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'General terms, hybrid theories and ambiguitiy. A discussion of some experimental results'.[i]

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Abstract

In this paper I examine two sets of experimental results about the semantics of general terms, by Genone and Lombrozo (2012) and by Nichols, Pinillos and Mallon (forthcoming).

The results of the two experimental studies allegedly reveal significant variations in semantic intuitions among participants as regards the correct application of general terms. However, the two sets of authors propose two entirely different semantic treatments of general terms in order to explain the significance and the impact of those results. Genone and Lombrozo espouse a hybrid semantics whereas Nichols, Pinillos and Mallon are inclined towards an explanation that appeals to ambiguity.

I will cast some doubts on the coherence of a hybrid theory and argue in favor of the ambiguity approach. Nevertheless, I will argue that the sort of ambiguitiy Nichols, Pinillos and Mallon postulate is easy to incorporate to (and is in fact already contemplated by) non-descriptivist approaches to the semantics of general, as well as singular, terms.

1.-Introduction

In their 2004 paper 'Semantics, cross-cultural style', Machery, Mallon, Nichols and Stich [MMNS], concluded that there are wide variations in intuitions as regards how the reference of a proper name is determined. Some speakers tend to give descriptivist or internalist answers[ii] when asked who the referent of a certain use of a name is, whereas other speakers are more inclined to give externalist answers, answers that are in line with the chain of communication picture.[iii] The results of MMNS (2004) were established on the basis of probes run in the US and in Hong Kong, using vignettes very similar to Saul Kripke's fictional Gödel case from *Naming and Necessity*.[iv]

Two aspects of MMNS' discussion are worth noticing for our purposes here. Fist, MMNS were motivated by Richard Nisbett and colleagues' work on a wide variety of cognitive differences between Westerners and East Asians (Nisbett, R. E.; Peng, K.; Choi, I. and Norenzayan A., 2001). MMNS's probes are geared towards testing if there are also such cultural differences in the responses to questions about the referential link between a use of a name and what that use refers to. On the basis of the results, MMNS conclude that there is a tendency towards descriptivism among East Asians and a tendency towards the so-called causal-historical picture among Westerners, and they suggest that the intuitions that give support to the theory of reference are relative to culture.

It is not clear that the differences MMNS found are linked to culture since as further work suggests, there is also wide intracultural variation in the responses to cases like the ones posed by MMNS. In fact some authors suggest that the variations can be found even at an individual level, as the

same person may be inclined towards different responses in different situations.

The second aspect to notice is that MMNS focused on the debate between classical descriptivism and the so-called causal-historical picture as regards proper names. However, both approaches to reference provide also different explanations of the connection between general terms (natural and artifactual) and the things and samples those terms apply to. Other authors (Braisby, N.; Franks, B. and Hampton, J. 1996 and Jylkkä, J; Railo, H. and Haukioja, J. 2009) have brought the debate initiated by MMNS to bear on general terms.

In the past (Marti 2009 and 2012) I have criticized MMNS, and a subsequent piece by Machery, Olivola, DeBlanc (2009) that follows a similar strategy. In a nutshell, I have argued that the questions asked in the MMNS 2004 and in the Machery, Olivola and DeBlanc (2009) probes ask the participants to express an opinion as regards how the reference of a particular use of a name is determined, a question whose answer requires reflection on practice, and does not capture what the participants' practice really is, i.e., how they themselves use names.[v] I still have the same criticism, but for the purposes of this paper I will set the concerns mostly aside, to focus on other aspects of the debate. My purpose in this paper is to examine two proposals, by James Genone and Tania Lombrozo [GL] (2012) and by Shaun Nichols, Ángel Pinillos and Ron Mallon [NPM] (forthcoming).[vi] In their papers GL and NPM focus on general terms and away from the cross-cultural issues. Both sets of authors set out to test the tendencies towards descriptivism and towards the causal-historical picture among the population. They both

conclude that there are wide variations among speakers, that those variations are intracultural, and that they affect even individuals' responses depending on the situation. However, they differ in their assessment of what kind of theory would be supported by their results. Whereas GL favor some sort of hybrid theory, one incorporating elements of descriptivism and elements of the historical account, NPM defend the virtues of an account according to which general terms, or some general terms, are ambiguous: on some occasions of use the connection between an utterance of a general term and its domain of application follows a descriptivist recipe, whereas on other occasions the connection can be traced via a causal-historical chain of communication.

The discussion of these two proposals will lead to a reflection on the differences between the postulation of hybrid theories and the endorsement of pervasive ambiguity, as well as to an assessment of the kind of theory that the data collected by GL and NPM support.[vii]

2.- The two studies and their conclusions

GL (2012) extend MMNS' (2004) line of inquiry to general terms, both natural and artifactual. GL presented participants with stories that are similar to the Gödel case exploited by MMNS. In each one of the stories there were slight variations depending on the degree of fit of the definite descriptions that two different hypothetical speakers associated to a kind term, and depending also on the causal-historical connection of the term to the kind. The participants in GL's study were asked whether the two protagonists of the vignette were thinking and talking about the same kind under four sets of conditions: (i)

different description and same causal origin, (ii) same description and same causal origin, (iii) same description and different causal origin, and (iv) different description and different causal origin.

As expected, cases (ii) and (iv) were unsurprising, but the data collected about (i) and (iii) revealed that there is an important split among participants. When the description associated with a term by the protagonists of the vignette was different but the kind at the end of the chain of communication was the same,[viii] 44% of the participants gave a positive answer to the question as to whether the two protagonists in the story were thinking and referring to the same kind. When the causal connection of terms used by two protagonists in the vignettes led to different kinds but the description in their minds was the same[ix] 53% of the speakers responded affirmatively to the question as to whether the two protagonists were thinking and referring to the same kind. GL conducted their experiment using terms for diseases, minerals, artifacts and legal documents, and their results are consistent. On the basis of the data GL also concluded that the split among participants was not due to their being unsure about the answer or to lack of comprehension. More importantly, GL conducted further probes to determine if the divergences could be attributed to the fact that different groups of participants had and expressed different intuitions some of them more descriptivist, and some of them more in line with the chain of communication picture. GL observed that the responses to (i) and (iii) type of questions were not correlated, so they concluded that the strategies pursued by participants "did not take the form of consistent reliance on a single factor" (2012, 728), namely they were not consistent with a purely descriptivist or a purely

historical approach. This, according to GL shows that "the findings support the hypothesis that reference judgments are a function of both descriptive and causal factors" (2012, 728), that "individuals utilize both descriptive and causal information in making judgments about reference, and that mixed patterns do not reflect . . . a heterogeneous population with some 'intuitive descriptive theorists' and some 'intuitive causal theorists,' as suggested by Machery et al.'s studies" (2012, 731).

GL conclude that the account of reference determination should take into account that both causal connections and descriptive information figure in the explanation of what constitutes the domain of application of a kind term and they suggest that a hybrid theory, one that incorporates both causal and descriptive elements, may provide the right sort of account. GL mention Gareth Evans' (1985) approach as the kind of hybrid theory that would be supported by the data collected, although they also express some doubts that the details of Evans' theory really do fit the data. I discuss below Evans' theory in the context of GL's arguments, but for the moment it is worth noting that GL do not make a specific commitment to a specific theory of reference determination. Their claim is just that speakers judgments about reference, in particular the judgments about what certain general terms apply to, are swayed by both descriptive factors and causal factors, and they advance the tentative hypothesis that a theory that combines both kinds of factors may be supported by the data, without exploring in their paper the form that such a theory might take. The kind of vignettes presented by GL are very similar to MMNS's (2004) and Machery, Olivola and DeBlanc (2009) vignettes. The questions that GL ask are questions that require from the participants a

reflection on what hypothetical speakers are thinking about or referring about. They are, thus, questions that invite theoretical reflection about practice and do not collect, in my view, data about practice. So, I would direct to GL the same objections I expressed in Marti 2009 and 2012.[x]

The situation is slightly different as regards NPM (forthcoming). NPM do not construct fictional stories that test participants' intuitions as regards counterfactual use scenarios; instead, they use real cases in the history of science in which experts misdescribed a kind. For instance, in the Middle Ages the catoblepas was described as "an animal said to be like a bull but with a head so heavy that the animal had to keep its head down at all times. It was also thought that the catoblepas had scales on its back." (NPM, forthcoming).[xi] Of course, Middle Age descriptions of the catoblepas are not satisfied by any real animal (it is worth noting that the descriptions of the alleged animal also mention that looking into the animal's eyes would cause immediate death).

Nevertheless, as NPM note, researchers believe that the introduction of the term 'catoblepas' was connected to sightings of wildebeest. Some of the choices NPM present to their subjects do prompt theoretical reflection. For instance, on several occasions participants are asked to judge the truth value of '"Catoblepas" refers to wildebeest.' However, other questions are closer, in my view, to capturing the participants' dispositions to use the terms in question. For instance, subjects were asked to indicate their level of agreement with statements such as 'Catoblepast exist' or 'Catoblepas are wildebeest.'

NPM hypothesized that if participants in their experiment were primed to stress the continuity of application of a term in spite of the presence of radical misinformation, by reading, for instance, about the story of the continued use of 'triceratops' in spite of the extreme original mischaracterizations of the dinosaur, they would make choices in line with the causal-historical picture, whereas subjects exposed to a story that heavily highlighted the radically erroneous nature of the characterization of the catoblepas (for instance, the inclusion of the statement that looking into a catoblepas' eyes would cause immediate death) would be more inclined to respond in ways more sensitive to the fact that the descriptive information associated with 'catoblepas' is not satisfied by any existing kind. By and large, the results support NPM's hypothesis.

The choice of the catoblepas story is, in my view, unfortunate. The description of the catoblepas, in particular the attribution of the property of causing death with its sight, makes it almost a mythical character. And in such cases, the fact that some rather unspecified alleged connection to some real animal, the wildebeest in this case, figures in the story may not be sufficient to push speakers to accept that the word does indeed refer to those real animals. Kripke has pointed out that ". . . if the story of the unicorns were historically connected to some genuine and ordinary kind of animal . . ., and the mythical traits attributed to it gradually evolved, we would probably not say that it turned out that unicorns really existed after all." (Kripke 2013, 50, fn 18). Thus, the observed tendency NPM observe among speakers to deny that the catoblepas existed when the erroneous nature of the Middle Age experts description was highlighted, may not be driven by descriptivism, but simply by

the presumption that 'catoblepas' refers to a mythical animal. Nevertheless, although I will refrain here from discussing the details, some of the speakers' reactions reported by NMP are indeed surprising[xii] and they reveal variations among speakers' reactions, even if it is not entirely clear what the cause of the variations is.

If we concede that NPM's hypothesis is corroborated, the interesting point for our purposes is that NPM's results appear to be approximately in line with GL's. Nevertheless, NPM put forward a different conclusion as regards the semantic explanation that supports the data. Instead of endorsing a unified hybrid theory of reference, NPM suggest that kind terms are ambiguous between a descriptivist and a causal-historical reading. Depending on the conversational setting, speakers will be inclined either towards responses consistent with a descriptivist disambiguation or towards responses consistent with a causal-historical approach.

NPM argue that a hybrid theory would not account for the data presented, and their target in such argument is the kind of theory suggested in Evans 1985. So, both in GL's and in NPM's case the discussion revolves around the adequacy of a hybrid theory like Evans'. Before assessing GL's and NPM's recommendations it is worth stopping to reflect on the nature of Evans' proposal and the sense in which it can be considered to be a hybrid approach.

3.- Hybridity and Evans's approach

Right off the bat, it is hard to see what a unified theory that incorporated elements of classical descriptivism –the kind of descriptivism Kripke,

Donnellan and others were arguing against, which happens to be also the sort

of descriptivism GL, NPM, as well as MMNS focus on—, together with

elements of the causal-historical approach would consist in.

      This is not because the causal-historical approach is an alternative to a

theory of the determination of reference such as classical descriptivism. The

chain of communication picture, or causal-historical picture is not a theory of

the determination of reference. What *makes* a use of 'Aristotle' refer to

Aristotle is not the fact that the use is connected to previous uses of the word,

since that connection at most will lead to the first use of the name, not to the

referent.[xiii] The picture presented by Kripke (1970) and Donnellan is rather an

intuitive explanation of the transmission of names –of words, in general, and

the preservation of their semantic function.[xiv]

      As an aside, let me point out that it is unfortunate that the chain of

communication picture continues to be presented as a theory of determination

of reference; and experimental philosophers, although not the origin of the

confusion, are contributing to it.

      In fact, a classical descriptivist could incorporate the causal-historical

chain into their approach. All a descriptivist would need to argue is that names

are bestowed by being linked to definite descriptions which are then passed

from user to user, from link to link of a chain of communication. From this

point of view, what makes a speaker a new link in the chain of communication

would be the acquisition of the definite description associated with the name,

a definite description that determines the reference of each use of the name.

Of course, a theory of this sort would be a descriptivist theory thru and thru

and not the kind of theory contemplated by GL, NMP and Machery and colleagues.

What makes classical descriptivism and any approach to reference consistent with Kripke's approach so difficult to combine in a hybrid theory is precisely the fact that Kripke's argument is essentially a negative argument. What emerges from Kripke's considerations, and in particular from the crucial semantic arguments –the ignorance and error arguments— is that reference does not have to be established via the satisfaction of descriptive information. And this amounts to the denial of classical descriptivism.[xv]   An approach to reference that is, arguably, hybrid is the sort of causal descriptivism proposed by Frederick Kroon (1987) a view according to which reference is determined by a description such as 'the individual at the end of the chain that leads to this use of N,' or 'the person that the members of my community from which I inherit my use of N refer to,' or something similar. This could count as a hybrid view, since it claims that reference is determined by the satisfaction of a definite description a speaker associates with N, as classical descriptivism urged, while at the same time incorporating the chain of communication into the descriptive content. But this is not the kind of theory that the data collected by GL, nor by NPM, appear to underwrite, since the descriptive information used in the vignettes does not include mention of the chain of communication and it is closer to the type of descriptive information contemplated by classical descriptivists such as Bertrand Russell.

Both GL and NPM take issue with the approach endorsed in Evans 1987, characterizing it as a sort of hybrid theory. But it is not clear at all that

Evans defends a hybrid of the classical descriptivist approach tested by GL and NPM and the chain of communication picture.

It is, to begin with, doubtful that Evans would incorporate elements of classical descriptivism as reference determiners, or partial determiners. For one of the features of Evans positive view is precisely that reference is not determined by the satisfaction of descriptive information and that it is a mistake to interpret Fregean senses as descriptive and as determiners of reference by fit. According to Evans, "there is absolutely nothing in the texts to support the claim that he [Frege] held that the way of thinking of any object *must* exploit the subject's knowledge of some description uniquely true of it." (Evans 1982, 18).

Evans' view is often characterized as a hybrid theory. Thus, for instance, according to M. Reimer (2010):

> Evans provides several examples of uses of proper names that are most naturally accounted for via a hybrid theory . . . Like description accounts, [Evans' approach] accounts for cognitive significance . . . as well as reference; like causal accounts, it preserves the intuition that one cannot refer to something with which one has no causal link whatsoever.

And Evans himself argues that the approach he proposes in some sense vindicates both descriptivism and the causal-historical picture, but it is far from clear that the approach he endorses can be characterized as a hybrid, or as providing support for classical descriptivism.[xvi]

For Evans' vindication of descriptivism does not afford definite descriptions a role in fixing the reference: even in the midst of his avowals of vindication

Evans insists that "the fix is by causal origin and not by fit" (1987, 23). As GL put it:

> Evans . . . proposed that each use of a name is associated with a file of information that an individual stores about the referent of the name. While some of the information in the file can be false (by not being generated by the appropriate source), reference will be tied to whatever is the dominant causal source of the information in the file. GL, 2012: 720)

Descriptive information plays an undeniably important cognitive role in Evans's approach. But this is something that neither Kripke nor Donnellan, nor even a true causal theorist of reference such as Devitt need deny.

As a theory of reference determination, Evans' theory is fundamentally causal, for the referent of a name is the causal origin of the descriptive information that speakers have in their minds. The information might be erroneous. The information that Aristotle was born in Stagira or that he tutored Alexander the Great may well be mistaken, even if Aristotle himself is the source of that information. So, as Reimer (2010) puts it: "Evans' theory avoids the problem of ignorance and error." But this is precisely because the description plays no role in determining the reference. The descriptive information associated with a name can be erroneous, and this is immaterial for Evans precisely because, *contra* classical descriptivism, the associated attributes do not determine reference. If there is massive error in the descriptive information associated with a name, we may of course conclude that the name does not refer or that a kind term does not apply to anything. But this conclusion would not, according to Evans, be based on the fact that

no object satisfied the descriptive information; it would simply be because the massive error would be taken as an indication that nothing could plausibly be the source of such radically wrong information. So, for example, the claim that experts believed catoblepas to cause instant death to those who looked into their eyes makes it very doubtful that any real animal could be the source of such a wild attribution.

Evans' theory is, in a sense, more causal than Kripke's and Donnellan's picture for it gives more of an explanation of the causal elements at work, something that Kripke and Donnellan do not do. In fact, there is little in Kripke's and Donnellan's remarks about the chain of communication that justify calling the theory 'causal'. Kripke himself stresses the social character of the picture (the fact that what we refer to depends not on what is in our minds but on our connections to other members of our speakers' community), over and above the causal factors, and he as often uses 'causal chain' (1980, 93 and 96) as "causal' chain of communication' (1980, 59, note 22, with quotes around 'causal'), 'historical chain' (1980, 8, note 9), and 'chain of communication' (pp. 91-3).

Some of the cases that Evans discusses are meant to highlight the differences between his approach and a naive chain of communication picture inspired by Kripke and Donnellan's remarks. Among those the 'Madagascar' and the 'Ibn Kahn' case stand out. In these two cases, the naive picture presented by Kripke and Donnellan appears to give the wrong results as to what uses of those names refer to: a portion of the mainland and not the island, in the case of 'Madagascar', and the scribe who out his name in the document, not the mathematician that did the proof, in the case of 'Ibn Kahn'.

Part of the reason to suppose that the picture presented by Kripke and Donnellan gives the wrong results, is precisely the fact that the picture they present is so under described. Kripke and Donnellan describe a very simplified and idealized picture of how names are bestowed and how their use is transmitted from speaker to speaker. The dubbing ceremony that is supposed to account for a name bestowal is acknowledged to be just a portrayal of a typical way in which names get tied to their bearers, but it is clearly not essential to the picture. There are other ways in which names are introduced: by description, for instance, or by a direct use without the mediation of a meta-linguistic ceremony. The picture is flexible enough to allow for the bestowal of a name in the kinds of circumstances that are contemplated in the 'Madagascar' and the 'Ibn Kahn' case. Speakers may, simply by mistake, start using a name with the intention to speak about a certain object and the practice may catch on. As Michael Devitt has suggested, a mistake can function as the initiation of a chain of communication, by grounding reference in a new object, even when the intention to dub is absent (Devitt 1981. See also Devitt and Sterelny 1999). According to Evans in such cases uses of the name refer to the causal origin of an attributive file, and one may disagree with his explanation, pointing rather, for instance, to the existence of a systematic practice that connects use to use even in the absence of a common file. But no matter what, and even if we accept Evans' account, the point is that the role of the file is not to determine the reference by searching and finding an object that satisfies the, or most of the, attributes associated, as classical descriptivism would have it.

Thus, it is not surprising that Evans' approach would not be a good match for the kind of data GL or NPM collect, as GL themselves acknowledge (GL 2012, 733) for the data appear to point towards the reliance, on occasion, on definite descriptions as reference determiners something not contemplated by Evans.

4.- Ambiguity and the causal-historical approach

As far as I can tell both sets of data –supposing them to portray actual speakers' use, reveal that individual speakers are in some occasions inclined to produce judgments that are in line with classical descriptivism whereas in some other occasions their judgments give prominence to the continuity of reference and reliance on previous uses that is highlighted by the chain of communication picture. Rather than suggesting a unified theory of reference that combines in each use some descriptivist and some causal elements, the explanation of speakers' responses suggests, in my view, the kind of approach NPM endorse, a form of ambiguity according to which in some cases the descriptive information associated with a term is dominant in determining what the term applies to, whereas in some other occasions the chain that leads back to the introduction of the term takes precedence.

The kind of ambiguity postulated by NPM is different, as they themselves point out, from typical cases of lexical ambiguity. But, if NPM are right, the data point towards different uses of terms on different occasions. In spite of the differences with other cases of lexical ambiguity, it would not be entirely surprising if, on occasion, the file of information that a speaker

associates to a term was taken to be that which determines what she is speaking about, for after all the file does play a cognitive role, and it is a file of things we think we know about the things we refer to. It is not unnatural to think that the information we possess applies to whatever it is that we are talking about. Whether this should be treated as a semantic or as a pragmatic phenomenon is a different issue, and it is not one that NPM have addressed, but it is something that needs to be elucidated, perhaps by designing tests that can provide evidence on this regard.

There is, certainly, a long-standing tradition in semantics that recommends resisting the postulation of ambiguities, springing from the presumption that a unified theory is in general preferable to the endorsement of different explanations. Nevertheless, the resistance is to be taken as a recommendation and in fact, the possibility that in certain conversational settings different factors, causal or descriptive, may take prominence is not far-fetched.[xvii] For instance, in cases where the descriptive information associated with a term is wildly implausible or off the mark the judgment of non-existence is entirely natural, and in fact there are plenty of historical examples: we say that there is no phlogiston, not that phlogiston turned out to be O_2 and that we were wrong in, for instance, attributing to it negative mass.[xviii]

An important question is what those variations show as regards semantic theory. NPM, for instance, argue that their results "put pressure on a univocal theory of reference along the lines of the causal-historical theory" (NPM, forthcoming). And the impetus for the debate around experimental semantics comes from MMNS (2004) who viewed their results as a threat to a

universal of a theory of reference, specifically the kind of anti-descriptivist theory endorsed by Kripke.

Experimental semanticists argue against the adequacy of Kripke's approach to reference on the basis of arguments about the apparent descriptivist semantics of some terms. The assumption, I believe is that an argument to the effect that a set of terms have a descriptivist semantics, or that some uses of terms follow a descriptivist mode of reference determination, constitutes an objection to Kripke's stance. It is worth reflecting on the structure of the dialectic, for it seems to me that there are deep confusions underlying the debate.

First of all, it is important to distinguish between intuitions about how reference is determined from data that is meant to collect actual usage. I have already argued (Marti 2009 and 2012) that the former are not relevant to semantic theorizing. So, I am focusing here on results that can plausibly be interpreted as reflecting usage, such as NPM's, and the issue to elucidate is whether the results and observations constitute counterexamples to the chain of communication picture. In order to see that they do not, it is important to remember the crux of the disagreement between classical descriptivism and the chain of communication picture. According to classical descriptivism reference is mediated, always mediated. From this perspective it is impossible to establish reference unless it is via descriptive material. Kripke's arguments open the door to a view according to which reference is established directly, by convention and without the mediation of definite descriptions, a view according to which the conditions of correct application of a term, be a singular or a general term, are to be found outside of the speakers' minds.

The kind of semantics that emerges from Kripke and Donnellan, and from Putnam's considerations about natural kind terms (Putnam 1973) contemplates direct reference. Their cases and examples are meant to show that reference can be direct, that it does not have to be established via a definite description, and this is accompanied of arguments directed to showing that proper names, in general, and natural kind terms, in general, do refer without the mediation of definite descriptions. To argue that the use of some terms is arguably guided by an associated definite description, is not to disprove the Kripke-Donnellan-Putnam approach, whereas showing that some terms refer or apply without the mediation of semantic mechanisms such as descriptions, does disproof descriptivism. It is not, and it should not be, part of the Kripke-Donnellan-Putnam approach to show that no term can have its reference or domain of application determined by associated descriptive material.

Nothing, as Kripke himself noted, prevents speakers from introducing a term and deciding to use it as a real abbreviation of a definite description. Of course, whenever a non-structured, tag-like term is introduced in a language, the potential for the associated reference-fixing description to stop operating is there. Speakers often do not pass along the chain of communication the descriptions that are supposed to determine reference, erroneous information is easily added to the original reference fixing attributes and uniquely identifying properties may be lost, giving rise to ignorance and error and leaving speakers with a file whose role is undoubtedly cognitive, but non-operational at the level of fixing reference, even though the chain that connects use to use and leads back to the introduction of the term is still

present and guarantees the appropriate connections that keep usage uniform. Name-like expressions have a tendency to be used as standard proper names.

None of this is a threat to the chain of communication, or causal-historical picture.

Of course, according to NPM, their results point in the direction of something stronger: it is not just that some terms are semantically connected with definite descriptions. Their point is that individual speakers do use terms descriptively and non-descriptively depending on the conversation setting. But even that is no threat to the chain of communication view. In fact, it is something that has been pointed out by one of the staunchest defenders of a causal theory of reference. Michael Devitt (1981,157-60) has often pointed out that proper names are used descriptively in some contexts. Literary historians, for instance, use 'Shakespeare', bypassing anti-Stratfordians concerns, to talk about whoever wrote the famous plays, when their focus is the discussion of the plays and their contents

> the names of authors . . . can have a double life. In claims about where "Shakespeare" lived, was educated, and so on, the name seems to function as a designational name. In critical assessments of "the works of Shakespeare", however, it often seems to function as a descriptive name, so that it would not matter to the truth of these assessments if the work was actually written by Bacon.
>
> (Devitt forthcoming 2015)

 Again, that names, or other expressions, can be used in a way that gives prominence to associated descriptive information in the determination of reference is no threat to the anti-descriptivist arguments by Kripke, Donnellan and others.

It is tempting here to engage in an argument as regards how extended the Kripkean uses, vs. the descriptivist uses are, as if the statistical distribution of different uses was relevant in order to establish one theory over another. In my view, arguing in these terms amounts to misunderstanding the dialectic of the Kripke-Donnellan stance against descriptivism. Kripke and Donnellan are arguing against a conception of reference according to which reference *has to be* mediated; the reference of an expression has to be established via an associated mechanism, a description that determines the reference on each occasion of use. The gist of Donnellan's and Kripke's arguments is that this is not so, and that speakers refer in the absence of such mechanism. Theirs is issue of contention is a about the possibility of reference: the descriptivist claims it is not possible to refer without the mediation of a description; Donnellan and Kripke's arguments and the examples they use to illustrate them are meant to show that it is, and it is also worth noticing, that the examples used by anti-descriptivists to make their case, cover a pretty substantial number of expressions and kinds of expressions.

If NPM are right, individual speakers, in certain conversational contexts, use terms to refer to whatever fits an associated descriptive file. This should not provoke changes in the chain of communication approach, although certainly would be interesting to continue to investigate what kinds of

conversational contexts prompt one or another usage and whether the

phenomenon is really a semantic phenomenon or a purely pragmatic one.

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[ii] Internalist because who the referent of a use of a name is, is determined by a description that the speaker has in mind and associates with the name.

[iii] Externalist because what the use of the name refers to depends on the objective position of the speaker in a network of users.

[iv] See Kripke 1980, 83-84.

[v] See Devitt (2011) for similar concerns.

[vi] Pinillos' paper in this volume discusses and develops the view further.

[vii] Two clarifications: (i) the concerns I expressed about MMNS's work apply also to some of the probes conducted by GL and NMP. More on this below, although for the purposes of the discussion here I will be setting these issues aside and I will proceed on the assumption that the data collected provides evidence about the use of terms by speakers. (ii) a recent discussion on hybrid theories of concepts is related to the issues discussed here (see Machery and Seppälä 2009/2010). Nevertheless, in this essay I concentrate exclusively on semantic issues.

[viii] For instance, when one of the speakers (an expert) associates with 'tyleritis' the descriptive information 'disease that affects muscles' whereas the other associates 'disease that affects the joints' to a disease that is at the end of a chain that ends with the introduction of the term for the disease that affects muscles.

[ix] For instance, in a story about Alex and Bob, speakers in two different communities that use the word 'tyleritis', a term that in each community is historically connected with the dubbing of two different diseases

[x] GL acknowledge this (GL 2012, 735), and for the purposes of the discussion in the present paper it should play no role in the argument.

[xi] I am told that in Greek 'cato' means 'down' and 'blepas' means 'looking'.

[xii] For instance participants do not react consistently to the pair of sentences 'Catoblepas are wildebeest' and 'Wildebeest are catoblepas.' See NPM (ms) for discussion.

[xiii] What positive story of how reference is established is compatible or suggested by Kripke and Donnellan's arguments is an important issue but not one we need to discuss in this paper. See Marti (forthcoming 2015) for discussion.

[xiv] It was Michael Devitt (1974 and 1981) who developed Kripke's and Donnellan's picture and turned it into a *causal theory* of the determination of reference. But there are many people who accept the picture without

accepting the details of Devitt's approach (as far as I can tell, Kripke and Donnellan among them).
[xv] In any case, what makes the two approaches difficult to combine, is not the fact that they are two incompatible theories of the determination of reference
[xvi] Evans himself does not characterize his approach as a hybrid theory.
[xvii] And, as we will see, it has in fact been contemplated even by staunch defenders of the chain of communication picture
[xviii] It is hard to determine though whether this is because nothing fits the associated attributes or rather, as Evans would have it, we conclude that oxygen could not be the origin of such massively erroneous information.

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