably sustain beliefs about the world.

To begin with, what conception of truth is Gassendi talking about? In the Logic he suggests the property of truth is applicable to three kinds of items: objects in the

⁵⁸This is at any rate the topic of concern in some contemporary literature: see Roderick Chisholm, Theory of Knowledge (Englewood Cliffs, NJ: Prentice-Hall, 1966).

⁵⁹For example, Gassendi suggests Descartes is wrong to think of his criterion as a means of judging not only the *truth* of our mathematical beliefs, but their *certainty* as well: "...I have otherwise judged that it is impossible to pass from a smaller quantity to a lesser quantity, without passing by their equality--just as two lines approach each other more and more nonetheless without meeting if we prolong them indefinitely. It seems to me in effect that to perceive these things clearly and distinctly that I would consider them as very true and indubitable Axioms; later on, however, arguments would be found to persuade me of the contrary as a thing more clearly and distinctly conceived. Even at present I hesitate still when I consider the nature of mathematical hypotheses. That's why it is permitted to say that it is true that I know such and so propositions insofar as I suppose or conceive such manner of acting for quantity, lines, and similar things; but that these so many propositions be true in themselves, that's something we cannot affirm with certitude." (DM, O III 314b-15a; R 198-200.)

world, our concepts or ideas of such objects, and propositions we form about the same. Object-truth, which he calls "truth of essence or existence," consists in that feature--which he believes may be found in all things--of having a particular identity, such that it is "exactly what it is and nothing else." Since all existing things, including copies and false versions of other things, necessarily have this property,⁶⁰ there is no sense in which we can talk about a corresponding object-falsity.⁶¹ For example, the pen in my hand is a true pen because it exists, and the pen in the Parker's advertisement is a true pen representation because the representation exists (though not necessarily the pen represented), whereas the pen in my thought "I would like to own a Parker pen" presumably does not bear any object-truth value in this context since there is no particular existent about which I am thinking.

Concept-truth or idea-truth, by contrast, is defined as follows: a concept is true if and only if the concept agrees with its subject, and false if and only if it disagrees with its subject.⁶² By 'agreement' between two things, Gassendi means that there is a property-wise correspondence between them, where the one is neither contrary to the other (as black from white) nor, if a matter of degree, overly disparate from the other (as one genus from another).⁶³ Hence our ideas are true if they and their subjects have some such regular correspondence. An even more basic requirement for concept or idea-truth, then,

⁶⁰"...whatever exists in reality...is true and may be called a truth of existence" (B 286).

⁶¹Part Two Canon I, II 105-6.

⁶²Part Two Canon I, II 105.

⁶³Part One Canon XVII, II 100.

is that the concepts or ideas are sufficiently complex as to have some subject-predicate structure, otherwise there is no place to find the requisite correspondence. Accordingly, an idea of a particular table is true if tables and the idea in question have no contrary or overly-disparate attributes; the idea of a blue table can be true, for example, because there is nothing contrary about the attributes of being blue and being a table. A 'table' idea is false, however, if it says tables are made out of courage--for tables and courage fall under entirely disparate genera, such that they bear no property-wise correspondence. On the other hand, a 'table' idea can be true even when the properties in question are rather disparate, so long as there is but a minimal generic commonality; the idea of tables made out of jello can be true because tables and jello fall under the same genus, physical bodies.

There is, or perhaps should be, some controversy as to whether Gassendi wholeheartedly embraces this concept-truth view. In an uncharacteristic moment of self-doubt, Gassendi cautions that we might not really have a way of saying a given concept is true or false unless we have an accompanying judgement that the thing represented by the concept is or is not as represented by the concept.⁶⁴ His worry is that it is wrong to attach a truth property to concepts *per se* given that we could have the agreement in question which marks a concept as true yet those concepts might fail to represent any possible worldly subjects, in which case it seems mistaken to speak of them as 'true' concepts. Thus, we might have a concept of unicorns or tap-dancing mice but since there is nothing which these concepts can actually be said to represent it is hard to see under what

⁶⁴Part Two Canon I, II 105.

circumstances we should accept that they are 'true'--or 'false', for that matter. If anything is true or false here, it is the accompanying judgement and not the concept itself--but then we are talking about propositional-truth and not concept-truth. It is tempting to say that, beyond his caution, Gassendi intends here to dispense with the notion of concept-truth--all the more so since we don't really talk about such a notion in contemporary truth theory anyway. An apparent problem with doing this is that Gassendi is committed to holding onto this notion throughout his discussions of that for which we need epistemic criteria. In particular he suggests that we need a criterion of the senses to pick out those of our ideas which are true. But given his view that we can only make judgements concerning the veridical character of our concepts if we can judge them to accurately represent some aspect of the world and given that such judgements take a propositional form, it seems that this criterion attaches after all to a species of propositional-truth.

At any rate, Gassendi shapes his full-fledged account of propositional truth around an agreement requirement similar to the principle feature of his original concept-truth view. To begin with, he defines propositions as judgements made by the mind which consist in a joining of agreeing ideas (affirmation) or a separating of discordant ideas (denial) and featuring a subject-predicate form joined by a copulative 'is' or 'is not'.⁶⁵ Accordingly, propositional truth consists in the agreement of a judgement or statement with its subject: "That proposition is true which declares to be the case what is the case, or declares not to be the case what is not; and conversely, that proposition is false which

⁶⁵And if they do not have that form explicitly, they can be reconstructed accordingly--so that even 'man is' should be understood as 'man is existing'. Part Two, II 102-3.

declares to be the case what is not the case, or declares not to be the case what is."⁶⁶ Here Gassendi adopts the Stoic or Epicurean conception of truth which, as Chisholm has indicated, closely anticipates the Tarski-Carnap truth scheme,

(TC) "x is y" is true if and only if x is y,

and perhaps even anticipates a full-blown disquotational theory of truth insofar as he takes this kind of truth to consist only in that property in virtue of which we assign truth-values to instances of something like (TC).⁶⁷

Whatever the merits of this approach to propositional-truth, we are now a bit closer to the traditional conception of a truth-property, according to which truth is an attribute of beliefs or propositions. For Gassendi, what apparently ties this kind of property to properties of concept- and object-truth is that they are all, to put it crudely, some measure of the accuracy of a reported item or state of affairs.⁶⁸ Still, concept- and object-truth differ starkly from propositional-truth in that their objects need not be expressible in propositional form, so we cannot take Gassendi to be talking in those other cases about quite the same property of which we speak in contemporary epistemology. Yet whether we accept that these different kinds of truth are all relevant to our own notion of what kind of property truth is, we ought to be able to accept, at least as a decent

⁶⁶Part Two Canon I, II 104-6.

⁶⁷Roderick Chisholm, "Sextus Empiricus and Modern Empiricism," Philosophy of Science 8 (1941): 371-384. Cf. B 287.

⁶⁸This point reflects his view that all warranted empirical belief is rooted in experience. Since all propositions are composed of sensory-based ideas, he argues, the viability of such compositions consists at least partly in the degree to which experience supports the relations (agreement or disagreement) of ideas in a given proposition.

effort, his picture of the sorts of accuracy with which we should be concerned were we to include among the various objects of knowledge not just propositions, but ideas and even worldly objects. The pressing question would then be what it could possibly mean for someone x to know, for example, an idea p without standing in the relation ' x knows that p ' and p having one or another propositional form. As we have seen, though, Gassendi also has doubts about the feasibility of applying a truth-predicate to non-propositional items.

One caveat to Gassendi's view of truth as a detectable property of the objects of knowledge (particularly relative to empirical knowledge) is that he does not think we can conclusively know that our beliefs about appearances actually are true. This is because he takes truth to be an essential quality of appearances, and he thinks we can have no knowledge of essences.⁶⁹ However, he maintains, we can know whether some such belief resembles the truth--is *verisimilitudinous*--by measuring beliefs about the apparent against other experiential information. Verisimilitude, or truth-likeness, is the property, generally borne by propositions or judgements, of approximately characterizing the way the world is; we take such propositions as veridical, though, strictly speaking, they are false. We may hope to attain many such approximations to the truth, but our hopes

⁶⁹The argument here is a bit fuzzy. If truth is an essential property of appearances--presumably in the sense that appearances are produced by species which bear veridical presentations of information from their emitting objects--yet we can't know essences, then it's something of appearances (one or more of their properties) that we cannot know. But Gassendi thinks it's something about our claims about appearances--namely, their truth or truth-likeness--that we cannot know. Incidentally, one standard argument in defense of the mere verisimilitude of our empirical claims relies on the premise that we just cannot trust our sensory information. But that premise is unavailable to Gassendi.

should be limited in the sense that our judgements about the world are frequently, if not always, a tad amiss. For Gassendi, however, the problem is worse than not being able to attain absolute truth given our modest intellects. We can never even land on a solid candidate for the truth with any permanance:

...on the truth itself I never pronounce myself, feeling incapable of discovering it; I am thus a man who holds no resemblance to the truth as warranted strongly enough--I am not entirely ready to give warrant away, if there arrives another [candidate] that appears to have greater weight.⁷⁰

An inability to conclusively assess the truth of our beliefs might well have been an embarassment to Gassendi's efforts to identify secure foundations of knowledge, but happily, it seems our acceptable beliefs don't even have to be conclusively identified as absolutely true. So how is it then, that we pick out the viable ones? Let us turn to the criteria he thinks allow us to gauge the objects of knowlege as even approximately true.

Gassendi responds to the Skeptic's search by proposing truth-criteria not in the form of marks or characteristics of truth *per se* but in the form of cognitive 'instruments of judgement'. The problem of the criterion, he proposes, is historically the problem of determining which of our cognitive faculties, if any, can serve as instruments for determining the truth.⁷¹ Following Epicurus, he identifies the proper faculties as the senses and the intellect (understanding). With our sensory capacities we detect the existence and qualities of the perceptually evident and so gauge the veridical nature of our basic appearance-beliefs. With our intellectual capacities we measure whether our

⁷⁰DM, O III 389a; R 536.

⁷¹Gassendi traces this approach to truth-criteria to Sextus Empiricus and the targets of his skepticist critique. B 293.

reasoned judgements about the non-evident as well as the evident are true.⁷² Following his distinction between kinds of truth, then, it seems we can fairly identify the senses as the criteria of object-truth, and the intellect as the criterion of the veridical nature of judgements, that is, propositional-truth.⁷³ Although Gassendi does not make this assignment explicitly, these are just the kinds of epistemic properties picked out by his proposed criteria.

That there are satisfactory criteria of this sort he believes to follow from our ability to draw warranted inferences from signs. If we can justifiably infer beliefs about the non-evident from signs, he suggests, some criteria will enable us to recognize the (approximate) truth of such beliefs.⁷⁴ It may well seem that this strategy unfairly hitches the fate of the criterion to the success of sign-based inference. Thus, if we found sign-phenomena were instead poor inferential tools we might not be able to pick out the same truths about the non-evident, but it does not follow that there could be no criterion--only that no criterion helps us determine the truth of those beliefs we inferred from signs.

But Gassendi does not think this an unfair strategy because he believes that a principal, if not the central, role of epistemic criteria is to provide a standard for identifying the truth of our beliefs about the non-evident; we employ the criteria primarily to pick out signs and their significance. Hence the understanding constitutes a criterion,

⁷²SP, Q I 80b-81b; B 290-91.

⁷³By contrast, Osler suggests (in her *Divine Will*) Gassendi intends that the senses are the criterion we use simply to judge object-truth. This suggestion makes it unclear, however, what Gassendi could then take to be criteria for judging propositional-truth or concept-truth.

⁷⁴B 293.

not only of propositional truth generally, but in particular of what counts as an indicative sign.⁷⁵ We use the senses to judge the truth or accuracy of our concepts of appearances in general, but 'signing' appearances in particular, and we use the understanding to judge the truth or accuracy of judgements about appearances in general, but judgements relative to evidence for 'signed' phenomena in particular. Moreover, the understanding serves as a standard of accuracy between its own judgements and those of the senses, so that in case the senses should mislead us and present us with an unreliable sign, the understanding can 'correct for' misperception and thereby lead us to a more fruitful interpretation of that sign, or else the rejection of the corresponding appearances as a sign, after all.⁷⁶

Indeed, in De Fine Logica Gassendi goes so far as to claim that the sole purpose served by criteria is to pick out viable instances of signs. He notes that the problem broached by the Skeptics--whether there exists any criterion by which we know something to be true--is not intended by them to raise doubts about appearances, for they at least admit that appearances exist and that we can make reliable claims about appearances, though they insist such claims are relative to how things seem to us. Rather, Gassendi suggests, the crux of their problem is how, if at all, we can go beyond appearances to make claims about what is hidden, and what criterion would allow us to judge that such claims are true.⁷⁷ This is not, however, the right way to construe the scope of skepticist worries--they are not so quick to allow that we may have knowledge

⁷⁵SP, Q I 81-82; B 347.

⁷⁶B 333.

⁷⁷B 329.

about an external world beyond appearances. More to the point, as Gassendi himself says in the Institutio Logica, they are concerned with the possibility of criteria we may employ for the broader task of identifying truth as a property of various objects of knowledge, and not just the phenomena we pick out as signs.

At any rate, his own account of criteria special to sign-identification suggests a picture of epistemic criteria which requires the reliability of our cognitive faculties in order that we can identify the truth or truth-likeness of our beliefs. When, for example, we identify apparent phenomena as signs of the non-evident, we use our understanding to make this judgement. But since this judgement is made at least partly independent of experience, Gassendi reasons, the standard we use to measure its resemblance to the truth should also be of a non-experiential or rational nature. Hence the faculty of the understanding itself constitutes a natural, if reflexive, candidate for this standard. There are two curiosities here. First, it is strange by modern lights to pick out a cognitive faculty--as opposed to, say, a rule--as a criterion. Yet it was not strange at all to Gassendi, who is familiar with those ancients who do the same, and in this instance he clearly follows Epicurus. Second, when he suggests that in order to gauge the merits of the understanding's judgements we need standards appropriate to reasoning tasks it performs and the understanding is best equipped to do this, he anticipates the modern claims that our reasoning ought to conform to rationality constraints, or rules delimiting the boundaries of legitimate inference and judgement, and that such constraints may be designated descriptively. Indeed, the reason that a cognitive faculty serves as a standard for gauging the veridical character of our judgements is that the faculty can pick out

whatever has the epistemic property in question, as part of a larger pattern of conforming to regular norms of cognitive activity. It is intriguing (and a small piece of efficiency) to suppose that he might have taken the same regularity and reliability in cognitive performance to underlie his identification of the senses as criteria of concept-truth or object-truth.

One problem with taking the understanding as a criterion, Gassendi concedes, is that different minds may yield diverse judgements so it is not *prima facie* clear which understanding of which person is suitable for serving as criterion, or how to rule out the possibility of diverse criteria. His suggestion in response is that any understanding is suitable if it obeys the rationality constraint that no judgement made yields an absurdity. Thus worthy judgements are those made by the person who "...having weighed all considerations, presents an argument that cannot be legitimately contradicted."⁷⁸ Presumably this kind of constraint is supposed to narrow the field of suitable understandings to a set of criteria which will not grow to permit judgements that are so divergent as to be incompatible--but Gassendi neglects to say how his proposal assures this goal. After all, the understanding of one person alone may generate unworthy judgements, and the intersection of the set of least-erring understandings may yet yield inconsistent judgements.

Whatever its immediate flaws, we now have some picture of the truth-properties Gassendi thinks we can identify, and the standards--really faculties--he thinks we can use to pick them out in objects of knowledge like concepts and judgements. A natural next

⁷⁸SP, Q I 81-82; B 347.

move for him, as somebody interested in accounting for the justification of our beliefs about the external world, would be to say how, if at all, picking out these truth-properties provides the requisite warrant. In fact, though, he does not tell us the criteria justify our beliefs as a direct result of indicating in some fashion that they are true or verisimilitudinous beliefs--rather, his discussion oddly runs together the distinct tasks of providing the standards for truth-properties and justificatory norms. Nonetheless, he suggests these criteria yield warrant for our empirical beliefs in the sense that the same reliability of those cognitive mechanisms which enables their performance as criteria of truth-properties also gives us such warrant. His proposal, in brief, is that we may justify our empirical beliefs by establishing their conformity with other beliefs that originate in sense-based ideas received under those normal or regular conditions which lend our cognitive faculties--and thus our beliefs--their reliable character.

Let's begin the Gassendist story of epistemic justification by recalling his physicalist account of how we can have true (or at least verisimilitudinous) empirical beliefs which are reliable despite the varying appearances of objects. One reason the same things appear to us in various ways in the first place is that distance creates perceptual distortions. Again, we perceive something x when our eyes are impacted upon by material species corresponding to x , which are transmitted to us as light corpuscles arranged in particular ways so that they represent the features of x . As a result, the distance between ourselves and x affects our perception by enhancing or diminishing the quantity and quality of the rays of light carrying the representative corpuscular arrangement, and this correspondingly enhances or diminishes the x 's qualities as

transmitted by the species. Thus species in our field of perception yield larger images of closer things and smaller images of further things. This physicalist model accounts for cases like the tower we perceive as round from a distance and square close-up: at short distance, the rays hitting our retinas are sufficient in number to give us images of the many edges we must perceive to distinguish the shape of a square tower, but from far away, fewer such rays reach our retinas, with the result that we perceive fewer edges and judge that the tower is round. It is not because the senses err that we come to this judgement but because the tower's corners are indistinguishable under normal conditions by normal perceptual processes. Thus while our belief that

(i) "the tower is round"

is unjustifiable by the evidence before us--and also false, our belief that

(ii) "the tower appears round to us"

is not only *not* false--the physicalist account suggests we can be justified in this belief as well.⁷⁹ As in contemporary reliabilist theories of epistemic warrant, Gassendi takes the normal state of our perceptual processes to be a necessary guarantor of our beliefs (the other guarantor being, of course, the regularity of those processes).

We might think that such false empirical judgements as (i) are cases where the senses err. But for Gassendi (like the Skeptics) the senses are not the source of error in empirical judgements. Rather, in such cases the understanding produces a judgement about the nature of a thing without properly assessing the relationship between the thing and its appearances, so that we are unable to accurately establish "...which of the different

⁷⁹B 344-45.

appearances produced in the senses...is in conformity with the thing."⁸⁰ Here Gassendi suggests that the source of warrant for our empirical judgements is itself a sort of judgement of the understanding where we assess the conformity of a thing's different possible appearances with the thing itself. One possible difficulty is the potential for a justificatory gap. What justifies this conformity judgement--and if it is another judgement of the understanding, what would in turn warrant *that* judgement? The natural response given the justificatory strategies we have visited so far might be that the regularity of the cognitive mechanism provides this warrant, but Gassendi himself is silent in this regard. Further, we may also wonder how, particularly for Gassendi, we are supposed to know the nature of a thing itself apart from any of its appearances. Indeed, if we had such access to those natures, why would we need to bother with empirically-based judgements to characterize those things for which we already had adequate, non-empirically-based pictures? It may seem here that Gassendi has tripped over his own injunction against certain knowledge about the world beyond appearances.

Yet we can more charitably construe him as proposing that the judgements about external things we produce by the understanding are warranted if they conform to other, experientially-derived beliefs made under normal circumstances (that is, for deriving such beliefs with working sensory apparatuses) where the extent of those judgements failing to conform is an indication of *their* extra-normality. That such a normality-constraint should be a crucial ingredient in determining the warrant for our empirical beliefs is a

⁸⁰B 345. By contrast, the senses cannot err since they apprehend appearances but do not make judgements about them.

natural corollary to his physicalist account of the variety in our apprehensions of the same perceptual givens. That account, let's recall, says we explain such variety in terms of the differing interactions of corpuscular species with our sensory organs, such that there must be a sense in which those organs function 'normally' or else there would be no regular correspondance between the variety we find in physical interactions and that which we attribute to our mental apprehensions. Clearly, all of our apprehensions will also require the normal functioning of sensory organs (in tandem with the right sorts of external physical events) in order that we take the empirical beliefs so grounded to be justified. As with contemporary reliabilism, this proposal faces the difficulty that such a requirement may account for what is necessary, though not what is sufficient, for lending warrant to our beliefs.

In any case, one clear result of Gassendi's cognitive faculty model of criteria and epistemic warrant is his conception of our perceptual faculties as the ultimate arbiters of our empirical claims. While the understanding performs all manner of operations on sensory information (e.g. compiling, generalizing on, abstracting from) and functions as the truth-criterion relative to a large class of beliefs, the senses provide the constituent ideas, standard of accuracy, and ultimate source of warrant, for our judgements about the world. Indeed, he makes an unusual appeal to the authority of the Stagyrte in proposing that we use experience and observation to correct our mental judgements:

...when it sometimes happens that reason seems to be in conflict with the senses, Aristotle teaches most strikingly that we must decide more on the basis of the senses than on the basis of reason; this because such reasoning cannot really penetrate the matter, but remains only apparent, while the real reason why the

matter appears as it does to the senses lies hidden.⁸¹

By purely reasoning about some phenomena x for which we have evidence from the senses y we may come up with an 'apparent' account of x which saves the phenomena, but the 'real' reason why x gives us y is 'hidden' and consists of some causal mechanism unknown and possibly unavailable to us. If we knew what really brought about this sensory evidence, we wouldn't have to rely on speculative reasoning; since we do rely on such speculation, Gassendi is careful to stress its limits. Thus, he proposes, neither the imagination nor the understanding can lead us to certain claims about the causes underlying evident phenomena because such pure 'penetrating' reasoning about the imperceptible leads us beyond all sensory experience and so beyond that faculty which is the ultimate source of our empirical ideas. Hence any claims about the underlying causes of appearances we attain without the benefit of sensory evidence rely either on baseless conjecture or else clairvoyant access to those underlying causes. Yet if we really had any cognitive access to underlying causes, it would come most naturally through perception first, and the understanding second if at all.

Gassendi tries to illustrate this position by refuting the armchair intuitions that an arrow shot straight up from the stern of a moving ship would land in the sea, and that people cannot live on the opposite side of the globe without falling off. In both instances our intellectual intuitions tend to lead us astray but can be corrected by sensory information: we can perceive that as the ship moves forward, the arrow moves forward too and so is likely to fall on the ship; and we can find people who have not fallen off the

⁸¹B 372.

opposite side of the globe. We tend to reason better in these cases has a function of having more complete empirical information, which suggests to him that reason by itself is deficient for generating viable empirical claims. Of course, we don't really need either particular empirical information or eyewitness accounts to generate the pertinent physical calculations in these cases; mentally rehearsing the proper thought experiments should suffice to demonstrate the correct accounts.⁸² While his examples may not be the most fortuitous, it seems fair enough to assume that some sensory experiences may suggest claims we could not possibly derive by reason alone--though it might well be that such acts would fail to establish the corresponding empirical claims only because of our own cognitive limits. The significance of Gassendi's view here, at any rate, is that the empirical claims we attain by acts of the understanding ought to at least fit information we gather from the senses, rather than the other way around.

Unfortunately, he is not always appear to be consistent in this regard:

...although it is admitted that the senses are sometimes misleading and that therefore the sign may not be reliable, still reason, which is superior to the senses, can correct the perception of the senses so that it will not accept a sign from the senses unless it has been corrected and then at last it deliberates, or reaches its judgement of the thing.⁸³

Can we fit this passage with the picture of experience as the test of reason? Margaret

Osler suggests that Gassendi sees the relation between perceptual and reasoned claims as

⁸²As I point out in chapter two, Gassendi is certainly familiar with the value of thought experiments. He is also perhaps best known in the history of science for physical confirmation of an important result first concluded by thought experiment, namely, dropping stones from the mast of a moving ship to demonstrate Galileo's views on terrestrial motion.

⁸³B 333.

a two-way street, where we use the senses to correct the claims of reason and then we turn to the understanding to correct our perceptual apprehensions. Although this interpretation seems charitable, it may not capture the subtlety of Gassendi's views. The passage in question does not suggest that the senses *per se* fail as criteria of appearances but rather that our percepts may fail to give us accurate pictures of the external world--which could be caused by external *or* internal factors--with the effect that we mistake particular appearances for signs and so make faulty sign-based inferences.⁸⁴ We might better understand this passage as the proposal that the understanding functions as a kind of feedback training of our perceptual faculties. For example, by the light of reason, memory, or some other act of cognition we might make clear the distorting effects of abnormal conditions for our sensory apparatuses on the perceptions we have of the external world. Once we discern a way to correct for those conditions, the understanding could then accept the perceptions so corrected as 'signs from the senses' and thereby the subject of viable judgements. According to this interpretation, the senses remain the source of basic idea-content, veridical character, and epistemic warrant for such judgements. By contrast, the view that the quoted passage indicates that the understanding and the senses correct each other neglects Gassendi's perspicuous and frequent designation of the perceptual faculties as the primary tools through which we come to know about the external world.

⁸⁴ Another reason for caution here is that Osler's proposal has it that our faculties of reason are at once superior and inferior to our perceptual faculties, and this may preclude attributing a coherent cognitive model to Gassendi.

4. Probabilism.

The regularity and normality of our perceptual apparatus may lend warrant to our empirical beliefs, but these features cannot make us certain of those beliefs. In De Fine Logica, Gassendi chastizes the 'dogmatic' philosophers for naively suggesting we might even be certain about empirical beliefs:

...the occasion arises only too frequently in the physical sciences to declare that we are fortunate if we attain not what is true but what is probable...we feel that in such an incapacitated state it should be considered a great gain if we can rise to the point where we may glimpse not the truth itself (in its very body, so to speak) but some slight image of it, or even its shadow.⁸⁵

Here Gassendi contrasts probability with truth and so once more conflates truth with certainty. But this seems to be an unfelicitous slip, and elsewhere he suggests that we can hold warranted beliefs about the world with less than complete certainty: evidence for an empirical belief which gives it a sufficient probability of being true may warrant our acceptance of the belief. This suggestion differs from two other traditional views on the warrant accompanying less-than-certain empirical belief: (i) the Skeptic's non-probabilist claim that we have no warrant for beliefs which the evidence does not make certain;⁸⁶ and

⁸⁵B 326-27.

⁸⁶This view comes in at least three varieties:

- (a) there are no beliefs for which we can have evidence that yields less than full certainty, thus the evidence for our beliefs is either so convincing as to make us certain or else not convincing at all;
 - (b) there are some beliefs for which we can have evidence that yields full certainty—it's just that we have no warrant for holding them;
 - (c) for all our beliefs (minimally, relative to some area of knowledge), we can have evidence that yields less than full certainty, thus we have no warrant for knowledge (in that area).
- Several ancient Sceptics, following the classical tropes, held a form of (a) according to which evidence from the senses fails to give us empirical knowledge because we cannot be certain about such evidence. On the other hand, we might think the canonical skeptic is closer to (c) since several classical figures suggest we have sufficient evidence from the senses to arrive at beliefs about appearances, though we lack any assuredness about such beliefs. The extent to which we

(ii) the rationalist's probabilist claim that, though we might have warrant for such beliefs, there can be no such evidence.⁸⁷ Gassendi's reply to these alternatives is that there is no reason to suppose the justification of our empirical beliefs requires that we are certain of them. He is satisfied with the probabilistic warrant we have for such claims as a product of the immediate and reliable nature of evidence from the senses. The reason for his satisfaction is that he believes we should be even less sure about reasoned judgements supposed by some to yield certain claims than we should be relative to our experientially-derived claims. This is so, he argues, because such judgements are at best indirectly based on sensory information, our surest source of epistemic warrant, whereas the claims derived from experience tap directly into that source.

What does Gassendi think it means to have probabilistic or certain warrant for a given claim? Beyond the quotidian observation that some claims are more likely than others, he suggests we have probabilistic warrant for propositions if they are supported by evidence we judge as reliable in virtue of either its frequency or the strength of its witness. On the other hand, we have certain warrant for propositions if they are indubitable. Since he defines certainty as the "sureness of assent which the mind gives to

can say Carneades remains a thorough skeptic of the Pyrrhonian order is the extent to which, rather than being a probabilist, he holds a view like (b).

⁸⁷This is because, for example,

(a) the support evidence from the senses lends to empirical beliefs is confirmatory but doesn't give us a reason to initially arrive at such beliefs; or

(b) nothing counts as sensory evidence for our empirical beliefs, because whatever we might standardly take to support those beliefs only compounds our misconceptions about the external world.

A view like (a) could be held by rationalists like Descartes or Leibniz. A view like (b) might be held by a platonist sympathetic to probable belief.

a proposition which it regards as necessary," he might seem to be in danger of running together epistemic certainty and some unrelated form of necessity (e.g. metaphysical or physical). Yet his concern is actually with what the mind comes to regard as necessary, which is neither the proposition nor anything it asserts, but that the proposition is true. Thus, for example, we may be certain of metaphysically contingent and necessary propositions alike.⁸⁸ We become certain of a proposition if we measure the evidence (reasoned or empirically demonstrated) for it and consequently judge that it must be true. For instance, Gassendi suggests, we can be certain of empirical propositions like 'it is day' just in case all our evidence from the senses supports such claims and none contradicts it. Here his concern is less with the psychological than the logical relation between the evidence and the proposition it is said to support, and his notion is apparently that this is some sort of entailment. This suggests that he takes certainty to be a function of logical necessity--not much better of an idea than linking our assuredness to metaphysical necessity. Perhaps the best that can be said of his views on certain knowledge is that he remains true to his empiricism, holding out the possibility of knowing claims with certainty on the basis of whatever evidence from the senses affirms such claims in indubitable fashion.⁸⁹

Among the empirical claims about which we can be certain, one significant range of cases for Gassendi are those judgements we reach on the basis of indicative signs. Crudely, we should be certain of such judgements because, given the nature of the sign, it

⁸⁸Part Two, Canon XIII, II 112-13.

⁸⁹Part Two, Canon XIII, II 112.

is not possible for the hidden phenomena which is the subject of our judgements to be otherwise. Consider once more his beloved case of the judgement that pores exist from the sign of sweat on the skin: "...a certain truth does exist, namely that there are pores, and this is demonstrated fully, not from the fact that there are pores, but from the fact that if there were not, two bodies would have to be in the same place at the same time."⁹⁰ Since we cannot admit belief in the physically impossible, and it is impossible for the two bodies--particles of skin and sweat-- to be in the same place at the same time, we may adopt an account which enables to avoid such a belief. Thus we take sweat on skin as an indicative sign of pores because it allows us to most easily avoid having to accept the physically absurd. For Gassendi this makes the pores claim not simply a likely story but the only explanation we can embrace, which he takes to be equivalent to a claim of which we may be certain.⁹¹ One consequence of this view is that certainty is apparently warranted if the claim best allows us to avoid (physically) absurd beliefs.

One obvious flaw here is that there could be any number of alternative accounts which enable us to avoid absurd beliefs, in which case we lack the stated grounds for being certain of any one of the alternatives. A further difficulty is that, while Gassendi's theory of signs promises to give us empirically-sound grounds for beliefs about the non-evident, the grounds for our certainty about such beliefs in fact has nothing to do with the empirical character of the evidence supplied by the sign or even the character of such

⁹⁰SP, Q I 81-82; B 347.

⁹¹Note that the justification for accepting sign-based inferences of this sort rests on our knowing with certainty other, more basic physical claims--for example, that no two bodies can be in the same place at the same time--for which we presumably require supporting empirical evidence.

inferences. Rather, our certainty results from the security of rules like 'don't accept absurd claims' plus an accidental feature of the sign-based claim as contrasted with its alternatives (to wit, the claim itself is not absurd whereas all the alternatives are). That we can be certain about such claims thus turns out to rely less on the strengths of experientially-attained belief than the fortuitous interpretation of a given sign so as to yield a claim which is not physically absurd.

In any case, failing certainty, Gassendi advises that we can maintain a proposition with probable warrant insofar as it tends "...more toward evidence than obscurity." A proposition is 'evident' for Gassendi if there is empirical data which augments its likelihood. In a 1642 letter to the Count Louis-Emmanuel de Valois (Valesio) Gassendi proposes that

Evidence is that which presents itself to the senses in such a way that it cannot be disputed since it fully recommends itself and nothing can be more probable.⁹²

What makes a proposition all the more probable (likely) then, is greater clarity, which he maintains, can be measured by the frequency of evidence or strength of testimony for it, such as the reliability of witnesses in its favor. Thus we might say "The heat of the sun will be strong at the next solstice" is probable because we have observed many more instances of the strength of solar heat at solstice than not, and the frequency of the affirming observations lends the proposition the requisite clarity. And we might take "A swallow has been seen at the equinox" to be probable, despite the low frequency of such

⁹²Q VI 150a; translated by Brundell (Pierre Gassendi, 102).

sightings, just in case we have a reliable or generally credible witness.⁹³

One sizable mystery we are left with is what counts as *credible* evidence. This is a question I look at in chapter two, relative to the question of evidence for a scientific hypothesis. In his general theory of empirical knowledge, though, Gassendi offers little guidance as to how we to judge the suitability of evidence for ordinary, prescientific or nonscientific uncertain belief.

Nor does he even attempt to explain the relations between the available evidence and the warrant it affords our beliefs. So, for example, the warrant for claims we base on inferences from commemorative signs consists in the 'proof of experience', which suggests only that we may be no more certain than is justified by our apprehensions of appearances.⁹⁴ Unfortunately, Gassendi never says how much evidence from those signs (plus their commemorative affinities to previous phenomenal descriptions) delivers what degree of support for these claims.

One promising aspect of this account, relative to sign-based claims in particular, is Gassendi's notion that we can have warrant for the high probability or even certainty of such claims. This illustrates his confidence in what signs can tell us about the non-evident despite his dismissal of knowledge of the essences or basic natures of phenomenal objects. Even without epistemic access to the essences of things, we can form judgements as to their hidden attributes on the basis of signs. Further, we can gauge the likelihood of those judgements by the degrees to which such signs provide credible

⁹³Part Two Canon XIV. II 112-13.

⁹⁴SP, O I 81-82; B 347.

evidence.

In sum, from these few and imperfect remarks on certain and probable belief we can see the outlines of his particular response to the Skeptic proposal that there can be no warranted empirical belief since any warrant should provide us with certainty yet all candidates fail. Gassendi thinks we can have even certain empirical beliefs, including claims from sign-based inference and such simple judgements with empirical content like "It is day." But he also maintains that we have merely probable warrant for many empirical beliefs, and he insists on the susceptibility of *all* empirical belief to disconfirming experience. So his view is not the classical anti-skeptical proposal that our best empirical beliefs lack certainty but may be warranted anyway because they are more probable than not, and more probable than the alternatives. Rather, he recommends that (1) whether or not we can be certain of our empirical beliefs, they are warranted by evidence which gives them at least some significant likelihood, and (2) we may be certain of some empirical beliefs but even those are revisable. This last point suggests that, after all, epistemic certainty is no more linked to logical necessity in Gassendi's view than it is to metaphysical or physical necessity.

Finally, it might be thought that Gassendi needs to maintain this probabilism lest he concede the impossibility of natural or non-revealed knowledge. Recall his view that all our knowledge--even in fields which are classical candidates for *apriori* knowledge (like mathematics)--is ultimately the product of information from the senses. If we had no certainty about any beliefs based on sensory information, it seems all our judgements would be no more than probable. And if such judgements couldn't pass as knowledge

because of their limited, probable support, then it might be contended that we have no natural knowledge at all. Then his probabilism would look like a way to salvage warranted beliefs about the world not attained by revelation. But this cannot be Gassendi's thinking since he holds that there are empirical judgements about which we *are* certain. Rather, it seems his motivation for probabilism is simply that we need to account for those judgements we hold though their supporting evidence doesn't make us certain of them. In such cases, he proposes, we can judge them to be likely in proportion to the reliability we attribute to the evidence for them, whether we gauge this by the frequency of such evidence, its credibility, or something else altogether.

5. Nominalism

Gassendi's rejection of knowledge of essences of sensory objects is, at least at first glance, part of a broader nominalism. An overview of his theory of empirical knowledge is not the place to explore such nominalism fully, but it bears mentioning an epistemic counterpart to that piece of his metaphysics.⁹⁵ In brief, he thinks we come to beliefs about universals or essences not because we have direct epistemic access to them but because we reason through abstraction and analogy about individuals and appearances of which we have reliable sensory-derived ideas. It follows that our beliefs about essences lack the sure-footing of those of our judgements which involve neither abstraction nor analogy.

Why does he think we lack direct epistemic access to essences of material things? Gassendi reasons that sensory information about a thing's qualities where our experiences

⁹⁵For discussion of Gassendi's nominalism about universals or essences, see for example Bloch *La Philosophie de Gassendi*, and Osler, *Divine Will*.

vary intersubjectively cannot yield judgements about those of the thing's qualities which, being essential, do not vary in that or any other way. The locus of this reasoning is his discussion (in the Disquisitio) of two classical instances of the relativity of sensory evidence to individual experience--that honey tastes sweet to me, though bitter to you; and that fire seems hot to us, though not so to insects that live near fire. Since different people have different experiences, our knowledge of honey's taste or fire's heat differs from person to person and thus is not a reliable guide to invariable characteristics of, respectively, the honey or fire. In such cases we are familiar with a thing's qualities only as we record them on a subjective basis, so we can know no more than how the thing appears to us.⁹⁶ As the Skeptics put it, we cannot rely upon knowledge of appearances to learn about essences. Gassendi, we have seen, rejects the further, radical Pyrrhonian proposal that even our judgements as to how honey or fire appears may be without warrant, yet he shares their view that whatever the nature of our appearance-beliefs they do not provide a sure guide to anything we might construe as the essences of things appearing to us.⁹⁷

This epistemic corollary to nominalism about essences might well be taken to constrain the range of our empirical knowledge, by limiting experientially warranted belief to claims about appearances.⁹⁸ Yet Gassendi clearly thinks we can surpass such

⁹⁶DM, Q III 388b; R 535.

⁹⁷DM, Q III 311b-12a.

⁹⁸Osler (Divine Will, 115) suggests the connection between Gassendi's nominalism and his theory of empirical knowledge is that, since we can know only individuals and appearances, our empirical beliefs can only come in the form of probable judgements about observation and experiment. This is one natural way to reason about these two positions, but the argument from

constraints insofar as he proposes that our inferences from indicative signs may yield justified claims about the naturally non-evident. Moreover, he supplies no reason to think any such essences as there may be cannot be counted even among the naturally non-evident. His atomist views in particular might be construed as suggesting something of the essential nature of matter, and he certainly thinks we can have warranted beliefs about atoms. But these are crude and hasty thoughts, and it is additionally a question as to whether he takes such beliefs to be experientially warranted, or else conceptually or theologically necessary. These issues, though, I touch on in chapter three.

C. Conclusion

Epicurus and the Stoics early on offer the makings of a promising response to the Skeptics: we can have empirical knowledge because our cognitive faculties can judge true from false beliefs and provide all the epistemic warrant we need to sustain viable beliefs. What they didn't and surely couldn't have suggested is a detailed and vaguely plausible physical or psychological account to say how those faculties might provide either truth-criteria or justification for our beliefs. But an early modern thinker, Pierre Gassendi--who makes a life's project out of reviving Epicurus studies--takes up this response to the Skeptics and provides the kind of story about how we attain perceptual beliefs which, he believes, explains in turn how we can tell those beliefs are true and warranted. The story he provides is, incidentally, worked up out of a physics he also

the relativity of our experiences of secondary qualities suggests Gassendi rather thinks the inverse, that our inability to know essences results from the character of our observations and the types of warranted judgements we can make about them.

borrowed from Epicurus, but the physiological details and epistemological lessons of this perceptual account, problems and all, is strictly his own. This makes Gassendi, I suggest, not so much a proponent of the causal theory like Descartes or Locke, but the first reliabilist.

Gassendi surely crafts his theory of empirical knowledge in response to basic epistemological questions (one principle source of which is the Skeptics) but his theory should not be considered merely or primarily as a *negative* view offered in response to prominent targets such as Aristotle or Descartes. Consider one such interpretation from the recent literature: according to Antony McKenna (1993), Gassendi proposes an alternative to Descartes's brand of foundationalism because the foundation in question relies on the cartesian posit of a completely body-independent seat of cognition--which Gassendi cannot accept. McKenna contends that since Gassendi assigns a significant cognitive role to the corporeal imagination, he needs some non-cartesian way of justifying our knowledge claims. Gassendi's alternative account says no part of cognition is untouched by or not founded in experience because we base all our judgements on sensory information, and so the only warrant available to him for our beliefs must consist in sensory information itself. This is an inventive way to tie together two debates but McKenna cannot be entirely correct. For one, Gassendi also thinks there is an incorporeal mental apparatus which also provides the warrant for some of our beliefs, namely, our theological and cosmological judgements.⁹⁹ For another, this is an inaccurate picture of the reasoning that leads Gassendi to his account of justification. Descartes of course

⁹⁹SP, Q II 398-424, 440-42; cf. Osler, *Divine Will*, 67-69.

accepts that we can know some things with certainty only because he posits a pure thinking substance free of the uncertainties and doubts which accompany our imperfect sensory information, and Gassendi surely rejects both Descartes's mental model and his proposed criterion. But it doesn't follow that Gassendi's rejection of the latter is the result of his rejection of the former. Rather, he builds his view of truth-criteria and epistemic warrant as part of a broad account of perceptual knowledge the details of which are separable from, if conforming with, the debate with Descartes over the seat and nature of cognition. Indeed, the physical details of Gassendi's perceptual knowledge account must be separable from this debate insofar as Descartes also offers a physicalist account of perception but ends up with intellectual, not sensory, criteria and justificatory norms.

There are a number of other cases of this interpretive phenomenon: Brundell (1986), for example, argues that the single most important strand uniting all of Gassendi's oeuvre is the anti-Aristotelian stance he sketches in the early Exercitationes, and Osler (1994) suggests that a principal motivation underlying Gassendi's view of criteria is the goal of undermining the cartesian picture of certain truths known through clearness and distinctness criteria.¹⁰⁰ I have tried to indicate here that Gassendi's theory of empirical knowledge is instead best understood as a *positive* perspective, in which he fashions a reliabilist account of truth-criteria and epistemic warrant, and a probabilist account of viable empirical judgement, both on the basis of his physicalist account of perceptual belief. Along the way, he uses each of these elements of his theory in response to a past

¹⁰⁰One problem with Osler's assessment is that, as we have seen, Gassendi actually thinks some judgements are certain.

or contemporary thinker, but the theory as a whole is startlingly novel for the seventeenth century.

The negative side of this theory bows in the direction of the classical Skeptics: we cannot be conclusively certain about, nor find ultimate truths among empirical beliefs. But the positive side rejects Skeptical doubt about empirical knowledge in rather robust fashion: we can have such knowledge because we can identify a variety of strengths normally associated with many of our beliefs about appearances--notably, their reliability, approximation to the truth, and likelihood by degrees. This constructive skepticist compromise suggests a practicable if approximate understanding of the world is within our grasp, which is certainly the stance Gassendi adopts as a practicing scientist. In the next chapter, I examine his views on scientific method; one of my central concerns is to outline the ways in which elements of his theory of empirical knowledge guide his ideas about observation, experiment, and the use of hypotheses and hypothetical reasoning. Specifically, how if at all does the way we gain perceptual beliefs provide warrant for our ideas and judgements in the natural sciences?

CHAPTER II

GASSENDI'S SCIENTIFIC METHOD: THE *REGRESSUS*, AND HYPOTHETICAL REASONING

While some central ingredients of Gassendi's views of scientific method come from his theory of empirical knowledge, the overall contours of these views are shaped by two age-old tensions present in his thinking. For one, he holds a thoroughly deductivist picture of empirical inquiry yet recognizes the value of nondeductive reasoning, and for another, he maintains the empiricist notion that what we know is based only on ideas from the senses yet recognizes the value of conjecture beyond experience. We can partly dissolve these tensions by distinguishing between an 'official' method he proposes (in his Logic) and his informal writings on method. But Gassendi draws on elements of each of these views when he is involved in the business of science, reporting and analyzing observations and experiments. This suggests he is unwilling himself to dissolve these tensions and believes there is better science to be done by straddling the divisive issues, or else, less charitably, that he fails to see how great the divides really are.

In his 'official' writings on method, Gassendi follows a traditional view that the syllogism is the model of scientific reasoning, and adopts with significant modifications a late-Renaissance picture of *regressus demonstrativus* method, which says there are two steps to every empirical inquiry, one analytic and one synthetic. According to Gassendi's rendition of this picture, we realize each of the primary scientific tasks of discovery,

justification, and explanation by identifying the middle term of that syllogism distinctive to the empirical inquiry and particular task at hand. This model takes empirical inquiry to be a matter of demonstrative proof, where the chain of reasoning we pursue in realizing a discovery task is sufficiently tight that by simply retracing our steps in reverse, we may realize as well the justification task. He believes the reverse holds true, too--that we can make discoveries by reversing the steps of their corresponding justifications.¹ At the same time, though, in this strongly deductivist framework Gassendi departs markedly from the Aristotelian goal of certain empirical knowledge and tries to accommodate the uncertain character of experiential data and inductive inferences upon which he believes we ultimately base our every scientific claim.

Naturally, Gassendi cannot maintain a wholly deductivist method if he wants to say nondeductive inference is even a minor component of our scientific reasoning properly understood. And in at least several of his descriptive and analytic accounts of experimental results, we find one form of nondeductive inference--conjectural inference from signs of nonevident phenomena--is rather a major element of his scientific reasoning. A further difficulty emerges when we try to fit these same experimental accounts to the guidelines of his deductivist method--those guidelines are a bit vague, for

¹Of course, this doesn't quite make sense, for if what we are calling justification steps actually come first then the 'discovery' task doesn't reveal anything new and the 'justification' has been developed without justifying any previously-made claim. As we will see below, Gassendi thinks that, if he is right that retracing our reasoning in making discoveries can give us the corresponding justificatory reasoning, then it is because 'justification' and 'discovery' refer to two bits of reasoning where one is the mirror-image of the other, so that reversing the one should give us the other. This is what he intends by suggesting that reversing the steps of a justification task 'yields' the reasoning of a corresponding discovery task.

example, as to how we are supposed to identify the syllogism which best captures the reasoning of a given scientific discovery. The spotty nature of these guidelines and his discussion of conjectural inference suggests there is much more to be said about method beyond his 'official' writings and Gassendi must recognize this himself, for in a number of his other writings he offers an account of the proper use of hypothesis and hypothetical reasoning.

By the lights of this account, hypotheses are conjectures we base on our available data, which we may use to suggest further claims about phenomena for which there is no available data. Their empirical basis dictates that hypotheses may be no more than probable, and their conjectural character suggests they may merely *resemble* the truth, which Gassendi believes occurs when they conform with the data. The hypotheses we accept, he proposes, are those which most closely resemble the truth. This account helps fine-tune his 'official' method by outlining rules for one sort of conjectural inference, but it also suffers from its own difficulties. For example, by the criterion of truth-resemblance alone it is not clear in all cases either how to decide among more than one hypothesis or what will count as sufficient evidence for hypotheses concerning future events. Moreover, this account underscores the aforementioned tensions in his oeuvre. First, there is a conflict with his 'official' deductivist programme insofar as reasoning by hypothesis entails conspicuously nondeductive inference, and second, there is a conflict with his empiricist programme insofar as reasoning by hypothesis entails conjecture beyond experience.

In the end, though, Gassendi relies on his empiricism to provide moorings for his

reshaping of traditional *regressus* method and his method of hypothesis. So his probabilist view, that our empirical beliefs may be warranted on the basis of evidence which gives them a probability less than one, is a primary premise of his view that we may maintain the viability of empirical demonstrations though we are uncertain about their conclusions. And his theory of signs, which licenses inferences about the non-evident on the basis of our ideas about the evident, is a primary premise of his view that we may reason by hypothesis about the non-evident yet appeal to empirical evidence in support of such hypotheses.

A. Gassendi's 'Official' Method of Resolution and Composition: The Regressus Recast

When Gassendi tells his 'official' methodological story in the Institutio Logica of his Syntagma Philosophicum, he follows in a tradition developed in the West since Aristotle, according to which we attain new findings and lend warrant to our claims in science by some combination of analysis and synthesis, or 'resolution' and 'composition.' To better understand this tradition, let's consider the theory of method fashioned by those Renaissance Italian writers who immediately preceded and greatly influenced Gassendi, as well as Galileo, and likely Descartes, too.

1. The Renaissance Tradition

In the Renaissance Paduan school of medical thought, several followers of Aristotle proposed a scientific method of discovery and justification known as 'demonstrative regress' (*regressus demonstrativus*), which combines the steps of determining causes from their effects and deducing such effects from said causes.

Primary exponents of this method such as Agostino Nifo and Jacopo Zabarella suggest these steps, rather than being circular when put together, actually allow us to find essential causes in the spirit of Aristotelian science, and in the equally Aristotelian recognition that it is not essences but the perceptually evident which is perspicuous to us. What steps could actually lead us from the perceptually evident to the essences of things, and give us a causal story to boot? Consider the two components of this *regressus* method, the 'resolution' or 'analytic' step (based on what Aristotle refers to as 'demonstration of the fact') and the 'composition' or 'synthetic' step (based on what Aristotle refers to as 'demonstration of the reasoned fact').² The first yields an unsure, conjectural grasp of a set of causes through analysis of (and, for some *regressus* theorists, induction on) the particulars of their effects, that is, our sensory data. The second yields a certain understanding of the effects as the demonstrated result of those causes. For example, given the framework of Aristotle's *De Caelo* one might seek the essential cause of the circular motion of the stars³ as follows:

Resolution step. Taking our observations which indicate that the stars move in circles, we seek the cause of this phenomenon through 'demonstration of the fact' (and possibly

²Nicholas Jardine points out that the Renaissance *regressus* theorists fashioned their method on the basis of Aristotle's discussion of these forms of demonstration in the *Posterior Analytics* 1.13. I am indebted in particular to Jardine's exposition of *regressus* method, in his "Epistemology of the Sciences," in *The Cambridge History of Renaissance Philosophy*, ed. Charles B. Schmitt, Quentin Skinner, Eckhard Kessler, Jill Kraye (Cambridge, UK: Cambridge University Press, 1988), 685-711.

³Aristotle develops his examples of demonstration of fact and of reasoned fact around a causal account for the claim that the planets do not twinkle (*Posterior Analytics* 1.13). I use an example which might more easily lend itself to the sometime *regressus* step of searching for the middle term through species-genus analysis.

induction on our observational data). The suggestion here is that, if we begin by considering the predicate 'moving in circles' as the effect, or middle term of a syllogism, all that remains in order to identify the cause is to determine the major and minor terms.

Thus:

(1) whatever constantly moves in circles has property *x*.

(2) y is something that moves in circles.

(3) y has *x*.

Now, from our celestial observations we can replace y in premise (2) as:

(2') stars move in circles.

and now all we need to do is solve for *x*, the cause (and minor term). This task, clearly the critical component of the resolution, is unhappily where the *regressus* theorists offer us the least precise or consistent guidance. Zabarella suggests we discover causes through the purely intellectual apprehension of universals⁴, Nifo proposes that this discovery is based on a demonstration from signs⁵; other possibilities include argument by elimination via disjunctive syllogism (proposed by Pomponazzi⁶) and Aristotelian species-genus analysis (used by Zabarella⁷). Using this last approach, we solve for *x* first by determining that the genus of stars is 'celestial bodies,' and their speciating difference is that they are composed of ether. We need go no further, for it is a cardinal point in Aristotle's De Caelo views that ether naturally moves in circles, such that anything made

⁴See in particular De Rebus and De Methodis.

⁵In Aristotelis, as cited in Jardine, "Epistemology," 688.

⁶See Jardine, "Epistemology." Jardine suggests Nifo may have embraced this method as well.

⁷And numerous others, including (as we see below) Gassendi. Zabarella's use of species-genus analysis is a bit sketchy; cf. Jardine, "Epistemology," 691-692.

of ether should naturally move in circles. Hence x is the property of being made of ether.⁸

Now, assuming (with Aristotle) that constant circular motion is natural as well, we have a syllogism where we reason from an effect to its cause:

(1) whatever naturally moves in circles is made of ether.

(2) stars naturally move in circles.

(3) stars are made of ether.

effect

cause

Our knowledge of the essential cause is at this stage only provisional, because it ultimately relies on sensory information we introduce in the second premise. This reliance suggests that, whatever the *proposed* method for discovering the cause, induction plays at least a background role in resolution because that discovery requires that we make some generalizations on the basis of our observed data. Most *regressus* theorists could accept a background role for induction in the context of their ostensibly deductivist resolution steps because they followed Aristotle in holding that we can actually gain certain knowledge from induction either through complete enumeration or by intuiting the relevant universal generalization. What makes the cause's identity after the reduction provisional for them is not the uncertainties of inductive reasoning but the recognition that other proposed causes might work just as well: the possibility exists that our resolution simply failed to pick out the right cause. Hence the need for a composition step, where we 'test' the identified cause to see if it leads, necessarily, to the observed effect.⁹

⁸Here the species-genus analysis yields a classification which is causally fundamental because, following *De Caelo*, the attribution of motion to ether is conceptually basic. That is, since circular motion for Aristotle is an essential feature of the stuff of stars (ether) there is nothing we can identify as a more basic cause of circular stellar motion.

⁹*In Aristotelis* f 6v, as cited in Jardine, "Epistemology," 689.

Composition step. The composition step entails a syllogism the object of which is to show that, by starting with the conjectured cause we are inextricably led to the observed effect. Thus for our example we now cast the provisional cause--being made of ether-- in the role of the middle term, from which we may deduce the effect:

(1) whatever is made of ether naturally moves in circles.

(2) stars are made of ether.

cause

(3) stars naturally move in circles.

effect

If the syllogism is valid, the *regressus* theorists suggest, then the effect must follow from the proposed cause and so the cause has been definitively identified and so distinguished from all other possible candidates. This last part is a bit of a mystery, since any candidate cause would also yield the observed effect given that the candidate is chosen through the resolution step and the composition syllogism is constructed correctly. The principle value of the composition step, it seems, lies mostly in affirming (what we should have recognized by this point anyway) that we have identified a plausible candidate in the first place.

Little wonder, then, that the overt history of the *regressus* method is rather brief, and not frequently featured in discussions of the methodology of the new science. Yet, as Nicholas Jardine (1976) has pointed out relative to Galileo, and as Daniel Garber's analysis (1993) of Descartes's methodology suggests, this Paduan method of discovery and justification lives on--in modified, covert form--into the early modern period. Perhaps, however, the most prominent early modern writer to embrace this method in a clearly articulated way is Gassendi, in Book IV of his Institutio Logica.

2. Gassendi's *On Method*: Resolution and Composition in the Contexts of Discovery, Justification, and Explanation

Gassendi never specifies that we need methods particular to the exact or natural sciences, but the theory he develops in the Institutio Logica suggests his primary methodological concern is the conduct of scientific practice in its various facets--discovery, justification, and explanation. To begin with, he defines method as "a progression of thoughts organized or arranged in a determined pattern".¹⁰ This odd way of putting matters is perhaps another unfortunate product of Gassendi's sometimes grotesque ways with Latin, but in any event it is reasonably close to the far more plausible suggestion that method is the set of guidelines by which we determine such patterns, and his subsequent discussion bears out this second possibility. Thus he claims that a theory of method should include all of logic, on the grounds that logic teaches us how to progress "methodically from simple thoughts through propositions to a conclusion arrived at by means of a syllogism."¹¹ Indeed, he thinks the central task of our method is to give us a basis for shaping the syllogisms we employ in his two preferred tools of scientific practice, resolution and composition. In this regard perhaps the most striking aspect of his method is the proposal that we choose to employ resolution or composition in accordance partly with the appropriate scientific practical context, and partly with the way we pursue our epistemic goals in a given context. And so he asserts in the beginning of Book IV that we need a method that is right for the different contexts in which we

¹⁰IL Book 4.

¹¹IL Book 4.

organize our thoughts--"sound enquiry and investigation, judicious analysis and assessment of what has been discovered, and formulation of the material in a manner appropriate for teaching it to someone else"¹²--that is, discovery, justification ('judgement'), and explanation ('instruction'). I consider each of these in turn.

a. Method in Discovery Contexts: How to Find the Middle Term of a Syllogism

Gassendi builds his account of discovery on a curiously scholastic view of how to think about questions:

When a question has been posed, it is particularly useful to find a middle term or argument which can be used to determine whether a positive or negative answer is true or false.¹³

Discovery for Gassendi, then, turns out to be a matter of finding answers to questions which can only be answered either in the negative or the affirmative, and he designs his corresponding method to help us determine the truth-values of just such answers. We might helpfully think of these questions as 'discovery hypotheses', where we have a candidate for a discovery only if we can judge whether it is true or false that an appropriately-phrased question is answered in the affirmative (or the negative). So, according to Gassendi, one such question as this method should help resolve is "Is man a substance?" And, presumably, his proposal should work for questions outside the purview of first philosophy, thus "Is the velocity of an object in free fall proportional to the time of the fall?"--though not "Is the velocity of an object in free fall proportional to

¹²IL Book 4.

¹³IL Book 4 Canon I.

the time or the distance of the fall?", for this cannot be answered simply by 'yes' or 'no'.

His proposed method begins like this: we consider such discovery hypotheses as the concluding lines of syllogisms, so that the predicate is the major term, the subject the minor term, and the only thing we don't know is the middle term. Maintaining a syllogistic framework to model our scientific reasoning and focusing our investigative energies on finding the missing term clearly preserves a key element of the *regressus* technique. But there is at least one distinctive element to Gassendi's approach. To solve for this 'hidden' middle term and thereby learn the truth-value of a candidate answer to our original question, Gassendi proposes that we follow the guidance of a sign.¹⁴ This suggestion is somewhat bare and not much argued but contains an important reference to his theory of signs: the key to finding truth-values for our discovery hypotheses, it seems, lies in grasping and interpreting indications of the non-evident (for example, underlying causes) among the evident phenomena.¹⁵

For the moment, let's simply tuck away this suggestion and consider the details of how the discovery method works. Take an acceptable question like "Is man a substance?", and convert it into the discovery hypothesis "Man is a substance"; to find out if this is true or false, we either pursue a resolution step which begins with the subject ('man') or a composition step which begins with the predicate ('substance'), depending on whether we are more familiar with the subject or predicate. Resolution for Gassendi

¹⁴IL Book 4 Canon I.

¹⁵As I note above, Nifo too proposes, en passant, an important role for sign-based inference in *regressus* method. But Gassendi differs from Nifo by having a well-developed theory of signs firmly rooted in his general theory of empirical knowledge.

entails a species-genus analysis, so that in this case we may discern that our hypothesis is true on the basis of *man* being a species of the genus *animal*, which is a species of the genus *living things*, which is species of the genus *bodies*, which is species of the genus *substances*. Composition entails a similar species-genus kind of reasoning, only in reverse; thus the genus *substances* includes numerous species but only one the difference of which corresponds to *man*, namely *body*, and so on through the genres *living thing*, *animal*, and *man*. By following either procedure we end up with a middle term which fits our syllogistic reconstruction of the reasoning that is required to discover the truth of the hypothesis, though each route may provide a different middle term, Gassendi notes. Here he suggests resolution yields *body* as a middle term while composition yields *animal*, which provides us with either a resolution-based or a composition-based discovery:

resolution-based discovery

- (1) whatever is man is body.
- (2) all body is substance.
- (3) man is a substance.

composition-based discovery

- (1) whatever is animal is substance.
- (2) man is animal.
- (3) man is a substance.

And in either case, Gassendi contends, identifying the middle term provides the key to unveiling the truth-value of the hypothesis, by establishing the primary supposition necessary for the rest of the resolution or composition step to follow suit.¹⁶

One might think at this point that either resolution or composition alone should suffice to reveal the truth-value of our discovery hypothesis. This depends in part on

¹⁶II. Book 4 Canon I.

whether whatever plays the role of middle term consistently serves as the crucial assumption in those *regressus* steps, and whether pursuing one of those steps in particular is highly correlated with consistent, viable truth-evaluations of our discovery hypotheses; these assumptions would need to be put to the test. But Gassendi ignores these matters and, at certain points, goes further in developing this methodological architecture, preserving the *regressus* theorists's link between resolution and composition through the claim that the composition step is the reverse form of the resolution step. This move suggests the composition step is a surefire way to generate results complimentary to our resolution step, but that presents a major difficulty if he wants such a complimentary relation to be central to his account of a viable discovery method.

To see why, we need look no further than Gassendi's own comparison of resolution and composition steps in his discovery method to those he thinks are fruitfully employed in geometry. Thus he tells us 'geometers' use a form of resolution when they assume as true a proposition with an as-yet unknown actual truth-value, and accept the initial proposition as actually true if they can deduce what are otherwise recognizable as true propositions ("something which is true, is agreed to be true, and is virtually a first principle"), and reject the initial proposition as actually false if they can deduce false propositions from it. This much might accurately characterize the 'geometers' in his acquaintance, though their set of 'virtual' first principles should have been rather bloated considering that we can deduce true propositions from false ones. At any rate, Gassendi further holds that 'geometers' employ a form of composition when they reverse the steps of a resolution-based deduction, taking in this context the conclusion as the primary

supposition to show that the primary supposition of the resolution follows deductively from it. The striking and strikingly implausible outcome of this view, then, is the suggestion that, in the natural sciences as in geometry, if there is a possible deduction x of proposition ϕ_n from an ordered sequence of propositions $\Gamma: \{\phi_1, \dots, \phi_{n-1}\}$ and ϕ_1 is the primary supposition of x (such that all other propositions of Γ ultimately follow from and rely upon ϕ_1), then there is a possible deduction y of ϕ_1 from a set of propositions Γ' where Γ' contains all members of Γ minus ϕ_1 , plus ϕ_n , and in reverse order of Γ , and ϕ_n is the primary supposition of y . This is not even true of geometry (otherwise we could deduce any manner of would-be 'axioms'), much less the natural sciences,¹⁷ and so it cannot work in either field to say that the two steps will lead to complimentary results. If we are to keep any of this method, then, we have to at least jettison the suggestion that resolution and composition directly compliment one another.

Happily for Gassendi, he never develops this poorly conceived analogy into the requirement that resolution and composition *must* bear these relations for any given middle-term search. Instead, he recommends that we pursue one step *in lieu* of the other, and that which one we choose depends on our relative familiarity with the subject and predicate of the discovery hypothesis (assuming we are not equally familiar with both, in which case it is not apparent how we are able to choose). So we might best understand this geometry comparison as an (ill-fated) analogy, without any commitment to imposing the link between the steps Gassendi thinks he has found in geometry onto the same steps

¹⁷Though if it *were* true of geometry it might not be totally surprising for Gassendi to hold it true of the natural sciences as well since he thinks geometry along with the entirety of abstract reasoning has an empirical basis and so is a species of empirical knowledge.

we find, very much separated, in other scientific pursuits.

But what of the search for the middle term, upon which rests our judgements as to the truth-values of the discovery hypotheses? Here, too, there are problems. For one, the prescription of a species-genus analysis (at least according to Gassendi's example) supposes all the information we seek in order to elect a middle term is included, in a fashion, in our conception of either the predicate or subject of the discovery hypothesis. But this supposition founders in the case of countless empirical hypotheses, e.g. 'cows with brown spots eat more than cows with black spots.' For another, we cannot pursue a species-genus analysis or synthesis if the predicate and the subject of the discovery hypothesis are not plausibly in any species-genus relation, as in 'twenty percent of all lemmings are not suicides.' Aside from these practical difficulties, it is not entirely clear what, in the context of empirical discovery, the structure of the syllogism contributes to finding out whether our discovery hypotheses are true or false.

Yet the big puzzle in successfully completing a resolution or composition step is how we are supposed to pick a particular term at the appropriate classification level (genus) to be the middle term. It is not much guidance to say the middle term is the one that allows us to fashion the primary supposition of a resolution or composition, if we don't know in advance what that supposition should look like. And if we did, there wouldn't be much point to going through the resolution or composition steps to begin with. But Gassendi thinks there is a way to pick out the middle term, and its place in the syllogism, with the help of sign-based inference. Though it is not wholly clear what he intends here, a couple of possibilities may be mentioned: (i) although the way we define

the differences that mark speciation is nowhere discussed here, it seems safe to assume, following Aristotle, that our observational data plays a key role, and in this respect sign-based inference could help us find 'hidden' clues to speciation among our perceptually evident data; and (ii) if our discovery hypotheses suggest causal relations, we may identify middle terms which were previously 'hidden' causes from their effects (as described in the hypotheses) which serve as signs of those causes. Even if these speculative remarks do not quite match, or perhaps exceed the details of, what Gassendi has in mind, his simple proposal that our scientific discoveries may rely on (sign-based) inference which takes us beyond what is evident to the senses suggests a nuanced difference with previous methodological views according to which we make discoveries through the senses alone or through reason alone.

b. Method in Justificatory and Explanatory Contexts

The assessment of procedures we pursue to arrive at discoveries is what Gassendi calls 'judgement.' We judge that a procedure lends warrant to our discoveries just in case its essential reasoning can be reversed and we can attain the new conclusion from the old one plus any intermediary suppositions. The hallmark of judgement, then, is that we reverse whatever step we pursued in the corresponding discovery context--if we proceeded by resolution in our discovery process then we proceed by composition in our judgement and vice-versa. Straightaway we can see a parallel between this suggestion--that resolution and composition compliment one another across methodological contexts--and the earlier, failed attempt Gassendi makes to link resolution and composition *within* the

discovery context. But here he views the discovery and judgement procedures as distinct, yet related in the way, as he puts it, Theseus views Ariadne's thread--the discovery procedure is a set of clues left behind in the process of leaving some initial state and coming to rest at a new state such that, by retracing those clues in the judgement procedure, we can attain the initial state. Hence the reversal of our discovery step constitutes the judgement step for Gassendi because he thinks retracing that first step enables us to assert the viability of the method employed to make the discovery.¹⁸

His model for this sort of procedural check is no longer geometry but arithmetic, where, for example, we can see if a series of addition operations warrants a particular result by reversing the series and substituting subtraction for addition.¹⁹ Gassendi unfortunately assumes here that all series of steps we employ in discovery and judgement contexts are reversible with the same results we can get in reversing series of arithmetic operations. But we can't insist that our series of discovery or judgement steps have this feature too unless we stipulate that any such series of steps which constitutes a valid bit of reasoning has a valid mirror-image or even near mirror-image counterpart. And this is simply false--consider the valid syllogism

All z's are x's, some z is a y, therefore some x is a y,

its invalid mirror-image counterpart

¹⁸II Book 4 Canon III.

¹⁹This procedural feature, which also holds for multiplication and division, relies on the more fundamental property of any pair of commutative binary operations # and $\odot$ where # is the *inverse* operation of $\odot$ and vice-versa, such that whenever $a \# b = c$, then $c \odot b = a$ and $c \odot a = b$. Another model Gassendi proposes here is chemical reactions, which he recognizes as reversible in the case of 'mixing' and analyzing metal compounds. II Book 4 Canon III.

Some x is a y, some z is a y, therefore all z's are x's,

and invalid (near) mirror-image counterpart

Some x is a y, all z's are x's, therefore some z is a y.

So it cannot be that a viable judgement method consists in a simple reversal of the discovery procedure. Yet this view of the relation between the epistemic contexts of discovery and judgement (really, justification) is not wholly without merit. Independent of the particular relations Gassendi suggests, it requires some imagination--particularly in the constrained *regressus* framework--to propose that, for each task realized in either of these contexts, the corresponding method is either one of two options, and that the choice of the best method for a task of one given context is an inverse function of the choice made relative to the corresponding task of the other context. We don't tend to think the contexts of discovery and justification are related in just this way--if indeed we think they are significantly distinguishable to begin with. But given the suggestion that we do pursue different methods relative to each context, Gassendi's view illustrates one way to see them as truly complementary, such that understanding the tasks realized in one should enable us to understand those realized in the other.

In addition to proposing that judgement inverts, and so serves as a formal check on, the procedure of discovery, Gassendi also suggests that we 'verify,' or judge the particular merits of, a given discovery on the basis of our experiential evidence. Our discovery claims are 'verified' first and foremost by sensory evidence and only secondarily, if it is at all necessary, by the lights of reason. Thus interpreting signs sometimes requires the use of reason (though as Gassendi emphasizes, the source and

strength of our reasoning lies in its empirical origins), but quite frequently our reasoned judgements lead us to affirm claims which we can later show are empirically false. The example he cites is the case of observations which demonstrate that an arrow shot straight up from the stern of a moving ship falls not into the ship's trail in the sea (as we might have reasoned, he suggests) but onto the stern itself.²⁰ As this case illustrates, one danger of judging empirical claims--or what he is broadly construing here as "discoveries"--by our reasoning alone is the possibility that we base our reasoning on the wrong premises, much as the standard Aristotelian account of the arrow shot from the ship is based on the wrong physics. Gassendi upholds sensory evidence as the primary tool of judgement, then, because he worries that a justification of empirical claims (or what he calls "judgement") by the lights of theoretically-derived reasoning does not feature the procedural check of independent testability which is built into our experiments and observations.

Finally, Gassendi proposes a theory of method in the context of instruction, or what amounts, at least in part, to explanation. Here he distinguishes between explaining practical operations or skills, with the associated objective of teaching correct performance or production, and explaining theoretical investigations, with the associated objective of teaching correct observation. In either case, he suggests, there are, correspondingly, both resolution *and* composition steps. Thus in the physical sciences, we begin explanations by 'resolving' natural phenomena and structures into their 'smallest

²⁰Here Gassendi alludes to the kinematic experiments he details in his De Motu, such as dropping weights from the height of a ship's mast.

possible components,' and proceed by 'composing' those components into the combinations that yield familiar macro-phenomena and macro-structures.²¹ Since explaining these kinds of relations and events entails explaining empirically-viable theoretical models, our objective in explaining scientific knowledge must be to teach how to observe. This follows from his view of science as a contemplative and non-creative exercise, where all performances or productions are in the exclusive hands of the Creator. Such a view is also consistent with Gassendi's view of astronomy--a contemplative science par excellence--as a model for scientific method. Yet it curiously also leaves intervention outside of empirical science, which in turn suggests little room for experiment. This must be an oversight or else a place where his 'official' method and his practical science do not mix. Far more curious still is the suggestion that the explanatory task most worth clarifying is not addressing how- and why-questions about which scientists typically wonder, but addressing the question of how, technically, we should have come to accounts we present in executing the other epistemic tasks of discovery and justification.

In sum, for each of these epistemic contexts of science Gassendi distinguishes a particular task and proposes a corresponding procedure that employs either resolution or composition (or, in the case of explanation, both). We may consider, by contrast, Descartes's suggestion that resolution and composition play fixed, distinctive roles in a unique method for establishing answers to scientific questions. According to his proposed method, we should be able to analyze a complex question into questions of

²¹IL Book 4 Canon V.

greater simplicity, until we reach a question sufficiently simple that it can be answered by an intuition of the mind--this is his reduction or analysis step. After this point we should be able to answer questions of greater complexity by a process of deduction, through a chain of intuitions in which we grasp the relations of, and warrant for, the propositions involved²²--this is his composition or synthesis step.²³ By following these steps, Descartes proposes, we can at once identify causes from their effects (a discovery task), deliver the warrant for believing that we have identified the correct cause (a justification task), and produce an account that answers pressing scientific questions (an explanation task). Thus his method weaves together the principle epistemic tasks of science into one seamless process. But for Gassendi, the tasks of confirming or disconfirming discovery hypotheses and judging our discovery-results do not constitute one inseparable process, and those methods corresponding to the tasks of the distinct contexts are not parts of some single procedure. Indeed, he insures that there can be no single, seamless procedure when he dissociates particular tasks from the designation of resolution or composition as

²²Descartes typically takes 'deduction' to mean our immediate comprehension of the inferential links between propositions (and their truth). As Daniel Garber notes, this cannot correspond to our modern notion of deduction; see his "Descartes and Experiment in the *Discourse* and the *Essays*," in *Essays on the Philosophy and Science of Rene Descartes*, ed. Stephen Voss (Oxford: Oxford University Press, 1993), 292.

²³Cf. the Rules for the Direction of the Mind. Whether one question is an *appropriate* simplification of a more complex question is a function of whether an answer to the proposed simpler one would deductively provide an answer to the more complex one. And given that there are many possible ways to deduce such answers, we need some way to identify the correct deduction; here Daniel Garber has helpfully proposed that Descartes takes experiment to play this role, in the Baconian manner of crucial experiment. But for whichever answers we attain, we have a foundation (either in basic natural facts, as per the Rules; or metaphysical principles as per the Discourse and the Principles) we can take as certain (either because we have reached them by simple intuition, as per the Rules; or because God guarantees them, as per the Discourse).

the proper methodological step. For example, though we execute a discovery task through one step and simply reverse that step to execute the corresponding justification task, it depends on our background information as to whether we employ reduction or composition in the first place. And, as another difference from the integrated cartesian model, we combine these two steps to execute the peculiarly Gassendist and metascientific 'explanatory' task where what we explain is how we arrive at our discoveries and judgements.

Let's summarize the key elements of this 'official' view so far. For one, Gassendi's insistence that our inferences adhere to syllogistic form, of course, conforms to the logic of his day, but in the context of spelling out a scientific method this yields a strongly deductivist picture of the practice of science or, at least, of the way we are best supposed to understand or, perhaps, rationally reconstruct that practice. This might seem to suggest that Gassendi has a strong *apriorist* streak to his scientific method; one further piece of evidence for this is his proposal that resolution best proceeds by species-genus analysis where we have a good grasp of the relevant essences. However, as I argue in the following section, Gassendi has a manifestly non-*apriorist* conception of deductive inference which, quite distinctively for his times, suggests such inferences are merely probable, on the grounds that their premises always derive from sensory claims.

An additional element of this 'official' method is that Gassendi appears enthralled by the possibility that analysis and synthesis might seamlessly be thought of as inverse methodological steps, and he moves from the non-starter suggestion that this inverse relation is plausible within the context of a single given discovery, to the only slightly

more promising proposal that, if one of the steps is executed in a discovery context and the other is executed in a corresponding justification context, then the two steps should complement one another as inverse operations. This relation of the *regressus* steps for Gassendi brings out the true character of the relation between resolution and composition: the reasons for evaluating a claim as worthy now should mirror (literally, it seems) the reasons for coming to that claim. This is undoubtedly what the *regressus* theorists who precede him have in mind by tying the two steps together in the way they do, and this also seems to be what is going on in Descartes's theory of method (albeit in an altogether different way). To be sure, we surely want a methodology where the way we judge the strength of our empirical claims may, where it is helpful, take careful account of the way we established those claims to begin with. Unfortunately for all of these thinkers, and for Gassendi in particular, however, there's no reason to think we need to do this by recreating that method of establishment in reverse.

In addition to his suggestions for tinkering with the orthodox *regressus* programme, Gassendi proposes a departure from the accepted scientific method of his day when he says we should (i) employ signs of the non-evident to discover crucial and missing information in discovery contexts, (ii) rely on the senses to render final judgement in any contest between reason and the senses over justification of our empirical claims, and (iii) explain how we attain our empirical claims by teaching how to observe. Each of these last proposals calls our attention to something missing in the received *regressus* method: detailed guidelines or rules for gathering evidence and judging its support for our empirical claims. In the end, though, these particular

proposals are too vague to add much lustre to Gassendi's refashioned *regressus* method. One way to greater specify these proposals would be to supply guidance relative to the attainment and judgement of basic information underlying empirical belief and the inferences by which we establish scientific claims--both of which feature the uncertainties that are not accomodated by traditional views of deductive inference, syllogistic or otherwise. This is the kind of guidance we look for in a theory of nondeductive inference, which Gassendi fails to specify in Book IV.

c. Induction and Probability in the Institutio Logica

However, even if Gassendi does not talk about nondeductive inference in Book IV, he elsewhere recognizes the critical role, in resolution tasks, of inferring general claims from the particulars of our empirical data, not least from the evidence of signs. Further, he admits our resolution-based claims are merely probable insofar as they represent individual possibilities among a field of alternatives. Thus in Book III (On the syllogism) Gassendi suggests that some class of syllogisms model our inductive reasoning, and another class model our probabilistic reasoning. With these suggestions Gassendi builds into his method some features suitable to his own empiricist scientific practice and thinking, and quite distinct from the methodological thought of other Renaissance and early Modern writers: the recognition that our inductive inferences inherently lack the information we would need to be certain of the claims they suggest, and the proposal that not even deductivist models of scientific inference can insure our certainty about empirical claims because the experientially attained premises we adduce

in support of such claims are no greater than probable.

We might think, on the basis of this last notion, that Gassendi has a good enough seventeenth century grasp of inductivist logic, and that it's rather deductivist logic he doesn't fully understand. Yet, while something is surely amiss in calling deductivist inference 'probabilistic', it seems Gassendi has hit upon a sensible point as well--that the use of deductive reasoning in empirical contexts, while providing certain formal guarantees, does not insulate empirical arguments from judgement by the measure of belief we invest in their premises. At the heart of this view (which, apparently, escapes both Descartes the deductivist and Bacon the inductivist) is a notion suggested by his understanding of reasoning with probability, that the strength *all* empirical claims share is the warrant from experience for any further claims we introduce in their support.

The roots of Gassendi's views on probabilistic reasoning lie, naturally enough, in his views on probability, which in turn stem from his account of empirical knowledge. Let's recall his suggestion that we are justified in (a) inferring claims about one set of natural things and events on the basis of 'signs,' or the sorts of evidence other sets of natural things and events provide, independent of intermediary personal authority or testimony,²⁴ and (b) characterizing judgements we make about events as more or less probable on the basis of the frequency of those events. These two items are crucial

²⁴This is what Arnauld and Nicole, Hacking (1975), and Garber & Zabell (1979) call 'internal evidence'. See Antoine Arnauld and Pierre Nicole, The art of thinking: Port-Royal logic, translated by James Dickoff and Patricia James (Indianapolis: Bobbs-Merrill, 1964); Ian Hacking, The Emergence of Probability (Cambridge: Cambridge University Press, 1975); and Daniel Garber and Sandy Zabell, "On the Emergence of Probability," Archive for the History of Exact Sciences 21 (1979): 33-53.

components of what Ian Hacking calls the 'modern' dual-aspect view of probability. As Hacking suggests, accepting the first item is a primary step towards accepting a 'degrees of belief' concept of probability,²⁵ accepting the second item is a primary step towards accepting a 'relative frequency' concept of probability,²⁶ and these two concepts constitute the dual-aspect view.²⁷ Indeed, something very like a 'degrees of belief' conception of probability appears to inspire Gassendi's proposal that we have warrant for inferences about things and events though we lack certain knowledge of them, and thus his proposal

²⁵Hacking contends that acceptance of the sorts of evidence we may adduce from signs contributed to the development of a 'degrees of belief' concept of probability because this acceptance signalled the broadened applicability across scientific areas of study of such earlier (generally legal) doctrines which take claims to be more or less believable in proportion to the credibility of the available evidence (generally personal witness). When it was suggested that the 'Book of Nature' could be 'read' in such a way as to yield analogous 'witness' for natural things or events not actually perceived, this opened the door for the assessment of all manner of naturalistic claims--previously characterized in the middle ages as either certain knowledge or else unwarranted belief--as credible in accordance with the likelihood ascribed to the available evidence; see his *Emergence of Probability*. Beyond Hacking's historical point, it is critical to stress here that the evidence we cull from signs--particularly indicative signs--has a substantial interpretive component, such that the concept of probability the theory of signs should be taken to support is precisely one which says the probability of a claim is the measure of the extent to which we find it to be credible--which is generally conceded to be an at least partly subjective function.

²⁶It is a surprisingly recent development, so Hacking proposes, to suggest that a claim as to the character of an event has greater or lesser probability as a function of the relative frequency of such events. Hacking argues that this notion of probability arose from such practical considerations as the study of games of chance and insurance statistics, as well as from the epistemological assessment of those claims for which we lack complete evidence as a principled affair. Gassendi's views, I propose, offer an early instance of the latter.

²⁷Daniel Garber and Sandy Zabell reject the notion that there is any difference between antique and modern probability concepts on the grounds that these elements which Hacking places at the root of the 'modern' view can be found in sources ranging from Quintillian to John of Salisbury. Yet they accept the notion that this concept, whether we want to call it 'antique,' 'modern,' or something else, bears these two aspects. What they think is peculiarly modern, by the way, is (i) the notion, as per Bernoulli, that evidence can be quantified and not simply qualified, and (ii) burgeoning growth in mathematical study of the theory of games of chance; see their "On the Emergence of Probability," 44, 48-50.

that claims supported by such inferences are "...not absolutely definite but assented to with a degree of uncertainty and hesitation."²⁸ Claims of this sort are, in the scholastic idiom, opinions (*opinio*): somehow greater than ordinary unevidenced belief if something less than certain knowledge.

Gassendi also offers an account of inductive reasoning, where he suggests we may infer claims about which we are less than fully certain. Yet he doesn't think induction is a probabilist enterprise, because his account of probabilist reasoning is actually deductivist. This is possible because his main concern about the less than certain nature of what he calls 'probabilist' reasoning is not with the conditional probability of some claim given our assumptions, but with the probability that we are wrong about those assumptions to begin with. And just in case inductive and this sort of probabilistic reasoning seem to meet--as when we embed background inductive reasoning in assumptions of demonstrative syllogism--Gassendi thinks our inferences count rather as deductivist reasoning. In short, to call a piece of reasoning 'probabilistic', it is not enough that we can evaluate the merits of the conclusion given our degree of belief in its less-than-certain supporting premises (that *would*, of course, make inductive reasoning 'probabilistic' as well). Rather, a piece of reasoning is 'probabilistic' for Gassendi only in the sense that we draw upon that degree of belief to suggest the fallibility of the conclusion though we recognize that our argument is a valid one; and, in the context of his logic, this recognition occurs only when the argument is an instance of viable deductive syllogism. Whatever the flaws of this view (which I note below), it is noteworthy that Gassendi

²⁸II Book III Canon XVIII.

attempts to fit a suitable ('modern') probability concept into a general theory of what makes for adequate reasoning patterns. It is further noteworthy that he focuses here more on the content of our inferences and less on their formal character, and that he sticks to inclinations he displays in his modified *regressus* method by suggesting we make room for probabilist inference without leaving behind the demonstrative force of deductive syllogism. This deductivist persistence *might be* a consequence of his recognition of induction's insecurities. His attempt to characterize a certain class of deductive syllogism as 'probabilist' (however quixotic) is *surely* an outcome of his global empiricism.

i. Inductive Reasoning

In Canon XI of Book III of the Logic, Gassendi takes enumerative induction to be the principal means of inductive inference featured in scientific reasoning, and points out that such induction falls short just in case we can't enumerate all the instances upon which we wish to generalize when making such inferences. In this respect he diverges from the logicians of his day, who (following Aristotle) propose that we *can* get from our set of instances to a generalizing conclusion--via an intuition of the relevant universal. At the same time, he also anticipates an important element of Hume's problem with induction, that justifying any generalization on particulars requires something that may lie beyond our cognitive powers, our empirical knowledge of all such particulars.

So what exactly is induction for Gassendi? He describes it as a kind of reasoning which, in his typical fashion, he models along the lines of syllogism, and the salient feature of which is that the concluding line is a generalization on enumerated members of

a given class. Each such inference relies on the generally unexpressed but necessary premise that all members of the class so characterized have been enumerated. This much resembles the prevailing Aristotelian view. He steps away from that view, however, when he suggests that it is this reliance which produces the principle difficulty with inductive inference: we could never insure that we have given such an enumeration. If we could, then our reasoning would be demonstrative and thus of an altogether different sort.²⁹ One way around this difficulty, Gassendi proposes, is to posit the conformity of all remaining unenumerated particulars. This proposal (which he attributes to Lucretius and Horace) nevertheless has only the force of a helpful supposition, and his only suggestion as to its warrant is its utility: "...because...it is exceedingly difficult, not to say impossible, for there to be a complete enumeration, when some have been enumerated...you suppose that apart from those enumerated there occurs none which is different."³⁰

In addition to classical enumerative induction, there are two other forms of reasoning Gassendi suggests are cases of inductive inference; argument by example and argument by witness or testimony. Argument by example easily fits into the class of inductive reasoning because it entails the enumeration of one case plus the implicit inference rule that we should generalize on the single case given. Argument by witness or testimony is even closer to the classical sort of inductive reasoning, the principal difference being that collecting witness or testimony are special, second-hand instances of the practice of enumerating bits of evidence on behalf of a claim. Thus he believes we

²⁹II Book 3 Canon XI.

³⁰II Book 3 Canon XI.

might accept that the sun's size is greater than the earth's on the basis of testimony from Archimedes and other mathematicians simply because we accept the premise that "*What Archimedes and the rest of the mathematicians hold must be accepted as true on the grounds that they speak as experts in the science.*"³¹ Yet it is not entirely obvious why we should accept what is ostensibly argument from authority as a viable sort of inductive reasoning (let alone a viable sort of reasoning altogether) unless, as with argument by example, we are allowed to generalize on a fairly narrow set of cases. Something like this must be going on here since Gassendi holds that what links all these forms of inductive reasoning is their common inferential move, which, he stresses, is at the same time their common weakness. Each purports to take us from particulars to generalizations yet each is missing a premise: the elusive generalization step.

ii. Probabilistic Reasoning

By contrast, Gassendi does not think 'probabilistic reasoning' is missing any premises--nor does he think it is, in the main, nondeductive. He proposes that what makes a bit of reasoning probable, or 'suasory,' is that its premises are contingent and therefore persuade us of the conclusion's truth without making that truth necessary.³² The measure of strength for such inferences is a function of the degree of clarity and certainty we attribute to the premises, rather than the conditional probability of the conclusion given that the premises are true. By adopting this measure Gassendi classifies as

³¹IL Book 3 Canon XI.

³²IL Book 3 Canon XVIII.

'probabilistic' *all* deductive inferences from empirical premises, and his reason for this classification is that we base such premises on information from the senses which, however otherwise well-warranted, never warrants our certainty. As an example designed to illustrate *how* such uncertainty may arise, he cites the cases where our being persuaded by some reasoning relies on our trust in the person presenting the reasoning. In these cases, he points out, our awareness that the presenter may be unreliable or even deceitful should lead us to withhold certainty from the premises presented, hence to any claims thereby inferred.³³ One problem here is that, if this is all it takes to render a piece of reasoning as 'probabilistic', then presumably any inference where it is merely possible that the premises are false, and not just those based on empirical claims, could count as well. Thus, despite Gassendi's apparent intentions, this view fails to pick out anything special about deductive inference in specifically empirical contexts. Moreover, it is a further consequence of this view that there are no definitively false inferences any more than there are definitively true ones. An inference such as

(1) Primes are divisible only by themselves and one.

(2) Four is prime.

(3) Four is divisible only by itself and one.

should be considered as merely 'probabilistic'--in this case, likely to be false--because the conclusion is based on a premise that we cannot be certain is false (recall Gassendi's proto-Millian stance towards the learning of mathematical truths and our resulting certitude about them). At this stage, it should seem that Gassendi understands 'probabilistic' reasoning to cover an extraordinary and not terribly useful range of cases.

³³IL Book 3 Canon XVIII.

But Gassendi actually conceives of 'probabilistic' reasoning more narrowly. For one, inductive reasoning cannot count, surprisingly enough, just because it is missing the all-important generalization step. Indeed, it turns out that a given piece of reasoning is persuasive and contingent in what he thinks is the *right* way only if it meets specific criteria which only deductive syllogisms can satisfy, such that any inference we call 'probabilistic' we must already classify as a species of deductive syllogism. So what does he think is the 'right way'? His criterion for determining that reasoning is probabilistic requires that we identify the standard sort of relation of the middle term to other terms, which is only possible for viable deductive inferences in syllogistic form. Specifically, Gassendi understands a claim to be certain only if the inference by which we attain it is certain, as merely probable only if that inference is probable and, with this understanding, prescribes these guidelines: (1) any claim we validly infer by conjunctive figure syllogism is certain just in case the middle term necessarily agrees with--that is, is the genus, property, or some other crucial feature of--the subject and the predicate, (2) any claim we validly infer by discrete figure syllogism is certain just in case the middle term necessarily disagrees with--that is, lacks any such feature of--the predicate, and (3) any other claim we validly infer by any other figure syllogism is merely probable.³⁴ What counts as a claim about which we may be certain, then, is not simply that it follows a bit

³⁴II Book 3 Canon XVIII. Conjunctive figure and disjunctive figure are two kinds of categorical syllogism, that is, syllogisms where the order is subject, middle term, predicate. In the former kind, there are three affirmative propositions where the first features the agreement of the subject and middle term, the second the middle term and predicate, and the third the subject and the predicate. In the latter kind, the first proposition is affirmative and the second two are negative, such that the subject and middle term agree but the middle term and the subject disagree with the predicate.

of reasoning in some canonical form of valid syllogism, but that some necessary relations additionally exist among the parts of the syllogism. Thus in the (conjunctive figure) syllogism:

- (1) Socrates is human.
- (2) All humans are animals.
- (3) Socrates is an animal.

the conclusion is certain because 'human' (the middle term) is the genus of Socrates and the species of 'animal', and so necessarily agrees with each.³⁵ On the other hand, in the (conjunctive figure) syllogism:

- (1) Rhetoric is an art.
- (2) Every art is useful to life.
- (3) Rhetoric is useful to life.

of identical form, the conclusion is no more than probable because 'usefulness' is neither the genus of art (the middle term) nor a necessary or universal property of art, such that line (3) cannot follow from (1) and (2) without some residual doubt. As Gassendi puts it, "...the mind is unable to assent to the conclusion without a degree of doubt; nor can the premises bestow upon the conclusion greater clearness and certainty than they themselves possess."³⁶

But by standard lights, in the syllogism just cited (3) follows *without doubt* from (1) and (2)--and in this it does not differ any from the first syllogism--from which we might conclude that Gassendi simply doesn't understand what it means to call reasoning

³⁵Strictly speaking, we cannot be certain of the conclusion here according to Gassendi because the premises are really derived from experience and not, as the Aristotelians would have it, by intuitions of those universals which yield the species-genus relations in question.

³⁶II Book 3 Canon XIX.

'probabilistic.' Then we can easily view this set of views as a peculiar, incoherent slip in the Institutio Logica. But it is more reasonable to suppose that he instead means to characterize any (deductivist) reasoning as 'probabilistic' where the actual truth-value of the premises is a contingent affair, and where he takes this very possibility that the premises may be false to entail that we cannot be certain of the claims we infer on their basis. In modern parlance, what he is really doing, then, is picking out the class of inferences which yield arguments we are unable to judge as sound because their premises have undetermined truth-values. If this is right, then calling a bit of reasoning 'probabilistic' for Gassendi apparently reflects the limits on the conclusiveness of our epistemic judgements about such contingent matters as appear in the premises more than it reflects any structural merits or deficits of the reasoning.³⁷

One oddity in this view should arise in cases where premises of what Gassendi takes to be 'probabilist' deductive inference incorporate or rely upon what we (and Gassendi) accept as inductive inference. Consider the following inference, based on enumerative induction, which Gassendi accepts as a viable instance of 'probabilist' reasoning:

- (1) A speech of Cicero has an elegant exordium, narration, confirmation, confutation, and peroration.
- (2) Whatever has these things persuades.
- (3) A speech of Cicero persuades.

Here, too, the premise (2) sometimes fails to be true, Gassendi warns, with the

³⁷Even so, that he sees reasoning as 'probabilistic' only if it is deductive to begin with suggests he has *some* notion of the priority of structure to content in the assessment of reasoning patterns (though he does not quite present it here with the precision of the modern concept of soundness).

result that "the conclusion is not a necessary one."³⁸ In this case, the inference is 'probabilistic' and, it seems, inductive. It is not, however, elliptical in the usual (offending) way, for the premise (2) provides the oft-missing generalization step. If cases of induction by this sort of "enumeration of parts" appear to us to count as what Gassendi takes to be induction and 'probabilist' inference, this is not to say there are probabilist inferences he himself would concede are inductive. For though we would see such cases as canonical instances of inductive inference, Gassendi lights upon the fact that they supply the generalizing premise which he takes to be the mark of demonstrative (and thus deductive) syllogism. And so he understands those cases as 'probabilistic' deductive reasoning which incorporates complete enumerations--where it is perhaps the key 'probabilistic' feature of such cases that the enumeration is truly tendered as complete.

iii. 'Probabilist' Reasoning and Demonstrative Scientific Method

The suggestion that a bit of reasoning is 'probabilistic' if it is deductivist in form and its premises have contingent truth-values has a curious result for Gassendi's picture of scientific method, namely, that all our empirical claims--even those he believes we reach by deductive inference--are probable at best. After all, we base reasoning to empirical claims on contingent premises, at least in the context of discovery where, whether we pursue resolution or composition steps, our assumptions are at least partly derived from experience.³⁹ Now, we might find this result curious because it seems to be inconsistent

³⁸II Book 3 Canon XIX.

³⁹Accordingly, for Gassendi, the only *non-probabilistic* deductivist inferences--because they are the only ones whose premises are not derived from experience--are those concerning canonical

with the goal of lending a sense of surety to scientific reasoning--his ostensible motivation for adopting a *regressus* account of scientific method. But there is nothing troubling about this result for Gassendi, whose general theory of empirical knowledge, we have seen, includes the notion that there is *nothing* we know about the world with certainty. There is also the echo here, in typical Gassendist fashion, of the ancient sceptical worry that we can know nothing on the basis of syllogism on the grounds that we have no viable reason for accepting our initial premises as true. Yet Gassendi rejects the Sceptics's view that warranted belief entails certainty about those beliefs and so he can accept that such probabilistic empirical judgements may be warranted. In the end, like Glanville, Mersenne, and other early moderns influenced by skeptical thought, he too tailors his scientific method to ancient caveats regarding knowledge from the senses. Thus it's not surprising that Gassendi insists reasoning employed in scientific method is 'probabilistic' (in his sense of the term) because it is based on contingent premises. It is also not controversial, since such reasoning generally is based on contingent premises. What *is* controversial or, minimally, quite unusual, is holding this view and all the while taking scientific method to be, in the main, a *deductivist* enterprise.

Other prominent seventeenth century writers on method avoid this sort of controversy by characterizing empirical reasoning as either inductive or else 'classically' deductive; Cartesian method is classically deductivist, for example, whereas Baconian method is squarely inductivist. Indeed, Bacon expressly rules out syllogism as a model of scientific reasoning. He suggests there is an unacceptably high risk that 'notions', or the

theology and cosmology.

concepts underlying the words of a syllogism's propositions, represent the data of experience in inaccurate or overly-general ways. The classic defense of syllogistic method in scientific reasoning--which recurs in Gassendi's Logic--is that it provides a structurally secure inferential framework, and thereby sufficient warrant, for our empirical claims. In response to such a defense, Bacon points out that this structural strength can never guarantee such claims without directly appealing to our surest ideas, which concern the particulars of experiential data. Yet all such deductivist reasoning is founded on rash generalization, and so he concludes, "our only hope therefore lies in a true induction."⁴⁰

Descartes, for his part, thinks deductivist inference is the premier mode of scientific reasoning, and induction in his view (in the Regulae) merely helps us attain those generalizations which serve as the starting points of our deductions. Thus he agrees with Gassendi that the deductions of scientific reasoning are based on premises we ground in experience, though he dismisses the idea that this makes the deductions probable in any fashion. After all, we come to those premises on the basis of intuitions concerning such experience -- about which we may be certain.⁴¹ For Descartes no more

⁴⁰"... if the notions themselves (which is the root of the matter) are confused and over-hastily abstracted from the facts, there can be no firmness in the superstructure.... (Novum Organum I xiv). Bacon also differs from Gassendi in that he advances an eliminative model of induction, not an enumerative one.

⁴¹Daniel Garber proposes that in the Regulae these intuitions are supposed to be rooted in natural philosophy and guaranteed by the method itself, whereas later in on the Principles, they are supposed to reflect basic truths of metaphysics and their ultimate guarantor is God; see his "Descartes and Experiment". Gary Hatfield has suggested to me that by the late 1630's, Descartes changes his mind and holds that the premises of empiricist deductions are not certain, and that evidence for this view is not only in the Correspondence (from 1637-38) but in the Principles as well; cf. Hatfield, "Science, Certainty, and Descartes," in PSA 1988: Proceedings of the Biennial Meeting of the Philosophy of Science Association (East Lansing, Michigan: Philosophy of Science Association, 1988), vol. two, 249-62.

than for Bacon, then, could it make sense to suggest scientific reasoning proceeds by deductive inferences which we don't even *expect* to guarantee the certainty of our claims.

Gassendi's chief insight in this context, it seems to me, is that *nothing* guarantees that our empirical claims are certain--not even the inferential structure of valid syllogism--because the aim of scientific reasoning is not preservation of truth (across the premises and conclusion of a syllogism) but establishing credible claims about the world on the basis of other such claims we already believe, though we are not certain of them. And thus we come back to Gassendi's 'modern' concept of probability, or at least the degrees-of-belief aspect of probability--that we make judgements about claims by the evidence in their support where such evidence is *direct* or without the benefit of intermediary testimony, such that the strength of our beliefs in those claims is a measure of the degree of our confidence, not in another person (as per their testimony), but in the evidence itself. What it means to say that our degree of belief in a given claim is less than whole is just that the direct evidence in its behalf is less than fully compelling and so fails to make us certain of that claim. And so it is with empirical claims we attain by valid syllogism: the evidence for the supporting premises is *never* fully compelling so we can never be certain of them--nor, as a result, of the claims they support. This insight leads Gassendi to propose, *in effect*, a familiar symmetry between inductive and deductive reasoning about empirical matters: in the former we lack certainty as to whether the conclusion follows from the premises; in the latter we lack certainty as to the truth of the premises themselves--but in either case, it is a consequence of our imperfect access to the way the world is, that the claims we infer by such reasoning cannot be known with

certainty. I say 'in effect', for although Gassendi's actual, quirky view of 'probabilistic' inference suits only deductive reasoning, still his picture of inductive reasoning (quite correctly) allows that we have insufficient information from the premises to be certain of our conclusions. That he does not *formally* recognize (and perhaps doesn't even recognize at all) this symmetry makes all the more impressive his recommendation that one way we judge both sorts of empirical reasoning relies on our recognizing this lack of certainty: we gauge the content-wise strength of a given deductive argument by our degree of belief in its constituent premises, just as we do in inductive argument. The difference--and it's substantial, of course--is that in a given inductive inference we automatically get with this content-wise strength some measure as well of its structure-wise strength, the conditional probability of the conclusion given the premises.⁴²

While it seems Gassendi has all the elements of this familiar story, he falls short because he is convinced it is a syllogistic form special to deductive argument that determines when we are looking at a piece of reasoning we can call 'probabilistic'. Yet he makes an important contribution here, obviously not to the history of informal (let alone formal) logic, but to the history of the study of empirical reasoning. It is to at least recognize what it means to have and rely upon contingent propositions at the base of one's inferences about empirical matters. For although it is a bit awkward that he limits his characterization of 'probabilistic' argument to deductive inference, to his credit he sees it is not a *formal* feature of valid syllogism that we cannot be sure of empirical or other

⁴²Here another difference emerges between Gassendi and Descartes: in distinguishing inductive and deductive reasoning, the views of the former are a bit more subtle or nuanced (not to say more successful) than those of the latter.

contingent claims we might so infer. It is simply a fact of life, so to speak, about such deductive inference. This may be indeed shaky ground upon which to assert our beliefs about the world, just as the ancient sceptics warn. But it is also the only grounds open to deductivists (including Gassendi in his *regressus* methodological mode) to build empirical claims upon. And so Gassendi accepts 'probabilist' deductive inference for the same reason he accepts inductive inference without the crucial generalization step: each regularly produces results -- that is, viable empirical beliefs -- even in the absence of the relevant necessitating beliefs. Utility emerges as a motivational theme not just here, in his analysis of inference patterns, but more globally, in his general views on method.⁴³ Let's recall that it was some measure of utility which drove him to shape his version of *regressus* method relative to the different epistemic contexts of science. On this note, let us next consider whether, and how, Gassendi himself finds the various pieces of his method useful in his own scientific practices and reports.

B. Gassendi's Actual Scientific Method I: Experimental and Observational Reports

To see the extent to which Gassendi employs this 'official' method in his own scientific practice, I look at his accounts of the barometric and vacuum or void-related experiments of 1648, where he presents some of his most detailed reasoning about experimental episodes. These accounts conform to the 'official' method insofar as we can construe the schema of their core reasoning as plausible instances of Gassendi's

⁴³I suggest in the last section of this chapter that Gassendi further thinks we impute to the consequences of our hypotheses just that degree of necessity we deem useful, as measured, for example, by the capacity of the hypothesis to connect and explain the available data.

'probabilist' deductive brand of argument, and as we can at least loosely construe them as resolution or composition steps we perform to realize discovery tasks. But this much is a banal achievement if the fit is so loose that we might well otherwise construe these schema to fit competing models. A bit more significantly, these accounts fail in several ways to conform to the method prescribed in the Institutio Logica. For one, the search for the middle term turns out to be a mysterious exercise--particularly where there is no univocal way to reconstruct the reasoning that underlies each account in syllogism form. For another, as the discussion in the last section suggests, it is a daunting if not unfeasible task to find a judgement-realizing step that corresponds to the discovery-realizing step which is the primary feature of these experimental accounts. And even more significantly, these experimental analyses reveal a central methodological issue not even discussed in the 'official' view: what are the guidelines for hypothetical reasoning? Though he wants a science based as much as possible on information from the senses, Gassendi's own reasoning in these and other scientific writings relies on hypothetical assumptions for which we have no direct evidence from the senses, and this raises the question of how we are to judge the proper role, merits, and admissibility of such assumptions in advancing empirical inquiry.

Gassendi's Experimental Account of Hydrostatics and the Void

At several points in his writings Gassendi describes a kind of experiment which he thinks demonstrates both the existence of a created void and the foundational barometric principle that the height of a liquid column in a tube is directly related to the

weight of, and thus the pressure exerted by, the surrounding atmosphere. Though performed by Pascal's brother-in-law Périer at Puy-de-Dôme, the experiment was first explained in print by Gassendi (and only subsequently by Pascal), who learned of the details from Périer through a witness to the experiments, one illustrious Monsieur Mosnier.⁴⁴

The historical background to this experiment begins with Galileo's speculation that a vacuum existing between particles exerts a cohesive force on matter. This speculation generated great debate and experimental activity around two issues: the existence and nature of the void, and the relation of atmospheric weight to the maximum height of a column of liquid. Following on the heels of Galileo's conjecture, Torricelli shows in 1644 that a column of liquid in a tube is sustained by the weight of the atmosphere external to the tube. In 1647, Pascal published his Expériences Touchant le Vide in which he defends the proposal that the space above the mercury could be a vacuum and not rarified matter; here he does not discuss Torricelli's suggestion (of 1644) that it is the pressure of the outside atmosphere which supports mercury in the tube.⁴⁵ In

⁴⁴There is a small debate in the literature as to the exact identity of M. Mosnier—whether he was Claude Mosnier, canon of the local cathedral in Clermont, or else Pierre Mosnier, a doctor from Lyons with some interests in the sciences and a passing acquaintance of Gassendi's. See Matton (1995).

⁴⁵In response to the Expériences the Father Estienne Noel argued the Aristotelian case (though likely at Descartes's behest) against the existence of a vacuum. Pascal in turn replied (letter of October 29, 1647) with an account of hypothesis acceptance that in part anticipates Karl Popper's. It is possible, Pascal suggests, for evidence to disconfirm a hypothesis, just in case the evidence runs counter to any phenomena to which the hypothesis commits us, but it is not possible for evidence to definitively confirm a hypothesis. Rather (and this runs against Popper's view) the more evidence we can mount in support of a hypothesis the more probable we can say the hypothesis is. This is likely one important piece of his motivation for his subsequent experiments at Puy-de-Dôme and the St. Jacques Tower.

September of 1648 Périer, at the behest of Pascal, performed the barometric experiment of measuring the height of the mercury in the tube at various heights of the Puy-de-Dôme, and shortly thereafter Pascal himself performed a similar experiment at the St. Jacques Tower in Paris--the results of which he took as evidence in favor of the hypothesis that a vacuum sits above the mercury as well as Torricelli's explanation of the mercury's suspension. In this first experiment, the height of mercury in a tube sitting closed end-up in an open basin (also filled with mercury) is measured at various altitudes, with the result that the height of the mercury column varies inversely with the altitude of measurement. Thus the mercury in the tube--at a height of 29 inches at sea level--falls when the apparatus is brought to the summit of a mountain, and the upper part of the tube becomes apparently empty. An equilibrium is established since any shift in the altitude will produce a inversely-corresponding shift in the height of the mercury. These two questions arise: how can we explain this inverse relationship, and what if anything is in the space above the mercury? Pascal, in his posthumous Traité de la pesanteur de la masse de l'air et de l'équilibre des liqueurs [Treatises on the Weight of the Mass of Air and Equilibrium of Liquids] (1663), suggests the relation between the mercury and the surrounding atmosphere is such that, as one ascends with the tube the air thins so the surrounding atmospheric weight decreases with the result that the mercury falls. All other things being equal, then, the greater the weight of the surrounding atmosphere, the higher the mercury will climb. Further, he interprets the empty space above the mercury as vacuum, on the grounds that nothing else could have come to take the mercury's place as

it falls.⁴⁶ But that interpretation appears in print nearly a decade and a half following an account Gassendi gives, one year after the Puy-de-Dôme experiment, in an appendix to his Animadversiones in decimum librum Diogenis Laertii (1649), and largely recapitulated (though with some noteworthy changes) in the Physics of the Syntagma Philosophicum (1658)—as enhanced by his own performance of a similar barometry experiment in 1650 at Toulon (which he discusses in a letter to his friend Bernier⁴⁷). Since Gassendi had close access to Pascal's work through his brother-in-law, it is not entirely surprising to find that the former anticipates the central points made by the latter.

The focus of the Syntagma discussion is not the barometric results but the experimental production of a vacuum. This is hardly startling, for the possibility that these experiments produced a vacuum had to excite Gassendi's imagination given his fascination with the Epicurean proposal that all is composed of atoms and the void. Indeed, one rather striking facet of Gassendi's analysis given his empiricist views is his suggestion that there must be a vacuum, or the void, *because* we can bring one about through experimentation. The thinking underlying this claim anticipates similar reasoning underlying many modern experiments in physics and chemistry, such as those demonstrations of the existence of particular elements or subatomic particles through their laboratory creation. The progressive methodological character of this point aside,

⁴⁶As Rochot points out, Pascal's Traités were still being edited and therefore unavailable at the time Gassendi argues for a similar interpretation of the barometry experiments; see Bernard Rochot, "Comment Gassendi interprétait l'expérience du Puy de Dôme," Revue d'Histoire des Sciences 16 (1963), 73-74.

⁴⁷Q VI 319 a-b.

though, the perceived novelty of this analysis at the time was the proposal that there is, by whatever cause, a void. Descartes, by contrast, tries to flesh out the Aristotelian suggestion that 'nature abhors a vacuum' with the proposal that subtle matter fills all interstitial spaces and would therefore fill in any such spaces that we could create, as for example, the space over the mercury. Thus Gassendi's interpretation (with which Pascal later agrees) has to say how the void can occur anyway or else why we don't need to worry about subtle matter.

The void can occur at the top of the tube, Gassendi contends, because it can occur in discrete packets, lying between and around any subtle matter that remains in the space above the mercury. In this Syntagma discussion, he argues that, while this space may contain a small number of particles of some kinds of matter, it does not contain air, and is thus largely interparticulate void. For there to be air in that space there has to be an entry point for the air to flow in. Compare, he says, a container of water where there is no opening. If we seal a container of water with fish living in it, the fish will suffocate and die. But fish breathe in the water, so their death after we seal the container means no air enters after the sealing.⁴⁸ By analogy, he reasons, there can be no air in a container of mercury unless there is an opening. But *ex hypothesi* there is no opening at the top of the tube and so there is no air on top of the mercury column within. And since there is no air above the mercury the apparently empty space must be largely void.⁴⁹ There are at least two problems here. First, the analogy is rather unfortunate, for in a sealed container of

⁴⁸See Margaret Osler's discussion in Divine Will.

⁴⁹SP, O I 205-206.

water there can be air, though perhaps there is not enough oxygen for fish to breathe--and likewise there can be air, in small parts, in or above the mercury in a sealed tube. Second, a lack of air in that space does not rule out the presence of 'subtle' matter.

Indeed, Gassendi recognizes at least the second problem and embraces the possibility that there can be several types of matter in the apparently empty space: corpuscles of light, heat, cold, and 'magnetism' and 'gravity' particles may all pass through and remain inside the glass of the tube, even if the tube does not permit air particles to pass (although he does not say why this last category is ruled out--perhaps because of their size). So what appears to us as a thorough void is actually an interparticulate void where the surrounding particles are much fewer and far between than they would be in an otherwise similar region of space outside the tube. We know there is some void in the space inside the tube because, if not, then the aforementioned particles of matter could not pass through the tube (here Gassendi follows closely Epicurus's reasoning that, since matter is impenetrable, only the existence of the void explains motion). But we also know this interparticulate void takes up a greater proportion of the space above the mercury than a comparable apparently empty space outside the tube, he claims, just because the total matter in the tube is, for lack of air, more attenuated than that outside the tube.⁵⁰

This experimental account is a no more than fair instance of Gassendi's 'official' method. To begin with, it provides a painful example of the gyrations necessary for telling a deductivist story about reasoning based on what, according to the Institutio

⁵⁰SP, Q I 205-206.

Logica, he counts as inductive inference. His overall reasoning fits a deductive syllogistic form; there is an apparently empty space, but no air, above the mercury, and wherever there is empty space but no air there is void, hence there is void above the mercury. The reasoning for his principal lemma also fits; there is no opening on the top of the tube, and wherever there is a container with no opening, no air can enter the container, hence there is no air on the top of the tube. Yet we may doubt a deductivist account is applicable here because his very reasons for thinking there can be no air above the mercury, by analogy to the water container, surely count for him as argument by example--and thus as a species of inductive reasoning. But Gassendi doesn't have these doubts, because he thinks his deductivist method accounts for any syllogism which supplies the requisite generalizing step--even if based on embedded nondeductive inference. It does not occur to him that we might better account for arguments with such embedding by telling a nondeductivist methodological story about them. But at least he must concede that his reasoning is, in his sense, 'probabilist' since his assumptions are empirically-derived and therefore contingent. And so the claim of a void atop the mercury is a 'probable' claim attained by deductive reasoning.

How well does this argument conform to his proposed *regressus* method? Our first clue in this regard comes in Gassendi's suggestion here that our lack of sensory access to the underlying phenomena should not prevent us from understanding the causal story of this experiment, by way of conjecture from the apparent effects:

Our faculty of understanding and knowing is...made such that if we are incapable of grasping by a direct view the constituent elements of things, we can at least grasp some of their effects; and from there it is convenient to content ourselves--

after having succeeded at guessing in some way things concerning these elements apart from some of their effects--with doing all we possibly can to accomodate to other effects those notions (whatever they are) we form about the preceding effects; this we do anytime we examine the causes of these effects, or ask how they are originally derived from their components.⁵¹

And so, in this context, he proposes that we reason from the apparent lack of air atop the mercury to a putative cause, the existence of a void which results from the mercury's sealing the cavity:

In the present question, it is certain that if we were naturally endowed with a clairvoyance great enough to perceive the constitutive elements of mercury and air, we could without hesitation and with no previous reasoning say what is the true cause of the effect on what we examine; but as we are not thus endowed, what can we do other than conjecture at first, following other effects, and then assure that the natural constitution of these bodies is such that this effect, and not another, could result from it?⁵²

In this last clause, we get the suggestion that our 'discovery' by resolution does not identify the only conceivable causes of the apparent effect, and so we need to ascertain (or 'judge') that no other causal account will suffice.

And as for the 'discovery' itself, the resolution step entails a search for the middle term. One might plausibly suppose Gassendi takes this search to consist in his effort to demonstrate, via his fish-tank lemma, that there can be empty space and yet no air. This much of his analysis can be shown to conform, more or less, to his method. Yet it seems clear that the procedure of conjecturing about causes as based on their effects could as well be tailored to a methodological account which does not require a search for the middle term. Moreover, if we want Gassendi's analysis to count as a discovery it should

⁵¹SP, Q I 207b.

⁵²SP, Q I 207b.

be mildly worrisome that the void is afterall not really something he finds, so much as postulates.

In any case, this account works according to the method of the Institutio Logica only if there is a corresponding 'judgement' which consists of a composition step that reverses the resolution step, reasoning from cause to effect. So what we need here is an inference from the causal claim that there is a void above the mercury to some effect pertinent to and confirming of the 'discovery', along the lines of the claim that there is no air above the mercury--as a result of the void. Perhaps this is something of what Gassendi is trying to do when he proposes that the existence of a void permits whatever subtle matter is in the cavity above the mercury to pass through the tube. But if the idea is that we need to stipulate the void to explain how the subtle matter can move through the tube, then he has reasoned again from effect to cause and so this won't count as the corresponding composition step. At any rate, that the void accounts for subtle matter does not provide confirming evidence for the claim that there is *no* air above the mercury; if anything, it suggests the opposite might be the case. Though it cannot be terribly surprising that this experimental account fails to conform very closely to Gassendi's proposed method, it is a little unsettling that it falls so short. Let's consider, by way of comparison, the other side of the hydrostatic experiments--accounting for the inverse relation between the level of mercury in the tube and the elevation at which the tube is held, which is the focus of the Animadversiones discussion.

Here Gassendi anticipates Pascal's own suggestion that the mercury in the tube is pushed upward to the degree the mercury in the basin is subjected to downward pressure

that increases as the elevation diminishes. The lower the elevation, he proposes, the larger the quantity of air in the surrounding atmosphere which hangs over the mercury, and as this quantity rises, so does the downward pressure of each component part of surrounding air. This rise in pressure is a result of a rise in the net gravity of the atmospheric air. Gassendi contends that gravity works through chains of special corpuscles which attach themselves to bodies that are then pulled down by such chains to the Earth's surface. Hence in this context he claims that gravity-chains pull air particles down towards the Earth, such that the weight of particles from any higher climb adds to the downward pull on any particles below, with the result that a lower particle of air "...possesses, other than that which is its own, the gravity added from all parts situated above it". A further consequence of this cumulative effect of gravity is that the pressure of air particles against each other increases, so that they push their way into the interparticulate void and their density increases, too. This heightened density increases the resistance of the atmospheric air hence decreases the ability of the mercury in the basin to rise, leaving the mercury in the tube suspended at a relatively higher level.⁵³

To spell out the mechanical underpinnings of these various relations and so give a microphysical account of the data that goes beyond an instrumental reading, Gassendi

⁵³Animadversiones (herein AN) II, Appendix. Gassendi stresses that the specific degree of force exerted on the instrument is a function of the local conditions, which in this case amounts to the particular mass of the air immediately above and surrounding the tube. Thus, whereas the vast surrounding mass of air allows for the overall constancy of forces exerted upon the mercury at any given location of similar elevation, the mass of just the single column of air, suspended directly above the immediate area of the tube, accounts for the particular degree of force exerted on that tube and the mercury within. By way of analogy (to this explanatory appeal to local mass) Gassendi accounts for the variable strength of a water jet from a hole in a container by suggesting that strength is a function of the pressure of a water column directly above the hole.

posits the kinds of forces and resulting motions of particles which he believes, given the right conditions, will vary with altitude. He takes the elements of this account as premises for the claim that a void plus the right corpuscular interactions are the crucial factors which yield the instrumental variance produced in the experiment. His reasoning is as follows:

- (1) A necessary condition for particle motion is the existence of either a disseminated (interparticulate) void, or an accumulated void, or both, and since there is motion there are such voids; and
- (2) A sufficient condition for particle motion is the presence of forces which are brought about by constituent corpuscular interactions and which, taken individually, may be directionally irrelevant to the path of the particles;
- (3) Thus there is even motion in the void when particles are contained in a circumscribed area with no apparent empty space, because both conditions are met. Under sufficient pressure the particles move closer together by entering the disseminated void, and so they become more densely packed.
- (4) One instance of this kind of motion occurs in the Puy-de-Dôme experiment, where the variable interaction of atmospheric particles produces a pressure which causes a proportionately varying motion of first the mercury and next the air above the mercury. Thus at a low elevation, this pressure forces the mercury to rise such that air and dust particles above the mercury in the tube move into the disseminated void and, at a high elevation, the pressure drops and the situation is reversed.
- (5) Hence in this experiment, the presence of a disseminated void at the top of the instrument and the aforementioned kind of corpuscular interaction accounts for the greater density of the collection of air particles and the rise in the mercury at lower elevations, and for the reverse of these effects at higher elevations.⁵⁴

The argument for the first point here--the necessity of the void for particle motion--alters slightly the grounds for the ancient suggestion that we need the void to explain motion,⁵⁵

⁵⁴AN, Appendix.

⁵⁵Cf. Epicurus' Letter to Herodotus, DL X 40.

but similarly begs the question. In Gassendi's version of the story, void must exist since it is possible to concentrate all matter found in a given space such that, with the matter pushed to one side, the resulting cavity would be perforce wholly empty. Hence empty space can be shifted around with varying distributions of matter, the implication being that atoms are unfixedly distributed in the medium of a void. The other side of this coin for Gassendi is that the emptiness is what allows matter to move around to begin with. Yet this story, like the ancient one, presumes that there is no plenum, and that there are such things as voids which we can shift around. This is a difficulty for ancient and modern alike, though it is one Gassendi thinks he can get around it by offering a variety of empirical arguments for the void, the general form of which is to suggest that there must be a void to explain appearances of moving objects. But this is simply to recapitulate the ancient argument and so these arguments also rely on the presumption that the plenist is wrong.⁵⁶

The second point--the sufficiency of a corpuscularian picture of forces for particle motion--rests, a little more securely, on independent physical claims. First, Gassendi contends that mechanical interactions of corpuscular bodies yield forces like gravity, a simple consequence of his view that *all* candidates for action-at-a-distance are best explained by supposing that the forces in question are the products of corpuscular interactions. I examine this facet of his corpuscularianism more closely in the next chapter; for now let it suffice to note that Gassendi here takes this much of his physics for

⁵⁶See Edward Grant, Much Ado About Nothing: Theories of Space and Vacuum from the Middle Ages to the Scientific Revolution (Cambridge, London, New York: Cambridge University Press, 1981).

granted. Next, with a nod to his rejection of inherent directional tendencies of falling bodies (in De Motu, the Physics, and elsewhere in the Animadversiones), he claims that just the direction in which particles move does not by itself tell us the kinds of forces that can bring about their motion. Gravity, for instance, may contribute to a set of forces the combination of which causes particles to move in any given direction, and not just downwards. As an example of such combined forces, he cites the case where air rises from an otherwise-sealed container with an opening on top. We might expect the air from within to travel up and down with similar ease; instead, upward motion occurs regularly. Why? Atmospheric air, subject to gravity and resulting downward pressure, pushes down on the container and the enclosed air with the initial result that the air within is pushed down. Yet that same pushed-down air then exerts an extraordinary pressure on the particles pushing it down (again, air from the outside), producing an ejection-like behavior which alone restores equilibrium. Thus the pulling-down force of gravity produces pressures which in the end force particles upward. It is implied here, if not so much as stated, that what enables this combination of forces to begin with is the homogeneity of the corpuscular interactions that underlie all of what we call forces. While the details may be skimpy, it is a general result of his corpuscularian physics that although one force acting alone may set particles in motion in one particular direction, the combination of various forces may produce motions in any other direction.

The third point is that sufficient force allows particles to 'find' a void which permits their motion, so that even when particles are constrained to a particular area they may become denser under pressure. In this way particles may move though there is no

apparent space, empty or otherwise, for them to move through. In the sealed tube of the instrument used in the barometry experiment, for example, it seems to the naked eye that when the mercury rises in the tube there is less space at the top (than when the mercury was lower) for the air particles to move around. If there was *no* place for the particles to go (without, quite impossibly, decreasing their minimal particulate volume) they would of course resist the pressure. But under sufficient pressure the particles move through--really, into--empty space formerly between them, which has the result of bringing them closer together and making the air a denser gas. This requires great effort or energy, Gassendi suggests, since the air particles must 'push into' an interparticulate void that otherwise separates them.⁵⁷

The fourth point is that in the Puy-de-Dôme experiment what provides the great effort required to generate this motion of air particles above the mercury in the tube is the pressure air propagates in the surrounding environment, from the atmosphere's upper surface downward. As a result of the atmospheric pressure, the mercury spread in the basin pushes the tube's mercury upward, and air particles in the space above move into the disseminated void. And so, Gassendi concludes, the inverse relation of the level of the tube's mercury (and density of the air above it) with the elevation of the apparatus comes about because those corpuscular interactions which contribute to pressure on the tube's mercury and air vary with altitude. At lower altitudes, then, so great is the

⁵⁷The implication here is that something about this separating space was fitting to the natural distribution of the air particles, so that only extraordinary force would condense them. This raises at least these two questions: what would be a *natural* distribution of gaseous particles, and why would extraordinary force be necessary to condense them if there is only empty space between them?

atmospheric pressure (and resistance of the basin's contents to downward pressure of the tube's contents) that the mercury is driven up toward the top of the tube, such that the air above becomes denser. But by bringing the tube to a higher altitude we decrease the atmospheric pressure and so the mercury falls.⁵⁸

The significance of this experiment is, as Gassendi and Pascal equally well indicate, that there is an inverse variation of atmospheric pressure with elevation. But one inventive aspect of Gassendi's account is that he tries to identify the underlying imperceptible physical conditions we need to generate the inverse relation of the mercury's height in the apparatus with the elevation of the apparatus.⁵⁹ In so doing, he tells us not just what foundational physics supports the story of the mercury's rise and fall, but also (as a result) precisely how we may relate the experimental data to the inverse variation for which it purports to give evidence.

The merits of these foundational details (and so the strength of this experimental account) rely on the plausibility of his physical picture (see chapter three). The question here, though, is whether his reasoning is a viable instance of his proposed *regressus* method. Like the Syntagma account, this account relates a discovery task. And, as in that

⁵⁸As a concomitant effect the air atop the mercury becomes less dense and so the interparticulate void expands.

⁵⁹In a similar vein, Alexandre Koyré maintains that Gassendi's explanation is superior because he introduces the elastic pressure of atmospheric air as the primary cause of the mercury column's variable height, whereas Pascal explains the barometry experiments in purely hydrostatic terms—that is, by reference to the balance of the weights of atmospheric air on the one hand and the mercury column on the other. It is Gassendi's atomism which leads him to an elastic picture of gases (or at least air) and thus, Koyré suggests, to the realization that a proper account must go beyond pure hydrostatics; see his Metaphysics and Measurement (London: Chapman and Hall, 1968), 128-129.

later discussion, the assumptions here are contingent and reached by induction (by enumeration of data at various elevations), hence the argument as a whole is again 'probable' though deductivist in Gassendi's unusual sense. But unlike the Syntagma account, the reasoning in the Animadversiones case runs from cause to effect--which counts in the *regressus* framework as a composition step. There are in the end two causal stories, where the first is a microcosmic picture that underlies the second. The first says that whenever there are forces (themselves the products of corpuscular interactions) that act on particles in a void, the particles move through the void. The second says that the right kinds of instruments and settings, together with the right combination of forces, produce motion in particular directions, such that if we vary these conditions the directions and extent of the motion will vary as well. Given that Gassendi builds the second causal story on the principles and conditions described in the first story, the apparent aim of the Animadversiones account is to say what kinds of effects we can expect to observe when we postulate certain entities and phenomena (which are imperceptible) as causal factors.

This approach at once raises at least two difficulties. For one, if Gassendi himself warns in the Syntagma that we lack access to imperceptible causes of these experimental results then perhaps we ought not take seriously claims of the earlier Animadversiones account which he bases on just such causes. For another, if in this context he offers only postulation and not proof that these causal factors exist, we may wonder in what sense the account provides illumination, much less discovery. I think that, in a fashion, Gassendi addresses these difficulties though the way he does this leads him fairly far astray of his

prescribed 'official' method; I say more about this below.

First, though, should we still be inclined to fit Gassendi's reasoning to his *regressus* model, we must now see how in this account he might be understood as looking for a middle term, and how we might construe a corresponding resolution step, in which we reason from effects to causes. The search for a middle term begins with the determination of what that term should be, and right away we have the problem of figuring out which of several plausible syllogistic reconstructions should provide the key middle term. Thus, for example, each of these distinct syllogisms reconstructs some crucial part of the reasoning in the Animadversiones account:

(A) whatever is in motion passes through the void, and whatever passes through the void is compelled by forces produced by corpuscular interactions, so whatever is in motion is compelled by forces produced by corpuscular interactions;

(B) we can measure the degree of atmospheric pressure exerted at any given elevation by the downward pressure transmitted by constituent particles of the atmosphere, and the downward pressure transmitted by atmospheric particles at any given elevation is the sum of pressures transmitted by particles at all higher elevations, so we can measure the degree of atmospheric pressure exerted at any given elevation by the sum of pressures transmitted by particles at all higher elevations;

and

(C) the instruments and settings of the Puy-de-Dôme experiment, together with varying atmospheric conditions, yields a particular range of combined forces acting on the contents of the instruments, and a particular range of combined forces acting on the contents of the instruments produces motion within an associated range of parameters, so as we vary the conditions, the directions and extent of the motion will vary as well.

This particular set of syllogisms alone yields a choice of three distinct middle terms, and there are likely yet other syllogisms which model some further, distinct element of the reasoning. Moreover, we can't reconstruct the entire argument in syllogistic form

without either generating multiple three-line syllogisms (and so creating multiple middle terms) or else losing some key steps--there are just too many moves in Gassendi's construal of the experimental reasoning. It does not seem that, in accordance with the *regressus* model, we can clearly identify a single middle term that picks out the germane discovery task of the experiment. So either the Puy-de-Dôme experiment as recounted here features more than one discovery task, in which case the reasoning Gassendi employs in his account does not fit the *regressus* model, or else there is one germane discovery task but nothing in the *regressus* model tells us which it should be. If the former option is right, Gassendi could say so much the worse for this experimental account; if the latter is right, he would have to say so much the worse for his methodological model (and perhaps the experimental account, too). One plausible merit of the first option, at least, is its implicit suggestion that the discovery tasks we associate with some experiments may be sufficiently complex that we cannot single out one particular finding as the lone target of such investigations. This may be a potentially fuzzy way to construct or account for experiments but it also does not place constraints on thinking about experiments which the investigators may not themselves adopt. In any case, it is not a facet of his view Gassendi himself identifies and it is far from clear that he would find it an attractive result.

Yet the fit of this account to the *regressus* method fares even worse if we try to identify the corresponding resolution step. The problem here begins with our inability to identify a unique discovery task. One reason the *regressus* theorist wants to identify such a task is, no doubt, so that we know precisely what syllogism we need to 'reverse' in the

step which realizes the judgement task. But in this account we couldn't assess *the* discovery task of the experiment, so we should be hard-pressed to identify what *the* corresponding judgement looks like. So even if Gassendi wants such a judgement to show his reasoning best (if not uniquely) accounts for the data, just how he can do this if he can't know the terms of the requisite resolution step is not entirely clear.

At this point it can't be shocking to find that Gassendi's experimental accounts don't neatly match the constraints of his 'official' method; we might rather be surprised to find that there is some loose affinity between the two at all. By contrast, perhaps the most striking methodological aspects of these accounts are those about which the 'official' method is silent. One example is the suggestion that investigators may set out to find evidence for a variety of claims in a single experiment, or they might interpret such experimental results accordingly. Another, more important example is the prominent role of hypothetical reasoning, and--save for hints about sign-based inference in Book IV--Gassendi says nothing about hypotheses in the Institutio Logica. Yet if the Animadversiones account is exemplary, then he thinks it is good method to introduce weighty assumptions about the kinds of causes there may be, deduce some of their effects, and then explain the data as effects of such causes. Thus in this experimental account Gassendi takes it that only by assuming a corpuscularian-mechanical picture and an interparticulate void can we allow for the motion of bodies.⁶⁰ These are, perhaps,

⁶⁰One difficulty in the Syntagma account was Gassendi's assumption that the void alone accounts for the motion of particles, though an ostensible aim of the experiment is to prove a void exists atop the mercury in the tube. But in the Animadversiones account he initially argues for the void by an inference to the best explanation strategy, and so may dissolve this apparent circularity. For we might best explain some physical phenomena like motion by postulating the existence of

untroublesome assumptions insofar as they are staples of his physical picture. Some of his other assumptions, however, lie on the periphery of his physics: the account of how the apparatus indicates changes in atmospheric pressure, for example, relies on the supposition that collections of gas particles behave in ways relevantly similar to collections of other types of particles (like mercury) with respect to forces like gravity.⁶¹ In either case, though, the 'official' method does not give us guidelines for reasoning from such unproven assumptions or by hypotheses. We are not told, for instance, when hypotheses are warranted starting points in scientific reasoning, what kinds of hypotheses we want in such reasoning, or the nature or measure of evidence for hypotheses. These are, however, topics which Gassendi addresses elsewhere in his writings, and I now turn to these views on a method of hypothesis.

C. Gassendi's Actual Scientific Method II: Hypotheses and Hypothetical Reasoning

In examining critically a method of hypothesis and hypothetical reasoning in science, one issue to resolve at the outset is what counts as a hypothesis. There is, from a most catholic perspective, a wide variety of things we take to constitute the hypothetical or conjectural part of science. In Gassendi's own time, one might think of hypothetical entities like cartesian vortices, idealized phenomenal generalizations like the law of free

voids generally, but come to decide that the specific cavity above the mercury contains a particular instance of void on the basis of experimental data we count as evidence for the void hypothesis.

⁶¹There are, naturally, other kinds of important assumptions Gassendi makes. For example, he assumes there is a general continuity of experimental conditions across elevations. Gassendi is at least obliquely aware of this in his own performance of the experiment in 1650, where he makes special note of the variance of the weight of the atmosphere with its temperature and humidity (Q VI 319a-b).

fall, conjectural guides to research like the Ptolemaic picture of the heavens, data-synthesizing explanatory models like Harvey's picture of circulation, and predictive models such as Kepler's model of planetary orbits. These very different kinds of items share at least one feature, that we accept them 'on credit' with the promise that, later on, that acceptance may be further justified in some manner. The point of this preliminary acceptance, according to one view with classical roots, is to be able to reason about, and further our understanding of, other phenomena for which we do have evidence, given the explanatory, inference-building, or exemplifying benefits (to name a few) of assuming such hypothetical items even if we lack satisfactory evidence directly on *their* behalf.⁶² Accordingly, proponents of this view contend, we reason by hypothesis when we take such items by assumption and then try to deduce consequences for which we should have or be able to find empirical evidence.⁶³ That we then seek support for the hypothesized items by further, optimally direct evidence is quite another task, and it is no small question as to when we have such support. But no matter our approach to this last issue we first need to decide what counts as initially admissible conjecture, under what circumstances we should have recourse to hypothetical reasoning, and when we have legitimate instances of the same, that is, when our hypotheses can be said to advance our

⁶²Plato, for example, sketches such a theory in the Phaedo and the Republic and relies extensively upon it in developing the Theory of Forms.

⁶³Plato writes in the Phaedo (101d):

If anyone should fasten upon the hypothesis itself, you would disregard him and refuse to answer until you could consider whether its consequences were mutually consistent or not. And when you had to substantiate the hypothesis itself, you would proceed in the same way, assuming whatever more ultimate hypothesis commended itself most to you, until you reached one which was satisfactory.

scientific knowledge.

The roots of Gassendi's views on these issues lie in his suggestion that we reason about the world by generalizing on particular sensory experiences and inferring claims about non-apparent phenomena from claims about apparent phenomena. The ready analogue in the context of scientific inquiry is that, on the basis of what evidence we have, we form hypotheses about phenomena for which we have no direct empirical evidence. Although Gassendi embraces this analogue, he worries that pure conjecture provides poor foundations for science. Instead, he insists, any hypotheses we consider as starting points for reasoning about empirical matters must be based on reports of sensory data, and we should refrain from taking claims we demonstrate on their basis to be true, given that such starting points have not themselves been demonstrated to be true. He has qualms, of course, about our ability to know *any* truths with certainty, so it is natural that he is not concerned with the actual truth of hypotheses so much as their resemblance to the truth, or verisimilitude, which he takes to be a function of their empirical adequacy--rather broadly construed. Just as we should initially consider only those hypotheses for which we have some minimum of empirical evidence, we should retain only those upheld by our best evidence, which he understands to include evidence from signs.

1. To grasp the distinctive character of Gassendi's views on hypotheses it is important to recall that in his times we find a debate over whether hypothetical reasoning actually has a place in science at all. As surprising as this might be by modern lights, it is further surprising how narrow that debate is usually construed to have been: arguments for the use of hypotheses are offered by those who maintain the possibility of *apriori* knowledge

about the natural world, and arguments against are offered by those who deny such a possibility.⁶⁴ Thus the view--primarily associated with Francis Bacon--that reasoning from hypotheses has no place in science, suggests we violates central empiricist principles by relying on presuppositions which go beyond our observation reports. In particular, such a reliance violates the principle that empirical inquiries (like any other, for the empiricist) consist in learning from observation reports alone, in accordance with the general principle that all knowledge is from the senses. Hypotheses, then, represent idle, groundless conjecture, unsuitable for the foundations of science.⁶⁵

We might think it an improbable task to eliminate conjecture or presupposition from scientific reasoning and we might also think it undesirable if it turns out that good science requires speculation beyond what is empirically evident. This is the suggestion of those early modern thinkers who reject the Baconian view: we need to go beyond mere induction on observation reports, they claim, if we are to grasp underlying causes and

⁶⁴We can find this 'clean' division in, for example, Ralph M. Blake, Curt J. Ducasse, and Edward H. Madden's Theories of Scientific Method: the Renaissance through the Nineteenth Century (Seattle: University of Washington Press, 1960), John Losee's A Historical Introduction to the Philosophy of Science (Oxford: Oxford University Press, 1972), and David Oldroyd's The Arch of Knowledge (New York: Methuen, 1986). Larry Laudan, for one, does not accept this division; see his The Idea of a Physical Theory from Galileo to Newton: Studies in Seventeenth-Century Methodology (Ph. D. diss., Princeton University, 1966), and Science and Hypothesis (Dordrecht: D. Reidel, 1981).

⁶⁵The standard view of Bacon might suggest to some that he actually accepts hypotheses *per se*; it's just that he rejects hypothetico-deductive reasoning. After all, we cannot have induction by ampliative inference unless we make such inferences from our data that are perforce merely hypothetical. According to this picture, Baconian hypothetical reasoning consists in induction coupled with his proto-Popperian view that we test such hypotheses by observation reports (and if they fail, we change the hypothesis). The problem with this interpretation of the standard view is that Bacon doesn't recognize that induction requires hypothetical reasoning, and he really is suspicious of conjecture--which he believes to pose the greatest difficulties in the premises, and not the inferential structure, of our scientific reasoning.

unifying principles in science, for these are not subject to observation. The more common and natural variant of this suggestion, which we find in Kepler, Galileo, and Descartes (and many others), is based on one or another non-empiricist set of principles. Distinctly less common is the variant, which we find in Gassendi, based on empiricist principles.

For Kepler, Galileo, and other early modern astronomers, the main reason to accept that science has a warranted hypothetical component is simply that astronomy doesn't go very far on the basis of observation alone. Kepler argues for his laws of planetary motion on the basis of their conformity, not merely with sightings, but with mathematically-derived conjectures about what he understands to be necessarily the structure of the solar system (as described, for example, by his area law). Galileo, for his part, suggests science requires a conjectural element to provide those imagined abstract approximations of physical and astronomical items (such as spheres rolling on planes, bodies in free fall, or planets in orbit) which we need in order to spell out those laws we take to govern the behavior of such objects⁶⁶--and overcome finite limits on our grasp of the possibly infinite range of physical phenomena.⁶⁷ For these scientists the appeal of the hypothetical stems from their view that our best characterization of the world is an

⁶⁶Laudan (*Idea of a Physical Theory*) proposes that Galileo thinks of such imagined abstract approximations of objects as idealizations. The problem with this proposal, as Martin Tamny has suggested to me, is Galileo's insistence that astronomy and physics concern the real and not the ideal. Laudan's suggestion might be fruitfully reformulated as the proposal that we best grasp Galilean science if we view what he took as imagined but possibly real objects--frictionless planes, perfect spheres, etc.--as ideal.

⁶⁷Galileo was noncommittal, or perhaps simply undecided, about whether the universe is infinite.

imagined (and idealized, for Kepler) one which, unlike any picture we build upon fallible data concerning further phenomena, may be guaranteed as true by demonstrative proofs similar to those we summon in mathematics.

We find a different non-empiricist defense in Descartes, who employs hypothetical reasoning throughout his scientific writings. Like these astronomers, he is also wants a picture of the world that is certain and rests on secure demonstration. But the primary reason he thinks we need conjecture beyond experience in science is rather that his prescribed method of 'analysis', or reasoning from data *to* first principles, demands hypothetical reasoning. This is not, by the way, quite so of 'synthesis', or deduction of actual facts *from* first principles, which he supposes to be evident by the light of reason. Even if Descartes were to count such principles as conjectural, he could not view them as *empirical* hypotheses;⁶⁸ but since they are founded on the bedrock of reason he doesn't think they are conjectural to begin with. By contrast, analysis is built on experience, and so the relevant methods are empirical ones--among which Descartes counts hypothetical reasoning in at least two respects. First, we use hypothetical models to show analogies between perceptually familiar and less familiar mechanisms, where the less familiar ones are usually perceived indirectly if at all. One example is the tennis-ball analogy Descartes uses to explain the mechanical behavior of light (in the Treatise on

⁶⁸Even if they are neither based on, nor maintained on the strength of, experiential data, one might think we should count cartesian first principles as empirical hypotheses because of their clear empirical content, such as we see in his laws of nature, e.g. the second law, that all movement is of itself along straight lines (PP II §32). But Descartes advertises even such straightforwardly physical principles as part and parcel of geometry or mathematics, and he asserts that we don't need any principles which don't belong to those abstract studies to explain the range of physical phenomena (PP II §64).

Light).⁶⁹ Second, we assume the existence of hypothetical entities to fashion explanations that unify our observations. One example is the proposal (in the Principles) that we best explain the observed motions of the planets by assuming that matter in the heavens moves along vortices. But in either case the reason for introducing hypotheses is that we need to make *whatever* assumptions our demonstrations require--as long as we can show they are foundational or follow from foundational principles--and, Descartes claims, we won't get those assumptions from experience. We draw upon conjecture, then, to help account for what we can't know from experience. The appeal of the hypothetical for Descartes, then, rests on our methodological needs which we determine on the basis of not what we know, but what we don't know, from the senses.

One alternative to these views which figures less prominently in histories of this debate, offers *empiricist* grounds for admitting a hypothetical component to science. This is Gassendi's approach. He agrees with Bacon that to be able to consistently call an account 'empirical' it must accord largely with the data, but disagrees that this implies a

⁶⁹See e.g. Laudan, Idea of a Physical Theory, 75-76. This particular cartesian analogy is curious relative to Gassendi's view of light: both Descartes and Gassendi hold that light is refracted and reflected in a mechanical fashion, but only Gassendi takes it to be literally the case that light is corpuscular. For Descartes the tennis-ball model may give a corpuscular picture of light suitable for explaining refraction and reflection, but we need a different picture to explain other elements of the mechanics of light (e.g. a model of light as rod-like to explain transmission). Laudan and Buchdahl see these varying models as (1) inconsistent and thus (2) evidence that Descartes did not take any of his hypothetical light models as the only one he thought to be correct; see Laudan, and Gerd Buchdahl, "Descartes' anticipation of a 'logic of discovery'," in Scientific Change, ed. A. C. Crombie (London: Heinemann, 1962), 399-417. Of course, he could not take them all to be true if they are inconsistent, but it *is* possible he felt that one hypothesis was stronger or more likely than another. Further, it is not clear that drawing on such different models must yield an inconsistent view of the nature of light. For if Descartes uses each model simply to provide an analogue with which to *explain* a distinct set of light's mechanical properties, then this use of the two models need not commit him to the ontology of either.

prohibition on either conjecture beyond observation reports or induction on the same. Indeed, as we have seen earlier in this chapter, Gassendi anticipates Glanvill and Hume in observing that induction actually requires an ampliative hypothesis which might not be either found in or directly warranted by the available data. Bacon's acceptance of induction as a secure method of inference founded on our data alone is, at least in its usual (crude) formulation, something of an act of faith. By contrast, Gassendi believes that the strength of at least one class of inductive inferences and much other conjecture-based reasoning, relies upon a thesis for which he thinks we have good empirical evidence: that there is a physical continuity across perceivable and imperceivable domains, such that the physical laws which govern the behavior of all bodies are invariant to their scale (and so their perceivability). The empirical evidence for this proposed physical continuity (and the scalar invariance thesis it supports), Gassendi holds, can be found in predictions about the micro-sized world shown to be correct by advances in microscopy.⁷⁰ If such a physical continuity characterizes the spectrum of bodies, he suggests, then we have grounds for accepting ampliative inferences from claims about the perceived to claims about the unperceived.⁷¹ Such inferences are perforce conjectural, but for Gassendi these are the kinds of conjectures or hypotheses which, rooted in experience, we want to initially admit for consideration. What is exciting for him about these particular conjectures is that they carry over a robust picture of mechanical explanation,

⁷⁰By the way, it is a bit odd that he is unconcerned about the great amount of disconfirming evidence provided by microscopy in this regard.

⁷¹One potential problem here is demonstrating such a physical continuity, or the concomitant scalar invariance thesis, empirically or otherwise; I discuss this in the next chapter.

without changes, from visible to subvisible domains.⁷²

2. One important clue to the kinds of hypothetical reasoning Gassendi endorses lies in what he means by 'hypothesis'. According to one formula he offers, an hypothesis is "an invention that is probable and adapted for calculations."⁷³ Following common usage of the term before development of the probability calculus, by 'probable' Gassendi means not so much 'likely' as 'unproven yet plausible', as in 'capable of being approved'. And in this passage he is weighing an astronomy hypothesis of sufficient abstraction (namely, that the motion of starry bodies in ether causes the motion of the earth) that it seems likely he intends 'calculations' to mean not necessarily computation in a mathematical sense but whatever practicable methods we use to determine the viability of some further claim. If this interpretation is right, then it seems he takes at least one sort of acceptable reasoning from hypotheses to consist of drawing inferences from conjectures we think are not only plausible but which we embrace in particular because we think they will facilitate such calculations or determinations.

By contrast, one point he does not make here is that hypotheses are supposed to give us a certifiably true and precise account of the phenomena. Indeed, he articulates why we should *not* look for this quality in a passage in the Disquisitio where he reminds

⁷²This might appear close to Descartes's motivation for developing a method of hypothesis, but there is at least one difference. Descartes thinks we need hypothetical models like his tennis-ball model of light-corpuscle behavior because the senses can't give us any information about light-corpuscles so we need to invent a reasonable story to bridge this gap, whereas Gassendi thinks we need such models because they help explain what is perceivable and accordingly what we take to be indicative of that which is not perceivable. In brief, we incorporate hypotheses into our scientific accounts just in case they are experientially-based and so underwrite the role of the senses as a source of information, even about things we cannot directly sense.

⁷³Q I 630b (De hypothesi Siderum in Aethere fluido, ac mota simul Terra incedentium).

Descartes of ancient Skeptical caution about their use in geometry and astronomy. The ancients, he writes,

...raised doubts concerning the method of proving by hypotheses, not insofar as they were conducive to stimulating the attention or revealing the solution being sought, but insofar as they were held to be so right and true to nature that actual things would be assumed to be exactly the way the hypothesis supposed they were....⁷⁴

However plausible or likely our conjectures may seem, it is dangerous to take them to be true unquestioningly, because we may very well lack proof or even any supporting evidence on their behalf. Here Gassendi rejects what he takes to be Descartes's view that we should entertain only those hypotheses we think are necessarily true, on the grounds that what is conjectural cannot be indubitable at the same time. This approach echoes the one Gassendi attributes to the Skeptics: we may accept an instrumental role for hypothesis in demonstration, if we carefully and constantly check the accuracy of each conjectural claim:

...you would not see [the Skeptics] proposing doubts against demonstrations, or clear natural principles that are neither hypothetical nor suppositious [*supposititia*], but only against the very hypotheses [*ipsas hypotheseis*], about which they would first ask 'if something [i.e. such a thing] ought to be accepted by hypothesis'.⁷⁵

What the Skeptics really worry about, according to Gassendi, is the possibility that we prematurely impute truth or correctness to our conjectures, not that we use them at all

⁷⁴Q III 384a; R 510; B 265-266; cf. B 340.

⁷⁵Q III 384a; R 510; B 265-266. Brush translates 'Sit-ne aliquid ex hypothesi accipiendum' as 'if that ought to be accepted *as a hypothesis*', which seems off the Latin mark and also does not indicate the real puzzle for the Skeptics, namely, deciding whether something should be accepted as a matter of conjecture (and not deciding on accepting some particular conjecture as against any competitors).

in order to generate demonstrations and predictions.⁷⁶ Their 'dogmatist' opponents, such as the Stoics, suggest we are warranted in counting hypothesis-based claims as true just in case we count our hypothetical assumptions as true. We want a method, they say, that allows us to construct systems of beliefs without first (and terminally) worrying about their foundations. The Skeptical view which Gassendi promotes here suggests in response that if the truth of our initial assumptions has never been demonstrated then neither has the truth of any claims premised only on the same.⁷⁷ This is of course accurate but seems to miss the point of the Stoic suggestion, that for the sake of constructing coherent belief systems we may accept proofs on the basis of otherwise unwarranted assumptions--this is ostensibly our principle motivation for adopting axiom-based systems.⁷⁸

In just this vein Craig Brush (1972) suggests that Gassendi fails to understand the nature of axiom systems and so rejects axiom-based proof.⁷⁹ But nowhere in Gassendi's

⁷⁶The Skeptics value such results anyway for their 'stimulating' or 'revelatory' properties--which they apparently feature even if false! Gassendi, as we see below, does not share quite this view because he thinks the same judgements we find stimulating or revelatory also approximate the truth (are *versimilitudinous*) in some fashion.

⁷⁷O III 384a; R 510; B 265.

⁷⁸The Skeptics, by contrast, don't even want a mathematics or geometry based on axioms, on the grounds that such axioms would bear no natural referents and therefore refer to no particular thing that can be known. But Gassendi parts company with the Skeptics here and recalls Aristotle's suggestion that we can posit, for example, that line is length without width just in case we are interested in the properties of length, and not width or anything else (B 340). In brief, there may be formal or conceptual reasons for stipulating postulates without natural referents--and such postulates should be acceptable as long as they are clearly defined and we deem them significant for the theory.

⁷⁹B 266n.

view do we find the suggestion that we cannot have proof in a system built on a fixed set of foundational claims or axioms for which there is no proof in the system. Rather, Gassendi's idea is simply that no demonstrations which rely on unwarranted claims (empirically or otherwise) have a place in empirical inquiry (where, along Millian lines, he includes geometry). This reflects a fair bit of caution, he points out, for if we based demonstrations on claims about the world we thought were 'absolutely true' without experiential support, we would undermine the purpose of our demonstrations by failing to provide the requisite empirical warrant.⁸⁰ If this is right, then we have the problem that, as we see in the next chapter, although Gassendi clearly wants a physics with a purely empirical basis, in the end he relies strongly on some foundational physical or metaphysical principles and claims based on historical authority for which he fails to provide any form of empirical demonstration. So a significant difference looms between his methodological prescription here and his actual physical theory. This is, however, not the problem Brush suggests, that Gassendi cannot have a coherent notion of proof from axiom-like assumptions.

In any event, Gassendi's view of hypothetical reasoning is rather novel in the context of his times, and certainly not identical to that of the Skeptics. For while he does not think we should reason from hypotheses lacking empirical support, he sanctions the use of unproven hypotheses in empirical demonstrations provided that there is *some* evidence by which they might be justified, that we withhold our considered judgement of them pending such evidence, and that we recognize this entails our inability to know

⁸⁰Q III 384a; R 510; B 265.

claims we so demonstrate to be true.

Thus his view also contrasts with the cartesian account of demonstration, that says whatever claims we establish must follow in part from axioms, first principles, or other such foundational beliefs for which we may lack empirical proof yet which are definitive, not provisional, for they are evident by the light of reason. There is a faint echo of this view in Gassendi's 'official' method when he insists that discoveries in science progress by demonstrative proof, but as I noted in part one of this chapter, he is willing to concede a great many such proofs yield claims that are far from certain because the premises on which they rest are uncertain, too. Moreover, he cannot match Descartes's account in any case since, as I suggest below, he holds that, though we must mount additional evidence to eventually confer approval upon such hypotheses, no amount of evidence will ever lead us to say that they are definitively true. In lieu of Descartes's claim that the foundations of scientific reasoning are secure only if they are indubitable Gassendi proposes that any starting point for scientific reasoning is secure just in case it is based on our best available experimental and observational evidence. This point is clearest in his insistence that we seek an evidentiary basis for the hypothetical element of science in experiential data.

For example, Gassendi praises Tycho Brahe as an exemplary astronomer because of his inventive hypotheses which he bases on keen observation and careful charting of the motion of heavenly bodies.⁸¹ By contrast, he decries the astronomy of J. B. Morin (the French astronomer and astrologist) as occult because it is based on hypotheses and

⁸¹See Gassendi's Life of Tycho Brahe. Indeed, Gassendi concludes from his own astronomical observations that, although Tycho's observations were partly discredited by Kepler, his charts were in the main highly accurate.

sky charts which Morin formulates without bothering to first make observations. In his polemic against Morin, Gassendi writes:

...you...gave Hypotheses and drew up Tables, about which there is nothing there to be said?...You have never made observations; You have never worked on making Hypotheses on any observations, neither of your own, nor of others? You have never composed Tables on any Hypotheses. What could [one] think of you, if it's not this: PARTURIENT MONTES?⁸² When [someone] opens your book, and sees there only some small fragments, fifteen days each, and all made in an office without seeing the Sky; is to know [*scavoir est*] your quarrel [*querelle*] of Longitudes, with the Treatise of the Equation of time, [also to know] parallaxes, and refractions [?]; will he have occasion to believe that this is the whole of Astronomy reestablished in its entirety?⁸³

Gassendi takes Morin to task for not at least bothering to borrow someone else's observations as a foundation for his hypotheses, and for not forming celestial tables on the basis of well-formed hypotheses. Hence even if Morin and other armchair astronomers don't first peer at the sky, their hypotheses should be formulated on the basis of data taken from sky-watchers, and they should design their charts accordingly.

It is no small irony, then, that while Gassendi insists we have no warrant for conjecture without some basis in experiential data, he himself adduces much non-empirical (theological, historical, and philosophical) evidence for his own physical picture, and in particular for his atomism. Yet as I argue in the next chapter, he also holds great hopes for empirically supporting his atomist picture. This suggests that he believes we have experientially-based grounds for conjecture about things such as atoms, which by hypothesis have no corresponding appearances. Whatever could those grounds be? We

⁸²*Parturiunt montes, nascetur ridiculus mus* (Horace): To them that desire to bring forth mountains, a ridiculous mouse will be born.

⁸³RC 134-135.

need such hypotheses, Gassendi proposes, in order to understand the underlying structure of the world despite our limited perceptual access to it. And, following the theory of signs, we arrive at such hypotheses by inferring claims about that nonevident part of the structure on the basis of claims about the evident part, either because of a regularity of correspondence between occurrences of the two (inferences by commemorative signs) or else because we cannot conceive of the second existing without the first existing (inferences by indicative signs). In several passages from his Physics and Disquisitio, Gassendi tries to justify such inferences on the grounds that we have good physical and historical reasons to believe the correspondences we postulate may well be regular and productive. To this end, he extols the merits of the optical picture underlying microscopic and telescopic extensions of our visual perception, and the history of our successful experiences with the same.

One point Gassendi needs to make about microscopy and telescopy to ensure the regularity of such correspondences, given his view of visual data as information transmitted by the material medium of light corpuscles, is that there is a guaranteed preservation of such information through the changing of light's path. In short, whatever images we attain through the naked eye we should be able to magnify so as to provide finer details of the image, without any other significant changes. Such a guarantee allows us to say we can have as viable ideas of what is contingently subvisible or supravisible as we do of what is visible. In Section Three of the Physics, he draws on his analysis of

convex and concave lenses⁸⁴ to offer at least one element of such a guarantee:

This, then, must make us see the [underlying] reasoning of Telescopes. Since the ordinary Telescope is made of two lenses, one convex and the other concave, the rays are reunited in such a way by the convex lens that, before the reunion of cones similar to those that we have said are formed by the eyeball and transmitted to the retina, one puts the concave which, dilating as little as these cones, pushes more before their points and makes them at the same time more distinct, such that before having been received in the eyeball a little after crossing, they reunite a second time in the retina, and represent the thing greater in proportion to the convexity. And one sign that the thing is seen after the rays's crossing is that, as the rays are received on paper [after being focused through a convex lens] the thing is painted as inverted, and that nevertheless as seen by the eye through the Telescope it is seen right-side-up.⁸⁵

The rays carrying parts of an image converge as they pass through a convex lens (which also inverts the image), and they are refracted when they pass through a concave lens placed before the point of convergence, so that, taking the former lens as an objective and the latter as an eyepiece, we get a magnified, focused, and non-inverted presentation of the image through such a telescope.⁸⁶ Naturally, this is not a guarantee that images are wholly preserved through the changing of light's path, but it does suggest images we

⁸⁴Gassendi's analysis of convex and concave lenses is partly based on his familiarity--through Peiresc's work--with the behavior of microscopes, and--through his own astronomical observations--of telescopes. As for his basic picture of optics, although Rochot proposes that he may have learned a great deal from Descartes's *Dioptrics*, in fact Gassendi's views follow Kepler's rather closely, which is rather natural since the former read, admired, and corresponded with the latter.

⁸⁵Q II 382-390.

⁸⁶The telescope Gassendi received from Galileo was built in this way (see Pierre Humbert, *L'oeuvre astronomique de Gassendi* (Paris: Hermann, 1936), Albert van Helden, "Gassendi and the Telescope: Towards a Research Community," in *Société Scientifique et Littéraire des Alpes de Haute-Provence, Actes du Colloque International Pierre Gassendi Digne-les-Bains, 18-21 Mai 1992*, in *Annales de Haute-Provence* 321-322, (1994): 329-339). In his *Dioptrics* of 1611, Kepler first suggests that such a combination of lenses would right the image and bring it into greater focus if the concave lens is placed before the point of convergence.

receive through this kind of telescope are not any more distorted relative to focus than our unmagnified images of mid-sized objects, and not at all distorted relative to position. Of course, Gassendi could not rule out much other distortion, particularly given the immature state of magnifying technology. More crucially, though, to say such magnification yields evidence for hypotheses about otherwise nonvisible things we need not only to keep the lenses's distortion of the image to a minimum, but also some reason why the mere act of enlargement itself does not yield gross distortion. As Gassendi puts it, "How do you say...when a face seen with a concave mirror becomes so large, this greatness is true, when viewed with a Telescope, a Microscope, and generally with a convex lens things become so large, and when a finger appears large like a leg, a flea like a snail, a mite like a pea, this greatness is true?" His answer draws on the analogy between enlargement through magnification and enlargement through physically coming closer:

I respond that it seems so, insofar as nothing, that is to say no part or particle, that one sees becoming larger appears in the thing which is not truly there, or could be said to be reported or supposed, yet foreign--since nothing else happens here than what happens when an object seen from afar as small, comes closer, and seems larger. Now in this way a thing seems larger because more of its parts which were before turned towards another place are now turned towards the eye and since, its being located between [in front of] those things which were seen first augments the number of those [visible] parts such that the thing seems larger; in the same way, I say, that in this way a thing seems larger, thus it seems larger in these other ways.⁸⁷

What ties the two kinds of enlargement together, and so warrants our taking magnification to enhance visual perception without a loss of perceptual information, is

⁸⁷Q II 382-390.

that in either case there is an increase in the number of an image's parts (that is, atomistically speaking, in the elements of the corresponding configuration of light-corpuscles) which reach the eye. Thus if a thing seems larger than before when we approach it, this is because we have increased the amount of information we have about it so that we have a generally more accurate assessment of its character (though not necessarily its size). There is, Gassendi notes, a limit on this kind of increase before our assessment of size diminishes in accuracy, as when we are too close to an object to reasonably judge how big it is. Yet our overall characterization of small objects can only become more accurate by coming closer to them, since this enhances our perception of both their previously and newly visible parts--and, in virtue of such enhancement, magnification through telescoping or microscopy also heightens the accuracy of our accounts of the heretofore nonvisible.

These elements of an optical picture suggest we should not reject conjectures we infer from claims about the nonevident on the grounds that they are founded on distortions of perceptual data, because we actually base those inferences on techniques for enhancing perception that tend to preserve the character of such data. But Gassendi additionally offers a positive reason for licensing such conjectures, namely, is that our experiences with those techniques have been so successful. In a passage of the Disquisitio where he proposes, against Descartes, that we may naturally and without the intervention of God enhance our initial ideas of things (and of God in particular), we find the suggestion that interposing lens between our eyes and those objects we do not directly observe allows us to make new perceptually-based claims about those objects:

...you consider the mite [*acari*] in effect as indivisible, as the mite is relative to our view....In effect whereas the image or idea of a mite, whether traced in your eye, brain, or understanding, twenty-five years ago represented only a little whitish point without any distinct parts, today to the contrary (since the invention of the microscope [*engyscopium*] and after its use) it represents to you an animal of small but appreciable size and provided with a head, tail, limbs, back, and other parts....Say...in what way the particular ideas of the limbs of a mite, discovered by microscope, should be contained in the idea that we had of it before the microscope. I think [you can] not, because you know the lens's property of convexity too well to doubt that it augments the mite's visible appearance, thanks to the deviation and displacement of the smallest rays coming from the mite's different parts, which [without the microscope] due to the insufficiency of [the mite's] too-little surface, are found elsewhere, and do not produce on the eye particular images separated from others at [distinguishable] intervals....If nevertheless you are not disposed to agree to the comparison with the mite, make the same comparison with the Moon, or the Sun (or whatever object you would) before and after the invention of the telescope: the same thing will result.⁸⁸

Gassendi may be faulted for premature optimism regarding the microscopy programme (and, to a lesser extent, the telescopic programme, too) but in this passage he identifies one case where magnification undeniably adds to our viable conjectures about the natural world. What allows us to say we have warrant for a richly detailed set of hypotheses about mites, for example, is that we base our inferences to those hypotheses on our reports of seeing those of their parts not visible to the naked eye through a microscope. And so, Gassendi can insist that there be some perceptually-based evidence for empirical hypotheses if we are to call them plausible, even if the hypothesis in question is supposed to tell us about objects we cannot perceive without magnification. He can insist on such a standard because he thinks we have good reasons--minimal distortion and success at discovery--to believe enhancing perception through magnification preserves the nature of the images (or, perhaps, other data) we magnify and

⁸⁸DM, O III 355a; R 378-380.

then consider as suggestive or supporting evidence.⁸⁹ If, by contrast, we couldn't come up with any such perceptually-based evidence in advance, then there would be no reason to even consider our hypotheses as bases for 'calculating' or determining the viability of further claims. But here Gassendi's empiricist caution enters the picture: even if we can produce such evidence, we are perforce not entitled to consider the claims we base on hypotheses as true since they are based on conjecture, not truths. What's more, this reserve carries over to Gassendi's criteria for judging an hypothesis as a properly accepted view (that is, among our core scientific beliefs), by which standard no evidence ever suffices to consider an hypothesis as true.

3. This suggestion--that hypotheses we keep as part of our science after empirical test nevertheless lack sufficient evidence to be called true--resembles that traditional perspective which says hypotheses are not the kinds of things that have definitive truth values. This perspective, and the converse position that hypotheses may be straightforwardly true or false, are the respective bases of the competing views which predominate discussion, from the middle ages through the early modern era, about what it means to endorse hypotheses. The first view, which I'll call the 'empirical adequacy' view, says we should accept hypotheses because they 'save the apparent phenomena', though they cannot actually be true (or false). The second view, which I'll call the 'truth' view, says we should accept hypotheses because we know them to be true, and we know

⁸⁹Again, Gassendi finds these reasons compelling because they are evidence of the regularity of, and productivity in, asserting correspondences between occurrences of evident and non-evident phenomena which, as per his theory of signs, should allow us to infer claims about the latter from claims about the former (in this case, for instance, reports of microscopic or telescopic images).

that they are true because we accept them on the basis of some indubitable evidence.⁹⁰

Gassendi's perspective, though it resembles the classical 'empirical adequacy' view, actually represents a distinct, third contender. He suggests we accept a given hypothesis because it comes closer to the truth, or is more *verisimilitudinous*, than any of its competitors, and that we recognize this neither on some indubitable basis nor merely on the basis of its agreement with the raw empirical data (that is, data about what is present to the senses), but on the relative strength of the total evidence in its favor.

We find the 'truth' view in, among others, Copernicus and Galileo. Copernicus claims we know hypotheses are true just in case we have indubitable evidence for them, which he thinks we may find in their apparent consequences: "For if the hypotheses assumed by them [earlier astronomers] were not false, everything which follows from their hypotheses would be verified beyond any doubt".⁹¹ Galileo, on the other hand, proposes that we know them to be true just in case we come to them by intuitions we cannot doubt. Whatever hypotheses we accept, he suggests, should reflect intuitions of *apriori* truths. We might well wonder, by the way, what Galileo intends by considering some claims as (merely) conjectural if they are supposed to be based on intuitions we take to report *apriori* truths. At any rate, the suggestion here is that our accepted hypotheses are not merely palatable because they are empirically adequate, but demonstrably true because founded on further, indubitable truths. The historical context here is that Galileo

⁹⁰Laudan (*Idea of a Physical Theory*) refers to these as 'instrumentalist' and 'realist' views, respectively. This is, I think, an unnecessarily risky choice of terms given the many diverse ways each term has been understood in contemporary debates.

⁹¹Nicolaus Copernicus, *Commentariolus*, 28.

wants a way to say that the Copernican picture is preferable over the Ptolemaic picture not because the first coheres with the available data--since they both do--but because it is true. Thus if we could intuit *apriori* our principle scientific claims, we would have indubitable knowledge of their truth and so, he maintains, scientific knowledge with certainty. Given this standard of acceptance, claims meeting only the test of empirical adequacy are unacceptable. One difficulty for this perspective is that the criterion of indubitable evidence might provide unsure foundations for judging the acceptability of an hypothesis--afterall, we could think ourselves unable to doubt something but just be incompetent to judge. The larger question here, though, is why indubitability is supposed in the first place to be an indicator of truth.

The 'empirical adequacy' view, by contrast, suggests the truth of hypotheses cannot be a criterion of their acceptability because no hypotheses are possibly true. It is not in the nature of hypothetical accounts to be true or false but simply to fit, better or worse, to the data from observation and experiment. For partly theological reasons, this supposition guided a rather popular picture of hypothesis acceptance from scholastic times up through, and including, Descartes. The chief theological reason in this regard is one of epistemic humility--we cannot really know what the true picture of the universe looks like because that would entail that we had God's picture, and we don't want to say we have such omniscience.⁹² Accordingly, it is supposed, no evidence counts as

⁹²One likely doctrinal motivation for this humility is the Church's condemnations (of 1270 and 1277) of a number of Aristotelian theses on the grounds that they constrict our view of God's omnipotence--God could create any worlds he wants to, with whatever physics he wants, and so forth. Edward Grant suggests that, as a result of these Condemnations, many scientists and philosophers sought plausible accounts of the way the world could be, but steered away from

sufficient for endorsing one view as true and calling all competitors false.⁹³

In the early seventeenth century an 'empirical adequacy' view continues to hold sway for at least partly theological reasons. In his arguments against Galileo, Cardinal Bellarmine proposes that the heliocentrist hypothesis is no more acceptable on scientific grounds than the geocentrist one since each saves the apparent phenomena, such that one should choose among them by the distinguishing criterion furnished by the Church. And Descartes adopts an 'empirical adequacy' view on the grounds that the world could have been fashioned any way the Creator wants. Although hypotheses of cartesian science must cohere with first principles of metaphysics and with experience so that we may "deduce true results" from them (our guarantee against empirically false or physically impossible consequences),⁹⁴ there may be multiple plausible accounts of the imperceptible workings of the universe. Thus while his corpuscularian account is empirically adequate if it generates true results, it is not necessarily *true* since the micro-composition of matter could have been--indeed, could actually be--otherwise. In the

suggesting that the world actually was this or that way; see his "Hypotheses in Late Medieval and Early Modern Science," *Daedalus* 91 (1962): 599-616.

⁹³So, for example, among late medieval and renaissance astronomers, Jean Buridan and Nicholas of Oresme suggest one merit of the geostatic hypothesis is that it saves the astronomically apparent phenomena, and if there is any *other* reason to embrace this hypothesis, it does not include any astronomical evidence. Thus Buridan cites the ancient worry that projectiles should fall 'out of place' on a moving planet--which may entail observable events but, in his times, must be non-astronomical evidence. And Nicholas suggests the relative character of our observations while in motion obviates *any* empirical criterion for choosing between competitor hypotheses, so that we must choose the geostatic view on theological grounds. Each view suggests that, at least in astronomy, we accept hypotheses not because we find compelling empirical evidence of their truth but because they account for the apparent data and meet our pre-set conceptual constraints--whether philosophical or theological. See Grant, "Hypotheses," 606-608.

⁹⁴AT II 142.

Principles Descartes makes this point in the framework of his famous clock metaphor:

For just as the same artisan can make two clocks indicate the hours equally well and are exactly similar externally, but are internally composed of an entirely similar combination of small wheels; so there is no doubt that the greatest Artificer of things could have made all those things which we see in many diverse ways.⁹⁵

Since, as a result of adopting this 'empirical adequacy' criterion, we can't count on finding that our accepted hypotheses are true, we may think Descartes should worry that he thereby cannot build a science consisting of certain knowledge of truths. Perhaps he takes solace from his earlier Regulae, where he points out that "...nothing that we construct [by way of conjecture] really deceives us, if we judge it to be probable and never affirm it to be true...."⁹⁶ That is, we don't set ourselves up for a fall if we don't expect to know (with certainty) our hypotheses to be true. At some points in his correspondence, Descartes throws such caution to the wind and proposes that we may judge an hypothesis to be true if it implies a particular result and we have good independent evidence for the result.⁹⁷ But this is not his considered view, as represented in the Principles, where he argues that we accept hypotheses if they meet particular formal and empirical criteria and not if we can say for certain that they are true, given that we cannot be certain in this regard.

Gassendi's alternative to these views suggests an hypothesis is acceptable on the

⁹⁵pp IV §204.

⁹⁶Rule 12, Regulae.

⁹⁷See for example the letter to Vatier of February 22, 1638 (AT I 563-564). As Laudan (Idea of a Physical Theory) points out, this view anticipates Peircean abductive inference. The two views feature the common difficulty (which Descartes later recognizes) that conflicting hypotheses might imply the same result, yet we wouldn't want to say they are all true.

basis of all available evidence (and not just data reporting the apparent, as the classical 'empirical adequacy' theorists propose) which indicates it *may* be true (and not, which indicates it *must* be true, as the 'truth' theorists propose). To begin with, he takes it as neither false nor a consolation that we cannot be certain that our hypotheses are true and instead take them to be probable; it is rather a simple fact about the limits on our epistemic access to what is contingently non-perceived or imperceptible in principle. If we base our conjectures on information about which we are less than certain, it can't make sense to insist that our criterion for accepting an hypothesis is that we are certain it is true. Indeed, Gassendi believes the ultimate structure of the world is sufficiently hidden to us that we can accept an hypothesis given its utility in scientific reasoning, and without ever needing to say it is true. To accept an hypothesis we only need to see that it *resembles* the truth, where such resemblance or verisimilitude is measured by the total empirical evidence for it--which for Gassendi comprises claims inferred from signs--and lack of evidence against it. In this way the criterion for judging hypotheses mirrors the criterion for entertaining them in the first place as suppositions of empirical reasoning, their broadly construed basis in experience. And so, when Descartes claims his suppositions in the Meditations are analogous to an astronomer's hypotheses, Gassendi protests that we cannot judge his claims as resembling the truth (let alone as certain truths) since they lack such a basis:

...the difference is great between your supposition and an astronomical hypothesis, for example. That's because the astronomers take something certain, namely, the observed position of the Planets; thus, to explain the cause of it they imagine circles that they do not conceive as fictive, but which they consider, if not real, at least as existing in a way that resembles the truth [*verisimile*], since it may come

about that the Planets follow them in their path. And the proof of this is that, if they thought that there exist other, more probable hypotheses, they would not fail to adopt them. But as for you, there is nothing you kept as certain in order to adjust your prior suppositions, and as material of your hypotheses, you have nothing real nor resembling the truth, but a wholly pure supposition. And do not say that from this hypothetical falsity you extricate something of truth, as is this famous saying: *ego cogito*, etc. In effect...it's exactly as if the astronomers, without having proceeded from any previous observations, posed hypotheses from which would be deduced whatever true observation.⁹⁸

Not only must the hypotheses we formulate not be wholly fictional, they must resemble the truth. Scientists--or at least astronomers--adopt causal hypotheses (to explain, for example, planetary positions) which they think resemble the truth on the supposition that such hypotheses may actually describe the real structure of the world. Yet they may well not describe that structure accurately and, accordingly, Gassendi refrains from identifying truth *per se* as the criterion for accepting hypotheses.

This much is consonant with a somewhat pessimistic version of the classical 'empirical adequacy' view: our perceptual grasp of that real structure could be sufficiently poor that any number of hypotheses might more or less conform with what we crudely perceive. Indeed, we find hints of this version in, curiously enough, Gassendi's Philosophiae Epicuri Syntagma, which largely summarizes Epicurus's views and is a work not typically thought to be terribly original.⁹⁹ But in the above passage from the

⁹⁸DM, Q III283b; R 54.

⁹⁹Q III 53. In these passages Gassendi suggests we might have a number of empirically adequate astronomical hypotheses, but that we shouldn't expect to find the same situation in basic physics where the hypothesis we should accept "agrees in but one way with appearances." The difference between the two situations seems to be a matter of the distinctive strategy for choosing among astronomical hypotheses. If we didn't say there could be more than one viable scientific hypothesis in astronomy, and that hypothesis failed, we would be tempted to fall back on a theological account. But this would be accepting that there are different, equally plausible ways to understand astronomy which might include

Disquisitio there is the further claim that we know scientists pick just the hypotheses they believe to be truth-resembling because if there were any others they believed to be 'more probable', they would have picked them. This suggests scientists judge between hypotheses by choosing those that are *closest* to resembling the truth. At this point it might be supposed that we can measure verisimilitude by simple empirical adequacy, but Gassendi has a richer sense of what is adequate here than what is suggested by the classical 'empirical adequacy' view.

Specifically, given his theory of sign-based inference, he cannot buy the suggestion of the classical view that the lone mark of acceptable hypotheses is that they save appearances, for he believes we also need to save the non-apparent phenomena we learn about from signs.¹⁰⁰ And insofar as we build our hypothetical explanations on elements of this expanded range of empirical evidence, we should judge them as acceptable or not on the same basis. In a later passage of the Philosophiae Epicuri Syntagma, he pleads for a method where our reasoning about hypotheses in astronomy incorporates such an expanded range of evidence:

...let it not be that the entire line of reasoning with regard to the causes of celestial

appeals to faith as well as experience or reason, and yet--at least by scientific lights--there should only be one way, and it's not theological at root. So to prevent us from falling into temptation, Gassendi proposes, we can simply allow that if an hypothesis fits the data, that's enough for us to embrace it, fully recognizing that this may lead us to embrace more than one candidate. We wouldn't have the same strategic needs relative to hypotheses of basic physics, though it is not entirely clear why. (It's also not clear why Gassendi should require this distinctive strategy, given his view--which I explain below--that even in astronomy some hypotheses should turn out to be more truth-resembling, hence more preferable, than others.)

¹⁰⁰Thus in astronomy "It is necessary...to draw conjectures as to what takes place in the heavens from those things which go on about us, from those things about us, I say, which are observed as fact and those things to be seen in the heavens." Q III 53.

phenomena be invalidated, as has happened when many have embraced an impossible theory, becoming lost in their vanity, and have tried but one approach and abandoned all others even though these are possible, being carried off to dream what the intellect knows better of, and not admitting to their reasoning things as they appear and things that are properly signs....¹⁰¹

That is, if our best empirical evidence includes evidence from signs, then we should judge a given hypothesis to have greater verisimilitude than any of its competitors on the basis of the full breadth of our experience and the way we interpret it, and not simply what is contingently, currently perceptible or what our *apriori* intuitions suggest. Though Gassendi restricts our best judgements of hypotheses to saying they may be no more than truth-resembling, he also offers a liberal view of what counts as admissible evidence to suggest such a resemblance.

In sum, Gassendi stands apart from both those who say that (a) because we devise hypotheses on an intuitive, non-experiential basis, we should judge them as true just in case those intuitions are indubitable, and those who say that (b) though we cannot know our hypotheses to be true, we judge them as empirically adequate if they save the appearances (and, following Descartes, if they agree with our basic metaphysical and physical principles). For one, Gassendi holds that we have neither an intuitive nor a non-experiential basis for devising hypotheses, and so he dispenses with the traditional grounds for thinking they may be indubitable. Further, because the range of viable evidence extends the range of our claims beyond notions about what is perceptually given, nor can we judge hypotheses as acceptable simply if they are merely empirically adequate in the classical sense. Rather, we need to draw on a full range of evidentiary

¹⁰¹Q III 57b.

support encompassing evidence from signs to gauge whether candidate hypotheses more or less closely resemble the truth. It is the hope of Galileo or Descartes that we make this conjectural element of science, of all things, conform with, if not bear the very mark of, our certain knowledge. This may seem bizarre by modern lights but is perhaps explained by their romance with the general mathematization of empirical inquiry, prior to the age of modern probability. That Gassendi, by contrast, insists on no such thing is (similarly) consistent with his empiricist suspicions of knowledge with certainty.

4. One nasty aspect of empiricist suspicions, generally speaking, is a tendency to be double-edged, and Gassendi does not readily escape this tendency. For example, if we are to include evidence from signs as support for hypotheses about the nonevident, we might well think it more appropriate to say hypotheses so supported do not resemble the truth so much as they resemble (more weakly) our understanding of the perceptual data, as supported by sign-based inference. Further, at least three other significant puzzles concerning Gassendi's method of hypothesis stem from his empiricist commitments: one traditional view of hypotheses suggests that what we report from experience should turn out to be knowable with certainty (given that they follow with necessity from the hypotheses we accept), verisimilitude provides an apparently weak guide to judging between competing hypotheses, and it is unclear how to attain satisfactory evidence for hypotheses about future events.

i. The question as to how our observational and experimental reports follow as consequences of our hypotheses arises because Gassendi accepts a modified version of the view that we form empirical hypotheses to 'save the phenomena' or account for the

appearances described in such reports; he simply takes these reports to include claims inferred from signs of the nonevident. One traditional version of this view says that we should be able to show that the experientially-based claims we make given that we accept a particular set of hypotheses can then be deduced from said hypotheses. Further, this view goes, if such claims follow with necessity from the hypotheses, then all we need to do is fix the hypotheses as necessary and then the empirical claims deduced from them are in principle knowable with certainty. Yet, according to a classical distinction between certain knowledge and probable *opinio*, our sensory claims are the stuff of the latter, not the former; Gassendi, for one, views all fields of study from geology to geometry (save theology and theological cosmology) as empirical endeavors and so proposes that our scientific claims are merely probable. So either this distinction is somehow wrong or else this traditional method of hypothesis went astray somewhere. Russell suggests that by the lights of empiricism we can see that this classical distinction is wrong: whatever division there is between knowledge and opinion is not marked by certainty or the lack thereof.¹⁰² This is an idea Gassendi shares, but it cannot help him to avoid saying that experientially-based claims are at once probable (because derived from the senses) *and* certain (because derived from our accepted hypotheses). To avoid this problem he instead relies on another modification of the 'save the phenomena' view. The traditional view must be wrong, he suggests, for our claims about appearances need not follow as necessary consequences of the the hypotheses we accept.

This may seem like a difficult notion for him to articulate. After all, he was

¹⁰²Bertrand Russell, Wisdom of the West (London: Macdonald, 1959), 73.

acutely aware that, in the long tradition of saving the phenomena in astronomy, previous astronomers crafted their hypothetical pictures of the world so as to account for all currently available data. Accordingly, and quite naturally (if unfoundedly), they held that future observation reports should follow from these hypotheses.¹⁰³ Yet what Gassendi denies is not this consequence relation but that it must be a necessary one. We should rather fix the degree to which our hypotheses's consequences are necessary, he claims, to the extent of their utility in our scientific reasoning. We find this suggestion in a letter to Jacques LeTenneur (May, 1649), where he praises LeTenneur for, among other things, proposing a theory of time similar to his own and recognizing that even a false theory of time may yield a picture of actual temporal phenomena we think is true. He writes:

There could not be anything better to say than what you advance concerning these false hypotheses, from which we draw true consequences...to want to extend the thing [schema] to nature as well as practice--[that] this is to not understand that nature only goes by its own ways, which are always directed from the cause towards its effect, as from what precedes towards what follows, with necessity. And [to not understand] that hypotheses are purely works of human understanding, which forges them for its convenience such that their consequences can depend on them with the necessity that the understanding desires.¹⁰⁴

Here Gassendi allows that we may draw true consequences from false hypotheses (whether or not we know they are false). The alternative, perhaps in the manner of Descartes, is that we could only draw true consequences from hypotheses we knew to be true--a somewhat mysterious proposal if we accept that hypotheses are essentially

¹⁰³See in particular his discussions of the three principal contending astronomical hypotheses (Ptolemaic, Tychean, and Copernican) in his Institutio Astronomica.

¹⁰⁴JAL 140-141.

conjectural or speculative. On the other hand, one might think Gassendi's proposal fares no better, for it apparently entails our accepting cases where, no matter how attractive its true consequences, the falsity of the hypothesis is stark and hard to swallow. So, for example, the particular case Gassendi cites in this passage concerns a false hypothesis about the nature of time, which suggests the direction of causality need not go from preceding events to following events.¹⁰⁵ Here he seems to say that, if we hold such an hypothesis (however false though it may be), we have reason to believe what we take to be its true consequences must follow from it. Since, however, the course of nature is from cause to effect, with a necessity (for example, physical or theological) over which we have no say, holding this hypothesis is extremely unpalatable. But Gassendi's next move is quite curious: he proposes rather that we should have a say in the degrees of necessity governing the projected consequences $c_1, \dots, c_n$ of a given hypothesis h (what amounts to the probability for all c/h), because we only fashion hypotheses for their utility in grasping the nature of the world. It is open to us, should we deem it useful for promoting an hypothesis (in which we have independent interests), to judge it less than necessary that particularly untoward characterizations of, say, our actual temporal data follow from whatever conjectures we offer about, say, the nature of time. In sum, his suggestion--much in the spirit of his 'probabilist' view of empirical demonstration--is that our observational reports and/or other characterizations of present and future data need not follow with necessity from our hypotheses. Quite apart from this, we must insure that

¹⁰⁵This approach might be attractive to LeTenneur's Scholastic opponents in particular given their suggestion that the duration of all events is to be considered as a whole occurring all at once.

such reports and characterizations *conform* with our hypotheses, or else there is something wrong with our hypotheses, namely, that they fail to 'save the appearances'. But that such reports and characterizations, justly seen as consequences of the hypotheses, follow them with *one or another degree* of necessity is simply a matter of utility. By dismissing the prevailing alternative (that the observational reports we generate are the necessary consequences of the set of accepted hypotheses), Gassendi hopes to escape what is for him the attendant repugnant conclusion that, in principle, we should then be able to know such empirical claims with certainty.

The way to avoid that conclusion, though, is actually far simpler. First of all, his worries are for naught because that something is necessary by no means guarantees that we are able to know it with certainty. In any case, it is of course accurate to suggest that we need not consider the observation reports we generate to be the necessary consequences of our background set of hypotheses, but this is because we are not obliged to fix the hypotheses themselves as necessary. So while it is intriguing to suggest that we fix the conditional probability of our data reports given the hypotheses we accept to the utility of those hypotheses, for Gassendi himself it is not necessary to pursue such intrigue. As he points out in his account of deductive inference, we have merely probable conclusions if we start with merely probable premises.

ii. There are a number of problems which arise out of identifying verisimilitude as the mark of approvable hypotheses. To begin with, Gassendi does not specify exactly why we might count one hypothesis or another as more or less truth-resembling, and one suspects this may be because if we knew exactly what it is like to for something to

resemble the truth we should have had some idea of what the truth of the matter itself is-- in which case we might as well hold hypotheses up to that standard and not settle for resemblance to the truth. One Gassendist response to this might be to say that we cannot be sure we knew what the truth of the matter is and so the cautious move is to simply call our standard 'resemblance to the truth'. But this response won't do given that there is a difference between resembling a truth of which we are sure (for instance, that $3.1 = \sqrt{9}$ resembles, in a sense, that $3 = \sqrt{9}$) and resembling one of which we are uncertain (for instance, that a particular red occupies one range of the wavelength spectrum resembles that a given wavelength associated by some with that red occupies the same range). Of course, there can't be any such difference for Gassendi because he proposes that there are no truths of which we may be absolutely certain.

Even if this last response is satisfactory, another problem with verisimilitude is that it does not necessarily enable us to pick out a uniquely acceptable hypothesis, for we might imagine that there are several competing hypotheses which resemble the truth to the same degree; then it is a bit of a mystery as to how we should judge between competing hypotheses. That we might need to judge between multiple, conflicting hypotheses with the same degree of verisimilitude could not be surprising for Gassendi, who suggests that we might need to judge between competing hypotheses with the same empirical consequences: "...the same appearances could be predicted and preserved by contrary hypotheses [the second of] which could not be true at the same time as the first."¹⁰⁶ For example, he claims distinct astronomical hypotheses may account for the

¹⁰⁶Q III 384a; R 510; B 265.

same celestial observations (as the Tychonic and Copernican systems account for the same data) though they cite incompatible causal events. Hence the irony that in Gassendi's verisimilitude view, as in the classical 'empirical adequacy' view, the approval of one hypothesis over its competitors may be underdetermined by the empirical evidence. This was bound to happen, perhaps, so long as Gassendi's view only differs from the 'empirical adequacy' view by more broadly construing what we should count as empirical evidence. Nowadays we might not even find this underdetermination to be a terribly uncomfortable result, for example, if we share Quine's view that we can easily enough pick out the group of (mutually supporting) hypotheses which best accounts for our empirical data without overriding simplicity and conservatism constraints. If this was Gassendi's view, there would surely be an additional, theological constraint to avoid overriding. But it's far from clear that he offers anything as substantive as the holism of Quine's account. And though he occasionally refers to what the Church prefers or what must be denied in order to conform with Providence, he never quite identifies theological constraints--or any others--as additional *scientific* guidelines for choosing among empirically underdetermined hypotheses. If he remained true to the Pyrrhonism of his earliest work, we might expect him to say that we just suspend our judgement in such cases. However, the mature Gassendi abandons the Skepticist ideal of *ataraxia*, and simply leaves us without directions for picking out the worthier of any two equally truth-resembling hypotheses.

iii. One further, empiricist-inspired difficulty is that Gassendi endorses the use of hypotheses as tools for predicting future phenomena and this appears to suggest that

hypotheses constitute a source of epistemic judgements beyond what is warranted by the senses. In his own writings on astronomy he prominently draws on the predictive use of hypothetical reasoning, typically building conjectures about future planetary motions on the basis of hypotheses concerning planetary motions, constructed in turn on the basis of observational reports. But while we may draw on those past reports as the basis for such hypotheses, the content of our conjectures concerns phenomena which have not even occurred and so are unavailable to our senses. Given a strict insistence on the sensory basis of warrant for our hypotheses, there should not be much evidence we can muster to justify initially entertaining, let alone accepting, hypotheses about such future phenomena. Moreover, lacking sensory acquaintance with the future, we cannot even bar bad predictions about such phenomena on the grounds that they fail an empirical adequacy test.¹⁰⁷ Two lines of Gassendi's reasoning suggest responses here. For one, we might believe future data will bear out our conjectures on the basis of our present and past successes in accounting for the current data. This does not get us very far since we may then wonder why we should take those past successes as evidence of future success. Alternatively, the theory of signs suggests we are justified in accepting inferences from claims about the evident to claims about the nonevident just in case we cannot otherwise explain the former claims. Then reasoning by hypothesis about the future (either by

¹⁰⁷Of course, we might judge between them in advance of the events they predict, on other grounds. Gassendi himself does this insofar as he embraces Kepler's picture of planetary motion even before he observes the passage of Mercury before the sun and declares this sighting to be sufficient evidence for approving the Keplerian picture over its competitors. But then empirical adequacy relative to future events is not only not the lone test of approving an hypothetical view, it may not even be the crucial test.

analogy, or direct appeal, to sign-based inference) looks acceptable whenever we have no better way to account for the current data. This suggestion raises a central question, however: why should we think there is no better way to account for the data if they are not all available, that is, given that the conjectured phenomena have not yet occurred? Perhaps the best we can say here is that Gassendi is in the good company of others who promote this brand of inference to the best explanation; I examine his views of this sort of inference strategy in my concluding chapter.

Conclusion

Whatever the difficulties with this method of hypothesis--and as we have seen they are serious--it remains a striking historical development not only as an alternative to the foundationalism of Descartes and Galileo or the inductivism of Bacon, but also as an advance over the classical sources of his own 'official' view of method. Gassendi says a bit about science's conjectural element in his 'official' writings on method (primarily in the *Institutio Logica*), though in this context he largely follows Epicurus's project of outlining acceptable modes of inference--which all but ignores the question of reasoning from hypotheses--and he focuses on fitting all scientific method to Aristotelian demonstrative syllogism. Yet even while Gassendi's more focused writings on hypotheses diverge topically from these classical sources, he crafts his method of hypothesis as a steady progression from several themes of his partly Epicurean and Pyrrhonist inspired theory of empirical knowledge.

For example, Gassendi suggests we initially consider only those hypotheses which

are formed in order to account for reports of data from experiment and observation, and this criterion is clearly inspired by the Epicurean proposal that whatever we can hope to know is based on ideas from the senses. Further, the notion that empirical hypotheses may resemble the truth just in case they match our perceptual data echoes the Epicurean concern that our concepts of nature match information from the senses. On the other hand, his empiricist proposal that we entertain just those hypotheses founded on our ideas of appearances also echoes the Pyrrhonist concern that hypotheses represent no more than probable conjecture because we cannot be certain about the claims we base on such ideas. And his proposal that we accept hypotheses which best resemble the truth since we cannot hold them up to the standard of truth *per se* reflects the Pyrrhonist and Epicurean belief that we may be epistemically acquainted only with appearances and not with the way things truly and essentially are. But perhaps the most significant aspect of Gassendi's theory of empirical knowledge recapitulated in his method of hypothesis is his notion of what counts as support for an empirical hypothesis, the product of his background Epicurean sign-theoretic conception of evidence.

By contrast, Gassendi does not build his 'official' method upon these key elements of his Epicurean and Pyrrhonist inspired theory of empirical knowledge. Rather, following loosely the *regressus* techniques outlined in the Renaissance Paduan methodologies, he endeavors to shoehorn various procedures of empirical inquiry such as discovery and justification into one or another kind of demonstrative syllogism. His version of *regressus* method suggests that crafting such demonstrations entails seeking knowledge of causes from effects, or else effects from causes, such that a demonstration

of one kind follows the inverse sequence of steps taken in a demonstration of the other kind. Though such composition and resolution schemata do not work at all the way Gassendi intends, his particular *regressus* method at least allows for some way of characterizing empirical demonstration as probabilistic (albeit according to a nonstandard understanding of 'probabilistic'), and suggests the principal difficulty of ampliative inferences including classical ampliative induction. This 'official' story falls short because he can't hope to guarantee that resolute and compositive schemata will successfully mirror one another, and because he fails to see that the sort of 'probabilist' demonstrations he licenses incorporate ampliative inferences--and so generate the same puzzles he worries about when he indicates we lack the generalizing steps necessary for warranting such inferences. Nowhere are these shortcomings clearer than in his actual scientific writings, where his experimental and observational accounts indeed reflect some adherence to the *regressus* method but in so doing also highlight these and other, related failings. Additionally, though, the 'official' method simply fails to provide guidelines for reasoning by hypothesis in empirical contexts.

What links these varied elements of Gassendi's method is his concern with how to talk meaningfully in the natural sciences about the non-evident. This very Epicurean concern leads him, we have seen, to propose a method of hypothesis which, given his theory of signs, permits some conjectures about the non-evident that we can still consider as empirical claims. But this same concern also inspires his 'probabilistic' characterization of demonstrative proofs based on premises the truth of which is not apparent, and his worries about claims said to follow ampliative inferences. This

discordant mix of optimism about, and suspicion of, claims about the non-evident founded on claims about the evident, suggests Gassendi has two objectives that may in the end be incompatible: a method that meets maximal empiricist constraints and a method that licenses conjectures and conjectural reasoning.

One place to see how Gassendi might try to attain both objectives at once is his scientific writings where, to some degree, he applies the various tenets of his scientific method and theory of empirical knowledge. In this chapter, I argued that he takes these tenets as at least partial guides to his actual conception and conduct of science, and in particular suggested that they guide his discussion of the Puy-de-Dôme experiments.¹⁰⁸ But perhaps the most interesting indication of how seriously he takes these tenets, and how we might expect him to resolve tensions between his empiricism and ambitions to justify claims about the non-evident, is revealed by the character of his version of a corpuscularian (or more specifically, atomist) hypothesis and the degree to which his corpuscularianism conforms to his method. If this particular hypothesis about the nonevident provides any gauge of the centrality of one's empiricist commitments, then we should be rather surprised by the conjectural atomist arguments of such seventeenth-century empiricists as Boyle and Locke who were inspired by the explanatory power of corpuscularianism (as part of a general mechanical philosophy) to defend at least this one bit of hypothetical reasoning in the sciences. The corpuscularian hypothesis looks like an especially good test case of Gassendi's empiricism because, given its prominence in his

¹⁰⁸ Another prominent example, for another paper, is how they guide his approach to deciding among the primary contending hypotheses in astronomy.

physics, it constitutes a major challenge to his claim that a secure science rests on warranted beliefs we base on information from the senses alone. The challenge is simply that the micro-entities to which this hypothesis commits us are, before modern developments in microscopy, not the subject of warranted beliefs we base on what our senses yield alone. His ready and natural response here will be to refer us to sign-based inference as the means of arriving at hypothetical claims about micro-phenomena which underlie macro-phenomena. As I argue in the next chapter, an important part of Gassendi's strategy here is to seek empirical evidence through signs for initially adopting a corpuscularian picture. But this raises in turn the question of how we are to read such macro-phenomenal signs as support for this hypothesis.

One further question is whether Gassendi's development and use of a corpuscularian hypothesis is paradigmatic of, or exceptional to, his theories of empirical knowledge and scientific method. If, as I suggest in my third and concluding chapters, this hypothesis is exemplary of those views then we may need to fine-tune some of our worries about what to expect from sign-based inference and conjectural hypotheses in a maximally-empiricist science such as Gassendi desires. For example, the corpuscularian proposal that the microworld resembles, and/or operates similarly to, the macroworld, raises the question of how we might think there are such relations as these resemblances or similarities that hold between what we perceive and what we don't or can't perceive. We should wonder not only about the evidence for those particular relations, but as well about the sorts of relations our accepted physical and logical constraints will allow us to build into such conjectures in the first place. As I indicate in the following chapter,

Gassendi is sufficiently concerned with such constraints to discuss and promote them, though he turns out not to be sufficiently interested to allow them to shape his specific brand of corpuscularianism.

CHAPTER III

GASSENDI'S ATOMIST HYPOTHESIS

Among the first steps Gassendi makes in laying out the broad geography of what can be known empirically--in the form of his general physics--is his introduction of a basic ontological thesis, atomism. While he tenders this thesis as a radical alternative to contending matter theories of his day, he makes no pretense as to its absolute novelty, and in actuality he is following in a long tradition of views including classical atomism and the related *minima naturalia* theories of the non-atomist scholastics. Indeed, the central claims of his atomism most closely resemble those of his ancient forerunners Epicurus and Lucretius: that there are two sorts of extended things, which are matter and void, that matter cannot be physically divided beyond a particular minimal component which itself has no parts, and that everything is composed of some combination of void and those elemental components of matter. From this last claim Gassendi develops the ancient atomist suggestion that all macro-sized properties and phenomena are the product of the combinations and actions of matter's ultimate parts. In short, he starts by adding this ontology to his mechanical world picture, and wants to end up with at least adequate, and perhaps even truth-resembling, accounts of physical phenomena. One requirement for achieving this last goal, naturally, is that his atomism is consonant with the other elements of his mechanical philosophy. But Gassendi's empiricism imposes an additional

constraint, which marks him as among the first modern atomists: since these atomist views are at the core of his physics and so (by his own reckoning) constitute empirical knowledge they should be largely if not wholly derived from sensory data.

Gassendi has rather mixed results in meeting these constraints. To begin with, his atomism sustains a picture of the world where all action is by the contact of one body with another. Yet it fails to wholly satisfy the tenets of the mechanical philosophy, most prominently because it violates the same principle of inertial motion for which he is reknowned in the annals of physics, and which he gives good reason to suggest is supposed to range over the motion of all bodies no matter their size. With respect to the empirical grounding of Gassendi's atomism, all the evidence he adduces for that hypothesis is at best indirectly the product of sensory data from observation or experiment. Though some might well balk at saying this satisfies a thoroughgoing empiricism, Gassendi's theory of sign-based inference suggests this means of gathering and inferring from the available evidence suffices for saying atomism is justified on empirical grounds.

A. Corpuscularianism, Mechanism, and Empiricism in the Seventeenth Century

Two novel and paramount tasks face corpuscularians of the early modern period. First, they must provide a suitable ontology for the mechanical philosophy which, among other things, means making the mechanist picture work all the way down to the subvisible level, and building that picture up from that level. One reason this is necessary is to guarantee what Dennis Deschene calls the 'scalar invariance' of physical laws, that such

laws work across the spectrum of magnitudes. Second, they must meet the empiricist constraints and interests of the new science which, among other things, means saying what empirical evidence it can be possible to adduce for atomism or other corpuscularian views.

As regards the first task, let us begin by understanding what is meant by 'mechanism' or the 'mechanical philosophy'--and how atomism as an ontological thesis might relate to it. I think the early modern philosophers and scientists who espouse the mechanical philosophy seek to promote a group of theses--some of which are scientific claims *per se* and some of which are claims about the way science should be done--but all of which are based on the view that material objects behave in the ways of artificial machines such as (to take a popular seventeenth century image) clocks. In brief, their behavior is--at least in principle--regular, measurable, repeatable, and predictable, and it is the product of the behavior of their component parts.

Martin Tamny distinguishes two facets of the mechanical philosophy. For one, he suggests, there is a mechanist methodology which says that our best physical hypotheses tell us about those 'real properties' of bodies which underlie manifest phenomena and that such hypotheses are supported by our best evidence, empirical or otherwise. For another, there is a corpuscularian ontology which says all macro-sized objects consist of micro-sized parts that bear those real properties. What ties the two theses together is the suggestion that, given the corpuscularian ontology, our best physical hypotheses will locate the quantifiable, real properties of bodies in corpuscles and so allow us to

reinterpret the behavior of all macro-physical objects in terms of the actions, interactions,¹ and states of their constituent corpuscles.²

This promise of mechanically explaining all physical phenomena by reference to the real and quantifiable qualities of matter might look somewhat reasonable in Descartes's hands, more so in Newton's, and somewhat less so in Gassendi's, as a function of their facility with mathematics and measurement. But the resulting outline for explaining physical phenomena is roughly similar for the various mechanical philosophers. Such explanations consist in giving (what are in the optimal case precise) descriptions of the sets and relations of basic properties of the elemental units of matter, and showing how these descriptions can be derived from, or are at least consistent with, our basic physical (and metaphysical) commitments--whether derived from experience, reason, or both.

This much strikes me as accurate but I believe there is at least one other thesis the mechanical philosophers generally share, namely, a commitment to identifying dynamical

¹That corpuscular interaction plays such a central role in this view, and many of the mechanical philosophers are committed to a 'no action at a distance' thesis, might seem to suggest that a corpuscular ontology is just what mechanists need to establish the latter point, but it is important to remember that there are a number of independent historical reasons for favoring the 'no action at a distance' thesis, among them a long tradition of rejecting the void. Moreover, it is not clear that a corpuscular ontology alone guarantees that there should be no action at a distance.

²The suggestion here is that there are methodological and ontological theses in most if not all instantiations of the mechanical philosophy, and the two are not the same thing. It is, naturally, a further step to suggest that the two theses are genuinely considered to be independent by any particular author. But Tamny's point is rather that the central thesis of the mechanical philosophy is the methodological one, whereas the ontological one is (just) an important ancillary. It might be inseparable in the minds of the mechanical philosophers themselves for various reasons, but it doesn't follow that they are indeed one and the same thesis or even that they can't logically be separated.

principles which govern the motion of matter, and thus the behavior of bodies, and to the view that these principles lend a regular, law-like character to the explanations they suppose their methodology to generate.³ More specifically, most if not all of the central figures among the mechanical philosophers hold that some foundational principles of kinematics or dynamics (e.g. a principle of inertia, a principle of conservation of motion or momentum) govern the motion of matter and so the behavior of all bodies.⁴ Such principles are crucial to the mechanical philosophy⁵ because they are required in order to effect the methodological thesis which, let's recall, promotes the explanation of phenomena like physical change and the sensation of manifest properties by reference to real properties. Thus precise characterizations of matter in motion allows accurate

³I think we can also generally identify in the works of the mechanical philosophers an *epistemological* thesis something like this: whatever qualities we know about through sensation are explainable in terms of the few basic properties of bodies.

In other words, to account for our coming to know of sensible qualities, we need (among other things) to identify the fundamental physical properties and our means of interaction with them. This thesis, too, strongly suggests a corpuscularian ontology, but it does not follow from this strong suggestion that the ontology of mechanism and mechanism itself are either identical or indistinguishable.

⁴This is not to suggest that the mechanical philosophers necessarily viewed such principles as sufficient for accounting for the behavior of bodies. Indeed, it is not even clear that they always thought such principles could fully account for the motion-phenomena in question. Thus, for example, the phenomenon of inertial motion for Newton was in some way related to the *vis insita* he held to be a real and inseparable property of bodies--in addition (and, some would say, in direct contradistinction) to its relation to the inertial principle he proposes as governing the motion of bodies. Thanks to Martin Tamny for this point.

⁵Naturally, the mechanical philosophy is not unique in this regard. Earlier attempts at fundamental principles of motion include, of course, Aristotle's noninertial views, but also the impetus theories developed by Jean Buridan and Nicholas of Oresme. As I. B. Cohen suggests, however, these are more aptly viewed as belonging to kinematics than to dynamics *per se* because there is no concept, or even suggestion, of 'force' or 'mass' in these earlier physical accounts. In any case, the novelty I am concerned with here is the role dynamical principles play in the context of the mechanical philosophy, telling us what constraints exist on the motions of bodies such that they behave with the sort of regularity we expect from the mechanist picture.

specification of the physical states of whatever bears such real properties, and regular (or minimally, regularizable) characterizations of matter in motion fulfill the promise of the machine metaphor—that mechanistic accounts have predictive power and the phenomena are repeatable (at least in principle) because the physical behavior as characterized is law- or principle-abiding.⁶

Armed with such laws of motion and the intention of employing a corpuscularian matter theory to give micro-physical accounts of macro-physical phenomena, it seems quite reasonable (at least given the hindsight of classical mechanics) on grounds of parsimony to have the same laws of motion govern both micro-sized and macro-sized bits of matter. For if these laws varied depending upon the size of the matter in motion, we would need extra rules to say when a given bit of matter was governed by which laws, and we would have the likely complex task of saying how this was possible to begin with. As it happens, whether or not the mechanical philosophers sought to avoid this sort of task, they generally adopted or assumed a meta-principle of the *scalar invariance* of their physical laws. Consequently, whatever physical laws govern macro-sized phenomena will also govern micro-sized phenomena, and vice-versa, so that the fundamental principles of motion should apply equally to atoms, mid-sized objects like tables and chairs, and objects of great scale like the planets.

So, for example, as Dennis Des Chene has noted, Descartes explicitly states such a meta-principle in an attempt to insure that his rules of motion range over all extended substance, such that his corpuscles and planets move in the same sorts of ways (very

⁶Thanks to Lisa Downing for discussing these points.

generally speaking), and the kinds of mechanical explanations we generate for either will closely resemble one another relative to those rules. To be sure, Descartes's continuum of matter gives him a special reason to hold that his rules of motion apply independent of scale. But the other mechanical philosophers also had good cause for maintaining a scalar invariant physics, and I propose that Gassendi held such a view for reasons both on atomist and general physical grounds.⁷ One example of the latter is that Gassendi is clearly interested in a dynamics which treats micro- and macro-sized bodies equitably. Thus, he takes his concept of weight in the case of atoms to correct the general Aristotelian view about the fall of bodies--that the natural tendency of bodies is to fall downwards, towards a worldly center:

"By weight...we must not understand an inclination of atoms toward the center of the universe...but a 'force' or natural impulse [that moves atoms] from one part of the universe to another without end."⁸

That there is no center of the universe towards which bodies fall is also the crux of his argument for a principle of inertia in *De Motu*. Of course, it's possible that Gassendi could construe the Aristotelians as being wrong in the context of the passage above on

⁷Martin Tamny has suggested another sort of motivation for the mechanical philosophers to have adopted a meta-principle of scalar invariance: by doing so they provide warrant for those claims about physical phenomena on one scale which they infer from claims about such phenomena on any other scale. Scalar invariance is beneficial, then, because it allows these scientists and philosophers to generate the sorts of results they are hoping the mechanical philosophy will yield. To attribute this motivation in Gassendi would be a reasonable supposition given his view that various elements of scientific method are best characterized in terms of utility (see previous chapter). Incidentally, alternative theses which suggest that macro-level events are radically distinct from micro-level events do not bear the same explanatory yield (for example, where the former are emergent phenomena relative to the latter) or else rely on notions unavailable to the early moderns (for example, where the former are statistical outcomes of the latter).

⁸MS Tours 709 folio 85r, from Brundell, *Pierre Gassendi*, 119-120.

wholly distinct grounds. But it seems unreasonable to suppose he takes his proposal concerning the fall of atoms to correct the Aristotelian view of falling macro-sized bodies unless he also thinks the falls of such bodies and their corpuscular components share the same dynamic characteristics (even if, somehow, they do not share the same causal principles).

Below (section B.3.c.), I explore in greater detail such issues concerning the mechanical philosophy as result from Gassendi's views of atomic motion. For the moment, though, let me close this discussion by sketching what I believe to be the mechanical philosopher's plausible expectations of an ontological thesis like the atomist hypothesis. If my characterization of that philosophy is close to the mark, then it seems the atomist hypothesis has a minimally fourfold significance in this context, not only as a prominent variant of mechanist ontological claim, but also as it corresponds to the aforementioned epistemological, methodological, and dynamical claims. I discuss some of atomism's consequences for the epistemological and methodological theses of the mechanical philosophy in my concluding chapter; in the present chapter I focus on some knotty questions concerning the motion of bodies, including what if anything accounts for variability in their motion, whether and how motion might be conserved, and whether atomic motion is to be construed as inertial along with supra-atomic motion. It emerges that Gassendi's atomism does not sit perfectly with these mechanist claims, which indicates that he has not been entirely successful in equipping his broader physics with an atomist ontology. Naturally, though, he would have been further remiss for not having tried, and these two ends of his physics at least feature some suggestive points. His

atomism suggests, for one, how physical phenomena on all scales results from the properties, relations, and states of micro-sized bits of matter, and his dynamics suggests, for another, that the motion of matter obeys cardinal principles which anticipate, at least crudely, two of Newton's laws of motion. One very enticing aspect of this whole picture is that Gassendi appears to have attempted to surpass a purely kinematic account insofar as he conceives of the motion of larger bodies as the consequence of their 'motive force' which is the composite of their constituent atoms's motion--which in turn he takes to result from their inherent weight. Given the difficulties in integrating his macro-physics and micro-physics, however, it is hard to see how he could have successfully developed the dynamics he outlines.

An altogether different sort of difficulty for Gassendi is saying whether his atomist hypothesis meets his own empiricist constraints. One way of putting this issue is to ask what empirical evidence for atomism would look like given that atoms are supposed to be below the threshold of perception. This particular way of putting things is historically significant in that the distinctive ontologies of various mechanical philosophies were largely not distinguished in their own times, and Dijksterhuis speculates that this is precisely because neither the theoretical entities they proposed nor the behavior of those entities were distinct on the observable level.⁹ Whatever the oversights of seventeenth century chemists and savants, the distinctions between atomism

⁹E. J. Dijksterhuis, The Mechanization of the World Picture (Oxford: The Clarendon Press, 1961), 429. Thus Descartes's parts of space (whether at rest or in motion) behave just like fine particles of matter, his fine particles in turn behave like atoms or their aggregates, and his interstices of celestial matter behave like the *vacuum separatum* proposed by anti-plenists like Gassendi.

and other corpuscularian or non-corpuscularian matter theories are clear enough and below I detail what these amount to, at least in Gassendi's view. But were such distinctions supposed to be empirically-rooted at all, and if so, how? I agree with the prevailing view that his arguments for atomism are by and large an *apriori* affair, and that, as Lynn Sumida Joy has proposed, historical analysis and tribute significantly shape the content of Gassendi's atomist claims. However, I propose that some of his historically-based arguments, and other, more original arguments depend on what at least Gassendi would construe as empirical evidence. That he would make this construal, I propose, depends on his notion (visited in chapter one) that our inferences based on the signs of unobserved phenomena constitute empirical reasoning such that the beliefs we thereby acquire or develop are the stuff of empirical knowledge.

Before I address these issues, though, there is much to be said about the nature of Gassendi's atomist hypothesis itself. I begin this chapter by briefly reviewing the historical path of corpuscularian and other matter theories which form a historical background, and contribute in various ways, to Gassendi's version of an atomist programme. Next, I examine the specific claims and contours of his atomism, paying particular attention to the case of atomic motion as a source of problems for his broader mechanist claims. On the other hand, his use of an atomist hypothesis in his accounts of attractive powers and organic generation reflect an attempt to provide an ontology for his physics which (broadly) conforms with his principles of motion. These accounts further suggest that Gassendi does not insist that atomist explanations be empirically viable in order that we initially entertain them, and that in pursuing his atomist programme he is

primarily concerned with identifying a plausible picture of the most readily identifiable underlying micro-structure of material objects and less interested in identifying what might in turn underlie *those* structures--or what may amount to the same question--why we should expect to find one sort of structure and not another.

*B. Changing Concepts of Smallest Particles: From Ancient Atomism
and Aristotle's Minima Theory to the Minima Naturae of Iatrochemistry
and Physical Corpuscularianism*

One key feature of the new science in the seventeenth century is the reemergence of the antique view that we best account for the nature of matter and the manifest qualities of familiar mid-sized material objects by postulating subvisible basic elemental particles from which all such objects are constituted. This early Modern corpuscular matter theory incorporates elements of two competing ancient perspectives, the atomism promoted by Leucippus (fifth century BCE), Democritus (460-370 BCE), Epicurus (341-270 BCE), and Lucretius (100-55 BCE), among others, and the theory of *minima naturalia* inspired by Aristotle's decidedly non-atomist picture of smallest physical parts. By the time Gassendi argues for adopting a largely Epicurean corpuscular matter theory as one of his central physical hypotheses, earlier corpuscularian theorists (antique and modern) had explored a wide range of results for taking particulate views of matter as foundational in physics (and in metaphysics, too). In the review of such theories that follows, I focus on Epicurus's atomism in particular since for Gassendi it is his views which hold the greatest promise among the corpuscularian alternatives.

Much of what unites the ancient atomists is articulated early on, by Leucippus and

Democritus,¹⁰ who attempt to explain the persistence, change, and manifest qualities of material objects, by the shape, arrangement, position, and interactions of their unchanging and indivisible constituent elemental particles.¹¹ There are infinitely many atoms moving constantly, *ad infinitum*, through a void of infinite expanse. Atoms themselves lack any internal void and so are wholly solid. Thus when atoms collide, one cannot invade the space of another; rather, they either bounce off one another or else come together to form aggregates (they cohere by means of mutually fitting protrusions or mutual pressure), out of which mid-sized objects as well as the Earth, heavenly bodies, and the vortices which generate and transport them are all constituted. Although atoms themselves are of homogeneous composition, they differ in shape and size and--since they are all of the same stuff with constant density--weight. These atomic qualities don't however change over time; otherwise atoms would lose the stability which the theory suggests makes them basic particles of matter.

Epicurus's chief advance beyond Leucippus and Democritus is undoubtedly his development of what has come to be recognized as the canonical reasoning for the primary atomist claims--that it is metaphysically necessary and empirically supportable

¹⁰Sources for Leucippus include: DL IX 31; and Aristotle's De Gen A §8. Sources for Democritus are more plentiful and varied, but some important ones include: DL IX 34f, Simplicius's De Caelo 293 and Physicorum 28 §15; Galen's De Elementis I 2; Theophrastus's De Causis VI §1.6 and 7.2, and De Sensu §49f. Numerous fragments are collected in Nahm (1964).

¹¹One prominent goal of the early atomists is a reconciliation of the Heraclitean claim that change is everywhere in the world with the Parmenidean claim that change is impossible (because whatever is, is, whatever is not, is not, and nothing passes from one to the other). Blending elements of these claims, they suggest something does exist without change for all eternity--the constituent particles of all matter--and it is just the nature and behavior of these particles which accounts for all apparent change.

that (i) the basic particles of all matter are indivisible and (ii) describing the nature and behavior of such particles suffices to account for all qualities of familiar macro-sized objects. Specifically, for example, in his Letter to Herodotus¹² Epicurus argues that atoms must exist if we give credence to the Parmenidean claim that nothing comes from or passes into nothing, and that we have empirical evidence for an atomist picture of the world given that we can infer from our sensory perceptions that there are such things as atoms (with their attendant qualities), not by having sensed them directly but instead interpreting what we do perceive as signs of the same. Even if we don't experience atoms first-hand, the suggestion is, atomism provides our only satisfactory account of phenomena we can experience.

Epicurus first proposes that perceptual data provides compelling evidence for the existence of bodies as well as their motion and transformation,¹³ and next, that these claims oblige us to accept the picture of bodies as composed of atoms, or the smallest bodies, in motion.¹⁴ This last bit of reasoning goes: If we perceive bodies in motion or undergoing changes, it must be because some of them have parts and others *are* such parts, and the latter kind must be indestructible or else they would not persist through change and dissolution of composites. But we know something persists through such changes among mid-sized objects, because whatever is there afterwards did not emerge from thin air--nothing exists which either comes from or passes into what does not exist.

¹²DL X 35-83.

¹³DL X 39-40.

¹⁴DL X 41.

Indeed, to speak intelligibly of change in a given mid-sized object, there must be something which maintains its identity throughout such change--otherwise there is no way to assign change to one and the same object--and we can only have this identity if some elements or parts of the object persist throughout. These parts which persist through those changes, Epicurus stipulates, are simply those indivisible bodies we call 'atoms'.¹⁵

There are several potentially troublesome assumptions here but perhaps the most troubling is the Parmenidean claim that nothing comes from or passes into nothing. Epicurus does not bother to suggest why we should think this true, nor why we must rely on it to explain the persistence of material bodies through change. If we knew, as Epicurus hints, that change involves interaction of matter only, we might reasonably infer that any change involves material bodies at its beginning and end, so that whatever persists through change must be some sort of indestructible matter. But this begs the question as to why change must involve interaction of matter only--and the only ready answer seems to be that *nihil ex nihilo*, by fiat.

Looking beyond such arguments for the existence of atoms, Epicurus retains the basic model of the earlier atomists--there must be void through which bodies may move, there is an infinite number of atoms, atoms differ in shape and size --yet he also adds numerous and sometimes divergent details--there are not infinitely-many kinds of atoms,

¹⁵Epicurus infers from this that they must persist forever, yet given the limits on what we observe of anything's persistence, it seems we have grounds for inferring no more than that they persist a long time.

and the total number of bodies is somehow constant.¹⁶ His most notable elaboration of earlier views, though, is his account of atomic motion--a chief component of his micro-phenomenal account of macro-phenomenal change.¹⁷ The better part of this account simply echoes Democritus and Leucippus, but he also adds the suggestion that the ever-present motion of atoms is an automatic result of their ever-present tendency to fall downward given their inherent weight.¹⁸

According to Epicurus, that atoms have a fixed weight and that this contributes to their constant motion yields a few characteristic results, for example, that all atoms move at equal velocities, and that atoms tend to move in one direction, namely, downward.¹⁹ This presents a difficulty: how to explain that atoms collide, move to the side, or move upward, if their inherent motion gives them the tendency to fall downward? From Lucretius and a number of commentators we learn that atomic motion in the Epicurean model incorporates a *clinamen* (swerve)--an aberration from the otherwise typical downward fall at a uniform rate.²⁰ These commentators suggest the *clinamen* insures that there will be atomic collisions, and that from such collisions atoms may be deflected sideways or upwards. Yet it is not clear Epicurus needs this additional swerve, given that it is introduced solely on the presumption that the downward path of atoms alone would

¹⁶DL X 41.

¹⁷DL X 54.

¹⁸DL X 61.

¹⁹DL X 61-62.

²⁰Epicurus himself does not mention the *clinamen*; one early source attributing it to his atomism is Cicero (for example in On Fate 18).

never allow them to collide. For this presumes in turn that there is one absolute sense of 'down', a view which Epicurus explicitly rejects.²¹ At any rate, Epicurus clearly wants atoms to collide in *some* way so as to produce not only upward and sideways motion but indeterminate states of affairs, because--unlike Democritus, perhaps--he wants his atomism without determinism.

We find another elaboration of earlier views in Epicurus's account of atomic size and indivisibility. Though he accepts the Democritean notion that it is easier to account for variety among macro-sized objects if we stipulate more differently-sized atoms, he rejects the suggestion that there are or need be infinitely many sizes, on the grounds that there are limits on what sizes atoms can be. On one hand, no macro-sized atoms have ever been perceived, and this suggests a limit on how large they can be.²² On the other hand, there should also be a minimum size for atoms, beyond which point matter can't be physically divided, or at least never is. In this vein he argues that must be a minimal size beyond which *any* finite body cannot be divided because no finite bodies can be divided infinitely. If they could be, then they would include infinitely many extended parts yet they have finite extension--hence infinite divisibility is impossible.²³

One further argument Epicurus offers in this context appears to be an attempt to establish the indivisibility of atoms while recognizing Aristotelian concerns not with atoms *per se* but with physical *minima*, bodies relative to which no others are smaller.

²¹DL X 60.

²²DL X 55-56.

²³DL X 56-59; cf. also Lucretius, *De Rerum Natura* I: 599-634.

Thus Epicurus suggests that minimal physical bodies cannot be divided though they may have parts, on the basis of an analogy he draws with minimal *perceivable* bodies which he claims don't have perceivable breadth²⁴ though they cannot actually lack breadth. Minimal perceivable bodies have no perceivable breadth in the sense that one cannot distinguish their parts by the senses yet they must have breadth since we can measure magnitudes by augmenting or diminishing their numbers, so that x minimal perceptibles in a row constitutes a measurable span (namely, of x minimal perceptibles's length). The same holds true of the atom, or physical minimal: "It is obvious that it is only in its smallness that it differs from what is observed in the case of perception, but does stand in the same relation".²⁵ In brief, Epicurus suggests physical minima do not have physically distinguishable parts though they must have magnitude (and so parts) if we are to say that atomic aggregates have magnitude. This sort of analogy seems doomed from the outset, though, for we cannot establish the indivisible character of one kind of minima by the indivisible character of the other, given that one sort of divisibility is physical and the other perceptual.²⁶ Worse still, we should hope to be able to divide perceivable minima

²⁴The actual term Epicurus uses is 'traversability', which seems to be most usefully understood in his writings as a thing's having breadth.

²⁵DL X 58-59.

²⁶David Furley points out that such reliance on analogy may cut both ways: we might think we can take apart whatever has parts on the micro-level just as we can on the macro-level. But then, since perceptible minima have physical parts which can, at least in principle, be taken apart, Epicurus should accept that atoms are physically divisible, too. Instead, of course, he holds the curious position that even though atoms have parts (simply because they are extended) they cannot be taken apart. See David J. Furley, "Knowledge of Atoms and Void," in Essays in Ancient Greek Philosophy, ed. J. P. Anton and G. L. Kustas (Albany: State University of New York Press, 1971), 614.

so long as we assume that physical minima (or anything else) are smaller--so if the analogy really did hold we might expect to be able to divide physical minima, too. Furley is no doubt right to suggest that Epicurus is trying here to offer an argument for atomic indivisibility from experience,²⁷ but it does not seem a particularly successful attempt. This failure, however, does not stop numerous subsequent commentators (Gassendi among them) from trying to salvage a viable argument for indivisibility from some of the pieces of Epicurus's account.

Finally, what is additionally distinctive here is the range of applications Epicurus seeks for atomism. He picks up the atomist (and materialist) thread running through the cosmology, psychology, and biology of Leucippus and Democritus, and provides a systematic atomist picture of natural and social phenomena, including the moral life.²⁸ So while subsequent commentators easily separate his hedonist ethics from his physics, there is a clear enough connection between, for example, his views that we should lead lives which enrich our happiness and friendships and that the material character of the soul

²⁷What makes this an argument from experience, Furley suggests, is that Epicurus thinks he is inferring the features of subperceptible minima from those of perceptible minima; see "Knowledge of Atoms," 614. This seems fair enough, if as Epicurus we count inference from the perceivable to the nonperceivable as viable argument from experience; as we see below, Gassendi adopts a similar strategy among his defenses of atomism.

²⁸This cursory review of Epicurean atomism is incomplete without mention of the accompanying cosmology which, by the way, Gassendi rejects nearly in its entirety. To begin with, there are infinitely many universes, and uncountably many gods which sit between the universes. They cannot be the creators for Gassendi because they themselves are composed of atoms (and because the universes are around forever 'in both directions'). The infinitely great amount of stuff, time, and space has the curious result that all possible physical combinations--e.g. atomic aggregates--are actualized at one point or another. As Philip de Lacy has noted, this in turn has the still more curious apparent metaphysical result that, while the nature of experience limits our sense of the possible, what is truly possible is limited only by the incredibly expansive (infinitely so!) actual; see Philip De Lacy, "Epicurus," in *The Encyclopedia of Philosophy*, ed. Paul Edwards (New York: Macmillan and the Free Press, 1967), vol. 3: 3-5.

entails its mortality--so that consolation lies in the present life. Such a robust set of explanations for a broad spectrum of natural and social phenomena gives a rich texture to ancient atomism--which generations of medieval and early Renaissance scholars and theologians come to find quite repellant--but which eventually attracts the late Renaissance students of the ancient world, and (save the material soul) Gassendi, as a promising and systematic alternative to the reigning Aristotelian world-picture.

The last of the major orthodox ancient atomists²⁹ is the poet Lucretius, who largely echoes Epicurus's views in his De Rerum Natura. His primary innovation is to introduce the clinamen on the grounds that nothing else explains collisions or the indeterminacy of motion he supposes necessary to accounting for free will. One of his arguments in this matter deserves attention. In Democritean atomism, it is the heavier atoms falling straight down on the lighter ones that causes collisions (the system being determinate in any event) yet Democritus claims atoms travel through the void, where there is no reason to assume the heavier ones will fall any faster than the lighter ones. So we need to introduce some other cause of motion to explain such collisions and Lucretius stipulates the clinamen as an alternative. This is, by itself, not a particularly good reason to accept the clinamen as a specific alternative, but the argument is noteworthy anyway because Lucretius postulates something that looks like weightlessness in the void. It would be anachronistic to make much of this, but it is interesting that Gassendi, as we

²⁹One other significant corpuscularian of the ancient world is Hero of Alexandria, who suggests in his Pneumatica that the world is composed of micro-sized, non-atomic particles and interparticulate void. Gassendi borrows in part on Hero's views on the void, though not on his particular brand of corpuscularianism; see O I 166, 172, 176.

will see, also seeks to insure that atoms have within them a ready source of motion--though without the clinamen and on a completely distinctive basis.

In response to the atomism of his day, Aristotle suggests we have a satisfactory account of change, stasis, and the qualities of material objects if we describe their constituent primary matter and substantial form.³⁰ The difference between Aristotle and his atomist opponents is not primarily over the reasoned or empirical merits of talk about smallest particles but over the conceptual role which such particles should play in physical theory. Thus Aristotle agrees with the atomists that, by the lights of experience (in particular, as shown by chemical reactions), material objects can be categorized by their constituent elements and that, by the lights of reason, there must be limits to the divisibility of particles (at least relative to particles of living things).³¹ However, he rejects their chief tenet, that the smallest particles are also the elemental building blocks of material objects. Rather, he proposes, the elements are chemical simples like air, water, fire, and earth, which combine to yield the different material substances with their particular qualities. The smallest particles, then, are no more than the physical minima corresponding to particular sorts of material substances they compose--that is, the

³⁰Generally speaking the atomism of the day would have been that of Democritus and Leucippus, but Aristotle may also have been replying to Plato, who recounts a myth (in the *Timaeus*; cf. 53c-57e) about the micro-physical structure of material objects. According to this myth, the elements of fire, air, water, and earth are composed of one of four basic types of (divisible) particles the arrangements and interactions of which help explain the qualities of those elements and the behavior of bodies they form (58a-64a).

³¹*Physics* I 4 (187b 28-34).

component pieces of a given substance's structure.³² And so, unlike Democritean atoms, these minima are not ontologically or conceptually basic to the physical theory, given that there should be a hylomorphist story to tell about how they came to be the particular kinds of (minimal) bodies they are. Hence they cannot be significant in determining the nature of the substances they compose, because they themselves are subject to change just in case either their own matter or form changes. Despite his concessions to the atomists, then, Aristotle never allows that there are smallest particles which share common features.

These Aristotelian views form the core of medieval discussions of manifest properties of material objects and, with minor alterations, they also provide the backbone of the 'Peripatetic' iatrochemistry heralded by van Helmont and Paracelsus. What unites authors throughout these various traditions are the views that an object has one or another manifest property *because* it has received the form(s) of one or more of the three or four principle elements in the right combination,³³ and that any subvisible structure material objects have is more or less irrelevant to such an explanation. One prominent response to

³²Physics VI. Nothing Aristotle says in the Physics rules out that a structure or its properties change when its initial particles are displaced, replaced, or added to, so nothing in Aristotle's view rules out a mechanical account of change, persistence, or the nature of properties. It's simply that he doesn't find such accounts elucidating because he takes the substantial form and primary matter of substances to be explanatorily paramount and any other aspects of change, persistence or the nature of properties to be derivative. While Aristotle and his followers might not have been captivated by a mechanical picture of matter, they (or at least Aristotle) did not deny it either, and from his own times onward there are many scientists who are acutely aware of the mechanical nature of many properties, including for example sound. What this suggests, rather naturally, is that to hold a mechanist view of material change and stasis does not require an atomist picture, and it seems the reverse should hold true as well.

³³The Paracelsans replaced Aristotle's set of four elements with the *tria prima* of (what they thought of as) mercury, salt, and sulphur.

this medieval and Peripatetic view of matter is the '*minima naturalia*' or natural minima theory, which makes one early appearance in sixteenth century iatrochemistry in the writings of Julius Caesar Scaliger (1484-1558). Scaliger recalls ancient atomism by suggesting that the subvisible structure of material objects is not irrelevant to explaining their manifest properties because not only are there smallest particles (to which Aristotelians may agree), but they are elementary in some respect. Moreover--and here he departs from the atomists--these natural *minima* share most or all kinds of features larger objects may have. These minima are at the limits of physical divisibility, like the atoms of the ancient models, yet they are thought to have some inherent fixed qualities which atoms do not feature, like coarseness or fineness. The principle difference with the atomists, though, is that Scaliger and other minima theorists of his time accept the Aristotelian picture of chemical composition as the interaction of the four elements, all the while suggesting whatever compounds we form result in part from the commingling of minima. So even as Scaliger and other minima theorists edge away from the notion that substantial form alone determines the character of substances, they continue to appeal to form and the basic elements (either Aristotelian or Paracelsan versions)--as well as the configuration and disposition of minima--to explain the generation, corruption, and standard qualities of individual substances. Thus, one debate among the minima theorists centered around the issue of whether, in chemical composition, when the minima of reagents commingle, the forms of the reagents subsist and/or a new, higher-order unified

form emerges.³⁴ The mark of natural minima theory, then, is its reaffirmation of an Aristotelian formal account of physical and chemical properties--which seems so curious to late seventeenth-century corpuscularians like Boyle who insist that an adequate account of such properties may appeal only to the basic structural elements of matter.

At the beginning of the seventeenth century, though, a new generation of iatrochemists, inspired by advances in experimental technique, further pursued the natural minima programme--now with growing hints of atomism. In the eclectic theory of Daniel Sennert (1572-1637), for example, minima are conceptually merged with Democritean atoms, which he understands to have chemical properties; as Emily Michael has recently suggested, Sennert's views give the lie to the notion that atomism and hylomorphism are doctrines that never meet in a single author.³⁵ David Van Goorle's Exercitationes Philosophiae (1620) promotes an atomism which, like Sennert's, borrows from the Paracelsan themes spelled out by Scaliger: we can explain chemical changes like condensation or rarification by appealing to the behavior of atomic parts of a given substance, but the range of changes which can take place are just those allowed by the nature of the basic elements from which a substance is composed. And one work in the Scaligerian vein is Sebastian Basso's Philosophia Naturalis adversus Aristotelem (1621), where we find an early molecular picture of material structure: elementary particles come

³⁴Andrew G. M. Van Melsen (1952) offers one well-known, if recently less venerated, discussion of these issues; see his From Atomos to Atom, trans. Henry J. Koren (Pittsburgh: Duquesne University Press, 1952). A more thorough and well-rounded treatment will appear in Foundation for Intellectual History, Medieval and Early Modern Corpuscular Matter Theory, Proceedings of a Conference/Workshop at St Andrews University, St Andrews, Scotland, August, 1996 (forthcoming).

³⁵See Late Medieval and Early Modern Corpuscular Matter Theory.

together to form secondary aggregates and these in turn come together to form tertiary aggregates, the higher-order compounds having generally greater stability than the lower-order ones. Yet this programme ran its course in short order--no one could produce, of course, compelling empirical evidence on behalf of those formal, qualitative elements of minima theory³⁶--and, with renewed interest in ancient atomist writings, early modern corpuscularians for the most part dropped their Aristotelian trappings in favor of views closer to those of the ancient atomists.

A proliferation of new atomist views was not, however, a guarantee of acceptability. In a well-known incident of 1624, Jean Bitaud, Antoine de Villon, and Etienne de Clave attempted to publically argue the atomist case against Aristotelian hylomorphism by drawing on experimental evidence, before a Parisian public audience. Their demonstration was halted by the authorities (on theological grounds, it seems) and the demonstrators arrested. And ten years later, Descartes suppressed the publication of his *Le Monde*--not, perhaps, because of the corpuscularian views he expresses there, though neither would those views have commended the work as doctrinally passable in

³⁶Yet what kind of empirical evidence could have been hoped for relative to quantitative elements of atomism? While several authors (including van Melsen (in *From Atomos*), Dijksterhuis (in *Mechanization*), and Pancheri (in "Atomism")) suggest atomism supplants natural minima theory in the seventeenth century because the former lends itself more easily to quantified description than does the latter (though this is not so for Gassendi) this does not mean, of course, that empirical evidence was any more forthcoming in this regard than it was for the natural minima theorists (in regard to the qualities of minima). Meinel recounts the disappointing efforts of numerous atomists, and in this light it seems the empirical evidence Gassendi offers for his brand of atomism is all the more plausible precisely because he is not as interested in a quantitative account as he is in a qualitative account; see Christoph Meinel, "Early Seventeenth-Century Atomism: Theory, Epistemology, and Insufficiency of Experiment," *Isis* 79 (1988): 68-103. Incidentally, Gassendi anticipates Newton's suggestion that sufficiently powerful lenses would enable us to observe atoms through microscopes; I discuss Gassendi's views on this matter in the concluding chapter.

the eyes of many Church authorities.³⁷ So it is against this backdrop of intolerance and somewhat rigid devotion to Aristotelianism during the first half of the seventeenth century that only a handful of less-well known corpuscularian works appear in print³⁸ and several larger figures (Descartes and Beeckman, for example) choose to express their corpuscularianism privately. This is the scene Gassendi enters when he proposes atomism as that 'most likely hypothesis'.³⁹

C. Gassendi's Atomism

The first signs of Gassendi's atomist sympathies appear in some of his earliest works and correspondence, in which he rejects much of the style and some of the substance of philosophy in the late Aristotelian tradition.⁴⁰ Indeed, some commentators have suggested Gassendi is an atomist not so much because he thinks it is the most likely physical hypothesis about the underlying structure of matter (as he suggests in the *Animadversiones* and the *Syntagma*), but because, as a thinker in the Renaissance mold, he simply dusts off this central component of Epicurus's package of views in an effort to

³⁷The *Principles* was, in the end, placed on the Index—though for attempting to account for the Eucharist in mechanical terms, and not for his corpuscularianism *per se*; see Roger Ariew, "Damned If You Do: Cartesians and Censorship, 1663-1706," *Perspectives on Science* 2 (1994): 255-74.

³⁸Some of these other corpuscularians of the early seventeenth century include: Nicholas Hill, whose *Philosophia Epicurea* (1601) anticipates Gassendi's project of reviving an Epicurean systematic philosophy; Claude Bérigard, who recycles antique atomist arguments against Aristotelian physics in his *Circulus Pisanus* (1643); J. C. Magnenus, who also revisits Democritean atomism in his *Democritus reviviscens sive de atomis* (1643); and Joachim Jungius, who (like Sennert) presents the transformation of metals in solutions as evidence of the discrete, particulate character of matter in his *Disputationum de principiis corporum naturalium* (1642).

³⁹Q I 335b.

⁴⁰See Rochot, *Les travaux de Gassendi*.

introduce a systematic philosophical alternative to the robust range of received Aristotelian views.⁴¹ Though Gassendi shares most of Epicurus's claims about atoms (and many of the Epicurean arguments for those claims), he disagrees on some key points, and this suggests it is the merits of the view itself, and not a general restoration of Epicureanism, which leads him to his brand of atomism.

Indeed, Gassendi states his primary grounds for embracing atomism clearly enough: he thinks atomism is a good theory relative to its competitors. Specifically, atomism offers explanatory power other views about the nature of matter cannot match:

...this theory of matter has the advantage that it does not do a bad job of explaining how composition and resolution into the primary elemental particles is accomplished, and for what reason a thing is solid, or corporeal, how it becomes large or small, rarefied or dense, soft or hard, sharp or blunt, and so forth....these questions and others like them are not so clearly resolved in other theories where matter is considered as both infinitely divisible and either pure potentiality...or endowed with a certain shape from among a very small range of possibilities, or endowed with primary and secondary qualities, which either do not suffice to explain the variety in objects or are useless....⁴²

However, even though he finds atomism to be the most promising matter theory among the various alternatives, Gassendi still thinks there is room for improving upon the ancient doctrine. This improvement is a question of conformity with something like the

⁴¹This is a common enough theme in much of the Gassendi literature (for example, in Rochot (*Les travaux de Gassendi*), Joy (*Gassendi the Atomist*), Osler (*Divine Will*) and it is the central thesis in Brundell (*Pierre Gassendi*). If this is really what interested Gassendi in atomism, there wouldn't be much of a point to studying his views over, say, Epicurus's. The real difficulty with this perspective, though, is that it distorts Gassendi's obsessive, learned rehabilitation of Epicurus into the sum total of his philosophical interests, instead of seeing it as the historical project he pursues at the same time as those distinct (though surely related) interests.

⁴²B 399 (*The Physics* §I, Bk III, Ch 8). By including views where matter is infinitely divisible and endowed with primary and secondary properties among the matter theories he deems less successful, Gassendi takes a swipe at Descartes's view.

rule that we may adopt an hypothesis we recognize is not entirely correct so long as we correct whatever is recognizably false about it: "...there is nothing to prevent us from defending the opinion which decides that the matter of the world and all the things contained in it is made up of atoms, provided that we repudiate whatever falsehood is mixed in with it."⁴³ In other words, atomism is a tenable hypothesis only if we first eliminate the falsehoods of the classical view:

...in order to recommend the theory, we declare first that the idea that atoms are eternal and uncreated is to be rejected and also the idea that they are infinite in number and occur in any sort of shape; once this is done, it can be admitted that atoms are the primary form of matter, which God created finite from the beginning, which he formed into this visible world, which, finally, he ordained and permitted to undergo transformations out of which, in short, all the bodies which exist in the universe are composed.⁴⁴

So although this cleaning-up bears salutary theological results,⁴⁵ it seems Gassendi's motivation in this regard is not primarily religious, for what makes the clean-up a worthy project is the notion that the theory is not wholly false or bears some resemblance to the truth about the nature of matter. Of course, after electing to fashion a 'truer' atomism, Gassendi lands on Church doctrine as the chief constraint guiding his amendment of ancient atomism, but it is not the only one.

⁴³Gassendi claims he can infer this rule from the attitude of the medieval French philosopher Guillaume de Conches (whom he refers to as 'Aneponymus') towards Epicurus, though his quotation from de Conches suggests instead that (a) there are no wholly false opinions yet (b) their true parts are unsalvageable anyway: "There is no opinion so false that it does not have some truth mixed in with it, but still the truth is obscured by being mixed with the false". B 398.

⁴⁴B 399.

⁴⁵As Gassendi puts it, "So stated, such an opinion has no evil in it which has not been corrected just as it is necessary to correct opinions in Aristotle and others which make matter eternal and uncreated in the same way, as others also make it infinite." B 399.

1. Gassendi's Background Metaphysics

In the first chapters of his Physics Gassendi introduces notions of absolute space and time according to which he views the universe as that which contains material Creation. In the colloquial language of metaphysics, he lays out the 'floor plan' of his picture of the universe in which he understands God to place the 'furniture', namely, atoms and their amalgams. More specifically, he holds that space, time, accident, and substance are the basic and real (that is, non-ideal) categories of existence, such that space and time are not modes of substance.⁴⁶

'Matter' refers to substance which exists in space and time; indeed, it is the sole and unchanging stuff of physical things and so must exist as long as there are physical things. If it did not, we would have to explain how something can come from nothing, which Gassendi takes to be naturally impossible (though not supernaturally, of course). Further, if as the scholastics maintain, matter was simply characterless stuff, we would have to explain how individuated things can be composed of something with no features. This charge is easily met within the confines of the scholastic view, for they take form to be what individuates things in the first place. But Gassendi wants bits of matter to have their distinctive qualities without the scholastic imposition of form; in particular, he proposes that matter has essential and accidental qualities and that we can only know the accidental ones, through experience.

The scholastics also distinguished between phenomenal qualities we know

⁴⁶Q I 184a.

experientially and a set of primary or fundamental elements which are the components of mid-sized objects and which, in virtue of their qualities, give rise to the phenomenal qualities. Thus mid-sized objects are hot because a significant part of their composition is whatever element that is characteristically hot. There are various views as to how many fundamental elements there are--three according to the Paracelsans, four according to the Aristotelians, and some combination of these last two sets according to van Helmont--but all these perspectives suggest there is nothing essential that all the elements share. Gassendi finds these accounts problematic on the grounds that we can't postulate enough primary elements to account for the variety we find in nature--the supposition here being that only micro-level variety among the elements or their properties begets macro-level variety. This approach is not very promising, for we do not need those sorts of variety on the micro-level to generate the broad spectrum of macro-level properties--what we need, at a minimum, is a broad range of possible structural combinations among the elements. This much was clear not only to the scholastics but to Gassendi as well, for it is precisely the means he proposes for generating macro-level variety. Indeed, having dismissed the scholastic view, he next suggests that we do not need to postulate more than one basic kind of matter, and that the combinations and interactions of matter's basic units suffices to yield the full range of macro-level properties. In this way, he maintains, we avoid the undesirable suggestion of the Scholastics or Peripatetics that qualitative variety may be shouldered by the self-same qualities we attribute to micro-level elements. Curiously, he later retreats to corpuscularian accounts of macro-level qualities which feature just such self-same micro-level qualities, when he proposes that there are heat-

corpuscles which are the source of heat in the larger bodies they compose.

As with matter and its elemental particles, the paramount claim Gassendi hopes to establish about the void is that there is one. In fact, following Hero of Alexandria, he holds that there are *three* kinds of void: *vacuum separatum*, or the void of infinite expanse beyond the atmosphere (in which God produces his Creation); *vacuum disseminatum*, or the interparticulate void between constituent atoms of whole bodies; and *vacuum coacervatum* or *grandiusculum*, or the interparticulate voids ‘cobbled’ together by experimental means (and which would otherwise not exist). As we have seen, Gassendi takes the barometric experiments of his day (including his own at Toulon) to demonstrate the existence of at least a partial, disseminated void. In addition to this empirical argument, he also shares ancient atomists’s reasons for accepting the void. Thus, for example, he rehearses the classical arguments that without a disseminated void between the parts of bodies, there is no ready way to explain how bodies may divide and separate down to the level of basic particles, and that without the *vacuum coacervatum*, there is no ready way to explain the motion of bodies through space.⁴⁷

Beyond reviving old views, this theory of the void also lays the groundwork for at least two novel elements of Gassendi’s physics. For one, it is because this *vacuum separatum* is the space in which Creation takes place that it exists independent of whatever matter lies within, and is accordingly non-relative and non-ideal. The motions,

⁴⁷In this last case, Gassendi goes beyond the classical arguments by dismissing the plenist view that mutual displacement of matter also makes motion possible--without empty space--on the grounds that only empty space explains the commencement of motion. For plenists like Descartes to explain how motion begins is particularly difficult given the centrality of circular motion in his scheme.

actions, and changes of matter take place against a backdrop of absolute space--a view which Newton later adopts. More immediately significant for his corpuscularian views, Gassendi presents the void as a feature of the universe with a well-defined structural role though without a distinguishable structure; he supposes the void to be a non-substantial attribute of the universe, leaving matter as the only physical substance. Gassendi notes that critics of atomism think it entails that the void is a substance on the grounds that the void is a sometime component of bodies (along with matter), and these critics worry that making the void a substance is a violation of the law *nihil ex nihilo*. But this criticism is based on a misunderstanding, Gassendi contends: a viable atomism may have it that both atoms and the void are 'principles' (by which Gassendi apparently means 'primary element', following a Scholastic tradition), but it does not follow from this that they are both substantial. Rather, atoms are primary principles of matter, whereas voids are principles of separation in which bodies are located (and by which they are separated and have the sort of supra-atomic structure they have).⁴⁸

2. Matter Can Be Divided into Elemental Parts, Not Infinitely.

To establish that atoms are the primary principles or elements of matter, Gassendi draws deeply on the well of ancient atomist argument. This sort of 'historical evidence' or 'testimony' tolerably counts as one manner of support for physical claims at the dawn of the early modern period, but given Gassendi's pronounced empiricism we should be especially surprised to see his heavy emphasis on this approach. He strikes an even more

⁴⁸B 403.

surprising anti-empiricist chord in the introductory chapter of his *Physics* (*Physicae Prooemium*), by suggesting that, in principle, there is nothing unattractive⁴⁹ about a stipulative physical account of the world which consists of the right physical postulates. Such a physical account would tell us that material objects are composed of ultimate particles:

...These ultimate particles [of matter] can be called Atoms or Indivisibles--not that they are completely derived of parts--but in the sense that there exists no force of Nature that is capable of reducing them. The Atoms are solid corpuscles and they comprise little bulk. When many Atoms adjoin one another, there may form a body which has a bulk, or let us say, a mass of a greater magnitude. The extreme tininess of an Atom is located, in inconceivable proportions, on [the other] side of all sensation....There are no sensible bodies, as little as they may be, that are not composed of a multitude of inconceivable Atoms, etc.⁵⁰

This is an unacceptable approach for Gassendi, but the problem is not that such pronouncements place his physics beyond the reach of empirical demonstration: it's that this sort of stipulative physics would raise more questions than it would answer: "...it would present so many things to insert for explaining the difficulties we would ordinarily meet along the way, that the [stipulated] propositions would be somewhat stifled by the number of things to say."⁵¹ And so Gassendi offers a series of reasoned defenses--largely amplifications, but sometimes simple reiterations, of Epicurean and Lucretian arguments--of such basic atomist tenets as claims as to their existence and status as the primary material principles.

⁴⁹The word he uses is not 'unattractive', but 'incongruous'. Perhaps he means something more specific here, such as 'contradictory' or 'inconsistent'--though with *what* would still not be clear.

⁵⁰Q I 130a-132b.

⁵¹Q I 132b.

Thus, for example, he proposes that, assuming material objects must have *some* substratum or composition from basic and indivisible elements or principles, we can establish that atoms, as the best candidates for the role of substrata, are the material principles in question. To get to this first assumption, he follows Epicurus and Lucretius in adopting the Parmenidean principles that nothing comes from nothing and (conversely) that all matter must come from something, and in holding that this entails a common substratum for all matter given that the composition and resolution of material things always yield matter.⁵² To establish that atoms are the best candidates for serving as this common substratum, he reasons that *whatever* would serve as matter's substratum cannot pass from existence,⁵³ and that elements of such substratum would never pass from existence only if they were indivisible and thus had no void, that is, were solid.⁵⁴ Yet this common substratum cannot be featureless, formless primary matter, as the Aristotelians propose; it should have *some* identifiable and unchanging features because such features are ineliminable from anything that is matter.⁵⁵ And so, he concludes, all material objects must be composed of elemental particles which share those features essential to matter--and those particles just are atoms.

⁵²Q I 232; AN 180. Cf. DL X 40, *De Rerum Natura* I 540-550.

⁵³Q I 232, AN 180.

⁵⁴"Just as every divisible body should be divisible because of an intervening void and parts which dissociate and even allow the ingression of an external power to separate them, what is indivisible should be such that it is entirely full and solid or such that it has no void from which it fears a separation of parts...." AN 180; cf. Q I 258b. The translation is Lynn Sumida Joy's (*Gassendi the Atomist*, 150).

⁵⁵Q I 258b, 259b.

The reasoning is far from watertight. For one thing, to suggest that the material substratum can only resist corruption if it is made of indivisible elemental particles is to presume that the substratum is composed of discrete particles to begin with--which his anti-atomist opponents had no reason to accept. Further, though his atomist story might suffice to explain the common material products of iatrochemical analysis and synthesis, it's clearly not necessary to do so. Even if we take such common material products as evidence of a material substratum (and it is not clear from his reasoning here alone why we should), we need further argument to accept that there must be only one kind of substratum with the same features across particles. Gassendi apparently recognizes as much, for (as we see below) when he asserts what he views as the particular features common to atoms, he bases his claims on the suggestion that he is identifying the properties any bit of matter, however small, must have (of metaphysical, physical and/or theological necessity).

In a different line of reasoning also borrowed from Lucretius, Gassendi tries to establish atoms as the primary principles of matter on the grounds that there must be some fundamental bits of matter which are impenetrable if we are to account for varying degrees of resistance in macro-sized objects. To begin with, he suggests that since all material things resist pressure to some degree, they all have one or another degree of solidity. The only way to explain this range of solidity, or resistance to pressure, he claims, is by supposing that the constituent elements of all bodies are solid and thus not soft. Otherwise, there could be no bodies harder than the softest ones, for if the constituent elements *were* soft, then more solid bodies could never be composed from

them. From this he concludes that all material things must be composed of maximally hard elements which, when put together with more empty space in between them, yield softer bodies and, when put together with less empty space in between them, yield harder bodies.⁵⁶ As in his previous line of reasoning, Gassendi wants to establish that some properties of macro-sized objects (their variable solidity here, their immutably material nature in his first argument) require an underlying atomic architecture. Unfortunately, in this second argument he once more offers an account that is not necessary to explaining the macro-property--for we can still have diverse degrees of resistance to pressure even if the basic elements of matter also range in degrees of solidity. Worse still, his account is not even sufficient. Given that atomic size and the amount of interparticulate void are also variable factors, there could be various combinations of these structural characteristics which yield identical degrees of solidity across the otherwise very different macro-level objects they compose. Then varying atomic architectures is not sufficient for varying the degrees of resistance to pressure in macro-sized objects. Despite these flaws, Gassendi is for his part convinced that we must postulate the existence of atoms to explain the properties of macro-sized objects--here, to account for properties they all share, and henceforth to account as well for the properties which differentiate them.

In each of these arguments, Gassendi relies on the crucial claim that there are natural bodies which cannot be further reduced or divided, and the importance of this claim to his atomism can be seen in his first major atomist pronouncement, De Apparente Magnitudine Solis humilis et sublimis, where he tells us such bodies are

⁵⁶Q I 261a. Cf. De Rerum Natura I 565-570.

...corpuscles of an extreme smallness in which, by natural force, one may finally resolve any composed bodies; in such a way that if we imagine a corpuscle which by whatever natural force can still be taken apart, this is not a true atom, rather atoms are those particles in which [this body] is reduced in such a way that they are in the end themselves incapable of any reduction.⁵⁷

But to establish that these material particles are truly elemental he needs to rule out further reduction and thus to say *why* matter cannot be infinitely divisible. If he fails to do so, it remains a viable corpuscularian option to hold that there is no smallest part of matter—a view which shows up in the non-atomist Stoic and cartesian pictures of matter. And these are our only choices, as Gassendi himself points out: it is either the case that we can't divide atoms at all or else that they are infinitely divisible. After all, if we allow that atoms, though they are physical continua, have *some* discrete parts we open the door to their having infinitely many parts:

In effect, all continua either have actually no parts, or else have infinite parts. This is because, if you call actual parts those which are actually divided, the continuum assuredly does not have even two or three [parts], since [such parts] are [themselves] indivisible. If it [the continuum] actually has two of them because it is actually divisible in two, one must with all necessity say that there are actually infinitely many, since...it is similarly divisible into actually infinitely many parts.⁵⁸

In the first option, Gassendi is apparently talking about non-physical continua such as lines, since whatever 'parts' we might assume these to have, as for example dimensionless points, cannot count as true parts since they are themselves indivisibles. In the second option, he is talking about parts that are divisible and therefore, possibly, of physical continua. But talk about physical continua for Gassendi just is talk about bodies

⁵⁷DA, Q III 466a (letter to Chapelain).

⁵⁸Q I 262b.

which contain no void, or atoms. Hence atoms cannot be physically divided into *any* parts, given that only the presence of void allows such division into parts. In this way he secures the partless nature of atoms *ex hypothesi*, and for the purposes of his physical theory, it seems this should be sufficient--yet he thinks there are additional, well-reasoned, and historically significant arguments for rejecting the infinite divisibility thesis. Thus for example he echoes the Epicurean and Lucretian reasoning that there cannot be bound and finite wholes the parts of which are infinite, for otherwise there should be nothing to prevent the sum of the parts from being greater than the whole (infinitely greater, of course). This alternate situation would produce the paradox, Gassendi notes, that the parts of the world do not outnumber those of a mite.⁵⁹

This sort of paradox aside, the better part of Gassendi's attempt to reject the possibility of matter's infinite divisibility focuses on an additional complication in ancient and early modern debates over that possibility. One view prominent in such debates is that this question is tied to mathematical and geometrical considerations as applied to physical accounts of matter's ultimate or near-ultimate particles. When, as in the passage quoted just above, Gassendi talks about physical and mathematical continua in the same breath, it seems he is happy with such considerations. Yet elsewhere he identifies that application as an unambiguous error which underlies what he takes to be the mistaken notion that physical magnitudes are infinitely divisible.

This disparity in his attitudes is perhaps excusable on the grounds that the application of such considerations may cut both ways. From the earliest Greek views on,

⁵⁹Q I 262a.

puzzles concerning infinite divisibility in the realms of mathematics and geometry gave rise to two conflicting groups of intuitions about the possibility of infinitely dividing matter. On one hand, one pro-atomist perspective has it that paradoxes mounted against the possibility of infinitely dividing numbers or line segments are just as compelling against the possibility of infinitely dividing matter. In a different pro-atomist vein, the stipulation of mathematical points with no parts is plausibly suggestive of the possibility that ultimate physical particles may exist with no parts, also. On the other hand, an anti-atomist perspective has it that, if lines, planes, and three-dimensional abstracta may have infinitely many points, so too may physical objects. For instance, in his Against the Physicists, Sextus Empiricus suggests that, since we can always progressively divide abstract magnitudes (like lines or planes), it seems magnitudes generally speaking are infinitely divisible, whether we are talking about spans of time or space, or extended bodies.

Gassendi argues that Sextus has introduced a straw man. The ancient atomists, he maintains, would not have conceived of physical indivisibles in the same way as mathematical (really, geometric) points, to begin with. Such points are indivisible of course since they have no magnitude--with the result that continua like lines or planes unproblematically contain infinitely many of them.⁶⁰ But as Gassendi suggests in the

⁶⁰In this vein, Gassendi distinguishes between mathematical minima, or points without dimension of which continua contain infinitely many, and natural minima, or three-dimensional entities which constitute bodies that cannot contain infinitely many elemental parts. In his mind, they are related just in the sense that there are an indefinite number of the former in the latter (and this same relation holds between physical and sensible minima, those entities so small as to be at the limits of our perception: "The natural minimum is the Atom itself, in which all resolution meets its end, with which all composition begins, whereas the Sensible minimum [extreme point

Animadversiones, this means substituting physical indivisibles for mathematical ones prevents us from doing justice to geometry, for only the former have magnitude.⁶¹ In general, whenever we employ elements of mathematics or geometry in physical accounts we must take care to distinguish them from their physical *faux amis*:

...It is not always permissible to transfer into physics whatever the geometers demonstrate abstractly. This can be established even from the fact that where the geometers themselves mention those parts of mathematics which have some connection with physics, they are often compelled to require entirely contrary suppositions. For example, Euclid himself in the *Optics* requires that a minimum angle be admitted. Similarly, Witelo accepts a minimum light which, if it is understood as divided, would no longer have the impulse [*actus*] of light. Here it is evident that they assume that in optics division is achieved as far as the minimum, whereas in geometry they would have permitted it to proceed to infinity.⁶²

One problem with recycling *any* of the reasoning strategies of mathematics or geometry in the case of physical objects, Gassendi suggests, is that empirical and non-empirical demonstrations do not face the same constraints: "If the Geometer demonstrates an apparently very easy thing, like the division of a line into two equal parts, do we imagine that this is an equally easy thing to realize and demonstrate in Physics, that is, on the terrain of experience?"⁶³ Specifically, he points out, we don't have

of sensation] is that beyond which nothing falls under the senses, where a mite is of this sort, for example, with respect to sight. It follows from this that the atom is as a midway between the mathematical minimum and the Sensible minimum: in the way that it is legitimate to suppose an indefinite number of points in atoms, it is legitimate to conceive of an indefinite number of atoms in the mite." Q I 160a.

⁶¹AN 415; Q I 263b-264a. In my review of Gassendi's Animadversiones discussion of atomic indivisibility I follow Lynn Sumida Joy's analysis of the relevant passages (Gassendi the Atomist, 157-160).

⁶²AN 416-417; cf. Q I 265b. The translation is Lynn Sumida Joy's (Gassendi the Atomist, 160).

⁶³AN 414.

even precise enough tools to divide any physical object into two exactly equal parts. At first glance, it would seem that Gassendi suffers here by comparison with those of his contemporaries who embrace physical idealizations (or who, as Galileo, employ exemplary physical objects with their irrelevant flaws abstracted away) and so allow the precision in physics that is first found in our accounts of operations like division on abstract objects.

To this sort of charge Gassendi has a ready response. The job of the physicist is to give an account of real objects, not fictive abstractions. Natural philosophy is, in a sense, limited to to what occurs in natural history:

It would seem that the physicist is thought to affirm the existence of a certain natural minimum [since] in the accomplishment of his task nature does not go to infinity. I say that the physicist must dedicate himself to what is perceivable, to what really exists in nature, even to things without making use of so-called abstractions conceived apart from matter.⁶⁴

More specifically, what is wrong with applying mathematical or geometrical abstractions to physics is that we abstract away the 'density and perseverance of matter'. Hence we do not consider dimensions of magnitudes as basic to physical accounts and instead must introduce those dimensions by supplementary postulates.⁶⁵ Such an introduction, Gassendi proposes, is just what those authors who confound physics and geometry do not offer, and so it is quite natural, though ultimately in error, that they should have taken such abstract objects as includes idealized bodies to be infinitely divisible:

⁶⁴Q I 264a.

⁶⁵"...the assumption of the generation of dimensions was held as if only necessary for conceiving of the existence of these dimensions." Q I 264b. The translation is Lynn Sumida Joy's (*Gassendi the Atomist*, 162).

...because as yet there was freedom to imagine anything whatsoever on account of the banishment of matter [the mathematicians and geometers] supposed not therefore that any dimension is composed out of indivisibles, but that each dimension is composed from smaller and smaller parts of its own kind--a body from bodies, a surface from surfaces, and a line from lines--and hence that each one is always divisible into divisibles or, what is the same, into infinity.⁶⁶

Their crucial mistake, in sum, is to consider magnitudes in the abstract instead of as physical features of the real world.⁶⁷ Gassendi allows that, if we avoid this mistake, and stipulate that we are talking about magnitudes which feature those characteristics special to actual (non-idealized) three-dimensional objects, then there is a place for geometrical characterization and reasoning in physics. Indeed, physics *should* account in this way for objects which are divisible into possibly innumerable though certainly not infinitely many parts:

And [let us] affirm, then, that it is possible to transfer to physics the geometric hypotheses, but in a way and an objective such that, although there would be neither indivisibility of dimensions nor an infinity of parts but only an extreme slenderness and the impossibility of enumerating the parts, it would nevertheless be possible in starting from these hypotheses to obtain a greater precision.⁶⁸

⁶⁶Q I 264b. The translation is Lynn Sumida Joy's (*Gassendi the Atomist*, 163).

⁶⁷For Gassendi, this is as drastic a departure from the true ways of scientific pursuit as if we were to take as false those beliefs basic to our picture of nature and to what we take to be true and necessary claims: "If one considers quantity separated from matter, which is not [actually] the case, then one must suppose other things [are not the case] which are indispensable to nature and [which] never cease to be the foundation of all true and necessary conclusions." Q I 265a.

⁶⁸Q I 265b. The sort of precision Gassendi has in mind here seems to be not so much the product of geometry as of arithmetic. In the example he offers, he rehearses the well-worn atomist proposal that we might reliably guess at the number of particles forming the diameter of a body at the lower limit of perception by judging the number of (presumably larger) parts which form the diameter of a distinctly larger body, and then judging the ratios between the two bodies: "Thus Archimedes assumed that the diameter of a poppy seed comprises 10,000 particles not because any art could discern so many parts in such a small thing, but because by applying this reasoning to a greater quantity [it seems Gassendi means 'magnitude'], the proposition would have followed more closely the less he erred through the neglect of this sort of small particle." AN 417-418; cf. Q I 265b. The translation is Lynn Sumida Joy's (*Gassendi the Atomist*, 161).

One further difference between reasoning about mathematical or geometrical abstracta and reasoning about real material objects is that only in the latter case could the limits on what is physically divisible be relevant to the limits on what is divisible in principle. In his earlier De apparente magnitudine, Gassendi calls this to our attention when he proposes that there may be bodies which, even if they have parts, are physically indivisible anyway--in contrast with mathematical points, about which we may not concede that they have parts at all:

That corpuscles of this kind exist, Democritus and Epicurus prove by this fact that if the mathematicians suppose some bodies that are divisible until infinity, they nevertheless do this by starting from hypotheses about things stripped of real existence, like a point without parts, a line without thickness, etc....But nature, in dividing or taking apart a body into particles of which it is constituted, only goes until a determinate degree, and never breaks up infinitely or indefinitely. From which it is clear that we speak of atoms not, as it is popularly believed, because these are mathematical points that are indivisible by the absence of parts, but because, even though these are real small bodies, there is no natural force at all by which they may be cut or taken apart.⁶⁹

If it is not physically possible to divide matter past a certain limit, then bodies of sufficiently small size have no physically isolable parts. Puzzles over infinite divisibility of abstracta could not be relevant to the question of infinitely dividing matter, then, because the presence or absence or limits on conceptual division of matter have no bearing on the limits of their physical division.

At the heart of Gassendi's worries here is the concern that we take loose affinities between our accounts of physical and abstract objects as the basis for treating such objects of say, physics and geometry, as if they share the right sorts of features that would

⁶⁹DA, O III 466a (letter to Chapelain).

warrant our talking about either with equanimity. That we ought not presume such analogous features is something of a constant among Gassendi's views. More than a decade before the Animadversiones, he dismisses Poysson's contentious proposal that we might move seamlessly from discussing mathematical points to discussing the smallest extended entities as though they were relevantly similar or even plausibly identical.⁷⁰ Even in the context of that earlier controversy Gassendi distinguishes what are mere abstract posits with no magnitude from what he takes to be extended bodies endowed with magnitude. In the later Animadversiones and Physics, then, it appears as a concomitant point that an infinite divisibility thesis is indefensible if it relies on this sort of failed analogy between physical and abstract objects.⁷¹

⁷⁰Poysson's query, as to whether we might better understand the nature of an extended thing having parts by imagining that we can assign magnitude to ostensibly partless mathematical points, quickly made the rounds of Gassendi's intellectual milieu, the circle of Marin Mersenne's correspondents. Mersenne forwarded the 'Poysson problem' to Peiresc, in a letter of October, 1635:

One question. Is there a demonstration perfect from a logical viewpoint, a mathematical viewpoint, and a viewpoint of knowledge from the senses, proving that there exists a given magnitude with extension, and which in a given time and place can be found in a truly mathematical point, in a point stripped of parts such that this magnitude would have in this point exterior parts? (TdL XIX 149)

Fast on the heels of receiving word of the problem (via Peiresc), Gassendi offers this dim assessment:

...I do not see at all that if many mathematical lines which are only in the imagination may meet one another in a mathematical point which is also only a supposition of mathematicians, at the same time many physical, sensible, and bodily lines can be found in a point which is mathematical and not physical, sensible, or bodily (consequently always having some size, however imperceptible to the sense). And, certainly, I am a little surprised that this serious man [Poysson] who suggested previously that light is a corporeal substance, afterwards would have it that many rays--that is, many bodies--meet each other in the same mathematical point, that is, penetrate one another and be in the same place--which is not possible by nature. (Fonds Dupuy 688 folio 90, letter of November 30, 1635)

⁷¹Gassendi makes this point in the Animadversiones as follows:

Surprisingly, there were not only some from ancient times who attacked Epicurus as if he

A further, different sort of argument for the infinite divisibility thesis is based instead on the claim that an essential feature of matter is its being extended. This argument goes: as long as something is extended we should be able to conceive of its parts thus it is inconceivable that anything, however small, would be extended though we could not continue to divide it into smaller and smaller parts. The parts of macro-sized objects are divisible, so why shouldn't the parts of micro-sized objects be divisible, too? In response Gassendi suggests that divisibility surely cannot be a feature of bodies we stipulate as not having divisible parts to begin with. Once again, the infinite divisibility thesis fails by the constraints of a physical assumption, namely, that even if we talk about atoms as having parts, then unlike the separable parts of macro-sized objects, whatever parts atoms may have are really non-separable elements of a physically continuous entity.⁷²

had held that the division of a magnitude is terminated in certain mathematical points; but there were also learned men from more recent times who inveighed against him with entire volumes as if he had said that bodies were constituted from surfaces, surfaces from lines, lines from points, and accordingly bodies and indeed all things from points, into which bodies and all things were in the same manner resolved. It was amazing, I say, since if they had been willing to attend in the least, they could have noted that those indivisibles, in which Epicurus held divisions to be terminated, are not mathematical points but the smallest bodies. For he made magnitude, none of which is admitted in a point, a property of them and, what is more, he made an endlessly variable figure, such as cannot be conceived in a point lacking magnitude and parts, a property of them. (AN 414; cf. Q I 263b-265a) The translation is Lynn Sumida Joy's (*Gassendi the Atomist*, 156).

⁷²In the *Animadversiones*, Gassendi embraces this view as a matter of following his ancient predecessors:

...[Epicurus] introduces a distinction between the parts that constitute a sensible body and the parts of which the atom is the set; this is because, having been separate, the former at a given moment are found conjoined, [and] as well can be separated after having been conjoined; whereas the latter, having always been conjoined, have never formed a union from separation, and could no more be resolved in separate parts. (AN 419; cf. also Q I 258-259)

In particular, he holds that we may conceive of two kinds of inseparable parts or aspects of atoms, which he calls 'atomic' and 'measurable' minimae, respectively. This first kind encompasses all physically inseparable aspects of atoms, and the second kind includes the smallest conceivable parts for which we can speak of any measure in those atoms with determinate shapes.⁷³ This second kind may be more closely defined as follows: for all atoms we stipulate as having particular edges and/or protrusions, we can say what their angles would measure (for example, given the right instruments)--though we cannot, of course, measure the actual micro-sized angles. Hence these measurable minima are "such that we conceive the extremity of an angle in a pointed atom."⁷⁴ For atomic minima, there is an analogous story relative to all parts or aspects of atoms: we should be able to conceive of atomic parts, like a top or bottom half, without requiring that they are actually separable or even that we may conceive of their separability.

Of course, if we *could* conceive of such parts as separated, then God could have created atoms that much smaller--in which case it turns out to be merely contingent that those particles to which our theory commits us have no separable parts.⁷⁵ That God could have created smaller atoms raises the possibility that the particular size of atoms as we think of them doesn't really matter as long as they have no separable parts. That atomists place a higher premium on the inseparability of parts than on the size of atoms is an old

⁷³Q I 268. There are, in addition, sensible minimae--the smallest objects or parts we can sense.

⁷⁴AN 45; cf. Rochot, Les travaux de Gassendi, 174.

⁷⁵This is not the incoherent suggestion that it is possible that what the smallest particles *could have been* could have had separable parts (in that case they wouldn't actually be the smallest particles), but that what the smallest particles *actually are* could have had separable parts.

and sore point. As one ancient argument goes, if the absence of internal void blocks the separability of parts so that atoms as individual plenums have inseparable parts, then we could also have absurdly large atoms as long as they are bodies with no internal voids. We would only have to insure that everything else in the universe could still be generated from such atoms. Unhappily, Gassendi's attempt to meet this difficulty consists of his rather arbitrary insistence that even the smallest familiar macro-sized objects dissolve into (finitely many) parts, so it must be the case that all God and nature have with which to generate the world's multifarious legions is atoms of one small size. Speaking once again of the mite (*acari*), he notes:

We imagine all these parts without the whole with which nothing that nourishes itself, lives, senses, imagines, or moves, can subsist; and we understand that nature must be able to distinguish, put in order, and organize into a whole, innumerable tens of thousands of particles, to form a small organism which, to the naked eye, is like a point. Nevertheless, since nature cannot go to infinity in its dissolution, but stops finally at something non-decomposable which is the [physical] minimum, this is what philosophers have generally called the Atom. In this sense, it is convenient to call as atoms those extremely small and non-decomposable particles of which we can conceive that many tens of thousands exist in one mite. If nevertheless we admit that fine membranes are formed from one sole layer of atoms, what a large number, or rather what an innumerable quantity must be brought together before we arrive at the thickness of a mite or of a spider's web.⁷⁶

3. The Contours of Gassendi's Physical Atomism

One consequence of the tininess which Gassendi attributes to atoms is that it should seem a quixotic enterprise for him to seek empirical foundations for his atomism, for in his times there is not the barest hint of perceptually-derived knowledge of such tiny

⁷⁶DA, O III 426a (letter to Liceti).

particles. Although he thinks there is an empirically viable source of at least some of our beliefs about atoms (as I indicate below, this is the indirect data of indicative signs), the general tenor of his characterization and defense of physical atomism signals a departure from his customary empiricism. As is the case with his defense of an indivisibility thesis, Gassendi relies first and foremost on reason, not experience, to account for the origin and quantity of atoms and what he takes to be their essential and inessential properties, their internal impetus, motion, and causal role, and their contribution to the motions and qualities we attribute to macrophysical objects. He is hardly to be faulted in this regard, for not only is direct empirical evidence unavailable, he is also responding in kind to those of atomism's critics for whom the use of reasoned argument devoid of empirical force is common strategy (among these critics he is primarily concerned with Aristotle, the Stoics, Descartes, and the early Christian writer Lactantius [250-317 CE]).⁷⁷

a. The Origin and Quantity of Atoms

One of the more lucid aspects of Gassendi's corpuscularian picture is his account of the origin of atoms. In response to Lactantius's worry that the ancients could not give any such account because they believe atoms to be eternal (On the Anger of God §X) Gassendi answers that we may simply stipulate that atoms are created by God.⁷⁸ It is possible, he admits, that God could have made macro-sized objects without making their

⁷⁷See for example Lactantius (1964) or (1965).

⁷⁸One incidental result of this stipulation is the following theological argument against the infinite divisibility thesis: if bodies were infinitely divisible, then one corpuscle could beget infinitely many newly created corpuscles; but given that there are only as many corpuscles as God creates they cannot be infinitely divisible.

constituent atoms. But since our physics tells us such objects can always be resolved into those constituents, to have created them as composites amounts to the same thing as having created the atoms at that point, too.⁷⁹ Hence we may as well incorporate the origin of atoms as a minor adumbration of the Creation story. Though this account will not satisfy the ancient atomists, Gassendi points out that they already have a viable non-Christian story to fall back on. If atoms are the primary stuff of matter, they could not have been created from anything else. Yet if they could not have been created from anything and by the Parmenidean dictum they were not created from nothing, then it is not only plausible but also necessary that they exist from the beginning of time lest they not exist at all.⁸⁰ One intriguing aspect of this last response is that while Gassendi clearly wants a theologically palatable atomism, he also tries to answer critics of atomism in terms with which the ancients would have felt comfortable. This is further evidence that he is interested primarily in a sustainable atomism whatever our background nonscientific views, and only secondarily in developing an atomism acceptable to Christians.

An issue which is a bit less clear is how many atoms God created in order to form the universe. Just as matter cannot have infinitely many parts there cannot be infinitely many atoms--for at least one common reason, namely that God's creation (unlike God) must be finite.⁸¹ And whereas Gassendi recognizes the folly of trying to come up with

⁷⁹B 400.

⁸⁰B 404-405.

⁸¹Q I 273. The argument goes: finitude is a privation, and God is perfect hence suffers no privations, whereas all of Creation by contrast suffers privations. Hence God is infinite but Creation finite. There are several dissenting moves possible. For one, it might be thought that Creation, being a Divine product, can no more suffer privations than the Divine Creator (though

any specific numbers, he insists there are no obstacles to God creating sufficiently many to constitute the components of all extant matter. In particular, *contra* Lactantius, it is neither the case that any knowable number of atoms cannot possibly suffice to constitute all the matter of the universe, nor that it is unintelligible to suggest that tiny atoms could constitute 'immeasurable masses'. Regarding this last point Gassendi proposes that we don't have any difficulty abstractly dividing enormous masses into their constituent parts (where the smallest, according to this theory, are atoms) so we should have no more difficulty adding up atoms to produce those same masses. Regarding the first point, he proposes that in principle there is no problem in expressing the number of atoms sufficient for constructing the whole universe. To see this, we simply take some estimate of the number of atoms which should be present in small particles--this is presumably a knowable number--and multiply such numbers by some large number we think represents the great quantity of such particles or larger objects. This, too, will be an expressible number. If we think the number of small particles is higher, then we adjust the second figure higher, but the product will still be expressible, and so on for any second figure we pick. Hence (by an unstated application of mathematical induction) the number will be expressible no matter how many supra-atomic sized bodies we think there are in the universe.⁸²

this move makes a mystery out of imperfections generally). For another, it might be held that finitude is not a privation at all but simply a contingent state of affairs which does not reflect a loss or lack of anything. Then the quantity of matter, atoms, or other parts of matter is finite merely as an accidental feature of the world.

⁸²B 406-407.

b. Properties of Atoms and their Aggregates, and the Role of Atoms in Macro-Physical Properties

Enumerating atomic properties is a far simpler affair, at least at first glance.

Gassendi distinguishes between two sorts of atomic properties: those which are inherent in and essential to all individual atoms, and those which are a feature of atoms in groups. To begin with, let's consider his list of inherent atomic features, which follows Epicurus's list closely: extension, size (*moles*), shape (*figura*), weight or mass (*pondus*), and solidity (*soliditas*). Generally, relative to each of these features all atoms resemble one another; for example, they have a common size or weight. One exception is shape: in order to account for tremendous variety among natural objects, Gassendi claims, there must be many (though not infinitely many) different types of atomic shapes, and many (again, not infinitely many) tokens of each type.⁸³

Another notable feature of this list of properties is that it signals Gassendi's rejection of the cartesian view that extension is sufficient to characterize what is essential to the least bit of matter. Descartes's view is wrong, Gassendi argues, for the same reason that the Scholastics are wrong to talk about featureless matter in the context of physical theory.⁸⁴ While we might conceive of matter with one feature such as extension (or none

⁸³Gassendi suggests we can expect the variety of shapes in atoms that we find in shapes of macro-sized objects like leaves or grains of wheat. Of course, if this analogy was thoroughgoing it's hard to see why we shouldn't also expect a similar variety in atomic sizes or yet other properties. *Q* I 270.

⁸⁴Descartes's views are famously more traditional than he pretends, and here it may be that his ascription of a single essential characteristic to matter—pure extension without any other determinate features—is something of a variation (drastic, to be sure) on the Aristotelian and Scholastic theme that everything is composed from featureless matter to which the introduction of form brings all other determinate characteristics.

at all, as per the Scholastics), it cannot actually come into existence without the features which God assigns at Creation, namely, size, shape, weight, and solidity.

A further difficulty with considering extension as the sole essential feature of matter or its constituent elements is that it does not, as Gassendi notes, distinguish matter from void. Minimally, he can use solidity to distinguish the two, given that he just understands 'having solidity' as the same thing as 'containing no void'. But stipulating that atoms must be solid is also felicitous for Gassendi because it helps him establish that atoms have resistance and impenetrability (*antitypia*). In short, whatever travels through pure void meets no resistance, so a lack of void accounts for their resistance; and interparticulate void is what permits penetration of one body by another, so a lack of (this kind of) void renders a given body impenetrable. Moreover, that atoms are solid suggests that they are indestructible, which is necessary for them to persist as the material substrata; and that they are indivisible, which is required of them as the ultimate material elements.⁸⁵

Yet the essential feature of atoms which does the most work in Gassendi's physics--and also generates the most difficulties--is their inherent weight, which gives them an intrinsic, natural *tendency* to move such that their rest is either provisional or else an illusion. I discuss atomic motion and causation at length in the following section, but here I mention briefly a few primary aspects of Gassendi's view. For one, the weight of atoms lends those capacities necessary for "...moving, ...imparting motion to others,...[and] rolling about," which in turn endows atoms with the robust set of further

⁸⁵Q I 266f.

capacities to "...disentangle themselves, to free themselves, to leap away [*prosiliendi*, to spring out], to knock against other atoms, to turn them away [*retundendi*, to check], to move away from them, and similarly [they have] the capacity to take hold of each other, to attach themselves to each other, to join together, to bind each other fast..."⁸⁶ For another, however, it must be the general tendency of atoms to move in straight lines given that, unlike what the Epicurean tradition says (though perhaps as Epicurus himself would say), the atoms here do not feature the *clinamen*, or swerve. Finally, there is at least a hint of vitalism or animism in Gassendi's proposal that God endows atoms with a robust set of capacities for moving themselves.⁸⁷ Even in cursory review, these various tantalizing claims suggest significant and controversial consequences of stipulating that atoms have inherent weight and resulting tendencies to motion; I discuss some of these consequences in section (c) below.

First, though, to round out this overview of the relations of physical properties and atoms, there are two other sets of properties which require explication: those properties

⁸⁶Q I 280b; B 400-401. While his term here is *vis*, he is clearly talking about a 'capacity' or 'power' which enables atoms to move in these various ways, and not about 'force' *per se*. This of course does not prevent him from subsequently referring to the very same *vis* in terms of one or another concept of force. As Westfall notes, it is rather common for physical theorists of the period to use the same term in different places to pick out a variety of meanings, and this is markedly the case with *vis* in much early modern physics; see Richard S. Westfall, *Force in Newton's Physics* (London: Macdonald; New York: American Elsevier, 1971). I briefly discuss Gassendi's views on atomic *vis* below.

⁸⁷There is also at least a hint of teleology or ends-driven design in the proposal that God endows atoms with these capacities for motion at Creation so that they may have what they need to move in ways required for fulfilling their God-given purpose. His talk about purpose is particularly conspicuous in the case of those atoms God designates as elements of the 'seeds' of organisms: "...from selected atoms he fashioned the first seeds of all things, from which later the propagation of species would occur by generation." B 401.

characteristic of atoms in groups, and the properties of macro-sized bodies which only exist in virtue of the properties and relations of their constituent atoms. Regarding this first set, Gassendi follows Lucretius in holding that such properties include the position of a group of atoms relative to neighboring spatial regions (*situs*), and the arrangement of a group's members relative to each other (*ordo*). In addition, Gassendi proposes that many atomic groupings constitute specific sorts of molecules (*moleculae*) or concretions (*concretiunculae*) which feature special aggregate properties that in turn underlie many of the familiar qualities or properties of macro-sized objects. Like atoms, these molecular structures serve as elemental building blocks of macro-sized objects, though on a larger scale and in a more dedicated fashion. Gassendi analogizes the different levels of material elements to the elements of language:

As letters are the elements of writing and from letters are formed first syllables, and then successively words, phrases, and speeches, so also atoms are the elements of all things. From the atoms the smallest molecules are joined together first, and then successively somewhat bigger ones, still bigger ones, the finest and the coarsest bodies, and finally the biggest bodies.⁸⁸

What it means for molecules to be *dedicated* elements of matter is that they have specific capacities for forming one or another types of macro-structures.⁸⁹ Thus, whereas the classical Epicurean view suggests any macro-sized object may be formed from any elemental bodies (namely, atoms), Gassendi holds that particular macro-structures often come into being only because the right sorts of molecules come together. Of course, this is an interesting structural requirement only if it is not the case that any molecules can be

⁸⁸AN I 23.

⁸⁹Q I 281b-282a.

formed from and resolved into any atoms, and Gassendi indeed insists not all molecular combinations are possible. Rather, molecules are specially brought together by the complimentary motions of and connections between such component atoms as stand in the right relations and feature the right protrusions.⁹⁰ Atoms may be held together by hook-and-eye protrusions, or by pressure of one set of atoms against another, or else by an absence of those peculiarly round and smooth sort of atoms which are noncohesive.⁹¹ Further, molecules once formed are not easily resolved into their constituent parts, though they are divisible in principle,⁹² which suggests that nature fashions many particular sorts of macro-sized objects not by the chance interactions of randomly available atoms but by the interactions of more complex structural elements which are generally already in place. This eliminates or at least minimizes the problem, posed by Lactantius and others, that forming standard sorts of highly organized macro-sized objects from their more basic

⁹⁰Q I 475a. A small point about the history of chemical thought is relevant here. Henk Kubbinga, following Bloch, suggests that Gassendi contributes to an early seventeenth century trend--also found in Jungius, Beeckman, and Basso--of promoting a corpuscularianism in which a modern concept of the molecule is a central and novel feature; see Henk H. Kubbinga, "La théorie moléculaire chez Gassendi," in *Société Scientifique* 1994: 283-302, and Bloch, La philosophie de Gassendi. While it is certainly a modern notion that special physical and chemical properties emerge in such atomic aggregates, the purely mechanical nature of these molecular bonds (that is, that they are realized entirely by contact action) militates against Kubbinga's suggestion. The familiar modern molecular concept, after all, has it that non-mechanical forces bind atoms together. Moreover, Gassendi's notion that all forces are mechanical at root precludes any attempt to construe him as holding the modern view.

⁹¹According to Gassendi, that some atoms *are* rounder and smoother than others helps explain variations in the rates of such physical phenomena as evaporation and melting across types of substances (for example, he suggests that water vaporizes more quickly than oil because the constituent atoms of water are rounder and/or smoother than those of oil). B 405-406.

⁹²Q I 260a.

atomic components is highly improbable.⁹³ Thus, for example, Gassendi suggests (following Lucretius) that structures of living organisms are composed of 'seeds' of life, or animate sorts of atomic aggregates:

We cannot say absolutely that sensible beings are made from insensible elements, but rather that they are made from things that, *though they in effect do not [themselves] sense nevertheless contain the sensory principle*, just as the principles of fire are contained and hidden in the veins of stone.⁹⁴

A further example of such specialization on the level of atomic aggregates is the distinctive crystal form of those molecules which serve as the structural bases of a diverse group of macro-sized stuffs including sugar, salts, and alum.⁹⁵ Gassendi proposes that this sort of structurally basic molecule appears only when the right kinds of individual atoms aggregate in such a way as to yield those crystalline features, for (apparently) no single atoms have the requisite structure.

The emergence of special properties like crystalline form when two or more atoms come together as molecular units is but one subset of the phenomena where supra-atomic properties emerge. Another, more familiar subset is the emergence of the particular qualities of macro-sized objects, which Gassendi explains by appealing to the dispositions and states of their constituent atoms or atomic aggregates. He outlines four broad sorts of atomist accounts of the qualities of macro-sized objects, each corresponding to a

⁹³B 407-408.

⁹⁴Q II 349 (Gassendi's italics). One upshot of this view is the strikingly prescient notion that there are organic and inorganic molecules. The distinction for Gassendi, though, is not hard and fast but along a continuum, for he shares the view, common for his times, that minerals also have some degree of animation.

⁹⁵Q I 271a. I discuss this case at greater length below.

particular sort of macro-level quality (what Gassendi sometimes refers to as 'modes of substances') he distinguishes. To begin with, there is the sort of quality Gassendi considers basic to all material objects (that is, all macro-sized objects have one or another such qualities), and these are produced directly by the inherent and defining properties of select individual corpuscles. These qualities include what the iatrochemistry of his day calls the elemental principles: heat, cold, dryness and wetness. Accordingly, it is the calorific, frigorific, dry, and wet atoms--distinguished by their shapes and sizes (for example, calorific atoms are small, round, and move rapidly; frigorific atoms are pyramidal, sharp-pointed, and move slowly)--that yield the familiar corresponding macro-phenomena when the right number of such atoms come together. Fire, for example, occurs when sufficiently many calorific atoms are brought together.⁹⁶ A second sort of quality, which includes rarity, density, firmness or cohesion, and fluidity, is produced by the specific states, combinations, or interactions of multiple atoms. For example, whether in air, flame, or liquid, fluidity on the macro-level results when atoms are aggregated loosely with few points of connection. Firmness or cohesion on the other hand results when atoms are aggregated tightly with many points of connection, including connection by the aforementioned hook-like protrusions.⁹⁷ Still other qualities are also defined by the

⁹⁶Yet the presence of heat atoms is neither a necessary nor a sufficient condition for heat--such atoms are also present in the pores of bodies that are not themselves hot, and heat may also be generated by friction or putrefaction (*Q* I 394f; and following Epicurus, cf. *SEP* 23). Curiously, he does not see that this makes special calorific qualities of atoms superfluous.

⁹⁷*Q* I 402-405; cf. *Bernier* III 135-140. Dijksterhuis points out that in seeking this kind of corpuscular account of cohesion Gassendi is practically alone in this period (*Mechanization*, 428). Thus, for Descartes cohesion is a result of the principle of inertia plus the relative rest of parts of space and matter--so that a body remains cohesive to the degree that there is no impact with other bodies--and for Galileo (following one Scholastic view) cohesion results from resistance to the

relations among atoms, but are further determined by the context of the given body containing them; these include color, odor, taste, softness, hardness, flexibility, tractibility, and ductility.⁹⁸ Finally, there are the capacities of macro-sized bodies which somehow arise from the form or the whole of a body,⁹⁹ which Gassendi construes as qualities produced by the spatial arrangements characteristic of the union of their distinct corpuscles.¹⁰⁰ These are primarily capacities for special influences or powers (including the attractive powers of gravity and magnetism) and capacities for susceptibility to the same. Insofar as these capacities yield particular consequences not manifest to the senses, Gassendi refers to them as 'occult' qualities. The 'occult' qualities which particularly interest him are those which, relative to particular organic contexts, are curative or harmful—including those qualities which enable plants to serve as purgatives, venoms, or antidotes.¹⁰¹ In sum, then, there is an atomist account of how macro-sized objects have their properties for all such properties as are directly produced by special properties of individual atoms, are produced by atoms in combinations, are dependent on the context of

vacuum.

⁹⁸For color, see Q I 432-441; for odor and taste, see Q I 409-415; and for softness, hardness, flexibility, tractibility, and ductility, see Q I 405-409.

⁹⁹The distinction between these last two sorts of qualities seems loosely analogous to the modern distinction between tertiary and quaternary structures of proteins, where the former are three-dimensional configurations (as in folds of β chains of hemoglobin), and the latter are spatial arrangements formed by aggregates of distinguishable units (as in four such β chains joined to form an oligomeric protein).

¹⁰⁰A thorough list of those properties for which Gassendi thinks we should be able to devise atomist causal accounts also includes: transparency and opacity, coarseness and fineness, smoothness and roughness, elasticity, malleability, fragility, fissility, evaporation and condensation, liquefaction and solidification, sound and light.

¹⁰¹Q II 160-168; cf. Bernier V 4 237-239.

a given containing body, or arise from the relations of atoms in the structure of a particular sort of body.

All these accounts rely on that commonplace of seventeenth century metaphysics, a distinction between primary and secondary qualities. The standard form of this distinction--as fashioned by ancient atomists and rediscovered by the early moderns, Gassendi included--suggests that the qualities of macro-sized objects we associate with our qualitative perceptions are actually unreal, ephemeral products of the real and lasting qualities found in matter's basic elements. It is of course merely accidental that some particular qualities are perceivable, and so it is a natural extension of the standard primary/secondary distinction to describe *all* qualities of macro-sized objects, perceivable or not, as the products of qualities marking corpuscles and corpuscular structures. Thus not only do we perceive a ball as red because of the right sorts of interaction between atoms of light, the ball, and our optical apparatus; it is also so that we take the ball to be fragile or flame-retardant because we think the right sorts of atomic phenomena obtain--without the benefit of being able to perceive those qualities. Moreover, it is also a somewhat natural extension of this global distinction, at least on general nominalist grounds, to suggest that *all* the macro-phenomenal qualities are unreal and if any qualities are real, only the corpuscular ones are. Indeed, given Gassendi's equitable treatment of perceptible and imperceptible qualities, it seems he needs such a global distinction to generate the foregoing sorts of causal accounts and avoid runaway inflation of real qualities or properties.

In the context of this global distinction, then, the perceptible or 'secondary'

qualities constitute a special case, where one or another story is told about how they are produced by the interaction of our sensory organs with 'primary' qualities of external bits of matter. For example, Descartes holds that such secondary qualities simply emerge from the mechanical motions of those corpuscles said to bear the properties in question; this is particularly clear in his accounts of light--which he claims we perceive as a consequence of pressure on our eyes--and color perception. Gassendi, for his part, adapts the Epicurean and medieval traditions of species theories. He proposes that our visual perceptions of secondary qualities result from perceptions of a sort of *material* species, that is, aggregates of light corpuscles which somehow represent the qualities in question and travel in streams emitted by the macro-sized bodies to which we precritically ascribe such qualities. Thus, for example, as I note in chapter one, Gassendi holds that this sort of species-transmission must occur in order that we perceive such visual elements of an object as its colors. As per the standard primary/secondary distinction, what we perceive as qualities of mid-sized objects arise from (qualities we associate with) the configurations of their parts, and ultimately from the atoms constituting those parts. Gassendi simply requires in addition that some representation of or information about those configurations is transmitted from the objects to our sensory organs. Hence, for example, while particular colors arise from distinct atomic configurations and positions, and we can change the colors of a thing by altering those atomic qualities (as through chemical reactions), we only *perceive* the colors of a given object if we perceive the

species that object transmits with just such information.¹⁰²

There are, famously, a series of knotty problems with the primary/ secondary qualities distinction, and Gassendi's version does not escape these. To take one example, the sort of explanatory account he bases on this distinction fails to say why we perceive secondary qualities of macro-sized objects, and not the primary qualities of atoms. Gassendi recognizes this shortcoming, and suggests, quite reasonably, that we don't perceive the primary qualities because the atoms which bear them are subperceptible given our particular sensory apparati. But as Dijksterhuis, for one, protests, that does not tell us *how* the mechanical interactions of these subperceptibles with our sensory apparati produce our particular sensations of macro-sized objects. Yet, as I suggest in chapter one, Gassendi indeed offers a mechanical account of visual sensation and also attempts to explain how other forms of perception work. In the end, though, I agree that his account is lacking in the sense that it is not empirically satisfying, for he builds this account largely on guesswork about the functional morphology of the relevant apparatus and the corpuscular structures underlying the corresponding secondary qualities.

Another sort of difficulty is that, while Gassendi's thoroughgoing atomism suggests all macro-phenomenal qualities arise from the more fundamental qualities of atoms, he does not even attempt to explain how we may ascribe such qualities as we do

¹⁰²Q I 367. This is, in some respect, a small addition yet there is arguably nothing simple about it: what exactly is a 'representation of atomic qualities supposed to look like? in what form does 'information about' atomic qualities come? if light is corpuscular as Gassendi proposes and we perceive it (for example) whenever it bounces off other objects and hits our eyes (see chapter one), then how do we perceive light's qualities (e.g. intensity, duration)? Surely not through *additional* species bearing further information.

not and cannot perceive. The difference is that, in the case of perceptible qualities such as color, the perceptual vehicle of the material species transmits representational content, whereas in the case of non-perceivable qualities such as cohesiveness, there is no such phenomenon. Hence it does not make sense to attribute our awareness of say, the cohesiveness of a body to a representational species the body emits. By default it must be that in ascribing such dispositional qualities (and any others which lack representational content) we rely on inferences from whatever ancillary perceptual information is available to us. Gassendi is silent on this matter so it is impossible to say how happy he would be with this approach (much less how he would develop it).

Beyond the problems associated with a primary/secondary qualities distinction, another difficulty with any such atomist accounts is justifying our characterizations of the more fundamental qualities we ascribe to atoms, given that any phenomena below the level of perceptibility is perforce unfamiliar. To arrive at such characterizations, we extrapolate from experience with familiar phenomena of macro-sized objects and posit that something relevantly similar occurs on the microphysical level. However, there is no *prima facie* reason to accept such posits save that they are explanatorily felicitous--and this, too, is not a sure bet. Dijksterhuis, for one, suggests Gassendi's atomist accounts of macro-phenomenal properties are not so much explanations at all as they are elucidations. His complaint seems to be that, whether we explain macro-phenomenal properties by assigning the same properties to atoms or groups of atoms or else suggest such properties arise from atomic motions and interactions, in any case we don't learn *how* or *why* it is that those micro-properties or phenomena should give rise to the phenomena we associate

with macro-sized bodies.¹⁰³ There is, of course, very little in Gassendi's account (or the accounts of other, more experiment-oriented seventeenth century corpuscularians, including L  mery and Boyle) which could be said to satisfy the need for such robust physical explanation. Yet it seems there is some sort of explanatory content here insofar as we stress the chemically significant suggestion of the account, that it is the actions and combinations of a standard set of micro-structures with a standard set of properties which underlie macro-phenomenal qualities. Gassendi's own chemical observations, we will see below, are quite rudimentary but the intent of his programme is clear: what changes we can effect on the micro-level of matter should make consistent differences on macro-level phenomena. If we take such a method of difference to yield viable explanation, then his account (like other such accounts based in part or whole on a primary/secondary qualities distinction) apparently provides more than simple elucidation. It remains a further question, of course, as to whether viable explanation alone warrants our merely extrapolating from familiar phenomena to begin with.

Whether or not these are viable or valuable sorts of explanation, one final problem is that these seem to be no more than qualitative microphysical accounts. That is a disappointing result in the heady days of seeking to mathematize the sciences--a central project for most of Gassendi's contemporaries. But it was not clearly a project in which Gassendi himself was interested, and even if he wanted a quantitative account, it is not evident that he could have one. Dijksterhuis, for example, argues that he could not formulate one because no such account could range over the vast variety of corpuscular

¹⁰³Dijksterhuis, 430.

shapes.¹⁰⁴ Such an account is not impossible in principle, though, at least for this reason. It would simply have to feature the sort of internal arch-heterogeneity we find in say, biomechanics, rather than featuring the homogeneity we have come to expect from the most general physical mechanics. Yet Gassendi offers his atomism as the basis of an internally homogeneous physics, and so the heterogeneity which marks his account of the micro-world is, after all, an untidy, multifarious basis for constructing objects of the macro-world--though this does not give us Dijksterhuis's claim, that it is as well so excessively unwieldy a picture as to not possibly bear quantification. More significantly, in his ambition to develop an integrated account of all motion Gassendi attempts (in a different way) to make his physics a homogeneous affair yet falls short, given what amounts to a perplexing set of views about the motion of atoms and their aggregates, and this is the topic I address next.

c. The Motion of Atoms, their Causal Role, and their Internal Impetus

The motion of atoms, for Gassendi, is internally and efficiently caused by an impetus or inclination to mobility.¹⁰⁵ Thus there is no direct immaterial cause of motion

¹⁰⁴Dijksterhuis suggests this feature alone of his corpuscularian hypothesis rules out the possibility that Gassendi could have subjected it to crucial experiment ('verification', in Dijksterhuis's view); it's not entirely clear, though, why the one is supposed to entail the other.

¹⁰⁵What is motion for Gassendi? He follows Epicurus in suggesting that motion is the passage of a body from one place to another. One reason for adopting the Epicurean account, Gassendi suggests (*Q* I 338), is the promise of a simple and intuitive alternative to Aristotle's murky definition, according to which motion is "the actuality of the potentially existing qua existing potentially" (*Physics* *Γ* 1, 201a11-12). Yet the Epicurean account is not without limitations, for it binds one's physics to talk about local motion alone. Gassendi, for his part, does not think this is a problem, almost certainly because (as we see below) he thinks that by drawing on the right kind of corpuscular story we can account for *all* motion as local.

in atoms; rather, when God imputes to atoms such an inclination along with all their other fundamental qualities, his action is an *indirect* immaterial cause of everyday instances of atomic motion.¹⁰⁶ Sometimes Gassendi refers to this inclination of atoms to motion as a force and other times he refers to it as weight (*pondus*) or heaviness (*gravitas*). This weight is not only, or perhaps not at all, one of the measurable characteristics of atoms (as size or shape)¹⁰⁷; rather it is a kind of internal impulse,

...a natural, internal faculty or 'force' [*vis*] by which an atom can move of its own self; or, rather, it is an in-born [*ingenita*], innate, native, and ineliminable [*inamissibilis*] inclination to motion, a propulsion and impetus from within.¹⁰⁸

By proposing this 'internal faculty', Gassendi locates the source of all causal agency among material objects in the atoms which constitute them, and it is evident from his extensive reviews of and responses to theories of natural causation from Epicurus to Kepler that Gassendi intends, along the way, to give his own view as to what this says about material causation. Notably, he tries to explain what causes physical events while avoiding the two untoward extremes of an atheistic atomism on the one hand and the gratuitous involvement of God in everyday affairs of the universe on the other. Whether entertaining a theory of light or magnetic attraction or the generation of organisms, he looks for a common causal story that underlies all natural phenomena and does not require the finger of God--though neither does it exclude God altogether. By locating causal agency (in a strict physical sense) in atoms, Gassendi provides the foundations for

¹⁰⁶Q I 280b.

¹⁰⁷Q I 273b.

¹⁰⁸Q I 273b.

such a story, allowing us to account for causation in nature by talking only about states and interactions of matter's constituent particles, without having to weave God into our causal picture at any stage past Creation.¹⁰⁹ Of course, when we ask how atoms came to have that agency, it is perfectly consistent to suggest, as Gassendi does, that God the creator must have made atoms in just that way.¹¹⁰ Yet while God remains, theologically speaking, the ultimate causal agent (the First cause *per se*), Gassendi considers atoms, which constitute "everything in nature except God, that has some capacity to act",¹¹¹ as the proximate causal agents behind all instances of physical motion. The idea is that atoms serve as the 'engines' of all material motion and change—including psychological drives—which suggests to Gassendi a slightly more colorful metaphor, the 'flower of matter':

When a child rushes towards a fruit which is shown to him, it is not only the metaphorical motion by which the fruit tempts him that is necessary, it is still and above all the physical or natural force which exists in this child and which directs and pushes him towards the fruit. This is why, for each thing, since the principle of action and movement is the most mobile and active part—in some way the flower of all matter [*quasi flos totius materiae*]—and the part which we are accustomed to calling form and which we can conceive of as a very loose arrangement of very subtle and mobile atoms, it seems more natural to say that the first cause of motion in physical things are atoms. This is because, while they move themselves in virtue of the force they have received from their Author from the beginning, they give motion to all things and are consequently the origin,

¹⁰⁹Gassendi writes: "Motive force, which in whatever way is in a concrete thing, owes its origin to atoms." *Q* I 384b; cf. 343b, 638a.

¹¹⁰For Epicurus, there is no need for a creation story because he thinks the atoms have been around forever, with all of their basic properties. But for Gassendi, at the very least his theological interests suggest that nothing *else*--for example, no other bodies--could have made atoms and invested them with a tendency to motion.

¹¹¹*B* 409.

principle, and cause of all movements that exist in nature.¹¹²

In brief, this internal principle of motion found in all fundamental particles is what entitles us to think of them as causal agents.¹¹³ It follows from this that causal relations between bodies are the product of phenomena internal to those bodies, and in this Gassendi avoids what are for him two prominent unsatisfactory alternatives. The first alternative is that we base our causal principles on the premise that agency is somehow located externally to the causal relations we seek to explain, as in some theologically-inspired views or the proposals of neo-Platonists or neo-Pythagoreans like Kepler who see causal efficacy in incorporeal sources such as the harmonies of the spheres.

Yet we need to locate efficient causation in something corporeal, Gassendi argues, because all actions of bodies are physical, and physical actions may be brought about only by corporeal principles.¹¹⁴ Among the incorporeal alternatives he rejects are those theories that suggest a source of physical action in a 'soul of the world' which, according to some versions, is God.¹¹⁵ One version of this view has it that a soul of the world produces a vital heat as the source of action, but he claims this cannot be taken seriously

¹¹²Q I 337a.

¹¹³B 409.

¹¹⁴B 415-416. Gassendi charges that the Aristotelian alternative, that *form* is an incorporeal principle of motion, is entirely incoherent since it is not possible to conceive of form completely bereft of matter, and even if it was possible, form would either be a merely passive mode of matter and so incapable of making matter active, or else an additional entity the origin of which would be a mystery.

¹¹⁵Brundell (Pierre Gassendi, 126-132) suggests that the sort of 'soul of the world' hypothesis with which Gassendi is specifically concerned is the kind endorsed by numerous kabbalists and 'chymists' like Robert Fludd.

as representative of this perspective given that heat is corporeal. In general, though, any such perspective that says God is the causally efficacious soul of the world introduces superfluity to our physics because we don't need to put little pieces of God into matter to account for the action of bodies:

It is sufficient that God be incorporeal and that he pervade and support the universal machine of the world, but it is not necessary for him to be like the soul, or the form, of the world in such a way that his substance is pulled apart...and cut into little pieces which become the individual souls...of men, but also of beasts, even of plants, even of metals, of stones, and of every single thing....¹¹⁶

Moreover, Gassendi proposes, it is absurd on any acceptable physical or theological theory to suggest *anything* incorporeal could be divided into parts or affected by anything corporeal.¹¹⁷

Worse still, other views proposing incorporeal agents of physical causation--primarily those of the neo-Pythagoreans--suggest that some objects might be causally related though they never actually interact. Any such view is clearly unacceptable to Gassendi, for it conflicts with his projects of a mechanistic physics and a cosmological and psychological panorama of causally-active spirits, according to which we tend to stipulate causal relations between objects only if we detect that they interact physically or else mentally or--as his psychology has it--as a hybrid of the two. The neo-Pythagorean proposal, by contrast, seems a superfluous afterthought to either the natural *or*

¹¹⁶B 412.

¹¹⁷B 413. A ready result of this is that God, souls, and angels cannot move matter by physical means. Gassendi proposes that God produces physical events not by his own motion but by his mere command, and that incorporeal souls stimulate only incorporeal acts (like mental acts). The capacity of angels for moving matter is a more vexing problem, for which Gassendi sees only recourse to theological explanation.

supernatural philosophies he envisions.

A second alternative is that we base our causal principles, following the Aristotelians, on the premise that causal efficacy lies in features or 'principles' (namely, substantial form) that are necessary to and definitive of, though not physically present in, bodies of a given kind. Gassendi rejects this alternative on the grounds that whatever is definitive of bodies of a given kind is not necessarily causally efficacious, and is particularly less likely to be so if you cannot physically locate the supposed agent.¹¹⁸ This flawed Aristotelian approach only looks viable, he maintains, if we presuppose that the causal principles may be somehow distinct from matter, such that attributing to one substance the ability to bring about action affecting *another* substance in virtue of their material character confuses material and efficient cause.¹¹⁹ But this supposition is made without justification, Gassendi contends. Thus, in response to Aristotle's charge that the early atomists don't even attempt an account of the cause or principle of motion, Gassendi proposes that Leucippus and Democritus simply make internal to atoms what Aristotle baldly and mistakenly takes for granted as a separate, extra-material causal principle. The ancient atomists, he suggests,

...only wanted to ensure that the efficient principle not be considered distinct in

¹¹⁸Q I 335a. Gassendi also takes the Aristotelians to suppose that substantial forms, as the active aspect of substances, constitute some kind of incorporeal causal agents, which he accordingly dismisses on the grounds (discussed above) that there can be no such agency among material objects altogether. Worse still for the Aristotelians, he argues, forms cannot be active as they are supposed because they are mere aspects of entities and not themselves distinct entities. If they cannot be active, surely they cannot be causal agents.

¹¹⁹Aristotle levels this criticism against pre-Socratics like Heraclitus but, as Gassendi notes, atomists are also subject to this charge, for they maintain that these two Aristotelian causes are actually (if not conceptually) indistinguishable as the primary principles of matter. B 411.

substance from the material principle. It is to be noted...that they considered atoms not to be inert and immobile at all, but on the contrary to be extremely active and mobile. And for this reason they held them to be the first principle of motion. However, Epicurus expressed the doctrine more explicitly when he said that the property of atoms which Democritus did not name was in fact their weight, the source of their motion.¹²⁰

Some offending element of this second alternative also shows up in the otherwise rather distinctive view of the Stoics. For while they think the causal principles of the physical realm are corporeal, like the Aristotelians they also fail to tie causal efficacy to particular bits of matter since they suggest those principles take the form of vital spirit or *pneuma*, and not distinctive bits of impetus-endowed matter (nor inert matter, at that). Though Gassendi himself never objects to their physics on these grounds, neither can this view commend their picture of physical causation to him.

In sum, Gassendi holds that locating causal agency in atoms, or more specifically, maintaining that atomic weight makes them essentially mobile, gives us a more plausible story of natural causation than the available alternatives. He takes his story to be more plausible because, unlike those alternative perspectives, it is consistent with his view that such agency is a feature internal to bodies and defined independently of any other, non-physical features thought to be characteristic of bodies. Here he seems to be relying on the background assumption, that underlying the causal relations of any set of objects--which we standardly take as evidence of some sort of causal agency--are some innate causal features of those objects so related.¹²¹ Without this assumption, we might well

¹²⁰Q I 333b.

¹²¹Apparently, Gassendi also assumes such causal relations should be *signs of* these causal features; more on this below.

think causal relations fail to indicate any particular sort of causal agency among the relata. But with this assumption it is natural to generally locate causal agency in the relata; and with his further claims that all there is is matter and void and that, in the context of our physics, matter is the primary bearer of causal relations, it is a natural next step for Gassendi to specifically locate causal features (namely, weight) in what he takes to be the basic unit of matter, the elemental particles.

The principal argument *against* atoms having inherent motion, as Gassendi sees it, is that we can take nothing—including matter in any form—to be the source of its own agency, or else we risk conflating artisan and artifact.¹²² To this he responds that while artifacts are by definition endowed with agency by distinct agents (namely, their creators), there is no appropriate analogy with small natural bodies which compose the structure of larger natural bodies and contribute their agency to those larger, encompassing structures. This is because the smaller bodies are not the intention-endowed creators of the larger bodies. Hence the motion of the encompassing structures may depend solely on the agency of their internal components though (disanalogously) artifacts must depend on agency from without. A more appropriate analogue to such natural aggregate bodies, Gassendi suggests, is a social grouping like a military unit which functions as a whole because of the agency of its internal components. In this case there is no trouble with reducing such corporate agency to the sort of individual agency God may simply build

¹²²This is a charge which dates to Aristotle; a theologically-minded critic could well refashion the problem as a conflation of the Creator and Creation.

into the elements of matter¹²³ Thus, he concludes, what he takes to be the principal argument against the inherent motion of atoms is simply misplaced.¹²⁴

This argument can be none too compelling for such opponents of inherent atomic motion because they are not as worried about whether the larger bodies composed of atoms may be said to get *their* motion from their constituent parts, as they are about where the atoms themselves get their own motion. But even if it were compelling, two sorts of problems remain. For one, the natural philosophy which results from Gassendi's proposal deserves a charge of what might in our times be called 'vitalism'¹²⁵ given that he proposes to find in atoms that inherent motive force which is a necessary and sufficient condition for the activity of living things. This is the very much the sort of active force we find in Leibniz's monads (or, perhaps more analogously, the derived *vis viva* his secondary matter features) and, like Leibniz later on, Gassendi tells a partly vitalist story about the workings of those corpuscles from which all organisms and (at least some aspects of) mental apparatuses are built. Some atoms are designated, perhaps in the moment of Creation, to come together and form 'seeds' which, with respect to the organic realm, carry developmental information for organisms; with respect to the chemical realm, for particular metals; and with respect to the geological realm, for particular

¹²³Q I 336b.

¹²⁴B 418–419. He adds that, if we now work our way back through the proposed analogy with artifacts, and we *did* assign internal agency to their parts, we would be stuck with the absurd result that they are all automata.

¹²⁵This is the modern term (say, after Driesch) for the attribution of living qualities to nonliving matter; in seventeenth-century terms, we might less anachronistically think of such views as belonging to a family of doctrines of living matter, a sort of 'animism'.

minerals. I indicate below exactly how badly this sort of view sits together with Gassendi's intentions for a mechanist natural philosophy; let it suffice for the moment to suggest that he should have some difficulties neatly sorting out what is organic and what is not, and saying how such different sorts of things obey the general physical principles he outlines.

Indeed, even putting these last difficulties aside, the physical difficulties with the picture of anything being the source of its own agency are quite independent of the artisan/artifact problem. Minimally, we don't want that picture because it makes a mystery out of his principles of motion and out of change in velocity and rest; I say more about this below. Moreover, as Brett notes, locating the source of motion in the atoms themselves renders irrelevant any notion of causality which involves a relation between two or more objects.¹²⁶ The crucial difficulty here is that Gassendi seems to suggest motion is not essentially imparted to one body by others, at least on the atomic level. This raises at least two questions: if there is no such imparting of motion, what accounts for the apparent phenomena of the same? and on the other hand, if motion really is imparted from one body to another, how is this possible? We know Gassendi is concerned with the second question, for he proposes that things which are moved require another, motion-imparting agent: "...the same thing cannot be both the mover and the object moved...whatever is moved is moved by something else...."¹²⁷ In this passage, by the way, he is actually interested in avoiding an infinite regress in causal explanation, by

¹²⁶George S. Brett, The Philosophy of Gassendi, (London: Macmillan and Co., 1908), 58.

¹²⁷B 419.

borrowing a page from Aristotle and requiring some primary mover in his physics--the atom, in his view.¹²⁸ At the same time, he commits himself to the view that motion may be imparted to one body by others--which, we will see below, does not fit very well with his proposal that atoms have their own internal causal agency.

A different though perhaps related problem with attributing internal causal agency to bodies is that we standardly attribute such agency to forces *external* to bodies causally related, as in impressed and attractive forces. Indeed, Gassendi himself discusses such forces at great length.¹²⁹ His ready response is that the external brand of causation in each case can be explained as the product of internal causal events. For example, he understands magnetic force as bodily attraction that occurs when one body issues a corpuscular stream which brings another body towards it by a grabbing action of corpuscular protuberances that may resemble hooks; I look at his views of attractive forces in greater detail below.

Finally, the proposal that atoms have an internal principle of motion raises the puzzling questions as to whether atoms are actually supposed to be in motion at all times, and if so, whether it is possible to account for atoms at rest, or at least those conditions under which we would think atoms should be at rest. As for the first question, does Gassendi think of his principle of motion as a constant impetus, an ever-present and

¹²⁸B 421-422. Where he differs with the Aristotelians, though, is in requiring not an *unmoved* primary mover but one with its own source of motion--that is, as endowed by God, from whom all motion originates. Aristotle, for his part, explains the origin of motion by a final cause (attraction to the unmoved mover as a goal-state), which Gassendi rejects as an appeal to a 'metaphorical motion' instead of a true physical cause.

¹²⁹Cf. *DeM*, Q III 478a-536b.

active force which compels atoms to move? This is what he seems to suggest when he writes that

...there persists in things, in a constant manner, as much impetus as there was since the beginning.¹³⁰

Or else does he think of this principle as a mere dispositional state, a capacity of atoms which simply enables them to participate in the causal nexus?

This last possibility amounts to the suggestion that Gassendi sees the motive principle of atoms as no more than a capacity, such that they need not always be in motion. Since Gassendi holds that *pondus* or weight gives atoms an inherent force (*interna facultas seu vis*) necessary for motion, this capacity for motion of course would be present at all times.¹³¹ But it does not follow from this that atoms would have to be constantly in motion. Moreover, Gassendi never states that this inherent force is sufficient for motion. If he never says this, then perhaps he does not think their motion is ceaseless.

One benefit of this interpretation is that atoms should turn out to be causally inert bodies (that is, bodies governed by a principle of inertia) which, as we will see below, is what he thinks macro-sized bodies are. Another is that claiming an atom is at rest does not have to be explained away. On the other hand, Gassendi would then have something new to explain, namely, when and how such a capacity or tendency is realized, so that

¹³⁰Q I 343b. Curiously, by talking about 'things' and not atoms *per se* in this passage, Gassendi appears to suggest that this innate propulsion persists at a constant rate for both atoms and their aggregates. However, he neither repeats this locution, nor does he express any such views elsewhere.

¹³¹Q I 273, following Epicurus; cf. SEP 17.

motion starts--or not, so that motion ceases. In any case, however, aside from his failure to stipulate that the motive force of atoms is sufficient as well as necessary for their ceaseless motion, there is no real textual support for this suggestion. The closest Gassendi ever gets to this sort of view is when he seems to suggest that the internal principle of motion is not what some Scholastics (like Jean Buridan and Nicholas of Oresme) think of as 'impetus':

...the idea that atoms have impetus, or the power to move themselves inherent in their nature, is to be rejected and also its consequence that they have motion by which they have been wandering and have been impelled every which way for all time.¹³²

In this passage he claims atoms do *not* have the power to move on their own, ceaselessly or otherwise as "inherent in their nature." One possible reading here is that, quite contrary to the view of atoms as containing a principle of motion, Gassendi suggests atoms only move if they are impacted upon, that is, they lack an internal and constant motive force. Yet in the very next sentence he proposes that atoms indeed have such a force:

It may then be admitted that atoms are mobile and active from the power of moving and acting which God instilled in them at their very creation, and which functions with his assent, for he compels all things just as he conserves all things.¹³³

It looks like this instilled 'power of moving and acting' must be the sort of internal impetus he seems to dismiss a moment earlier. I think the key to understanding these passages lies in his suggestion that this power 'functions with [God's] assent', and that this

¹³²B 399.

¹³³B 399.

is so because God compels things in the same way he conserves things. Let's recall, in this regard, that Gassendi views God as the primary metaphysical cause (that is, as Creator) even if matter has its own internal principle of action. Thus, if we take God's compelling things to be the same as to his causing motion, then this passage appears to suggest that God conserves and compels after Creation without interfering directly in it, like the hands-off guarantor of the Clockmaker model. Accordingly, God's assent to atomic motion is not divine intervention but divine permission. The power atoms have to move is very much their own--just as the Scholastics might have said (had they been atomists, that is). What Gassendi seems concerned with here is blocking an inference from the claim that impetus is internal to bodies to the further claim that such impetus is *intrinsic* to bodies, in the sense that it need not have been implanted by anything else, such as God. In this passage, then, he is interested not in dispelling the notion of an impetus internal to atoms, but in stressing that this impetus must be an artifact of Creation and not an eternal feature of (equally) eternal atoms, as it might be if for example it were "inherent in their nature". Not only does he say nothing here or elsewhere to suggest that internal impetus consists merely in a tendency to motion; this passage provides more evidence that he conceives of it rather as a constantly active atomic property.

As Gassendi views it, one consequence of stipulating that a constant force compels atoms to move and that the atomic weight which just is this impetus is a fixed and ineliminable property¹³⁴ is that atoms have a constant degree of mobility. That is, for

¹³⁴His word is 'mode', but we have already seen that he uses this term to indicate something very like 'property', 'feature', or 'quality'.

each atom, there is some set degree to which it is propelled by its impetus. Gassendi thinks this best explains first, what he takes to be paramount features of atomic motion, its perpetuity and spontaneity, and second, variation in the mobility of the macro-sized objects they constitute.¹³⁵ He considers, by way of contrast, two alternatives. One is that individual atoms may *vary* across time in their inherent mobility and that this directly translates into variations in the mobility of macro-sized objects. Another alternative is that some atoms are wholly inert whereas others are mobile, and this is what gives the objects they compose their apparent variety in mobility.¹³⁶ Gassendi may have been thinking here of the Stoics, who--though they were not atomists--proposed that some matter is inert and some is active, or its own efficient cause. Yet, however well these two alternative scenarios account for variation in the mobility of macro-sized objects, they fail to insure that atomic motion is perpetual and spontaneous. For example, neither scenario provides any guarantee that atoms which meet obstructions or restraints will be able to resume motion once the obstructions or restraints are removed. Thus Gassendi rejects both alternatives and embraces the view that all atoms are endowed with a constant mobility.

In light of this constant degrees of mobility among atoms, Gassendi suggests, it is not obvious how macro-sized objects they compose may vary in their mobility. He says

¹³⁵B 417-418.

¹³⁶B 400. In this regard, Gassendi does not even consider the Aristotelian view that all matter is essentially inert without the presence of form, most likely because he has already dismissed the idea that form, as immaterial by definition, can act as a physical cause of material motion, much less contribute to a variety in the motion of bodies.

that we need some sort of corresponding variation on the micro-physical level, and proposes that a sort of variation in atomic motion arises from those states where--because of obstructions or restraints--the impetus of atoms is constrained. This is not exactly coming to a state of rest, Gassendi maintains; what he seems to have in mind is that such atoms continue to move even when restrained, perhaps by spinning or vibrating.¹³⁷ In those collisions where one or more atoms blocks the path of another, he suggests their motion is merely prevented in one direction such that they veer off in another; they neither lose nor gain speed. In the very moment of collision, atoms 'pause' though they do not rest *per se*, that is, they do not lose their inherent mobility. When the one set of atoms ceases to block other atoms, they all begin their trajectory-wise motion anew, at original speed; thus their motion retains its spontaneity.¹³⁸ In sum, while the impetus of atoms cannot be permanently or wholly diminished by collisions or combinations with other atoms, its effects can be temporarily and partly blocked. In this way atoms themselves may move perpetually yet with some vicissitudes, and so allow for variation in mobility among the larger bodies they compose--even larger bodies at rest.¹³⁹ The biggest puzzle

¹³⁷Correspondingly, those atoms set in motion have 'freed' impetus--but in no case does Gassendi want to say that atoms ever loses their impetus.

¹³⁸One awkward result of saying that directly after being constrained, atoms pick up their pre-collision velocity where they left off, is that acceleration and deceleration do not occur at the atomic level. Perhaps the single merit of this noninertial facet of atomic motion is that it is consistent with at least one other characteristic Gassendi attributes to atoms; as Dijksterhuis has noted, if they are truly solid, then atoms cannot behave as elastic bodies.

¹³⁹This last sort of case Gassendi believes is evident from such empirical data (borrowed from ancient sources including Epicurus) as our observations of particles of dust move around in rays of sun light, and particles move around in the vapor created from the compound of mercury, tin, and sublimate. *Q I* 287f, following Epicurus; cf. *SEP* 18.

about this proposal is why Gassendi thought there was a problem here to begin with, since macro-physical objects may vary in their mobility just as long as their atoms exhibit *any* sort of variety in their motion--that is, across atoms and relative to, for example, direction or rate--whatever the constancy of their being able, or even compelled, to move.

Aside from such puzzles and problems, one apparent merit of Gassendi's imputing motion to atoms is that he thereby expresses a plausible way of tying mechanics to atomism--one principle objective of his physics. For if the 'principle of motion' or impetus is internal to atoms, everything that is in motion is corporeal (and conversely, nothing that's not a body moves), and everything corporeal is composed of atoms, then talk about motion *just is* talk about moving bodies and, more particularly, their constituent moving atoms. This picture of things is the product of a significant, if flawed, approach in the histories of either atomism or mechanics: among his fellow mechanical philosophers only Newton also attempts to guarantee that physical explanations are mechanical by attributing the source of continuing material motion to a feature of matter itself. Such a strategy is, of course, even more foreign to those earlier Aristotelians who identify motion as a category conceptually distinct from those objects which move, or to those post-Newtonian physical theorists who admit fundamental forces and action through a continuum (e.g. Euler and Boscovitch) rather than account for all physical phenomena in terms of corpuscles in motion, alone--or at all.

Nevertheless, this approach is flawed, and this way of tying mechanics to atomism is only *apparently* advantageous, because Gassendi's proposal of an innate atomic motion fits poorly with some rather important mechanist claims which, I argue, he either states

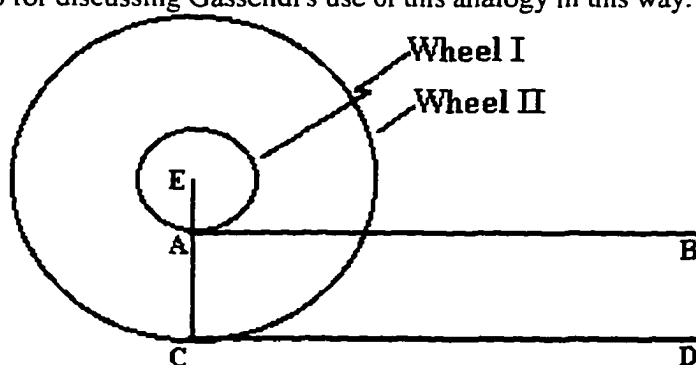
outright or transparently assumes. These claims include the suggestion that the differing primary material features of bodies should produce variable rates of motion, a principle of conservation of momentum, and a principle of inertia. That Gassendi's views on atomic motion fail to match up smoothly with these general physical principles, I suggest, is an indication that he equally fails to satisfy the rather significant (meta) principle of scalar invariance which, as I propose at the beginning of this chapter, he wants in order for these general principles to govern all manner of physical entities.

As to the first claim, Gassendi indeed holds that we should find variable rates of motion among different atoms--insofar as we could measure the rates of atomic motion altogether, that is--but not because their central features differ. By contrast, he thinks there are altogether different reasons why atoms on the one hand, and macro-sized bodies on the other, should have variable rates of motion. In the case of macro-sized bodies, rates of motion differ because of factors such as size, internal composition, and external resistance or attraction. In the case of atoms, however, external factors should not matter given their internal and constant impulse to motion. Further, there is no relevant sense of internal composition since atoms are solid (that is, contain no vacua) and are therefore not physically divisible. Thus if atoms vary in their speed it seems this should be because at least one atomic feature such as size or shape or weight also varies. Yet he instead accounts for variable rates of atomic motion by allowing that any two atoms may travel different distances over the same time on the grounds that, among their respective

collected states-in-motion, there may also be states of rest.¹⁴⁰ Hence atoms may move at different rates even though they appear to move at identical rates.¹⁴¹

It is not entirely clear why, to begin with, Gassendi thinks that without factoring in states of rest we should expect atoms to move at uniform rates. One might think he is

¹⁴⁰Gassendi introduces the notion of atoms at rest by way of an analogy with a proposed solution to the paradox of Aristotle's Wheel (*rota aristotelis*)—which he discusses among his attempted resolutions of the paradoxes of motion classically taken to have anti-atomist consequences (Q I 340-342). The solutions he proposes rely on special features of atomic motion. In this paradox, wheel I of circumference x , in the middle of a wheel II of circumference $n > x$, are fixed together such that, when the larger wheel travels a given length, they appear to both travel the same lengths (as measured along the tangents AB and CD extending from the radii drawn from center point E of both circles to points A and C, respectively) in the same time though the distances traversed by these wheels over the same period of time *should* differ by some factor of $n-x$ (see diagram below). Gassendi's solution is to suggest that any two such wheels travel identical lengths—arriving at the same place at what seems to be the same time—because the inner wheel which he supposes *should* travel faster (clearly he's wrong just here) rests along its path in motion. This solves the paradox, he claims, by factoring in rest as a normal component of measured velocity, and so distinguishing between truly identical speeds (truly identical distances traversed in identical times) and merely apparent identical speeds (distinct distances traversed in identical times because of differences in rest time). By analogy, then, atoms may travel at different rates even if they otherwise move at the same speeds because of intervening rest states. Thanks to Carla Rita Palmerino for discussing Gassendi's use of this analogy in this way.



¹⁴¹Q I 355a. If atoms have internal impetus yet pause occasionally, this means that the distances they travel over a given time are relative to the ratio of their total time in motion to their total time at rest. Dijksterhuis (430) compares this with measuring the density of bodies by the ratio of their total atomic mass to their total volume (comprising atoms and void). Yet this comparison is not quite apt: the ratios in the former case—without incorporating velocity—are only partial functions of the distances travelled over a given time, whereas in the latter case the ratios alone give the densities.

inferring that atoms have a constant degree of motion, or speed, from their inherent and constant degree of mobility. Yet constant mobility does not yield constant velocity, and one would have thought Gassendi should recognize this. Alternatively, some commentators (such as Pancheri) have suggested that Gassendi is simply following a view sometimes attributed to Epicurus on the basis of DL X 61. In that passage, Epicurus claims, somewhat confusingly, that atoms all travel at a speed which is faster than one which we may contemplate. The problem with this interpretive suggestion is that it doesn't follow from this that atoms all travel at the same speed--and moreover, Gassendi never quite endorses this view as his own. The closest he comes is his gloss on the Democritean and Epicurean view: "...whatever way the atom moves in the boundless void...it always moves with the same speed and does not change its path, as long as a meeting with obstacles capable of changing it does not take place."¹⁴² Though he cites this ancient view approvingly, he never defends it by independent argument; what's more, this view does not guarantee uniform rates of motion for atoms which, for example, meet obstacles yet are not subsequently at rest.

In any case, this proposal introduces problems for both the notion of an innate motive principle and the notion that atoms should vary in their rates of motion as a direct consequence of their varying in size or shape, let alone their weight. This first sort of problem arises from the suggestion that atoms may actually cease to move--a possibility which Gassendi apparently rejects insofar as he takes their internal impetus to be constant. That there could be, after all, states of atomic rest easily fits the (previously

¹⁴²AN 422.

rejected) alternative interpretation of the internal principle as no more than a *potential* for motion. Ironically, the less-accomodating standard interpretation—that atoms contain a constant and constantly active impetus—together with the claim that atoms actually move at the same rate may well have inspired Gassendi to design his account of variation in atomic motion. For saying that atoms are constantly in motion at uniform speeds suggests that whatever looks like variable speeds requires some explanation, which the atoms-at-rest proposal attempts to provide.

But what support does Gassendi adduce for the proposal that atoms may be at rest? One element of support is his suggestion, following Epicurus's view of atomic collisions as checks, or ἀντικοπή, that moments of collisions constitute moments of rest.¹⁴³ Another possible source of support here is the claim that atoms constitute coherent solids just in case they remain fixed in position, either because they are held in place by hooks or pressure or simply in virtue of their shape and resulting fit with surrounding bodies.¹⁴⁴ This last point is unsatisfactory, though, because fixed position is not the same thing as a state of rest, and Gassendi even states that such atoms may continue to move, perhaps (for example) by vibrating.¹⁴⁵ The first suggestion is a bit more plausible, but if the impetus is truly internal and constant it is hard to see how

¹⁴³This point is made by Rochot and Partington; Epicurus's views are discussed in Lasswitz and Bailey. See Rochot, Les travaux de Gassendi; J. R. Partington, A History of Chemistry (London: MacMillan and Co., Ltd., 1961), vol. 2, 462; Kurd Lasswitz, Geschichte der Atomistik vom Mittelalter bis Newton (Hamburg and Leipzig: L. Voss, 1890), vol. 2, 174f; and Cyril Bailey, The Greek Atomists and Epicurus (Oxford: Oxford University Press, 1928), 329.

¹⁴⁴Q I 394f, following Epicurus; cf. SEP 23.

¹⁴⁵"...even in compound bodies atoms continue to move (imperceptibly to be sure) in an uninterrupted fashion..." Q I 279a.

collisions should be able to strip atoms of this feature, even momentarily.

Yet the main difficulty, as I see it, is that Gassendi ignores here a mechanical principle to which he at least implicitly subscribes in other contexts, that the character of any instance of material motion is determined by the character of the matter in motion and any interactions it endures. More specifically, Gassendi embraces the commonplace of the mechanical philosophy that, if macro-sized bodies have different shapes, sizes, and weights, then--all other things being equal--they should move at variable rates. Further, if such bodies with varying features should collide with one another, they should also rebound at variable rates. And while Gassendi clearly accepts this as true on the macro-level, he thinks there is variation in the motion of atoms only if there are fewer or greater instances of their coming to rest. As I noted above, he distinguishes atoms from larger bodies in that the motion of the former is not diminished by external factors, nor is their motion composed of that of any internal parts, for they have none. But none of this rules out the possibility that, if their inherent features of shape, size, and/or weight differ, then so too may their rates of motion.

While Gassendi is actually silent about this possibility, several points in his Physics partly suggest it, anyway. For one, he certainly thinks atoms may differ in size and shape, and while he is not explicit about atomic weight, he gives no reason to doubt that weight (that is, considered as mass) differs across atoms, too. It might be objected that, since Gassendi equates atomic weight with atomic motion and takes the latter to be constant, we should infer that the former is supposed to be constant, too. Yet that would rule out variation only in the motion of any single given atom, and not across atoms. For

another, if atoms may truly come to a rest, they may do so only if they first decelerate, and regain their original speed only if they first accelerate from rest, and this alone suggests that variation in rates of motion is possible even in a single given atom, and that this may be brought about by interactions with other bodies such as constraints or collisions. Finally, supposing that atoms can decelerate and even come to a rest and that there is a conservation of momentum, we should expect collisions between atoms to produce changes in the rates at which they travel: for each deceleration on the part of one atom, there should be a corresponding acceleration in at least one different atom with which it collides. Now, none of these points contradicts Gassendi's proposal that we explain different speeds by interspersed rests, nor does any of them depend on the claim that there is within atoms an internal and constant impulse to motion. And unlike these last claims, each is supported by fundamental principles accepted by other mechanical philosophers as well as Gassendi in his macro-physicist role. This raises a question, then, as to why in his role as an atomist he introduces an account of variation in atomic motion which has nothing to do with--indeed, is superfluous to--such standard mechanist sorts of accounts.

A second area of puzzlement regarding the internal impetus of atoms and the mechanical philosophy concerns the conservation of total momentum. One contemporary and influential principle of conservation, enunciated by Descartes, says the total momentum of all matter is conserved because the sum of accelerations and decelerations across all matter or all space (which are equivalent for Descartes) remains constant. But it seems strange, not to say impossible, to propose that we could sum over accelerations

and decelerations of different bits of matter which are supposed neither to accelerate nor decelerate because, as Gassendi suggests, they contain their own constant impetus.

Yet he actually proposes something that looks a bit like a principle of conservation, according to which there is a mutually compensating transfer of impetus among colliding atoms, hence no overall loss or gain:

...when one atom impacts upon another, it receives as much repulsion as it gives the other impetus...¹⁴⁶

Thus there is not only an overall conservation of momentum, there is also a conservation of the momentum particular to each atom.¹⁴⁷ As Dijksterhuis has noted, Gassendi's principle differs from the cartesian one in that Gassendi's suggests there is a conservation of total momentum because the sum of accelerations and decelerations for each atom remains constant (at zero) given that atoms always retain their internal impetus.

One immediate difficulty here is that any such compensatory transfer clearly entails the possibility of transferring impetus, however this would seem to be precluded by the notion that each atom's impetus is fixed. But even without this problem, can such a principle guarantee conservation of total momentum? There is a good reason for pessimism at the outset: Gassendi addresses the conservation of momentum from quite a different perspective than do the authors of the classic principle. As Dijksterhuis puts it,

¹⁴⁶Q I 343b.

¹⁴⁷MS Tours 709, folio 306 r-v, as quoted in Brundell (Pierre Gassendi, 78 and 193). One point in favor of this proposal is that it certainly seems consistent with Gassendi's account of weight as a necessary and definitive feature of atoms. For if an atom's weight cannot change lest we lose a definitive characteristic of the atom, and weight just is the motive force internal to atoms, then the original motive force should not change either.

views like Descartes's have it that there is conservation whatever the variations in the momenta of individual bodies and their constituent corpuscles, whereas Gassendi holds that conservation results from maintaining precisely the opposite situation, that is, keeping atomic motion uniform: once set in motion (in Creation), atoms continue on indefinitely with their internal impetus intact. But the primary reason this proposal fails to guarantee conservation of momentum is that Gassendi's physics also features a non-natural or violent motion which is not innate to bodies but instead results from the collisions of atoms. The upshot of this additional momentum is that there should be a *surplus* beyond the momentum his conservation principle sustains. If Gassendi had a classic version of the conservation principle, he might be able to accommodate innate atomic motion, but he cannot defend additional sources of momentum on the basis of his own principle.

Finally, a third source of difficulties for the internal impetus theory concerns the principle of inertia. The historian Alexandre Koyré (1955) submits that Gassendi's physics is not scalar invariant insofar as he characterizes inertia according to a general principle which holds on the level of macro-sized objects but not on the level of atoms. I propose, following Koyré's brief assessment, that atomic motion in Gassendi account is fundamentally and irradicably noninertial.¹⁴⁸ Specifically, his atoms like those of Epicurus are fundamentally noninertial because they neither gain nor lose velocity, nor do they ever come to rest; and they are irradicably noninertial because the character of their

¹⁴⁸In (1996), I argue that past attempts to excuse or dissolve this variance have not been successful--and that, in principle, they could not succeed.

motion is determined by God who, naturally, is not governed by the principle of inertia.

Consider Gassendi's principle of inertia, which he formulates in his De Motu.¹⁴⁹

According to Gassendi's version of the principle,

- (I) a body is at rest or else moves at a uniform rate along a rectilinear path unless subject to some force, which may either accelerate or decelerate the body, and/or change its path.

That the motion of bodies is naturally inertial suggests that the only way they move but not in a straight line is when something disturbs what would otherwise (and naturally) be their rectilinear path. One key result here is that such inertial motion and rest are equivalent states, so that there is nothing 'unnatural' or 'violent' about the state of a body in motion and not falling toward the Earth (as the Aristotelians maintained). This is because the inertia theory recognizes that rest is a special case of uniform rate of motion (i.e. equal to zero) and that, by contrast, acceleration and deceleration of any kind or degree entails some change in rate of motion--which the principle (I) says is the non-standard (for the Aristotelians, 'unnatural') state.

What sorts of bodies are supposed to behave in these ways? Gassendi presents the principle without exceptions, so we should have no reason to suppose that it is less than maximally general in scope. Accordingly, micro-sized as well as mid-sized objects (like stones in an idealized void deep in space--Gassendi's example) should be naturally inertial.

But since Gassendi's primary micro-sized objects--atoms--are initially endowed

¹⁴⁹Other, less satisfactory versions of the principle had been propounded not long before by Beeckman (in his unpublished Journals), Galileo (in the inadequate terms of circular motion), and Descartes (in The World, which did not see publication until after Descartes's death).

with motion by God, they cannot fail to move at a fixed rate. This suggests, quite contrary to (I), that atoms as created may neither decelerate nor accelerate after the addition of force, as impressed in collisions. Moreover, although Gassendi stipulates that atomic motion follows a rectilinear path, he makes this point quite independently of (I); atoms would move in straight lines if the principle of inertia was not part of Gassendi's physical picture. While Gassendi wants a thoroughly atomist physics, if the principle of inertia does not apply to the microlevel, then even his atomism need not obey his physics. Koyré concludes, it seems reasonably, that Gassendi's science is inconsistent.¹⁵⁰

The really bad part of all this--which Koyré and his adherents do not seem to have noticed--is that if he is right, then it is hard to see why the principle of inertia should apply to anything bigger than atoms, either. After all, Gassendi wants accounts of macro-sized phenomena to be reducible to accounts of micro-sized phenomena, so if the corpuscles themselves don't obey his principle of inertia then their aggregates shouldn't be expected to, either. Indeed, it is not clear how their aggregates could do so.

To examine this matter in a little more detail, let's first look at Gassendi's mature formulation of a principle of inertia (by the way, he may have an unsatisfactory one as well, but that's besides the point here). In Section XVI of the first letter of *De Motu* (to Pierre Dupuy), he considers the case of a motionless stone in imaginary empty space. If a force impels the stone, he writes, then

..it is probable that it would move indefinitely in a uniform fashion, slowly or

¹⁵⁰At least one recent commentator--Thomas Lennon (1993, 14) shares Koyré's assessment; Lennon also takes Olivier Bloch (1971) to hold this view, but Bloch actually proposes a way to escape this inconsistency; see my (1996).

rapidly, depending on whether a small or great impetus had been imparted to it.¹⁵¹

Why? Gassendi regards this sort of motion as having the same uniformity as horizontal motion generally, which is only brought to an end by a countervailing perpendicular motion. But as there are no such countervailing motions in the imaginary empty space, so the stone must travel forever in that direction. Gassendi writes:

...whatever direction the motion had started in, it would behave like the horizontal one and would not be accelerated or slowed down, and therefore would never stop.¹⁵²

Next, Gassendi offers this thought experiment: Take a piece of string and hang a stone on it. If you push the stone forward, its motion would be uniform and perpetual except for the air resistance, and the weight of the string. Indeed, at first glance, it seems that the motion actually *is* uniform and perpetual. This is because the string neutralizes the stone's attraction to the Earth, so there is no countervailing perpendicular motion. Now, assume the stone is in a vacuum and the string is replaced by a weightless ray with no parts (it is in either case motionless, but here Gassendi emphasizes that it is now weightless as well). Set the stone in motion and it will continue in uniform fashion for perpetuity. Gassendi concludes that

...a motion imparted in empty space where nothing either attracts it or holds it back or resists it in the least would be uniform and perpetual....¹⁵³

We can see this principle enacted in the regular course of things if we simply

¹⁵¹DeM, Q III 495; B 139.

¹⁵²DeM, Q III 495; B 139.

¹⁵³DeM, Q III 495; B 141.

imagine that everything aside from a given body set in motion has been annihilated; then the body's motion will be rectilinear, uniform, and perpetual. That the motion of no body in our experience is so, then, is an indication of the countervailing perpendicular forces which act on those bodies. To arrive at a principle of rectilinear inertia, it seems Gassendi was clever enough to first abstract away all those other bodies which, because of attraction or repulsion, might otherwise interfere with uniform and perpetual motion.

And just to make sure that we understand the consequences of this view which are inconsistent with the Aristotelian view (though not with medieval impetus theory), Gassendi points out a bit later, in Section XIX of the same letter, that the motion of a moving body is transferred to it by the mover, so there need not be a continuous action on the part of the mover for the motion to continue. He notes that the mover bears a transferable motion to begin with only because it is itself in motion; indeed, anything propelling any other thing must itself be in motion:

...to understand how this transference takes place, reflect on the fact that before the stone is hurled into the air, it is joined to the hand for a certain period of time and may be considered one and the same with it as a single moving object because one and the same motion applies jointly to them both, or because the hand moves the stone with the very same motion as itself....¹⁵⁴

And further on, he generalizes this point:

...what I say of the hand may be understood to apply to any other physical mover. Indeed, there is no mover in nature which is not in motion itself, so that it accompanies the object it is moving a certain distance, directs it on the course it will hold, and by passing on an invested right as it were, impels it; nor would it ever impel something by simple contact without a forward motion of this sort.¹⁵⁵

¹⁵⁴DeM, Q III 498; B 144.

¹⁵⁵DeM, Q III 498; B 144.

This last claim might be confused with the suggestion that everything *is* in motion, which would explain why Gassendi could have taken the principle of inertia to be consonant with the view that atoms are in perpetual motion. Yet this claim entails no more than that anything which *may* move something else needs to be in motion when it does that moving. We can guess that Gassendi was, or at least should have been, aware of this much from his mention in this very passage of the possibility that a body 'finally comes to rest'.¹⁵⁶

¹⁵⁶As Olivier Bloch and Fred Michael have pointed out, Epicurus, too, has something that looks like a principle of inertia--and this may well have been a source of inspiration for Gassendi. But this is not Gassendi's announced path to that principle; he comes to it (in *De Motu*, at any rate) by first wondering about what causes the motion of bodies falling to Earth (where, by the way, he offers a corpuscularian account of the Earth's magnetic attraction), and then wondering about what happens when you position a body in space but take the Earth away, along with all other bodies in the space.

Further, the Epicurean account--which specifically covers the motion of atoms--is simply incompatible with a principle of inertia. Consider first Epicurus's suggestion that atomic motion is, generally, linear, and that

The atoms are in continual motion through all eternity. [DL X 43]

which, together with the proposal that

When they are travelling through the void and meet with no resistance, the atoms must move with equal speed. [DL X 61]

yields an account of atomic motion that indeed looks close to the inertial picture. That is, it seems Epicurean atoms move not only rectilinearly *and* perpetually, but also--as long as nothing resists their motion--uniformly. Yet there are two significant problems. For one, while atoms cease to move rectilinearly (that is, downward) because of resistance provided by the *clinamen* or swerve (by the way, an obviously noninertial source of motion), Epicurus never says that the otherwise perpetual character of atomic motion is subject to the resistance of other objects. To the contrary, in the passage just quoted, he says the motion of atoms lasts forever. For another, even his account of the uniform velocity of atoms must be, in the end, noninertial. To be sure, the well-known passage from the *Letter to Herodotus* certainly suggests the velocity of atomic motion has a properly inertial character:

Neither will heavy atoms travel more quickly than small and light ones, so long as nothing meets them, nor will small atoms travel more quickly than large ones, provided they always find a passage suitable to their size, and provided also that they meet with no obstruction. Nor will their upward or their lateral motion, which is due to collisions, nor again their downward motion, due to weight, affect their velocity. As long as either motion obtains, it must continue, quick as the speed of thought, provided there is no obstruction, whether due to external collision or to the atoms' own weight counteracting the force of the blow. [DL X 61]

And yet this suggestion falls short. One problem--which revisits Gassendi--is that no

With this short review of Gassendi's formulation of a principle of inertia in tow, we can see that while his atoms travel in straight lines they do so for the wrong reasons. Thus "the innate motion of atoms is straight and exceedingly swift,"¹⁵⁷ as he puts it, yet this rectilinear motion is not sustained for any inertial reasons but by an impetus internal to the atoms. Further, the reason Gassendi's atoms move in straight lines is that this is the course he thinks God designates when investing them with the proper impetus for perpetual motion. Atoms move in straight lines, and not swerves or, for that matter, figure-eights, because God wants them to move in straight lines, and not particularly because they conform to inertial behavior. Indeed, it doesn't really matter *why* God might want to start things up this way--but given the mysteries of the Divine and the omnipotence of God it seems presumptuous to assume that it's because rectilinear motion alone conforms to the principle of inertia.

In this context, however, the more interesting noninertial aspect of atomic motion is that atoms retain for all perpetuity a constant and innate propulsion or impetus and, in the absence of atoms coming to a rest, they move at a constant velocity. These features of atomic motion conflict with the principle of inertia in several ways. For one, if the level of impetus in, and the velocity of, each atom remains constant, then there cannot be either acceleration or deceleration of atomic motion: the former requires that some force adds

obstruction or collision can wholly decelerate the rate of motion in any case, given that atoms must remain in motion. More broadly, though, even when atoms move at a uniform rate unless obstructed, they may (by Epicurus's account) also move noninertially with respect to the perpetuity or rectilinearity of their motion, which is to say they are not moving inertially at all.

¹⁵⁷Q I 385a; thanks to Fred Michael for this translation.

to velocity, the latter requires that some force diminishes velocity, and the constancy of impetus and velocity prohibits either. And even supposing genuine variability in the speeds of atoms, it seems acceleration should be noninertial anyway because it would occur not only with the addition of a new force but also with the simple removal of obstacles from the path of an atom. For although atoms move in some way even when an obstacle blocks their path, they are not continuing along their previous trajectory at 'initial' or pre-obstacle speeds; and when such obstacles are removed, Gassendi supposes, they immediately resume their previous speed and direction of travel.¹⁵⁸ For another, as I argue above, given that atoms cannot lose their motion they cannot be at rest--which conflicts with any full-fledged inertial theory, including the one he formulates in De Motu. And if, as seems to be the case, he thinks anyway that they somehow can be at rest, the instances he describes are limited to such special circumstances as are created by other atoms, including moments of collision and/or being fixed in position. Hence even if the motion of atoms were somehow inertial, his theory would treat inertial motion and rest as two drastically different states, where inertial motion is completely normal and involves no other atoms, and rest is aberrant and requires the actions of or relations with other atoms. But this runs contrary to a principal insight of the principle of inertia, namely, that rest and motion are to be considered as equivalent states, in contradistinction to the Aristotelian view that rest and all motion outside of free-fall are aberrant 'non-natural' physical states.

¹⁵⁸Unless we suppose atoms to vibrate or spin at the very same velocity, in which case it is hard to see when this variability would occur.

Naturally, most problems of these sorts arise only if we expect to begin with that atomic motion should obey the same mechanical principles of motion as the motion of supra-atomic bodies. This in turn is a reasonable expectation only if, as I contend in the first section of this chapter, Gassendi supposes that the basic mechanical principles to which he subscribes are scalar invariant, so that his atoms behave like any other bodies are understood to behave in the rest of his physics. In addition to the reason I cited in the beginning of this chapter for holding Gassendi to a scalar invariance thesis, one further piece of evidence for his embrace of this thesis is his proposal that the motion of supra-atomic aggregates is the aggregate motion of constituent atoms.

For Gassendi, as for Epicurus and Lucretius, the motion of supra-atomic bodies is a composite quality, comprising the sum of motions of its constituent atoms. In his view, one consequence of this theory is that, when atoms are bound in even those smallest of aggregates known as 'concretions' or 'molecules', their inherent motions--if countervailing--may cancel each other out or at least slow down the aggregate's overall motion in a particular direction. Another consequence is that, if all atoms of an aggregate move in the same direction, then the aggregate should move in that direction, too; if not all constituent atoms are moving in the same direction, then the aggregate should move along the path traced by the greatest number of atoms.¹⁵⁹ Moreover, if some atoms in a body move slower than others, the faster ones should force the slower ones to keep pace by propelling them into fixed positions so they move along with the faster ones (this sort of fixed positioning also facilitates repetitive motion, Gassendi maintains, and so allows

¹⁵⁹Q I 384.

bodies to develop 'habitual' movements). In sum, the nature of an aggregate body's 'motive force' (*vis motrix*) is a product of the internal impetus of the atoms it comprises. When these aggregates grow, then, their speed increases relative to that of the atoms added:

...as other atoms join on to them, the molecules become a little larger and move slower or faster in proportion to the motion of the newcomers. Again, as further atoms are added, the molecules constantly become bigger and bigger until they are perceptible and whatever motions they have can be observed. Finally, the object comes to the notice of the senses; and not only single atoms, but also somewhat larger clusters and masses are gradually united to it and incorporated in it. In such a way all bodies, large as well as small, may be made and may execute particular motions and the actions particular to them in conformity with the fabric of the atoms. Hence it happens that whatever motion or action natural bodies have ought to be regarded as received from their atoms.¹⁶⁰

Now we can see why it is wrong to suggest that noninertial atomic motion can sit comfortably alongside the inertial motion of aggregate bodies. Gassendi says that *whatever* motion such aggregates have they receive from *their* atoms.¹⁶¹ It follows that the motion of atoms must be governed by the same principles governing the motion of all other, larger bodies--that is, unless atoms were simply supposed to lack properties which appear on the supra-atomic level, such as inertial motion. But then we need some

¹⁶⁰B 422-423.

¹⁶¹By the way, Gassendi hints here at one empirical test of his atomism; since the motion of aggregates is determined by their constituent atoms, and we can detect variations in the motions of aggregates, in principle we ought to be able to infer the motion of even single atoms from slight enough variations on the macro-level. Naturally, confirmation is hard to attain here; we would not likely find such an inference compelling unless we previously accepted that macro-sized bodies are aggregates of atoms. On the other hand, there is a possible falsification hypothesis lurking here: if the rates of change in the motion of macro-sized bodies varied widely and independently of minute additions to their total mass, we should have no reason to think such bodies are aggregates of atomic components that are the source of their motion. Gassendi does not pursue either of these lines of reasoning but this is hardly surprising given his inability to attain the relevant measurements.

account of how inertial motion arises or emerges from noninertial motion, and Gassendi's claim here (that aggregate motion is just the sum of the constituent atoms's motion) rules out any plausible account, for it is not possible to sum over noninertial motion and get inertial motion.¹⁶²

Another set of difficulties emerges when we consider the contribution atoms are supposed to make to the motive force of aggregate bodies against the background of what Gassendi intends in this context by *vis* or 'force'. Much could be said about how to best understand, or even translate, *vis*, but here he uses the term to indicate that quality of macro-physical objects which enables them to effect transit from one place to another; crudely, it seems he intends something like 'capacity to cause motion'.¹⁶³ If we assume a principle of inertia, as Max Jammer notes, 'force' then looks like 'capacity to cause

¹⁶²It has been suggested to me by several persons that it should be possible to sum over noninertial motion and get inertial motion, for example, if the forces of atoms in noninertial motion should cancel each other out such that the aggregate body they compose was in an inertial state. If this is simply to suggest that atomic aggregates could be at rest or travel in a rectilinear paths at uniform rates despite the very different behavior of their atomic constituents, then I must agree. Yet I am concerned rather with the question of whether their behavior can be characterized as inertial: how do the aggregates sustain such states and what should happen if some other, countervailing force were introduced? It is these circumstances, and not the accidental path or rate of the moving body, which is prescribed by the principle of inertia. Given a composition of atoms which behave noninertially, there is no guarantee that an aggregate so composed which happens to mirror inertial behavior over some period of time would continue to do so indefinitely. In any event, there is no reason to suppose (as one would have to, in order to iron out the conflict at hand) such instances represent the standard case.

¹⁶³Henry Margenau calls this a 'prescientific' sense of 'force' that was acceptable only when 'motion' was taken to indicate 'increment in vector velocity' and 'cause' was taken as 'dynamic enforcement'; see his The Nature of Physical Reality (New York: McGraw-Hill Book Co., 1950), 223. By 'prescientific', he doubtless intends no slight on the physics through D'Alembert but something like 'intuitive' or 'commonsensical'.

acceleration'.¹⁶⁴ Thus, if supra-atomic bodies get their motion from their constituent atoms, then apparently such bodies have a capacity to cause motion and acceleration because of the motion of the atoms they comprise. But this gives inherent atomic motion an unexpected consequence for Gassendi: the sum of such motion is thereby not a proportional amount of motion in the aggregate bodies (a tricky enough notion) but (somehow) an aggregate potential. It is entirely mysterious as to how he thinks we should understand such a notion.

Further, surely this scenario leaves us with a source of motion and acceleration among macro-physical bodies which makes their motion noninertial, for no impact with other bodies is required. Gassendi doesn't really mind this result; indeed, he actually likes it because it enables him to talk about familiar objects which appear to move on their own, as for example animate ones. However, it is unacceptable if we are to take seriously his commitment to a principle of inertia (at the very least, on the macro-level), which states unequivocally that non-rectilinear motion and acceleration are triggered only upon impact with other bodies.

The problem with Gassendi's view of atomic motion, in short, is the same problem which afflicts his atomism on the whole: he wants to explain motion (or other

¹⁶⁴Jammer further points out that, as another consequence of the development of a principle of inertia, 'force' has as well historically been taken to mean 'whatever deflects bodies out of uniform motion in a straight line'; see Max Jammer, "Force," in *The Encyclopedia of Philosophy*, ed. Paul Edwards (New York: Macmillan and the Free Press, 1967), vol. 3: 209. If Gassendi's atoms move noninertially, however, this is not a view he can accept. Other, more familiar traditional views include the notions that 'force' refers to $m \cdot a$, and that 'force' is whatever balances a weight, so that force is equal to the amount of weight held in equilibrium; there is no evidence to suggest Gassendi subscribes to either of these last views.

properties, at that) on the phenomenal level by appealing to the building blocks of matter which, he is convinced, must have special, distinctive qualities in order that the explanations be truly informative. While this may seem like a promising (or minimally, inoffensive) requirement for physical theories, it is hardly sufficient to guarantee either of two beneficial results: that the mysterious is not explained by the equally mysterious (an old complaint about the explanatory schema of first, hylomorphism, then later, corpuscularian matter theories); or that the special and distinctive qualities of atoms are consistent with the basic mechanical principles of his physics. With respect to this last result, here Gassendi fails to insure that his atoms behave according to the same dynamical rules he stipulates as governing macro-sized bodies. This might not be a failure if he either didn't have a scalar invariant physics or else if he provided a supplementary set of dynamical principles governing only the micro-physical world. Neither of these hold true, however, and so the motion of atoms is an exemplary case of the inability of atomic features to yield the features of phenomenal world according to the principles he lays out in his general physical theory.¹⁶⁵ As for the first result, we must see just what use Gassendi wants to make of his atomism in scientific explanations, and this is the matter I entertain next.

¹⁶⁵The other primary atomic features, size and shape, merit further discussion; in this context, however, suffice it to say that these cannot bring on difficulties of the same order as does atomic motion simply because there aren't principles governing size and shape at the heart of Gassendi's physics, save the notion that there is a physical minimal (which in any case is not a dynamical principle).

D. Gassendi's explanatory use of the hypothesis in the sciences

Gassendi puts his atomist hypothesis to many different uses in accounts of physical, chemical, biological, and geological phenomena. For example, as we saw in chapter two, his atomism is indispensable to his attempt to explain the results of barometric experiments. What this one case does not indicate, though, is the broad extent to which he believes an atomist approach to problems of physics and biology can provide mechanically viable solutions where none previously existed (whether by design, because mechanical solutions were thought inappropriate, or by happenstance, because no such solutions were successful to date). This use of atomism is particularly evident in his attempts to explain attractive forces such as magnetism and gravity, and the reproduction of organisms in plant and animal generation.¹⁶⁶

¹⁶⁶A thorough review of the full range of Gassendi's atomist scientific accounts will lead us too far astray from the central considerations of this inquiry. However, a sampling of his chemical or iatrochemical accounts is in order. In addition to his suggestion (discussed above) that the four elements are kinds of molecular aggregates, some of his proposals include:

i. *How vapors are created*: Liquids, like solids, are composed of atoms and void--only with more void separating the atoms. When heat atoms remove other atoms from a liquid, they further increase the distance between the remaining ones, and the result is atoms at a great remove from one another, or vapors (see Q I 398f; and following Epicurus, SEP 23).

ii. *The solubility of silver and gold as a product of atomic make-up*: Aqua regia dissolves gold, and aqua fortis dissolves silver, as a result of the fit of their respective atomic structures. Specifically, gold atoms fit the pores of aqua regia and silver atoms fit the pores of aqua fortis (see Q II 33f, 38-39).

iii. *An atomist theory of light*: Contrary to Descartes, light has nothing to do with pressure. Rather, it is a property carried by particular atoms (*atomi luciferae*) which are identical with heat atoms. These tend to travel at greater than average velocity because they generally have fewer obstacles in their paths (see Q I 422f).

Another physical theory of note is Gassendi's view that sound consists in special particles so that sound travels when those particles travel. In contrast to a typical sound-wave view, there is little room here for the role of the surrounding medium: Gassendi takes the velocity of sound particles, like that of light particles, to be invariant with respect to the air or wind in which they travel. In an experiment he models after a similar one of Mersenne's, he judges the velocity of sound to be 1,473 ft/sec (his calculations are off by 435 ft/sec) and invariant to the pitch of the sound (Q I 418-419). Bernier's report (III I XII, p 196) is a bit different, however; he

1. Attractive Forces

There is a wide range of putative action-at-a-distance phenomena for which Gassendi thinks there are viable mechanical explanations, and these include not only gravity and magnetism actions, but also those actions he says we typically attribute to occult properties like sympathy and antipathy:

Sympathy and antipathy...though they may strike us with a certain stupor, are subsumed under the general ways of action and endurance [*patiendi*] which rule all natural things. These ways entail that there are no effects without causes, no causes acting without movement, no causes acting on objects at a distance--that is, to which [such a cause] is not present itself or through a part [*organum*] that serves to conjoin them or transmit the object--that consequently nothing moves anything else except by touching it or by a corporeal part touching it. From which it follows that when we say two things mutually attract and attach to each other by sympathy, or repel and separate each other by antipathy, we must understand that these things occur as in other bodies, with no other sensible difference than what holds for the subtlety and coarseness of the parts.¹⁶⁷

This may be viewed as the hard case for divising materialist or mechanical explanations, given that Gassendi views sympathy and antipathy as plausibly having psychological or even social effects. By contrast, the attraction and attachment phenomena of gravity and magnetism should be easier for him to characterize in terms of interactions of atoms, since at least the objects of such powers of attraction (*vis attrahendi*) are always, and uncontroversially, composed of matter. In Section I, Book V of the *Physics*, he tries to explain both gravity and magnetism as the attraction of one

suggests that canons fired from the Place de Grève are heard at the Observatoire, a distance of 1260 toises or 7560 feet, seven seconds after being fired; thus the sound goes 180 toises/sec. or 1080 ft/sec., which is only off by 42 ft/sec.

¹⁶⁷Q I 450a.

body by another in virtue of atoms which emanate from the second and pull the first towards it. The atoms which bring about magnetism or gravity are of the same slight size as those which give living things the capacity for sensation, and through their like sensitivity, magnetic or gravitational atoms may 'retrieve' those objects which have separated from the Earth. Gassendi suggests a parallel with the molecular aggregates of biological significance, where we find that some collections of atoms are 'sensitive' in the sense that they play the role of 'seeds' for organic compounds (more on this in the following section):

It seems that there is in the magnet and the steel a force analogous to the senses; and that the cause of this is the attraction of which we have spoken and which resembles what animals experience....Like a sensible object, by the species or image it sends, turns towards it and attracts the soul which has the force to transport towards the sensible object a body of whatever thickness, so the magnet, in the way of the transmitted species, seems to turn towards it and attract the soul of the steel. We would believe this with difficulty if experiment would not certify it, that a thing as slender as is the sensible soul (whether it is as the flower of the substance or a very fine breath, or whatever you will agree to) would be capable of transporting the mass so heavy and so inert as that of a body. But then why not believe that there is in steel a soul or, certainly, at least something analogous to the soul? This something, however slender, can nevertheless transfer to the magnet the mass of steel, again, even if it is very heavy and inert.¹⁶⁸

Spink calls this a 'hylopsychistic' account, and so places Gassendi in the tradition of Maignan and Telesio.¹⁶⁹ Yet it is not entirely fair to think of these as 'psychist' views, for Gassendi never abandons his intention to develop mechanist accounts of magnetism and gravity. Thus, for example, what gives gravity or magnetism atoms their 'pull' is not

¹⁶⁸Q II 132.

¹⁶⁹John S. Spink, French Free-Thought from Gassendi to Voltaire (London: University of London, Athlone Press, 1960), 94.

a spiritual force but the combination of the right kinds of protrusions (like hooks)¹⁷⁰ and configurations which enable them to fasten to objects, plus the brute force of their internal impetus which leads them in one or another direction and in so doing drags along the objects to which they are fastened.¹⁷¹ The charge of psychism likely rests instead on Gassendi's attributing a soul, or minimally, 'something analogous to the soul', to inanimate objects like the steel attracted by a magnet. What then we are to make of this attribution? One promising lead here is that his talk about the 'flower of the substance' is reminiscent of his reference to atoms as the 'flower of matter', in which case he intends nothing spiritual, rather that the inherent mobility of matter's basic elements lends all material objects their capacities for motion and change. The suggestion that magnetism works by one body attracting the 'soul' or 'flower' of another body, accordingly, may just as well amount to the claim that such attractions are purely mechanical interactions of matter, where the magnetic matter is a bit more slender or subtle than what it attracts.

In the gravity account his mechanism is even more perspicuous. Gassendi's primary interest in developing this account is to rebut the Aristotelian notion that a falling body seeks its proper place, namely, the place where the Earth is. The 'proper' place, he asserts, is just wherever the body happens to be. Hence what a falling body 'seeks' is whatever body towards which it falls, where its motion in the case of bodies falling to

¹⁷⁰Q I 271.

¹⁷¹Partington (*History of Chemistry*, vol. 2, 465) compares this suggestion to Faraday's proposal that polarized particles constitute lines of attractive force in a dielectric medium. Of course, Gassendi as a thoroughgoing atomist could not accept a medium or field of force which, as electromagnetic theory developed, turned out not to be particulate.

Earth is contingently downwards but--as per his principle of inertia--otherwise akin to rectilinear motion in any other direction. The contingency in question is gravity, according to Gassendi, the cause of which is a dragging-down action of special particles.¹⁷² More specifically, rigid streams of atoms emanate from the Earth, upon impact with objects above the Earth these atoms somehow attach to those objects, and these streams then draw in the objects towards the Earth. Gravity weakens the further up one goes in the atmosphere, not because the strength of a force weakens over greater distances, but simply because fewer such streams of matter can reach the higher climes.¹⁷³ The mechanical details here, as in the magnetism account, are somewhat problematic. For one, it is strange to think that such special atoms should attach to their objects even though the solidity of atoms suggests we should expect them to bounce off, unless they feature some inherent adhesive quality. For another, it is not clear how those atoms, once attached, wouldn't continue to move (upward, in the gravity case) or stay in place with their attached objects.

Beyond the commonalities of the magnetism and gravity accounts, one *difference* between them is that the former is distinctly easier to harmonize with his views about the internal weight of atoms than is the latter. In the case of magnetism, we typically take the

¹⁷²Q I 346a.

¹⁷³Gassendi is convinced of the need for a mechanical and atomist account of gravity even before he has an adequate grasp of how gravitational phenomena work. Although in the *Physics* he comes to agree with Galileo's law of falling bodies (the law of odd numbers), he initially errs in *De Motu* by suggesting that the distances travelled by bodies in free fall over equal times are in equal proportions (that is, by consecutive integers and not squares of the times). To make up the slack which shows up experimentally he proposes in that earlier work that, in addition to gravity, a downward force impressed by atmospheric pressure--kickstarted by the attraction from corpuscular streams--is necessary for accelerating the motion of bodies in free fall.

attraction of a body to a magnet to entail outside causal influence, and this is evident when we measure the weight of the body before and after it is attracted by the magnet. Since the body weighs more afterwards though apparently none of its own properties have changed, it must be some external force which causes the increase, and the best candidate for this force is the additional element of the magnet. For Gassendi, such talk about gross external causal influence on the macro-level can be translated into a corresponding micro-level account that appeals only, or primarily, to the internal causal influence of atomic weight. In this instance, a weight change in the attracted body means that on the micro-level, atoms with their additional weight are super-added to the body, which is what his picture entails insofar as the special magnetic atoms attach themselves to the attracted body.

The gravity account, by contrast, cannot work this way according to Gassendi, who suggests there is no analogous increase in the weight of falling bodies because they move on their own and without any super-added weight.¹⁷⁴ It is hard to see exactly what he has in mind here since he takes gravity, like magnetism, to have an external causal influence on bodies. One might imagine that in the case of gravity he thinks there is directional influence only--but that's certainly not what's going on in the magnetism case. Another possibility is that he thinks gravity merely activates an already extant tendency of falling atoms to move--but this doesn't do justice to the dragging-down action of gravity which Gassendi proposes. In *De Motu* he actually advances the view that both an external gravitational force and the internal weight of falling bodies account for their

¹⁷⁴Q I 346-347.

increasing rates of acceleration, that is, that this rate increases because of a joint force comprising the dragging action on the part of the gravity-atoms and the inherent *pondus* of the bodies's constituent atoms. Later, by the time he writes his Syntagma account, he corrects his calculations of the acceleration rate and determines that one element of this joint force--the weight of the falling body--suffices for his explanation, which is now in keeping with the Galilean account.¹⁷⁵ Unfortunately, that still leaves Gassendi with his Syntagma account of gravity as a dragging-down action by special atoms, which seems superfluous to explaining the rate of acceleration in falling bodies. In short, his account suggests such a dragging action is somehow causally responsible for the downward motion of falling bodies at the same time that the inherent *pondus* of those bodies's atoms are causally responsible for the same motion. Once again, as with the principle of inertia, the inherent weight or motion of atoms appears to create more problems than it solves.

2. Generation

However difficult it might be, on the basis of Gassendi's dynamics and theory of matter, to explain away the appearance of action at a distance in magnetic or gravitational phenomena, it seems as though it should be a considerably more complex task to draw upon these elements of his physics in order to account for intricate biological or physiological systems where even the macro-level phenomena, much less the underlying structures, are opaque to experience. Gassendi doesn't seem to recognize this difference, though, and contentedly--almost facilely--suggests a wide range of corpuscular accounts

¹⁷⁵See two notes back. Clark documents this transition in Gassendi's account; see Joseph T. Clark, "Pierre Gassendi and the Physics of Galileo," Isis 54 (1963): 352-70.

of physiological and psychological behavior across a diverse range of organisms.¹⁷⁶ One biological account which we might think quite difficult to develop fully within the confines of Gassendi's atomism is his account of generation. In his Syntagma he includes two brief treatises -- De Plantis and De Generatione Animalium -- which demonstrate this interest, if not success, in addressing the emerging study of generation from a broadly materialist and specifically atomist viewpoint.

Gassendi holds that generation may be either spontaneous, from elements in the

¹⁷⁶Some atomist accounts of biological and psychological phenomena include:

i. Shellfish grow and marrow production increases during full moons because of increased humidity produced by the moon. This is possible because lunar moisture corpuscles are excited by sunlight and then transported to the Earth by rays from the sun reflected off the moon (Q I 450a-451a).

ii. Gassendi echoes Lucretius's suggestion that lions avoid cockcrows because the crows emit corpuscles which harm the lions (Q I 453b-454a). Sheep avoid unfamiliar wolves because wolves emit corpuscles of an odor offensive to sheep (Q I 456a). Although he distinguishes the shock of people at the sight of wolves, familiar or otherwise, as the product of simple fear (Q I 451b), his human psychology is also atomist (at least this end of it), and so he is surely committed to some sort of underlying atomist story about such fear in persons, too.

iii. Electric rays (torpedos) stun persons because of dulling corpuscles they emit (Q I 455a). Natural poisons typically enter victim organisms by seeping through their pores, and what makes this possible is the tiny dimensions of their constituent particles. If those poisons can be cured by contact with elements of the attacking organism's body (e.g. scorpion or dog) it is because those particles can be reabsorbed by the attacker's body (Q I 455b). Poisons which provoke strange behavior may work by producing activity in the victim sympathetic to activity of the attacker, and this may be explained as chemical alterations the poisons bring about in the victim's sensory capacities and attunements. Thus people who are bitten by tarantulas dance around like tarantulas because poison particles passing through their systems recalibrate their hearing such that their physical responses to audio stimuli are similar to those of tarantulas--the victims are led by altered chemistry to move rhythmically with their attackers (Q I 456a)!

Relative to psychology, Gassendi rejects out of hand Lactantius's complaint that cognitive attributes like the 'senses, thought, memory, the mind, genius, [or] reason' cannot be built out of or be produced by atoms (Lactantius lodges this complaint because he is unhappy about the materialism of the classical atomist scenario). A viable atomist story, he maintains, may account for the senses and their functions though not the mind and reason (Q I 282; B 408). Thus, for example, we can explain memory loss as a physical change in the brain resulting from undernourishment; folds in brain matter preserve the physical embodiment of memory, but a deficiency in nutrition leads to the deterioration of those folds (Q II 406-407). This is very close, by the way, to Descartes's view in The Treatise on Man (AT XI 177-178; Alquié I 451-452).

earth, or else non-spontaneous or pre-organized, from parent organisms. In plant generation, both types entail the development of a new organism from a single unfertilized seed which bears the soul of the new plant. Such souls are composed of a spirit-like substance which directs the division, differentiation, and development of the material corpuscles in the growing seed; this ontogenetic pathway produces the same results regardless of whether the seeds arise by spontaneous or pre-organized generation (from here on, SG and PG). Two distinct processes may yield similar results because seeds composed of elements either from the earth or from other plants may share an identical corpuscular composition:

...nothing forbids our saying that atoms or corpuscles were created by God in the beginning and given a certain mass, shape, and motion, and that while these are being variously moved, and while they are meeting, interweaving, mingling, unrolling, uniting, and being fitted together, molecules or small structures similar to molecules are created, from which the actual seeds are constructed and fashioned within the plant. In other words, the corpuscles which change into a seed within a plant are also attracted from the earth itself, and the only difference is that the fashioning of seeds can be more easily accomplished within a plant because of a more select supply of corpuscles or similar principles has already been produced and is now gathering....¹⁷⁷

Gassendi does not tell us, for either sort of generation, precisely how the right sorts of matter come together in just the appropriate configuration, nor does he specify the subsequent behavior of any plant seeds so configured. However, following his general molecular views, it is reasonable to surmise that the seeds giving rise to plants are formed from just the right combinations of only those atoms which may compose vegetative matter. If this is so, then while seeds which emerge from SG and PG might comprise the

¹⁷⁷De Generatione Plantis, GPA 798.

same atomic combinations, only the occurrence of SG would depend on the chances that requisite but otherwise independent components are sufficiently abundant at the right place and time, and that they tend towards the proper configurations. So whatever else Gassendi says, PG *should* occur with greater ease because the proper atomic mix for generation is already present within existing plants.

Though he may gloss over that particular distinction, Gassendi is clearly aware that PG accounts are more useful for generating biologically significant explanations than are SG accounts. Generally speaking, one advantage to PG accounts over SG accounts is that only the former can tell us how a particular plant has a given set of traits as a consequence of any other plant having the same set. In other words, PG accounts can tell us about relations between individual plants and about inheritance phenomena, whereas SG accounts are rather more limited in what they can tell us about how a set of traits appears in a given plant. In Gassendi's picture, we see this distinction in that the seeds of PG alone, as the product of parent organisms, bear special vehicles--miniature souls--which enable them to transmit their characteristics across generations and so allow for inheritance. He suggests the contribution of the parent organism to the characteristics of the spawn is a function of the relation between two material souls: a soul which is spread thinly throughout a plant's structure and which generally animates it, and a miniature soul or *animula* in the seed which is part of the larger soul until the seed falls from the plant. The role of the animula is to transmit ontogenetic information from parent to new plant, and it does this by receiving, in concentrated form, 'ideas' and 'impressions' from all parts of the soul of the parent plant, and communicating such information to the new organism

which the seed produces. One particularly interesting suggestion here is his notion that the animula is partly 'transfused' from the root and accordingly may mimic the parent soul's directions and guidelines for development.¹⁷⁸

By contrast, Gassendi never suggests any analogous structure or process in SG seeds, and it is hard to see how they could acquire anything like the same soul or animula of PG seeds. Yet even if there were an animula in SG seeds, this miniature soul would of course have no relations to the soul of any parent plant because there is no parent. While there are no such sources of directions, 'ideas', and 'impressions', Gassendi supposes anyway that SG seeds somehow also bear a similar sort of developmental or organizational information. Again, what makes this is possible--and what also makes it possible that the two modes of generation yield roughly equivalent products--is that the atomic compositions of each type of seed do not differ:

Hence when we have conceded that the seeds are equal, equal functions necessarily ensue from the equal dispositions we have assumed, that equal stamina are formed, and that equal plants arise from them...¹⁷⁹

There is a hitch here: the seeds could not be wholly equal, since the souls we find in PG seeds are related to their parent-souls and this guarantees some continuity across individuals whereas nothing in SG does. One might claim that Gassendi believes that "equal plants arise from them" because, for example, he thinks the souls of PG seeds make a difference for trait-inheritance but not for generation *per se*. Equivalent

¹⁷⁸Lest we lose sight of the fact that Gassendi is talking about a *material* soul of plants, here he is proposing that a portion (that is, an identifiable *chunk*) of the parent's soul is responsible for development and is more pronounced in some parts of the organism than others.

¹⁷⁹De Generatione Plantis, GPA 801.

characteristics may arise in plants whether or not they resemble those among members of previous generations, according to this view. But then on grounds of parsimony it is hard to see why he would introduce a distinct mechanism for trait-inheritance altogether. In contrast to animal generation, Gassendi thinks plant generation is a relatively simple affair, and so perhaps in a tidier version of his theory SG alone might have sufficed. Having brought in animula, though, it seems he is concerned with two very different generative processes despite any protests to the contrary. Thus while it is fair enough for him to conclude that seeds involved in the two processes *may* have ontogenetic pathways with equivalent outcomes because he thinks they have common atomic compositions, it is hard to see why he thinks this commonality alone *guarantees* such an equivalence.

Animal generation also features animulae, in semen, and here one parent may suffice (in SG, such that the semen are created in the moment of generation) or else two parents may be required (in PG, such that the semen meet in the moment of generation). In either case, the semen contain complete though folded-up and rudimentary forms of the animal offspring, and so in animal generation, like plant generation, new souls contain a plan for development. What is different, though, is the preformationist idea that the mature physical characteristics of new individuals are somehow contained in premature forms found in the semen. In Gassendi's view, at the moment of the semen's creation, its tissue matter is differentiated into familiar body parts of the mature fetus. He writes:

...the semen both is manifestly heterogeneous and consists of the same parts from which gradually and in one series the organic parts will be perfected....¹⁸⁰

¹⁸⁰De Generatione Animalium, GPA 815.

An unusual aspect of this preformationism is his proposal that the semen of each parent, and not the embryo created by both, contains its own new organism in miniature. As I suggest below, this creates the puzzle of saying which semen the offspring will resemble, and in what respects.

Another distinctive feature of this account is that Gassendi thinks that animal generation, much more than plant generation, requires sex to ensure that offspring receive the requisite matter and genetic information. Following a popular view among biologists of his day, he suggests sexual generation involves the meeting of two seminal fluids and this meeting is solely responsible for the creation of new fetuses (in PG). The two semen contribute to the material tissue (equally) and the animula-soul (less than equally, it turns out) of the new fetus. Since this soul is responsible for the development of traits in new individuals, combinations of souls from two semen in sexual reproduction account for the mixture of traits in offspring.

The principle problem of explaining animal generation, in Gassendi's view, is saying how dominance is achieved among inherited traits, and what characteristics of generation account for dominance relations. We might expect him, as a preformationist and mechanist, to say dominance results from physical interactions of tissues contributed by each parent's semen to the embryo. One such scenario would have it that bits of tissue from the two semen destroy, or merge or connect with one another so as to fashion particular mixtures of traits. Yet Gassendi thinks such physical interactions cannot account for dominance. He considers the possibility that characteristics carried by each semen are joined in the fertilized embryo, and among those characteristics is the sex of

the offspring; in this case, he suggests, we should expect that the male child of two parents would exactly resemble the father, and the female child would exactly resemble the mother. Here he assumes that all the characteristics carried by a particular semen would be linked -- and that physical interaction of the two semen would be such that the semen of one parent would contribute all the tissue material of the new individual whereas that of the other parent would contribute none.¹⁸¹ However, male children sometimes resemble their mothers and female children sometimes resemble their fathers, and Gassendi reasons from this that the key cause of dominance of traits cannot be the utter domination of all the physical bits from semen over those of the other. Further, he draws the general conclusion--rather unjustifiedly--that trait dominance cannot be decided at all by interactions among the tissue matter of the distinct semen.

Instead, he proposes that the key cause of dominance must be sets of *impressions about traits* held by the parents. The offspring inherits one set of traits instead of another--the father's over the mother's or vice-versa--because that set develops from a bundle of dominant impressions in the fetus. In this much there are some broad parallels with our contemporary account of genetic dominance: for each offspring, there are two potential sources of developmental information, so there is a surplus of both information sources and information, and choices are made by some form of struggle between the two sources or carriers of developmental information. Gassendi's story of this struggle is of course

¹⁸¹This is a strange line of argument for Gassendi, for as we have seen, he thinks each semen only contributes tissue matter to the fertilized embryo whereas the development of particular characteristics in the new individual is directed by an animula-soul. So it seems he should have allowed that, in the merger of under-differentiated tissue matter from each semen, there could be physical interactions of this matter which play a role in deciding which traits are dominant.

quite different from our own, though it is potentially even more complex. He suggests that offspring feature more traits of the father if the soul of the mother contains more impressions of the father than the soul of the father contains impressions of the mother, and more traits of the mother if the situation is reversed. Each animula contains a 'precis' of the corresponding parent soul's information consisting of 'ideas' and 'impressions', and the collections of such information from the two animulae vie to direct the development of the fetus.¹⁸² Gassendi does not suggest the nature of their struggle: how can different ideas or impressions combat one another? What is particularly puzzling in this regard is that he does not indicate whether he considers such ideas or impressions to be material in the first place. The animulae, as animal souls, are material, but it doesn't follow from this that the bits of information they carry are as well. On the other hand, it is at least plausible that Gassendi thinks our genetic information is carried by material impressions and ideas, just as he holds that material folds and grooves in the brain are vestiges of neural activity from which we form memories. But without a clear picture of what these ideas and impressions are supposed to be, much less how they might interact, it is difficult to grasp exactly how he proposes to explain the dominance we find among inherited traits.¹⁸³

¹⁸²In fact, according to Gassendi, there are other parties besides the parents who may be ideally represented in the animulae: if one parent's impressions of, for example, a statue, is stronger than whatever the other parent's impressions, then the offspring will develop traits which match the impressions of the statue.

¹⁸³Martin Tamny has suggested to me that the proposal that different perceptions of parents can affect the physical characteristics of their offspring at first glance appears to suggest that non-material perceptions, through their resulting impressions, can impact upon physical objects (in this case, the developing fetus). Yet Gassendi's mechanical account of perception (and other cognitive faculties) at least in principle should allow him to hold an account of corporeal change

Like a number of other early modern philosophers—including Descartes and Malebranche—Gassendi has an account of animal generation which straddles standard categories of biological mechanism, vitalism, and preformationism. His view is mechanist in the sense that he believes the development of new individuals results from interactions among the atomic amalgams which constitute the animulae and tissue of the two semen. On the other hand, his view is at least reminiscent of vitalism, in the sense that he thinks there is a life-force characteristic of only those aggregates of atoms which constitute semen (or seeds, in the case of plant generation). What makes this look like vitalism is that this life-force, the animulae, enable semen or seeds to direct ontogenesis simply on the basis of what they contain at the inception of the new individual, that is, without the benefit of interactions with any other external matter. Naturally, this cannot really be vitalism since his account has it that all the relevant players are composed of atoms, not spirit-stuff, and in any case there is nothing truly emergent about this life-force—it is more like a bud to the tree of the parent organism's soul. Finally, his account is preformationist in the sense that he holds that plant seeds and animal semen contain in miniature, undeveloped form some or most of the actual physical characteristics of the maturing embryo, and ultimately, of the new individual.

resulting from mental impressions which is nonetheless consistent with his materialist mechanical philosophy. This materialism cannot easily be reconciled with his additional proposal that there is a second, immaterial mind which is the seat of such higher-order cognitive capacities as abstract reasoning—but this is a distinct problem (addressed by, among others, Bloch ([La Philosophie de Gassendi](#)) and Emily Michael and Fred S. Michael in their "Gassendi on sensation and reflection: a non-cartesian dualism," [History of European Ideas](#) 9 (1988): 583-95, and "Two early modern concepts of mind: reflecting substance vs. thinking substance," [Journal of the History of Philosophy](#) 27 (1989): 29-48.

How well do these aspects of his account fit together? His mechanism would be at odds with his talk of miniature souls if they were composed of soul-stuff, but they are not, so it is hard to see any conflict here--which is what makes it unreasonable to think of Gassendi as a true vitalist in any sense. These same views about animulae would also sit poorly with his preformationism if all ontogenesis required is that the preformed tissue matter of seeds and semen naturally unfolds in the course of generation and development: this would make the organizing principle of the soul extraneous. Yet here, too, Gassendi's views are actually compatible, given that we assign the role of physical unfolding to semen or seed, and the role of developmental direction to the souls of semen or seed. The resulting picture has it that each seed and semen contains tissue which expands and becomes differentiated, plus a soul which directs the expansion, differentiation, and management of the different bits of matter. Indeed, Gassendi suggests the task of the soul is to

...apply the given parts to parts...by replacing them in the position and in the arrangement with respect to one another...they...form a complete diminutive body.¹⁸⁴

Thus Gassendi bridges this span of views by maintaining a steadfastly materialist picture of the principle agents of generation, all the while distinguishing between tissue matter which has the potential for developing into a mature new individual, and a material soul which lays out the plan for this development. It is not clear precisely how these different bits of matter must be related for generation to take place, and in the end, all Gassendi offers us is a gloss on what a viable account should look like. What is noteworthy about

¹⁸⁴De Generatione Animalium, GPA 810.

this gloss, however, is his insistence that any such account is viable only if it is told in terms of the interactions of the smallest bits of matter out of which the whole world, organic stuff or otherwise, is composed.

3. Some Lessons

What do these case studies tell us about Gassendi's programme for atomist scientific explanation? For one, it seems he intends to employ his atomist hypothesis in a way that promotes mechanical explanation, at least in the sense that his physical accounts require that the atoms behave according to the relevant dynamical or kinematic laws.¹⁸⁵ In particular, and Gassendi's noninertial characterization of atomic motion notwithstanding, it seems his proposed magnetic or gravitational atoms should move inertially. Consider his report and explanation of the barometry experiments conducted by Pascal's brother-in-law at Puy-de-Dôme (which I describe in chapter two), where Gassendi proposes that the column of mercury in the experimental device rises at lower elevations because of greater atmospheric pressure--which he thinks is a consequence of greater gravitational force acting on that portion of atmospheric air closer to sea level. Again, according to his gravity account, the overall downward pressure in what Pascal calls the 'sea of air' results ultimately from the pressure exerted by individual air particles which are dragged downwards by the force of gravitational atoms. Once the particles of air attach to the gravitational atoms, though, they no longer move as a result of their internal impetus but instead as a result of interactions with other bodies, namely, the

¹⁸⁵Below, I say why we should take Gassendi's law(s) of motion to be dynamical, and not merely kinematic.

atoms dragging them downward. If they should behave noninertially at this point, for example, under the causal influence of their internal impetuses, it is hard to see that gravity in Gassendi's terms would necessarily have the expected effect.

Further, consider those cases where it is apparent that gravitational forces accelerate the motion of a body, as when it is rolling down an inclined plane. Gassendi cannot explain this as a change in the features of the body's constituent atoms because he thinks the weight (and so the motion) of atoms does not change under any circumstances. What he could say, given the constraints of his account, is that acceleration occurs because of the reeling-in action performed by gravitational or magnetic chains of atoms. But this scenario presupposes that atoms as well as supra-atomic bodies can pick up speed after all; as the amalgam of a body and those gravitational or magnetic atoms attached to it rolls downward, each of these elements must be gaining speed *individually* since the rolling body never gains new constituent (gravitational or magnetic) atoms, it simply attaches to them.¹⁸⁶ Conversely, it seems that under the influence of countervailing gravitational or magnetic forces, individual atoms should be able to decelerate, and perhaps lose all velocity, too. Thus it appears that Gassendi cannot help but characterize some atomic motion as inertial--despite his stated views to the contrary--in order to consistently maintain his corpuscular accounts of magnetism and gravity. This adherence to general dynamical or kinematic principles such as those governing the

¹⁸⁶In the case of magnetism there could be no such gain because, if there was, then the force of the attraction from the originating source of the magnetism corpuscles would be correspondingly diminished--which would tend to prevent magnetic attractions underway from coming to fruition. Nor in the case of gravity could there be such a gain because, if there was, then there would be an heightened resistance to acceleration from the newly-augmented mass of the body.

inertial motion of bodies suggests that, whatever else he proposes in the way of noninertial atomic behavior, he is actually quite concerned to pursue his atomist programme for explanation along mechanist lines.¹⁸⁷

Another point these case studies suggest is that Gassendi is less interested in the empirical viability of his atomist scientific explanations than his global empiricism might suggest, for though he may have thought there are feasible tests of the consequences of such explanations, he never says what these should look like. His contact action accounts *should* bear out if there are weight changes which correspond to the magnetic and gravitational phenomena we observe, but these are generally not changes which he could have expected to be able to measure. And we could put his generation account to the test by gauging the effect of sustained perceptual activity by parent organisms on the physical characteristics of their offspring. Nevertheless, Gassendi is not so concerned with such tests that he bothers to describe any. We might say he has let his role as a clarion for empiricism lapse here, but it seems equally if not more likely that he just does not think these need be empirically demonstrated accounts of such phenomena. What concerns him most, rather, is simply showing that we can generate merely plausible explanations of these and a wide range of other natural phenomena if we assume matter is ultimately composed of atoms which interact mechanically so as to produce perceptually-accessible

¹⁸⁷This adherence is also further evidence that Gassendi intends his physics to be scalar invariant, for there would be no reason to think atoms should move inertially to begin with unless we took the relevant principle to govern the motion of all bodies independent of their size.

phenomena.¹⁸⁸

Finally, these case studies suggest Gassendi fails to design atomist accounts of macro-level phenomena which escape the charge of *obscurum per obscurius* or 'explaining the mysterious by the equally mysterious'. The promise of his atomist ontology is that the special qualities he attributes to atoms or their aggregates, such as the power of attraction or the capacity for bearing developmental information, should turn out to be more or less plausible as vehicles for mechanically effecting those phenomena on the macro-level he seeks to explain (though the particular ontogenetic capacities he ascribes to seed or semen molecules seem likely to end up on the distinctly less plausible side). But these qualities of atoms or their aggregates cannot truly deepen our understanding of the macro-level phenomena, because Gassendi simply states that some atoms or their aggregates bear such qualities while others do not, and provides us with no clue as to how or why the micro-sized bodies come to have them. This is precisely the charge that Boyle later on brings against the peripatetics and other iatrochemists, and of which he is even cautious relative to the corpuscularian hypothesis.

How fair is this charge? Let us first consider the most fundamental of atomic qualities. In some of Gassendi's historical remarks he echoes the ancient view that outside of size, shape, and position, there aren't any differences among atoms, and from this standpoint it indeed makes no sense to start looking for any such distinctive fundamental features which would yield the special qualities in question. Perhaps he

¹⁸⁸I return to this point in my concluding chapter. Specifically, I address whether these accounts even initially plausible, by Gassendi's standards.

deemed it unlikely in the first place that those special qualities could arise from size, shape, or position alone; this would help to explain why he subsequently proposes that atoms may differ so much among themselves that the four elements may be composed of distinct sorts of atoms and, as I have argued above, that the quantity of motion in individual atoms may be variable as well. This might make a promising start for explaining the presence of such complex properties of atoms or their aggregates as attraction or bearing ontogenetic information--except that in such instances as the four elemental kinds he also fails to say how or why particular atoms have the qualities he assigns them. One possible response, inspired by theological concerns, is that this is just the way God made such-and-so atoms. Yet it is not clear that this is the route Gassendi wants to pursue, and his method of hypothesis supports the equally feasible suggestion that these accounts are merely plausible proposals for saying how an ontology encompassing only atoms and void could yield so many aspects of the macro-level world, be they phenomenal (as for example, gravity, or animal generation) or theoretical (as for example, the four elements, relative to his iatrochemistry). Then, if Gassendi were right about the particular qualities he assigns to atoms and their aggregates, he would have at least identified some of the basic structural characteristics underlying our macro-level world. The problem is that he still would not have told us how those characteristics came to be features of the atoms which bear them, nor why they should be the basic ones as opposed to any others, and if only in this respect he earns Boyle's criticisms of those chemical and iatrochemical theories which bestow us with one set of mysteries in lieu of another.

E. Conclusion: Gassendi's Atomist Programme, the Mechanical Philosophy, and Empirical Viability

I close this chapter by addressing two issues I began with: how does Gassendi's atomism fit into his broader mechanical philosophy, and what is the empirical viability of his atomist hypothesis? First, however, I briefly examine an issue which has come to dominate much of the literature on his atomism, namely, whether it has any novel character. One prominent view is that it differs in relatively insignificant ways from the atomism of the ancients, and numerous interpreters suggest the notable differences are those which Gassendi himself stresses: the rejection of certain ancient claims on theological and/or doctrinal grounds, or what Margaret Osler has called the 'baptism' of Epicurean views. Yet, as I have tried to suggest, Gassendi also distinguishes his atomism from its ancient sources by more detailed accounts of macrophysical phenomena such as the forces acting upon, and the manifest qualities of, macrophysical objects. His atomist explanations of gravity and air pressure, for example, have no real forerunners among the ancient atomist accounts.

It is also pertinent in this context to point out that Gassendi rejects some of Epicurus's views not simply on doctrinal grounds but in order to avoid untoward results for his physics or (theologically-influenced though not necessarily doctrinal) cosmology. Epicurus, for example suggests there are infinitely many atoms in infinitely great space forever existing across infinite time, and Gassendi surely (as is oft-reported) rejects this on doctrinal grounds--Bruno, afterall, was burned at the stake for a similar view. But Gassendi also cannot accept this Epicurean view because he thinks that if there were

infinitely many atoms, then things could be as they are naturally and without any need for God's providence--since eventually infinitely many atoms acting on their own could yield the structures and relations familiar to us. Thus it is to preserve not doctrine but God's ultimate role in determining the outcome of bodily interactions that Gassendi rules out the possibility of these claims of an infinite universe and the infinitely many atoms that might inhabit it.¹⁸⁹

Completely apart from modifying Epicurean claims, Gassendi crafts a novel atomism insofar as he tailors his views specifically to counter the competing matter theorists of his time, and in particular the premier non-atomist corpuscularian, Descartes. The part of cartesian matter theory which is perhaps most unacceptable to Gassendi is the plenist view that matter equals extension such that there is no space not occupied by matter:

In our times appears the famous Descartes, who, considering the World as being neither finite, nor infinite, but indefinite, as well in fact for Matter, which in the beginning occupied the entirety of space or, rather, was itself space (because he does not distinguish space from bodily extension, since as well he admits no void at all), then would have been fragmented by God in a way to be resolved [as in resolution] into tiny particles comparable to Empedocles' small fragments or Heraclitus' bits.¹⁹⁰

On the other hand, whether by charity or ignorance, Gassendi is less concerned with what marks cartesian corpuscles as different from his own. In particular, he insists that the cartesian sort, though they are not atoms, share the same mechanisms and features as

¹⁸⁹Cf. his letters to the Count of Alais of October 24 and November 7, 1642 (Q VI 158a, 159a). One unfortunate result of this view, by the way, is that the argument from design--if not for God's existence, then at least for God's job description--requires a very specific design.

¹⁹⁰Q I 257.

Epicurean atoms:

...for these large bodies as well as for the smallest bodies found in nature, for all their qualities and for all the set of phenomena, he requires nothing else in these fragmentary particles that what Epicurus requires of his atoms, nothing other than Shape and Movement under three small dimensions, with an appropriate position and order.....¹⁹¹

Indeed, Gassendi suggests in this context that the *only* feature of these corpuscles which distinguishes them from atoms is their further divisibility:

He refuses to call these little fragments atoms as well, because they would themselves be divisible, and this neither in a limited manner nor to infinity, but indefinitely, to what he *pretends*...¹⁹²

To be sure, the presence or lack of a limit to matter's divisibility is *the* defining issue between atomist and anti-atomist corpuscularians. Hence it is not surprising that Gassendi focuses on the cartesian proposal that there is no reason to think we cannot divide matter infinitely; he responds as follows:

And for the rest, I ask you, whilst from such division and subdivision to infinity more and more parts are actually revealed, do you think that those that can be revealed are of some determinate number, or not? If you say that they are, there won't be a sufficient number to divide them to infinity. If [you say that] there aren't, then there will actually be an infinite number of them. And certainly, how may the continuum not be exhausted, if they do not actually possess infinite parts, or indeed parts which, being infinite, render it [the continuum] inexhaustible? And, in fact, the parts that come from it by subtraction, do they become actually preexisting in it; otherwise, how could they have been subtracted from it? Thus those that remained from the subtraction, actually came to be, because otherwise they could not have been subtracted from it.¹⁹³

The merits of this response to Descartes aside, I highlight this passage simply to

¹⁹¹Q I 257.

¹⁹²Q I 257.

¹⁹³AN 218a.

illustrate Gassendi's move beyond classical atomist arguments to independent reasons for thinking there are limits on matter's divisibility. This is not to suggest that he offers a thorough rebuttal of cartesian anti-atomism; he glosses over such significant differences as the capacity for motion of those corpuscles he and Descartes posit and the overall nature of that motion and its conservation. All the same, his concern with and novel contributions to a debate over a paramount distinction among corpuscularians count as further evidence against the canard that his atomism is purely a 'baptized' Epicureanism. Rather, Gassendi is acutely aware of the competing contemporary as well as historically significant matter theories and, as I have suggested above, he intends his atomist hypothesis not so much as an historically-validated or historically-competitive thesis (as, for example, Lynn Joy has proposed), as a way of characterizing the physical world in keeping with contemporary scientific standards.

One element of those standards, in Gassendi's view, is a broad commitment to the new and widely-promulgated mechanical philosophy, and his atomism is surely consonant with several mechanist views--most obviously the ontological thesis (some form of corpuscularianism) and a methodological thesis governing the explanation of sensible qualities (by appeal to the qualities and states of micro-sized bodies). But as I have suggested, there are also several aspects of the mechanical philosophy with which Gassendi's atomism is not consistent, and these include a meta-principle of scalar invariance, according to which our most general physical principles should describe the character and behavior of atomic and supra-atomic bodies alike. I have already mentioned in the beginning of this chapter some reasons why Gassendi should hold this

meta-principle. As a consequence of some of his central atomist claims, there is one more reason: if he did not, then a number of paradoxical results arise in some cases where the supra-atomic bodies in question are merely small molecules. For example, let us suppose that he does not intend his laws of motion to be scalar invariant, so that supra-atomic bodies obey the principle of inertia while atoms do not. Then, as per his atomist hypothesis, the motion of atoms is unceasing and at a fixed rate, such that they can neither lose nor gain motion. However, since bodies which comprise multiple atoms may accelerate or decelerate, we get the perplexing result that two single atoms may travel only at a fixed rate, unless they happen to collide and adhere to one another, in which case their joint motion may either accelerate or decelerate. Even more perplexing is the scenario where two single atoms collide but do not adhere to one another: they must continue on at their original fixed rates. And consider the case where two atoms are conjoined and travelling at velocity v . If they should come apart (spontaneously, for example), then the two parts of what was formerly an aggregate will split the measure of their conjoined weight (nothing wrong here) --and, accordingly, also their conjoined velocity. That is, one should continue to travel at some other velocity, x and the other at $v-x$. But that's simply wrong,¹⁹⁴ and paradoxes such as these suggest that Gassendi should

¹⁹⁴Gassendi actually has a ready response to this last case: since heavier and lighter weights do not differ greatly in their rates of free fall, he says, it must be that velocity is not covariant with weight, such that all atoms (no matter their other characteristics) will travel at the same, uniform rate. Hence, he might well suggest, this sort of case should not arise. It's not clear what is most incorrect about this response, but two candidates stand out: first, as Gassendi is aware, there is a slight and even measurable difference in rates of objects in free fall, and while it does not tell us much about their relative weights, nor should it tell us anything about rates of atomic motion; and second, if weight *just is* the tendency to motion then the measures of the two had better be covariant.

have accepted scalar invariant laws of motion. Of course, were he to say so he would need to revise his view that atoms behave noninertially.

Another, broader tenet of the mechanical philosophy (or so I have argued) is a commitment to dynamical or, minimally, kinematic, principles. Gassendi's atomism at least conforms with the aims of this tenet even if he fails in the end to lay out viable principles governing the motion of atoms. It seems to me that, owing to the idiosyncrasies of his atomism, Gassendi is peculiarly successful among the earlier mechanical philosophers in suggesting a dynamical characterization of bodies in motion, albeit a characterization which he does not fully enunciate and which in any case would be irreparably flawed. One of the striking features of Gassendi's atoms, in contrast to corpuscles posited by numerous other mechanical philosophers, is that they are not inert, and it is a striking consequence of this view that he is compelled to talk about the motion of atoms in terms that extend beyond mere geometric characterization (as is the case, for example, in Descartes's characterization of bodies in motion). Thus, beyond the physical notions of matter in motion entailed by such kinematic principles as that describing inertial behavior, his atomism entails notions of mass and force. Force (*vis*) is the product of atomic weight, and the force of more than one atom joined together collectively yields the motive character of supra-atomic bodies. And at least on the atomic level, mass (which Gassendi mistakenly conflates with weight) just is motion or the tendency to the same, so (in principle) it should be the case that those principles governing motion can be somehow rewritten as laws we would deem part of a dynamics. Yet this would surely have disastrous results; as I suggest above, there is no coherent way

to transform a standard principle of inertia in this manner if we suppose the mass in question to be an inherent and unremovable feature of bodies we would otherwise expect to behave inertially. Nonetheless, it remains a mark of distinction that Gassendi's physics, broadly considered, attributes force to bodies as a product of their mass and can thereby accomodate some dynamical characterization of matter in motion, albeit not a very serviceable one. This aspect of his science of motion contrasts with much of the physics of his day, which tends to be guided by the notion that force is the province of God or, as Gary Hatfield has suggested of the cartesian view, God himself. Koyré and others have proposed, quite correctly, that Gassendi's physics is all the poorer for his lack of mathematical sophistication; for all that, however, his conceptual grasp of what physical accounts may tell us seems to have been richer than that of other early mechanical philosophers including Descartes (though not, it seems, Galileo).

Empiricist constraints constitute another element of those scientific standards to which Gassendi believes atomism (and any other physical claims) must be held. Whatever we know of the physical world is known only through the senses, and such is knowledge of appearances and not of the intimate or interior nature of things (cf., for example, DM 186-9). Yet time and again Gassendi draws on *apriori* reasoning and outright historically-based arguments to defend many of his atomist claims. Thus: atoms which constitute liquids must have few edges so that they may flow freely, and all material objects must consist of smaller constituent elements because--as the ancients argue--nothing comes from nothing. How does he try to reconcile such reasoning, and such claims about the unperceived, with his resolute empiricism? Among the

considerations here are whether he actually suggests we come to atomist claims on the basis of *apriori* reasoning, what evidence experiments and observation generally--or microscopy in particular--might provide of the subvisible, and the role his theory of signs plays in supporting the claim that the perceivable gives us reliable insights into the unperceivable.

With respect to whether Gassendi intends that we arrive at his principle atomist claims through *apriori* means, the answer must be a qualified 'yes'. He surely presents such claims without the benefit of experiential knowledge yet he just as surely leaves ample room for experiment and observation as a means of empirically grounding further results and putting the core claims of his atomism to the test. By way of comparison, we may recall Patrick Suppes's pointed analysis (1954) of Descartes's claim that there is no action-at-a-distance. Suppes suggests that Descartes holds this view as a consequence of his *apriori* 'laws of nature' (including his principles of motion) which all prohibit change in the state of a body by any other means than collision. Additionally, force (for Descartes, the cause of motion in bodies) is inertial only, such that bodies may act only on, or resist the actions of, other bodies by impact alone. Thus, Suppes concludes, contact action is built into cartesian physics as an *apriori* constraint imposed by his more general metaphysical principles. It follows that experience might confirm though never disconfirm Descartes's views on this matter.

In contrast, relative to this very matter of contact action, Gassendi does not follow Descartes's path though their perspectives are quite similar in the end. First, it is not Gassendi's inertial principle which guarantees contact action so much as his atomism

which, we have seen, is at least formulated independently of the principle of inertia, and may as well be inconsistent with it.¹⁹⁵ Second, the way atomism guarantees contact action is not a matter of metaphysics *per se*: Gassendi holds that the motions of aggregate bodies are simply the aggregates of atomic motions (the void is inert) and there is only contact action among atoms, so nothing else could account for change among bodies of any size. Clearly such views about the nature of matter and motion are introduced and defended here on the basis of reason alone and, in other cases, even borrowed freely in the spirit of a global Epicureanism. But no background metaphysical principles prevent us from suggesting on the basis of empirical evidence that any of these claims is wrong. And so, unlike Descartes--or at least Descartes according to Suppes--it appears that Gassendi thinks of his physical claims (generally speaking) and atomist claims (specifically) as hypotheses which are in principle testable by, if not initially derived from, experience. We can even find some of his atomist accounts which at least hint at experiential foundations--as when he tries to explain the Earth's attractive powers by drawing analogies with what he takes to be the contact-action behavior of magnets and their corpuscular emissions.¹⁹⁶

Nonetheless, against a background of introducing the better part of his atomist programme on an *a priori* basis, Gassendi then adduces a fair amount of what he thinks of

¹⁹⁵Insofar as he introduces a principle of inertia on the basis of a thought experiment with some element of appeal to experience, it also seems he does not see that principle as Descartes does--that is, as either ineluctably a matter determined by reason instead of experience, or as deduced from such higher-order principles as the immutability of God.

¹⁹⁶Q II 128-135 (Book III Chapter 5).

as experimental and observational evidence for certain elements of that programme. But what evidence from experiment or observation might count as empirical support for claims about atoms? One classic strategy in arguments for atomism, echoed in the seventeenth century, consisted in proving the existence of the void in order to demonstrate the discrete structure of matter. Along these lines, Gassendi proposes several experiments in the Animadversiones and the Syntagma--first suggested by Hero of Alexandria--to produce evidence of the void.¹⁹⁷ One principal experiment entails the use of a pneumatic bombard to compress the volume of a set number of air particles, such that those particles have nowhere to go save the previously unoccupied space of interstitial void.¹⁹⁸ Assuming such experiments demonstrate the existence of the void, though, what does this show about the existence or character of atoms?

As Christoph Meinel points out (1988), some early moderns like Basso, Van Goorle, and Descartes suggest such an approach would be pointless, for in their view there can be a corpuscularian structure to matter even if (really, especially if) there is a plenum. Yet their suggestion does not detract from the classic strategy, adopted by Gassendi, of arguing for atomism by demonstrating that an interparticulate void could exist; for if this was taken to show that matter is discrete, then if *some* corpuscularian view were true, atomism would be true. I discuss relations between arguments for atoms and for the void further in my concluding chapter. What is striking here, though, is

¹⁹⁷AN Book I; see in particular the section entitled "In nature, other than Bodies, there is the Void; and if it is proven that there are bodies, then we prove as well that there is a void mixed in by this reason, that otherwise there would be no movement."

¹⁹⁸See the accounts in Joy (1986), and Osler (1994).

Gassendi's use of this classic strategy in his attempts to support atomist claims by experimentally demonstrating the possibility of a void. Most notably, he reasons in just this way when he reports on the dissolution of alum in salt-saturated water, which he intends as a new version of an ancient experiment (reported by Aristotle) designed to show that there are interparticulate voids, and thereby, that matter comes in discrete particles.¹⁹⁹

The experiment is as follows: if we take a vessel filled with loose ashes and an empty vessel of equal size, each vessel, they contend, will hold the same amount of water. The official explanation of the void proponents is that the water enters into voids between the ash particles. What's wrong with this experiment, Aristotle suggests, is that it is absurd to suggest that the water and ash (or any two bodies) are in the same space at the same time--which is what must be the case if we don't assume to begin with that a void may exist. But what's really wrong with this experiment, as Francis Bacon notes, is that the reported results are implausible: each vessel cannot in fact hold the same amount of water, by a long shot.²⁰⁰ Gassendi attempts to refashion this experiment and demonstrate the existence of the void but on the basis of more plausible results: if we take one vessel of water saturated with salt and another without salt added, and try to dissolve alum in both, we find that the salt-saturated water dissolves alum just as well as non-salt-saturated water does. He concludes that there must be differently-shaped interparticulate voids, each shape being capable of receiving a particle of a corresponding shape. Thus salt

¹⁹⁹Physics 4.6 (213b21-22, 214b7-8).

²⁰⁰Sylva Sylvarum 1.34.

particles are cubes and so are received by cube-shaped voids, alum particles are octahedrons and so are received by octahedron-shaped voids, and so forth.²⁰¹

As Meinel points out, if the circumstances are controlled so that the phenomena this experiment produces consist only in filling such voids in just this way—that is, so that nothing else is occurring—then the volume of the solutions as a whole should not change. Yet, as Morin soon protested, we would increase the volume simply by adding materials other than alum, and this suggests a class of experiments resembling Gassendi's (but with other materials dissolved) does not and cannot yield a consistent explanation of how adding any one volume to any other will give you anything less than the two volumes added together. So much the worse for demonstrating the existence of interparticulate void, much less atoms, Morin concludes.²⁰² While Gassendi does not respond to Morin, he can easily meet this objection by stipulating that the interparticulate voids, or at least some of them, have fixed shapes, which is distinctly possible if the surrounding particles are joined in a stable fashion. Then it is possible to have the differently-shaped voids he posits in his alum-dissolving experiment, and thus to have differential rates of dissolution and, consequently, differential volumes among distinct sorts of solutions, in accordance with the sorts of voids present in the different solutions. Indeed, this kind of response fits Gassendi's molecularist views nicely, for if there are molecules which persist through chemical reactions, this allows voids which are enveloped by such molecular structures to have the requisite unchanging shapes.

²⁰¹Q I 195-196.

²⁰²De atomis et vacuo contra Petri Gassendi Philosophiam Epicuream (1650).

In his writings on iatrochemical issues such as the transformation of metals Gassendi also hints at, though never fully develops, another sort of experimental reasoning which later writers like Boyle present as a means of demonstrating the unchanging character of matter's primary elements. If we can demonstrate that the constituents of compounds persist through chemical reactions, this line of reasoning goes, then those constituents must be unchanging by their nature--as we suppose atoms to be. While such arguments are still qualitative in character, they generally stand in contrast to the Peripatetic accounts which explain changes in a given substance as the result of change in Aristotelian forms.²⁰³ Now, some early modern corpuscularians--including Sennert and perhaps even Gassendi to an extent--do not entirely reject Peripatetic explanation yet they all set out to account for substantial change in compounds in terms of the contribution of constituent particles or their aggregates to the emergence of new qualities.²⁰⁴ Accordingly, the Peripatetics held that reports of such substantial changes serve as empirical evidence of atoms, given the supposition that only durable, indestructible atoms could persist through those changes. It is curious, and not a little problematic, that they thought the parts of compounds must be absolutely immutable by nature just because they could persist through a wide variety of reactions.

²⁰³The Peripatetics, for their part, say the introduction of a new form brings these new qualities. Hence one issue that concerns them is whether or not the old forms (of the reactants) disappear, and whether the new form comes from some heavenly bank of forms or else somehow 'educed' from the potentiality of the preexisting matter. Further, they are interested in what happens to the form of the compound in chemical analysis and what the source is of new forms of whatever 'parts' emerge.

²⁰⁴Sennert certainly thought the two approaches were compatible, and given Gassendi's reinterpretation of the Aristotelian elements and notion of form in atomist terms, it seems he may have as well.

But Gassendi's problem is rather that he never quite arrives at this strategy of empirical argument. He suggests, for one, that metals are transformed by such natural efficient causes as sunlight, and that metals may have germinal or seedlike bases in virtue of which they may undergo generation-like phenomena (possibly transmutation, too) and he thinks these germinal bases should have corpuscular constituents so that whatever transformations occur, occur to elements with a common base and so with the greatest flexibility. And while he also expresses an interest in the transmutation of metals as practiced by the Peripatetics and others, he never goes so far as to infer from such transmutations or any other like chemical reactions that the constituent elements of a compound must persist unchanged. It is very plausible that he would find such a claim agreeable but the closest he actually gets to this is saying that corpuscles which surround and contain interparticulate voids are structured around the voids they contain in a sufficiently stable formation that it goes unchanged even when the voids are filled, for example, in the dissolution of salts. This is to assert the stability of molecular aggregates, however, and not the unchanging character of the atoms themselves.

If such physical and chemical phenomena as are directly perceivable do not seem to yield strong empirical evidence for atomist claims, perhaps more and/or greater evidence is to be adduced from peering into the subvisible realm through a microscope. Indeed, Gassendi was a contemporary of microscopy's early modern masters, including Leeuwenhoek, Swammerdam, Hooke, Grew, and Malpighi. Although Gassendi himself had little direct experience with microscopes, as an observer of the skies he was very much aware of the magnifying effects of telescopes, and he links the two instruments by

suggesting both may help us narrow the gap between what is visible in principle and what is currently not visible.²⁰⁵ What we can see through microscopes, then, constitutes empirical evidence for our beliefs about the subperceivable;²⁰⁶ for example, we may be able to roughly judge (if not to exactly measure) the size of the smallest particles from microscopic observations.²⁰⁷

I return to Gassendi's enthusiasm for (and adventures in) microscopy in my concluding chapter, but let me note a pertinent difficulty in this context. The suggestion that what we see in microscopes gives us evidence (empirical or otherwise) for atomist claims depends on our initially establishing the claim that what we see there--the structures and processes of the microscopic world--genuinely resembles and operates similarly to what we see in the macroscopic world. In those early days of modern microscopy, it cannot be surprising to find that Gassendi rushes to embrace this resemblance and similarity. Nor can it be too great an empiricist triumph for him, though, for the viability of this last claim depends crucially on what we take such microscopic data to represent and this is not necessarily an empirical matter. There can be little doubt such data are produced by a genuine and consistent amplification of our sensory capabilities: our notions as to how microscopes work (based, for example, on our understanding of the laws of refraction) permit us to agree that we observe images which are magnified to a particular degree and which, as magnified images, have particular

²⁰⁵Q III 456.

²⁰⁶Meinel also cites Henry Power as another early modern corpuscularian who promotes such a strategy for empirically demonstrating atomist claims.

²⁰⁷Q I 268-269; cf. AN I 207. This is a curious mixture of optimism and pessimism!

distinguishing features—a narrow band of grey, three rounds spots, a certain crystalline form, and so forth. But this by itself does not entail that the data from such observations represent empirical evidence of just those phenomena or entities we claim to 'see' through the microscope. Without any additional evidence to suggest such particular representations, what we 'see' through microscopes is strictly an interpretive matter and in the absence of supporting empirical evidence such interpretations are perforce rooted in *apriori* commitments including, for example, what the observer's working physical hypothesis entails regarding the micro-structure of matter. For his part, Gassendi thinks we have good empirical evidence to suggest the regularity of the amplifying powers of microscopes (as we saw in chapter one), but he offers no evidence to suggest his particular interpretations of his microscopical observations. Thus when he takes such observations to provide evidence of atoms and their behavior, this very well could be because his working hypothesis about matter's ultimate structure suggests to him a fruitful interpretation of the data. In this light, such evidence as Gassendi thinks we can obtain from microscopy seems a mixture of what we know from the senses and from our background theoretical commitments, and this suggests that the atomist claims he believes to be supported by that evidence are rather 'mixed' *apriori/aposteriori* assertions.

In the end, no 'pure' empirical evidence can be available relative to atoms or their properties as long as we maintain that such evidence is attainable only through direct perceptual acquaintance. If all we perceive directly are the phenomenal features of things, then we can have no direct perceptual acquaintance with those things, including atoms, which are subvisible and so have no phenomenal features. An interesting early

modern strategy for addressing this issue is based on a primary/secondary quality distinction, which says the phenomenal features of a thing are produced by the enduring and underlying features of that thing or its constituent elements. A number of atomists who hold this distinction suggest specifically that the enduring qualities are the ones atoms bear, and that it is in virtue of atoms contributing to the structure of a particular supra-atomic object that they also contribute to causing the particular secondary qualities we associate with that object.²⁰⁸ Accordingly, this novel empirical strategy for gathering data about atoms recommends the pursuit of an experimental chemistry in order to gain a robust enough grasp of the enduring qualities underlying the phenomenal ones so that we can characterize atoms as fully as possible without the benefit of direct perceptual acquaintance.

Meinel refers to this sort of strategy as 'epistemological' yet what is striking is that such a strategy is, by itself, precisely lacking a key epistemic ingredient, namely, an identifiable inference mechanism or set of rules for warranting our talk about those endurable qualities we may not perceive on the basis of data which reflects only those ephemeral qualities we do perceive. In brief, what is missing is some justificatory scheme, and this is where Gassendi provides the crucial supplement of his theory of sign-based inference. This theory, which says that we are licensed in interpreting perceivable

²⁰⁸As Christoph Meinel notes, Bérigard (1643) proposes that a core of 'natural' or primary qualities distinguishes different kinds of atoms, and their aggregates yield 'familiar' or secondary qualities, and Basso (1621) proposes that there are primary through quaternary aggregates which persist through reactions. He also notes that Jungius (1632) runs together the concepts of atom, element, and 'pure substance', though it not clear why this is supposed to suggest that atoms have the kinds of primary qualities which yield secondary qualities. Oddly, Meinel neglects to cite Gassendi's views on phenomenal and enduring qualities.

phenomena as indicative signs of subvisible phenomena just in case they are undeniable on pain of contradiction (or what he believes to be equivalent, satisfactory to reasonable persons), may well ultimately fail to justify any such characterizations of atomic qualities on the sole basis of our information about supra-atomic phenomena. After all, if the signs are not read correctly or are not truly indicative at all (say, if our senses fail to satisfy Gassendi's requirement that they serve as reliable guides to veridical belief), there isn't much justification for interpreting the available evidence according to atomist lights. All the same, the theory of sign-based inference marks Gassendi's attempt--unmatched by any of his atomist contemporaries and little addressed by his other contemporaries--to say how we might ever warrant the connections we draw between those features of the world we perceive and those features of the world we don't.

Ironically, if at the end of the day one or another of these strategies is successful in producing evidence from our sensory data for atomist claims then Gassendi's classically-inspired *apriori* arguments might look to us like no more than an elaborate if not superfluous display of scholarly erudition. This is not a particularly offensive prospect for Gassendi (at least the charge of being elaborate), whose frequent, lengthy quotations and learned Greek and Latin mark him as a humanist of the first order, and who accordingly seems to believe that the greater the provenance of a theory, the less likely it is to be wrong, or at least drastically so. One might wonder how Gassendi conceives of his two distinctive approaches to buttressing his atomist claims. It seems to me that, while he takes empirical evidence for atomism to be requisite as a matter of meeting scientific constraints, he thinks himself compelled to offer *apriori* reasoning and

historically-based argument as well--though only for rhetorical purposes, to address an audience he believes to place profound weight on the opinions of great authors and on arguments which echo worthy (that is, classical) tradition.

A far more dangerous prospect for Gassendi, on the other hand, is that he would run afoul of the empiricist assertion that our sensory-derived knowledge can tell us nothing about the intimate or inner natures of things. At least at first glance, though, it seems that this is just what any viable empirical evidence for atomism threatens, given such central planks of the atomist programme as the claim that we have an intimate grasp of any given bit of matter if we can know the states and behavior of its constituent atoms. However, two points Gassendi makes suggest he is in no real danger of violating this empiricist tenet. First, according to his theory of sign-based inference, the only inner natures we absolutely cannot learn about from what is perceptually available to us lie beyond not only our senses but also our justified sensory-based inferences about the imperceptible.²⁰⁹ It follows that we might well be able to learn about some natures of things though they are imperceptible, perhaps on the basis of such inferences; what's more, perhaps those things would include atoms and their features and behavior. Second, Gassendi never pretends that his atomist programme is anything other than

²⁰⁹Hence the truly unknowable inner natures might well include, for example, the *theologically* necessary features of things to which we can have no access because we cannot even reason about them, on the basis of indicative signs or anything else. One possible instance in this regard is the nature of the origin of matter and its characteristics, a feature of creation about which we have no sensory evidence nor any other (wholly) justified beliefs based on sensory evidence. Of course, Gassendi believes that in any case he is compelled by doctrinal constraints to accept a particular viewpoint on this issue. Just to be sure, he also rejects a variety of alternative creation stories on the grounds that all of them are contrived from fantasy, that is, without the merit of being supported by Holy Scripture. See Q I 480-486 (*De Primaevuo exortu, primave Genesi, & constitutione Rerum*).

hypothetical,²¹⁰ and that whatever claims we may consequently make about the inner nature of matter are also limited in the sense that our finite powers of sensing and reasoning prevent us from fathoming the ultimate mysteries of Nature as well as the background theological parameters of any such account:

...in all good faith we will attach ourselves to expose and explain what is presented to us as closer to the truth in the domain of Nature's things but which, as such, we have no hope to discover in them the deepest recesses, nor even to not remain ever-remote from them--so confining us, full of admiration, to what is only sort of an entryway. We understand that Nature is an immense and sacred temple in the sanctuary of which the Divinity stays, spreading and putting into work its power and unextinguishable wisdom. But it is not granted to us, poor little Men that we are, to access and penetrate them. It is permitted for us to reproduce, to a certain measure, some effects of the operations of Nature; but if, in a way, our look becomes more penetrating, it is not less intensely remote from the depths where the secret and Divine power accomplishes his admirable works.²¹¹

All the same, Nature is no more than a partly closed book for Gassendi, and he offers his atomism, empirically supported or otherwise, in the same spirit of the utilitarian solace he bids us for the limitations we face in attaining profound knowledge of the world's inner nature and workings:

I don't say this, certainly, to deter us from this sort of research and contemplation, since, as little as it may be that we can see clearly, that little is more precious than gold, and there is no occasion where we must heed more the counsel of the Poet who ends by this consideration:

One can advance up to a certain point, if it is not given to go beyond. [Horace]

I only draw attention to the above right away in order that he who advances full of hope does not complain after having been abused, if, for lack of having penetrated everything more deeply, he finds anyway what he needs.²¹²

²¹⁰See, for example, Q I 480-486.

²¹¹Prooemium to the Physics, Q I 128.

²¹²Prooemium to the Physics, Q I 133.

CONCLUSION: ATOMISM, THE METHOD OF HYPOTHESIS, AND EMPIRICAL KNOWLEDGE.

In the preceding chapters I have outlined the theories of empirical knowledge and scientific method which Gassendi believes to constitute the proper framework for a viable defense of an atomist hypothesis. Some commentators, such as Brundell and Joy, claim the significance of this defense of atomism is its reintroduction of ancient views of Epicurus and Democritus against a backdrop of the reigning Aristotelian metaphysics and the principal corpuscularian challenge of Descartes. But Gassendi does not earn his place in the annals of physical theorists simply because he reminds his contemporaries of a previously forgotten and ultimately wrong doctrine. Rather, he is rightly considered among the first *modern* atomists because he defends such views on at least partly empirical grounds. His efforts in this regard are not entirely successful, for as I suggested in the last chapter, his atomism fails to wholly conform to his own empiricist tenets. In the present chapter, I explore two further central issues concerning relations between his atomist hypothesis and theory of sensory-derived knowledge. First, I address the paramount issue of atomism's fit with our formalized empirical beliefs. Is Gassendi's defense of his atomist hypothesis exemplary of his scientific method? Next, I close by examining the possibility that Gassendi has drawn too tight a connection between his arguments for atomism and his account of empirical knowledge: are these views

mutually supporting so as to yield, jointly, a circle in his thinking?

A. Atomism and Scientific Method: the *Regressus Demonstrativus*, Probabilism, and Inference to the Best Explanation

Neither historicist interests nor theological concerns alone bring Gassendi to atomism, if indeed they play a pivotal role at all. Rather, his empiricism directs his choice of matter theories. Thus, to support an atomist hypothesis he follows at least one element of his proposed method, drawing on evidentiary claims he infers from indicative signs. Further, he extols the explanatory virtues of atomism in accounting for those phenomena evident to us, and commends the atomist hypothesis as most probable on the grounds that the best explanation of the total empirical evidence gives us the most likely account. Each of these strategies marks his commitment to developing a physical theory on the basis of information from the senses.

Yet like much else in Gassendi's scientific *oeuvre*, his atomist arguments do not generally conform to the formal elements of his 'official' method. There is no reason to think, for example, that the salient arguments for the various elements of his atomism can be laid out in such a fashion that they clearly manifest a search for the middle term, or that, by a sort of species-genus analysis, we reveal the identity of any such middle term. Gassendi himself certainly never proposes that his arguments for atomism should adhere closely to these elements of his method. Some of his casual remarks in the Physics nonetheless suggest that he believes we should arrive at atomist claims in something very like the *regressus* fashion:

Our knowledge is subject to this condition: we cannot penetrate the inner nature

of things, even though we can know certain of their effects. We must therefore take ourselves to be satisfied if, having divined something of these natures from the occasion of some effects, we can attempt to accomodate to other effects the notions that we have admitted concerning their natures, whatever those notions could be, when we wonder about the causes of things or search how they have their origin in the natures which are proper to them.¹

Here Gassendi echoes his proposal in the Logic that we reason from effects to causes, thereby 'divining something of these natures', and then try to say how those natures could produce other effects. In the present case, the suggestion is that we may infer the behavior and character of atoms from the surface-level phenomena we take them to produce, and say how such atomic phenomena might bring about other effects on the supra-atomic level. In fact, however, the pattern of Gassendi's actual reasoning departs substantially from this suggested path. His approach in the Physics is to propose what atoms should look like--largely by way of *apriori* assumption--and see if such a model adequately accounts for the phenomena of bodies which atoms compose. Along the way, he provides certain bits of what he takes to be empirically-derived evidence on behalf of some parts of his atomist programme. It is by no means the general case, though, that he *starts* his investigations into the ultimate constituents of matter by inferring the nature of atoms from the surface-level phenomena.

Gassendi's arguments for atomism more closely conform with that part of his method in which he presents a probabilist conception of viable inference in scientific reasoning. This conformity is independent of whether, in arguing for atomist claims, he derives the premises for those arguments from experience or reason. All such arguments

¹Q I 207.

are probabilistic in his view, let us recall, because the claims on which they are premised are in turn generally based on sensory-derived ideas and so less than certain. The only exceptions to this are those foundational claims in theology and theological cosmology upon which Gassendi builds a set of constraints on the nature of matter and its ultimate constituents. All the same, the particulars of whatever matter theory is workable given those constraints are to be decided in the end by appeal to experience. Hence the method by which we attain those particulars yields claims which are probable at best.

Adopting atomism as our matter theory of choice, for Gassendi, is not merely a question of conforming to the probabilism of his method; making this choice may actually further develop our set of methodological tools by promoting appeals to ampliative inference in mechanical explanation. To see how this is so, first consider that (as I argued in the last chapter) his meta-theoretical picture of physics suggests our physical laws or law-like generalizations should range over the visible and subvisible equally. It follows that we should aim for a matter theory which accounts for material phenomena in a correspondingly general way and, as we saw in chapter three, one attractive aspect of the atomist hypothesis is that it yields mechanical explanations which are, generally speaking, scalar invariant, at least for all supra-atomic bodies. The exception in his account is the peculiar qualities of atoms themselves, especially atomic motion.² No matter the scale, then, there are roughly identical causal accounts to explain why the smallest molecules and largest supra-atomic bodies have such qualities as

²As I suggested in the last chapter, Gassendi sets the stage for this variance by devising mechanical explanations on the basis of behavior of atoms with qualities *not* characteristic of supra-atomic bodies.

hardness or density or attraction. The only differences in such explanations should account for their differing degrees in their respective qualities.

By throwing out concerns over the scale of phenomena we seek to explain, it is a consequence of this particular matter theory that we can explain any material phenomena mechanically regardless of whether they are perceivable, and so embracing atomism removes one obstacle to ampliative inference from claims about the visible to claims about the subvisible. Hence our choice of physical theories shapes our pursuit of scientific reasoning by allowing us to expand the warranted use of hypotheses to account for such sub-perceivable phenomena. This expansion provides a further reason for Gassendi to place his atomist hypothesis at the center of his version of the mechanical philosophy, for in so doing he bolsters its explanatory power.

Gassendi is also interested in widening the traditional scope of the hypothetical method, and to this end he proposes that we may draw on the evidence of what he calls 'signs' to attain justified beliefs about the subperceivable. By admitting inferences to such beliefs, we broaden the range of admissible evidence which may be considered as collected from the senses and so, even by empiricist standards, may be adduced in favor of a given hypothesis. Recall that, according to his theory of signs, we can infer such claims about phenomena we cannot directly perceive on the basis of either recurrent signs of previously perceived phenomena ('commemorative' signs) or else perceivable phenomena we can only explain as manifestations of unperceivable phenomena ('indicative' signs). Thus, as Gassendi would have it, we have signs of atoms or at least their behavior just in case we empirically detect phenomena which are explained only by

the hypothesis that they are caused by the motions and other properties of atoms. Indeed, in the case of the atomist hypothesis, such evidence from signs must be prevalent because we have no direct epistemic access to these subvisible entities. What sort of sign-based evidence does he think we have for atoms?

For one, Gassendi thinks we can adduce evidence of atoms from signs we find in phenomena which are directly observable--what we perceive 'on the surface', so to speak. He suggests that what will count as such signs might be a matter we can judge in advance, for we can interpret surface-level phenomena as signs of atomic behavior if such phenomena fall into the category for which there is only one plausible explanation (in this case, an atomistic one). In this regard he cites the Epicurean prescription that we *'distinguish attentively between the things which have only one way to agree with sensible appearances and those which may agree in many ways with what occurs around us':*

...since natural philosophy consists of judging things which are hidden from us in accordance with those which are perceivable by their appearances, Epicurus wanted to distinguish between things which manifest themselves as if they can only happen in one way, and those for which it is possible to happen in many ways. Thus we could say that those which can only occur in one way behave in a certain fashion and the sensible appearances correspond to them, but we could not affirm the same of those which can occur in many ways, that they would take place in the same certain fashion, nor that their sensible appearances would correspond accordingly. In the first group he includes the existence of the void and indivisible principles. To the second group he says belongs what he calls the SUBLIME [*sublimia*]*--as for example the phases of the moon, eclipses, comets, thunder, etc..., all things for which it is necessary to give many modes of causality.*³

Since neither atoms ('indivisible principles') nor void have any appearances, he likely

³Q VI 156 (letter to Louis-Emmanuel de Valois, the Count of Alais, of October 3, 1642). Epicurus's rule appears in his Letter to Pythocles.

intends rather that we count in the first group whatever surface-level phenomena the sensible appearances we may take as occurring in one way only, and that by the confluence of underlying atomic states and interactions and/or the presence of the void. This is not a very promising strategy given that the very reason for identifying a phenomenon as belonging to the first group is that we cannot conceive of it occurring unless we assume an atomist account. It seems more promising to dispense with this Epicurean preselection, and simply judge our signs as indicative of some subperceivable phenomena--whatever that turns out to be--just in case the appearances of the surface-level phenomena could not occur otherwise.

In the surface-level signs of atomic behavior he actually proposes, this is in effect what Gassendi sets out to do. Two examples of such signs are the appearance of air at the top of the instruments he describes in his interpretations of the Pascalian barometry experiments (see chapter two), and the appearances of attractive phenomena which he explains as instances of contact action (see chapter three). In the barometry experiments, he argues, there is only one plausible way to account for what appears to be empty space through which light and other fine particles may pass: there must be an interstitial void at the top of the instrument. Without stipulating at least a partial void, there is no other way to explain the fact that there is an empty space which appears without any open passage to the surrounding atmosphere, and without stipulating that there are particles which must be imperceptibly small, there is no other way to explain the fact that light corpuscles, for example, may pass through the glass body of the instrument and travel through or remain interspersed with void. By stipulating even a partial, interstitial void, this account further

suggests the atomist characterization of the tiny particles as discrete units of matter; moreover, if the ultimate particles are discrete then it is possible, *contra* the plenists, for them to be indivisible.⁴ Hence we can take the appearance of this seemingly empty space as a sign of matter's atomic structure because the only plausible account of the perceivable phenomena suggests that the underlying subperceivable phenomena consist of atoms travelling through a void. The weak point, naturally, is the preliminary suggestion that the surface-level phenomena could occur under no other circumstances.

From this perspective, the attraction phenomena case is even weaker. Here Gassendi proposes that appearances of bodies being pulled towards each other or falling regularly to the ground should be taken as signs of underlying atomic seizing and pulling. In the case of gravity he has elsewhere ruled out the possibility that bodies have an innate tendency to fall to the Earth, and in either case he has already dismissed the possibility of action-at-a-distance on the grounds that all physical events occur as a result of atomic states and interactions in the void whereas no action-at-a-distance account bears an atomistic interpretation. The gravity case has an air of plausibility but the latter reason for ruling out alternative accounts is purely stipulative and in fact based on the presumption that something very close to atomism is correct to begin with. It is hard to

⁴The reasoning here is that the denial of a void entails the continuity of matter, which precludes the possibility of discrete particles which, being indivisible, are truly ultimate. Thus if there is a void, matter cannot be continuous and there should be some limit to its divisibility, say, at the atomic level. Yet the plenist arguments are a little more complex. Descartes, for example, denies the void on the grounds that whatever there is, is extended (extension being a property of all matter); that there are no limits to the divisibility of matter is indeed consistent with this denial but does not follow as a straightforward consequence. Stipulating that there actually is a void should have no bearing, then, on the indivisibility of matter--and in any case would constitute an instance of denying the antecedent; on the other hand, demonstrating the existence of a void, as we saw in the last chapter, has a greater bearing on the issue.

see why Gassendi thinks there can be no other interpretation of such attraction phenomena possible, and so it not clear why he takes appearances of the same as signs of atomic behavior.

The other sort of sign-based evidence Gassendi thinks we have for atoms we find in phenomena which are only indirectly perceivable, as seen through a microscope. Thus, in a letter to Peiresc of July 6, 1635, Gassendi proclaims that his crystallization experiments confirm "one of my old dreams [*resveries*] concerning the principles of the philosophy of Epicurus" by establishing that there are structures which maintain their basic shape though they are broken down into smaller and yet smaller sized forms. He refers to this process as 'resolution' by which he means the chemical method of dissolving crystals:⁵

...I recognize in particular that these large solid forms, whether cubes, octohedrons, or others, are composed of other lesser ones of the same shape, and that those of the lesser ones until their being resolved into very minute ones, they are almost insensible and remain shaped in the same way, from which I conclude that they can be resolved until their [constituent] atoms, which by some sort of necessity must be of the same shape. The composition demonstrates this, by the manner in which I observe that they grow larger from the moment that they become like [the size of] mites.⁶

In short, when we dissolve larger crystalline structures of salt or alum in water, the smaller crystals maintain the same shapes, down to the point that the forms can no longer be seen with the naked eye. Further, we should be able--in principle--to continuously

⁵It is tempting, though completely unsubstantiated, to suppose that at this early date Gassendi may have been thinking, analogously, of that element of the *regressus* by which we determine that the micro-structures bear the underlying causal features of the macro-structures.

⁶TdL 538-539.

dissolve the crystals until we reach the atoms they comprise, and given that each successive dissolution which we perceive through a microscope produces crystals of an identical shape, there is every reason to believe that we will find that shape on the atomic level, too.

Gassendi is not, of course, pretending to see atoms or even atomic aggregates through the microscope. He is proposing instead that the shapes of small crystals he *does* see when peering through the lens may be taken as signs of the shapes atoms or their aggregates feature. His point here is that the identity between the forms of the largest and smallest crystals we detect indicates that the forms of the large crystals are legitimate signs of the forms of the small ones. Since the smallest crystals we detect are formed from other, yet smaller constituents, *these* should also have forms identical to those crystals they compose, on the grounds that all the ones on a larger scale have been identical. Accordingly, we should be able to take the forms of the smallest crystals we detect as indirect evidence, or indicative signs, of the forms of those still smaller, subvisible crystals, all the way down to the level of their constituent molecules--and perhaps even atoms.

That, anyway, is the suggestion of Gassendi's claim to have found evidence for the Epicurean matter theory. Of course, this strategy could not be used to adduce evidence for atomism *per se*, because no atoms have the crystalline structure (that is, according to the classical or early modern doctrines) for which any higher-order crystals could serve as signs. It is more plausible to believe this strategy may yield evidence for Gassendi's molecular views--which, strictly speaking, is just what he claims regarding these

observations--but here, too, he runs into difficulties. For one, he infers that the lowest-order crystals we can detect may serve as signs from the successful role of higher-order crystals as signs, yet there is nothing about either putative signs which he suggests is such that, without the phenomena for which we take them as signs, these more familiar phenomena could not occur. But this is contrary to his rule for identifying indicative signs. Instead, in this instance, we are supposed to recognize the higher-order crystals as signs because microscopy demonstrates the properly analogous or even identical crystalline structure in component crystals, and we are to infer by analogy that the same holds true of any lower-order component crystals. Such a strategy not only neglects the official guidelines for indicative signs; it also incurs the problem of Gassendi's lack of faith in argument by analogy. It is a species of inductive syllogism, he proposes, and as such it is enthymatic--hardly the stuff of dreams confirmed.

Even without the canonical foundations of Gassendi's theory of signs, though, the merit of this experimental account is its powerful suggestion that we might take stable structures which *are* perceivable as signs of stable structures which are not, and that we might be able to iterate this strategy until we had an account of matter's ultimate particles. For all the promise of this strategy, Gassendi may well have regretted his non-standard condition for identifying indicative signs, or else lacked confidence in his analogy, for he restates this crystallization account in his Syntagma (two decades afterward) without expressing the same hopes for providing evidence on behalf of atomism. There is no question, however, of his dedication to the hypothesis that crystals have underlying molecular and atomist structures, and in his effort to display the explanatory range of that

hypothesis he once more presents his account of the crystallization experiments, albeit without any suggestion of producing evidence of atoms as provided by sign-based inference (though he continues to maintain that these experiments yield evidence of matter's molecular structure).

This account of crystal resolution is exemplary of Gassendi's approach in the Syntagma to the special sciences: whatever aspects of geology, chemistry, meteorology, etc. for which he believes he can account by appealing to the atomic structure of the world indicates the robust character of the atomist programme. Yet it does not, we generally tend to think, indicate that atomism is correct. All the same, the arguments he presents for individual elements of that programme contribute to a larger strategy of establishing that atomism bests all other competing matter theories in accounting for the available evidence and so comes closest to approximating the truth. Thus, quite apart from Gassendi's use of deductive or inductive reasoning, we find in this grand strategy or 'long argument' a striking appeal to inference to the best explanation--what Peirce would later call 'abductive' reasoning. Some commentators (including Brundell, Jones, and Joy) see this strategy as primarily an historical exercise, but Gassendi does not review a wide variety of past and current perspectives on matter theory simply to provide a convincing provenance for atomism--though he surely recognizes its value in light of the Church's anti-atomist sentiments. Instead, he sets out to provide the relevant contrast class against which atomism's accounts of perceptually-gained data is gauged the most successful, where these competing views include classical hylemorphism, cartesian corpuscularianism, and those views characterizing the composition of matter as a function

of a rationally and/or mystically-determined structure, including, for example, neo-Platonist appeals to ideal solids as the ultimate constituent structures of all bodies, and the 'Soul of the World' views of Kepler and the Rosicrucians. The reason for such wide explanatory success across the special sciences according to Gassendi is that atomism best accounts for what he sees as all the relevant constituent phenomena, including the coming together and falling apart of *whatever* we might count as fundamental bits of matter, and the advent of particular basic qualities of supra-atomic bodies, among which he includes solidity, size, density, and hardness (Physics I.3.8). There are several reasons, he believes, why atomism better accounts for such phenomena than the competing theories. For one, as I noted in chapter three, atomism does not appeal to at least some types of occult forces, including those entailed by the aforementioned accounts of harmonic or other mystically-derived structures. For another, it is at least partly empirically founded and so empirically testable. Finally, as I noted in A.2 above, it facilitates appealing to the mechanical philosophy in accounting for all manner of natural phenomena. This has the consequence of widening the range of phenomena present to the senses that is covered by the matter theory we choose--which in turn allows us to widen the range of possible evidence on behalf of such an account.

This last point raises the possibility that Gassendi's view of inference to the best explanation (IBE) differs rather strikingly from at least one current conception of this mode of reasoning. Timothy Day and Harold Kincaid have recently suggested this account of how IBE works:

IBEs are arguments beginning from some batch of accepted facts--call them the

'starting facts'--and inferring to one among several competing hypotheses. Those hypotheses are assumed to be evidentially equivalent on all dimensions except explanatory power (were this not the case, we could chose among hypotheses on predictive grounds and explanatory power would not be the sole ground of the inference). We determine the hypothesis with the greatest explanatory strength and thus infer to its truth from the starting facts.⁷

Day and Kincaid propose that IBE arguments are not fundamental forms of inference the use of which constitute indefeasible strategies in defense of hypotheses identified as the best among a set of alternatives. Unlike some critics of IBE, their concern is not that IBE strategies do not work because they yield inherently poor inferences or lead us to unpalatable consequences (van Fraassen, for example, believes such strategies lead us to violate the probability calculus) but instead that IBE strategies are not fundamental at all. In this they are rejecting the claim, made notably by Gilbert Harman, that IBE strategies lie at the foundation of all ampliative inference. Rather, they hold that such strategies are useful under some circumstances, not possibly compelling under others, and that this variable utility is determined by such contextual factors as the audience for a given explanation and the background information they bring to bear on a choice of hypotheses. The heart of their argument against considering IBE as a fundamental, or noncontextual, argument pattern, is their claim that IBE strategies as classically rendered are either empty or defeasible appeals to explanatory coherence or else defeasible appeals to the comparative merits of a particular causal story. In either case IBE may be trumped, for example, by background assumptions having nothing to do with coherence or causation--such as empirical adequacy.

⁷Timothy Day and Harold Kincaid, "Putting Inference to the Best Explanation in Its Place," *Synthese* 98 (1994): 275.

Gassendi's use of, and brief remarks related to, IBE suggest some ways in which this perspective may be off the mark. For one, Gassendi holds that we should judge explanations as the best given considerations of overall explanatory *utility*, however that is to be construed.⁸ Thus Day and Kincaid could be right to suggest that IBE is defeasible if we understand an explanation's being best in terms of coherence or causation, yet this does not exhaust all the possible understandings of what makes an hypothesis the best choice among a set of alternatives. Unless there is some reason to think our choice of the best explanation as determined by which is most useful is defeasible, too, there is no reason to think that IBE on the whole is generally defeasible.

For another, Gassendi does not think we need to start from a position of equivalent evidence for the various alternative hypotheses in order to identify the one which offers the best explanation. Indeed, he holds that an indication that one offers the best explanation is a capacity to generate further evidence on behalf of the *selfsame* hypothesis. Thus the atomist hypothesis bests its competitors relative to explanatory power in part because there is sufficiently little available evidence for the competing hypotheses that there is not much for them to explain.⁹ Accordingly, they suffer a deficit

⁸Clearly it matters exactly how we construe 'utility', but it is most important to stress how utility is not just a matter of coherence or causation. Thus, for Gassendi, atomism has greater theoretical utility than its competitors not only because it is coherent and tells a causal story with great explanatory power, but because it stands as an alternative to theories which are unacceptable on other (e.g. theological) grounds, and because it enables us to expand and develop our means of reasoning empirically.

⁹Very crudely, atomism and Aristotelian prime matter theory share the same broad explanandum: change. However, while these competing hypotheses are supposed to explain, roughly speaking, the same phenomena, (i) Day and Kincaid are specifically concerned with the evidence and not the phenomena-explananda generally, and (ii) the term 'change' is sufficiently theory-laden that Gassendi cannot accept that change occurs in matter as Aristotelians recognize it

in explanatory power. By comparison, it is a strength of the atomist hypothesis that accepting it leads us, through an optics premised on a corpuscularian theory of light, to additional evidence on behalf of corpuscularian views generally, and perhaps atomism specifically.¹⁰ Of course, it may happen that we have roughly equivalent evidence; this is the case in Gassendi's choosing among the broad astronomical hypotheses of Ptolemy, Tycho, or Copernicus. But as the matter theory case illustrates, it is not a *necessary* condition for IBE strategies. If this point is correct, then Day and Kincaid are wrong to suggest that in an IBE strategy we need to assume all hypotheses are evidentially equivalent save with respect to explanatory power. Their reason, let's recall, is that otherwise we would be able to identify the best hypothesis, not necessarily by their respective explanatory powers, but possibly by such factors as predictive power (which they take as having nothing to do with explanatory power, on the strength of asymmetries between the two). But if, following Gassendi, explanatory power is determined by such factors as the number of things which need to be explained, then there is no reason to think that we cannot judge between two or more hypotheses by their explanatory power when the evidence for each is unequal.

Another issue regarding IBE--whether it collapses into some other form of reasoning--arises when we consider his apparent reliance on such a strategy in a different

lest he accede to their prime matter theory to begin with.

¹⁰This proposal looks a little bit like Kitcher's notion that explanatory power is forward-looking, in that it entails the capacity of a candidate hypothesis to offer robust explanatory patterns (1981). The difference is that, while Kitcher is concerned with creating future explanation (with an economy of reasoning) Gassendi is concerned instead with creating future justification for the very hypotheses under consideration (by broadening our notion of what is admissible as evidence).

context, namely, his standard brand of indicative sign-based inference. Not all sign-based inference can be construed as a species of IBE reasoning; as I note in chapter two, there are very different rules for warranting claims we infer about the nonevident from different sorts of signs. In the case of commemorative signs, we are warranted just in case we have good reason to believe such phenomena as we take to be signs were previously and regularly associated with those phenomena for which we take them to be signs (smoke as a sign of fire is Gassendi's standard and classically-derived case). Commemorative sign-based inference, then, is a species of induction. With indicative signs, on the other hand, we are warranted--generally speaking--in making such inferences just in case it is not possible to otherwise account for the presence of those phenomena we take to be signs. As we saw in the microscopy case (A.1 above), however, Gassendi proposes an exception to this rule: when we designate perceivable phenomena as signs of what we cannot perceive on the grounds that past cases justify the belief that the former are reasonable analogues of the latter, we again rely on induction. Nonetheless, in *standard* inferences from indicative signs--those which work according to the rules given in the Logic--we grasp the nonevident from signs we identify in surface-level phenomena because there are no better alternative accounts available for what we detect on the surface.

Whether this is to genuinely count as inference to the best explanation depends on how we construe the underlying reasoning here. Thus, the reason Gassendi cites in the Logic for suggesting that there are no better accounts available is that it would be impossible (that is, on pain of contradiction) to explain the surface-level phenomena given any other hypothesis. But this is not IBE *per se*, one might say--it is

straightforward deduction of the form

if Q were not the case then P could not be the case

P is the case

thus Q is the case.

Yet it is not clear that, in presenting one account as explanatorily superior to its competitors, Gassendi ever actually relies on the reason given in the Logic. Certainly he never suggests anywhere else that we make avoiding logical contradiction the mark of successful attribution of indicative signs or inferences to warranted claims from the same. This is a good thing, too, given that there is no logical contradiction in supposing that even Gassendi's paradigmatic cases (sweat appearing without nonevident pores) are wrong.

Another possibility is that, as with many instances of IBE, the underlying reasoning here is simply an instance of induction. As Richard Fumerton has noted, an IBE argument of the form

P is the case

if Q were the case then we should expect P

thus it is highly probable that Q

turns out to be an inductive argument of the form

P is the case

Qs generally bring about Ps

thus it is highly probable that Q

if our basis for expecting P is the claim that, with sufficient regularity, Q leads to P.¹¹

Hence indicative sign-based inference might turn out to be a case of inductive reasoning

¹¹Richard Fumerton, "Inference to the Best Explanation," in A Companion to Epistemology, ed. Jonathan Dancy and Ernest Sosa (Oxford and Cambridge, MA: Blackwell Publishers, 1992), 208.

if Gassendi failed to proscribe any such basis for such expectations. Indeed, he does not specifically rule out that we might expect to find a given evident phenomenon because of the presence of some other, nonevident phenomenon on the grounds of our beliefs that the latter generally bring about the former. But he can't rule out that possibility because that is precisely what commemorative signs are. Indicative signs, by contrast, are supposed to yield claims about phenomena which are nonevident in principle, so there is no place for any 'observation' that what is not evident to us regularly brings about what is evident. Rather, it emerges that inferences based on such signs are genuine instances of IBE, for the same reason that Gassendi's grand strategy for defending atomism turns out to be an appeal to the best explanations. Those claims we so infer are the best, he believes, because of their attendant explanatory power, not because of any truth-preserving character of the inference structure, nor any ampliative assertions suggested by the evidence amassed. But by appealing to explanatory power, his account of indicative sign-based inference employs an IBE strategy. The significance of Gassendi's employing that strategy, either in this case or in the context of his 'long argument,' is not so much any particular strength or weakness of IBE, but that he might employ it altogether. It is hardly unusual for the seventeenth century, but it is most unusual in the hands of such an ardent empiricist; even more striking is his construal of appeals to IBE as an empirically-viable, even robust, means of reasoning to claims about the world.¹²

¹²One may contrast this with those uses of IBE for specifically non-empiricist ends, including defense of Platonist forms and cartesian vortices--and, plausibly, the various competing astronomical pictures of his day.

B. Is There a Circle in Gassendi's Reasoning?

The most tantalizing outcome Gassendi promises for his theory of knowledge and empiricist method is the feat of mustering empirical justification of his atomist views.

Although, as I suggest above, there are places where he apparently manages this feat—or at least has convinced himself of this—it comes at a fairly high price. Since Gassendi also defends his empiricist principles by appealing to his particular brand of corpuscularianism, there is minimally the appearance of circularity. He has, I suggest, two plausible routes of escape: look for other, significant defenses of either sets of principles which don't engender the circularity charge, or else defend the circularity as admissible under the circumstances. The former approach, I propose, is not likely to succeed, leaving Gassendi with a circle to defend.

If the proposed circularity truly emerges when his arguments for empiricism and for atomism are conjoined, there is indeed a significant problem with Gassendi's thought, for these two views are central to his philosophy and his contribution to the Scientific Revolution. First, as we saw in chapter one, the core of his development of a modern empiricism is his constructive skepticist programme, according to which we can form justified beliefs about the world on the basis of our sensory information. Gassendi attempts to supply the requisite justification via the suggestion that our sensory data represent worldly phenomena *reliably* because those data consist of impressions on our sensory apparati by corpuscular emissions emanating from or reflected by the objects of our sensation, which thereby accurately transmit the phenomenal characteristics of said objects. This proposal, however problematic in the details, may be understood as giving

one of the more compelling arguments heard in his times for justifying empirical claims: we can know about the world if we build such knowledge out of sensory-based claims we judge as, if not guaranteed, at least well-supported, by certain basic physical facts about the world and our interaction with it. In short, we justify our empirical knowledge by appealing to the physiology of our senses and the basic structure of the world. This is rather a modern approach to empirical knowledge, and almost an anticipation of Hume and even contemporary naturalizing epistemologists. Yet as I note below he cannot quite lay claim to the modernity of more complete empiricists like Hume, given that his suggested means of empirical justification ultimately rests on *apriori* claims about the micro-structures underlying perception.

It is striking anyway that Gassendi, in his efforts to develop a natural philosophy along empiricist lines, marshals something he takes to be empirical evidence in support of an atomist view. This move is at once a tremendous conceptual advance over his contemporaries and *prima facie* absurd in light of what we might ordinarily think of as reasonable empirical evidence for *any* atomist thesis, except those theses that suggest atoms are ridiculously large. I have suggested that we might only agree that Gassendi has given empirical reasoning for atomist views if we accept as well his peculiar view of what constitutes warranted reasoning about empirical matters. Peculiar or no, he intends to strike a contrast with the scholastic physical science of his day, which defines the basic composition of matter in terms of *whatever* gives macro-objects their essential composition, and which says the merits of such an account is that it is conceptually coherent, occasionally consistent with empirical research, and always consistent with

contemporary theology. Initially, Gassendi also espouses atomism on similar grounds--at first embracing the view, it seems, because he venerates Epicurus, and then finding a way to fit the doctrine to theological constraints. Yet Gassendi's mature atomism draws on an empirical defense and thus looks quite refreshing when contrasted with purely *apriori* defenses of atomist or other corpuscularian views common to, for example, Sennert and Descartes. We next see such an appeal to corpuscularianism as the best explanation of macro-phenomena supported by the empirical evidence in Boyle's Excellency of the Mechanical Hypothesis. Indeed, as Olivier Bloch, Margaret Osler, and others have noted, Boyle credits Gassendi (along with Descartes) for giving a modern formulation of a corpuscularian hypothesis, and clearly Boyle was himself inspired by Gassendi either in the original or else through the medium of Walter Charleton.¹³ Gassendi thus identified some essential tools of the Scientific Revolution, among them the need to develop a means of justifying epistemic claims about the world, and the need to develop a theory of the ultimate constituents of matter which permits easy empirical applications as well as admitting empirical defense. As I have suggested in previous chapters, one outstanding problem for Gassendi is that he failed to adequately develop either tool, but another, perhaps related, problem is that for each of these tools his means of developing the one appears to have relied on the prior development of the other.

In bringing together his arguments for atomism and constructive skepticism,

¹³See Bloch, La Philosophie de Gassendi, and Osler, "Sources of Robert Boyle's philosophy of nature: Gassendi's voluntarism, and Boyle's physicotheological project," in Philosophy, science, and religion in England, 1640-1700, ed. Richard Kroll, Richard Ashcraft, and Perez Zagorin (Cambridge; New York: Cambridge University Press, 1992).

where might a circularity emerge? To begin with, Gassendi suggests a central plank of his constructive skepticist programme--that we have reliable, probabilistic knowledge of the physical world--is true (that is, at least approximately so) because atomism is. Yet he also counts signs of subvisible phenomena as compelling evidence for his atomist matter theory on the constructive skepticist grounds that we may have limited, less-than-certain epistemic access to the nonevident through those inferences we license from such signs to viable claims about what lies beyond our direct perceptual grasp. In sum, to accept the constructive skepticist program it is necessary that we endorse atomism, though accepting atomism (at least by Gassendi's empiricist standards) entails that we embrace constructive skepticist principles. Let's examine the links between these arguments:

First, let's recall the atomist underpinnings of Gassendi's constructive skepticist programme. The mark of distinction which sets constructive skepticism (herein CS) apart from the classical strong skepticism is that this modern brand says we can know about the world though our knowledge cannot be certain, and the measure of degrees of support for our empirical beliefs is derived from the sensory evidence we adduce on their behalf. Gassendi in particular proposes that we can make justifiable judgements about the physical world because our sensory inputs are reliable, and because our judgements about those inputs are at least sometimes accurate. His grounds for suggesting that our sensory inputs are reliable are these: under normal conditions, our perceptual organs simply record a physical interaction with the outside world without significant discrimination, and so, without benefits or losses of information that accompany mental operations on those inputs. This simple recording must itself be reliable, too, and this he assures us is

guaranteed by the manner in which perceptual data are produced and in which perception occurs. In brief, groups of light atoms are transmitted from or reflected by sensed objects to our sensory receptors, thus preserving intact the detectable characteristics of such objects at the point where those atomic aggregates are received and "read" by our receptors.

As I noted in chapter one, we find some of the details of this story in Gassendi's optical and visual perception theory, where he accounts for (a) the transmission of images and light by groups of corpuscles which are emitted by particular sorts of bodies like the sun or fire, or else reflected by the bodies whose images we perceive, and (b) our perceptions of images and light as the result of interactions of our retinas with those emissions or reflections. This optical and visual perception theory is proposed as something of a response to and update of the scholastic view that images of objects which hit the retina are immaterial but film-like *species* or representations that transmit likenesses of those objects to the eye.¹⁴ Instead, Gassendi proposes, we perceive images of objects that do not themselves emit light because such objects reflect rays of light-corpuscles which consequently change configuration so as to bear images of the objects, which are then transmitted to our visual apparati through a material impact upon the retina. Thus visual perception entails that these rays strike our eyes and, Gassendi further suggests, the different corpuscles included in a given ray represent different parts of a thing we perceive. Perception of light *per se* simply entails that such rays of light-corpuscles are emitted from the right sorts of bodies, or else reflected off others, with an

¹⁴Q III 426b-427a.

intensity such that they bear no images at all.

Putting aside the abundant difficulties with this account of optics and visual perception, we can see that Gassendi uses it to address the classical Sceptics's worry that our senses may yield false information thus rendering them as unreliable sources of our knowledge. According to Gassendi's account, our senses cannot yield false information because they are responsible merely for acting as receptors of whatever information impacts upon them. Given that our receptors are in working order, this process is accurately executed. Whatever unreliability we can attribute to our sense-based knowledge, then, is the product not of sensory acts but of false inferences we make upon the faithful if inadequate information the senses provide. For example, Gassendi suggests we take rays emitted by perceived objects to represent the actual shapes and angles of those objects. If the object is close, then we perceive a sufficient number of distinctive rays and infer that we accurately detect the object's actual contours. In such cases we hold that the representation is accurate. If the object is far away, then the number of rays we perceive gives us an inadequate and deficient representation of the object. Our failure to accurately sense far objects is, then, the result of mistaken inferences on the basis of true but inadequate information as, for example, in a perceptual act featuring few distinguishable rays.¹⁵ That we fail to attain empirical knowledge because of poor inferences suggests we can succeed in this regard if our reasoning as well as our sensory information is up to snuff. As I noted in chapter one, this view clearly has its flaws, but the critical point here is just to note that Gassendi tries to use his atomist views in one

¹⁵Q I 85a.

line of his defense of CS. In short, what leads him to the claim that we have reliable knowledge of the physical world is a view of perception and light which presumes an atomist theory of matter.

Next, let's consider Gassendi's use of CS principles in defense of an atomist hypothesis. As I argued in chapter two, the empirical method Gassendi crafts neatly coheres with his CS programme: we tailor and maintain our scientific picture of the world so that it matches evidence from the senses. By contrast, he suggests, the scholastics attempted to fit sensory evidence to their accounts derived from reason alone and so were saddled with false evidence reports and unconfirmable theories.¹⁶ We find very much the same criticism, rather more extensively detailed, in Boyle's rejection of the Paracelsans and other iatrochemists. In similar anticipation of Boyle, Gassendi recommends that we endorse a theory if it best explains a wide range of phenomena which, we have seen, he thinks is at least partly indicated by the breadth and character of the evidence we gather. But in this instance there is no direct evidence of the phenomena that may lead us to if we want to endorse the theory because we do not or cannot directly observe that phenomena. Rather, as I note above, Gassendi holds that the relevant evidence we may gather consists of signs we understand to *indirectly* indicate imperceptible phenomena, so that, in the case of finding evidence for an atomist matter theory, we need to gather those data which indicate the existence of atoms or atomic behavior.

In section A and chapter two I suggested two places in Gassendi's corpus where he

¹⁶Q IV 504a.

believes that he has found evidence of this sort: in his own crystallization experiments and Pascal's hydrostatics experiment. In these experiments Gassendi takes the surface-level phenomena or what he sees through the microscope as signs of atomic phenomena because he thinks we cannot have the former without the latter, and because he assumes there is a physical continuity between macro-level and micro-level structures, that is, that there is no relevant change in the general nature of physical bodies as a function of size or scale. These assumptions lead him to suggest that macro-level structures give us some notion, by analogous reasoning, of what the micro-level structures are like. The crucial point in the present context is that the first such assumption is unjustified without his CS theory of signs. In attempting to adduce empirically-grounded reasons for embracing atomism, Gassendi cannot point to atoms but he can fall back on sign-based evidence to indicate that there are micro-sized bodies and structural phenomena which serve as fundamental units of macro-sized bodies and structural phenomena. Once again, his line of reasoning is far from complete yet the intent is clear: Gassendi tries to use his CS views in one line of his defense of his atomism. To embrace atomism on empirical grounds, he proposes, requires that we have reliable knowledge of the physical world in a manner prescribed by CS, through the medium of perceived signs.

And so we are left with at least the appearance of a circle. Gassendi tries to defend CS principles by reference to his theory of perception, which is in turn a thoroughgoing exercise in corpuscularian physical theory, for which his best empirical argument relies on CS principles. It seems he cannot hope to defend the atomist hypothesis by insisting on CS principles, and the other way around, on pain of circularity.

Some element of either his physical or epistemic picture has to budge if a circle is to be avoided.

It might be objected that I have ignored here the considerable element of Gassendi's defense of atomism which relies upon theological and *apriori* philosophical reasoning. In short, it might appear that I am suggesting Gassendi takes CS principles as necessary for endorsing atomism on the false grounds that he offers no *other* reasons for being an atomist. Thus if CS principles are merely sufficient for endorsing atomism, we need not concern ourselves with any putative circularity. Yet, as I have argued in chapters one and two, he strives throughout his Logic and the debate with Descartes to convince us that there is no such thing as *apriori* knowledge, that all our knowledge is ultimately rooted in experience, and that we should dispense with axiom-based physics and *apriori* astronomy models, in favor of observational accounts and careful measurements. Accordingly, it is a paramount concern for Gassendi that we attempt some empirical demonstration of atomism.

Of course, he also adduces much 'evidence' in favor of atomism that seems to have little to do with experience; he is quite concerned with what authorities past and present have to say about this ancient doctrine of athiest repute. At the same time, though, his concern with the testimony of authorities is consistent with the empiricist interests which inspire his adoption of CS principles. Gassendi is in the midst of a swirl of seventeenth-century intellectual activity the aim of which is to justify evidence from the senses as a legitimate form of testimony about events in the world. For many scholars of the day, defending the possibility of testimony from the senses, and devising means of

measuring the same, were simple and obvious extensions of admitting and devising legal and scientific standards for personal and written testimony.¹⁷ Thus it was eminently reasonable for Gassendi even on empiricist grounds to defend atomism by reference to the authoritative testimonies of not only sensory experiences but the witness of trustworthy scholars as well.

At any rate, perhaps the proposed circle is *not* to be avoided, on the grounds that the alternative is a slide to total scepticism. After all, for Gassendi, if we lack atomist assumptions then we cannot justify our empirical beliefs to begin with and so we never get past dogmatic assertions about what we cannot perceive. We can always fall back on an orthodox skeptical state of blissful indecision, he believes, but even in his early Exercitationes (1624) he has begun to eschew that orthodoxy for his 'middle way'. He realizes at that stage that to answer the skeptic and address the problem of the criterion we need some account of perceptual knowledge, and it is clear to him from the moment of his De Apparente Magnitudine (1642) onward that such an account relies on an atomist theory of light and matter. Conversely, if we don't assume *some* principles like those of CS which warrant our use of ampliative inference and probabilist reasoning to attain beliefs about what we don't or can't directly perceive, then we miss out on a good deal more than atomism. Gassendi is acutely aware of this throughout his scientific writings but particularly in his astronomy reports: he holds that telescopy like microscopy yields

¹⁷Barbara J. Shapiro reviews a number of the early modern concerns with evidence from testimony; see her Probability and Certainty in Seventeenth Century England (Princeton: Princeton University Press, 1983). Hacking (Emergence of Probability) fruitfully suggests that the scientific concerns with gauging such evidence may reflect analogous concerns in legal and commercial spheres.

data we can only admit as empirically viable if we accept his theory of signs and the notion that such images as we attain from telescopic may be the basis of a verisimilitudinous account of the world, even those elements of it which are nonevident. And so each of these assumptions (atomism, CS) look crucial to building our empirical knowledge. The problem is that we generally do not accept such circular reasoning as apparently results from Gassendi's building on those assumptions.

We might 'bite the bullet' and decide that the circle is at least momentarily palatable if there was some way to escape circularity once we make our empiricist science a going concern. Although Gassendi himself could not accept an empiricist defense of atomism that did not rely on something like CS principles, it is possible to imagine a 'corrected' Gassendi according to whom we might come to justify CS principles independent of atomist background considerations, only after we first accept and rely upon the atomist premises which yield the undesirable circle. And so we might adopt an atomist theory of light, for example, in order that we first dismiss the strong skeptic, allow the possibility of knowledge through the senses through sign-based inference, and thereby permit empirical support for the matter theory Gassendi takes as the cornerstone of our physical understanding. We would be left with a circle though perhaps not an onerous one, just in case we were then able to produce reasons independent of atomism for maintaining CS principles. For this 'corrected' Gassendist strategy to work, however, we would need to specify the kinds of expected justification of CS principles which would be independent of atomism in this way. Some possibilities include: the CS methodology yields good scientific results; it is theologically *prominent* methodology; it

is theologically *warranted* methodology, or some combination of these. The first kind of justification here probably looks better to us than the others, but it is the Gassendist who has to find these possibilities plausible and/or palatable.

The *other* approach is to try punching holes in the circle, by showing that he does not really need, to begin with, either CS principles to generate evidence for atomism, or else atomism to support CS. Concerning the first option, it does not seem that Gassendi would have so vigorously promoted Epicurean philosophy without also holding that good physical scientists following proper empiricist protocols should naturally arrive at an attractive atomist thesis. Of course, some other, non-CS epistemic principles might warrant our empirical beliefs and so provide an acceptable alternative for licensing the kinds of inferences Gassendi takes to yield evidence for atomism. But given his deep-seated conviction that all knowledge comes from the senses, a set of epistemic principles should be admissible in this context only if it provides some account of how perception contributes to belief. Since any such underlying account of perception is in his view an atomist one, choosing alternative epistemic principles does not help escape the charge of circularity.

The second option--that he does not really need atomism to support CS--is especially inviting if we think Gassendi's reliance on a physical hypothesis to buttress a theory of empirical knowledge seems oddly misplaced in the seventeenth century. However, it is a *commonplace* of early modern thought to appeal to physical accounts in defense of claims about the character of our knowledge--and not necessarily in the form of a causal theory of knowledge. Even Descartes relies on a primary/secondary qualities

distinction--part of his corpuscularian programme--in his attempt to establish clarity and distinctness as the mark of our most secure sense-derived ideas. Against this historical background, what distinguishes Gassendi's account includes, first, his reliance on the physical picture for his *entire* theory of knowledge given his claim that *all* knowledge originates in sense-derived ideas (here he anticipates and probably influences Locke), and second, his suggestion that it is just this physical picture which gives us good reason to accept, if only as verisimilitudinous, our sensory-based judgements about the world. Hence Gassendi's theory of empirical knowledge, according to my interpretation, is novel and controversial for his times, but it is hardly so radical as to suggest that the interpretation must be flawed. More to the point, perhaps, this second option cannot work since it is precisely atomism to which he appeals in order to guarantee the reliability promised by his CS principles, and is not clear what alternatives are even available to him. If these two options fail, though, it seems the circle cannot be avoided.

Another drawback to the thoroughgoing nature of his physicalism, by the way, is his accompanying weakness for attempting to complete an explanatory account whatever the available supporting data. This indicates why, though he attempts to develop elements of what we may think of as a naturalized epistemology, he cannot be considered as modern as Hume. Gassendi wants to defend his CS principles by appealing to an atomist physical picture, the defense of which may be partly empirical but the general plan of which is laid out in stunningly fanciful detail, a product of his vividly imagined expansion and 'improvement' on Epicurus. In his physics, this amounts to such creative proposals as his suggestion that gravity is a pulling-down on objects which results from

their being attached to rays of special 'gravity' atoms.¹⁸ In his physiological account of perception, this amounts to such proposals as the claim that perceptual data are transmitted via corpuscular packets. But if the reliability and so the justification of our empirical beliefs depends on this physiological account, then we need to worry about Gassendi's failure to empirically substantiate such basic elements of that account as the aforementioned claim. It might be bad enough to find that in Gassendi's reasoning for two principal elements of his philosophy, he holds that each of those element crucially supports the other. Yet beyond the apparent problem of circularity, he does not even manage to develop his theory of knowledge thoroughly on the basis of observational and experimental reports and so he fails to meet those of his own empiricist obligations necessary to generating one half of the circle.

One way to think about these problems is by appealing to the context of Gassendi's times. The suggestion that we cannot justify endorsing atomism without something like CS principles (*one* half of the circle) is likely to arise for anyone who, even in an age of sophisticated microscopy, maintains some version of a corpuscularian hypothesis on empirical grounds. Given that we lack observational access to the subvisible, the only way to claim empirical support for such an hypothesis is to suggest that it may be demonstrated on the basis of *indirect* empirical evidence. And, it may be further supposed, such evidence is only admissible by empiricist standards if you have a theory of knowledge which, like Gassendi's, allows that we may infer claims about the nonevident from claims about the evident on roughly the same, if not identical, grounds

¹⁸Q I 388-393.

for allowing inferences to claims strictly about what is evident.¹⁹ Hence whatever account warrants our admitting such indirect evidence should allow that, at a minimum, the method which takes us from the evident to the nonevident guarantees some empirical means of confirming claims about either, and that whatever principles by which we justify a given empirical judgement are indifferent to the perceivability of objects of such judgements. Gassendi's account takes this form: (a) reasoning by analogy and ampliative inference can allow us to draw on available experimental or observational data to make judgements about hidden mechanisms or qualities which are only acceptable if we cannot have the available data without the existence of what is hidden, and (b) trustworthy and relevant sensory information may lend probabilistic warrant to our empirical judgements, whether they concern bodies which are directly perceivable or not. In short, then, assuming global empiricist scruples and an interest in promoting a corpuscularian hypothesis such as his atomism, one needs something at least similar to the core principles of CS.

Going the other way is a bit tougher: it's not a prerequisite of robust empiricist perspectives that we have to be atomists. As a thoroughgoing empiricist, however, Gassendi wants to secure a trustworthy characterization of sensory information by telling a physical and physiological story about the interaction of the senses with the material world. He could have plead ignorance here but the contemporary work of iatrochemists

¹⁹One could imagine that the standards for warranting our inferences to claims about the evident and the nonevident need not be homogeneous, but they have to be fairly similar for the empiricist or else the standard will deviate too far from whatever counts as knowledge from the senses. The non-empiricist naturally does not face this sort of constraint.

(not to mention his Epicurean tendencies) suggest to him that the world really is composed of small particles. So an appeal to some sort of corpuscularian hypothesis is not unreasonable, and perhaps expectable, despite the apparent flaw that emerges when putting these arguments together. This is not to suggest that there's anything inevitable about these arguments, but simply to indicate that Gassendi's argumentation proceeded along fairly natural if mistaken paces. In this sense, his particular predicament is peculiarly a product of his times. Yet his problems here also foreshadow difficulties faced by later thinkers: defending empiricist method on empirically-derived grounds requires great care and, most likely, a non-foundationalist view of justification. There are most, if not all, the elements of the latter in Gassendi's reliabilism, but it seems he never quite recognizes the circle in his thinking, much less carefully suggests a clear way around it.

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