

Why Don't Mediaeval Logicians Ever Tell Us What They're Doing? Or, What Is This, A Conspiracy?¹

Paul Vincent Spade
Indiana University

I

What I want to talk about here is a puzzle for historians of philosophy who, like me, have spent a fair amount of time studying the history of mediaeval logic and semantic theory. I don't know how to solve it, but in various forms it has come up repeatedly in my own work and in the work of colleagues I have talked with about it. I would like to share it with you now.

Reduced to a caricature, the puzzle can be stated like this: "Why don't mediaeval logicians ever tell us what they're doing?" Like any good caricature, the question so stated no doubt exaggerates the problem. But, again like any good caricature, it is based on a reality. And the reality in this instance is a very mysterious phenomenon indeed. Let me elaborate.

First, some historical background. Mediaeval logic developed in several stages. In the early period, it was mainly a matter of preservation. Boethius's translations of Aristotle and Porphyry, together with his commentaries and his other logical writings, and together with a few writings from other authors, transmitted the logical lore of late antiquity to the early Middle Ages — sometimes in garbled form, but sometimes with considerable sophistication.

It wasn't until the second quarter of the twelfth century that things began to change. Peter Abelard represents the culmination of this "Old Logic," as it came to be called. And, in fact, in his fertile mind the seeds of what would be important new developments were already beginning to sprout. But it was only after Abelard that the characteristically "mediaeval" contributions to logic began to develop in earnest. Whole new genres of logical literature began to emerge, treatises devoted to exotic sounding topics like "ampliation and restriction," "insolubles" and "syncategoremata." This "*logica modernorum*" — the "logic of the mod-

¹ This paper was prompted by reflection on an offhand comment made by Professor Norman Kretzmann during the 1980 Summer Institute on Medieval Philosophy, held at Cornell University under the auspices of the Council for Philosophical Studies and the National Endowment for the Humanities.



erns”, as it was called — grew and flourished until the end of the Middle Ages, and for that matter well beyond.

This much of the story has been told many times. But what has *not* been told — or at least has not been told often enough — is the fact that, in case after case of the new logical genres that grew up in this later period, *we simply don't know what was really going on*.

We do know the important texts, in most cases. We know the mechanics and details of many of the new logical theories and techniques that emerged during this period. We even know of theoretical disagreements that arose in some of these theories, and we know by and large who lined up on which side of these disagreements. We know in fact quite a lot about the logic of this period. But what we too often don't know is: *Just what did they think they were doing?*

Let me give you four examples in some detail. Then, at the end, I will step back to look at our puzzle once again, from a broader perspective. The examples will not require any specialized knowledge on your part, just a clear head and normal native good sense. You may not follow all the steps on the fly, because I'm afraid it will all come rather quickly. But that doesn't really matter for now. (There will be no quiz at the end.) And I'll bring it all back to the main point in the end.

II

Exhibit A: The theory of “*obligationes*.”

One of the new genres of literature that grew up in the *logica modernorum* was something called “*obligationes*.” I've talked about *obligationes* in various contexts and venues in the past, and I have no intention of getting into all the gruesome details here. Instead, let me just paint in very broad strokes.

The “*obligationes*”-literature consisted of treatises describing the rules for a special kind of disputation, called an “obligation” or “*obligatio*.” And what was an obligation? Well, first of all, despite the name, it didn't have anything especially to do with ethics or morality. (So the theory of “*obligationes*” is not the mediaeval version of what we now call “deontic logic.”) Rather, an “*obligatio*” was a stylized disputation-form that involved two parties, the “*opponens*” and the “*respondens*” — the “opponent” and the “respondent.” Those of you familiar with European academic customs may recognize this terminology. In fact it is my view that, whatever else they may have been, the mediaeval disputations *de obligationibus* are remote but direct ancestors of the modern-day academic “thesis defense.”² In some European universities, it is still the custom to this day that in

² See my “Opposing and Responding: A New Look at *Positio*,” *Medioevo: Rivista di storia della filosofia medievale* 19 (1993), pp. 233–270, where this view is first worked out.



the oral defense of a thesis or dissertation, one member of the examining committee is designated the “*opponens*” — a kind of “Grand Inquisitor” — and the poor, hapless candidate is called the “*respondens*.” But while that connection is undeniably there, the situation is more complicated than that.

Typically, an “*obligatio*” began with the opponent’s setting out a claim, called the “*positum*.” (‘*Positum*’ or ‘*positio*’, of course, is just Latin for the Greek ‘*thesis*’. Nevertheless, despite this terminological coincidence, we must not think of the *positum* as the “thesis” the candidate or “respondent” brings to the defense, since it’s the *opponent*, not the respondent, who gets to pick the *positum*.)

In any event, the *positum* set out by the “opponent” could be almost any proposition, although it was usually something false, or at least something not *known* to be true. Some authors even allowed the *positum* to be not just false but outright *impossible*. Disputations starting from such a *positum impossibile* are especially interesting cases that have not yet been studied fully.³

After the initial setting up of the *positum*, the disputation then proceeds with the opponent’s “proposing” — not “positing,” this time, but “*pro*-posing” — a series of statements to the respondent, one after another. To each such proposed statement, the respondent had to reply according to certain rules, by *conceding*, *denying* or *doubting* it.

What were the rules? Well, without going into details, the usual rules required the respondent to frame his replies so that they be **(a)** consistent with the *positum*, and furthermore **(b)** consistent with everything he had responded previously in the disputation. (Obviously, special rules had to be devised for cases where the *positum* is impossible, since nothing whatever is consistent with an impossibility.) In cases where the *positum* and the respondent’s previous replies underdetermined the responses he had to give, he was required to answer according to his knowledge of the actual facts in the real world — granting what he knew to be true (that is, *really* true, not just true under the hypothesis of the *positum*), denying what he knew to be false, and “doubting” (in effect, suspending judgment on) everything else.

Now this set of rules gives obligatory disputations some very interesting formal properties, which have been studied at some length over the last twenty years. These properties are strikingly reminiscent of what is nowadays called “counterfactual” reasoning — “what if” reasoning, “thought experiments.”⁴ And

³ Recent work by Christopher J. Martin, at the University of Auckland, has gone a long way to rectify this situation.

⁴ See my “Three Theories of *Obligations*: Burley, Kilvington and Swyneshed on Counterfactual Reasoning,” *History and Philosophy of Logic* 3 (1982), pp. 1–32. See also Mikko Yrön-suuri, *Obligations: 14th Century Logic of Disputational Duties*, (“Acta Philosophica Fennica,” vol. 55; Helsinki: Societas philosophica fennica, 1994); and Peter King, “Mediaeval Thought-Experiments: The Metamethodology of Mediaeval Science,” in Tamara Horowitz and Gerald J.



once we become sensitive to the characteristic vocabulary of these disputations, we begin to notice the peculiar terminology of *obligationes* turning up all over the place, in lots of other sorts of mediaeval academic literature as well — not just in logical treatises devoted explicitly to the topic, but in discussions of metaphysics, for example, or physics or theology — anyplace, in fact, where “what if” reasoning is appropriate.⁵ Certain authors’ commentaries on the *Sentences* of Peter Lombard, for instance, abound with this vocabulary. And Archbishop of Canterbury Thomas Bradwardine’s († 1349) famous *De causa dei* is in effect a long, sustained obligational disputation directed against the Ockhamists who, he thought, were latter-day Pelagians, heretics on the question of the role of free will and grace in salvation.

For now, you don’t really need to know anything more about the mechanics of these disputations. Still, what exactly was their purpose? Notice that, oddly, there doesn’t seem to be anything at issue in such “disputations,” nothing really “in dispute”; at the end of one, even if it is conducted with perfect correctness in accordance with the rules, the respondent hasn’t defended the *positum* in any real sense or done anything that would tend to show it is true. Remember, we often know at the outset that the *positum* is false — and in some cases even impossible! So the *positum* is not really being “argued,” and it is hard to see anything else that is really being argued either. At the end of an obligational disputation, it appears that no theoretical question is settled by the disputation, and in fact that none has even been asked! What then is going on?

Faced with this oddity, modern scholars who looked at this literature at first suggested that such disputations were nothing more than schoolboy exercises, that they were meant for practice or examination, nothing more. And there’s no doubt something to that. There does seem to be a link with the modern “thesis defense,” after all. Again, some of the sample disputations in the mediaeval treatises are such that the respondent would definitely have to keep his wits about him, so that he would certainly be “exercised” — in a sense. Besides, there do survive university statutes from the Middle Ages that required undergraduates to take part in disputations *de obligationibus*. So they were not merely fictional constructs of a particular logical genre; such disputations actually occurred. (Yet, oddly, although we possess many, many logical treatises *describing* the form, we don’t have a single text that actually purports to preserve one of these live obligational disputations, as we do for “quodlibetal” or “disputed” questions.) Again, some of the treatises from the period actually say outright that one of their purposes was to see whether the respondent “has the art” or skill — in other words, to test him.

Massey, eds., *Thought Experiments in Science and Philosophy* (Savage, Md.: Rowman & Littlefield, 1991), pp. 43–64.

⁵ Spade, “Opposing and Responding.”



But to test him for what? As we've just seen, it's not purely argumentative skill at proving a point or defending a thesis, since there seems to be nothing really being defended or proved in the disputation. It's not even quite logical skills in general that are being tested, because the rules appeal at crucial points to the respondent's knowledge of actual facts, not merely to the logical connections among propositions. Those features of the rules are left completely unmotivated if their purpose is to test or exercise purely logical skills.

No, if we look closely, it seems that all that is really being exercised or tested by an obligational disputation is the respondent's skill at disputing according to the rules of obligational disputations! And while that's no doubt true, it's also tautological, and so doesn't clarify very much.

Furthermore, the main problem with the idea that the purpose of these disputations was primarily pedagogical rather than theoretical is that it simply cannot be reconciled with another odd fact about the literature. For it turns out that there was something of a hot controversy in the mid-fourteenth century over competing sets of rules of *obligationes*. The rules I sketched for you a moment ago became more or less "standard" ones, but several other sets of rules were offered too, and there was some dispute about which ones were right. Whatever this controversy was about, it *certainly* wasn't about what made for good pedagogy! No author ever suggests that the reason for adopting one set of rules rather than another is that they would make a better examination or would provide a more thorough "work out." In fact, no one ever gives any reason at all for adopting one set of rules rather than another. All they say is in effect, "Those other rules are wrong, because *these* are the rules instead."

Well, modern scholars have offered lots of suggestions to explain this literature. (In fact, I've made something of a small cottage industry of it!⁶) But the fact remains, none of us really knows, and we're all just speculating in desperation.

To me, the odd and striking thing about this situation is that the mediaeval authors themselves don't tell us! They either say nothing at all about their purpose, or else what they do say is vacuous — to see whether the respondent "has the art" — and so is of no help at all.

That's Exhibit A.

⁶ See my "Roger Swyneshed's *Obligationes*: Edition and Comments," *Archives d'histoire doctrinale et littéraire du moyen âge* 44 (1977), pp. 243–285; "Obligations: Developments in the Fourteenth Century," Chap. 16, Part B, of Norman Kretzmann, *et al.*, ed., *The Cambridge History of Later Medieval Philosophy*, (Cambridge: Cambridge University Press, 1982), pp. 335–341; "Three Theories of *Obligationes*" (as in n. 4 above); "If *Obligationes* Were Counterfactuals," *Philosophical Topics* 20 (1992), pp. 171–188; "Opposing and Responding" (as in n. 2 above); and "The Logic of 'Sit verum' in Richard Brinkley and William of Ockham," *Franciscan Studies* 54 (1994–97), pp. 227–250.



III

Exhibit B: The theory of “exposition” and “exponible” propositions.

As early as the twelfth century, logicians began to analyze certain propositions with misleadingly simple appearances. For example, ‘Socrates is beginning to speak’ looks like a straightforward affirmative present-tensed proposition. But it carries within it a concealed negation, since to say Socrates is beginning to speak implies, at least on one standard reading (there were others), that he wasn’t speaking earlier, but is now. For the same reason, the proposition carries with it a concealed reference to some time other than the present — other than the time conveyed by the present tense of the verb ‘is’. So the proposition is more complicated than it looks on the surface, and logicians became very adept at spinning out the implications of such cases and the traps they conceal.

By the fourteenth century, this practice had become codified into something called the theory of “exposition.” Propositions like this came to be called “exponibles” — that is, they can be “expounded” into several other propositions, called their “exponents.” Here, for example, is how William of Ockham describes the situation in his *Summa logicae*.⁷

You need to know that any categorical [proposition] from which there follow several categorical propositions [that are] so to speak its “exponents” — that is, they express what the proposition conveys by its form — can be called a proposition *equivalent* to a hypothetical proposition.⁸

Notice how Ockham puts this: the exponents “*express what the [original] proposition conveys by its form.*” This suggests that “exposition” is something like logical or philosophical “analysis,” that its purpose was to clarify things, to make the obscure plain, make the implicit explicit, to “set out” — *exponere* — the logical form of the original proposition. And that interpretation is certainly all right as a first approximation; it’s no doubt on the right track. Indeed, it was the obvious plausibility of this interpretation that was partly responsible for the fact that from the late ’60s to the middle-’80s, philosophers of the so called “analytic” persuasion discovered that mediaeval logic — any by extension mediaeval philosophy in general — might be intellectually respectable after all.

But although this understanding of the literature is on the right track, there are problems. And the problems show that, despite the undeniable plausibility of

⁷ *Summa logicae* II.11 (OPh I.279.5–8).

⁸ That last expression is Ockham’s name for what others, and what he himself elsewhere, call an “exponible” proposition. ‘Hypothetical’ here doesn’t mean a “conditional” proposition, but rather a compound proposition — in this case a conjunction, joined by ‘and’.



this picture (it *is* on the right track), this isn't *exactly* what was going on. As a result, we are left pretty much in the dark about just what was really going on instead.

Notice, in the quotation I just read you Ockham says each of the exponents "follows" from the original exponible proposition. If the exposition is complete, therefore, so that the exponents exhaust "what the [original] proposition conveys by its form" (as Ockham puts it), then this means the original exponible is equivalent, in some fairly strong sense, to the conjunction of all its exponents. But this in turn means that the denial of an exponible is not itself exponible; by De Morgan's Law, it will be equivalent to the disjunction (not the conjunction) of the denials of the original proposition's exponents. (If I'm *not* walking and chewing gum at the same time, then I must be *either* not walking *or else* not chewing gum at that time.)

Some authors explicitly acknowledged this fact (that "exponibles" are equivalent to the conjunction of their "exponents"), and recognized that the denials of exponible propositions are therefore not exponible, and so do not have "exponents," but instead have what they called "causes of truth." These "causes of truth," just as we would expect, turn out to be the denials of the original exponible's exponents, and are related disjunctively. That is, any one of them will suffice to make the denial of the exponible true; you don't need all of them together.⁹ (So if the exponible 'Socrates is beginning to speak' is expounded as and amounts to 'Socrates *was* not speaking earlier, and now he *is* speaking', then 'Socrates is *not* beginning to speak' must amount to '*Either* Socrates *was* already speaking earlier, *or else* he is *not* speaking now'. And that last is a disjunction, not a conjunction, so that it cannot be an "exposition" of the proposition 'Socrates is not beginning to speak' — at least not if "expositions" always amount to conjunctions.)

Now, remember the obvious and appealing view that "exposition" amounted in effect to a kind of logical or philosophical "analysis"? Well, if that's what it was, then what is the purpose of this apparently arbitrary restriction to propositions that amount to conjunctions? What reason is there to think that superficially simple propositions that conceal a more complex logical form and so are in need of analysis are always concealing conjunctions? Or what reason is there to be especially interested in, and *to write a whole genre of logical literature about*, only those that do? Once again, just what is going on here?

It gets worse. If exposition is even anything like the recent notion of logical or philosophical analysis, we would expect the full exposition of a proposition

⁹ E.g., Richard Lavenham. See the discussion in my "Five Logical Tracts by Richard Lavenham," in J. Reginald O'Donnell, ed., *Essays in Honour of Anton Charles Pegis*, Toronto: Pontifical Institute of Mediaeval Studies, 1974, pp. 70–124, at pp. 84–85.



to be more logically perspicuous, more explicit, than its unexpounded original. But what then are we to do with the following case?

Ockham tells us that the proposition ‘A white [thing] runs’ has as its exponents ‘Something runs’ and ‘A whiteness is in it’.¹⁰ Is that any clearer than the original? Does it make explicit some hidden logical complication in the original proposition? Notice that the exposition has the relative pronoun ‘it’ in the second exponent (‘A whiteness is in *it*’), taking as its antecedent the subject of the first exponent (‘Something runs’). The original proposition had no such complication; it was simply ‘A white [thing] runs’. Do we increase logical or philosophical clarity, do we make anything more explicit, by adding anaphoric cross-reference where there seems to have been none before? Hardly! Anaphora is a notorious complication in semantic theory.

Claude Panaccio, in an excellent paper that appeared in *The Cambridge Companion to Ockham*,¹¹ suggests that, “Actually, the main job of the *exponentes* [= exponents] in Ockham’s semantics, to be judged from the numerous examples he gives, is to render conspicuous the *ontological* import of the propositions under analysis.” That is, he suggests, the point is an ontological one, not a matter of logical clarity; its purpose is to uncover the ontological presuppositions of what the proposition claims, not its logical structure. In the present instance, the concrete adjective ‘white’ in the original proposition ‘A white [thing] runs’ is replaced by the abstract noun ‘whiteness’ in the exposition, which in a way, I suppose, does make quite explicit the ontological commitment of saying that the thing is white — there must *be* an entity called a “whiteness” in order to make the original proposition turn out true.

Well, this is perhaps a good way to read Ockham, but what are we to do with the following example from another author, a certain Richard Billingham in the mid-fourteenth century? He tells us that the proposition ‘Every man runs’ is expounded as ‘A man runs and nothing is a man unless (= *quin*) it runs’.¹² Here there is surely no gain in *ontological* explicitness. And, as for being logically “clearer” — well, not to mention the new anaphoric cross-reference in the exposition, anyone who has ever learned Latin knows that sentences with *quin*-clauses in them are harder than plain old sentences like ‘Every man runs’. If anything, it’s

¹⁰ *Summa logicae* II.11 (OPh I.281.55–56).

¹¹ Claude Panaccio, “Semantics and Mental Language,” in Paul Vincent Spade ed., *The Cambridge Companion to Ockham*, (Cambridge: Cambridge University Press, 1999). Emphasis added.

¹² L. M. de Rijk (ed.), *Some 14th Century Tracts on the Probationes terminorum: Martin of Alnwick O.F.M., Richard Billingham, Edward Upton and Others*, (Nijmegen: Ingenium, 1982), p. 55 § 34. See E. J. Ashworth and Paul Vincent Spade, “Logic in Late Medieval Oxford,” in J. I. Catto and T. A. R. Evans (ed.), *The History of the University of Oxford*, vol. 2: *Late Medieval Oxford*, (Oxford: Clarendon Press, 1992), Ch. 2 (pp. 35–64), at p. 44.

the *quin*-clause that is complicated. No, from the point of view of “analysis,” this exposition seems to be a net loss.

One last example. Richard Lavenham, a rough contemporary of John Wyclif in the second half of the fourteenth century, expounds the comparative proposition ‘You are stronger than a man’ as ‘You are strong, and a man is strong, and it is not the case that a man is *as* strong *as* you’ — which turns out to be false, since it would falsely imply that you are not as strong as yourself. The point of this exposition seems to be to replace the comparative degree of the adjective (‘stronger than’) by a negative ‘as ... as’ construction plus the positive degree (‘*not as* strong *as*’). And that is an interesting logical point; it breaks down comparative constructions into positive constructions plus negation.

But then Billingham turns around and goes just the other direction, expounding the ‘as ... as’ construction in the affirmative ‘You are *as* strong *as* a man’ by a negative comparative: ‘You are strong, and a man is strong, and no man is *stronger* than you’.¹³ There is no gain in ontological explicitness either way, and if the point is supposed to be increased clarity or explicitness we end up in a vicious circle.

So what is going on in the theory of exposition? Apart from suggestive but unsatisfactory remarks like Ockham’s claim that the exponents “express what the [original] proposition conveys by its form,” our authors themselves have practically nothing to say. Again, I find this fact very puzzling and mysterious.

That’s Exhibit B.

IV

Exhibit C is the theory of “proofs of propositions.” This is not “proof theory” in the sense in which that term is used in modern mathematics and logic. But again, just like Exhibits A and B, what it is instead is not clear at all.

The theory of “proofs of propositions” seems to have got seriously underway around 1350, with people like Richard Billingham at Oxford, whom we have already met. According to the theory — as presented in Richard’s work *The Youths’ Mirror* (= *Speculum puerorum*) — some propositions are “immediate” in that they cannot be “proved” — in whatever sense is at stake in this theory — cannot be “proved” by any more basic propositions, but only by a direct appeal to the senses or the intellect; other propositions are then “proved” (in this sense, whatever it is) on the basis of these “immediate” ones.

The theory recognized three ways by which, or by a combination of which, *non*-immediate propositions could be “proved”: **(1)** by “resolution,” which in effect amounted to expository syllogism (I’ll give you an example in a mo-

¹³ See Spade, “Live Logical Tracts,” pp. 91–92.



ment); (2) by means of something called “*termini officiabiles*,” which I translate as “auxiliary terms” and which seems to have something to do with “propositional attitudes,” or — more generally — with words governing indirect discourse; and (3) by “exposition,” which we’ve already talked about. (Historically, the theory of exposition was older than and seems to have had nothing originally to do with any more general theory like the later “proofs of propositions.” Nevertheless it was incorporated into the latter theory later on.)

Recall that “immediate” propositions were ones that could not be “proved” on the basis of any more basic propositions, but only by direct appeal to the senses or intellect. Immediate propositions are made up entirely of “immediate terms.” And what kinds of terms are “immediate” terms? Well, on the one hand we have personal pronouns like ‘I’ and ‘you’, the demonstratives ‘this’ and ‘that’, indexical adverbs like ‘here’, ‘there’, and so on. These are said to be “immediate” expressions in the sense that their meaning cannot be further explained in any given case except by a brute pointing — which is what Billingham seems to mean by a “direct appeal to the senses.” As for a “direct appeal to the intellect,” Billingham seems to be thinking of verbs, where the immediate expressions are the copula ‘is’ and its tensed or modal variants. These are said to be “immediate,” not in the sense that their meaning can be explained only by pointing, because you can’t do that with these expressions, but in the sense that they can only be grasped directly by the intellect; they cannot be subsumed and categorized under any “broader” predicate.

Well, all right. That’s already murky enough, I suppose. But the real difficulty with the theory of “proofs of propositions” is that it doesn’t seem to fit its own description of itself. The sample “proofs” given in the texts often don’t succeed in reducing things to “immediate” propositions, as the theory says is always possible. And furthermore, there doesn’t seem to be any possible way in which they *could* be so reduced. Let me just give you one example, the proposition ‘A man runs’ (*homo currit*).

Billingham says that this proposition is to be proved by “resolution,” the first of our three ways. And here is his proof: *This runs and this is a man; therefore, a man runs*. (That is the expository syllogism I promised you earlier.)

Now as an argument, there’s absolutely nothing wrong with this “proof”; it’s undeniably valid. It doesn’t of course “prove” anything in the sense of settling something that was previously in doubt. And, in fact, if you’re really in a position to “prove” the proposition ‘A man runs’ that way, you already know *more* at the outset than you end up proving in the conclusion. If you can actually point to a running man, as you do in the premises of that proof, then you not only know that *a* man runs; you can also give a particular example of one.

So the “proof” does not advance our knowledge of runners at all in that sense. But that’s no real objection. Even in our present-day sense of the term, we



often “prove” things we already know. The point of the proof in such a case is not to settle a doubt, but to show us a significant relation between the conclusion and certain other propositions. And Billingham’s proof” might be said to do *that*.

But still, the “proof” does not seem to do what the theory promises. We are supposed to be able to “prove” ‘A man runs’ on the basis of “immediate” propositions containing only “immediate” terms. But look at the premises of the proof Billingham actually gives. The two demonstrative pronouns are immediate expressions, and the second premise contains the copula ‘is’, which is an immediate verb. But the premises also contain the non-immediate words ‘man’ and ‘runs’, which are exactly the same words we started with in the original proposition. What has been gained in terms of immediacy?¹⁴

To be sure, this isn’t exactly an inconsistency, but it does seem to leave an unfulfilled promise. Furthermore, there doesn’t seem to be any way the promise *can* be fulfilled, given the resources of the theory. In Billingham’s example, there are no “auxiliary terms” (*termini officiables*) or indirect discourse. It doesn’t seem that “exposition” is going to get us any closer to immediacy. And, as for “resolution,” the only remaining one of the recognized ways of “proving” propositions, we’ve already gone as far as we can with that. There seems to be no way to “prove” the proposition ‘A man runs’ on the basis of propositions containing *only* “immediate terms,” as the theory says can be done.

So once again: *Just what is going on here?*

Billingham was by no means alone in this enterprise. There were a few precursors, and there was certainly a long line of authors after him who developed and refined the theory of “proofs of propositions.” What did think they were doing? Once again, what they say they’re doing doesn’t fit their actual practice, and what they’re really doing instead is left entirely unexplained. We know of lots of texts on this topic, and I’m afraid they’re all like that.

To some extent the situation here is unlike that of, say, the *obligationes*-literature, where we are fairly clear about the rules and mechanics, but just don’t know the point of it all. Here we’re not yet even sure about the mechanics. Still, our authors are amazingly silent about their starting point and about the purpose of the whole thing.

V

So far, we’ve seen those mysterious *obligationes* disputations, where nothing seems to be really in dispute, and nothing else seems to be tested or examined either. We’ve seen the theory of “exposition,” a kind of logical or philosophical “analysis” that doesn’t seem to gain anything and is in some cases circular. And

¹⁴ For further details, see Ashworth and Spade, “Logic in Late Medieval Oxford,” pp. 42–45.



finally, we've seen the theory of "proofs of propositions," which doesn't fit its own description of itself and doesn't seem to be doing anything else very useful either. Curiouser and curiouser.

My last exhibit, Exhibit D, is the theory of "supposition," which I could talk about for days. (But — I hasten to assure you — I won't.)

The theory of supposition goes back at least to the twelfth century, and in a way is the centerpiece of the *logica modernorum*. (Be warned: whatever 'supposition' means here, it doesn't mean "hypothesis" or "assumption.") Supposition theory seems to have come in two parts, and the relation between them is a dark mystery in its own right. One part of the theory is pretty clear (and I won't be talking about that part); it was a theory of reference, and is *not* the part I want to talk about here. But the other part involved something called "descent to singulars" and, at least in some authors, a corresponding "ascent *from* singulars." And *no one* knows what that was all about.¹⁵ In the proposition 'Every dog is a mammal', to give a quick example, the term 'dog' was said to have a certain kind of "supposition" — it was called "confused and distributive" supposition, and we'll meet it again. Why does it have confused and distributive supposition? Because we can "descend to singulars" under the subject term 'dog' by means of a conjunction, like this: "Every dog is a mammal; therefore, this dog is a mammal, and that dog is a mammal, and so on for all the dogs." That is, by definition, what *counts* as confused and distributive supposition.

Perhaps you can already see why some scholars earlier in this century took this part of supposition theory to be a theory of quantification, treating universally quantified propositions, for example, as amounting to disguised conjunctions (possibly infinite conjunctions) — just as we sometimes do today, if only for heuristic purposes to motivate quantification theory for beginning logic students.

But this won't work, unfortunately. If it were going to work, if these "descents" were supposed to capture the logical and semantic force of quantifiers, then the original proposition you start with would have to be in some fairly strong sense equivalent to the more complicated and expanded proposition you can "descend" to — in my example, to the "conjunction of singulars." But it doesn't always turn out that way.

Consider the proposition 'Some dog is not a terrier'. The predicate 'terrier' here has confused and distributive supposition, mediaeval authors explicitly agreed. As before, we can validly descend to a conjunction of singulars, only this time we descend under the predicate term instead of under the subject. So we get: "Some dog is not a terrier: therefore, some dog is not *this* terrier, and some dog is not *that* terrier, and so on for all the terriers."

¹⁵ I argue the point in "The Logic of the Categorical: The Medieval Theory of Descent and Ascent," in Norman Kretzmann, ed., *Meaning and Inference in Medieval Philosophy*, (Dordrecht: Kluwer, 1988), pp. 187–224.



But that conjunction is *not* equivalent to the original proposition. Let me show you why not.

Now in fact, of course, our original proposition ‘Some dog is not a terrier’ is true, since there are all kinds of dogs besides terriers. But it *could* be false. Suppose by some ecological disaster there were only two dogs left in the whole world — Rover and Fido — and they both are terriers. In that case, the proposition ‘Some dog is not a terrier’ would be false. But the conjunction of singulars we descend to under the predicate would still be *true*: “*Some* dog (namely, Fido) is not *this* terrier (pointing to Rover) and *some* dog (namely, Rover) is not *that* terrier (pointing to Fido) — and that’s all the terriers there are.”

So the original proposition is not in the appropriate sense “equivalent” to the expanded conjunction after all; it’s perfectly possible for the one to be false and the other true. In short, this mysterious second part of supposition theory won’t work as a theory of quantification that is supposed to operate by treating quantified propositions as amounting to the propositions you can “descend” to from them. It just won’t work.

Some early scholars of this literature, once this problem was noticed, argued that even if it doesn’t work, nevertheless that’s what the mediaeval authors intended, and they just got it wrong! In other words, although no one ever put it quite as baldly as this, *they were stupid* — all of them — and stupid about something that’s really fairly easy.

Of course, it’s always possible that this is right. But I’m reluctant to put much confidence in that explanation. In fact, if you think about it, the only appeal this account has is that, if this wasn’t what our mediaeval authors thought they were doing, then *what on earth did they think they were doing?*

That’s just my point: we just don’t know. What *did* they think they were doing?

VI

These then are my four exhibits. It’s time now to draw the moral of the story. There are others exhibits I might have presented to you as well, but these will suffice to make my point. The exhibits I’ve shown you are not unimportant or peripheral parts of late mediaeval logic and semantic theory; on the contrary, they are prominent, and — in the case of supposition theory, absolutely fundamental — parts of the enterprise. Yet, despite our best efforts, we really don’t know what was going on.

This is something I find extremely mysterious. Other branches of mediaeval philosophy still have their murky areas and unanswered questions — yes. That goes without saying, there as in any other branch of scholarship. But it is only here, in this one area of mediaeval philosophy, in late mediaeval logic and semantic theory, that I repeatedly find this fundamental murkiness about even the most



basic starting points. This isn't the case with *all* the topics that come up in late mediaeval logic and semantics, to be sure. But it's true of an astonishing number of them. In case after case, these theories seem to have sprung full-grown from the head of Zeus — without explanation, without motivation, without preparation, without anything to help us figure out what was really going on. The earliest texts we possess already presuppose a well-understood agenda that we are simply not privy to.

How can we explain this?

The obvious suggestions that come to mind won't really do the trick. You might suppose, for example, that the problem is simply that we've just not yet located the earliest relevant texts, where these theories are just getting started and where the motivation is made explicit, that what we're dealing with is examples of later developments of theories to which we still need to find the earliest witnesses.

But on further reflection, this just doesn't seem plausible. People have been working in this field for a rather long time now. We do seem to have located at least the most important early texts, the antecedents of the more developed theories that are so mysterious to us. Of course, that may be presumptuous. But even if we're wrong about this in any given case, it's highly unlikely that we're wrong about it repeatedly — again and again, in fundamental areas of late mediaeval logic and semantics, whereas we're *not* in quite this same puzzling situation with other branches of mediaeval philosophy.

Alternatively, you might hypothesize that what we see here is in effect just testimony to the thoroughgoing oral nature of mediaeval culture, that the theories we're dealing with got started, at the most basic level, in oral conversation — so to speak, over “high tea” — and that by the time anything got set down in writing they were beyond the foundational stages and things could be taken for granted.

Well, no doubt there's some truth to this. But still, it doesn't explain why we find this systematic murkiness again and again here, in this branch of mediaeval philosophy and not so much in others.

I would be very grateful for any help you can give me with this recurring puzzle. Perhaps, as often happens with philosophical problems, the real source of the difficulty is not that we don't know the answers to our questions but that we have the wrong “focus” on things and are just asking the wrong questions to begin with.

Or perhaps this puzzle is not as unique as I suppose to the fields I'm most familiar with. In any event, I remain really, really puzzled!

