

Sense, Reference, Speech Acts, Norms and Other Issues. Replies to *Organon F* Papers (Part I)

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If I found the first 2015 Supplementary Issue of *Organon F* in the library shelf, I would be struck by its strong assembly of authors: brilliant philosophers and at the same time people whom I have had the pleasure and honour of personally knowing for quite a long time. But I have been presented the volume on an occasion which made it clear that it is somehow (rather intimately) connected with me – despite my repeated pleading not to be reminded about my (regrettable) anniversary. Now, after some time, with all the solemnity belonging to my age, I would like to say how grateful I am for this generous gift to all my friends involved: the authors, the editor (and author of the warm introductory words), the editor-in-chief of *Organon F* and his colleagues. To the latter I would also like to thank for their kind readiness to publish my (tedious but still too brief) reactions on all the papers. I have learned a lot from them, not to speak about the pleasure of an intellectual adventure they involved me into. And although my replies don't – by far – exploit all the impulses they offered, I hope that they at least indicate how inspiring this reading has been for me.¹

¹ In what follows I preserve the order in which the papers appeared. The replies will come out in two parts: the present one focuses on the philosophy of language, the next one (to appear in *Organon F*, 1/2016) on the philosophy of literature.

Peter Pagin²

Peter critically examines an argument I have presented for the first time in Kot'átko (1998). In that paper, like in some later texts, I have focused on Donald Davidson's concept of utterance meaning.³ I took (and still take) it as powerful in two respects: it is applicable to essential part of everyday communication, and it is extremely economical by giving us three valuable things by the same stroke: the general account of utterance meaning (as being constituted by the match between communicative intention and interpretation), the notion of communicative success (as consisting in reaching the match) and the principle of identifying the meaning of particular utterances (an utterance means that *p* if and only if it was so meant by the speaker and interpreted by the audience).

Inspired by these attractions, I have tried to analyze the requirements this triple notion imposes on the correlations between the speaker's and the interpreter's attitudes (those which can be relevant for the choice of communicative strategies)⁴ and found them very modest. I agree with Peter (p. 8) that this should not worry Davidsonians at all: quite on the contrary. But I have argued that the Davidsonian account cannot be generalized for all kinds of communication and that the Trinity I have celebrated above should be dissolved, however blasphemous such a suggestion may appear: what we need is a general account of utterance meaning which allows for various notions of communicative success and various principles of the determination of utterance meanings, both depending on the type of discourse in question or even on specific features of the communicative situations.

As a prelude to this suggestion I have offered an example: Paul says "Martial wrote witty epigrams" intending (in agreement with what he takes

² Reply to Pagin (2015). All the page references which appear without the author's name refer to this paper.

³ In one of its most concise expositions, Davidson speaks about the central importance of "situations, in which someone intends (or assumes or expects) that his words will be understood in a certain way, and they are. In such cases we say without hesitation: how he intended to be understood, and was understood, is what he, and his words, literally meant on that occasion" (Davidson 1994, 11-12).

⁴ I have a bit more developed and in some respects modified the analysis, as well as conclusions drawn from it, in Kot'átko (2000).

to be the standard meaning of “epigram”) to assert that Martial wrote witty epigrams. At the same time he hopes to be interpreted by John (in agreement with what John, according to Paul’s belief, takes to be the standard meaning of “epigram”) as asserting that Martial wrote witty epitaphs. Paul’s aim is to embarrass John, granted that his misinterpretation comes to light. Peter (not another character in this fiction but my actual opponent Peter Pagin) classifies this desired effect as something Paul is “primarily interested in” (p. 13), which is apt with respect to the position of this aim in the chain of intentions ascribed to Paul in the story. But we should keep in mind that this is a *non-communicative* attitude motivating Paul’s communicative intention to express certain proposition with assertive force. The non-communicative attitudes may help us in identifying and understanding the communicative intentions but should not themselves be included into the analysis of utterance meaning. (I mention this just because in the main part of Peter’s argument the term “primary” plays an important role in the analysis of *communicative* intentions.)

Peter analyzes the Martial case⁵ in a way which should show that it cannot serve as a counter-example to Davidson’s account of utterance meaning: more precisely, that it is not a case where the following condition (extracted from Davidson’s concept of utterance meaning) is violated:

- (a) If *S* makes an utterance in order to perform certain speech act, she intends and expects that act to be assigned to the utterance in *A*’s interpretation.⁶

Let me try to summarize Peter’s interpretation of the Martial example (needless to stress, anybody interested in this issue would do better to read his full analysis in its original version). On the *primary* level, Paul intends to express the standard meaning of “epigram” and John interprets “epigram” (as uttered by Paul) as having its standard meaning. So, on this level, the communication is successful in Davidson’s sense. Moreover, “Paul pri-

⁵ He suggests two alternative construals of the situation I describe in the Martial example: for short, I will focus on the first one, which, according to Peter, corresponds to the scenario I rely on in my polemics with Davidson.

⁶ Peter’s objection against my use of the term “intend” here will be discussed later. A correlative condition (b) concerning the interpreter’s attitude (which also plays a role in my original argument) will be omitted here for the sake of brevity.

marily intends *epigram* to mean just what he expects John to primarily interpret it as meaning, in accordance with the condition (a)" (p. 11). According to Peter, a clash appears just on the *secondary* level, due to the difference between Paul's and John's beliefs about the standard meaning of "epigram" and to Paul's relying on this difference in his strategy. Paul secondarily intends "epigram" to mean epigram, while John secondarily interprets (and is expected by John to interpret) "epigram" as epitaph. And Peter argues that the primary level is what should be taken as relevant from the Davidsonian point of view: hence Davidson's position is not threatened by Martial example.

I have to admit that I have a problem with the „primary-secondary“ distinction in Peter's analysis, in particular with the conclusion he draws from his specification of Paul's primary intention and John's primary interpretation. It would make good sense to me if I could read Peter's specification of Paul's primary intention and of John's primary interpretation (cf. (i) in (Paul) and (i) in (John) on p. 10), so that both Paul and John *defer*⁷ to the standard meaning of "epigram". Then it would be natural to say that it should be this primary intention and interpretation what matters in our account of the situation and of the meaning of Paul's utterance. But since John and Paul have certain (mutually incompatible) beliefs about what the standard meaning of "epigram" is (as it is put in (ii) both in (Paul) and (John) on p. 10 in Peter's paper), I find it appropriate to say that instead of deferring to the standard meaning, Paul *straightforwardly* means epigram by "epigram", taking himself to conform thereby to the standard, and John *straightforwardly* interprets "epigram" as epitaph, taking this to be in conformity with the standard. The result is a *straightforward* discrepancy between communicative intention and interpretation: this is what I take to be a more plausible description of the communicative situation in question,

⁷ In his note 2, Peter relates the term "deference" to his alternative analysis of Martial example, which I am not discussing here. Our ways of using this term do not seem to coincide. In my use, the speaker defers to the standard meaning of a term if she does not know what that meaning is, is aware of her ignorance, but intends her utterance to have that meaning, whatever it may be. This is certainly not the case in my exposition of the Martial case, nor in Peter's reconstruction of it that I have been just discussing. But I would not take his second construal of the Martial case as an example of deference either: there I would say that Paul and John are submissive with respect to the standard, rather than that they are deferring to it.

than Peter's conclusion that there is an agreement between the speaker and the interpreter. Peter evaluates the situation as a communicative success, I take the same situation as a clear example of misunderstanding. Because what matters from the communicative point of view is, according to my view, the difference in the literary genre Paul and John take the utterance to mention (since this is likely to make the following conversation confused) rather than the fact that each of them takes himself as conforming to the standard. Moreover, Paul expects and intends John to *straightforwardly* interpret "epigram" as epitaph, *in accordance* with John's presupposed belief about the standard meaning, rather than to *defer* to the standard meaning.

In case of genuine deference the situation and its implications for our discussion would be indeed different. If we (as I think we should) include the deference, whenever we assign it to the speaker, into the construction of the content the speaker intends to express,⁸ we would get for Paul's case something like: "Martial wrote witty pieces of poetry belonging to the genre referred to by experts as 'epigram'". And we would get the same construal of the content John assigns to Paul's utterance in his interpretation. Moreover, granted that they both belong to the same linguistic community, the description "the genre referred to by experts as 'epigram'" would pick out the same literary genre in Paul's and John's case: so we would have a two-level match between intention and interpretation. This would be a clear example of communicative success. And granted that Paul expects and intends John to defer to the standard meaning of "epigram" in his interpretation of Paul's utterance, the condition (a) is not violated.

But, as I have suggested, the situation is not like that. Neither John nor Paul are actually *deferring* to the standard: rather, Paul means his utterance and John interprets it *in accordance with what they take to be* the standard. And Paul wishes John to interpret the utterance in a non-standard way, *in accordance* with his (presupposed) false belief about the standard, rather than to *defer* to the standard.

This is how I have construed the situation in the Martial example. Obviously, if Paul, under the conditions just specified, intends to make a mea-

⁸ For details see Koťátko (2012). I have had a discussion about some implications of this account with Peter earlier, profited from it a lot and tried to reply to (what I took to be) the main objection in Koťátko (2006).

ningful utterance, he must count with some principle constituting the utterance meaning independently of the audience's interpretation: then the utterance meaning he relies on also cannot depend on the match between the communicative intention and interpretation. My claim was that this kind of communicative strategy makes sense and, moreover, it can be successful. To admit this means to assume (as Paul does in our example) that the speaker can intend to assert that p without intending to be interpreted as asserting that p . It is quite likely that the people who witness the conversation would be disposed to describe it so that Paul asserted that Martial wrote witty epigrams and has been misunderstood by John (provided that John's way of interpreting the utterance becomes manifest in his reaction).

In general, I believe that our account of utterance meaning should be flexible enough to provide a space for various kinds of communicative situations differing in the role they assign to factors like intention, interpretation, linguistic conventions (or other kinds of social standards) in the determination of utterance meanings. These differences will depend either on the attitudes of the communicants (e.g. on the degree of their submissiveness or indifference to linguistic conventions) or on the social setting within which the conversation takes place. Examples taken from highly institutionalized or ritualized types of communication, like issuing legal enactments or military orders (cf. the army-example in Kot'átko 1998, 235), pronouncing wedding formulas, nomination formulas etc. show perhaps more convincingly than the Martial case that the match-account of utterance meaning cannot be generalized any more than its conventionalist rival.

In this discussion, as well as in the original paper, I have been speaking about Paul as intending to be interpreted in certain way: now I should say a few words concerning Peter's criticism of this use of the verb "to intend" (p. 6). Having in mind my underdeveloped linguistic intuition in English, I do not have anything to object against Peter's characteristics of the standard (ordinary) use of this word. On the other side, I think I have not deviated from the way it is used in relevant parts of the philosophical literature, including Gricean semantics (here the speaker is taken to mean something by her utterance if she *intends* to produce certain effect in the audience on the basis of her recognition of this intention and *intends* to...), Searle's speech act theory (cf., e.g., the condition 8 of his famous definition

of promise) and also in Davidson's way of speaking about utterance meaning. For instance, in the passage quoted above from (1994), Davidson speaks about the central importance of "situations, in which someone intends (or assumes or expects) that his words will be understood in a certain way, and they are". Surely, one cannot intend to produce some effect *E* by his act *A* unless one *believes* that doing *A* is, in given circumstances, an appropriate way for producing *E*:⁹ but to require that one is *certain* about *A*'s leading to *E* (as Peter does on p. 6), does not seem to be compatible with the examples just given.

In accordance with this, I suppose that the readers will understand me as writing these lines with the intention to meet Peter's objections, without suspecting that I do not realize how easily I can fail. Let me add that Peter's brilliant paper is much richer than my response could reveal and I can only recommend it for careful reading.

Alberto Voltolini¹⁰

Alberto has been generous enough to put aside our disagreements concerning the language of fiction and turned instead to the Carnap-Heidegger controversy. I can only admire his subtle and elaborate discussion of Carnap's analysis of Heidegger's famous sentence (in Carnap's version: "The nothing nothings") and agree with his conclusion that it is by far not as devastating as intended. Under the proper analysis, as Alberto shows, the sentence is logically well formed and (under certain non-trivial assumptions) even comes out as true. This is in fact all I can say in reaction to this brilliant paper – except rather general moral concerning rules of philosophical discussion, which I cannot avoid voicing in confrontation with this famous case.

I have to admit (as a confession rather than as a matter of programmatic declaration) that I never went into such a systematic interpretative enterprise concerning any piece of Heidegger's texts. But as a Carnap's reader,

⁹ A well-known Schiffer's argument against one version of Grice's definition of speaker's meaning heavily relies on this platitude, cf. Schiffer (1972, 19-21).

¹⁰ Reply to Voltolini (2015). All the page references which appear without the author's name refer to this paper.

I could not have failed noticing that his analysis could hardly count as an attempt to understand Heidegger's text, which means to find its *most charitable interpretation*: the reading which maximizes its philosophical value (including, of course, its coherence and truthfulness) and is compatible with the text itself plus the available non-textual (but interpretatively relevant) evidence.

According to Quine's famous sentence, you, as an interpreter, should keep in mind that "your interlocutor's silliness is less likely than your bad interpretation" (Quine 1960, 59). Similarly, if a philosopher of language comes with a criterion of meaningfulness which is not met by a considerable part of our everyday conversation and even by sentences in which that very criterion has been introduced, she should take it as a good reason to look for another theory (rather than to turn to rhetorical tricks, e.g. some variants of the "leather metaphor"). And if we apply our analytical apparatus on a piece of theoretical text and the outcome is a plain nonsense, our first thought should be that there may be something wrong with the apparatus chosen or the way we have applied it – in particular if the text in question is a work of one of the best educated and most influential thinkers of our time.

I suppose that everybody who read for the first time Kant, Hegel and of course Heidegger, found herself involved in the adventure of *radical interpretation* and hence must have recognized that applying previous interpretational training and routines acquired in it would not do. I do not see why this should not apply to the skills in operating with the kind of technical apparatus we are accustomed to. Moreover, here we should keep in mind what Frege said (and what Fredrik Stjernberg commented in the paper we will shortly come to) about the danger that "the formulaic mechanism would take over to such an extent that it suffocated the thought completely" (Frege 1980, 4f). In our case the "complete suffocation of thought" would have the form of paralysis of our interpretative abilities. Interpretation of a philosophical text, in particular not belonging to the intellectual tradition we are acquainted with, may be a serious challenge for our intellect, intuition, imagination and sensitivity: the strenuous work they are supposed to do cannot be reduced to operations of our technical apparatus. Nothing like this has happened in Alberto's text, despite all its technical precision.

Fredrik Stjernberg¹¹

Fredrik has done an admirable work in order to properly expose and clarify the tension between Frege's "intuitive" criterion of the identity of senses (relying on the speakers' authority concerning senses, cf. p. 44 f.) and his admitting that senses of some expressions are not known to anybody. Moreover, he has explored possible ways out and found the most promising one in a (specific kind of) pragmatic account of Fregean senses. Like in the preceding case, I find the author's arguments convincing and have nothing to add to them – except few general remarks concerning the possible functions of Fregean senses.

Confronted with the kind of inquiry to be found in Fredrik's paper and with its results, a philosopher of language can still ask whether she really has a useful application for Fregean senses in the specific field of her study. What is important from my point of view is that, despite Frege's claims to the opposite, postulating senses as ideal entities in Fregean strong reading (i.e. as inhabitants of the Third Realm) is neither necessary nor sufficient to explain the possibility of successful communication.¹² It is not sufficient, since we are still left with the task of explaining how it comes that the speaker and the audience understand the expressions uttered in the same way. And no matter whether we see the explanation in their sharing of linguistic conventions or in their creatively coordinating their communicative strategies (concerning the choice of expressions on the part of the speaker and their interpretation on the part of the audience), the ideality of senses does not make our task easier at all. And if we succeed, in both cases heavily exploiting psychological notions (like intention, belief and preference), we have all we need to explain what is the communicative success based on and it comes out that the ideal senses "do not oil the wheels" of the theory (to borrow a well-known metaphor from Davidson).

Another doubts about the usefulness of senses concern the presumption that senses (no matter how we interpret their ontological status), construed as complete modes of presentation of the entities referred to, is what we have to know to be able to successfully communicate. As Fredrik reminds

¹¹ Reply to Stjernberg (2015). All the page references which appear without the author's name refer to this paper.

¹² I have tried to say more about this in Koťátko (1995).

us (cf. especially p. 55), Frege admits that in case of expressions with extremely complex senses, it would be unrealistic to expect that whenever we use them we operate with their complete senses (as something we grasp in its complexity). Then the expression serves as a “receptacle” for sense and we know that we can open it if necessary, by going through the expression’s definition: owing to such a definition, “we can cram the sense into the receptacle and also take it out again” (Frege 1979, 209). Now, it requires just one more step to realize that there are no such definitions available for the expressions we use in everyday communication and any choice of a definition would be a matter of an arbitrary decision. So there is no general criterion for deciding what belongs into the receptacle “whale” and what is just a piece of extra-semantic knowledge we associate with the expression on the basis of what we have learned at school, read in popular zoological magazines, etc.¹³ The actual use of the expression, which can be the only source of its meaning, simply does not provide us with any such criterion. Then, to follow the well-known line, if we find a creature which in all respects resembles to whales, as we know them, except that it is not a mammal, there is no chance to decide whether calling it “whale” should count as a continuation of or a departure from the existing usage, and hence as a preservation of its meaning or its replacement by a new one. Similarly, we cannot decide whether it directly follows from “Ann is a whale” that “Ann is a mammal”, or we need an extra empirical premise “All whales are mammals” to justify such an inference. Hence it would be perhaps more realistic to say that the expression serves as a receptacle which (in any particular moment), contains all pieces of information which count (in that moment) as knowledge about whales – but then the content of the container corresponds rather to Putnam’s “stereotype” than to Fregean sense. In Putnam’s version (cf. esp. Putnam 1975), the core element of the meaning of “whale” is neither the Fregean sense, nor the stereotype, but an indexical identification of exemplars of animals for which (together with all other individuals sharing the essence with them) the term “whale” has been introduced.

Fredrik shows that Frege’s intuitive (epistemological) criterion for identification of senses can function only provided that we replace the real sub-

¹³ This is of course one of the famous indeterminacies analyzed by Quine, namely the inextricability of meaning (cf. e.g. Quine 1960, 38).

jects with their idealised counterparts. This raises questions concerning the practical usefulness of the criterion but should not strike us too much provided that Fregean senses, as Fredrik also emphasizes, are not supposed to be of human origin (for instance are not bound to such human creations like language) and that's why are not construed so as to match human cognitive capacities. The question then, of course, is why the author insisting on this status of senses repeatedly appeals precisely to this criterion (cf. Stjernberg 2015, Sec. 5). Here is an analogy which immediately comes to my mind: in a bit more recent (though already also classical) literature we find at least one even more striking example of a similar discrepancy. Grice's well-known attempts to define the speaker's meaning acquired (under the pressure of more and more complex counterexamples) the form of an infinite series of conditions, specifying intentions the speaker must have in order to mean something by her utterance. After some (unsuccessful) attempts to stop the regress or to make it harmless Grice (1989) had to admit that the terrestrial speakers (as finite beings) are unable to mean something by their utterances in the full sense and that the speaker's meaning is an ideal limit to which we can only approximate. This should be much more striking than Frege's problem: we end with a psychologically unrealistic construct, while the original promise of the Gricean project was to bring the semantic notions down to earth by showing their psychological basis. Schiffer (1987), after trying to lock the (apparently unavoidable) infinite regress into the notion of "mutual knowledge", suitably located in the definition of speaker's meaning, had to admit that this attempt failed together with all others, the whole project of intentional semantics is in ruins and his conclusion was that "meaning is theory resistant" (since its basic level, speaker's meaning, proved to be undefinable). Fredrik does not seem to be equally sceptical about the prospects of semantics based on the Fregean notion of sense, as the suggestion discussed in the last part of his paper shows.

Kathrin Glüer – Åsa Wikforss¹⁴

Let me say in advance that I share Kathrin's and Åsa's position in their dispute with normativists and I have repeatedly had an opportunity to refer

¹⁴ Reply to Glüer – Wikforss (2015). All the page references which appear without the authors' name refer to this paper.

to their arguments or apply a modified version of them. In particular, I fully agree that there is nothing intrinsically normative in the notion of conditions of correct application of an expression (similarly like in case of conditions of correct use of a washing machine, tommy gun etc.). The fact that an expression *E* has certain conditions of correct application assigned to it by conventions of English acquires normative consequences only within the context created by a norm which commits us to conform to conventions of English (like: “everybody employed in this office is obliged to speak correct English”). The obligation clearly comes from the outside, *in addition* to the fact that *E* has certain conditions of correct application, rather than being intrinsic to that fact.¹⁵ How it comes out that expressions have certain meanings and hence certain conditions of correct application (and what makes the links between expressions and meanings in some cases relatively stable) can be explained without using normative terms. In case of sociolects like English, it can be done in terms of regularities fixed by certain complex of attitudes of the kind specified in Lewis’ famous definition of convention.¹⁶ In case of an idiolect of some speaker *S*, the fact that certain expression has certain conditions of correct use is simply a matter of *S*’s communicative habits. And in case of particular utterances, the fact that an expression *E* has been used with certain conditions of correct application (whether or not these conditions have been fulfilled), is a matter of *S*’s communicative intentions. They may be based on *S*’s idiolect or on her (true or false) beliefs about the standard meaning of *E*, or on her (true or false) beliefs about the interpreter’s idiolect, or on her (true or false) beliefs about the interpreter’s beliefs about the standard meaning of *E*, etc. Finally, if we accept Davidsonian account of utterance meaning (in general or for the type of discourse or communicative situation in question), we will say that an expression *E*, as it appears in given utterance, has certain conditions of correct application, if and only if they were assigned to *E* (on that occasion) both in the speaker’s communicative intention and in the audience’s

¹⁵ Generally speaking, Lewisian convention is “a regularity sustained by a special kind of a system of beliefs and desires”, defined in non-normative terms (Lewis 1983, 179). In agreement with this, Lewis warns us not to confuse conventions in his sense with “our famously obscured friends, the rules of language, renamed” (ibid.). No wonder, Lewis’ theory of convention has become a target of some normativists.

¹⁶ A scheme of a scenario showing how such regularities and complex of attitudes may have developed can be borrowed from Schiffer (1972, 189), or Grice (1989).

interpretation. It makes no difference whether this match has been reached in a standard or a highly non-standard way: there is no general reason to allow conventions, rules (which should not be confused with the former – cf. above note 15) or other kind of standards to re-evaluate or retouch the result of the speaker's and audience's cooperative enterprise, which led to understanding. This counts, of course, for those communicative situations, in which the interest in mutual understanding weights more than the standards or our loyalty to them. Presumably, most of our everyday conversation (unlike the use of language in highly formalized or ritualized areas of discourse) is of this kind.

On the level of a sociolect (where the philosophers' insistence on the normativity of meaning can be expected to be most obsessive), the fact that *E* has such and such (conventionally fixed) conditions of correct application gives, quite straightforwardly, rise to hypothetical imperatives like: "If you want to conform to conventions of English, use *E* in such and such way!" It surely makes good sense to react to the introductory conditional clause by saying: "I don't! (or: I don't care!). I simply want to be understood." And it certainly would not be right to reply: "But that's, practically speaking, the same." While in many cases conformity to linguistic standards is the best way to understanding (and to fulfilling our practical aims which require understanding), in other cases it would generate misunderstanding. Obviously, the cases in which the audience makes systematic mistakes in connecting certain words with meanings or ascribes such mistakes to the speaker (and interprets her utterances in accordance with this assumption) or believes that the speaker ascribes such a mistake to her (and expects the speaker to base her choice of words on this assumption) etc. are of this kind.

According to Kathrin and Åsa, we should approach the term "correct", as it appears within the term "conditions of correct application", or to be precise, within some principle of the form

$$(C) \quad w \text{ means } F \rightarrow \forall x (w \text{ applies correctly to } x \leftrightarrow x \text{ is } f)^{17}$$

as a placeholder for a suitable basic semantic concept (p. 69). The choice depends on our favourite semantic theory: Kathrin and Åsa mention *truth*

¹⁷ Here "*w*" is a word, "*F*" gives its meaning and "*f*" is that feature in virtue of which "*F*" applies. Kathrin and Åsa adopt (C) from D. Whiting for the purposes of their polemics with his normativist position.

and *warranted assertibility* as “the main contenders” (p. 66). In the final part of their paper, they react on the fact that their normativist opponent, Daniel Whiting, shares their view that the basic semantic concept (they focus on *truth*) is not normative. Then, in order to show that “correct” in (C) must be interpreted normatively, “he would have to argue that the anti-normativist fan of truth-conditional semantics, for instance, is missing something essential to meaning by interpreting ‘correct’ in (C) as a placeholder for ‘true’...” (p. 72).

But, as we know, not everybody is ready to the concession concerning the notion of truth which Kathrin and Åsa appreciate in Whiting’s case. Instead of reproducing the views of their opponents (like Hans Joachim Glock) insisting on the normativity of truth, let me turn to one of the greatest authorities of the recent past and recall the way in which Michael Dummett construed the relation between the notion of truth and the notion of correctness. Dummett famously argued that we can justify the meaning-theoretical relevance of the notion of truth¹⁸ only by specifying the “linking principle” between that notion and our practice of making assertions. And the core of solving this task consists, according to his view, in deriving the notion of truth from the “more basic” notion of correctness of assertions. Cf. e.g.:

Any workable account of assertion must recognize that assertion is judged by objective standards of correctness and that, in making an assertion, a speaker lays claim, rightly or wrongly, to have satisfied those standards. It is from these primitive conceptions of the correctness or incorrectness of assertion that the notions of truth and falsity take their origin. (Dummett 1976, 83; cf. also 1973, 289; 1991, 165f. et al.)

The first question of course is whether we should follow this way of relating the basic concepts we are dealing with here (*truth*, *correctness* and the *theory of meaning*). If it is worth doing, the second question is whether the notion of correctness from which the notion of truth is derived here is *normative* or not. The answer seems to be “yes”, since Dummett speaks about the objective “standards of correctness” to which all speakers are committed – other-

¹⁸ Since Dummett takes semantics as having by definition *truth* as its central notion, the same point can be put as a problem of justification of the presumption that a meaning theory must have a semantic basis.

wise all assertions would not be judged by these standards (cf. the quotation above) and it would not automatically belong to making an assertion that the speaker “lays claim... to have satisfied those standards”. That looks like a challenge for anti-normativists. Although in (C) the term “correct” occurs in quite a different context than in the sentence quoted above from Dummett, if it serves in (C) as a placeholder for the notion of truth and that notion is derived from the normative notion of correctness of assertion, the occurrence of “correct” in (C) in the end serves as a channel through which normativity comes in. So, when defending their interpretation of (C), the anti-normativists have a good reason (the one presented here, not to mention others) to go into confrontation with Dummett.

This is compatible with the platitude that the conditions of correct use of expressions and the correctness conditions of assertions are two different issues, belonging to two different areas: debates about expression meanings and debates about speech acts and utterance meanings. While I share the anti-normativist position in the former respect, I believe that speech act types should be defined in terms of their normative consequences (namely the commitments they impose on speakers). And the same holds for utterance meanings, if we identify them with speech acts performed in utterances. This is not motivated by any of the arguments which appeared in the dispute between normativists and anti-normativists: rather, it is an outcome of my critical confrontation with the Gricean-type intentionalists. As I understand Kathrin and Åsa (cf. p. 67), their anti-normativist interpretation of the principle (C) does not commit them to go into polemics with me in this respect.

To summarize my views on this topic in the most general (and rather declarative) way, I suggest that from the following three claims about normativity of meaning we accept the first one and reject the remaining two:

- a) One of the ways of identifying the utterance meaning is to specify the normative consequences of the utterance (of the kinds generally characterized in the definition of the relevant speech act type).
- b) The utterance meanings must be determined with normative force.
- c) The assignments between expression types and their meanings must be normatively fixed.

Stephen Barker¹⁹

In the first part of his rich and inspiring paper Stephen focuses on the interpretation of explicit performatives (like „Hereby I order you to leave“).²⁰ He defends the classical Austinian account, according to which performative verbs, as they appear in these sentences, serve to specify the illocutionary force of utterances (in which these sentences are used), rather than to assert that the speaker performs an act with that force. As Stephen points out (p. 75f.), sentences in which these verbs occur are indeed used to *describe* performative acts of the speakers, and in doing so are supposed to match reality (to represent how things are), but not in a way which is assessable as true or false. The idea that there are ways of representing reality which do not admit truth-evaluation is by far not uncontroversial and a considerable part of Stephen’s paper is devoted to its explication and defence. Let me proceed in the reverted order, i.e. start with this latter, more general issue, and return to explicit performatives in the end.

I should say in advance that I do not see any real opportunity for a polemic confrontation with Stephen’s views: instead, I will try to shortly sketch the reasons, why they are so close to my way of thinking about these issues. We seem to share the view that one should not waste the importance of Austin’s lesson (which is, unlike the theoretical elaboration of Austin’s position, fully in Wittgensteinian spirit) that there is a variety of ways in which our utterances may succeed or fail, equally relevant like truth or falsity in case of assertions. Truth/falsity may seem to be the only (or the most) relevant dimension of evaluation of utterances only if we focus exclusively (or primarily) on assertions – which we should avoid doing if we are really interested in how the actual human communication works.²¹ If

¹⁹ Reply to Barker (2015). All the page references which appear without the author’s name refer to this paper.

²⁰ In fact, at least two other authors participating in the *Organon F* volume referred to were involved in the debates about this topic, namely Peter Pagin and Manuel García-Carpintero: it would be exciting to see them discussing with Stephen at a colloquium on explicit performatives (in some attractive place, if possible).

²¹ Nevertheless, in his discussion about explicit performatives, Stephen distinguishes the question whether they should or should not be approached as assertions from the question whether they admit truth-evaluation: after he answers the first question negatively, he brings independent arguments for the negative answer to the second one.

this is right, we should not have any problem with taking propositional contents of non-assertive utterances as determining other kinds of *conditions of satisfaction* (to follow John Searle's terminology) than the truth-conditions. I find Searle's term quite suitable²² – and will stick to it, although Stephen does not use it. Instead, he speaks about *representation conditions* (p. 91f.), which is perfectly compatible with my terminological preference, since the satisfaction conditions of an utterance specify the state of affairs *represented* by the utterance as that state of affairs which, if it obtains (if it is a *fact*) in the world we are speaking about, would make the speech act performed in the utterance *satisfied* (in a way corresponding to its force). For instance, it would make an order obeyed, promise fulfilled or assertion true.

To be sure, once we say this we are confronted with various kinds of objections and widely shared ways of thinking about truth, meaning and related issues. Let me mention some of these challenges, more or less straightforwardly linked with Stephen's arguments.

(1) One of the implications of this view is that the true/false assessability must be justified on the level of illocutionary acts, rather than automatically belonging to locutionary acts. I fully agree with the way Stephen puts this point (cf., e.g., p. 93) and with his corresponding abandonment of what he labels as the *principle RT*:

RT: A sentence *S* is true/false-assessable iff *S* has propositional content, it is a representation of how things might be. (p. 76)

One may insist that the true/false assessability cannot depend on the assertive force of the utterance, since, as Stephen points out, "being unasserted does not imply not being true/false assessable. For example, constituent sentences of a logical compound are not asserted, but they are true/false assessable" (p. 82). Similarly, Peter Geach (1972) has argued against the doctrine of the "Oxford philosophers of language" that a proposition can be evaluated as true or false only if it is introduced into discourse in a way which raises the claim for truth, as it happens in assertions. He objected that it makes a good sense to evaluate as true or false both dis-

²² Obviously, the applications of the term "satisfaction" to expressions, rather than to utterances, like: "x satisfies 'is tall' iff x is tall", should be then read in an equally generalized way, i.e. not as automatically replaceable by "'is tall' is true of x iff x is tall".

juncts in, for instance, “It’s raining or it’s snowing”, though neither of them is asserted. One may feel inclined to interpret this as showing that the truth evaluation can be applied to bare propositions, in abstraction from the force with which they are introduced into communication. But this is certainly not what happens in our example. Here, the truth evaluation of the disjuncts is justified by the assertion of the disjunction as a whole: we ask whether at least one of the disjuncts is true in order to find out the truth value of *what is asserted*. Similarly, an order can have disjunctive conditions of satisfaction, like in case of “Wash the car or return the money!”: we ask whether at least one of these conditions has been fulfilled in order to find out whether the order has been *obeyed*. Of course, to say that an order has such and such conditions of being obeyed, or to say that these conditions are fulfilled, is to make assertions, and the latter assertion is (in our case) true if the addressee of the order has washed the car or returned the money. But this platitude clearly does not justify the claim that the disjunctive *content of the order* in our example has such and such *truth-conditions*.

(2) Perhaps one can still insist that even the analysis of the way the propositions are introduced into discourse in non-assertive acts, like order or promise, cannot do without the notion of truth. For instance, to order to a person *P* to wash the car is to order to *P* to behave in such a way as to make the proposition “*P* has washed the car” true. The question is what the possibility of such a paraphrase is supposed to show. As philosophers should know better than anybody else, it is (usually) quite easy to introduce into the analysis the concepts we want to have there: but such a maneuver does not in itself amount to discovering a new parameter of the phenomenon analyzed. It is always useful to recall Wittgenstein’s remark (from Wittgenstein 1953, § 22):

We might also write every statement in the form of a question followed by a ‘Yes’; for instance: ‘Is it raining? Yes!’ Would this show that every statement contained a question?

Examples of this kind can be produced *ad libitum*: for instance, the assertion “*P* washed the car” can be paraphrased by saying: “*P* behaved in such a way that if he were ordered to wash the car, the order would have been obeyed”. Does this show that assertions are parasitic on orders? I think everybody will agree that what I have offered is just an artificial and bizarrely

monstrous construction. And this is how we should comment also the suggestion from the beginning of this paragraph: the paraphrase of an order in terms of making a proposition true.

(3) It is certainly true that an analysis of one speech act type can help us in understanding other speech act types; and analogically for the types of conditions of satisfaction. But even here, i.e. in the order of explanation, there is no *a priori* reason to assign the privileged role to the notions of assertion and truth. Dummett (1978) gave us an opposite example when he based his considerations concerning truth conditions of certain types of assertions on considerations concerning conditions of obeying analogically structured orders. Similarly, Austin's analysis focusing on linguistic acts establishing institutional facts (as paradigmatic examples of "doing things with words") helps us to understand better what is going on in linguistic communication in general, including acts of assertion (after the performative/constative opposition has been abandoned). And the same holds for Searle's paradigmatic analysis of promise.

(4) Insisting on the central position of the notion of truth may be motivated by our appreciation of the role it plays in some theory (regarded as) successful in important respects. A prominent example is, of course, Davidsonian theory of meaning construed on the basis of Tarski's theory of truth. Stephen (on p. 85) mentions and criticizes Davidson's well-known attempt to show how such a theory, while keeping truth as its central notion, can deal with non-indicative sentences and non-assertive utterances (cf. Davidson 1984b). Lepore and Ludwig, in their attempt to provide a general account of logical form on the basis of Davidsonian truth-theoretic semantics, have admitted that "non-declarative sentences ... present an especially interesting challenge to any conception of logical form grounded in truth-theoretic semantics, since uses of them are neither true nor false" (Lepore – Ludwig 2001, 130). They focus on imperatives and notice that their uses "admit of bivalent evaluation, though they are not truth-valued". To account for this, they introduce the "notion of fulfillment conditions which subsumes both compliance conditions for imperatives and truth-conditions for declaratives" (Lepore – Ludwig 2001, 131). Nevertheless, their truth-theoretical approach commits them to "exhibit compliance conditions as recursively specifiable in terms of truth-conditions". This requires that they construe imperative sentences as having a "declarative core", assuming that "the compliance conditions for im-

peratives will then be exhibited as given in terms of the truth conditions for their declarative cores” (Lepore – Ludwig 2001, 131). So, the notion of “fulfillment conditions”, which they present as “a generalization of the truth-theoretical approach” (Lepore – Ludwig 2001, 131), plays a subsidiary (if not just decorative) role within the truth-theoretical framework: no “interpretive fulfillment theory” as a generalization of “interpretive truth-theory” (Lepore – Ludwig 2001, 113) is in fact proposed. Obviously, this framework does not provide any space for asking the question whether the attractions of Davidson’s theory, like its ability to systematically account for the compositionality of meaning or its ability to link (within the broader framework of the theory of interpretation) in a productive way meanings with beliefs and other types of attitudes, can be preserved if the notion of *truth* gives up its central position in favor of the more general notion of *fulfillment*.²³

Davidson’s fundamental considerations (esp. in Davidson 1984a) about the form of the desired theory of meaning (a theory which would, on a finite basis, generate interpretative theorems for each of the infinite number of sentences of the object-language), famously resulted in accepting – as one of the possible solutions – the model of Tarski’s truth theory. I believe that they could, with small modifications, equally well result in accepting an alternative model with theorems of the form “*S* is satisfied iff *p*”.²⁴

(5) Utterances can represent states of affairs (as their conditions of satisfaction) with a variety of communicative functions, corresponding to the variety of speech act types. The specification of various ways in which propositions can be introduced into discourse then very much depends on the central notions on which our speech act theory is based. I do not think that speech act types can be defined in terms of speaker’s intentions: at least on this level (on the level of definitions of speech act types) I would not say, with Stephen, that to *X* (i.e. to make a promise, assertion etc.) is “to produce a sentence with such and such intentions. That’s what consti-

²³ Not surprisingly, the correlative notion of the conditions of satisfaction (in Searle’s sense), together with the notion of conditions of success, play the central role in an attempt to build the Searlian type of speech act theory on formal basis: cf. Searle – Vanderveken (2009).

²⁴ I have tried to show this in Kot’atko (2001). The same applies to the way in which McDowell (1980) specified the general aspirations of Davidson’s programme and their fulfillment in the Tarskian theory.

tutes *X*ing” (p. 78). It’s true that in speech acts we manifest certain intentions, beliefs and other attitudes (in case of assertion that *p* it is the belief that *p*, the intention to produce or activate the belief that *p* in the audience etc.).²⁵ I believe that speech acts can be defined in terms of these manifestations – or, correlatively, in terms of commitments imposed by these manifestations on the speaker. But one can certainly manifest the belief that *p* and the intention to create or activate that belief in the audience without actually having any such belief and intention – and the utterance will still have the assertive force. The possibility of such discrepancies opens space for various kinds of linguistic tricks which, in turn, can serve as evidence that the manifestations (on which these tricks are based) indeed belong to the act of asserting, independently on the actual speaker’s beliefs and intentions. The way in which the propositional variable *p* is imbedded within the specification of manifestations (or commitments) constituting an act of asserting that *p* is, within this approach, supposed to characterize the way in which a proposition is introduced into communication in assertion – and analogically for other types of speech acts. Within this framework, there is no space for assigning any privileged position to expressing propositions with assertive force, which opens space for truth-evaluation.

(6) As to the case of explicit performatives, I have always regarded the original Austinian version, defended by Stephen, as most intuitively plausible, found it natural that John Searle preserved it in the original version of his speech acts theory²⁶ (which exposed him to forceful attacks, cf. e.g. Stampe 1975) and has been surprised by Searle’s conversion (in Searle 1989).²⁷ As Stephen points out, it is undeniable that in “I hereby order you to leave” the directive force is specified in another way than in “Leave!” In the former case the directive act is being *described* (p. 75) – and to admit this allows us to preserve the presumption that the meaning of the compo-

²⁵ These are just two of the manifestations constituting the act of asserting. The second one is, moreover, simplified (the precise formulation must account for the plurality of possible communicative functions of assertions). A complete definition of assertion in terms of manifestations has been proposed e.g. in Koťátko (1998).

²⁶ There, performative verbs are classified as “force indicating devices” precisely like “word order, stress, intonation contour, punctuation, the mood of the verbs”; cf. Searle (1965, 6).

²⁷ I have had an opportunity to express that surprise already in 1990, when Searle gave a talk on this issue in Prague.

nents of the sentence contribute to the meaning of the whole in the standard compositional way (they jointly participate in the complex representation of how things are). But there is no reason to insist that the descriptive specification of the directive force is somehow *less direct* or straightforward than the indication of that force via the imperative mood. In particular, we should reject the presumption that what the descriptive specification in our case *directly* serves to must be an act of assertion – and then we will not face the task of construing a mechanism which would bring us from assertion (as a directly performed act) to an order (as an indirectly performed act).

An analogy with the general theory of descriptions might be illuminating. Some orthodox Russellians, when confronted with the actual communicative function of sentences like “The present French president is a socialist” (in particular with the typical ways we react to them and report about them), feel obliged to admit that descriptions can indeed serve to refer to individuals, but in an indirect way – on the level of implicatures (cf. Neale 1990). The right reply is, I think, that we need not invoke anything like the Gricean mechanism of implicatures (and ascribe to the speaker the corresponding communicative strategy, directed at generating quite a complex kind of reasoning in the audience) in order to justify the following report of John’s utterance of our sentence: “John said about Holland that he is a socialist.” And quite similarly, if John says to Paul “I hereby order you to leave”, nothing like Gricean mechanism (and no correspondingly complex speaker’s strategy) is needed to justify the claim that John ordered Paul to leave. On the contrary, reports like “John asserted that he ordered Paul to leave” would be, in the ordinary communication, most probably evaluated as bizarre – as totally missing what happened (what move has been made) in conversation. This argument concerns the way we report about performative utterances, but Stephen (among other things) draws our attention also to the speaker’s communicative intentions: “Why would speakers want the extra illocutionary act to get in the way of *Xing* [e.g. ordering in our case or cursing in Stephen’s example], since *Xing* is the main point” (p. 78).

Stephen refers to Bach and Harnisch as proponents of (one version of) the implicature-approach to explicit performatives and I fully share his critical attitude. In general, saying “I hereby order you to leave” is perhaps a more ceremonial or pompous, but not at all a less direct way of ordering

somebody to leave than saying “Leave!”.²⁸ So, I agree with Stephen that “performatives are not indirect speech acts but primarily performances of orderings, commandings, adjournings, that is of *X*ings, and not acts that are constituted by assertions allowing us to infer that a performance of *X*ing is going on” (p. 79).

I also agree with Stephen (p. 81) that uttering “You ought to leave the room”, unlike “Leave the room!” or “I order you to leave the room” can function as an assertion, but I would not describe it as an “utterance in which a speaker defends the state of desiring that the audience leaving the room”. As far as I can see, uttering such a sentence with the affirmative force does not include manifestation of such a desire on the part of the speaker nor manifestation of an intention to produce such a desire in the audience. My evidence for this is that it is quite coherent to say: “You ought to leave the room, but I will be happy if you stay.” Claiming that one ought to do something (according to some standards applying to her) and expressing the wish that she does not do so does not establish straightforward inconsistency, nor a communicative defect of the type of Moore’s paradox.

Marián Zouhar²⁹

An important part of Marián’s work in the philosophy of language concerns rigid designation – and the present paper continues in this line in an innovative and most inspiring way. Following the tradition of our polemical

²⁸ As to specific form of explicit performatives, my underdeveloped linguistic intuition in English does not enable me to appreciate all of Stephen’s confrontations of cases in which the same verb (specifying some kind of illocutionary act) once has and once lacks performative function (cf. pp. 77-78 and 80). As far as I can see, no standard translation of these examples into Czech could serve to demonstrate that contrast. I cannot avoid mentioning an opposite case – one in which my Czech linguistic intuition enabled me to identify a mistake in an English article on speech acts. Searle (1975) claims that unlike the English sentence “Can you pass me the salt?”, the Czech sentence „Můžete mi podat sůl?“ (which is a straightforward translation of the former) cannot serve to perform an indirect speech act of a request. I have been pleased to demonstrate to him (sitting at the table of one of the Prague pubs) that it can.

²⁹ Reply to Zouhar (2015). All the page references which appear without the author’s name refer to this paper.

exchanges, I will abstract from all the points of agreement and focus on the rest, rather marginal in the context of Marián's inquiries. Let me start with Marián's quotation of Kripke's remark (see Kripke 1980, 21, footnote) explaining *de iure* rigidity as a case "where the reference of a designator is *stipulated* to be a single object, whether we are speaking of the actual world or of a counterfactual situation". I read this as saying that it is part of introducing an expression of this kind into our vocabulary that it is designed to play the referential role mentioned – in virtue of which rigidity belongs to its basic semantic status. "Introducing an expression into our vocabulary", as I understand it, can mean both making it part of the conventionally fixed reservoir of some sociolect, or using it for fulfilling an *ad hoc* communicative function – provided that this use is successful (i.e. that the communicative intention behind it is recognized by the audience – and on this basis the audience assigns the intended meaning to the utterance). When admitting both possibilities I no more follow Saul Kripke and I clearly depart from Marián's course of reasoning, as we will see shortly.³⁰

Marián also goes beyond Kripke's explicit remarks on this issue when he asks "how it happens that proper names are rigid *de iure*?" (p. 104). Nevertheless, his reply is based on the Kripkean idea that proper names typically acquire their referential functions in the acts of baptism and on the interpretation of baptism as a convention establishing act. Granted this, "names designate their objects because of the linguistic conventions introduced during the baptismal acts" (p. 105). This, according to Marián, means that "an object needs not satisfy any descriptive condition to be designated by a given proper name. ... So, the name designates the object irrespectively of

³⁰ When Kripke in (1977, 256) defines semantic referent as "given by a general intention of the speaker to refer to certain object whenever the designator is used", he adds (in footnote 20): "If the views about proper names I advocated in 'Naming and Necessity' are correct..., the conventions regarding names in an idiolect usually involve the fact that the idiolect is no mere idiolect, but part of a common language, where the reference can be passed from link to link." The way in which the term "convention", is used here, is certainly incompatible with its Lewisian (by far most influential) definition as a regularity prevailing in certain community, fixed by a complex of shared beliefs and preferences reflected in "common knowledge". But more importantly in our context, Kripke admits cases, corresponding to referential use of descriptions in Donnellan's sense, in which a proper name is (successfully) used to refer to an individual which is not its semantic referent (not to speak about referent assigned to it by conventions of some sociolect). I will refer to Kripke's well known example later.

virtually any property the object exemplified or might have exemplified” (p. 104; cf. also p. 115). When put so straightforwardly, the claim raises certain doubts, since a few lines above Marián described the property an object *must have* to become (in the typical way) the bearer of a name: namely to be linked with it in an act of baptism. In general, one can certainly say that an object can be a referent of a singular term, be it definite description, name or demonstrative (as used on particular occasion) provided that it meets certain empirical condition: it uniquely satisfies the description in question or has been assigned to the name in question as its bearer at the beginning of the chain to which the given utterance of the name belongs, or is the contextually most plausible candidate for the referent of the given use of a demonstrative. This, I believe, needs to be added to the contrast between descriptions and names drawn by Marián in Section 4: both linguistic conventions connecting names with objects and satisfaction relations between descriptions and objects are established by empirical facts (consisting in the objects’ bearing certain empirical properties).

One might insist that these two kinds of facts (and correlatively two kinds of properties) should not be treated at the same level. Nevertheless, Marián treats them so in his exposition of the puzzle which is the central topic of his paper. The second of the two lines of reasoning which Marián introduces as generating the puzzle (p. 110) results in the statement that “there is at least one property P and at least one possible world w such that [an object] o exemplifies P with respect to w even though o fails to exist in w ” (this is Marián’s paraphrase of the conclusion from his Abstract). The property presented (at the same level with all other properties of objects) as justifying this claim is the semantic property of being named by (some name) α .³¹

But let me return to Marián’s explanation of the *de iure* rigidity of proper names from their being introduced via conventions. My basic objections can be summarized as follows:

³¹ I have to add that the way out of the paradox, which Marián apparently finds most promising, includes resignation on construing this property (together with other language-dependent properties of objects) in the standard way, i.e. as an intension (function from possible worlds to extensions). His defense of this move (p. 115) is rather problematic for those, who approach language, including semantic relations between names and their bearers and linguistic conventions fixing these relations, as part of our world, and hence depending on how things are in this world.

(1) As I have pointed out, the link between a name and the individual designated by it needs not be established in a conventional way. For instance, some expression can effectively function as a name *ad hoc* – and even then it designates rigidly and its rigidity is *de iure*, in virtue of its semantic status of a name. For instance, I can say “Plato is coming!” or “Jacket is coming!” in a context in which it is clear to you as my audience that I mean our colleague John, although I have never used these expressions to refer to John before.³² Then the terms “Plato” and “Jacket” function as proper names of John and *eo ipso* as rigid designators, without any convention governing our future linguistic behaviour being established. You may find this way of referring to John unacceptable, there need not be any reason to expect that I will continue in this practice on any future occasion, nevertheless, if my referential intention has been recognized by you and you have interpreted my utterance on this basis, the expressions “Plato” or “Jacket” functioned as names of John – and they did so successfully, i.e. in a way which led to understanding.

(2) The opposition between the referential function’s being established via convention and it’s being established in virtue of the fact that certain individual satisfies certain descriptively specified conditions³³ is, from my point of view, difficult to accept, not only for the reason mentioned above. The convention introducing the name *N* for the person *P* can precisely make *P* being the referent of *N* in virtue of the fact that *P* satisfies certain description conventionally linked with the name as fixing its referent. This is the case of the name “Jack the Ripper” in Kripke’s well-known example: it has been introduced for the person who, in the actual world, committed those and those murders (but has never been revealed). Then it refers with respect to any possible world to that individual who committed those murders in the actual world.³⁴ Similarly, the description “the person who

³² Or, to slightly modify Kripke’s scenario (from Kripke 1977): I say “The old Smith is coming” – in a context in which it is clear that I intended to speak about Jones but mistook the names.

³³ Cf., e.g., p. 113 f.: “Anyway, we cannot retain both the idea of conventionally established name-bearer relations and the idea of name-bearer relations being determined such that the bearer of a name satisfied some kind of condition” (p. 114).

³⁴ I find this incompatible with the way in which Marián presents the contrast between “the idea of conventionally established name-bearer relations” and “the idea of satisfactorily established name-bearer relations” in his footnote 28 (p. 114).

committed those and those murders” can in some uses function rigidly, i.e. to refer, with respect to any possible world, to the person who uniquely satisfies it in the actual world, as it is in the utterance of “The person who committed those and those murders need not have killed anybody – if he properly studied Kant’s second *Critique*”. In both cases the term refers to certain person with respect to all possible worlds due to the fact that it satisfies certain description in the actual world (plus, in the former case, in virtue of the fact that the referent of the name “Jack the Ripper” has been, in the actual world, fixed by means of that description). Nevertheless, in the former case, the term is rigid *de iure*, due to its semantic status of a proper name, while in the latter case it is rigid “merely” *de facto*, due to its status of description (a kind of expression which does not include rigidity as part of its general semantic characteristics).

(3) I agree with Marián, David Kaplan and others that we should approach names as obstinately (rather than persistently) rigid, i.e. that they “designate the same thing with respect to every possible world, whether that object exists there or not” (as Marián quotes from Salmon) and I share Marián’s view that this is “the other side” of their rigidity *de iure* (cf. p. 106).³⁵ But I cannot join Marián when he claims that this correlation stems from the fact that “both features can be explained in terms of the conventionally determined link between proper names and their bearers” (p. 106), due to the objections presented in (1). Marián develops his point in considerations concerning the order in which proper names are introduced into language and possible worlds into our apparatus (cf. his argument on p. 107 f.). I find these things hardly comparable: obviously, language with its proper names and their referential functions, with its devices for describing actual as well as counterfactual states of affairs etc. comes first – and the terms like “proper names” or “possible worlds” have been introduced as instruments for the description and analysis of these phenomena. But once we have them, they enable us to say that proper names, independently of the development of linguistics or logical semantics, always served to refer to individuals with respect to the actual world as well as to other possible worlds and that in fulfilling this task they always functioned as rigid design-

³⁵ Cf. also Kripke (1980, 21, footnote): “Since names are rigid *de iure*... I say that a proper name designates its referent even when we speak of counterfactual situations where that referent would not have existed.”

nators. What we attempt to capture here by means of the term “possible worlds” was always part of the communicative potential of our language – if it is true that it always served (among other things) to speaking about counterfactual states of affairs. I find it difficult to imagine the position from which I could say, with Marián, that “the name-bearer relation is not dependent on any possible world whatsoever”. Here, the speaker manifestly exploits the possible world apparatus: then he should be able to say that the name-bearer relation is based on things which happened in the actual world (typically, but not necessarily, the act of baptism) and that the relation is (in virtue of its being naming relation) such that the name designates the same object with respect to all possible worlds (including those in which that object does not exist). This clearly does not explain how it comes out that names are obstinately rigid designators: what I want to say is just that it is not quite easy for me to accept Marián’s explanation.

As to the presentation of Marián’s puzzle, I have a problem, mentioned in another context above, with the second step in the second line of reasoning, leading to one of the incompatible claims involved in the constitution of the puzzle. I mean the following assumption: “2. For any linguistic convention *c* and any possible world *w* it holds that *c* is in force regardless of how things are in *w*” (p. 109). The question is how could a convention *c* be in force in some world *w* without certain things happening in *w* – things constituting and preserving the convention in some community. David Lewis’ famous definition provides one possible list of such things (conditions which must be fulfilled in the community in question): if you accept this definition, you have to admit that once certain beliefs and preferences (of the kinds specified in the definition) cease to prevail in the community of citizens of the Czech republic, Czech language will cease to be the language conventionally fixed as the language of that community. Similarly for any alternative list of conditions you might adopt. So, “Praha” designates Prague in Czech in virtue of certain things being the case in the actual world. It refers to Prague also with respect to any other possible world regardless of how things are there (even the existence of Prague there is not required and, obviously, linguistic conventions which are in force there do not matter at all). But if the relevant things change in the actual world, “Praha” will cease referring to Prague in Czech with respect to the actual as well as any other possible world. Marián insists that “everyone who believes in rigidity *de iure*” should accept 2 “without much ado”.

I firmly believe in rigidity *de iure* but don't know how to get rid of the ad hoc just voiced.

For the reasons mentioned above in the remark (1), I have some problems also with the first step in the same argument, namely with adopting the claim "1. For any expression *e* it holds that if *e* is a proper name then *e* is a *de iure* rigid designator and there is an object *x* and a linguistic convention *c* such that *e* designates *x* on the basis of *c*" (p. 110). But one can still obtain the conclusion of this argument (and hence the puzzle) if one starts with a modified version of the step 3, namely with adopting the claim: "3'. If there is a linguistic convention *c* and a name α such that α rigidly designates *o* on the basis of *c*, then for any possible world *w* it holds that α rigidly designates *o* with respect to *w* regardless of *o*'s existence or non-existence in *w*." This is based on the assumption that proper names are obstinately rigid designators but does not rest on the stronger assumption 2. Second, 3' mentions a conventionally established relation between a name and an object designated by it, without committing us to the assumption that the naming relation must be based on a convention, as the assumption 1 did.

The line of reasoning then can continue in the way suggested by Marián to the resulting claim incompatible with the conclusion of the first Marián's argument – and so we get the puzzle without being involved into the controversies mentioned above. This opens way to Marián's considerations about possible ways of solving the paradox and their costs, which I find brilliant and illuminating.

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