BUENOS AIRES SYMPOSIUM ON RIGIDITY: RESPONSES

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Abstract

In this article the following criticisms of the essentialist conception of general term rigidity presented in the previous papers are considered and responded: (i) the identity of designation conception of rigidity can provide us with a better alternative account for general term rigidity (Orlando), and (ii) the essentialist conception fails to meet the condition of extensional adequacy, both because it (allegedly) over –and undergeneralizes (Zerbudis). Against (i), it is claimed that the proposed definition of general term rigidity cannot feature in lost rigidity arguments against description theories because it is circular, and then fails to do the primary work that rigidity is supposed to do, namely, distinguishing terms that are covered by a description theory from those that are not. As regards (ii), after insisting that the essentialist view need not be commited to the condition of extensional adequacy, both charges of over– and undergeneralization are addressed: while the argumentation aimed at showing that some examples (such as 'paperweight') are cases of overgeneralization is rejected, the cases of undergeneralization (of the likes of 'frog') are admitted to be still in need of a better explanation than the one given in Devitt (2005).

KEY WORDS: Rigidity; Designation; Description theories of terms; General term; Natural kind term.

Resumen

En este artículo se analizan y responden las siguientes objeciones a la concepción esencialista de la rigidez para términos generales contenidas en los artículos precedentes: (i) la concepción de la rigidez como identidad de designación puede proporcionarnos una definición más adecuada del carácter rígido de los términos generales (Orlando), y (ii) la concepción esencialista no puede cumplir con la condición de adecuación extensional, tanto debido a que (supuestamente) sobregeneraliza como a que subgeneraliza (Zerbudis). Contra (i), se sostiene que la definición alternativa propuesta no puede ser utilizada en argumentos basados en la pérdida de rigidez contra las teorías descriptivistas porque es circular; por consiguiente, fracasa en alcanzar el objetivo principal que se adscribe a la noción de rigidez, a saber, distinguir las expresiones que pueden ser explicadas en términos descriptivos de aquéllas para las cuales ello no es posible. En lo que concierne a (ii), tras insistir en que la concepción esencialista no tiene por qué comprometerse con el requisito de adecuación extensional, se consideran las dos acusaciones mencionadas: mientras que se rechaza la argumentación tendiente a mostrar que ciertos ejemplos (tales como 'pisapapeles') son casos de sobregeneralización, se acepta que algunos casos de subgeneralización (tales como 'rana') requerirían de una explicación más adecuada que la presentada en Devitt (2005).

PALABRAS CLAVE: Rigidez; Designación; Teorías descriptivistas de los términos; Término general; Término de clase natural.

Earlier versions of the papers in this symposium were delivered at a very enjoyable workshop in Buenos Aires in March 2008. I thank Eleonora Orlando for organizing that workshop. And I thank her and Ezequiel Zerbudis for their interesting contributions. I will respond to them in turn.

1. Eleonora Orlando

Saul Kripke defines a rigid designator as one that designates the same object in every possible world in which that object exists. If a designator does not do this it is non-rigid (1980, p. 48). Kripke introduces this distinction when discussing singular terms and his application of it in that context is straightforward and convincing. He goes on to apply the distinction also to general terms, without giving a new definition (pp. 127-136). How are we to make sense of this? One way of doing so sticks with the above definition by taking general terms to designate *abstract objects* of some sort, kinds, properties, or attributes. Orlando favors this way and calls it "the identity of designation" conception of rigidity. For her, a general term is defined as rigid if and only if it designates the same property (attribute) in every possible world.[1] Or so it seems at first.

Proposals of this sort were seen to have a problem from the very beginning. I summed the problem up in "Rigid Application" (2005)[2] as follows:

> Too many terms, including ones like 'pencil', 'hunter', and 'bachelor', seem to come out rigid. Yet, the criticism continues, rigidity was supposed to distinguish natural kind terms from these non-natural ones. The proposal seemed to trivialize the notion of rigidity by making it indiscriminate. (p. 140)

I went on to criticize a proposal of this sort by Joseph LaPorte (2000). Let me adapt my criticism by replacing LaPorte's example with Orlando's. LaPorte avoids trivializing rigidity by claiming that whereas kind terms like 'red' are rigid, those like 'Lucio's favorite color' are not. The latter term designates *being red* in this world but perhaps *being green* in another. But what is the basis for this claim? It depends on the unargued assumption that there is not a property of *being Lucio's favorite color*. For, if there were, 'Lucio's favorite color' would surely designate it

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[1] Oddly, she does not state this definition explicitly but it is clearly implicit.
[2] All unidentified references to my work are to this paper.

in every possible world and hence be rigid. So Laporte's proposal rests on "selective realism" about universals, a piece of highly controversial metaphysics. The rigidity issue is left with little substance beyond this metaphysical issue.

I went on to make another criticism of LaPorte. This arises from the methodological requirement that a notion of rigidity "must do some theoretical work" if it is to be of any interest (p. 140). I argued that LaPorte's proposal fails this requirement and suggested that this will be the fate of all similar ones. It is, indeed, the fate of Orlando's proposal. And it is very instructive to see why.

On the basis of the theoretical work that Kripke did with rigidity in discussing proper names, I argued that "the primary work we should expect from a notion of rigidity for kind terms is featuring in lost rigidity arguments against description theories of meaning for some terms" (p. 145). These arguments have the following form: the term in question is rigid; a description of the sort that the description theory alleges to be synonymous with the term is not rigid; so, the term is not synonymous with that description and the theory is false. LaPorte did not even attempt to do this work with his notion of rigidity. Orlando thinks she is doing it but she is not. Here's why.

Orlando aims to solve the trivialization problem without committing to a selective realism like LaPorte's. So she needs some basis other than the nonexistence of *being Lucio's favorite color* for saying that 'Lucio's favorite color' designates *being red* in this world but maybe *being green* in another. She finds this basis by extending John Perry's theory of singular terms (2001) to general terms:

> a *referential general term* may be said to (directly) *name* a property, whereas a *descriptive one* may be considered to *denote* a property by means of *expressing/connoting* a different, more complex one.

She then applies this theory to her example: whereas 'red' is a referential term, 'Lucio's favorite color' is a descriptive one. What has all this got to do with rigidity? Well, because 'red' is referential it designates the same property, *being red*, in every possible world and is rigid; whereas, because 'Lucio's favorite color' is descriptive it designates different properties from world to world, sometimes *being* red, sometimes *being green*, and so on. So, *Orlando's basis for classifying a term as rigid is that it is not covered by a description theory*. So, manifestly, the claim that it is rigid cannot be the premise of an argument to the conclusion that it is not covered by a description theory! Such an argument would

be blatantly circular. So, Orlando's notion of rigidity cannot feature in lost rigidity arguments. So, her notion fails to do the primary work that rigidity is supposed to do.

Indeed, as her paper progresses Orlando moves further and further away from her initial definition of a rigid term as one that designates the same property in every possible world toward a definition of it as one that is non-descriptive. Thus, toward the end of section 1, she claims that "general terms can…be classified as rigid or non-rigid, *at least partly,* on the basis of their non-descriptive or descriptive character respectively" (original emphasis). By section 3 the qualification has gone: "on the proposed account, general terms are classified as rigid and non-rigid on the basis of the absence or presence, respectively, of a descriptive component." This classification seems to be definitional. Certainly, she has provided no other basis for classifying a term as rigid. So when she goes on to claim that "rigid general terms, precisely because they are non-descriptive ones, cannot be covered by a description theory" she is simply expressing a tautology: rigid terms are *defined as* non-descriptive. She has, in effect, given the game away of coming up with a *useful* notion of rigidity.

The failure of Orlando's account of rigidity to do the needed theoretical work becomes more vivid when we move away from terms that are *obviously* descriptive like 'Lucio's favorite color' to ones like 'tiger', 'lemon', 'water' 'pencil' and 'pediatrician' where there has been genuine debate about whether they are descriptive.[3] If we could establish that one of these terms designated the same property in every possible world then *maybe* we could mount a lost rigidity argument against its being synonymous with certain descriptions. But how could we establish that it does so designate? Orlando's notion of rigidity tells us only that *if the term is non-descriptive* then it does so designate. But, of course, if we already knew that it was non-descriptive we wouldn't need a lost rigidity argument that it was non-descriptive! Indeed, we wouldn't need any argument at all.

What has gone wrong? I think two things. First, although Orlando agrees with my view that the primary task of a notion of rigidity is "to distinguish kind terms that are not covered by a description theory from ones that are", she misunderstands it. As our earlier quote shows, the right way to understand it is as follows: the task for rigidity is *to feature in lost rigidity arguments against description theories*. Thus suppose that

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[3] For an example of the debate, see Devitt and Sterelny (1999, pp. 83-101).

we are assessing the description theory that 'Aristotle' is synonymous with 'the last great philosopher of Antiquity'. Consider the truth conditions of:

(1) Aristotle was fond of dogs
(2) The last great philosopher of Antiquity was fond of dogs

I capture Kripke's lost rigidity argument as follows:

> In the actual world, the truth conditions of both (1) and (2) involve a certain ancient Greek that we refer to by 'Aristotle': if he liked dogs they are both true; if not, not. But in the nonactual situation where that person died before reaching maturity and studying philosophy, the truth conditions of (1) and (2) would involve different people. The truth of (1) would still depend on whether the person we refer to by 'Aristotle' was fond of dogs: 'Aristotle' is a rigid designator. But the truth of (2) would depend on whether whoever then fitted the description 'the last great philosopher of Antiquity' was fond of dogs, presumably on whether Plato was: that description is a non-rigid designator. So this description theory of `Aristotle' is wrong. (p. 144-145)

This is an effective argument against the description theory because it rests on two intuitive premises *that are independent of the theory*: the premise that, in the nonactual situation, 'Aristotle' designates the person we call 'Aristotle'; and the premise that, 'the last great philosopher of Antiquity' does not. The premises are independent because they are about rigidity defined as designation in every possible world. If rigidity were defined as non-descriptivity then rigidity's distinction of the non-descriptive from the descriptive would be trivial and rigidity could not feature in an effective argument against the description theory.

The second thing that has gone wrong arises from a conflation of the *definition* of rigidity with its *source*. Kripke defines rigidity as designation in every possible world. Wherever a term is rigid, we have an interest in explaining the source of that rigidity. The explanation is often, although not always, that the term is non-descriptive; the rigidity of 'Aristotle' is an example. This appeal to the non-descriptive nature of 'Aristotle' to explain the source of its rigidity does nothing to undermine the lost rigidity argument that 'Aristotle' *is* non-descriptive. However, as I have demonstrated, if rigidity is defined as non-descriptivity then it cannot feature in a lost rigidity argument. Orlando talks often of the non-descriptive source of rigidity and it rather looks as if she slides from this into treating non-descriptivity as the definition of rigidity; see earlier

quotes. This slide may explain why, although she gives a nice explicit statement of the lost rigidity argument for names, she never gives one for general terms. I suspect that had she attempted a statement she would have seen that defining rigidity as non-descriptivity destroys the argument.

I concluded my discussion of LaPorte as follows:

> LaPorte's rigidity distinction rests on controversial metaphysics, does little if any theoretical work, and is left unexplained and mysterious. I think that this will be the fate of any attempt to base a distinction on the idea that some terms rigidly designate abstract objects. (p. 154)

This still seems right to me.

2. Ezequiel Zerbudis

My paper is largely devoted to demonstrating the virtues of a different notion of rigidity for general terms. The notion is one of rigid *application*:

> a general term '*F*' is a rigid applier iff it is such that if it applies to an object in any possible world, then it applies to that object in every possible world in which the object exists. Similarly for a mass term. (p. 146)

I argue that rigid application can do much the same theoretical work for kind terms as Kripke's rigid designation does for singular terms, the work of refuting description theories and of explaining certain modal *phenomena*.

Stephen Schwartz (2002) argued against an earlier presentation of this view (Devitt and Sterelny 1999, pp. 85-86) on the ground that rigid application does not mark out the class of natural kind terms: it does not satisfy what Zerbudis calls "the condition of extensionality". Schwartz claims, on the one hand, that some nominal kind terms like 'television set' are rigid appliers. Zerbudis agrees, saying that my notion "overgeneralizes". Schwartz claims, on the other hand, that some natural kind terms like 'frog' are not rigid appliers. Zerbudis agrees again, saying that my notion "undergeneralizes". My response to Schwartz was that "even if these claims are right, they are not grounds for dissatisfaction" with my notion of rigidity because it is not the primary task of such a

notion to distinguish natural from non-natural kind terms. "The primary task is to distinguish kind terms that are not covered by a description theory from ones that are" (p. 154). It follows that I reject Zerbudis' condition of extensionality.

Indeed, we should wonder why this condition has any *prima facie* appeal at all. First, as I note (p. 163, n. 29), 'natural kind term' is vague. The problem is that it is far from clear what is for a *kind* to be natural and which ones count as natural. Philip Kitcher says that "natural kinds are the sets that one picks out in giving explanations" (1984, p. 132, n. 16). Similarly, Richard Boyd says that "the naturalness (and the 'reality') of natural kinds consists solely in the contribution that reference to them makes to [the accommodation between conceptual and classificatory practices and causal structures]" (1999, p. 141). But, as I have pointed out, kinds that seem to be quite unnatural fit these accounts:

> being a motor car, a hammer, or a paperweight are causal-explanatory kinds. We need somehow to capture the idea that natural kinds are causal-explanatory "in science". (2009, pp. 201-202)

Second, however we tidy up our account of natural kinds, it is hard to see how 'natural kind term' could come out as a theoretically significant description in semantics. Thus, 'plastic' is not likely to be classified as a natural kind term and yet it is surely semantically just like the paradigmatic natural kind term 'gold': the two terms seem equally non-descriptive (p. 163, n. 23); and if there is any acceptable sense in which 'gold' is rigid then surely 'plastic' will be rigid in that sense too. Furthermore, the biological term 'predator' must be counted as natural and yet it seems descriptive and non-rigid. And what could be the principled basis for counting terms from the social sciences like 'unemployed' and 'nation' as not natural? Yet they are surely descriptive and non-rigid.

In sum, I think we should start an investigation of the sorts of cases that concern Zerbudis with the presumption that the condition of extensionality is probably false and is, in any case, theoretically uninteresting: 'natural kind term' does not cut semantic nature at its joints; it does not describe a natural kind! This having been said, the cases themselves deserve attention: it is interesting to know what terms *are* rigid appliers. So, let us turn to the cases.

Overgeneralization. Let us start with the cases of alleged overgeneralization: 'table', 'paperweight', and 'television set'. Each of these

terms refers to objects that have a certain function. The objects may have this function in virtue of being designed to have it but I emphasize another way that they can have it: by being regularly used in the appropriate way. I argue that these terms are not rigid appliers: objects that have the appropriate function in this world might lack it in another. So these terms do not lead to any overgeneralization of rigid application.

> *Function by design*: Consider this story of a certain wooden object. It was designed to be a table, made in a certain factory, and then sold and used as a table. The term 'table' clearly applies to it. But it might not have done. Suppose that this very object had been made in the same way from the same materials in the same factory but designed not to be a table but rather a light shade for a very modern building. And that was how it was used for its entire existence. Then the term 'table' would never have applied to it. So, 'table' is not a rigid applier (p. 156).

> *Function by regular use*: Consider this story of a certain stone. A person found it on a beach, brought it home, and used it for years to secure the papers in her study. The term 'paperweight' applies to it.[4] Yet, she might have left it on the beach, preferring another stone, and the term 'paperweight' would never have applied to it. So 'paperweight' is not a rigid applier (pp. 155-156).

Zerbudis is not convinced. He emphasizes a distinction between *what something is* and *what something is used for*. So something that is a cup might be used as a paperweight without being a paperweight. This is absolutely right, but beside the point. The occasional use of an object as a paperweight does not make it one, *regular* use does. As a result of this regular use the object is not only a cup but *also* a paperweight. Zerbudis objects that "it is a cup in a stronger sense than that in which it is a paperweight". It is hard to know what to make of this. If the stone in our story is a 100% paperweight then so also is a cup that is regularly used as a paperweight. And it seems to me indubitable that the stone is

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[4] Paperweights and the like are usually called "artifacts". Yet, as this story makes clear, such objects are sometimes not made by us. So I prefer to call them "implements". Zerbudis wrongly attributes to me the view that "for an object to become a particular implement/artifact it is sufficient that it be used as such." My view is actually that "the nature of an implement quite often involves not only a function but also a physical characteristic: a pencil is not a pen, a chair is not a stool, a sloop is not a yawl" (p. 163, n. 27).

a 100% paperweight. Zerbudis agonizes over this, wondering "whether or not whatever is involved in getting a paperweight out of a stone should be understood in terms of a relation of constitution". The answer is that it should *not* be so understood: the paperweight *is* the stone, it is *not constituted* by the stone. The object that is the paperweight/stone is constituted by stuff of some stony sort. But the object is one thing, the constituting stuff another.

Zerbudis also has trouble with my discussion of the light shade, arguing that "the *phenomena* that Devitt's example draws our attention to are simply *phenomena* of indeterminacy", that there are no determinate moments in processes of production at which objects that are the final products come into existence. But this is not what I am drawing attention to at all. I am drawing attention to *the natures of the final products*. Had the table which is the final product of a certain physical process been produced by that very same process but with different intentions for its future, it would have been a light shade not a table. That is my point.

Zerbudis is off on the wrong track again in the support he finds for the idea that any television set is essentially one:

> Suppose that I have a television set, and begin to take pieces from it one after another, until I completely dismantle it. I ask then, at every stage: "Is it still a television set?"' and "Is it still the same object as before?" It seems to me clear that the answers to both questions will always be the same, no matter which answer we actually give at any stage…

Replace the first question about *being a television set* with a question about the most accidental property of the set, indeed about *absolutely any* of its properties, and the answers would still be the same; replace it, for example, with "Is it still the first thing that Oscar goes to when he gets home?". Yet, manifestly, these same answers do not show that *being the first thing that Oscar goes to when he gets home* is an essential property of the set. When an object ceases to exist it is no longer around to have *any* of its former properties!

In discussing alleged overgeneralizations, Zerbudis has much to say about indeterminacy. I think indeterminacy is everywhere: it is indeterminate whether a certain x is a mountain, a certain y, a planet, a certain z, a tiger; it is indeterminate at what moment in production the table becomes a table and at what point in the use of the stone to secure paper it becomes a paperweight. But indeterminacy is irrelevant to our

essentialism issue. Some objects are determinately tigers and, I argue, those objects are essentially tigers. Some objects are determinately tables and, I argue, those objects are not essentially tables.

In sum, Zerbudis' discussion of overgeneralization is not a success. I am much more sympathetic to his discussion of undergeneralization.

Undergeneralization. 'Frog' is surely a natural kind term and I concede to Schwartz that it is not a rigid applier. I am not much bothered by this because, as already emphasized, I reject Zerbudis' condition of extensionality. Still, it is a bit disappointing that 'frog' is not a rigid applier and I set about removing the disappointment. This is where I now think I went wrong. Influenced by my earlier discussion of complex demonstratives and referential descriptions (2004), I am led to the following view:

> 'Frog' is rather like the complex term 'adult human'. That term has a part, 'human', that is explained by a causal theory (let us suppose) and a part, 'adult', that is explained by a description theory. (p. 158)

Zerbudis' rejection of this is forthright: "The keystone of the whole argument, namely, that terms like 'frog' are partly descriptive, is simply false." I think he is more right than I was about this. I need to start again at removing the disappointment of discovering that 'frog' is not rigid.

Given the work that rigidity is supposed to do I sought to define a new notion of rigidity that would refute certain description theories of terms like 'frog'. The analogy with demonstratives and descriptions led me down the wrong path to a notion of "weak rigidity". A better path is suggested by the fact that a frog is a *mature* or *fully developed* member of the kind tadpole-frog; similarly, butterfly, of the kind caterpillar-butterfly. So we define the following notion of rigidity for organisms:

> a general term '*F*' is a mature-rigid applier iff it is such that if it applies to an organism in any possible world, then it applies to that organism in every possible world in which the organism exists and develops to maturity.

'Frog' is a mature-rigid applier: anything that is a frog in some world will be a frog in any other possible world in which it exists provided it does not die as a tadpole. And this notion of mature-rigidity will serve to refute any description theory of 'frog' constructed in the usual way from

descriptions of the readily observable properties of frogs. In the actual world those descriptions apply to frogs but in another possible world they might apply not to frogs but to other organisms altogether. The descriptions form a non-mature-rigid applier.

My earlier failure to lessen the disappointment with a notion of weak rigidity led me to look to modes of reference:

> 'Frog' is indeed quite like the natural kind term 'tiger' in that its reference is largely determined causally. But it is a bit unlike 'tiger' in that its reference is partly determined by an associated description that picks out the frog-stage of a tadpole-frog. (p. 159)

Zerbudis makes a telling point in response: "It seems also reasonable to suppose that 'frog' is as semantically simple as 'tiger', *not involving in any sense a reference to its kinship with 'tadpole'*" (emphasis added). I now think I should not have rushed into this view of the mode of reference of 'frog'. Better to see the problem with that mode as an instance of the quite general, and very difficult, "*qua*-problem" for historical-causal theories of reference grounding.[5] The problem has been posed as follows:

> Any sample of a natural kind is likely to be a sample of many natural kinds; for example, the sample is not only an echidna, but also a monotreme, a mammal, a vertebrate, and so on. In virtue of what is the grounding in it *qua* member of one natural kind and not another? As a result of groundings, a term refers to all objects having the same underlying nature as the objects in the sample. But *which* underlying nature? The samples share many. What makes its nature as an echidna relevant to reference rather than its nature as a mammal (a nature it shares with kangaroos and elephants)? (Devitt and Sterelny 1999, p. 91)

Applying this general problem to 'frog', we should ask: in virtue of what is 'frog' grounded in just the frog-stage rather than the whole tadpole-frog? My answer in the paper is, in effect, that the grounder associates a description along the lines of 'adult'. But this seems implausible. I would have done better to be guided by the following response to the general problem:

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[5] I allude to this problem on two occasions (Devitt and Sterelny 1999, pp. 161, n. 10, 162 n. 18) but not here where I should have.

       People group samples together into natural kinds on the basis
of the samples' observed characteristics. They observe what the
samples look like, feel like, and so on. They observe how they behave
and infer that they have certain causal powers. At some level, then,
people "think of" the samples under certain descriptions –perhaps,
'cause of *O*' where *O* are the observed characteristics and powers– and
as a result apply the natural kind term to them. It is this mental
activity that determines which underlying nature of the samples is the
relevant one to a grounding. The relevant nature is the one that is, as
a matter of fact, picked out by the descriptions associated with the term
in the grounding. (Devitt and Sterelny 1999, p. 92)

For reasons we go on to give, this is not "close to a complete solution" to
this very difficult semantic problem. But it may be that the complete
solution would show that Zerbudis is right in thinking that 'frog' is
semantically similar to 'tiger': they might be alike in all respects except
the somewhat minor one that although they are both mature-rigid
appliers, only 'tiger' is a rigid one.

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