MEDIAEVAL THOUGHT—EXPERIMENTS: THE METAMETHODOLOGY OF MEDIAEVAL SCIENCE

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I. Introduction

THE modern understanding of mediaeval science has been shaped by the pioneering scholarship of Pierre Duhem, who put forward as a central claim that the components of "modern" science—most notably the kinematic and dynamic insights of Galileo—are found, or at least are prefigured, in earlier mediaeval scientific writings, out of which modern science evolved.[1] Scholars who have followed in Duhem's footsteps, such as Charles H. Haskins, Annaliese Maier, Edward Grant, A. C. Crombie, William A. Wallace, Marshall Clagett, James A. Weisheipl, Lynn Thorndike, and John Murdoch, have carried out their research into mediaeval science in the shadow of Duhem's central claim. Dissatisfaction is occasionally expressed, but, in the absence of a developed alternative to Duhemian orthodoxy, such complaints echo in a void, without any theoretical support.[2]

A reassessment of Duhem's central claim presupposes an understanding of the mediaeval paradigm of science formed independently of its possible intellectual progeny; once such an understanding has been attained, questions of continuity and influence can be sensibly raised, but not before. In this article, I will try to construct a picture of the scientific paradigm current in the mid-fourteenth century, concentrating on physics for two reasons: Duhem's case is strongest with regard to physics, and physics, unlike, say, alchemy or astrology, was recognized as a science (as *scientia*) at the time.[3] Henceforth when I speak of "mediaeval science" my meaning is properly restricted to fourteenth-century physics, unless noted otherwise. In particular, I will argue for five theses: (i) that the achievements of mediaeval science, even the three achievements traditionally singled out as anticipating modern science, were part of a completely different scientific paradigm; (ii) that this paradigm took as the measure of success of its theories and hypotheses not experimental confirmation, empirical justification, or saving the appearances, but rather the ability to deal with examples and purported counterexamples; (iii) that the method of mediaeval science was thought-experiment rather than actual experiment or testing; (iv) that there was a developed body of reflection on the method of thought-experiment, found in treatises on *obligationes*, which constituted the meta-methodology or philosophy of scientific method in support of mediaeval scientific practice; and (v) that this method has its own virtues and vices quite distinct from those of modern scientific method.

II. Three Achievements of Mediaeval Science

The Duhemian tradition identifies three achievements of mediaeval physics, all put forth in the second quarter of the fourteenth century, as supporting its central claim: Heytesbury's Mean-Speed Theorem,[4] Bradwardine's Function,[5] and Buridan's theory of *impetus*.[6] Each had an immediate influence on contemporaries; each, it is claimed, prefigures Galileo's scientific investigations; each is said to be directly influential in the history of physics—to such an extent that Galileo is accused of pirating mediaeval results.

- *The Mean-Speed Theorem*. In cases of bodies moving with constant acceleration (positive or negative), William Heytesbury argued that the distance traversed would be the same as if the body were moving for the same length of time and no acceleration with a uniform speed[7] equal to the instantaneous speed at the middle instant of time of the interval of uniform acceleration.

- *Bradwardine's Function*. To determine the speed at which a body with a given force will move through a medium with a given resistance, Thomas Bradwardine argued for the general claim that the speed will vary arithmetically when the proportions of force to resistance are varied geometrically.

- *The Theory of Impetus*. To explain the motion of bodies not in obvious contact with any mover, as in the case of projectile motion, Jean Buridan argued for the existence of an enduring quality, called *impetus*, imparted to the moving body by the mover, which varies in direct proportion to the mass (the quantity of prime matter) and the speed of the moving body.

The claims made on behalf of these three achievements of mediaeval physics in the Duhemian tradition are forceful and striking. Heytesbury's Mean-Speed Theorem has been cited as an anticipation of Galileo's analysis of acceleration of bodies in free fall, Duhem suggesting that Galileo was acquainted with it.[8] Bradwardine's Function has been considered a mathematical breakthrough to the theory of exponential variation, and in its analysis of force, resistance, and speed likened to Galileo's analysis of these.[9] With regard to Buridan, Clagett writes: "one cannot help but compare Buridan's *impetus* with Galileo's *impeto*, and Newton's quantity of motion (momentum)."[10] As well as forceful and striking, these claims may seem extravagant. And so they are.

The most obvious grounds for skepticism about such claims come from the material basis of mediaeval physics: rather than the result of direct observation and experimentation, the enterprise of mediaeval physics is textual, either commentary or questions on Aristotle, or independent treatises on particular questions derived from Aristotle. Heytesbury states the Mean-Speed Theorem in his *Regulae solvendi sophismata*: a book for intermediate logic students, giving rules for resolving logical puzzles (sophisms).[11] A survey of its six chapters should point up the sceptical doubts. The first, "Insolubles," is concerned with the Liar and its variations. The second, "Knowing and Doubting," deals with what we should call epistemic-doxastic logic, investigating the logical behavior of propositions with such terms. The third, "Relative-Terms," deals with questions of what we should now call anaphoric reference. The fourth, "Beginning and Ceasing," deals with the assignment of intervals and limit-points of intervals (e.g., "Socrates begins to run"). The fifth, "Maxima and Minima," deals with the extrema of functional variation and the conditions under which such extrema exist. The sixth and last chapter deals with the three categories in which Aristotle says that non-substantial change takes place: Quantity (growth or augmentation), Quality (alteration), and Places (locomotion). The Mean-Speed Theorem is stated in this last section. The entire context of the *Regulae* is logical, or logico-mathematical; it is far removed from the experimental researches of a Galileo, or even of a contemporary alchemist. Certainly, the literary form of a work need not reflect its content, but in this case I think it tells against the experimental nature of mediaeval physics: the conception of "physics" under which the Mean-Speed Theorem is naturally classified together with anaphora and paradoxes of self-reference is distinctly non-modern, having little to do with experimental confirmation of hypotheses.

A sense of the alternatives available at the time sharpens the doubt. Heytesbury's slightly older contemporary, Richard Kilvington, addresses the question of instantaneous speed in his 1325 *Sophismata*, rejecting the Mean-Speed Theorem for two main reasons.[12] First, because it begs the question; one must stipulate the instantaneous speed to determine the instantaneous speed. Second, because it is conceptually incoherent; speed is the rate of motion, but motion only takes place during an interval and not at any instant. Instead, Kilvington maintains that there is no sense to "instantaneous speed" at all.[13] How were such debates as these settled? Whose position was better? And the answer is: by recourse to logical considerations—example, counterexample, generalizing plausible rules covering some cases to fit others. There is no hint of anything empirical in this process.

Now it could be, and indeed has been, maintained that Heytesbury and Kilvington are working on highly abstract forms of physics, a kind of mediaeval "theoretical physics." Support for this comes from the fact that it is not clear what *would* count as experimental or inductive support for the Mean-Speed Theorem; it just isn't the kind of thing which can be tested, there being no instruments capable of measuring speed at an instant. It is, rather, a high-level conceptual innovation of mathematical physics. Furthermore, the logico-mathematical procedure is appropriate to determining the truth of the Mean-Speed Theorem, because it is peculiarly apt to reveal conceptual weaknesses and expose hidden inconsistency or incoherence. Thus, it is said, mediaeval physics is recognizably physical science, but at a highly abstract level; not experimental, but theoretical, physics.

While there is some justice in this reply, I think it misses the point. Theoretical physics, no matter how theoretical, qualifies as physics for us in virtue of being hooked up to experimental testing in the long run, and the Mean-Speed Theorem cannot be directly tied to experience. Is there an indirect connection? There is a testable consequence of the Mean-Speed Theorem: the so-called "Distance Theorem."[14] Yet there is no indication that during the mediaeval period the Distance Theorem was ever justified on any basis other than geometrical considerations. In fact, they could not have performed the relevant experiments: Galileo notes that he had to construct a more accurate clock in order to experimentally test it; the technology would have been beyond mediaeval resources. Just as it does not suffice for something to be called "physics" that the content of its principles concerns the motion of bodies in space (for then astrology would qualify as physics), so too it does not suffice that the principles have testable consequences (astrology would again qualify).[15] Rather, the justification of the physical principle has to be tied to the actual testing, to actual experience. The Kilvington-Heytesbury debate, therefore, cannot be interpreted as a form of abstract, highly theoretical physics.

Perhaps it is a good thing that Bradwardine was not concerned about experimental confirmation of his function: the fact of the matter is that an earlier theory, explicitly rejected by Bradwardine, seems to better fit experience, namely the view that speed varies in proportion to the difference between force and resistance.[16] The rejected theory is the natural one, given the mediaeval understanding of force; Bradwardine's rejection, like the problem itself, is not based on experiment, but on textual considerations—he says that Aristotle (*loc. cit.*) takes force and resistance to be related by a proportion, not by their difference. And that, for Bradwardine, seals the matter.

The further development of Bradwardine's Function, by philosophers such as Oresme and Swineshead, is mathematical rather than experimental. The proportionality constants are never given; numerical values are assigned in examples for illustrative purposes, but not derived from testing. Heytesbury remarks that it would be more of a hindrance than a help to determine such values (*Regulae*, fol. 41rb), a sentiment echoed by Swineshead (*Liber calculationum*, fol. 52ra of the 1520 Venice incunabulum). Oresme's extension of Bradwardine's Function is used to criticize astrology, but the grounds it proposes are solely mathematical, even though observational determination of the proportions would make the criticism decisive.[17] Despite its testable content, Bradwardine's Function is no more a piece of modern physics than the Mean-Speed Theorem.

III. Experience and Experiment

With Buridan the issues we have been discussing can be put in sharper focus. When Buridan argues for the existence of *impetus*, he does seem to appeal to experience in a straightforward manner. Like Bradwardine, he begins with a textual problem: Aristotle, in *Physics* IV.viii 215^{a}14-17, seems to endorse a theory known as "antiperistasis" to explain projectile motion, whereby the air surrounding the moving body rushes in from behind, in the space vacated by the moving body, to impart a continuing force to the body and keep it in motion. Buridan objects to this theory by citing three experiences, *experientiae*: (i) some things, such as a blacksmith's wheel or a child's top, spin without changing their place, and so it makes no sense to say that air rushes in from "behind" the moving body; (ii) reducing the surface volume exposed to the air pushing from behind, as when the handle of a lance is sharpened to a point, does not reduce the projectile's speed; (iii) if those pulling a boat upstream suddenly stop, so that the boat continues to drift upstream through its momentum, a person on the boat does not feel wind from behind pushing him forward, and neither would straws at the rear of the boat be bent over. On first glance, (i)-(iii) seem to be the sort of thing I was demanding: a recourse to experience to support or attack a theory. Indeed, that is how Buridan's examples have been construed; Clagett praises their "strongly empirical and observational character."[18] But appearances can be deceptive and, in this case, are deceptive.

With regard to the appeal to experience—to *experientiae* or to *experimenta* (Buridan uses both terms)—it is misleading to view such appeals as evidence for an empirical method. Often the sense of "experience" is no stronger than the "experience" of the old farm hand, a simple way of stating "what everybody knows." Such appeals are *not* to be confused with modern experiments or testing: no question is being put to Nature. Compared with those of modern experiments, the standards of precision and care are laughable at best. The recourse to commonly-held beliefs is not equivalent to observational reports; common beliefs are theory-laden and held in the absence of, and sometimes in the face of, evidence. What "everybody knows" is a miscellaneous mixture of fact, superstition, prejudice, report, rumor, and wishful thinking. Francis Bacon, well after the mediaeval period, cited as a certain *experimentum* the "fact" that a man whose feet are planted in the soil at the top of the Himalayas will live forever. Nor are there rules for what counts as relevant evidence; astrological predictions are "confirmed" time and again by such unlikely events as a bird flying north on the day of a fatal accident. The sense of "experience" Buridan appeals to supports astrology as well as physics.

Even mundane examples of the sort cited by Buridan do not count as putting a question to Nature, since there is no record of anyone putting such claims to the test. This is worth emphasizing: we have *no* record of a mediaeval physicist drawing a testable consequence from a theory and then attempting to actually test it, or have it be tested. Buridan asserts that sharpening the handle of a lance does not reduce its speed; why believe his claim? The answer that it need not be tested because it is "obvious" what would happen is misguided on two counts. First, the "obvious," like "what everyone knows," is treacherous. It was "obvious" that a large stone will hit the ground before a small stone, if both are dropped simultaneously from the top of a tower. Second, and more important, this answer misses the point, which is that Buridan's appeal to experience is not an appeal to observation, much less an appeal to observation in cases of intervention.[19] Buridan does not seek to justify a physical theory, or to show that a physical theory is unjustified, on the basis of the evidence produced by controlled experimentation; he appeals to common beliefs to illustrate his contention that a given theory is or is not justified. The *experientiae* or *experimenta* of mediaeval science are completely unlike the experiments of modern science.

This conclusion can be supported by considering the kinds of *experientiae* actually appealed to by mediaeval physicists in addition to the cases mentioned above. Bradwardine rejects the theory that the speed of a body is proportional to the difference of the force and resistance, citing an *experimentum* in which a weight is attached to a fly. While discussing whether motion is relative or absolute, Buridan posits that God could rotate the entire cosmos as a single solid body. To extend Bradwardine's Function, Swineshead imagines a rod of uniform density approaching the center of the universe through a void. Such cases are introduced exactly on a par with the mundane examples of sharpening lances—and this fact, if nothing else, should suggest that our understanding of the mediaeval enterprise has to be changed.

If we focus on the wide variety of these cases for a moment, a general picture starts to emerge. The philosophers of the High Middle Ages, it seems, were generally *indifferent* to whether the examples they describe could be concretely realized. It is not an essential feature of mediaeval science that it proceed by way of realizable *experientiae*, much less by modern experiment. We could sharpen lances easily; we would have difficulty attaching a weight to a fly; we could not observe a rod approaching the center of the universe; it is impossible for us to rotate the cosmos as a single solid body. But the cases are on a par, introduced and appealed to in the same fashion and in the same terms by mediaeval physicists, and that strongly suggests the so-called "empirical and observational character" of some examples is simply accidental. Heytesbury uses a nice phrase: he says that he is proceeding *secundum imaginationem*, "according to imagination" (fol. 43vb of the Venice 1494 incunabulum; see also fol. 161vb). In fact, Heytesbury explicitly states that some of his cases are not physically possible—acceleration to infinity, diminution to zero quantity—but that they are imaginable, and hence should be considered.[20]

Consider such cases postulated *secundum imagination*: they seem to cover a wide variety of examples. They include possible situations which we could bring about, though there is no evidence that anyone ever did: sharpening lances after Buridan's lecture. They also include possible situations which we cannot bring about, due to our lack of the relevant power, but which could be brought about through the action of some other agent (typically God): rotating the cosmos as a single solid body. Then there are possible situations which not even God can bring about, for example those which would involve changing the past. Finally, there are situations which are not possible at all, examples constructed on the basis of *per impossibile* reasoning: Heytesbury's case of a body with actually infinite acceleration, or a body containing neither hot nor cold thence able to be simultaneously heated and cooled.[21] The only unifying mark all these cases have, I believe, is that they are thought-experiments. In order that this not be a vacuous claim, we shall have to look a little more closely at thought-experiments.

IV. The Method of Mediaeval Science: Thought-Experiment

A more adequate picture of the scientific activities of the fourteenth century can be constructed, it seems to me, by examining the Aristotelian paradigm of *scientia*. Very roughly, an organized body of knowledge counted as *scientia* if it could be organized and understood as a deductive structure: a certain class of universal propositions are self-justifying and necessary, and their logical closure under the four forms of entailment (the four causes) includes all other propositions of the *scientia*. This view gives a paradigm of knowledge, representing its ideal form: it is not meant to explain the acquisition of knowledge, but to characterize its final shape—much like the covering-law model of scientific explanation. While there were philosophical disagreements on particular points, for example, how one *scientia* was to be set off from another, the central points of the paradigm were generally accepted.

How are the basic propositions—the "principles"—of a *scientia* learned? Aristotle offered only sketchy remarks in *Posterior Analytics* II.xix to the effect that the mind, after romping through ἐπαγωγῆ, rose to grasp principles through νοῦς.[22] Since such principles are self-justifying, carrying their justificatory warrant on their face, as it were, the problem was understood to be how to bring about the insight which allowed one to "see" such ineluctable truths. How does the speculative intellect get put into high gear? The answer I want to propose is that one does so through considering thought-experiments, that thought-experiment is the methodology of mediaeval science.

Striking as this claim may seem, it is a corollary of a more basic mediaeval view, namely, that any knowledge worthy of the name of *scientia* is academic, the fruit of the academic method of inquiry—disputation. Siger of Brabant, for example, argues that the knowledge of truth presupposes the ability to resolve any objections, doubts, or counterexamples raised against the view to held to be true: "The knowledge of the truth in any subject is [!!] the solution of doubts; just as it is said of judges that the judging is improved by hearing arguments from both sides, so too considering first the arguments for each side of a contradiction, leading to doubts, improves the judging of truth."[23] Learning how to generate objections, doubts, and counterexamples was the life-blood of the mediaeval university, and thought-experiments are an obvious source of difficult cases.

This view may gain some plausibility if we also recall what counted as a *scientia*: the paradigm cases were metaphysics, mathematics, and physics. They shared a unity of formal structure as *scientiae*; hence it is not unreasonable to think that they shared a unity of method as well. Thought-experiments seem peculiarly appropriate to geometry (the most sophisticated mathematics in the Middle Ages), perhaps due to the ideality of geometrical objects: dimensionless points, perfect circles, and the like. Geometric proof seems no more than an extended thought-experiment. Metaphysics, too, is full of thought-experiments, perhaps due to its *a priori* structure. For example, Duns Scotus, while discussing the relation between time and modality, puts forward a case in which which an angel with free will exists only for an instant and chooses one of two exclusive options; this imagined case allows him to argue for his view of the relation between modality and time. Hence physics, like mathematics and metaphysics, should be characterized by self-justifying basic propositions, and the speculative intellect is goaded into action to see the truth of these principles through considering appropriately constructed thought-experiments.

A few words about thought-experiments are therefore in order. First, some take the "mentalistic" aspect of thought-experiments seriously, and hence investigate topics such as conceivability and its relation to possibility; the nature, scope, and reliability of introspection; the mental mechanics involved in imagination; and the like. While such topics may be worthy of investigation, they seem never to have been raised in connection with thought-experiments during the High Middle Ages. This suggests that mediaeval philosophers took thought-experiments to be rather like consistent sets of sentences, enabling them to bypass "mentalistic" aspects and focus on logical aspects. Second, I take the "experiment" part of "thought-experiment" seriously: the imagined situation may be distinguished from the claims about what takes place in the imagined situation. It is perfectly possible to imagine, with Swineshead, that a rod of uniform density is approaching the center of the universe through empty space, and to inquire after its behavior—and indeed to disagree with Swineshead's claims about what would transpire.

Taken together, these two considerations suggest a general characterization: a thought-experiment consists of a set of sentences D which are to be understood as a description of a situation, together with the claim that D describes a case in which ϕ, where ϕ describes "what happens" in the situation.[24] We may treat thought-experiments solely on the logical level by taking p to be the sentence describing the characteristic ϕ, so that (for example) Swineshead may be understood as giving a description D, a set of sentences describing the case in which a rod of uniform density approaches the center of the universe through the void, and then proposing that in a non-trivial sense p, a sentence describing the behavior of the rod, "follows from" D. No loss of generality is involved in the linguistic move.

These points made, the obvious question remains: how does one determine what happens in a thought-experiment, which sentences follow from which descriptions—and indeed what sense of "following from" is relevant here? Mediaeval philosophers took the question seriously and consciously addressed the philosophy of their scientific method. The mediaeval writings on *obligationes* are the philosophical reflections on their practice of constructing thought-experiments and arguing on their basis; the literature on *obligationes* is the metamethodology of mediaeval science.

V. Obligationes

A typical *obligatio*[25] has the formal characteristics of a debate or mediaeval dispute: there are two parties, an opponent and a respondent. The opponent begins by laying down some claim, a proposition such as "Socrates is running." His action is called *positio*, positing, and what he posits is the *casus*, the case, also known as the *positum*.[26] The respondent admits the case—or, if he does not, there is no dispute—and then the opponent proceeds to put forward (to propose) other propositions. To each proposition, the respondent either concedes, denies, or "doubts" its truth; he is "obliged" to give certain responses in accord with rules; hence the name *obligationes*. The point of the exercise, if there is an identifiable point, is to trap the respondent in a contradiction; an obligational dispute explores "what happens" given the *positum*.[27]

Obviously, much turns on the rules according to which proposed sentences are conceded, denied, or doubted. And this is the material which makes up the bulk of the literature on *obligationes*. Some fairly obvious rules can be stated: any proposition which is a direct consequence of the *positum* must be assented to, and any proposition the negation of which is a direct consequence of the *positum* must be denied. Beyond that, there is no general agreement about the correct rules. For example, it seems a minimal condition that no one dispute includes a conceding by the respondent of a sentence and of its negation, whether at the same stage or at different stages. But it is less clear that the same response should be given to a sentence no matter when it is proposed. For example, a proposed sentence which was doubted at an earlier stage might, when proposed again, be conceded, due to additional information gained in the course of the dispute. Of the many philosophical debates over the proper rules of *obligationes*, three in particular show that writings on *obligationes* constitute a theory of thought-experiments.

First, some philosophers argued that the *positum* need not be possible. This corresponds, in thought-experiments, to reasoning *per impossibile*. A lively debate ensued in the *obligationes*-literature whether anything whatsoever followed from a contradiction; the general consensus seemed to be that the spread of a contradiction is limited, although no precise criteria could be stated; many authors put severe restrictions on their use.[28] Regardless of how this rule was applied, though, it seems that ordinary propositional logic was a component of obligational disputes: disjunction introduction, conjunction simplification, detachment—in short, all the standard rules of propositional logic, enhanced by mediaeval logical and semantic theory—governed the relation between sentences at different stages of an obligational dispute. Hence the standard practice of mediaeval physicists, and indeed the standards of intellectual rigor in general, could be subsumed under *obligationes*. For example, the standard practice of deducing consequences of a theory as a check on the acceptability of the theory falls under propositional logic; thus *reductio ad absurdum*, appealing to logical incompatibility, as well as Buridan's three examples, appealing to commonly-held beliefs, can be understood in obligational terms.

Second, there were philosophical debates over how the respondent should treat proposed sentences which are not logical consequences of the set of sentences composed of the *positum* and the sentences such that each was either conceded or its negation denied. Such sentences were called *impertinens,* independent, and a debate among Walter Burleigh, Richard Kilvington, and Richard Swineshead arose as to their proper treatment.[29] The standard view, which we may call "Burleigh's Rule," held that such sentences should be treated as they are in the actual world: if known to be true, conceded; if known to be false, denied; otherwise doubted. To see why Burleigh's Rule was adopted, note that an obligational dispute may be viewed as asking what would happen were a given condition, specified in the *positum*, to obtain. What sort of reasoning takes contingent facts in the actual world as relevant to determining what would happen under a given condition?

The answer: counterfactual reasoning, taking "counterfactual" in its broad modern sense as any subjunctive conditional. When the *positum* is not impossible, each stage in an obligational dispute can be seen as constructing and narrowing down a class of possible worlds as similar to the actual world as is compatible with what has been conceded and denied at earlier stages.[30] This interpretation may be stated more exactly: let D

stand for the set composed of the *positum* and sentences either conceded or sentences whose negation was denied at any earlier stage of an obligational dispute, and let *p* be a newly-proposed sentence; then to this stage of the obligational dispute there corresponds a counterfactual of the form $\ulcorner D \mathbin{\Box\!\!\rightarrow} p \urcorner$. This view is confirmed by noting that, so construed, *obligationes* share most of the properties commonly associated with counterfactuals.[31] Hence *obligationes* include the logic of counterfactuals, just as they include propositional logic. Yet the logic of counterfactuals is not the whole of *obligationes*, as has been claimed, for, aside from the difficulties with impossible *positio* (corresponding to *per impossibile* forms of reasoning) noted above, it is a substantive claim that the distinction between the abstract and the concrete matches the distinction between the possible and the actual. While it might be true that all possibilia are abstracta, the converse surely does not hold; abstract entities need not be possible "beings" in any ordinary sense. The ideal objects of geometry, for example, need not exist in any possible world. This is perhaps most clear in the case of metaphysical speculation. To take a standard mediaeval example, based on Aristotle, *Metaphysics* VII.iii (1029^{a}10-20): imagine two individuals of the same species, such as Socrates and Plato, and strip away from each all non-essential properties; once the level of the essence has been reached, how many essences are there—one or two? This is a perfectly valid thought-experiment and was often stated in the language of *obligationes*, but there is no possible world in which the mere essences of things, unencumbered by their non-essential properties, exist.[32] Therefore *obligationes* include the logic of counterfactuals, but only as a proper part.

Third, several authors allow for distinct species of *positio*, in cases in which the *positum* includes explicit semantic content. These varieties were useful in investigating logical and linguistic issues. One example of this is the kind known as *institutio* or *impositio*, in which a new meaning is stipulated for an expression; the question is what logical relations are preserved, for example "'God' stands for Brunellus the Ass" or "The sentence uttered by Socrates is false." Other forms include explicit reference to the respondent: for the kind called *petitio*, the respondent is said to concede, deny, or doubt some claim, for example, "You conceded that the King is in Paris"; for the kind called *sit verum*, propositional attitudes, typically stipulating the state of knowledge of the respondent, are included, for example, "You know that the King is seated or not seated." Each of these three kinds of *positio* explicitly involve the expressive form of the sentences initially used to describe the situation in question, allowing for an investigation of the paradoxes of self-reference, substitutivity in semantically opaque contexts, the function of indexicals and terms sensitive to semantic context, epistemic and doxastic logic, and the like.

The general conclusion I want to draw should be apparent: the literature on *obligationes* express the mediaeval theory of thought-experiments, including various forms of reasoning which at first glance appear to have little to do with one another. A thought-experiment is construed as a set of sentences D which describe a situation, and a proposed sentence p which is said to follow from D, then the concern with other forms of *positio* is readily intelligible. While it is usually convenient to talk directly of the imagined situation, using the material mode, no harm is done by retreating to the formal mode to talk about the sentences describing the situation, and this allows philosophical questions to be raised about the description itself: questions involved in the philosophy of logic and language. Furthermore, when the *positum* is possible, the way in which the proposed sentence is said to "follow" from the earlier stages of an obligational dispute is clear; it or its negation either follows deductively or counterfactually. Finally, impossible *positio* takes account of *per impossibile* reasoning, and justifies methods of indirect proof such as *reductio ad absurdum*.

VI. Obligationes and Metamethodology

To return to the question raised in II under what conception of physics is the Mean-Speed Theorem naturally grouped with logical and semantic puzzles such as the Liar, problems about knowledge and belief, anaphoric reference, propositions evaluated with respect to instants and limits, functional extremes, and the motion of bodies in space? The answer: a conception of physics as an Aristotelian *scientia* which proceeds by way of thought-experiment. The whole battery of concepts traditionally associated with modern science is simply absent: testing, experimentation confirmation, induction, statistical projection, repeatability, and the like. If *obligationes* literature is of the metatheory of philosophical method, then we should expect to see in its armamentarium features relevant to various kinds of philosophical reasoning: and that is just what it provides.

We can distinguish two versions of this conclusion, however. The weak version is that the theory of *obligationes* provides the logical basis for a metamethodology; the strong version is that it was consciously understood as providing the logical basis for a metamethodology. The weak version is undoubtedly true: the uniformity of the terminology of *obligationes* in mediaeval philosophical literature, and especially scientific literature,[33] the character of the philosophical debates over the proper rules of *obligationes*; the indifference to testing and experiment combined with an almost obsessive use of counterexamples, distinction, and argument—all these testify to the truth of the weak version. And, in a sense, this is sufficient for my thesis; characterizing a paradigm in which intellectual activity took place need not depend on the contemporary understanding of the paradigm; there is indeed some reason to think that we, at a historical remove and free from its influence, can more accurately describe the paradigm than a mediaeval physicist could.

The mediaeval understanding of induction (ἐπαγωγῆ or *inductio*), which prompts the intellect to grasp first principles, offers further support. Aristotle describes induction at the start of the *Topics* as beginning from ἐνδόξα, that is, beliefs which are "plausible" in the sense that they are held by everyone, or by the majority, or by the wise (either all of the wise, the majority, or the most famous).[34] What is more, Aristotle explicitly says that dialectical reasoning is the way in which the first principles are learned (*Topics* I.ii 101^{a}37-^{b}3): "since the principles [of a science] are primary to all else, they must be arrived at in each instance with regard to the commonly-held beliefs (ἐνδόξων); this process belongs especially or most fittingly to dialectic." The mediaeval theory of dialectic is given in treatise *de obligationibus*. Hence the method involved in the intellectual grasp of first principles is systematically explored in the write on *obligationes*, which writings therefore are the metamethodology of mediaeval science.

While the weak version is not in doubt, direct evidence for the strong version is harder to find; treatises *de obligationibus* rarely draw connections to philosophical practice; practicing physicists rarely draw attention to their method. Until further research is accomplished, I can only offer some general considerations in favor of the strong version. When discussing examples used by other philosophers or their treatment of counterexamples, mediaeval philosophers often have explicit recourse to the rules of *obligationes*, though not at the methodological level. The many members of Merton College who wrote on *obligationes* never saw fit to draw the connection between these treatises and their research in physics.[35] Furthermore, throughout the history of treatises *de obligationibus* no explicit statement of their philosophical role ever appears, a fact which has lead to scholarly confusion and doubts about their purpose and function; some scholars have tried to infer the purpose of *obligationes* from examining their historical origins.[36] Yet it would be a mistake to infer from their historical origins that in the fourteenth century they were not consciously understood as the metamethodology of *scientia*. Even at the very beginning of their history treatises on *obligationes* were closely connected with the study of fallacies, insolubles, and sophisms; the earliest treatises date from the first decades of the thirteenth century, that is, when translations of the *Physics* began to be circulated and studied in earnest.[37] It is not until the first half of the fourteenth century that the number of such writings begins to increase, at which time they increase dramatically. I think that the proper explanation of this fact is that *obligationes* were somewhat of a "found tool." They developed out of the logical milieu of the late twelfth century, when philosophers were trying to assimilate the *Topics* and the *De sophisticis elenchis* and combine their understanding of these texts with the native tradition in logic which had evolved in the preceding century. Historically this took place as the medieval universities were being organized, and it readily became apparent, with the academic monopolization of knowledge, that *obligationes* would rationalize this development. The treatise on *obligationes* bear the stamp of their origin, but were used in the fourteenth century as the general support for mediaeval philosophical and scientific practice.

VII. Conclusion

The Duhemian tradition claims that the components of modern science are found or prefigured in earlier mediaeval scientific writings: a claim which, at the very least, grossly distorts the facts. There are features of this claim I would not dispute; there is a genuine similarity between mediaeval and modern authors in the vocabulary used and concepts at issue; if problems can survive radical changes in scientific paradigm, I am willing to concede that there is a continuity of problems as well—free fall, the nature of motion and speed, the analysis of force and resistance. But these similarities should not conceal the deep divergence between mediaeval *scientia* and modern scientific method. I hope to have indicated some of the complexities on the mediaeval side; I would expect a closer look at Galileo to show that his procedure was more "mediaeval" in this respect than has generally been acknowledged. To some extent these are non-issues: who anticipated whom is neither historically pressing nor philosophically interesting. But this dismissal conceals the deeper point of the nature of the mediaeval scientific paradigm and its historical relation to the beginnings of modern science, which is, I take it, both historically pressing and philosophically interesting.

The method of thought-experiment has virtues as well as vices. It is peculiarly well-suited for uncovering conceptual incoherencies and inadequacies; it demands a high degree of rigor as well as logical sophistication; it is as precise an analytic tool as can be found. More generally, it seems appropriate for investigating *a priori* truths, such as those found in mathematics and geometry, and perhaps physics. Its vices, especially as a method for scientific knowledge, are only too apparent. While it is a method which supports armchair speculation, there is only so much that can be learned in an armchair, without engaging in concrete practice and activity. Thought-experiments, in their mediaeval use, support theories which have no check or control, no way to test their correctness or incorrectness, as opposed to the modern experimental method. In the pejorative sense associated with the term ever since the Renaissance, any dispute over the correctness or incorrectness of a theory which is isolated from practice is a purely *scholastic* question.

NOTES

1. See Pierre Duhem, *Études sur Léonard de Vinci: ceux qu'il a lu, ceux qui l'ont lu,* 3 vols., Paris 1906-1913, and *Le système du monde: histoire des doctrines cosmologiques de Platon à Copernic,* 10 vols., Paris 1913-1959. He concisely summarized his views in the *Rendiconti della Reale Accademica dei Lincei* (Classe

scientia, fisica, matematica) Vol. XXII (1913), p. 429: "In the fourteenth century, the Masters of Paris, having rebelled against Aristotle's authority, constructed a dynamics entirely different from that of the Stagirite; the essential elements of the principles thought to have received mathematical expression and experimental confirmation from Galileo and Descartes were already contained in this dynamics.... Galileo, in his youth, read several of the treatises where these theories were presented." Marshall Clagett, echoing these sentiments, has written that "Galileo was clearly the heir to medieval kinematics" (*The Science of Mechanics in the Middle Ages*, Wisconsin 1959, p. 666, hereafter abbreviated as *SM*). Research into Galileo's knowledge of mediaeval science continues; see A. C. Crombie, "Sources of Galileo's Early Natural Philosophy" in *Reason, Experiment, and Mysticism in the Scientific Revolution*, edited by M. L. R. Bonelli and W. R. Shea, New York 1975, and William A. Wallace, "Galileo Galilei and the *Doctores Parisienses*" in *New Perspectives on Galileo*, edited by R. E. Butts and J. C. Pitt, New York 1983. Duhem's portrait of Galileo as a pure experimental physicist is undoubtedly idealized—some would say mythologized—but it is the contrast between the method of mediaeval physics and modern experimental method which interests me, and accordingly I will ignore the dubious historical accuracy of Duhem's claims on behalf of Galileo. When I speak of the "Duhemian tradition" I have in mind the works of Duhem, Maier, Crombie, and Clagett, again prescinding from individual differences.

2. John Murdoch, no believer in the Duhemian orthodoxy, has written: "It would be an error to regard these new and distinctive fourteenth-century efforts as moving very directly toward early modern science...the whole enterprise of many a medieval scholar who treated motions was worlds away from that of Galileo and his confreres [*sic*]...medieval discussions of motion should not be viewed solely as providing some kind of background from, or against which, early modern thinking about motion developed" (John Murdoch and Edith Sylla, "The Science of Motion," in *Science in the Middle Ages*, edited by David Lindberg, Chicago 1978). This sentiment seems to me to be exactly correct, lacking only a positive alternative conception of mediaeval physics.

3. The restriction is non-trivial. There was a great deal of practical knowledge, and even theoretical knowledge, about scientific matters during the Middle Ages which was not recognized as *scientia* and hence not subject to pressures that it conform to a paradigm, for example alchemy and pharmacology. My argument is directed at the *academic* conception of *scientia* current in the fourteenth century, discussed in §3 below. Astronomy provides a good example of the kind of split I have in mind: as an academic discipline, astronomy was classified as a branch of mathematics, primarily concerned with what we should call celestial mechanics; its debates were largely geometrical, working out the ramifications of the theoretical system of epicycles or concentric spheres. Such theories only took into account very general phenomena as constraints and were rarely tested against experience. However, astronomical matters were connected to practice in three ways: for purposes of navigation, whether exploratory, commercial, or military; for calculating the date of religious feasts; for the popular though officially disapproved uses of astrology. Each of these practical needs spurred the collection of observations, keeping of records, adoption and improvement of technology (such as the astrolabe and the sextant), and collectively accounted for whatever scientific progress there was in mediaeval astronomy. To some extent, the mediaeval science of optics (*scientia perspectiva*) is similar; it too was classified as a branch of mechanics, largely concerned with trigonometry, but had practical connections.

4. William Heytesbury (1305?-1372/3) states the Mean-Speed Theorem in his 1335 *Regulae solvendi sophismata* (on fol. 40v of the 1494 Venice incunabulum of Heytesbury's works); a better version of the text in which the theorem is stated is

given in Clagett, *SM* pp. 277-83. No proof is offered in the *Regulae*, but a proof is offered in the *Probationes conclusionum*, a separate treatise also attributed to Heytesbury appearing in the 1494 Venice incunabulum. (The attribution to Heytesbury is not secure: see Curtis Wilson, *William Heytesbury: Mediaeval Logic and the Rise of Mathematical Physics*, Wisconsin 1956, p. 210, for a statement of the doubts.) The Mean-Speed Theorem is also stated by two contemporaries of Heytesbury: Richard Swineshead (fl. 1340-1355), in the fourteenth chapter, entitled "*Regulae de motu locali*," of his 1350 *Liber calculationum* (printed in 1477 Padua and 1520 Venice incunabula; Clagett gives a better text, *SM* pp. 298-304), states the theorem and offers four proofs; John Dumbleton (fl. 1340-1350), in his *Summa logicae et philosophiae naturalis* (extant only in manuscript, but text given in Clagett, *SM* pp. 317-25), states the theorem and offers a distinct indirect proof—one quoted verbatim by Giovanni Marliani in the late fifteenth century (Marshall Clagett, *Giovanni Marliani and Late Medieval Physics*, New York 1941). Swineshead and Dumbleton were, like Heytesbury, members of Merton College at Oxford; in consequence, the theorem is sometimes called the "Merton Mean-Speed Theorem." It may, however, be an exaggeration to think there was a distinct school of thought, the "Mertonian School," to which these thinkers, along with Richard Kilvington and Thomas Bradwardine (and Walter Burleigh as an honorary member), belonged; see Edith Sylla, "The Oxford Calculators" in *The Cambridge History of Later Mediaeval Philosophy*, eds. N. Kretzmann, A. Kenny, and J. Pinborg, Cambridge 1982 (hereafter abbreviated as *CHLMP*), pp. 540-63.

5. Bradwardine (1295?-1349) states his general rule governing the ratio of proportions, which has come to be known as "Bradwardine's Function," in his 1328 *Tractatus de proportionibus velocitatum in motibus*, edited and translated by H. Lamar Crosby, *Thomas Bradwardine, His Tractatus de proportionibus: Its Significance for the Development of Medieval Physics*, Wisconsin 1951, p. 112.

6. Jean Buridan (fl. 1330-1360) argues for the theory of impetus in his *Quaestiones subtillissimae super octo Physicorum libros Aristotelis* VIII q. 12, printed in a 1509 Paris incunabulum and reprinted as *Johannes Buridanus: Kommentar zür Aristotelischen Physik*, Minerva G.M.B.H. Frankfurt-am-Main 1964; other discussions by Buridan are thoroughly explored in Annaliese Maier, *Zwei Grundprobleme der scholastischen Naturphilosophie*, 3rd edition, Rome 1969, pp. 101-28.

7. I regularly translate *velocitas* by "speed," rather than by "velocity," since modern physics construes velocity as a vector, and speed as a scalar, quantity. The claim of Marshall Clagett, "Richard Swineshead and Late Medieval Physics," *Osiris*, Vol. IX (1950), pp. 131-61, that Swineshead in fact distinguishes between the quantity of motion and the direction of motion, seem to me a strained reading of the text, and in any event is not sufficient to show that one and the same thing, *velocitas*, possessed both magnitude and direction (i.e. is a vector).

8. The mathematical formalization of the Mean-Speed Theorem, taking d as distance over the interval [a,b], S as speed (evaluated at the extrema of the interval), and t as time, is:

$$d = [S_a + (S_b - S_a)/2]t$$

Since the difference $S_b - S_a = at$ (where a is the acceleration), this reduces to the familiar kinematic equation:

$$d = S_a + at^2/2$$

Where the initial speed is zero, as in the case of dropping a body, the result is:

$$d = at^2/2 \text{ or, more generally, } d = \alpha t^2$$

which may be applied directly to the case of free fall. However, the first reference to the free-fall application of the Mean-Speed Theorem appears only in Domingo de Soto's 1555 *Quaestiones super octo libros Physicorum Aristotelis* (Salamanca 1555 incunabulum fol. 92v), not in the High Middle Ages. See Pierre Duhem, *Le système du monde* (*op. cit.*), Vol. III, p. 128ff.

9. Mediaeval mathematics was not well-equipped to deal with exponential variation. Marshall Clagett, in *SM*, p. 675, says of Bradwardine's Function that "it foreshadowed the differential equation used so universally in modern mechanics." Annaliese Maier, in *Ausgehendes Mittelalter*, Vol. I, Rome 1964, pp. 425-57, is more emphatic: "Without a doubt it was Thomas Bradwardine who...anticipated to the greatest extent the methodology of the natural science of the future."

10. Marshall Clagett, *SM*, p. 523; on p. 525 he says "it can scarcely be doubted that *impetus* is analogous to [Newton's] inertia." This sentiment is representative. See also, for example, Pierre Duhem, *Le système du monde* (*op. cit.*), Vol. 2, p. 154; Alexandre Koyré, *Etudes Galiléennes*, 3 vols. (Actualités Scientifiques et Industrielles, Nos. 582-54), Paris 1939, Vol. 3, pp. 113-42; Annaliese Maier, *Die Vorlaufer Galileis im 14. Jahrhundert*, Rome 1949, p. 137ff. The comparison to Galileo is based on the content of Galileo's claims about *impeto*, while the comparison to Newton is made plausible due to Buridan's loose formulation: if *impetus* varies in proportion to the quantity of matter and varies in proportion to the speed, then it seems to follow that it varies in proportion to the product of the quantity of the matter and the speed—which is taken as the analogue of the Newtonian analysis of momentum as the product of mass and velocity. (The comparison with Newtonian inertia is also supported on the strength of Buridan's claim that it is an enduring quality.)

11. James Weisheipl, in "Ockham and Some Mertonians," *Mediaeval Studies*, Vol. XXX, pp. 196-97, has expressed doubts about Heytesbury's veracity, given the high level of logical sophistication displayed in the *Regulae*, but Heytesbury is quite explicit on this point: "*...vestrae sollicitudini iuvenes studio logicalium agentes annum primum prout facultatis meae administraret sterilitas...*" (on fol. 4va of the 1494 Venice incunabulum).

12. An edition of Kilvington's *Sophismata* is being prepared by Norman and Barbara Kretzmann; my information is derived from a typescript of their work, for which I am grateful. Kilvington attacks the notion of instantaneous speed in Sophisms 33/34.

13. A modern way to put his point would be to say that $S = d/t$ which is undefined for $t=O$, i.e. over *no* interval. This complaint is echoed by others. Marsilius of Inghen, author of the *Quaestiones in octo libros Physicorum Aristotelis* previously attributed to Duns Scotus (and appearing in the Wadding-Vivés edition of his works), says in Book q. 5 "instantaneous motion is neither fast (*velox*) nor slow [i.e. has no 'speed' at all], since these are only defined with respect to [an interval of] time." One of Kilvington's arguments suggests taking an arbitrarily small interval, where the point at which the speed is to be determined is intrinsic to the interval; no matter how small the interval chosen, the absolute value of the difference of the functional extrema is nonzero; and so, Kilvington concludes, the notion of "instantaneous speed" is incoherent. If he had taken the difference in ratio to the length of the interval, he would be well on the way to the differential calculus, since mediaeval mathematics possessed the machinery of limit points and convergent sequences. Heytesbury does not address the question.

14. In the *Probationes* a special case of the Distance Theorem is proved on the basis of the Mean-Speed Theorem, namely the claim that a body with constant acceleration and beginning from rest will traverse three times as much distance in the second half of the interval of time as in the first. The generalized form of the theorem, that in equal periods of time the body will traverse distances as proportionally related by the series of odd numbers 1, 3, 5, 7,...was stated and proved by

Nicole Oresme in his *Quaestiones in libro Euclidis Elementorum* (MS Vaticana, Chis.F.IV.66 fol.29rb, text given in Clagett, *SM*, p. 344, n. 13). Galileo's statement and discussion of the Distance Theorem is given in his *Discorsie dimonstrazioni matematiche intorno a due nuove scienze* (eds. A. Carugo and L. Geymonat, Turin 1958), at the beginning of the Third Day (p. 212 in the standard Edizione Nazionale system).

15. There is good science (analytic chemistry) and bad science (phlogiston theory); there is also non-science (geometry) and pseudoscience (astrology). The distinction, no matter how imprecise and difficult to draw, is nevertheless real. The Kilvington-Heytesbury debate is non-science rather than bad science or pseudoscience; it is non-science because there are no links to experimental control.

16. Mathematically, the rejected theory holds that $S \propto (F - R)$. Versions of this theory were held by earlier philosophers, notably Philoponus, Avempace, Averroes, and later by Galileo in his youth; see Ernest A. Moody, "Galileo and Avempace: The Dynamics of the Leaning Tower Experiment," *Journal of the History of Ideas*, Vol. XII (1951), pp. 163-93 and pp. 375-422.

17. Nicole Oresme (1325?-1382), *De proportionibus proportionum*, edited by Edward Grant, *De propor-tionibus proportionum* and *Ad pauca respicientes*, Wisconsin 1966, pp. 384-87. Oresme develops the notion of "irrational" properties of proportions, i.e. where $(a/b)^x = c/d$ only if x is irrational, arguing that it is mathematically probable that any two proportions chosen from a set of unknown proportions will be irrational, the larger the set the greater the probability; hence the force-resistance proportions governing the motion of celestial bodies are likely irrational, and so, by Bradwardine's function, their speeds are incommensurable, entailing that the celestial bodies never occupy the same position twice. But this last result contradicts the basis of predictive astrology, which must therefore be unable to predict. In fairness to Oresme, it should be noted that there was no way in fact to determine the proportionality constants required: there was no access to celestial phenomena, and there were no instruments capable of such measurements.

18. Marshall Clagett, *SM*, pp. 563-64.

19. I use the term "intervention" as a catch-all to describe the characteristics of modern scientific experimental method: the design of an experiment, including the separation and identification of the relevant variables; the use and development of technology to bring about conditions for an experiment; the measure of reliability of the equipment; the conditions for repeatability, including the control for generality in the sample, and actual repetition to the level required; the notion of statistical confirmation and statistical projection of laws, entirely absent in the mediaeval period; and other features—concerning which see the the second part of Ian Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science*, Cambridge 1983.

20. Both examples come from the *Regulae*: in the first case, a body continuously increases in acceleration such that "it has every imaginable degree of speed all the way up to an infinite degree of speed *secundum imaginationem*" (fol. 43vb); in the second case, a magnitude is diminished incrementally at a constant rate until there is no quantity, and "although this is not possible strictly speaking [i.e. not physically possible], still it may well be permitted for the sake of the argument, since it does not include a contradiction" (fol. 48va: *quamvis enim hoc non sit possibile de virtute sermonis tamen ex quo casus non claudit contradictionem satis poterit admitti gratia disputationis*). These passages, and the one cited in the next paragraph, are discussed in Edith Sylla, "The Oxford Calculators" in *CHLMP*, pp. 557-60; she is following work done by John Murdoch, who, more than any other scholar, has called attention to the *secundum imaginationem* procedure of mediaeval physicists. See especially John Murdoch, "Philosophy and the Enterprise of

Science in the Later Middle Ages" in *The Interaction Between Science and Philosophy*, ed. Y. Elkana, London 1974, pp. 64-70, and John Murdoch and Edith Sylla, "The Science of Motion" in *Science in the Middle Ages*, ed. David Lindberg, Wisconsin 1978, pp. 246-47. The "imaginary" character of Heytesbury's work is also discussed by Curtis Wilson, *op. cit.*, pp. 24-25. Gaetano di Thiene, whose commentary on Heytesbury's works is printed with the text in the 1494 Venice incunabulum, describes the method concisely: "even though the posited cases are not naturally possible, nevertheless they are imaginable without contradiction, whence they are to be granted by the logician" (fol. 89ra commenting on the *Sophismata: licet casus nunc positi de facto non sunt possibiles, sunt tamen imaginabiles absque contradictione, quare a logico admittendi*; see also his remark on fol. 48va). Swineshead, too, remarks of several cases that "all of these are conceded imaginarily and denied *de facto*" (see Sylla, *art. cit.*, p. 562; *Liber de calculationibus*, fol. 15ra of the 1520 Venice incunabulum).

21. Heytesbury mentions this case in his *Sophismata* at fol. 170va, and explicitly describes it as impossible: *dato per impossibile quod esset aliquod corpus alterabile quod nullam caliditatem nec frigiditatem heberet....* See Sylla, *art. cit.*, p. 560, who says of this case that Heytesbury "is interested in performing thought-experiments, but he is unconcerned with even their theoretical realisability." Similar cases are discussed by Swineshead (see the preceding note).

22. This view is not explicitly stated, but is inferred on the basis of two passages in *Posterior Analytics*, II.xix: at 100^b2-3 Aristotle writes that "we must become familiar with the first [principles] by induction (ἐαγωογῆ)," and at 100^b8-9 he writes "Since nothing can be truer than scientific knowledge (ἐπιστήμη) except ηοῦs, it must be voûs,which grasps the first principles." The standard translation of the *Posterior Analytics* during the Middle Ages, erroneously attributed to Boethius (e.g. in *Patrologia latina* Tom.LXIV, ed. J. P. Migne, Paris 1847, 721D-761B), was in fact made by James of Venice, q.v. Lorenzo Minio-Paluello, "Iacobus Veneticus Grecus: Canonist and Translator of Aristotle," *Traditio*, Vol. VIII (1952), pp. 265-304; in his translation the cited passages read: "*Manifestum est quoniam nobis prima inductione cognoscere necessarium est*" and "*Nihil verius contingit esse scientiae quam intellectus; intellectus utique erit principiorum...*" (from *Aristoteles latinus*, Vol. IV.1-4, ed. Lorenzo Minio-Paluello, Bruges-Paris, Desclée de Brouwer 1968, p. 106.14-15 and p. 107.1-2 respectively). Neither the translations nor the original Greek force the construal of voῦζ, or *intellectus* as some form of "intuition," that is, as an independent faculty through which knowledge is gained. (There is indeed some doubt that this interpretation, though standard, is correct for Aristotle: see *Aristotle's Posterior Analytics*, translated with notes by Jonathan Barnes, Clarendon Press 1975, pp. 256-57.) However, the standard mediaeval interpretation of the passages took *intellectus* as a source of non-discursive knowledge. Robert Grosseteste, author of the standard commentary on the *Posterior Analytics* during the Middle Ages, glosses these passages by describing the "intellectual vision" of the first principles: the *mentis aspectus* is directed upon them (*Robertus Grosseteste: Commentarius in Posteriorum Analyticorum libros*, ed. Pietro Rossi, Florence 1981, p. 406.71). Any mediaeval philosopher reading the text would have had Grosseteste's commentary present to hand. For a discussion of the mediaeval understanding of *inductio*, see §5 below.

23. That is, "*cognitio enim veritatis in aliqua rerum solutio est dubitatorum.*" This claim comes from Siger of Brabant's preface to his question on the *Liber de causis*, only recently recovered, edited by A. Marlasca in *Les "Quaestiones super librum De causis" de Siger de Brabant*, Louvain 1972, prohemium p. 35. Similar sentiments are expressed by (for example) Henry of Brussells; see Martin Grabmann, "Die Aristoteleskommentare des Heinrich von Brussel und der Einfluss Alberts des Grossen auf die mittelalterliche Aristoteleserklärung" in

Sitzungsberichte der Bayerischen Akademie der Wissenschaften, Heft 10 (1943), p. 82. These passages are noted and the disputational character of mediaeval education discussed in Anthony Kenny and Jan Pinborg, "Medieval Philosophical Literature" in *CHLMP*, pp. 11-42.

24. This characterization is adapted from Aron Edidin, "Philosophy: Just Like Science Only Different," *Philosophy and Phenomenological Research*, Vol. XLV (1985), pp. 537-52. Edidin does not use it as a characterization of thought-experiment, but as the form of "philosophical intuitions" when presented with cases for consideration, which he develops as a candidate for contemporary metaphilosophy; he does not mention mediaeval philosophy, but much of his discussion is easily adapted for our purposes.

25. The description of *obligationes* is based primarily on Walter Burleigh; see Romuald Green, *The Logical Treatise "De obligationibus": An Introduction with Critical Texts of William of Sherwood and Walter Burley*, The Franciscan Institute 1982. It seems likely that the treatise Green here attributes to William of Sherwood is in fact by Burleigh as well: see Paul Spade and Eleonore Stump, "Walter Burley and the *Obligationes* Attributed to William of Sherwood," *History and Philosophy of Logic*, Vol. IV (1983), pp. 9-26. The best general introduction to *obligationes* is given by Paul Spade and Eleonore Stump, "Obligations," in *CHLMP*, pp. 315-41.

26. This is a deliberate simplification. Technically, the *casus* was a stipulation outside the context of the disputation about the nature of reality, while the *positum* was an element in the disputation which may or may not conform to the *casus* as described. In similar fashion, and for the same reasons, the description of the start of an obligational disputation is simplified: no account is being taken here of *depositio* and *dubitatio*, in which the *positum* is held to be, respectively, false and doubtful. This last simplification dominates my discussion of the rules of *obligationes*, since it was an open philosophical question whether the rules for *despositio* ought to mirror those for *positio*, to say nothing of what the proper rules for *dubitatio* should be.

27. That *obligationes* explore what "happens" given the *positum* is explicitly stated in the early literature: the *Tractatus Emmeranus de falsi positione*, ed. L. M. De Rijk, "Some Thirteenth-Century Tracts on the Game of Obligation," *Vivarium*, Vol. XII (1974), p. 103.22-23, says that when falsehoods are posited they should be conceded to see what happens—*indigent positione ut concedantur et videatur quid inde accidat.*

28. William of Ockham, in his *Summa logicae* (eds. Philotheus Boehner, Gedeon Gál, Steven Brown, *Guillielmi de Ockham: Opera philosophica* Tom.I, Franscican Institute Press, 1974), p. 739, offers two restrictions: first, not all the rules for *positio* hold when the *positum* is impossible, second, only a sentence whose impossibility is not obvious can be laid down as a *positum*. The second restriction rationalizes the first, since it is clear that the obligational dispute beginning with an obviously impossible sentence would be short and uninteresting. Nevertheless, such cases have to be taken into account, because otherwise the legitimacy of proof by *reductio* would be open to question.

29. Walter Burleigh, in Green, *op. cit.*, §§3.14-3.16; Richard Kilvington, in his *Sophismata* (forthcoming, eds. N. Kretzmann and B. Kretzmann); Richard Swineshead—who is not to be confused with Roger Swineshead, mentioned above—in his *Obligationes*, edited by Paul Spade, "Roger Swyneshed's *Obligationes*: Edition and Comments," *Archives d'histoire doctrinale et litteraire du moyen âge*, Vol. XLIV (1977), pp. 243-85. During the middle of the fourteenth century, Robert Fland described two currents of thought in the literature on *obligationes*, the "old view," associated with Burleigh, and the "new response," associated with Richard Swineshead (ed. Paul Spade, "Robert Fland's *Obligationes*: an Edition," *Mediaeval Studies*, Vol. XLII, 1980, pp. 41-60); Kilvington's position is best seen as interme-

diate between Burleigh and Swineshead. For simplicity I concentrate on Burleigh's Rule. Kilvington's position is not clear, but he argued that the order of the proposed sentences should make no difference to the response given (*Sophismata* §47). Swineshead went farther and argued that any proposed sentence should be evaluated only in light of the *positum*, disregarding all other sentences (*ed. cit.*, §2). Both Kilvington's and Swineshead's positions allow for inconsistency of a fairly strong sort: in the course of an obligational dispute, the same proposed sentence may be conceded at one stage and denied at another stage. Burleigh's Rule does not permit this, and seems to have been a far more popular view. Burleigh's Rule is discussed in detail by Eleonore Stump, "The Logic of Disputation in Walter Burley's Theory of Obligations," *Synthese*, Vol. LXIII (1985), pp. 355-74.

30. The "counterfactual interpretation" of *obligationes* has its origins in a lecture delivered by Norman Kretzmann at the NEH Summer Institute on Mediaeval Logic, held at Cornell University in 1980, and has received definitive formulation in Paul Spade, "Three Theories of *Obligationes*: Burley, Kilvington, and Swyneshed on Counterfactual Reasoning," *History and Philosophy of Logic*, Vol. IV (1981), pp. 89-120. I am indebted to both for my understanding of the counterfactual interpretation, and rely on Spade's exposition for my discussion.

31. For example, (i) strengthening the antecedent fails; (ii) transitivity fails; (iii) contraposition fails—each of which is characteristic of the logic of counterfactuals. Furthermore, assuming omniscience on the part of the respondent (so that *dubitatio* drops out), (iv) conditional excluded middle holds, which is a part of some modern systems of counterfactuals. However, on the most obvious reading of Burleigh's Rule, strengthening the consequent also fails, which is *not* a desirable feature. See Spade, *art.cit.*, pp. 100-3.

32. The latter claim is made explicitly by several authors, e.g. Walter of Mortagne (TQG i.3) and Peter Abailard (LI I.ii.14), who argue that such unencumbered essences could not exist; they are careful to use the subjunctive formulation. There are further difficulties if one is wedded to the modern understanding of possible worlds. The general difficulty is that modern modal systems are not strong enough to distinguish between the possibility of something and its possible being, since the possibility of something just is its being in some possible world. This makes nonsense of Anselm, for instance; see Peter King, "Anselm's Intentional Argument," *History of Philosophy Quarterly*, Vol. 1 (1984), pp. 147-66, in which this issue is discussed at greater length.

33. Mediaeval philosophical literature of this period is of two kinds: text-directed, such as a commentary, gloss, or paraphrase/abbreviation, and independent treatise. There seem to be four major styles of philosophical writing: exegesis, *quaestiones*, *disputationes*, and sophismata. (There are exceptions: opuscula such as Aquinas's *De ente et essentia*; letters and sermons; dialogues, works with literary formats. But most philosophical literature of the fourteenth century is in one of the four major styles.) Commentaries, even those highly bound to the text, include both exegesis and *quaestiones*, and it is common to find a commentary consisting only of *quaestiones*, such as commentaries on Peter Lombard's *Sententiae*. Independent treatises tend to be handbooks or *summae*, which often consist in *quaestiones* (e.g. Aquinas's *Summa theologiae*); recorded disputations, either as *quaestiones disputatae* or *quaestiones quodlibetales*; or sophismata. The simple structure of each is as follows: a *quaestio* begins with a question, then cites arguments, quotations, or examples supporting positive and negative answers to the question, after which the issues underlying the question are discussed, distinctions are drawn, and an answer endorsed, closing with the resolution of the earlier material supporting the other answer to the question; a disputation has much the same form, except that it begins with a thesis and the issues are treated at greater length and evolved in a systematic way; sophismata begin with a sentence (the "sophisma-sentence" in

Kretzmann's terminology) and a stipulation about the case, followed by plausible arguments both for the truth and the falsity of the sentence, after which the issues are discussed, distinctions drawn, a resolution proposed, and one set of arguments replied to. The internal differences in structure, while real, nevertheless do not affect the claim that each of these is rationalized by the theory of *obligationes*, since they share the procedural feature of argument, counterexample, refutation, proof, and the like.

34. The passages in question are *Topics*, I.viii 103^b3-7 and I.xiii 105^a35-^{b}3, the latter reading in Boethius's translation (standard in the Middle Ages): *aut omnium opiniones proponenti aut plurium aut sapientum et horum vel omnium vel plurimorum vel notissimorum* (ed. Lorenzo Minio-Paluello, *Aristoteles latinus*, Vol. V.1-3, Bruges-Paris, Desclée de Brouwer, 1969, p. 19.19-21). This is precisely the sense in which Buridan appeals to experience: he relies on beliefs which are held by everyone or by the majority ("what everybody knows"), with recourse to the opinions of philosophical authorities (the "wise").

35. See the introduction to Green *op. cit.* for a list and discussion of the Mertonians who wrote *obligationes*.

36. Aside from a logic of counterfactuals, discussed above, *obligationes* have been called on all-purpose philosophical tool by Eleonore Stump in "Roger Swyneshed's Theory of Obligations," *Medioevo*, Vol. VII (1981), pp. 169-74; as containing "a nucleus of rules for an axiomatic method," according to Philotheus Boehner, *Medieval Logic: An Outline of its Development from 1250 to c. 1400*, Manchester 1952, p. 14; as "ingenious exercises among schoolboys [having] little objective value," in the words of James A. Weisheipl, "Developments in the Arts Curriculum at Oxford in the Early Fourteenth Century," *Mediaeval Studies*, Vol. XXVIII (1966), pp. 163-64, who is followed in this view by Green and De Rijk; as a method of testing logical skills, according to Alan Perreiah, "What Obligations Really Are," *Medioevo*, Vol. V (1979), p. 123, as a rudimentary theory of dialectic, or perhaps of dialogue-logic, by Eleonore Stump, "The Logic of Disputation in Walter Burley's Treatise on Obligations" *op. cit.*, and by C. L. Hamblin, *Fallacies*, London 1970; and so on.

37. Translations of the *Physics* appeared as early as ca. 1150, but it was only in the first quarter of the thirteenth century that serious efforts at philosophical assimilation took place. Only four treatises on *obligationes* are known to date from the thirteenth century, and more precise dating is a matter of scholarly dispute: (i) the *Obligationes Parisienses*; (ii) the *Tractatus Sorbonnensis de petitionibus contrariorum*; (iii) the *Tractatus Emmeranus de falsi positione*; (iv) the *Tractatus Emmeranus de impossibili positione.* Each has been edited by L. M. De Rijk in a series of articles entitled "Some Thirteenth-Century Tracts on the Game of Obligations," *Vivarium*, Vol. XII (1974), pp. 94-123, Vol. XIII (1975), pp. 22-54, Vol. XIV (1976), pp. 26-49.