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Book Discussion

Critical Study of Amie L. Thomasson, *Ordinary Objects*,
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There are no tables and chairs; stones don't break windows; there may be sculptors, but none has ever made a sculpture; there is only one thing, all other things being merely its parts; or, there are only elementary particles, and they compose nothing. This is how things really are. If we thought otherwise, it is only because we didn't think hard enough.

These and similar views prosper in contemporary philosophy; some even call them 'serious metaphysics'. But Thomasson would have none of that. And in her new book she sets out to save her furniture and the other ordinary objects she holds dear from being reduced to nothingness by hard-headed metaphysicians.

Thomasson discusses and rejects most, if not all arguments currently considered by many philosophers to pose serious difficulties to the claim that ordinary objects exist. She considers, each in a separate chapter, problems of causal redundancy, problems of collocation, problems of vagueness, questions of composition, problems of rivalry with science, and parsimony arguments. Her counter arguments commit her to some kind of analyticity, a commitment that needs defense in our post-Quine post-Kripke age. Thomasson supplies it in the second chapter of her book, which leads to a chapter in which she applies her concept of analyticity to questions of identity, persistence and modality. In a later, related chapter she also discusses the conceptual question of which existence statements make sense. Her constructive conceptual ideas and destructive critical diagnoses lead her, in the tenth, penultimate chapter, to develop a general defense of ordinary objects ontology. And she concludes her book with a general discussion of what metaphysics can, and cannot, achieve.

Thomasson covers a very wide field, and it would be impossible to follow her to all areas she visits. But I think we can at least touch most of the main issues she discusses.

Presupposition-Failure and Composition

The structure of Thomasson's argument against problems of causal redundancy (chapter 1) is similar to that of her arguments against problems of collocation (chapter 4) and against an Occamist parsimony argument (section 9.1). We shall therefore consider here only the first of these issues. As a representative of the idea that ordinary objects are causally redundant and consequently ontologically redundant as well, she quotes (p. 9) van Inwagen (1990, 122):

All activities apparently carried out by shelves and stars and other artifacts and natural bodies can be understood as disguised cooperative activities [of simples properly arranged]. And, therefore, we are not forced to grant existence to any artifacts or natural bodies.

Moreover, if we *disallow* this kind causal overdetermination, then we have an argument *against* ordinary objects. This is indeed Merricks' position (2001, 81; quoted in Thomasson, p. 10):

If there were baseballs, they would break windows ... But given the Overdetermination Argument ... if there were such objects, they would not have causal powers, so there are no such objects.

The breaking of the window would have been caused by both baseball and its atoms, and therefore it would have been overdetermined, if there had been baseballs. And the claim that *both* baseball and 'atoms arranged baseballwise' caused the breaking of the window, concedes Thomasson, sounds amiss.

However, she correctly observes, the problem here is *not* due to the alleged causal overdetermination involved. It is akin to what Ryle called a *category mistake*. She gives examples, taken from Ryle (1949, 17, 22), of statements that are similarly inappropriate yet do not involve causality: There was 'a parade of batteries, battalions, squadrons *and* a division'; 'He bought a left-hand glove and a right-hand glove *and* a pair of gloves'. What is wrong with these statements is 'that conjoining items in a list with "and" ... normally presupposes that the items conjoined are separate and independent, but this supposition is violated in cases like these' (p. 13).

Thomasson now considers two options: (i) statements with presupposition failure are neither true nor false; (ii) they can be true, but they are misleading. (i) If they are neither true nor false, then so is the claim that *both* baseball and atoms arranged baseballwise broke the window, and consequently so is also the claim that the breaking of the window is overdetermined. (ii) If they may be true although misleading, then we should say that the breaking of the window is indeed overdetermined, but that it is misleading to claim that. It is misleading because this is

not ‘*real* overdetermination of the sort that is supposed to be objectionable’ (15). If we look at ‘Kim’s original formulation of the problem of explanatory exclusion, he stated it as the principle that “two or more complete and independent explanations of the same event or phenomenon cannot coexist”’ (Thomasson, p. 15; Kim’s quotation taken from his 1993, 250). But the baseball-explanation and atoms-arranged-baseball-wise-explanation are *not* independent. So we don’t have in this case real over-determination, or over-determination of the problematic sort.

Accordingly, we can either say that in the case of the breaking of the window we do not have overdetermination, or that we do have one, but that it is not of the problematic sort, as the baseball and its constituents are not independent. The threat of overdetermination is dissolved, and we can safely continue to break windows with baseballs.

I think this is excellent, and indeed completely dissolves the argument from causal overdetermination or redundancy (and similarly with the other related arguments mentioned above). Moreover, in her book Thomasson is more thorough and cautious than I could be here, and therefore even more powerful. In dissolving these arguments she has done an important service to contemporary metaphysics.

However, I do part company from her in the diagnosis of the kind of dependence involved between baseball and atoms arranged baseball-wise that renders the overdetermination claim not true or at least misleading. Thomasson suggests that the dependence involved is *analytic*: if there are atoms arranged baseballwise that cause a certain effect then *it follows* that there is a baseball that caused that effect. And we should not allow general overdetermination only if the relation between one object causing the effect and the other one causing it is *not* analytic (p. 16). By contrast, I shall try to show that the relevant dependence is between whole and its parts: if one object is composed of the others, or both are composed of the same objects, then we do not have real causal overdetermination.

The baseball is of course composed of atoms arranged baseballwise. So the case Thomasson considers supports both diagnoses: the analyticity diagnosis and the composition one. Let us therefore look at a case in which we have composition *without* analyticity. Consider the following statement:

- 1 A diamond, *and* carbon atoms arranged in an isometric-hexoctahedral crystal lattice, scratched the glass.

I don’t think the conceptual relation between diamond and carbon atoms arranged in an isometric-hexoctahedral crystal lattice is analytic. We can explain what a diamond is without mentioning carbon at all, and vice versa; and similarly, the explanation of what a lattice is, what is an atom, and so on, also seems to be mutually independent of the explanation of what a diamond is. Yet diamond *is* carbon atoms arranged in an isometric-hexoctahedral crystal lattice. And once we know that, statement (1) seems to be amiss in the same way in which the one about

baseball and atoms-arranged-baseballwise breaking the window was. Thomasson would not be able to apply the analyticity criterion to explain the problem in statement (1), so she would in any case need the composition criterion as well; and this renders her analyticity criterion redundant for the cases she considers.

To check whether the analyticity criterion is not only redundant but actually false, we should consider a case in which it analytically follows that if one entity caused an effect then another one caused it as well, although these entities do not stand in any composition relation (none is constituted of the others, and they are not constituted of the same things). But I don't think there are such entities; so I don't think we can show in this way that Thomasson's analyticity criterion is false. The most we can therefore do, and did with the diamond example, is to show that it is insufficient and redundant. I think this is a good reason for rejecting it.

And there is yet a more general consideration against the analyticity criterion. The relation between entities is analytic *under a description*; change their description, and the relation need not be analytic any longer. But if two things had a common effect, then if there was causal overdetermination there was one *no matter how we describe those things*. So analyticity cannot rule out problematic causal overdetermination. We should not look for a *linguistic* relation between *descriptions* of things, but for a *physical* relation between *things* themselves.

Thomasson did not claim that her analyticity criterion is the only possible one. In endnote 10 (p. 204) she even suggests a constitution relation as a possible criterion. But I have tried to show above that we are justified in *rejecting* her criterion and substituting a composition criterion for it.

Let me, however, emphasize again that my criticism above is not meant as a *rejection* of Thomasson's argument, but as an *elaboration* of it. Thomasson is correct in observing that the problem in the double-counting statements she considers is not due to their causal features, but to a presupposition failure; she is correct in relating it to Ryle's category-mistake examples; and she is right in claiming that such double-counting claims do not show real causal overdetermination, and that therefore they do not constitute a problem to our ordinary objects ontology. I disagree with her only in observing that the presupposition failure is due to factual composition relations, and not to linguistic description ones.

The Defense of Analyticity

Since Thomasson relies on a concept of analyticity, she next turns to defend it from Quine's criticism, as well as from the Kripkean ones. Now I have criticized above her diagnosis, that the presupposition failure in the problematic arguments is due to the analyticity relations. I consequently doubt whether she indeed needs to rely on the concept of

analyticity in her project, or at least on a concept as strong as the one she tries to defend. However, this concept is of interest in its own right; we shall therefore examine Thomasson's defense of it.

The first two sections in Chapter 2 are devoted to a critique of Quine's rejection of analyticity in his 'Two Dogmas'. I don't think that Thomasson adds here to what Grice and Strawson, to whom she refers several times, already did in their 'In Defense of a Dogma'. In fact, her criticism is weaker than theirs, among other things since she omits several important points they make. For instance, Grice and Strawson observe that since Quine admits synonymy of terms that were *introduced* as synonymous by means of definition, his claim that synonymy by *usage* is meaningless doesn't make sense (pp. 206–207).

But Thomasson doesn't need to add anything to what can already be found in 'In Defense of a Dogma': the criticism of Grice and Strawson there is extremely powerful, and they demolish Quine's argument against analyticity. The fact that this has not been acknowledged by American philosophy is probably due to the status Quine acquired as the national philosopher of America, and to the rivalry with the Oxonian philosophy of the fifties. More than half a century since, we should let bygones be bygones and admit that Grice and Strawson did show that Quine's critique of analyticity is ineffective.

Thomasson, again following Grice and Strawson, notes 'that accepting that there is an analytic/synthetic distinction does not preclude us from retaining Quine's positive insight in "Two Dogmas" that confirmation is holistic' (p. 37). She is correct in retaining confirmation holism, but some historical justice needs to be done: the insight isn't Quine's; it is Duhem's. Quine indeed did not read Duhem before he wrote 'Two Dogmas', but we should consider also the following:

Further, it is, in general, impossible to test even a single hypothetical sentence. In the case of a single sentence of this kind, there are in general no suitable L-consequences of the form of protocol-sentences; hence for the deduction of sentences having the form of protocol-sentences the remaining hypotheses must also be used. Thus *the test applies, at bottom, not to a single hypothesis but to the whole system of physics as a system of hypotheses* (Duhem, Poincaré).

This paragraph, with its original italics, is taken from Carnap's *The Logical Syntax of Language* (1934, 318). Quine was of course well acquainted with this work and with its author, who thanks Quine for 'valuable suggestions with regard to terminology' in the preface to the English edition (1937). Quine heard of Duhem's insight from Carnap; the idea subsequently acquired a life of its own in his thought, but he should be credited only with promulgating it, not for its origin. It is *Duhem's Thesis*.

After having dealt with Quine, Thomasson turns to Kripke. According to pure causal theories of meaning, originating in Kripke and Putnam's work, the meaning of concepts is determined by causal relations to the relevant items. Consequently, aprioristic conceptual analysis, of

the kind Thomasson attempts to defend, is generally impossible. Therefore, to show the unacceptability of pure causal theories, Thomasson mentions two problems they face: the *qua* problem and the problem of handling nonexistence claims. These, she claims, show that pure causal theories are wrong, and she suggests some kind of hybrid theory to replace them.

Thomasson first discusses the *qua* problem (sections 2.3–2.4). Now although I too reject Kripke and Putnam’s account of meaning, I do think they can handle the *qua* problem as it is usually presented (e.g. by Devitt and Sterelny, section 5.3). First, the problem: according to causal theories, the meaning of a natural kind term is determined by means of paradigmatic instances. To give the meaning of ‘tiger’ one should point to a few tigers and say that a tiger is anything of that kind. However, these tigers are paradigmatic instances of several natural kinds: of tigers, felines, mammals, animals, etc. Thus, according to causal theories, it should be indeterminate which one of the several natural kinds to which tigers belong the term ‘tiger’ names. But secondly, this problem can be solved, by a slight modification of causal theories, which preserves their spirit: instead of referring only to tigers when determining the meaning of ‘tiger’, one can also refer to paradigmatic *non*-tigers. If one said, Tigers are anything of *this* kind (referring to a few tigers) but not of *this* kind (referring to a lion, say), then ‘tiger’ cannot name felines, mammals or animals in general, otherwise it would name the lion as well. I have elaborated on this in (Ben-Yami, 2001; section 2.1), a paper where my own criticism of the causal theory can also be found, so I won’t expand on the issue here.

But as mentioned above, Thomasson, following Devitt and Sterelny, tries to overcome the *qua* problem by developing a hybrid theory, according to which ‘nominative terms must be associated with a sortal or, more generally, categorical concept’ (p. 39). This sortal does double work: it removes ambiguity by determining to what kind of entity one referred, and also determines the conditions under which the term refers again to the same individual. Thomasson distinguishes between these two functions, calling the former *application criteria* and the latter *co-application criteria*. She emphasizes that it is important to distinguish them, since they are relatively independent (pp. 40–41).

I am not sure, however, of the validity of the distinction. Could something be an elephant for an instant? Or should an animal exist for some time to be considered an elephant? If the latter, then the distinction between application and co-application criteria collapses: Suppose that in order to be an elephant an animal should exist for some minimal interval of time. Then since the application conditions of ‘elephant’ involve a finite time interval, we can, by partially overlapping such intervals, extend the application conditions to any time interval, and in this way derive the co-application conditions from the application conditions. So the validity of the distinction depends on whether an animal should exist for some minimal interval of time in order to be an elephant:

couldn't a wizard conjure up an instantaneous elephant? I think that if we considered such a thing an elephant, we would do that because it resembled ordinary elephants—those animals that *live for years* in the normal course of things. If so, then the application conditions that involve a minimal time interval do have precedence, and one cannot in general distinguish application from co-application conditions.

Turning back to Thomasson's hybrid theory of meaning, such theories face in their turn well-known problems, which Thomasson mentions. In particular, it is difficult to specify what kind of sortal or category our terms presuppose. We can, for instance, introduce a proper name of something without knowing whether it is an animal or an artifact—an example Thomasson herself mentions (pp. 50–51). In this case there are two very different categories of things involved, and the simple sortal theory doesn't therefore seem capable of handling it. Such difficulties push Thomasson into allowing dubious conditional categorical intentions and similar epicycles. In this way her position loses much of its initial plausibility.

The second problem Thomasson discusses is that of handling non-existence claims. Thomasson thinks that an entity 'must exist if it is to be referred to' (p. 46). This assumption is common to almost all contemporary work on names of fictional characters and meaningful non-existence statements. I think, however, that we need not accept it. If we consider what is involved in reference, then there is no reason to limit it to existing things. Only a false picture of reference made philosophers think otherwise. I have discussed it in (Ben-Yami, 2004, *passim.*), so again I just note it here.

Thomasson, in any case, follows Donnellan (1974; missing from her bibliography) in adopting a metalinguistic theory of nonexistence claims, a theory she has defended elsewhere (Thomasson, 2003). Here she adds that because of various additional difficulties involved in non-existence claims, one should add to this position that the reference of proper names generally depends on the sort of entity to which the speaker intended to refer. Here is her example of a use that makes the addition necessary (p. 47):

A sentence like 'Atlantis' doesn't exist' may be true if speakers intended to refer to an ancient city, and the name use chain ends in a creatively decorated nightclub mistaken by a stoned clubber for an ancient city. But if the users and grounder intended to refer to a nightclub, no mistake would have been made; the usage chain would not be blocked but properly grounded, and the nonexistence claim (presupposing uses with that intention) would be false.

But I think these possibilities do not justify associating the term *just* with some high-level sortal ('animal', 'nightclub', etc.). Take a name shared by a fictional character and a real person, say 'Adam' (I assume that the biblical Adam did not really exist). If someone says 'Adam existed!', intending to refer to the biblical Adam, the truth or falsity of his statement depends, according to Thomasson, on the chain to which his

use of the name is related. But here specifying a sortal would not help, since the man asserting Adam's existence obviously thinks Adam was a man, like any real Adam. So the disambiguating work in this case cannot be done by sortals. But since the difficulty seems to be the same as the one in Thomasson's Atlantis example, the disambiguation there also did not depend on the fact that 'nightclub' and 'city' are *sortals*, but on the fact that they are different *descriptions*. And the same point can be made with city names; e.g., Plato's Atlantis versus Florida's Palm Beach. (And one need not consider specifically *existence* claims to demonstrate the point: any sentence with a name the speaker knows to name more than a single thing would do.)

These cases show, in fact, that we need to reject *more* of Kripke's account of names than Thomasson would. *Pace* Kripke, the descriptions we supply *do* show to whom or to what we refer. So my conclusion actually supports Thomasson in her criticism: she should and can reclaim from Kripke more analyticity than she attempted to. But this claim of mine obviously requires more support than I supplied here: we should reconsider Kripke's arguments against the claim that the descriptions a speaker would supply show to whom the speaker refers. I hope to do that on some other occasion.

Perhaps I should summarize my main claims in this section. First, I claimed that Quine's rejection of analyticity was unjustified, as Grice and Strawson have already shown, and that Thomasson does not add anything significant to their criticism. Secondly, I claimed that the *qua* problem can be solved by causal theories of reference. Thirdly, I claimed that Thomasson's distinction between application and co-application conditions is problematic, as is her hybrid theory of meaning. Thirdly, I claimed that the difficulties she spots in Kripke's account of names should be overcome by rejecting more of Kripke's theory than she does, and that she fails to develop the necessary amendment to his theory. So all in all, I do think that the concept of analyticity can be saved from Quine and Kripke, but that Thomasson has failed to supply a significant contribution to this end.

Existence Claims

In chapter 6 Thomasson turns to examine which existence claims and questions make sense, and which do not. 'Which things exist?': this is a typical metaphysical question. But the use of 'thing' here, claims Thomasson, is problematic (section 6.2).

When we ask how many books are there on the shelf, or, how many things did you get for Christmas, the answer to such questions involves no conceptual difficulty. We know what would count as a book, and we know what would count as a thing we got for Christmas. When we count the latter, for instance, we don't count the wrappers, we count the book as one thing and we don't count its pages as separate things, and so on. However, when we move to generic metaphysical existence ques-

tions, ‘thing’—as well as ‘object’ and some other terms—although used as count nouns, lose their grip. ‘Thing’ is then not associated with any application conditions, what would count as a thing and what would not therefore becomes under-specified, and consequently generic questions involving it cannot be answered. As a corollary we see that replies to such questions, replies that seem to disagree, are only in apparent conflict: no reply is in fact possible, because what counts as a thing is under-specified.

The logical tradition seems to argue against this conclusion: in logic, the general form of existence statements is ‘There exists *something* such that...’: we seem to be making a generic use of ‘thing’ here. As the success of logic seems to show that such uses are fine, it therefore seems to follow that there should be nothing amiss in the generic existence statements of ordinary language. Thomasson tries to show that this is not the case in section 6.3. The apparent generic use of ‘thing’ in logic is in fact a *covering* use, she claims, ‘generalizing over at least a wide range of category-specific quantificational claims’. Bare quantification, in which ‘thing’ involves no category, is impossible, since the truth-value of ‘ $\exists xFx$ ’ is determined by substituting proper names for ‘ x ’ in ‘ Fx ’, and ‘singular reference can only be disambiguated to the extent that names, demonstratives, and other singular terms are associated with a category (or categories) of entity to be referred to’.

I find this convincing, and I think it successfully eliminates the currently fashionable talk about ‘absolutely everything’: this is revealed as meaningless, mistaking the count noun ‘thing’ for a name of a kind of thing. And Thomasson even strengthens her arguments in the rest of chapter 6, especially in section 6.6, ‘Can We Revive the General question?’.

Still, I think that something should be added to her criticism of the apparent bare quantification in logic. Where exactly do we assume categories there? We have a domain of discourse containing objects, and interpretation functions correlating every individual constant with an object in the domain: where are the implicit categories? We should reply to this difficulty by noticing a significant difference between logic’s domain of discourse and our world. The domain of discourse in logic is a *set*, which arrives already articulated into objects; but our world is not a set, and it is not so articulated. We distinguish objects in it by means of various concepts and categories. The articulated domain of logic is already a result of carving nature in its joints, or elsewhere, by means of our concepts. Consequently, the use of ‘thing’ in logic implicitly presupposes the use of concepts of various kinds. There is no such thing as a generic use of ‘thing’.

Armed with her analysis, Thomasson proceeds in chapter 7 to discuss composition questions. Van Inwagen (1990), Horgan and Potrč (2000) and others tried to find a general reply to the general composition question, when is one thing a part of another? They failed in their attempt, and ended with skeptical conclusions: more or less nothing is composed

of anything... Thomasson rejects their arguments against local replies to specific composition questions (7.2). She then examines the presuppositions of the general composition question. If 'thing' is here used to cover many classificatory concepts, then the answer is not conceptually problematic and would more often than not be highly disjunctive. But if one tries to make a neutral use of 'thing', then we already saw that this is meaningless. In this way skepticism about ordinary object that derives from a quest after the general relation of composition evaporates into meaninglessness.

She then turns, in chapter 8, to consider the alleged rivalry of common sense ontology with that of science. Eddington famously argued that physics shows that he has no solid table (1928). Thomasson, with Stebbing (1958), relies on the paradigm case argument to assure us that Eddington's table is indeed solid. Moreover, with Ryle, Stebbing and Sellars, Thomasson doubts whether physics speaks of tables at all. She also rejects Sellars's 'science is the measure of all things, of what is that it is, and of what is not that it is not' (1956, 82–3; Thomasson, p. 145): no image of the world, be it scientific or manifest, aspires to be complete, since each makes use of different sortals. And since a neutral use of 'thing' has been shown to be meaningless, no image can even aspire to completeness.

In chapter 9 Thomasson shows, by means of the same argumentative principles we have already encountered, that Occamist parsimony considerations against ordinary objects fail. She also notes, against paraphrastic eliminativist arguments, that since the relation between 'chair' and a paraphrase that makes no use of 'chair' is analytic, the paraphrase in fact guarantees the existence of chairs and cannot therefore be used in support of the elimination of chairs (p. 165). Following Searle (1969, 107), she argues against Quine's ontological criterion, that if a theory does not mention an entity of kind *S* it might still be committed to it: the theory might guarantee that the conditions necessary for the existence of *S* are fulfilled (pp. 166–167). I found these three chapters powerful and convincing.

The Unsung Hero

In the last two chapters of her book, chapters 10 and 11, Thomasson draws some general conclusions, especially methodological ones, from her earlier discussions. In chapter 10 she surveys the previously discussed faults of the arguments against our common sense ontology, and concludes that

the diagnoses offered above ... [purport] to show that what appear as problems for a particular metaphysical view (the view that there are ordinary objects) are in fact no problems at all, resulting as they do only from misunderstandings bred in misuses of language. (p. 180)

She also shows that the common sense view is not committed to 'too many objects' and that neither does it lead to antirealism, as some have suggested.

In the final, eleventh chapter, we then consider the methods of metaphysics. The book concluded 'where most of us started: accepting that there are tables and chairs, sticks and stones' (p. 188). But this is what we should expect from metaphysics: appearances notwithstanding, metaphysics is unlike science, and does not teach us about the world. Metaphysics is concerned with the nature of our concepts, and its methods are those of conceptual analysis. For instance, 'answering questions about identity conditions for entities of various kinds is not a matter of looking deep into the world, but rather must be based on a form of conceptual analysis' (p. 191). For that reason, 'radically revisionary views on any of these topics should be met with suspicion' (p. 191). Thomasson mentions as one such unacceptable revisionary conclusion Currie's 'discovery', that names of paintings 'in fact refer to types of action that can survive the burning of any canvas' (Currie, 1989). I agree with her observations here.

Moreover, philosophical existence questions are also only conceptual. Consequently, unlike scientific theories, we should not decide between competing metaphysical existence claims according to criteria of parsimony, simplicity or explanatory power. There are no 'serious, philosophically resolvable, debates about what exists rather than miscommunications, shallow debates about meanings, or pseudodebates involving incomplete statements and unanswerable questions' (pp. 195-196). '*Apparent* disputes are so rampant,' italicizes Thomasson (p. 198). What one *should* do in metaphysics is either clarify meanings or revise meanings (make them more precise or clear) for specific purposes. Yet these 'are shallow debates about meanings, not deep debates about what *really* exists in the world, or what it is like' (p. 200). When metaphysical debates are not about meanings, they are 'mere pseudodebates about what exists' (p. 200).

I presume the reader has been waiting for some paragraphs now, as I did while reading much of the book, for Ludwig Wittgenstein to make his appearance. Pseudodebates, clarifying meanings in order to remove 'misunderstandings bred in misuses of language', ending where we started, the contrast between philosophy and science, metaphysics unable to teach us about the world: it seems that Thomasson has consulted the *Philosophical Investigations* while writing the final chapters of her book.

And the analogies go even further, into turns of phrase, analogies and the reaction to Moore's commonsensical rejection of skeptic arguments. Thomasson uses at least once Wittgenstein's 'language-game' (p. 184, line 31). The revision of words' meaning for certain specific purposes is compared to the change in rules of a game in order to improve it in various respects (p. 200). To Moore's rejection of skepticism, by claiming that we are much more certain of the existence of tables and chairs than of any philosophical argument that rejects them (1959), Thomasson reacts by maintaining that 'showing how, reflectively, we can make sense of our unreflective common sense worldview is arguably one of the chief

tasks of philosophy' (p. 3). And she returns to a comparison of her approach with Moore's in her final chapter (p. 188). Compare Wittgenstein on the same subject, as recorded by Ambrose (1979: 108-109):

Philosophy may start from common sense but it cannot remain common sense. ... No philosopher lacks common sense in ordinary life. ... You must not try to avoid a philosophical problem by appealing to common sense; instead, present it as it arises with most power. You must allow yourself to be dragged into the mire, and get out of it. Philosophy can be said to consist of three activities: to see the commonsense answer, to get yourself so deeply into the problem that the commonsense answer is unbearable, and to get from that situation back to the commonsense answer. But the commonsense answer in itself is no solution; everyone knows it.

Thomasson might not have been familiar with this paragraph, but it serves to show how close her approach is to Wittgenstein's. The similarities to his methodology and meta-philosophy are striking. And yet, Wittgenstein is never mentioned in the whole book, not even in a footnote. Why?

It is impossible that Thomasson is unaware of the affinity of her ideas and Wittgenstein's. Even philosophers who haven't studied Wittgenstein in any depth are aware of his ideas on the nature of philosophy: we are not dealing here with some obscure figure, but with one of the greatest philosophers of the twentieth century. And a philosopher as learned as Thomasson is surely acquainted with his ideas. Moreover, all along the book, when she has a debt to anyone or notices some similarity in opinion or approach, she acknowledges it. Even if she is not well acquainted with Wittgenstein's philosophy, whose interpretation is notoriously difficult, and she couldn't therefore be sure of the amount to which her ideas approach his, she could have at least noted *that* in some endnote. But the silence is total.

And we cannot credit Thomasson with developing independently of Wittgenstein her methodological and meta-philosophical ideas. Every undergraduate is exposed to them, not to say a philosophy professor. Thomasson's is a re-presentation of Wittgensteinian ideas, often in a less powerful and usually in a less developed form than in the *Philosophical Investigations*.

Is it possible that Thomasson was afraid that had she acknowledged the affinity of her ideas with Wittgenstein's, they wouldn't have received the attention they deserve? Mainstream American analytic philosophy is largely hostile today to Wittgenstein's philosophy, and acclaimed Wittgensteinians would be stigmatized and waved aside without much consideration of their ideas. So is her silence a conscious tactical decision of Thomasson's?

Be that as it may, I am afraid her meta-philosophical ideas will suffer the same fate as those of Wittgenstein. People—philosophers included—usually prefer grandiose fantasies to sober common sense. It is nicer to imagine that you engage in some kind of super-physics (meta-physics), revealing the essence of being and such things, than to

acknowledge that you are removing linguistic confusions without discovering any new fact about the world. For that reason so many mainstream philosophers are antagonistic to Wittgenstein's ideas, and for that reason they will not accept Thomasson's rendition of them either. But I do hope that her book might help a few philosophers to free themselves from the illusions of metaphysics.

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