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**Examiner's report on Matej Drobňák's PhD thesis submission "Language and Meaning in Community" (thesis supervisor: Professor Jaroslav Peregrin)**

The submitted PhD thesis of Matej Drobňák addresses selected topics in philosophy of language and meta-semantics. The first two chapters (after Introduction) focus on the notion of shared language, while the topic of the fourth and fifth chapter is semantic inferentialism, its alleged advantages over other theories of linguistic meaning and its prospects of being grounded in empirical evidence. The driving question of most of the material was whether one can reconcile the tensions between language conceived as a relatively stable and enduring socially shared entity and language used as a tool for daily communication – a tool that is in many respects remarkably flexible.

Let me first comment on the strengths of the work. The thesis is written in a very clear and accessible language. Some parts of the thesis present a submittable material – in fact, Drobňák already published or submitted papers on which the thesis is based to decent journals such as *Philosophia*, *Philosophical Studies* or *Organon F*. Another virtue of the thesis is that it comes to grips with difficult and fundamental problems surrounding linguistic meaning, and some of its proposals are original contributions to international debates. To give an example, I applaud Drobňák's pioneering attempt, in the last chapter of the thesis, to provide inferentialist theories of meaning with resources to accommodate the influence of context on what is being said. Another part of the thesis I read with great interest was the third chapter containing two-pronged criticism of the popular "minimal semantics" programme championed by Emma Borg and other prominent authors. Drobňák convinced me that not just for theoretical but also for empirical reasons this theoretical framework faces serious troubles.

My main misgiving is that the thesis looks more like a collection of loosely connected pieces rather than a properly systematic treatise. In particular, the transition from the third to the fourth chapter is a bit abrupt. The author's explanation (in the Introduction and in the thesis Abstract) of why this is so is, unfortunately, not able to deflect an impression in the reader that a major change of topic occurs in the transition from the third to the fourth chapter.

I will now shortly comment on five claims made in the thesis. Some of my questions could be taken up in the discussion at the defense of the thesis.

(1) On Quine's alleged preference for idiolects (Chapter 2): Drobňák's hypothesis is that Quine came to prefer idiolects over socially shared languages because once one adopts strict behaviorism in explaining how language is learned and used in communication, as Quine did, one cannot determine whether utterances by different people using "shared" language have the same meaning or not. This problem concerns that part of any natural language that goes beyond the level of observation sentences. That is, the problem concerns most utterances of any natural language. My worry is that the move to idiolects does not seem to help much with the problem of meaning determinacy. It seems to me that if *all* that matters to language use is just the publicly observable behavior, then even the notion of an idiolect suffers from the same problem as the notion of a socially shared language. No-one, *including the speaker herself*, can have any certainty as to the semantic determinacy of what is being said. It would thus seem more appropriate to dissolve this problem by abandoning Quine's strict behaviorism (as e.g. John Searle recommends in his "Indeterminacy, Empiricism, and the First Person", *The Journal of Philosophy*, LXXXIV, 1986, pp. 123–146) instead of trying to fix it by moving to idiolects. Unless one presupposes that an individual can privately fix her meanings independently of social practice – a presupposition Quine would be reluctant to make, I believe – the move from shared languages to idiolects buys little explanatory advantage.

(2) I am not sure I understand Drobňák's explanation of how one could deflect Quine's scruples about the sameness of public meaning of standing sentences. As Drobňák puts it, the trick consists in "redirecting the focus of a hearer. If a hearer wants to learn a sentence beyond observation sentences, her aim must be to find a correlation between the occurrence of a sentence and the occurrence of other sentences. Co-occurring sentences, or rather the stimuli caused by their being uttered, can then be understood as external stimuli that must be singled out as the meaning of a sentence" (p. 19). However, no examples of how this process works are

given, so it is hard to evaluate the plausibility of Drobnák's proposal. Moreover, that a sentence A is uttered before (or after) B does not of itself imply that B is deduced from A (or that A is deduced from B), as inferentialism promoted by Drobnák requires. Other principles of association between uttered sentences might be involved in communication. And when Drobnák later admits that the correlated sentences can only be *potential*, not uttered in fact (p. 20), the explanation seems to collapse. One cannot use evidence that is just potentially available but not materialized. In summary, I would like to hear more about Drobnák's proposed strategy and about how exactly it solves "Quine's challenge" for the publicity of meaning.

(3) On p. 20, Drobnák writes that "our acceptance of inferential relations between sentences is shaped by other speakers from whom we learn a language. The acceptance of some inferences has a specific status as it is required as a proof of a basic linguistic competence." Using inference acceptance as a proof of linguistic competence might be a legitimate enterprise but it does not amount to full blown inferentialism. Full blown inferentialism is the claim that accepting some inferences is *constitutive* of speaking a language, not just a way of ascertaining that the speaker is on the right track. Some clarification is, therefore, in order. Does Drobnák subscribe to the full blown version of inferentialism? My impression is that he does, but the quoted remark triggers uncertainty.

(4) On the criteria of "meaning-constitutive inferences": Drobnák intends to flesh this notion out naturalistically, anchoring it in how the majority of speakers within a community uses words (Drobnák speaks about "widely shared" communicative and corrective practices). My worry is that making the correctness of language use thus depend on how the majority of speakers behaves constitutes an impossible standard. How to determine that an inference is accepted by a vast majority of speakers within "my" community? As an individual, I will always encounter only a small sample of inferences (and concomitant corrective behaviors). This limited basis will, on Drobnák's standards, never be enough to assure me that I am using the words of my language in a correct way. I would first need to run empirical inquiry as to how words are widely used in my community.

(5) Minor comment on p. 49: A linguistic community "is a group of people who can smoothly communicate most of the time". Strictly speaking this is incorrect. Two people can smoothly communicate while each of them is using a different language – say, Swedish and Norwegian.

Smoothness of communication is thus too weak a criterion of when linguistic practices constitute a linguistic community.

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Despite my reservations and critical comments, I recommend the submitted dissertation with the tentative grade of pass. **I recommend that the title “PhD” is granted to Matej Drobňák on the basis of this PhD submission**, which is a valuable contribution to the fields of meta-semantics, philosophy of language and philosophy of linguistics.

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