Against rigidity

MARY LOU GRIMBERG

1 Introduction

> Proper names are not connotative: they denote the individuals who are
> called by them; but they do not indicate or imply any attributes as
> belonging to these individuals. When we name a child by the name
> Paul, or a dog by the name Caesar, these names are simply marks used
> to enable those individuals to be made the subject of discourse (J.S.
> Mill, 1843, 1896, *A System of Logic*, p 20).

Saul Kripke (1980), in his monograph *Naming and Necessity* resurrects this Millian
view, according to which proper names are no more than convenient, but meaningless,
labels. It typifies an approach which has been seen to give rise to numerous,
apparently intractable, difficulties, and has been challenged by both Frege (1892,
1980) and Russell (1905, 1991 and 1918, 1985).

Now, after a century of argument and theorising, it might be said that in some
respects little has changed. The revived Millian view of proper names still gives rise
to the same range of problems.

My ultimate objective is to provide an alternative account that, on the one hand, will
go some way towards resolving the traditional problems and, on the other hand, will
avoid those difficulties which Kripke believes attach to both the Fregean and the
Russellian alternatives. It is not my purpose, however, to embark on a review of
Fregean or Russellian philosophy, nor even to conduct the following discussion at a
predominantly philosophical level. Rather, wherever possible, I wish to reconsider,
from a linguistic viewpoint, certain problems which have surfaced in the
philosophical literature.

Some discussion of the philosophical background is, of course, necessary.
However, this will be kept to a minimum. Furthermore, it is arguments concerning
the related theses of rigid designation and direct reference, rather than the earlier
philosophical theorising, that constitute the true starting point and focus of this
investigation. However, it is important to note that 'rigid designation' and 'direct
reference' are not notational variants, and direct reference will not, in fact, be
discussed until a later paper (in preparation).

It is undoubtedly the case that Kripke's characterisation of proper names as both
Millian and rigidly designating has considerable intuitive appeal. Nonetheless, there

are four quite distinct reasons why I believe this characterisation should be reassessed. One of these reasons is pitched at a largely, although not entirely, atheoretic and intuitive level. These initial intuitions are given further support, at a linguistic level, by morphological evidence that is difficult to account for if the Millian/Kripkean picture is accepted. Finally, at a philosophical level, there is an apparently unresolved incoherence. These four motivations for instituting this inquiry may be summarised as follows:

I.   The thesis of rigid designation, as defined by Kripke, effectively restores the Millian view of proper names and also resurrects the problems associated with such a view.

II.   Kripke's stipulations concerning the identity of individuals, on which the thesis of rigidity depends, are at variance with certain intuitions expressed by other researchers (and experienced by myself) regarding how proper names are actually used.

III. Linguistic facts concerning the productive morphology of proper names are not readily compatible with the thesis that proper names have no descriptive content.

IV.   There is considerable confusion in the philosophical literature regarding the definition of rigid designation.

In discussing these four issues I hope to show why I feel that a renewed inquiry into rigidity is warranted. However, in themselves, points I to IV are not intended to serve as arguments demonstrating the falsity of the thesis. More precisely, such a demonstration is not my primary aim in this article, although - perhaps inevitably - there is some overlap in intentions.

2 The traditional problems

2.0 These fall into three major categories, each category being associated with a specific 'puzzle'.

2.1 Frege's puzzle

This concerns identity statements. If the Millian (or some essentially similar) view of proper names is correct, why does (1), below, appear to be informative, whereas (2) is clearly tautologous?

(1)   Cicero is Tully.

(2)   Cicero is Cicero.

The problem is broached by Frege:

> ...if we were to regard equality as a relation between that which the names '*a*' and '*b*' designate, it would seen that $a = b$ could not differ from $a = a$, provided $a = b$ is true) (Frege, 1980, p 56).

Frege's well-known solution is to postulate the bifurcation of meaning into two distinct components: Sinn and *Bedeutung* with *Sinn* being invariably rendered into English as 'sense' and *Bedeutung* being variously translated as 'nominatum', 'denotation', 'reference' and 'meaning'.

  This solution is more complex than it at first seems; for although Frege writes as though sense were a unitary concept, in fact he requires it to fulfil three quite distinct functions. These are identified by Tyler Burge (1977, p 356) and summarised by Nathan Salmon[1]:

Sense$_1$. The purely conceptual representation of an object which a fully competent speaker associates in a particular way with his or her use of the term. Sense$_1$ is a psychological or conceptual notion...

Sense$_2$. The mechanism by which the reference of the term (with respect to a possible world and a time) is secured and semantically determined. Sense$_2$ is a semantical notion.

Sense$_3$. The information value of the term; the contribution made by the term to the information content of sentences containing the term. Sense$_3$ is a cognitive or epistemic notion ... (Salmon, 1982, p 12).

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[1]See also Linsky (1977).

David Kaplan (1989a and 1989b) sees this conflation of functions as problematic, and in his analysis of indexically used expressions, sense$_2$ is replaced by *character* and sense$_3$ approximates to *content*, although Kaplan comments (1989b, p 568) that his notion of content 'is closer to Russell's *signification* than to Frege's *Sinn*'.

In this article I shall not be committed to this Fregean analysis, and the term 'sense' will be largely avoided, while 'meaning' will be used purely intuitively and pre-theoretically. It will be intended to indicate, imprecisely, whatever it is we understand - when we do understand - a word or an expression. In a forthcoming paper, I will suggest that meaning may be more usefully characterised (not defined) in terms of on-line conceptual representations. However, the Fregean view that meaning (whatever it may be) is compositional is embraced.

2.2 Russell's puzzle

This concerns 'empty' reference in general, and by extension vacuous names. Russell discusses the problem with respect to examples, the best-known of which is reproduced as (3) below:

(3)   The present King of France is bald.

The difficulty is this: How can an utterance of (3) be evaluated for truth or falsity when there is no King of France, that is to say when the definite description has no reference? The puzzle may be extended to include proper names; in the extreme case it takes the form of a negative existential:

(4)   Father Christmas does not exist.

If the Millian view is correct, and the meaning of a proper name *is* exhausted by its reference, how can we even understand (4) if it is true?

The puzzle is resolved by Russell in two stages:

Stage 1: The elimination of definite descriptions from propositions. This is done by assuming a quantificational analysis for descriptions. Thus (3) may be formally expressed as (5):

(5) $\exists x(Fx \supset \forall y(Fy \supset x = y) \& bald(x))$
       [F = 'The King of France']
       This may be read as (6):

(6)   There exists an entity x, such that if x is the King of France, then for every entity y, if y is the king of France then x is identical to y, and x is bald.

For the sake of simplicity the modification 'present' has been omitted from (5) and (6). However, assuming this quantificational analysis, (3) may now - with or without this modification - be straightforwardly evaluated for truth or falsity.

Stage 2: The assimilation of proper names to definite descriptions. This is done by postulating that every proper name is a disguised description:

> The names we commonly use, like 'Socrates', are really abbreviations for descriptions. ... When we use the word 'Socrates' we are really using a description. Our thought may be rendered by some such phrase as, 'The Master of Plato', or 'The philosopher who drank the hemlock', or 'the person whom logicians assert to be mortal', but we certainly do not use the name as a name in the proper sense of the word (Russell, 1985, p 62).

This two-stage solution is also well-known and will not be discussed further here.

2.3 Kripke's puzzle

This is the puzzle about belief (see Kripke, 1979, 1994). If the Millian view is correct, and the meaning of a proper name may be identified with its referent, rational language users would seem to be capable of holding, simultaneously, contradictory beliefs. For example, Tom may believe ('sincerely assent to') the proposition expressed in (7), while simultaneously and equally sincerely assenting to the contradictory proposition expressed by an utterance of (8).

(7)   Tully denounced Catiline.

(8)   Cicero did not denounce Catiline.
        (adapted from Kripke, 1994, p 355).

This puzzle is also noted by Frege, who distinguished between *oratio recta* and *oratio obliqua*. According to Frege, in *oratio recta* a name '*expresses* its senses' but '*means* or *designates* its meaning', p 61), whereas in *oratio obliqua* (reported speech or

intensional contexts), a name refers to its *sense* not to its usual referent.[2] However, as Kripke effectively denies that a proper name has any sense, this solution is not available to him.

This should not be taken to imply that no solutions have been suggested; indeed there is an extensive literature on the subject. Perhaps the most promising approach is that identified by Steven Schiffer as the '*hidden-indexical theory*':

> The theory begins by holding that the relation expressed by "believes" in sentences of the form "A believes that S" is a three-place relation, B(x,p,m), holding among a believer x, a mode-of-presentation-less proposition p, and a mode of presentation m under which x believes that p. Thus it is possible for x to believe p under one mode of presentation m while believing not-p under a second mode of presentation m', and while suspending judgement altogether under a third mode of presentation m'' (Schiffer, 1995, p 108).[3]

Schiffer detects serious problems (which will not be discussed here) inherent in this 'naive' version of the theory. However, modification of the theory to overcome these initial problems leads to further difficulties, so that ultimately he rejects it. One of his reasons for doing this is that he believes that the related hidden-indexical theory of descriptions is not compatible ('cotenable') with the theory that indexicals are directly referential (i.e., rigidly designating). However, as I shall argue (elsewhere, also forthcoming) that indexically used expressions are not directly referential, it would remain open to me to adopt some version of the hidden-indexical theory of belief reports.

More to the point, it is not clear to me how far, or whether, Kripke would be prepared to accept the applicability of modes of presentation to proper names. I suspect not very far, as they can be equated with Frege's sense₁. In any event, if Schiffer is correct, the hidden-indexical theory would be of small help to Kripke (and none to Kaplan) as it entails the demolition of a supporting pillar (the directly referential function of indexicals) of the thesis of rigidity. Furthermore, by reinstating the Millian view of proper names (or something very like it), Kripke also resurrects Frege's and Russell's problems concerning identity statements and proper names.

Although there are significant differences between the Fregean and Russellian solutions, both of which purport to resolve Kripke's puzzle also, a discussion of those differences will not be undertaken here. Kripke also ignores the differences and

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[2]For a more detailed discussion, see Dummett (1981), chapter 6, and Luntley (1984), pp 84-85.

[3]See also Crimmins and Perry (1989), Richard (1990), and Fodor (1990).

focuses on the similarities; it is the fundamental thesis that proper names have descriptive content and are, indeed, disguised descriptions - a thesis that he attributes impartially to both Frege and Russell - that Kripke challenges.

3 The identity of individuals

The problems associated with this issue cannot be considered in a vacuum; some discussion of Kripke's alternative to what he calls the 'Frege-Russell' model of proper names must first be undertaken. As has already been indicated, Kripke postulates that proper names lack all descriptive content. It is therefore incumbent on him to explain how the reference of such terms is determined. His suggestion is that proper names are linked to their referents by a causal chain. This chain stretches, unbroken, from the original naming event through an indefinite number of utterances, to its most recent use:

> When a name is 'passed from link to link', the receiver must, I think, intend when he learns it to use it with the same reference as the man from whom he heard it (Kripke, 1980, p 96).

Kripke seems to imply that this is *all* the hearer standardly intends, but this is implausible. Furthermore, it is not borne out by the empirical data. When a language user hears a proper name for the first time, in the absence of the actual referent, he normally requires that it be accompanied by some identifying (although not necessarily uniquely so) description. Mira Ariel comments on this requirement:

> Languages that do not impose a grammatical distinction between familiar and unfamiliar name-bearers dictate instead that unfamiliar proper names should be accompanied by some description. Hence [3a] below is a more natural phrasing of the facts than [3b] when the referent of the name *Joan Smith* is not familiar ...
> [3]   a   Joan Smith, an IBM engineer from Tel-Aviv, was recently accused by the company of theft.
>   b   Joan Smith was recently accused by IBM of theft.
> (Ariel, 1990, p 40)

Ariel (p 39) also notes that a survey of newspaper articles revealed that *all* unfamiliar names were accompanied by some, at least partially identifying, description. That is to say, a speaker (or writer) who introduces what might be an unfamiliar name into

the discourse does not assume that her hearer will be content to determine to use *that* name with the same reference - whatever that may be - that the speaker herself intended. The speaker recognises that the hearer is going to require more than this, and to the best of her ability she will usually supply what is required. Thus:

> Harry Greenway, Tory MP for Ealing North, and a senior member of the committee, wrote to its labour chairman, Greville Janner... (*The Times*, 15.5.95)

Or:

> The Sinn Fein leader Gerry Adams said yesterday that a meeting with Patrick Mayhew, the Northern Ireland Secretary, was not a precondition for his party continuing talks with British ministers (*The Times*, 15.5.95).

Interestingly, even when there is very little that can usefully be said to identify the bearer of the name, something is almost invariably offered. This is typically demonstrated in local journalism:

> Joining in the fun last Thursday was Jean Faulkner who attended the nursery ... in 1940 ... She is pictured with nursery nurse Dorothy Folds and Sam Dennis *aged three and a half* [emphasis added] ... (*Review*, St Albans and Harpenden, 11.5.95).

There is a further consideration. Let us suppose that it happens that Dorothy Folds, say, is not a nursery nurse, and that this is pointed out to the reader. Will he then be happy in his turn to repeat the news item, saying of Jean Faulkner and Sam Dennis - to both of whom properties are attributed - that they are pictured with Dorothy Folds, to whom no properties are attributed? That is to say, will such a reader-speaker be content simply to *intend* to refer as the original user did, and to expect *his* hearers to do the same?

   It seems unlikely. What a speaker in such a situation will - again almost invariably - do, is insert into the report the only item of information about Dorothy Folds that he has, and this is that she is *called* 'Dorothy Folds'. Language users employ this device constantly, as is illustrated in (9) and (10) below:

(9)   Someone called 'Jack' phoned while you were out.

(10)   He keeps talking about some woman called 'Jasmine'.

I am not suggesting that being a nursery nurse is any part of the meaning of 'Dorothy Folds', or that 'Harry Greenway' *means* 'Tory MP for Ealing North'. What I do suggest, is that names are not simply passed from speaker to speaker like a relay baton. Furthermore, what the evidence seems to suggest, is that if a speaker is to feel that she is *using* a name rather than merely *mentioning* it, then she needs to have access to some 'mode of presentation' with respect to that name, that is entirely independent of any supposed causal chain.

In view of this evidence, how can we be sure - how can Kripke be sure - of just what a speaker of an unfamiliar name *intends*?

However, even if Kripke is correct concerning intentions, if this were all there were to his picture it might be tempting to view it as little more than an elaborate restating of Mill's views. In fact, Kripke's aims - and his achievements - are considerably more far reaching than this. Although the arguments in *Naming and Necessity* are informally presented, the underlying philosophy has its roots in mathematical logic. His model of the referential functioning of proper names is part of a much larger edifice. As Hilary Putnam (1983, p 56) explains, Kripke assumes a 'set of objects called *possible worlds*' - an idea originating with Leibniz[4] - in order to combat the difficulties associated with attempts to combine quantifiers and modal operators.[5]

The suggestion is that necessity and possibility may be expressed in terms of this set of worlds; possibility involving at least one such world, and necessity entailing every possible world. Each world is itself a 'model for the non-modal part of language' and 'determines a universe of discourse that the quantifiers range over' (Putnam, p 56). In this way, the problems attaching to any attempt to combine modality and quantification are not so much resolved as avoided.

From positing a set of possible worlds as a formal mechanism for expressing modality, it is but a short step to the postulation of the trans-world identity of individuals; that is to say of the constancy of individual essences across worlds. This may seem to be a fundamentally metaphysical notion. However, the motivation for the claim is also cognitive and linguistic. That is to say, Kripke's intention is to describe how the language user conceives of the entities she refers to when using a proper name. The philosophy of mind and the philosophy of language are overlapping disciplines.

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[4]See Parkinson (1973), pp xii, 54 and 187.

[5]For a discussion of these difficulties see Hookway (1988), chapter 7, and Quine (1980), chapter 8.

Kripke's suggestion is that certain expressions, in particular proper names, refer to - designate - entities that have the property of retaining a constant identity across worlds. Such terms are in direct contrast to definite descriptions; for a definite description may denote - may be satisfied by - a different individual (or no individual at all), in each of the worlds in the model.

Thus, according to Kripke, the name 'Aristotle', say, always refers to the same entity - ARISTOTLE - in all possible worlds. (This is a controversial over-simplification, as will become apparent in section 5, below.) The description 'the last great philosopher of antiquity', on the other hand, although it may be satisfied by Aristotle in this world, might be true of some individual other than Aristotle in some other world.

In order to avoid confusion (and forestall objections) I should point out that, for the sake of simplicity, Kripke follows the convention of the 'classical description theorists' in assuming that names have unique referents. He comments:

> As a speaker of my idiolect, I call only one object 'Aristotle', though I
> am aware that other people, including the man I call 'Onassis' ... had the
> same given name (1980, p 8).

Where appropriate, I shall adopt this convention also. I shall further assume that when two, or more, individuals share a phonetically and/or orthographically identical name, this is in fact an instance of homonymous ambiguity.

It is the constant association of a (possibly ambiguous) linguistic symbol with the same individual across worlds that Kripke calls 'rigid designation'. As has already been noted, this is an over-simplification, for it is not clear whether the linguistic symbol is associated with any individual at all in those worlds in which the customary referent does not exist. Robert Adams suggests that confusion on this point may be avoided if we are careful to observe the distinction between designating *in* and designating *at* a possible world:

> A singular proposition about an individual *x* cannot be true *in* a world
> in which *x* would not exist, because the proposition also would not exist
> there. But we can say that it is true *at* such a world if it correctly
> characterizes that world *from our vantage point* [emphasis added] in the
> actual world. ... A name or other expression *n rigidly* designates an
> object *x* at (though not *in*) every possible world (Adams, 1989, p 33).[6]

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[6]See also Adams (1981).

Putnam makes a related point:

> When we say 'Aristotle might have been born in Athens', we do not just mean that someone *named* 'Aristotle' might have been born in Athens. ... What we mean is that *the same individual* ... named Aristotle [sic; in this world] ... might have been born in Athens ... [and he] might have been named Diogenes [sic] ...(Putnam, 1983, pp 56-57).

It should be apparent from these two extracts that the distinction between designating *in* and designating *at* a world relates to two quite separate issues. The first, discussed by Adams, is represented by the claim that even *at* worlds *in* which an individual does not exist, the name by which that individual is known in this world designates that same individual. The second, introduced by Putnam, is represented by the claim that a name such as 'Aristotle' designates ARISTOTLE even *at* those worlds *in* which Aristotle is called by some other name - 'Diogenes', perhaps - and does *not* designate some individual other than Aristotle who *happens* to be called 'Aristotle' in that world.

   Among rigid designation theorists, the first claim - that made by Adams - is controversial; the second claim - that made by Putnam - is not. Adams' claim will be discussed in more detail in section 5.

   If the theory of rigidity is to be understood, it should also be noted that the causal chain theory mentioned briefly above and the theory of rigidity, although intimately related, are at least partially dissociable. An expression may be rigidly designating without being causally linked to its referent. Indeed, this might almost be taken as a definition of *de facto* rigidity. This relates to definite descriptions, attributively used in Donnellan's sense,[7] which necessarily designate the same entity in all possible worlds, e.g., 'the positive square root of 16'. However, that this expression, in all circumstances of evaluation (i.e., necessarily) designates the same entity - the number 4 - is a fact about mathematics, not about language or the human mind. *De jure* rigidity, on the other hand, is as much about language and the mind as it is about the world. It is Kripke's expressed claim that a *de jure* rigid expression picks out the same entity in all possible worlds by semantic stipulation, i.e., by linguistic fiat. (See Kripke, 1980, p 21, fn 21).

   With this much of the theoretical framework now in place, it is possible to return to the second ground for objection - the arguments concerning the identity of individuals.

   Any story concerning rigid designation depends, by definition, on the assumption of individual essence; it is these hypothesised essences that are claimed to persist

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[7]Donnellan (1966).

across worlds. They account for the otherwise puzzling fact (if it is a fact) that there is some possible world, W_2 say, in which Aristotle is not a philosopher, is not born at Stegira, never even meets Alexander the Great, and indeed never does any of the deeds by which he is known to us, and at which it still makes sense to say that he is nonetheless Aristotle.

For Kripke, individual identity is essentially linked to origins, more specifically as Putnam says to 'causal continuity' and 'composition' (p 64). Clearly, such a claim may be challenged; and it has been. A.J. Ayer (in an unpublished lecture cited by Putnam) suggests that 'there is nothing wrong with such a modal assertion as "Aristotle might have been Chinese".' (Putnam, op cit, p 65). However, such an assertion is not compatible with Kripke's account; for given Aristotle's phylogenetic origins (causal continuity) and genetic make-up (composition), he could *not* have been Chinese.

Putnam suggests that Ayer's point might have been a duallist one (it probably wasn't), and that Aristotle's identity might be determined according to spiritual rather than physical criteria. Kripke's notion of individual essence is not the only possible one. We might, therefore, be tempted to ask why it should be supposed that language maps on to this or that theory of metaphysics.

This would be sadly to miss the point. If all that were required for a refutation of the thesis of rigidity was a demonstration that Kripke's metaphysics are not universal, then the battle would be over. Demonstrably, language does not, by some semantic stipulation, map on to Kripke's metaphysics. It does not do so for Ayer - and a single dissenter is sufficient to demonstrate the lack of universality. However, the significant question concerns not why it should be supposed that Kripke's metaphysics are to be universally favoured by language, but rather why it should be supposed that language maps onto any metaphysical theory - however ill-formed - at all.

Peter Carruthers makes a similar point on a related topic:

> I doubt very much whether we *do* use the phrase 'human conscious thinking' with the intention of designating a natural kind ... folk-psychology is not *intended as* a scientific theory, even though it may actually be one. ... All the same, there is nothing to stop someone turning the phrase 'human conscious thinking' into a natural kind term by *fiat*, simply by deciding to govern their own use of the phrase by an intention to designate the real internal structure of the activity, whatever it might turn out to be. And so, for them, if it is naturally necessary that human conscious thinking involves natural language then it will also be metaphysically necessary (Carruthers, 1994, p 6).

The point that Carruthers is making is that, in general, when we use the expression 'human conscious thinking', in Donnellans's terminology, we use it attributively rather than referentially. This being so, it is a definite description that may be satisfied by different entities (internal structures) in different circumstances of evaluation (possible worlds). Carruthers suggests that, in general, speakers are totally indifferent to - even unaware of - the possibility that, on the one hand human conscious thinking might have been other that it actually is, or on the other that it is open to us to determine to refer to human conscious thinking *as it actually is* and that *that* could not have been different.

Interestingly, Carruthers does not entertain similar doubts about the way we use proper names:

> Metaphysical necessities, in general, result from true identities expressed by terms which are rigid designators - that is, which designate the same items in all possible worlds in which they exist. Thus 'Ruth Rendell is Barbara Vine' is metaphysically necessary because we use the names 'Ruth Rendell' and 'Barbara Vine' with the intention of referring to the person who is actually the referent in all hypothetical and counter-factual circumstances (p 5).

How can he be so sure? Why should the ordinary language user become a self-conscious metaphysician when using proper names, but remain hopelessly unscientific when using terms such as 'human conscious thinking'?

Kripke's point - and probably that of Carruthers also - is that unless she is prepared to take a metaphysical stance the ordinary language user just does not resort to proper names. The claim is that the metaphysical stance is built into the semantics of the names themselves. However, it should not really matter what the details of this stance are. Thus Ayer's example does not constitute a counter-example, or certainly not a very frightening one; it may be regarded as no more than a challenge over detail.

For Kripke, individual identity is defined by physical origins and composition, while for someone voicing Ayer's objection, individual identity might be essentially spiritual. In familiar linguistic jargon, the underlying principle remains intact; it is only the parameters that vary. In this case, those parameters are set, not by language-specific grammars, but by culture-specific beliefs.

However, I should like to ask again, this time with respect to Kripke: How can he be so sure? Do language users really privately determine to designate individual essences (however such essences may be represented) whenever they use proper

names? I doubt it. If I search my own intuitions, I find that (11), say, has implications for me quite distinct from those which Kripke (and Carruthers) prescribe.

(11)   Eric Blair is George Orwell.

With respect to (9), Kripke would make the following claims:

(i)   'Eric Blair' and 'George Orwell' are both rigid designators.
(ii)   (11) is an identity statement.
(iii)   (11) expresses a necessary identity; i.e., if (11) is true it is necessarily true.

I wish to challenge all three claims.

With respect to (i), the arguments I shall advance in this introductory article are largely intuitive and may, on that account, be considered flimsy. Nonetheless, I believe they merit consideration, for it is the fact that I *have* these intuitions, combined with the substantial problems associated with the Millian view of proper names, that has persuaded me that the thesis of rigidity should be questioned.

With respect to (ii), the question of whether (11) is or is not an identity statement will be discussed at length in a forthcoming paper. With respect to (iii), although Kripke is clearly correct to assert that identity is a necessary relation, if it should turn out that (11) is not an identity statement at all, then it will not express a necessary identity either. Furthermore, if (11) is not an identity statement, then there are no *a priori* reasons for asserting that if it is true it is necessarily true. Points (ii) and (iii) will not be discussed further in this article.

In order to see how my intuitions might work with respect to (i), let it be stipulated that in some world - W_2 say - Eric Blair never writes a word. Let it further be stipulated that *Nineteen Eighty-Four, Animal Farm, Down and Out in London and Paris*, and all Orwell's other works, are written by Aldous Huxley. Furthermore, let it be stipulated that in W_2 Huxley takes the name 'George Orwell' when writing and publishing these works.

My intuition is that, with respect to W_2, Aldous Huxley just *is* George Orwell; not *a* 'George Orwell', other than the 'real George Orwell', but simply George Orwell. This does not seem obviously absurd to me. Perhaps it will seem less absurd if, instead of being couched in metaphysical New Speak, the above outline is expressed in ordinary language, as in (12) to (14) below:

(12)   Eric Blair might never have written anything.

(13)   Aldous Huxley might have written *Nineteen Eighty-Four, Animal Farm, Down and Out in London and Paris*, etc.

(14)   Aldous Huxley might have published all these works mentioned in (13) under the name 'George Orwell'.

After considering sentences (12) to (14), and upon introspection, I am happy to add (15):

(15)   Aldous Huxley might have been George Orwell.

Alternatively, suppose it should be discovered, perhaps tomorrow, that Aldous Huxley really did write all those books, and that for some undisclosed reason Eric Blair took all the credit, masqueraded as the author, and pocketed the royalties. That is to say, let us suppose that Eric Blair masqueraded as George Orwell. It would now seem that even though Eric Blair went through much of his life answering to the name of 'George Orwell', in reality Aldous Huxley *was* 'George Orwell'. By this I certainly cannot mean that Aldous Huxley *was called* 'George Orwell', because he wasn't. Nor do I mean that he was Eric Blair.

What I am suggesting is that I can accept, without any metaphysical or linguistic misgiving, the idea that the property of being George Orwell is transferable.

Kripke would certainly respond, as would Putnam (see the Aristotle-Diogenes example above), that all I am demonstrating by my first example is that it is possible to imagine a world in which Aldous Huxley does all, or most of, the deeds attributed to Eric Blair in this world, up to and including taking the name 'George Orwell'. He would argue that this does not make Aldous Huxley the actual man GEORGE ORWELL who is designated by that name in our language.

Well, of course, if proper names do designate unique individuals, then Kripke is clearly correct, one individual *cannot be* another individual, so it is nonsense to say he might have been. However, the suggestion I wish to defend is that proper names do not designate individuals, rather they denote properties. This may sound quite Russellian, but ultimately my account diverges from his. Furthermore, I am not imagining a world in which Aldous Huxley performs all the deeds commonly attributed to Eric Blair, I am imagining a world in which he does just that portion of Eric Blair's acts which are associated with the name 'George Orwell'.

Kripke's response to my second example concerning Huxley and Blair would possibly be that this is just a case of mistaken identity, whereas my position is that the properties denoted by 'George Orwell' were simply predicated of the wrong subject.

Kripke's fundamental claim is that proper names are given at 'initial baptisms' not as descriptive terms, but as labels of convenience, which - in Mill's words - 'enable individuals to be made the subject of discourse'. Furthermore, he claims that such namings may be achieved either by ostension or by description. Either way, the name given is a rigid designator, and the individual picked out by the ostension or the description is identifiable across worlds. As an example of descriptive naming, Kripke cites Leverrier's (putative) naming of the plane Neptune:

> Neptune was hypothesized as the planet which caused such and such discrepancies in the orbits of certain other planets. If Leverrier indeed gave the name 'Neptune' to the planet before it was ever seen, then he fixed the reference of 'Neptune' by means of the description just mentioned ... Nevertheless, ... 'Neptune' was introduced as a name rigidly designating a certain planet (Kripke, 1980, p 79, fn 23).

Gareth Evans makes a similar claim. He suggests that a name may be introduced by what he terms 'reference-fixing stipulation', as demonstrated in (16) below:

(16)   Let us call whoever invented the zip 'Julius'.
        (From Evans, 1982, p 31.)

He calls such names 'descriptive', and later comments:

> ... it is a feature of the way proper names, pronouns, and demonstratives are used in English that, in evaluating the truth with respect to a possible situation of a sentence containing one of these terms, we are exclusively concerned with whether or not the referent of the term (if any) satisfies (with respect to that situation) the relevant predicate. And I assume that this holds good of those expressions whether or not they have their reference fixed by description ... we would not say...*If you had invented the zip, you would have been Julius* (p 60).

Evans also remarks that 'these facts should come as no surprise', and that the name 'Julius' was introduced into the language by a reference-fixing agreement that precludes the use it is put to in his (22).

Of course, *once it is agreed* that proper names are always and only rigid designators, the inadmissibility of (22) is so obvious that it hardly needs commenting on. But what is the situation if this prior commitment to a theory has not been made?

When I first read the passage just quoted I was disturbed, because I had in fact assumed that 'Julius' *could* be used in just the manner which Evans' claims is disallowed. This was a particularly naive mistake to make, and demonstrated to me that I had completely misunderstood the theory.

Now I am less embarrassed by the error. It seems to me that, qua language user, my intuitions should not be ignored, and if when *I* use the names 'Julius' or 'George Orwell', say, it does *not* seem to me that I am making some internal stipulation to the effect that I am using them in just the way Kripke and Evans describe, then maybe I am not. I might even want to say that, qua language user, my intuitions are as valid as theirs. Donnellan (1979) makes a similar point with respect to Leverrier and 'Neptune'.

Firstly, he remarks that we must be careful to distinguish the 'theoretical issue' of whether it is possible to introduce names as rigid designators by reference-fixing description from the 'factual issue' of whether this is what we actually do. He further comments that Leverrier probably gave no indication of whether he intended the name 'Neptune' to function as a rigid designator or as an abbreviated description. Indeed, it is unlikely that Leverrier gave much - if any - thought to the matter; the man was an astro-physicist, not a philosopher. We might well echo Carruthers' view on folk-psychology and say that it is almost certain that Leverrier had no intentions at all regarding trans-world identity. He just wanted to *name* whatever was disturbing the orbit of Uranus, not rigidly designate it.

It is also idle to suppose that he made some private stipulation to the effect that in some counterfactual situation, in which some other entity satisfied the description, that other entity would *not* be Neptune, or that in that same counterfactual situation in which the planet which in this world *is* disturbing the orbit of Uranus does not do so, that planet would still *be* Neptune.

Of course, Kripke is not suggesting that Leverrier actually made any such conscious and arcane stipulations. His suggestion is that that just *is* the way we use language and those just are the entailments which fall out from such a use. To which Donnellan responds:

> Kripke tells us that this is an example of the introduction of a name as a rigid designator, but why is he so confident that it is not an example of a name introduced as a abbreviation (1979, p 47)?

He continues:

> For my part, when I think of such examples as the "Neptune " case, I don't find myself with any strong intuitions one way of the other (p 49).

Even more tellingly, he concludes:

> ... I doubt that anyone would have any strong intuitions about this concerning a situation such as that of Leverrier and the introduction of the name 'Neptune' - at least *not one not motivated by a theory about the matter* [emphasis added].

Indeed, this last point bears emphasising: predigested theory should not be allowed to exert undue influence over intuitions. Dan Sperber and Deirdre Wilson, in a different context, make just this point:

> It is well known that linguistic judgements may be affected by explicit teaching or conscious theorizing... (Sperber and Wilson, 1981, p 297).

It might be added that the same is true of philosophical judgements.
    There are four, more or less distinct, strands to Donnellan's argument:

(i)   Proper names may be divided into two categories: descriptive and non-descriptive.
(ii)   All non-descriptive names are rigid designators.
(iii) Descriptive names may be either a) rigid designators, or b) the equivalent of abbreviated descriptions.
(iv)   Whether a descriptive name is a rigid designator or an abbreviated description is entirely dependent on the speaker's intentions. The speaker may make an internal stipulation to use such a name as a rigid designator. Presumably, he may also make an internal stipulation to use it as an abbreviated description.

    Donnellan is surely correct to question Kripke's assumptions concerning Leverrier's state of mind; his conclusions, however, are less obviously correct.
    It is instructive to note that in (i) Donnellan lays the foundation for analogising proper names to definite descriptions. This analogy with the referential/attributive distinction is completed in (ii), (iii), and (iv). It seems to me that Donnellan's instincts are sound and that there is a strong parallelism between these two categories of referring expression. Unfortunately, I shall argue in a forthcoming paper that the attributive/referential distinction is illusory, and that *all* definite descriptions are attributive. However, I shall endeavour to restore the parallelism by arguing that *all* proper names are predicates and denote properties, not individuals. This will not, however, commit me to the view that all proper names are abbreviated descriptions in the Russellian sense - the sense that Donnellan assumes.

The point about examples such as 'Neptune' and 'Julius' is that they force certain intuitions into prominence, but I do not wish to claim that such names are categorically distinct from other proper names. On this point I agree with Kripke; irrespective of how reference is fixed at a name-giving, whether it is by ostension or description, there is only one category of proper name.

This may seem an unsatisfactory conclusion, for regardless of how uncertain some speakers may feel about 'Neptune' or 'Julius' or even 'George Orwell', such uncertainty does not seem appropriate with respect to - quite possibly - the majority of proper names. It is unquestionably the case, for example, that Donnellan would be happy to concede that 'Saul Kripke' rigidly designates Saul Kripke.

I shall not discuss this further here. In later papers (in preparation), however, I shall try to show a) that however appealing the notion of rigid designation may be, there are serious flaws in the analysis of how it actually works with respect to indexically used expressions; and b) that the assumption that such terms are rigid designators is not compatible with the empirical evidence. If these two points can be satisfactorily demonstrated, then I suggest that this will cast renewed doubt on the validity of the rigid designation account of proper names also. In other papers (also forthcoming), I shall suggest how these doubts may be resolved by the adoption of an alternative account.

For the moment, it should be sufficient to note that, although I disagree with his overall view, Donnellan's comments concerning intuitions lend support to mine. I may now, perhaps, reiterate the claim that qua language user my intuitions should be as valid as Kripke's. This might be especially so with respect to those intuitions which pre-date my philosophical education.

The discussion is now reduced to mere arm-waving, where the only sound to be heard - in Michael Devitt's felicitous phrase - is the clash of semantic intuitions.[8] It will not be pursued at this level. The intuitions in question were introduced because they constitute one of my *reasons* for wishing to query Kripke's account of proper names; they were never intended to substitute for argument.

4 Productive morphology and eponyms

> There are three main groups of eponymic words; those derived from mythological or fictitious names; those which are descriptive of a person or his works (as, for example, Shakespearean or Shavian); and the 'true' eponymic words which have become a part of the language in a fuller

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[8]Devitt (1994).

sense and are taken from the names of people who actually exist or
once existed (Cyril Leslie Beeching, 1989, p vii).

For Beeching, 'entering into the language in a fuller sense' seems to entail
lexicographical recognition. Thus, in group 3 - that of the 'true' eponyms - he includes
'Darwinism' (and possibly 'Darwinian'), but explicitly *excludes* 'Shakespearean' and
possibly excludes 'Thatcherism' and 'Majorism' also, although 'Gaullism' gains
admittance. This seems quite arbitrary and in the absence of a dictionary, groups 2
and 3 become indistinguishable. There is, however, a distinction which I believe
Beeching wishes to make and which - as will later become apparent - may be
significant.

   There are certain proper names, 'Cardigan', 'Sandwich' and 'Quisling', for example,
which on the basis of anecdote, invention, or association, etc., are used to name
entities quite distinct from the original bearers of those names. Such terms possibly
do form a distinct class. It is, for example, because we already know what sandwiches
are that - if we make any association at all - we know that the Earl of Sandwich ate
them. We do *not* 'calculate' the meaning of 'sandwich' by searching through our
encyclopedic knowledge of John Montagu for appropriate attributes. Furthermore,
not only are such words fully institutionalised, they are also created quite
independently of productive morphological processes. Perhaps these are the 'true'
eponyms.

   Such terms are not, however, the subject of the following discussion. In what
follows 'eponym' is taken to have a much wider sense which embraces, more or less
indiscriminately, Beeching's groups 1 and 2 and also morphologically derived terms
which, due to their institutionalisation, Beeching places in group 3. Perhaps I should
call all these terms 'quasi-eponyms' to distinguish them from the true. For the sake
of convenience, however, I will continue to call them simply 'eponyms'.

   Having stipulated how I intend to use the term, I would now suggest that if proper
names indeed lack all descriptive content, it might prove difficult to give a
satisfactory account of the semantics of eponymy. The eponymous derivations
'Homeric' and 'Hobbesian', for example, seem to be at least *as* descriptive as the
analogous general term derivates 'metallic' and 'mammalian'. In the case of both pairs
it seems plausible to suggest that we draw on encyclopaedic knowledge and make a
choice of attributes associated with the base term which are appropriate in the context.

   It may not be possible to identify precisely what property, commonly associated
with 'Homer' (or Homer), say, is also associated with the derived adjective 'Homeric',
and this indeterminacy may be taken as indicative of the proper name's fundamental
lack of meaning. But this conclusion would be misguided. Outside of a context of

use 'metallic' is equally indeterminate; it might be intended to attribute properties relating to taste, smell, appearance, texture, or possibly composition.

It might be objected that there is a fundamental confusion in this argument, and that whereas general terms clearly *do* contribute some meaning (however indeterminate) to their derivates, the meaning of eponymous derivations is *always* institutionalised independently of any descriptive content erroneously assumed to inhere in the base term. That is to say, it might be claimed that the meaning of general term derivates is compositional while that of eponymous derivation is not.

I shall present two separate responses to this objection. The first appeals to the phenomenon of 'semantic coherence'; the second to the rules of productive morphology.

(i) *Semantic coherence*: This term was coined by Aronoff (1976) to describe certain phenomena. For example, a particular group of de-adjectival nominalisations formed by the addition of 'ness' have totally predictable, tripartite meanings, which are transparently compositional. It is this transparency and predictability which Aronoff calls 'semantic coherence'. Spencer lists the three elements of meaning:

All words of the form *Xousness* mean each of the following three things:

(a) 'The fact that Y is Xous', e.g. 'His callousness surprised me'.
(b) 'The extent to which Y is Xous ', e.g. (again) 'His callousness surprised me'.
(c) 'The quality or state of being Xous', e.g. 'Callousness is not a virtue'. (Spencer, 1991, p 88)

Now it seems to me that similarly predictable, tripartite, transparently compositional meanings may be derivationally produced from proper names. For example, a name such as 'Blair' may first be made adjectival by the addition of the derivationally productive suffix '-ish'. Thus: 'Blairish'. This may be inelegant, but it is acceptable. To this may be added '-ness': 'Blairishness'. Now consider (17) and (18) below:

(17) His Blairishness surprised me.

(18) Blairishness is not a virtue.

I would suggest that the interpretation of these sentences exactly parallels that of Spencer's a), b), and c). It must be conceded that despite this compositional

transparency (semantic coherence), there is still a degree of semantic indeterminacy. However, a similar degree of indeterminacy of meaning is also to be found in 'callousness'. On this evidence, it would seem to be quite arbitrary to suggest that the meaning of 'callousness' is compositional whereas the meaning of 'Blairishness' is not.

(ii) *Morphological rules*: Current morphological theory is a minefield of conflicting views; I shall therefore restrict the discussion to comments on two of the least controversial interpretations of observed regularities.

One question that needs to be addressed is this: If proper names are not meaningful words, how can they constitute satisfactory bases to which to attach derivational suffixes? However, before this question can be addressed, we need to ascertain that proper names do in fact constitute satisfactory bases for derivational suffixation. That is to say, we need to know whether eponymous derivations are rule-governed or merely ad hoc.

Dorothy Siegel (1979), distinguished two types of derivational suffix, Class I and Class II. Class I suffixes may induce phonological changes in the base to which they are joined; Class II suffixes are 'phonologically inert' (Spencer, p 97). Spencer continues:

> Most importantly, Class I suffixes may cause stress shift in the base to which they attach.

An example of a Class I suffix that may attach to a proper name is *-ic*, as in 'Homeric' and 'Napoleonic'. In both cases the stress is indeed shifted to the penultimate syllable. This is neither conclusive nor surprising, and these stress shifts would probably occur with nonsense words also. Nonetheless, such conformity is encouraging.

Fabb (1988), (cited in Spencer), distinguishes suffixes according to rather different criteria. He identifies four groups of suffixes, each group defined by selectional criteria. For example, whereas the suffix '-ism' attaches either to roots or to other suffixes, the suffix '-ian' (or '-an') may only attach to roots. Thus, while both 'vulgarian' and 'vulgarianism' are possible, of the converse pair, 'vulgarism', and '*vulgarismian', only the first term is acceptable.

Interestingly, the same rule-governed patterns are seen in eponymous derivations. As Pinker (1994, p 135) notes, 'Darwinian', 'Darwinism' and 'Darwinianism' are all possible. 'Darwinismian' and 'Darwinsian', on the other hand are not. This is despite the fact that 'Darwinismian' has the possible interpretation 'pertaining to Darwinism' and 'Darwinsian' has the possible interpretation 'pertaining to two (or more) Darwins' (plausibly Erasmus and Charles). In the latter case, it is not only the fact that '-ian'

must attach to roots that makes the resulting (non-) word unacceptable, this unacceptability is determined by the even more powerful morphological rule which prohibits any derivational suffix whatever from attaching to an inflectional suffix.

Once again, the rule-governed conformity of eponymous derivation is encouraging. It is at least compatible with the claim that the meanings of eponymous derivates and those of general term derivates are equally compositional. But can anything else be deduced from it? I suggest that it can.

Many writers have suggested that not only are proper names meaningless labels, or non-descriptive, non-connotative, or whatever, but they are also not even fully lexical. Three such views will be discussed below.

Fred Sommers, for example, makes the following bold claim:

> The right way of understanding rigidity in proper names is to understand it in pronouns. And this is an easy thing to do as soon as one recognises that *proper names are pronouns*. ... Note that pronominalisation *precedes* baptism. ... We can thus view the official act of baptism as an act that introduces a *special duty* pronoun that may henceforth be used in place of the highly equivocal pronouns 'it', 'he' and 'him' that have hitherto been used to refer to the thing in question (Sommers, 1983, p 230).

For Sommers, a proper name is not only pronominal, it is anaphoric, and the antecedent subject term, i.e., the first link in a Kripkean causal chain is 'an indefinite referring expression used in an epistemic context'. (p 231).

Tyler Burge argues that proper names have combined metalinguistic and demonstrative elements. He asserts:

> Roughly, singular unmodified proper names ... have the same semantical structure as the phrase 'that book' (Burge, 1973, p 432).

In fact, this is not a totally accurate description of his own theory. For Burge, a proper name - 'Alfred', for example - is more closely equivalent to 'that person named "Alfred"', which Burge reduces to 'that Alfred'. If Burge is correct, then a proper name is a disguised demonstrative and reference may then only be assigned in a context. As Burge correctly recognises, this reduces proper names to the status of indexicals. In another article he acknowledges this:

> ... the differences between meaning and sense are easiest to notice with indexicals (including proper names)... (Burge, 1979, p 398).

Recanati (1993) also argues that proper names are a combination of metalinguistic reference and indexicality. For Recanati, the meaning of a name 'NN' is appropriately interpreted as 'the bearer of "NN"'. the most obvious way in which this varies from Burge's account is that the demonstrative element is missing.

The nature and function of indexicality will be discussed at length in a further forthcoming paper, so will not be discussed in any detail here. However, what must be noted is that a major feature of indexicality is that it is a function of pro-terms - pronouns, pro-adverbs, even pro-verbs - not of nouns. Therefore, it would seem to follow that anyone advocating an indexical theory of proper names is also committed to recognising that if a proper names is indexical it must also be pronominal.

Now, the empirical evidence relating to eponymous derivations suggests that such a view cannot be correct. It has been shown not only that proper names accept derivational suffixes, but that they do so in a rule-governed manner. Pronouns, on the other hand, appear not to accept suffixation *at all*. If this is correct, then the matter is simple. Proper names cannot be pronouns; in which case they cannot plausibly be indexical either.

Unfortunately, it is difficult conclusively to demonstrate a negative. For example, consider (19) below:

(19)   S (*pointing at John Major*): *Heism is on the way out.

This is clearly unacceptable, despite the fact that (17) does have a possible - and accessible -interpretation, as given in (18):

(20)   Majorism is on the way out.

It might be argued that all the unacceptability of (17) demonstrates is that 'he' cannot accept '-ism', not that 'he' cannot accept any suffix whatever, and certainly not that *no pronouns at all* can accept suffixation. Perhaps the only response to this objection would be to multiply unacceptable instances, taking care to match the bases with theoretically compatible suffixes. That is to say, as '-ness'. for example, requires an adjectival base, nothing would be learned by noting that it cannot attach to 'it'.

The trouble is, even if a thousand carefully suffix-matched, but unacceptable, examples were adduced, this would only lead to an inductive inference, not a deductive proof. Nonetheless, it is worth looking at a few more examples:

(21)   S (*holding aloft a book*): *Thisishness is an unattractive trait in a woman.

(22)   S (*indicating a small child*): *Churchill did not have a happy thathood.

(23)   S (*placing his hand on a bust of Shakespeare*): *I am writing a Sonnet that is quite himian in its lyricism.

The examples (21) to (23) would all have highly accessible interpretations *if we were accustomed to interpreting pronominal derivates*. More precisely, the interpretations would be highly accessible if the language system provided the mechanism for accessing them. But it doesn't.

It seems to me, therefore, that valid counter-examples to the claim that pronouns do not accept suffixation must be adduced if any objection to that claim is to be sustained. In the absence of such examples, it seems safe to conclude that pronouns indeed do not accept suffixes and that therefore proper names are not pronouns, and if they are not pronouns, they are not indexical either.

Before closing this discussion, it should be noted that there are at least three *pro-terms* which *do* accept suffixation: 'thisness', 'thatness' and 'suchness'. However, they do not constitute counter-examples to the above argument. Firstly, although 'this' and 'that' may function as demonstrative *pronouns*, they have a second function as demonstrative *adjectives*. Indeed, it is not clear to me that they are not better defined as ambiguous terms. If this is correct, then if 'this$_1$' is pronominal it is 'this$_2$' - the adjectival member of the pair - which accepts suffixation. This is an uncontroversial claim because, as we have seen, the suffix '-ness' only attaches to adjectival stems. 'Such' need not concern us as it is *never* pronominal.

There is a second line of defence against these putative counter-examples. It seems to me that when 'this' and 'that' do occur in derivational structures, this is a case of mention rather than use. I would further suggest that 'thisness' and 'thatness' are fully lexicalised items rather than productive forms.

It might be worth noting here the distinction between institutionalisation and lexicalisation. According to Laurie Bauer (1983) a 'nonce formation can be defined as a new complex word coined by a speaker/writer on the spur of the moment to cover an immediate need' (p 45). Institutionalisation follows when (and if) 'the nonce formation starts to be accepted by other speakers as a known lexical item ' (p 48). Thus, a productive form such as 'Darwinism' may be said to be institutionalised. Of lexicalisation, Bauer writes that this occurs when because of some change in the language system, a lexeme has, or takes on, a form it could not have had if it had arisen by the application of productive rules' (p 48).

What Bauer has in mind is the lost productivity of certain affixes, for example the '-th' as in 'warmth'. Hence 'warmth' may now be considered to be not only institutionalised but also lexicalised. 'Coolth', on the other hand, is not a part of our lexicon, and cannot now be formed by the application of productive morphological process. Of course 'coolth' can still occur as a nonce form, not because '-th' is still

productive (it isn't), but in virtue of a kind of playing with language. It might even come to be accepted as a kind of common currency; that is to say, it might become institutionalised. Unlike 'Darwinism', however, it would also be *lexicalised*; 'Darwinism' could still be formed today by the application of productive rules. 'Coolth' could not.

It now seems plausible to suggest that 'thisness' and 'thatness' are also lexicalised items, in that they could not be formed by the application of productive rules, and that like 'coolth' they were formed by a kind of playing with language.

It should further be noted that not all writers would agree with Bauer's definitions or divisions. This should not matter if he has, in fact, identified true distinctions. However, if I understand him correctly, the distinction he draws between institutionalisation and lexicalisation is tenuous. Nonetheless, 'thisness' is still either several steps, or at least one step, removed from posing a threat:

(i)   It may be a lexicalised item, originally created by non-productively attaching the otherwise productive suffix '-ness' to a metalinguistic echoic use of 'this'.
(ii) It may be merely institutionalised rather than lexicalised, but nonetheless non-productively created, by attaching '-ness' to echoic 'this'.
(iii) It may be a non-metalinguistic (i.e., non-echoic), derivationally produced lexeme.

Even in the worst-case scenario expressed by (iii) 'thisness' is de-pro-adjectival, not de-pronominal, and consequently does not touch the conclusion that proper names cannot be pronouns because pronouns do not enter into morphologically productive processes.

Finally, I would like to return to the question of the 'meaningfulness' of proper names. I have argued that the facts of eponymous derivation suggest that proper names contribute some descriptive content to their derivates. I have also argued that these same facts demonstrate that proper names are neither pronominal nor in a any sense indexical. As their meanings, therefore, can be neither anaphorically pronominal, nor demonstrative, nor token reflexive (i.e., metalinguistic), it remains to be shown just how that meaning might be characterised.

In a later paper this question will be discussed in greater detail, but for the moment I would like to suggest - somewhat vaguely - that, in Millian terms, proper names are connotative in the way that general terms are connotative. Thus 'Saul Kripke' denotes Saul Kripke and connotes the property of being Saul Kripke. This is a view that has been eloquently summarised by Leonard Linsky:

Common names, for Mill, have both connotation and denotation. Thus 'horse' connotes certain properties, and the name 'horse' denotes the things that have those properties. By contrast, proper names have no connotations; they do not denote in virtue of the possession of certain properties by their denotations, but so to speak, directly. Thus Socrates received his name by being dubbed 'Socrates'; and he might just as well have been given any other name.

The contrast is misleading. After all, we might have named horses by another name, too, 'cow' or 'Pferd'. However, once the convention by which they are called 'horses' is established, it is not correct to call them 'cows'. A horse is not a cow. Just so Socrates could have been named 'Plato' or 'Moses', but once he has been named 'Socrates', it is just as wrong to call him 'Plato' as it is to call a horse a 'cow'. What is correctly called a 'horse' is so called in virtue of its possession of certain properties, just as what is called 'Socrates' is so called in virtue of his possession of the requisite properties. From this point of view, proper names are words like any others (Linsky, 1983, pp 16 -17).

I have quoted this passage at length because I believe it accurately represents the facts. John Tienson, on the other hand, quotes the same passage because he believes that the argument it embodies is flawed (as does Linsky). Tienson argues that '[g]eneral terms apply to new instances' whereas proper names do not. He observes:

If you know the meaning of a general term, normally you can apply it to things you encounter for the first time. If you could not tell whether some new object was a horse, or brown, that would be good evidence that you did not understand the word 'horse' or 'brown' (Tienson, 1986, p 73).

His assumption seems to be that the impossibility of there being *new* instantiations of Pavarotti, say, demonstrates that 'Pavarotti' is not correctly applied to an entity in virtue of the possession by that entity of some appropriate property. (The example is not Tienson's.) This argument is clearly fallacious. Oddly enough, Tienson recognises the fallacy, but then goes ahead and commits it anyway.

Indeed, this argument may be two-ways fallacious. Firstly, the fact that only a single entity may be correctly called by a certain name does not demonstrate *that that* name does not, in some sense, *describe* that entity, or conversely that *that* entity is not correctly called by *that* name in virtue of the possession of some appropriate property.

Secondly, I suggest that there can be, and indeed are, new instantiations of the entities that are denoted by proper names, but that these new instantiations are not generally recognised as such.

I shall argue in a forthcoming article that a proper name is a special type of mass noun. If this is correct, and 'Luciano Pavarotti', say, names the entire mass that is Pavarotti, in much the same was as 'coal' names the entire mass that is coal, then Tienson is correct to assume that we will not encounter separable and novel bits of Pavarotti in the way that we might encounter separable and novel bits of coal. But we can, and do, encounter novel *time-slices* of Pavarotti, which are conceptually, if not mathematically, as far from being the entire mass of Pavarotti as a single lump of coal is from being the entire mass of coal.

Furthermore, if we understand the word 'Pavarotti', then we can tell whether this or that time-slice of a particular entity is a time-slice of Pavarotti or of Placido Domingo or of George Formby or of Margaret Bennet - provided we know the *meanings* of the words naming those entities also.

We do not say that the man singing in the Park (in the rain) is Pavarotti because he is *called* Pavarotti, but because he *is* Pavarotti. Furthermore, we can tell that *that* man is Pavarotti in virtue of his possession of certain requisite properties. If certain of his attributes - his appearance, the quality of his voice perhaps - conform with certain bits of encyclopaedic information and perceptual memory we have about and of Pavarotti, then we say that that man *is* Pavarotti. This, I suggest, is also the way we can tell whether a new object, encountered for the first time, is or is not a horse, or brown, or whatever.

An objector might now say that reference-fixing criteria are being confused with connotative meaning; and that of course a language user can fix the reference of 'Pavarotti' by checking off properties. Such an objector might further assert that Pavarotti is not Pavarotti in virtue of the possession of this check-list of attributes, which may be convenient for identification purposes, but are entirely contingent.

This is the argument of the rigid designator, but I do not understand it. I doubt if many speakers (outside of a laboratory) recognise water or gold or dandelions or horses or brown in virtue of any properties that Kripke (or the objector) would consider anything other than contingent. Even chairs and tables, which presumably may be recognised by what turn out to be defining properties, do not in virtue of this fact confer those properties onto 'chair' and 'table' as their respective meanings. Should we then conclude that none of these words has any connotation? Perhaps we should. Some such consideration was presumably behind Hilary Putnam's despairing cry 'Cut the pie any way you like, 'meanings' just ain't in the *head*!' (Putnam, 1975, p 227), or Jerry Fodor's more resigned comment:

> The older I get, the more inclined I am to think that there is nothing at all to meaning except denotation; for example, that there is nothing to the meaning of a name except its bearer and nothing to the meaning of a predicate except the property it expresses (Fodor, 1990, p 161).

I understand Putnam and Fodor, and - depending on how 'meaning' is understood - they may even be right. What I do not understand is the claim that the (intensional) meaning of 'Aristotle' or 'Gorbachov', say, is more elusive (to the point of non-existence) than the (intensional) meaning of 'horse' or 'computer' or 'hot' or 'headache', let alone 'good' and 'happy' and 'pain'.

  I do not want, either here or later, to enter into any discussion concerning the relative merits of informational semantics or theories appealing to conceptual roles or holistic or atomistic meaning. All I wish to suggest is this: whatever meaning is, proper names have as much (or as little) of it as any other word. I shall further argue that it is quite wrong to suggest that they do not attribute properties, although just what those properties are may be uncertain. Connotations are notoriously difficult to pin down.

5 Defining rigidity

If it could be shown that rigid designation cannot be coherently defined, then there would be no need to go further. The theory would collapse, and another account of the semantics of proper names would have to be sought. In fact, defining rigidity is not a straightforward task, and although the problems are philosophical, and in some cases arcane, the issue is of such fundamental importance that I will - briefly - outline the most significant of those problems.

  Kripke, the founding father of rigid designation, makes various claims concerning its nature. He writes:

> Let's call something a rigid designator if in every possible world it designates the same object (Kripke, 1980, p 48).

However, on the next page he qualifies this:

> ... a designator rigidly designates a certain object if it designates that object *wherever that object exists* [emphasis added].

Kaplan, whose work on demonstratives and direct reference is equally seminal comments:

> There are two 'definitions' of 'rigid designation' in *Naming and Necessity* ... The first conforms to what seems to me to have been the intended concept - the same designation in all possible worlds - the second ... conforms to the more widely held view that a rigid designator need not designate the object, or any object, at worlds in which the object does not exist. According to this conception a designator cannot, at any given world, designate something that does not exist in that world (Kaplan, 1989a, p 493).

In fact, when pushed (by Kaplan), Kripke redefines his positions on rigidity, and Kaplan reports his response:

> In a letter ... Kripke states that the notion of rigid designation he intended is that "a designator *d* of an object *x* is *rigid*, if it designates *x* with respect to all possible worlds where *x* exists, and *never designates an object other than x with respect to any possible world*" (Kaplan, 1989b, p 569).

As can be seen from this response, Kripke does not wish to be drawn on the question of whether a rigid designator can or cannot designate an object at worlds in which that object does not exist. Kaplan comments that the definition 'is designed to be neutral' on just this point as Kripke wishes 'to avoid getting bogged down in irrelevant discussion of the existence question' (1989b, p 569).

This is confusing, partly because in the preface to *Naming and Necessity*, which predates his response to Kaplan, Kripke seems to endorse the stronger position, i.e., that a rigid designator has the same designation all possible worlds:

> ... a proper name rigidly designates its referent even when we speak of counterfactual situations where that referent would not have existed (p 21, fn 21).

Also, it is not clear that the existence question should be dismissed as irrelevant when the definition of rigidity is under discussion. Moreover, it is not only in the Preface of *Naming and Necessity* that Kripke appears to adopt the stronger position. In the body of the text, he writes:

> ... when we speak of a counterfactual situation, we speak of it in English, even if it is part of that counterfactual situation that we were all speaking German ... in describing that world, we use *English* with *our* meanings and *our* references. It is in this sense that I speak of a rigid designator as having the same reference in all possible worlds. I also don't mean to imply that the thing designated exists in all possible worlds, just that the name refers rigidly to that thing. If you say 'suppose Hitler had never been born' then 'Hitler' refers here, still rigidly, to something that would not exist in the counterfactual situation described (pp 77-8).

It is not clear to me that there is any distinction between designating 'at worlds in which the object does not exist' and designating a referent 'even when we speak of those counterfactual situations where that referent would not have existed'.

Why then does Kripke retreat to the weaker, neutral, position in his later response to Kaplan, when he already appears to have committed himself to the stronger? Kaplan also appears baffled by these apparent contradictions and comments:

> It is good to know [Kripke's] mind on the matter, and I regret misrepresenting his views. I cannot, however, feel embarrassed by my reading of the textual evidence (1989b, p 570, fn 8).

The important point to note is that there appear to be (at least) three distinct definitions of rigid designation current in the literature:

RD1: This is the view which Kaplan identifies as the 'more widely held'. According to this definition, a rigid designator does not, indeed *cannot*, designate an object at worlds in which that object does not exist. This view is possibly held by Carruthers, who, in the passage quoted above, observes that rigid designators 'designate the same items in all possible worlds in which they exist'. This does not, of course, preclude the possibility that they also designate entities in (or at) worlds in which those entities do not exist. Carruthers may just be maintaining a neutral position.

RD2: This is the middle ground - held by Kripke. The definition is neutral with respect to whether a designator does, or does not, designate an object at worlds in which that object does not exist.

RD3: This is the strong position - held by Kaplan. According to this view, a rigid designator has the 'same designation in *all* possible worlds' (Kaplan, 1989a, p 493)

irrespective of whether the object thus designated does or does not exist in all of those worlds.

Nathan Salmon notes the apparent emergence of yet a fourth definition:

> Dummett (1973), Linksy (1977), and Putnam (1973) each define yet a fourth notion of rigid designator, distinct from each of the three notions given here. They call a designating expression a *rigid designator* if it designates the same thing with respect to every possible world with respect to which the expression designates anything at all (Salmon, 1982, p 33, fn 36).

Salmon further comments that it is unlikely that the above-mentioned writers intend to diverge from the definition proposed by Kripke. Their confusion over what exactly was proposed by Kripke, however, is instructive.

It has been suggested by various writers[9] that there are distinct problems associated with **RD1**. The most notable of these problems takes the following form: if a possible worlds treatment of modality is adopted, a statement of the form of (24) below may be paraphrased as in (25).

(24)   Saul Kripke might not have existed.

(25)   There is a possible world in which Saul Kripke does not exist.

The truth or falsity of the propositions expressed by (24) and (25) can then be evaluated by checking the worlds in the model. If there is at least one world in which (or at which) (26) is true, then (24) and (25) will be true also.

(26)   Saul Kripke does not exist.

If we assume **RD1** the problem should now be apparent. At any world in which Saul Kripke does not exist 'Saul Kripke' fails to designate anything. At such a world (26) either cannot be evaluated at all or it is false. For those who are reluctant to adopt a tri-valent logic, Saul Kripke will thus turn out to be a necessary existent, as in every world in the model (26) will be false. For those who are prepared to concede that (26) is unevaluable rather than false (24) will also be unevaluable, whereas intuitively it seems to be true.

Nathan Salmon discusses the problem with respect to (27).

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[9]See, for example, Gallois (1986) and Smith (1984).

(27)   Nathan Salmon is dead.

He writes:

> It is assumed that I cease to exist when I die. On the usual theories of the truth-value of a simple subject-predicate sentence containing a non-denoting subject term we should not expect the sentence displayed above to be true with respect to a future time if the name 'Nathan Salmon' denotes no one with respect to that time (Salmon, 1882, p 37).

In other words, if **RD1** is assumed it becomes impossible to say truthfully of any individual that he is dead. Kaplan makes a similar observation:

> There are worlds in which Quine does not exist. It does not follow that there are worlds with respect to which 'Quine' does not denote. What follows is that with respect to such a world 'Quine' denotes something which does not exist in that world. Indeed, Aristotle no longer exists, but 'Aristotle' continues to denote (him) (Kaplan, 1975, p 503).

Unfortunately, the adoption of **RD3**, as Kaplan advocates, is not a solution free from problems. In order to promote his view of rigidity, as defined by **RD3** Kaplan writes:

> ... it is a striking and important feature of the possible world semantics for quantified intensional logic, which Kripke did so much to create and popularize, that variables, those paradigms of rigid designation, designate the same individual in *all* possible worlds whether the individual "exists" or not (Kaplan, 1989b, p 493).

As my knowledge of quantified intension logics is strictly limited, I will stipulate that Kaplan's assertion is correct. The problem starts when we try to assimilate natural language to formal languages.

  It may be that the variables of certain formal languages do indeed designate the same individual in *all* possible worlds, in fact I have just stipulated that they do. If an attempt is made, however, to equate these variables with the pronouns of natural language, I suggest that the picture is vastly changed. Now it is quite clear that in his account of demonstratives - and other indexical usages - of natural language Kaplan does assume just such an equivalence. As this assumption is seminally important to my argument, I shall quote his views at length:

> Pronouns in natural language have often been analogized to variables. Pronouns are lexically ambiguous, having both an anaphoric and a demonstrative use. An anaphoric use of a pronoun is *syntactically bound* to another phrase occurring elsewhere in the discourse. In meaningful discourse, a pronoun not used anaphorically is used demonstratively. As I saw the matter, a demonstrative use of a pronoun was simply a *syntactically free* use. Like a free occurrence of a variable, it requires something extralinguistic, a *demonstration* ... to *assign* it a value. Demonstrative and anaphoric occurrences of pronouns can thus be seen to correspond to free and bound occurrences of variables. What I want to stress is that the difference between demonstrative and anaphoric uses of pronouns need not be conceptualized primarily in terms of lexical ambiguity; it can also be seen in terms of the syntactic distinction between free and bound occurrences of terms. I saw an analogy between variables and pronouns as even closer than had been thought (Kaplan, 1989b, p 572).

It should be noted that the use of the past tense in the above extract is misleading. In this passage Kaplan reviews and *reaffirms* the conclusions he reached in *Demonstratives*. He is not setting the stage for a reversal of opinion. This is evidenced by his next comment:

> I believe that the case of the free pronoun, the demonstrative, can take a lesson from the case of the free variable.

If we overlook Kaplan's confusion over the distinction between syntactic anaphors and semantic anaphora, his views are quite clear, and I shall try to show that they are mistaken.

   What is striking about the above passage is Kaplan's apparent indifference as to whether a pronoun such as 'he', say, is best defined as lexically ambiguous or as a single word with multiple functions. However, I hope to demonstrate in another paper that when certain characterisations of direct referentiality and *de jure* rigidity are considered, the lexical status of pronouns assumes vital importance. I shall argue that if pronouns are *not* ambiguous - and I hope to show that they are not - then it will follow that they *cannot* be directly referential or *de jure* rigid either. If this is correct, then they cannot be equated with the variables of quantified intensional logic.

   If all this can be adequately demonstrated, it will affect Kaplan's claims concerning rigidity quite profoundly. Kaplan claims that variables 'are paradigms of rigid

designation' (see extract quoted above). If being a paradigm is to mean anything, then this may surely be reduced to:

If anything is a rigid designator than a (free) variable is.

This is simple modus ponens: $P \supset Q$,
where $P = \exists x$(rigid designator(x)),
and $Q = \forall y$(free variable (y) $\supset$ rigid designator (y))

As was seen above, Kaplan further claims that pronouns may be analogised to variables and that the free pronoun 'can take a lesson' from the free variable. If this claim is to be taken seriously, then it must surely mean that the free pronoun is to natural language what the free variable is to quantified intensional logic. Kaplan's intention must surely be to claim that, with respect to natural language, a free pronoun is a prototypical rigid designator. That is to say:

If anything is a rigid designator then a free pronoun is.

This in turn may be more formally expressed as $R \supset S$,
where $R = \exists x$(rigid designator(x)),
and $S = \forall y$(free pronoun(y) $\supset$ rigid designator(y)).

Now, if it can be shown that free pronouns (i.e. demonstratives or other indexical usages) are *not* rigid designators, i.e., if it can be shown that ~S, then by modus tollens it follows that ~R. Less formally:

If a free pronoun is not a rigid designator then nothing is.

I am inclined to think that this is probably correct, and that if it *can* be shown that free pronouns really are not rigid designators, then the whole edifice of rigid designation would be radically undermined, and indeed nothing in natural language would designate rigidly.

  However, such a conclusion would be precipitate. It could be that the major premise is not sustainable. That is to say, it might be argued that if a free pronoun is not a rigid designator, it quite clearly cannot be prototypically rigid. This would leave the door open for the suggestion that even though free pronouns may be shown not to be rigid, nonetheless there may be another category of terms - proper names say - which are.

Denying the major premise but hanging on to the thesis of rigidity seems to me to be gallant but ill-advised. If this major premise is denied, then with respect to natural language the proponents of rigidity will have lost much of the support of quantified intensional logic, since the analogy between variables and pronouns will have been denied also.

Finally, once this analogy has disintegrated, then Kaplan's strongest argument in favour of **RD3**, with respect to natural language, will have disintegrated also. I suggest that the effect of *this* would be to add to the uncertainty over whether rigid designation can, in fact, be coherently defined.

All this is yet to be demonstrated. However, it is clear that an analysis of pronouns and their indexical and demonstrative functions is fundamentally important to the entire thesis of rigidity. Therefore, although proper names are my ultimate target, it is with an investigation of pronouns in particular and indexicality in general that I believe any investigation of rigid designation should start.

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