Rigidity and Modal Asymmetry:
The intuitive Kripkean argument revisited

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Much of what has been discussed in the theory of reference in the last twenty-five years is strongly influenced by considerations centring on the business of devising a semantics for quantified modal logic. In this context, discussion of the property of *rigidity* plays an important role. This property is conceived of as a semantic modal property that distinguishes proper names from descriptions. It is argued that there is a *semantic modal asymmetry* between expressions of these types. In this talk I shall challenge this assumption. By examining the intuitive Kripkean argument or test employed I arrive at two rather nonconformist results: Firstly, it seems that the test does not establish a genuine *semantic* asymmetry: Rigidity appears to be a *pragmatic* property. Secondly, the test does not seem to demonstrate an *asymmetry* at all: When applied correctly it suggests that both proper names and descriptions (even discounting notorious cases like "the even prime") can be *used* rigidly—or so I shall argue.

1.   Why an argument for the rigidity thesis is needed

As is well known, the notion of *rigidity* is introduced in Saul Kripke's lectures *Naming and Necessity*. There, Kripke states that an expression is a *rigid designator* if and only if it designates the same object with respect to all possible worlds. He then goes on to say: "In these lectures, I will argue, intuitively, that proper names are rigid designators […]" (Kripke 1980, 48).[1] It is easily seen that these statements differ markedly with respect to their status. The first one is a mere stipulation; it cannot be subjected to criticism save for methodological reasons alone. The second one contains a substantial claim: it is meant to state that the proper names of *natural language* are rigid designators. Since applying such sophisticated technical notions to natural language expressions does not go without saying it is clear that the rigidity thesis is in need of justification—the thesis is not justified in itself. In particular, as Kripke has pointed out himself, the thesis is not simply justified by the possibility of treating expressions of a formal modal language in certain ways:

> In speaking of rigid designators, we are speaking of a possibility that certainly exists in a formal modal language. Logically, we as yet are committed to no thesis about the status of what we ordinarily call 'names' in natural language. (Kripke 1980, 4)

If this is correct it seems that a similar line of thought applies to other substantial notions of a semantics for a formal modal language as well. Hence it appears that what is called for to establish the rigidity thesis in the first place is a modally innocent argument.[2] Clearly, in order to be effective, such an argument must not *presuppose* the applicability of the central notions of a semantics for quantified modal logic to natural language expressions. Rather, it has to establish it in its *conclusion*. It is precisely because he acknowledges this point that Kripke appeals to an intuitive argument to make the case for the rigidity thesis:

> In these lectures, I will argue, intuitively, that proper names are rigid designators, for although the man (Nixon) might not have been the President, it is not the case that he might not have been Nixon (though he might not have been called 'Nixon'). (Kripke 1980, 49)

This passage is meant to provide the justification we need: it contains the intuitive argument for the rigidity thesis that is called for. The argument, which can be conceived of as a test or criterion for an expression's being rigid, will be in the focus of attention of the present talk. I shall first give a reconstruction of it and then go on to comment upon it.

2.   The Kripkean argument for the rigidity thesis

Let us start by examining the general structure of the intuitive argument. Consider, for instance, the following pair of sentences:

(1)   Aristotle might not have been Aristotle
(2)   Aristotle might not have been the inventor of formal logic

The general form of the test is best outlined by representing it schematically. It is easily seen that it involves a schema consisting of two modal sentences whose tokens differ only with respect to one position:

(MS-1) α might not have been α
(MS-2) α might not have been β

What is being tested is the behaviour of two type expressions, represented as "α" and "β", in the presence of the natural language modal two-place predicate "might not have been". The predicate involved provides a *modal context* with respect to which the behaviour of these expressions is to be evaluated.[3] For the sake of convenience, let us call $\ulcorner\alpha\urcorner$ the *primary test expression* and $\ulcorner\beta\urcorner$ the *expression for comparison*.

Against this background we may now proceed to reconstruct the crucial argument. Regarding sentence pairs such as (1)/(2) the Kripkean argument

for the rigidity thesis may be reconstructed thus: Where the first sentence contains a repeated occurrence of the proper name "Aristotle" the second one contains a description that describes the same person named by the proper name. But apparently (1) and (2) differ in truth-value. For all we know Plato might have been the inventor of formal logic, had the development of philosophy only taken another course. So it seems that (2) is true. But what about the truth-value of (1)? Could any alternative course of things have brought it about that someone other than Aristotle is Aristotle? Apparently not. This suggests that we have a semantic difference—a difference in truth-value—caused by variation within a modal context—the predicate "might not have been". Since (1) and (2) differ only with respect to the replacement of a proper name with a description describing the same object, it is sensible to assume that the semantic difference found is caused by a semantic modal difference between the expressions involved, i.e. the description and the proper name. And since these expressions are co-designative it is tempting to trace this difference back not to a difference with respect to *what* they designate but to a difference *in the way in which they designate it*. Unlike the description the proper name appears to call for a constant interpretation even in modal contexts like "might not have been". In other words: the great philosopher of antiquity is designated 'rigidly' by the proper name but not by the description. So this is the conclusion the Kripkean draws: the difference in truth-value of sentences like (1) and (2) illustrates that proper names are *modally rigid* while descriptions are not.

3.   Submitting the intuitive argument to closer inspection

So much for a first approach towards the reconstruction of the intuitive Kripkean argument. More needs to be said in order to assess whether the intuitive argument really makes its point. But up to now the line of thought employed seems quite powerful. Note in particular that it does not presuppose the special perspective of a semantics for quantified modal logic. Rather, it uses that perspective to explain a difference that can be detected without invoking that perspective beforehand, and this is precisely what is needed. So the intuitive test seems to make a strong case for the rigidity thesis as concerning the semantics of natural language. Actually, it is even hard to see how an appropriate argument could only be thought to make the case for this thesis and neither rely on the Kripkean argument nor be a mere variant of it. Surely some sort of appeal to a semantic difference a modally innocent competent speakers of natural language can detect must be involved. Other sorts of consideration, e.g. purely metaphysical considerations, can hardly in themselves succeed in making what is meant to be a semantic claim.

It is clear that the intuitive argument is the cornerstone of the rigidity thesis. Therefore, it would seem bad practice to simply be content with the argument as it stands. To appeal to intuition to make the case for a given

claim is a perfectly legitimate move but it is also legitimate, even necessary, to ask upon what the intuition appealed to might rest. If this question is pursued, however, the result of analysis may be of two sorts: Scrutiny may either reveal that the intuition appealed to really does lend support to the thesis under consideration or it may reveal that this is not the case. I shall argue that closer inspection shows that no support for the rigidity thesis can be derived from the intuitive argument. It even lends support to the opposite claim that proper names and descriptions do *not* in principle behave differently in modal contexts. But before turning to this let me briefly safeguard against some possible misunderstandings of the direction of my critique. Neither shall I take up Paul Ziff's (1977, 325-7) point and dispute the truth-value difference the Kripkean alludes to, nor shall I object like Jason Stanley (1997, 569 & 578) that alluding to it does not refute *descriptivism* (i.e. the claim that for every proper name there is a synonymous description). Both criticisms deserve to be taken seriously but relate rather indirectly to the considerations of the present talk. Also, the argument spelled out in my reconstruction should not be confused with the modal argument recently discussed by Scott Soames (1998, 13–17). The argument Soames is concerned with takes the assumption that proper names are rigid designators as a *premise*; by contrast, the intuitive argument sketched above aims at deriving this claim as a *conclusion*. Thus the arguments are quite distinct and the argument discussed here is conceptually and methodologically prior to the argument discussed by Soames.[4]

Given this, it will be clear that the point I am after in this talk is a different one. I take as my starting point the assumption that *there is* a truth-value difference for the Kripkean to detect. And I shall try to bring to light the conditions under which that truth-value difference *obtains*. So I shall be concerned exclusively with interpretations of utterances that make the case for the Kripkean truth-value difference. However, given the approach taken here this involves making explicit the hidden test conditions. One of these conditions seems to be the requirement that on of the test expressions be used *attributively*. Quite generally, it seems that in order for the truth-value difference the Kripkean detects to occur two requirements must be complied: Firstly, both the primary test expression and the expression for comparison must be taken to apply to the same object throughout the utterance; secondly, one of the expressions involved must be taken to be used referentially and the other must be taken to be used attributively. Otherwise, no relevant truth-value difference of the sort the Kripkean witnesses seems to emerge.

Let me start by explaining the need for the first requirement. Both the primary test expression and the expression for comparison must be taken to apply to the same object throughout the utterance: If the primary test expression and the expression for comparison applied to numerically distinct objects (2) would be false (because of violating the implication carried by (2)

that the designatum of the one expression is identical with the designatum of the other.) Moreover, with respect to the primary test expression alone it is clear that if any resulting truth-value difference is to be relevant it must be used for the same object wherever a token of it occurs. Someone talking about Aristotle Onassis in the first sentence and talking about the philosopher in the second could not serve to establish the claim investigated here.

Now it could be thought that this case can be ruled out by making the assumption that every proper name names at most one object. An adherent of this view would have to insist e.g. that John Smith from, say, Islington and John Smith from, say, Hackney do not have the same name. However, Kripke regards such an assumption as a methodological simplification only (and I fully agree).[5] Strictly speaking, therefore, it is a *false* assumption, and in order not to make ourselves dependent upon a false assumption we must demand that both the primary test expression and the expression for comparison must be taken to apply to the same object throughout the utterance.

But this alone will not suffice so let me come to the second requirement. One of the expressions involved must be taken to be used referentially and the other must be taken to be used attributively: If both the proper name and the description were used *referentially* (for the same object, of course) something wrong would be said with both (1) and (2) it seems and no truth-value difference Just reconsider (2):

(2)   Aristotle might not have been the inventor of formal logic

By the very law of identity, Aristotle is *necessarily* identical with himself. However, with respect to the referential use of both the description and the proper name (2) seems to amount to quite the opposite claim. Therefore, the primary test expression and the expression for comparison must not both be used referentially. Apparently, however, they must not both be used attributively either: It is sometimes said that in using an expression attributively we are talking about the entity to which the expression "really" applies (Donnellan 1966, 285). Now the entity to which the proper name *really* applies could not have been different from the entity to which the description *really* applies since, again, they are one and the same entity. Thus, a truth-value difference can only result if one of the expressions is used attributively and the other expression is used referentially, and this is why we need the second requirement.[6]

4.   The relevance of the referential-attributive distinction defended

In the light of the foregoing it appears that the test for rigidity has an intimate connection with the referential-attributive distinction. We even appealed to it to make the case for the Kripkean truth-value difference. Now,

like Kripke I assume that the referential-attributive distinction is pragmatic rather than semantic:[7] The difference concerns *what is communicated* by a certain speaker's use of an expression rather than *what is expressed* by the expression itself, and neither the attributive use nor the referential use of an expression can be properly understood without taking the speaker's *communicative intentions* into account. Given this we seem to face a certain problem, though. Kripke wants to make a semantic claim but the reconstruction offered only provides a basis for a pragmatic claim. Moreover, Kripke explicitly says that his views in *Naming and Necessity* have "no special connection with the referential-attributive distinction." (Kripke 1977, 24). Does this mean the reconstruction went wrong?

To be sure, on my reconstruction of the argument the semantic difference is a genuine semantic difference, namely a difference in truth-value. However, it is not a difference between the propositions expressed by instances of

(MS-1) α might not have been α
(MS-2) α might not have been β

but a difference between what is communicated by someone using the expressions to be substituted for "α" and "β" in the way indicated. Can the Kripkean simply object in the following fashion?

> Well and good. You have demonstrated that my test sentences can be *used* in a way such that the intuition of there being a truth-value difference involved is accounted for. However, I was suggesting that the truth-value difference is a result of what these sentences *express*.

This rejoinder seems quite inappropriate. It is not enough to assure that the truth-value difference involved concerns the propositions expressed. We need a reason for assuming this, especially now that an explanation of the intuition that there is a truth-value difference has been given. The Kripkean has to offer an alternative explanation of the intuition underlying the argument under consideration, and there is only one explanation I can think of that does *not* invoke the referential-attributive distinction, namely the appeal to *Russell's Theory of Descriptions*. Due to the scope of the logical operators involved the Russellian analysis of description sentences gives rise to at least two different readings of (2):

(1') $\Diamond \neg$ (Aristotle = Aristotle)
(2') $\Diamond \neg (\exists x) (Fx \ \& \ (y) (Fy \rightarrow y = x) \ \& \ (x = \text{Aristotle}))$
(2") $(\exists x) (Fx \ \& \ (y) (Fy \rightarrow y = x) \ \& \ \Diamond \neg (x = \text{Aristotle}))$

In the first of these readings, (2) expresses a true proposition, in the second it does not. Since Aristotle *did* invent formal logic it is wrong to say "one and only one person is the inventor of formal logic and it is possible that this person is not identical with Aristotle". Thus (2″) is false. But it is surely possible that it is not the case that one and only one person is an inventor of formal logic and is identical with Aristotle. For all we know formal logic might have been invented by Plato. Hence it is possible that the unique inventor of formal logic is not Aristotle. Therefore, (2′) is true, and in this reading of (2) there is a truth-value difference with respect to (1).

If the Kripkean relies on this line of thought, his claim that (1) and (2) have different truth-value would have to be refined. He would have to say that there is at least one reading of the test sentences according to which they differ in truth-value. Note, however, that if the Kripkean relies on this point to make the case for the truth-value difference he explicitly makes himself dependent upon the quantificational analysis of descriptions. His rigidity thesis will only be true if this analysis is correct. Now, Russell's analysis is very widespread, perhaps even more widely accepted than Kripke's, still I think that we have reason not to make Kripke's point dependent upon Russell's. On the one hand, Russell's analysis might turn out to be false. On the other hand, Kripke explicitly objects to tracing rigidity back to scope interaction phenomena in modal contexts. Let me explain.

Of course, this is not the place to recapitulate the old and long controversy about Russell's theory. Still, let me briefly draw your attention to a new argument apt show that Russell's theory is problematic. What is it to be a description? Russell is very explicit about that being a (definite) description is a matter of grammatical form only.[8] But what is the grammatical form of descriptions? We are usually told little more than that a (definite) description is "a phrase of the form 'the so-and-so' (in the singular)" (Russell 1910, 205; 1919, 167). Clearly, this does not qualify as an exhaustive answer—just think of Russell's own example "my only son" (1905, 47). Moreover, the restriction to *singular* expressions is implausible too.[9] Does this mean that Russell's claim that being a description is a matter of grammatical form only is void? Not at all. All things considered, it seems that the claim can be substantiated independently, namely by taking descriptions to be *determiner phrases* in a linguistic sense of the word: functional phrases of the structure DP → Det NP, whose head is a determiner—e.g. the definite article—and whose complement is a noun phrase—e.g. a noun.[10] On this interpretation, both phrases like 'the so-and-so' and phrases like "my only son" would count as descriptions: both are determiner phrases. Quite generally, interpreting descriptions as determiner phrases allows us to show that *all* examples that Russell and his followers are prepared to count as descriptions do really qualify as such. (Think, e.g. of Donnellan's example "Smith's murderer".) So it seems that the Russellian claim that being a description is a matter of grammatical form only can be plausibly defended

and even be made more precise: Being a description truly *is* a matter of grammatical form (namely a matter of belonging to the functional syntactical category of determiner phrases).

However, once we accept the unbracketed part of the claim only (let alone both) we seem to run into serious difficulties. Syntactical form (in the sense of belonging to a certain syntactical category) is now taken to be decisive, but by parity of syntactical form we are now barred from excluding *demonstrative*, *plural*, *mass*, or *incomplete* expressions like "this vase", "the men in the yard", "the gold in Zurich", and "the table" from the list of descriptions.[11] Thus it appears that the Russellian analysis of descriptions has to give an adequate account of such expressions also. It is doubtful, though, whether it can really deal with them. Of course, there have been many efforts to integrate descriptions of these kinds into a more general Russellian analysis but the success of these efforts has been at best partial, leaving open the question of whether a Russellian analysis can be reconciled with all the recalcitrant expressions mentioned.[12] So it seems that there are both methodological and substantial reasons for not making the correctness of the Russellian analysis a precondition of the correctness of Kripke's rigidity thesis.

Indeed, if we look at *Naming and Necessity*, such an approach appears to be quite faithful to Kripke's original intentions, and this is the second reason why we should not make Kripke's rigidity thesis dependent on Russell's Theory of Description: In the preface Kripke argues explicitly that his views do not reduce to alluding to scope interaction phenomena of descriptions in modal contexts. We may add on his behalf, then, that his views do not rely on such phenomena either.

Once we take serious the foregoing considerations it seems that we neither could nor should resort to the Russellian Theory of Descriptions to make the case for the truth-value difference. Remember, however, that to detect the truth-value difference is vital for the intuitive Kripkean argument. So since there does not appear to be a way of reconstructing this truth-value difference without invoking the referential-attributive distinction it seems that we have to be content with the reconstruction sketched above. And since the result of the test thus ultimately rests upon pragmatic features of the expressions used it seems that rigidity, contrary to what is generally assumed, is best conceived of as a pragmatic property.

5.   Applying the Kripkean test in reverse order

It has become clear that rigidity is best accounted for as a *pragmatic* property (of (uses of) expressions) rather than a semantic one. In the remainder of the talk I shall argue that the test yields an even more surprising result: proper names and descriptions do not in principle behave differently in modal contexts.

Up to now, we have used a proper name as primary test expression and a description as expression for comparison. But now note that nothing hinders us from applying the test in the reverse order, using a description as primary test expression and a proper name as expression for comparison:

(3)   The strange looking man might not have been the strange looking man

(4)   The strange looking man might not have been Jones

For illustration, we might sketch a corresponding scenario: The police find an inscription in the sand reading "Jones" and they have no idea whoever (or whatever) this might be. However, they notice a strange looking man watching the scene from a distance. As soon as the man realizes that the police have spotted him he disappears in the crowd. Certainly, the strange behaviour of the man gives rise to some speculations. One officer says: "We should have tried to catch the man. It might have been Jones." The other replies: "Don't worry. He might as well not have been Jones." Unbeknownst to the policemen, however, the strange looking man actually was the person referred to by the inscription in the sand. Given this, it is perhaps easier to see that (3) and (4) can be used in a way such that they provide an instance of the schemata (MS-1) and (MS-2) listed above.[13] However plausible this scenario might be, though, the essential point remains: if we submit these sentences to the very same test conditions as the sentence pair (1)/(2) we must conclude that the very same truth-value difference is found: If the description "the strange looking man" is used referentially for the same object throughout the utterance, and if the name "Jones" is used attributively, and, finally, if these expressions happen to apply to the same object then the utterance of (3) comes out as false while (4) comes out as true.[14] This result now prompts the same sort of consideration as in the case of sentence pair (1)/(2) albeit the other way round:

Where the first sentence contains a repeated occurrence of the description "the strange looking man", the second one contains a proper name naming the very same person. But (3) and (4) differ in truth-value. This suggests that we have a semantic difference—a difference in truth-value—caused by variation within a modal context—the predicate "might not have been". Since (3) and (4) differ only with respect to the replacement of a description with a proper name naming the same object, it is sensible to assume that this semantic difference is caused by a semantic modal difference between the description and the proper name. And since the expressions are co-designative it is tempting to trace this difference back not to a difference with respect to what they designate but to a difference in *the way in which they designate it*. The description appears to call for a constant interpretation even in modal contexts like "might (not) have been" while the proper name seems not to demand this. Thus we arrive at a very unpleasant conclusion

from the Kripkean point of view: The difference in truth-value of sentence pairs like (3) and (4) illustrates that descriptions are *modally rigid* while proper names are not.

6.   Two morals

When applied in reverse order the test for rigidity derives a bizarre result. Thus it casts doubt on the plausibility of the Kripkean case in the first place. On the whole it seems that the considerations proposed neither succeed in establishing the one claim nor the other. Given the reconstruction presented it is easy to see the mechanisms responsible for this: The results obtained by the application of the test depend only upon the order in which insertion takes place and the way in which the expressions inserted are used. So there is a negative conclusion to be drawn: the test can hardly demonstrate a *semantic* modal asymmetry between proper names and descriptions since its result depends on non-semantic features of the test conditions only. (We might even substitute *both* $\ulcorner\alpha\urcorner$ and $\ulcorner\beta\urcorner$ by co-designative descriptions (or co-designative proper names) and we would still get the same result.)

Besides this negative conclusion, however, there is also a positive moral to be drawn. Although the test does not demonstrate what the Kripkean wants it still does establish a substantial point: Both proper names and descriptions can be *used* in a way that constancy of interpretation concerning the evaluation with respect to (alternative) possible worlds is required. We may express this considerably shorter: the test shows that both proper names and descriptions can be *used rigidly*. Roughly, an expression $\ulcorner\alpha\urcorner$ will be used rigidly by x (with respect to a certain context of use U) if (with respect to the communicative intentions of x as manifested in her usage of $\ulcorner\alpha\urcorner$ with respect to U) it is used for the same object with respect to every (U-revelant) circumstance of evaluation. Of course, this could be further explored. For the time being, however, suffice it to say that in the light of the consequences drawn the notion of rigidity does not cease to be important. So unlike Kent Bach I do not regard rigidity as an *illusion*—even though I fully subscribe to the following claim:

> Kripke's appeal to our intuitions about names may be compelling, but I believe these intuitions have been misdescribed as semantic and are properly understood as implicitly pragmatic. (Bach 1994, 168)

In the present talk I have tried to make explicit in which sense the intuitive argument is properly understood as appealing to pragmatic features. Actually, this seems to be an instance of a strategy once adopted by Robert Stalnaker, namely "the general strategy […] to use pragmatic theory […] to take some of the weight off semantic and syntactic theory." (Stalnaker 1978,

331). It seems that the behaviour of natural language expressions in modal contexts lends itself neatly to an application of such a strategy.

7.   An alternative focus?

I aimed at giving a strong and charitable reconstruction of the intuitive argument which is so vital for the Kripkean rigidity thesis. However, I ended up with a critical analysis of that view. This might raise the issue of whether the reconstruction given really was charitable enough. Let me come to a close by taking up one possible worry.

Throughout my reconstruction I have been focussing on the contrast between the members of sentence pairs like (1)/(2):

(1)   Aristotle might not have been Aristotle
(2)   Aristotle might not have been the inventor of formal logic.

Still, one might be tempted to ask whether this really was the right focus. An objector might wonder whether we had not better considered sentence pairs like this one:

(1)   Aristotle might not have been Aristotle
(5)   The inventor of formal logic might not have been the inventor of formal logic.

The objector might be inclined to argue in a first step that (1) has a true reading while (5) has not and add in a second step that the (assumed) lack of the true reading of the first indicates that the proper name "Aristotle" is a rigid designator. Those sympathetic with objections of this kind will assume that I have not accounted for sentence pairs of the latter type. But they face at least two general questions. Does their move render the results of the reconstruction given above irrelevant? Does their move really introduce a new perspective? I shall briefly indicate why I think that the answer to both of these questions is no.

The envisaged strategy involves claiming that (5) is ambiguous. Since there are two kinds of ambiguity, syntactic ambiguity and semantic (i.e. lexical) ambiguity, we shall have to ask what kind of ambiguity is claimed for (5). Is (5) said to be syntactically ambiguous or is its ambiguity derived from a lexical ambiguity of its constituent expressions? Should the opponent prefer syntactical ambiguity it is hard to see how his objection could not be either ad hoc or an implicit appeal to a Russellian analysis of descriptions. But the latter case was already discussed. Should the opponent prefer to claim that (5) is ambiguous due to the lexical ambiguity of (one of) its constituent expressions, however, there are two problems I would like to point out. First of all, the likely constituent expression to be blamed for being am-

biguous will be the description involved. But on this view it is hard to see what advantage there is in focusing on pairs like (1)/(5) instead of pairs like (1)/(2) for, clearly, if (5) is ambiguous because the description "the inventor of formal logic" is, so is (2). Pairs of the latter type were already discussed, though. Moreover, the advocate of the semantic ambiguity claim faces another problem: What kind of systematic lexical ambiguity of descriptions does he claim if not the assumed systematic ambiguity of descriptions used referentially and descriptions used attributively? If this is what he resorts to he will have to make a concession Kripke is not prepared to make. He will have to subscribe to the view that the referential-attributive distinction is semantically significant despite the fact that Kripke argues at length that it is a pragmatic distinction. Against this background it seems clear that focusing on pairs like (1)/(5) rather than pairs of the type I originally considered neither renders the reconstruction presented irrelevant nor introduces a new perspective. We are simply referred back to the same issues discussed in the original reconstruction.

8.   Conclusion

What is the general impact of the investigation presented above? One way of viewing it is that it measures the price we have to pay in order to stick to the rigidity thesis. Understood as a thesis concerning the semantics of natural language expressions the rigidity thesis is not to be had without presupposing a Russellian analysis of descriptions or without assuming the semantic significance of the referential-attributive distinction. For those of us who, like Kripke, are not willing to pay this price the consequence is a different one. It consists of regarding both the notion of rigidity and the test invoked to attribute a given expression that property as pragmatically rather than semantically relevant.

It is perhaps helpful to point out that this consequence is less threatening than one might think. For instance, the application of quantified modal logic to natural language is not at all revealed as dubious or even unwarranted. What we might have to admit, though, is that in formalising our talk about counterfactual situations some pragmatic features of this talk are being captured too. It may well be that Kripkeans will not admit this light-heartedly. But then it is hard to see what relevance their talk of rigidity might have with respect to natural language expressions.[15]

Notes

1   In this talk, the distinctions between rigidity *de jure* and rigidity *de facto* (Kripke 1980, 21) and between *obstinately* and *persistently* rigid designators (cf., e.g. Stanley 1997, 556) are deliberately neglected. Both these distinctions are useful and important but since the considerations presented below do not direct their critical impetus against them I leave their discussion for another occasion.

2   For this reason, I think that Jason Stanley's approach in Section 4 of his "Names and Rigid Designation" either falls short of being a reconstruction of the intuitive argument or does not, contrary to what he (1997, 565) suggests, depend upon the premise that variables under an assignment are rigid designators.

3   It is clear that the predicate is complex and might well be further decomposed; it is far from clear, however, that this matters for the outcome of the test.

4   Soames addresses the issue discussed here only in passing. He writes: "Our ultimate ground for thinking that the name *Aristotle* is a rigid designator is our conviction that there is a certain individual x, such that for every possible world w, the proposition that Aristotle was a philosopher is true at w iff x was a philosopher at w, and similarly for other propositions." (Soames 1998, 2) Unfortunately, the reconstruction he offers can hardly claim to be modally innocent or intuitive. So it does not seem to qualify as a reconstruction of the intuitive argument aimed at here.

5   Cf. Kripke 1980, 7. Also, cf. Cartwright (1997, 68): "Kripke intends the thesis […] to be compatible with the fact that most proper names are names of more than one thing. […] Indeed, Kripke's entire discussion of these matters proceeds under the simplifying assumption common in philosophical discussions of reference, [that] no proper name designates more that one thing."

6   I take it to be a plausible requirement that the primary test expression is to be used systematically, i.e. in the same way wherever it occurs, so I ignore the possibility that the truth-value difference results from using it once referentially and once attributively.

7   Let me briefly remind you of Donnellan's classic example (1966, 285–6): (i) Shocked by the brutal manner of the killing (but not knowing who did the deed) we exclaim: "Smith's murderer must be insane!" (ii) Watching the trial against Jones (who is charged with murdering Smith) we sum up our impression of his strange behaviour by saying "Smith's murderer must be insane!" The first case involves an attributive use of the description, the second its referential use. For Kripke's view cf. Kripke 1977, 21. Note, however, that he tends to describe the attributive use of an expression in terms that easily suggest the mistaken view that using an expression attributively is a semantic usage rather than another pragmatic one.

8   Cf. Russell's emphasis in his 1918, 244: "I want you to realize that the question whether a phrase is a definite description turns only upon its form […]." Here, "form" is best understood as *grammatical* form. This is also what Russell suggests elsewhere (1919, 168.) Note that *pace* Linsky (1967, 62-63) we must not read "form" as "logical form" on pain of trivializing the Russellian analysis.

9 Russell seems to impose this restriction because he takes expressions like "the inhabitants of London" to stand for *classes* (1919, 181). But this is not a good reason to exclude them since an expression like "the class of inhabitants of London" surely qualifies as a description yet it stands for a class and is grammatically singular. Quite generally, the singular-plural distinction is irrelevant for grouping along syntactical (or logical) categories. Neither for being a proper name nor for being a general term nor for being a quantifier is it a necessary condition to be grammatically singular (or plural). If we want to treat descriptions grammatically (or logically) on a par with expressions of these categories we must not subscribe to the singularity restriction.

10 Cf. Abney 1987. In this talk, determiners are understood as elements of the functional syntactic category DET consisting of the definite article ("the"), the demonstrative determiner ("this"), and the determiner element in genitive noun phrases ("Gray's Elegy") and possessive pronouns ("my"). Noun phrases are taken to be phrases expanding roughly along the following lines: NP → (AP) N (PP). For our present concern, linguistic questions of the type of whether the determiner involved in descriptions like "my only son" is the personal pronoun or the possessive morpheme may safely be ignored.

11 Nor are we allowed to exclude generic descriptions ("the whale is a mammal"). They do not seem to constitute a particular problem for the Russellian analysis, though.

12 Cf. e.g. Sharvy 1980; Neale 1990.

13 It will not help to object that the modality involved in this story is perhaps epistemic rather than metaphysic since this applies to sentence (2) as well. Anyway, all that is required for the argument is that (4) and (3) be interpreted in the same way as (1) and (2)—and this is surely possible.

14 Some theorists might be tempted to object that proper names cannot be used other in another way than being used referentially. Kripke, however, does not have such reservations: He holds that Donnellan's distinction *does* apply to proper names and that proper names *can* be used attributively (cf. e.g. Kripke 1980, 25 & 1977, 18.) Actually, there is nothing mysterious about attributive uses of proper names. Think of a doctor at a hospital scanning though some medical records. He may ask the nurse "Who is Jones? Have I seen him before?" Is he using the proper name referentially, as "merely one tool for doing a certain job" (to use Donnellan's phrase), or attributively? Clearly, to assume the former is to render his question pointless.

15 Versions of his talk were presented to the research group "Attitudes, Concepts, Objects" of the Doctoral Programme of Cognitive Science at the University of Hamburg early in spring 1998 and to the Oxford Graduate Conference in Fall 2000. I thank the participants of these meetings, the participants of the present conference and some other friends and colleagues for discussion, critical questions and helpful remarks. In particular, I am indebted to Kent Bach, Bill Brewer, Carola Eschenbach, Ben Hoefer, Andreas Kemmerling, Wolfgang Künne, Severin Schroeder, Mark Siebel, Mark Textor and Timothy Williamson.

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