*In A. Bianchi, ed.,* On Reference*, Oxford, Oxford University Press, 2015, pp. 93-106.*

REPETITION AND REFERENCE

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[W]e are, for the most part, language *consumers*. Words come to us prepackaged with a semantic value…. In our culture, the role of language creators is largely reserved to parents, scientists, and headline writers for *Variety*; it is by no means the typical use of language.

David Kaplan (1989: 602)

At some point the name “Aristotle” entered our practice and then its semantics was finished. Passing it from one to another is like passing the salt.... If this picture has merit, then again we see that it’s a mistake to see each user as fixing reference. This would be like reinventing the wheel. It’s there already.

Howard Wettstein (2012: 118n)

What makes it the case that a certain linguistic particular, for example the sound with which my utterance of “Giulia is not in this room” begins, or the scribble at the extreme left of my inscription of the same sentence, *refers* to something or someone (Giulia, in the example)? If we restrict this metasemantic question to those linguistic particulars that may be seen as proper name tokens, everyone knows Saul Kripke’s quite convincing answer. Let me quote the first appearance of it in the second lecture of *Naming and Necessity* in full:

Someone, let’s say, a baby, is born; his parents call him by a certain name. They talk about him to their friends. Other people meet him. Through various sorts of talk the name is spread from link to link as if by a chain. A speaker who is on the far end of this chain, who has heard about, say Richard Feynman, in the market place or elsewhere, may be referring to Richard Feynman even though he can’t remember from whom he first heard of Feynman or from whom he ever heard of Feynman. He knows that Feynman is a famous physicist. A certain passage of communication reaching ultimately to the man himself does reach the speaker. He then is referring to Feynman even though he can’t identify him uniquely. He doesn’t know what a Feynman diagram is, he doesn’t know what the Feynman theory of pair production and annihilation is. Not only that: he’d have trouble distinguishing between Gell-Mann and Feynman. So he doesn’t have to know these things, but, instead, a chain of communication going back to Feynman himself has been established, by virtue of his membership in a community which passed the name on from link to link, not by a ceremony that he makes in private in his study: ‘By “Feynman” I shall mean the man who did such and such and such and such’. (Kripke 1972: 91-2)

However, one should not forget that Kripke took care to make it explicit that he was presenting not a theory but only “a *better picture* than the picture presented by the received views” (93; see also pp. 96-7), “a picture which, if more details were to be filled in, might be refined so as to give more exact conditions for reference to take place” (94). The fact is that even today, more than forty years later, we do not possess a full-blown theory built on this picture.[[1]](#footnote-1) My aim here is to take some steps towards producing one.

But why did Kripke abstain from refining his picture “so as to give more exact conditions for reference to take place” and thus from offering a theory? He mentions different kinds of reasons. The first, jocular, is that he was “sort of too lazy at the moment” (1972: 93). A more serious one, which I will discuss at length later, is that he came across a problem that he did not know how to solve without introducing some circularity. Finally, here and there he expresses some skepticism towards the task itself. “One might never reach a set of necessary and sufficient conditions,” he writes. Indeed, he confesses to being “always sympathetic to Bishop Butler’s ‘Everything is what it is and not another thing’ – in the nontrivial sense that philosophical analyses of some concept like reference, in completely different terms which make no mention of reference, are very apt to fail” (94). As a consequence, he abstains from offering a theory because if he did, the result would have the “defect” that “is probably common to all philosophical theories”: it would be “wrong” (64).

While the task may indeed prove to be very difficult, Kripke’s way of characterizing it seems to me somewhat misleading, and as a consequence his skepticism (if it is to be taken seriously) unjustified. If what is sought for were an *analysis* of the *concept* of reference, as Kripke writes, then he could certainly be right, as that concept might very well turn out to be primitive. But to assume that a theory of reference deals with the concept of reference to offer a, presumably *a priori*, analysis of it is no more reasonable than to assume that a theory of, say, water deals with the concept of water in order to offer a, presumably *a priori*, analysis of it. What is sought for, to come back to the question I started out from, is something that tells us what makes it the case that certain linguistic particulars refer to certain other, as a rule non-linguistic, things. If this is so, a theory of reference deals with, or at least should deal with, *reference*, a worldly relation that some worldly entities bear to some other worldly entities, in order to discover, presumably *a posteriori*, its *nature*, and hence explain when, and only when, the former bear it to the latter. In short, the question that I qualified as metasemantic at the beginning is in fact a *metaphysical* question – it asks about the nature of one of the most basic relations appealed to in semantics – and as such requires a *metaphysical* answer.[[2]](#footnote-2)

If this is the task, Kripke’s sympathy for Butler’s motto is simply out of place. While the concept of reference might very well be primitive and thus non-analyzable, the relation of reference *must* be accountable in terms of something else, if it is real (and I have no doubt about this). The following passage from Jerry Fodor’s *Psychosemantics* illustrates the point vividly, even though it concerns the aboutness of mental states rather than linguistic reference:

I suppose that sooner or later the physicists will complete the catalogue they’ve been compiling of the ultimate and irreducible properties of things. When they do, the likes of *spin*, *charm*, and *charge* will perhaps appear upon their list. But *aboutness* surely won’t; intentionality simply doesn’t go that deep. It’s hard to see, in face of this consideration, how one can be a Realist about intentionality without also being, to some extent or other, a Reductionist. If the semantic and the intentional are real properties of things, it must be in virtue of their identity with (or maybe of their supervenience on?) properties that are themselves *neither* intentional *nor* semantic. If aboutness is real, it must be really something else. (Fodor 1987: 97)

Perhaps Fodor is too radical here. Perhaps completing the catalogue of the ultimate and irreducible properties of things is not a prerogative of physicists. Even so, it is hard to believe that semanticists are contributing to it. Therefore, *pace* Butler and Kripke, reference “must be really something else.” Which means, roughly, that it should be possible, at least in principle, to spell out in a non-circular way what reference is, and hence to offer “exact conditions for reference to take place.”

We may then conclude that, to answer the metasemantic question I started out from, a theory of reference may, and should, be sought for. The theory must take the following form:

(1) ∀x ∀y (x refers to y ↔ x R y),

with “x” ranging over linguistic particulars and “R” being a signpost for a compound two-place predicate whose non-logical constituents express ultimate and irreducible properties and relations, or, more reasonably, properties and relations that are more basic than reference and whose explicability in terms of ultimate and irreducible properties and relations is not precluded.

In what follows, I will offer an outline of such a theory. For brevity, I will call it simply *my theory*. However, I must say that the expression should be taken as modified by implicit scare quotes. In fact, the two notions my theory makes use of need to be better understood, and one major problem needs to be solved. In its present form, my theory is not much more than a hint at how I think Kripke’s picture should be developed. If I nevertheless present it here, it is because I hope that the discussion may help to reshape the research in this field. And, in the end, I am confident that it is possible to refine it so that it will eventually become a satisfying theory of reference, and so an adequate answer to the metasemantic question I started out from.

I will proceed as follows. Before presenting my theory, I will discuss a desideratum, which has to do with my rather unorthodox views about the relationship between language and thought. As will soon be realized, this desideratum makes it even more difficult to formulate an adequate theory of reference. Only at this point will I state my theory and try to elucidate it and to show that it satisfies the desideratum. Then, I will mention a couple of minor problems, and show how I think they can be solved. Finally, I will present and discuss what I take to be the major problem.

Let us begin with the desideratum. In my opinion, standard accounts of the relationship between language and thought are wrong. These accounts view public languages as a means to express and communicate independently formed thoughts. On the contrary, it seems to me that there are good reasons to claim that language constitutes thought: we think by means of the language we speak.[[3]](#footnote-3) As a consequence, while according to standard accounts the semantic properties of linguistic expressions are to be explained in terms of the intentional properties of mental states, in my opinion things should be the other way round: the intentional properties of (post-perceptual) mental states are to be explained in terms of the semantic properties of linguistic expressions. Just to give a simple example: whereas according to standard accounts my utterance of “Giulia” referred to Giulia because I was somehow thinking of her, I would rather say that I was thinking of Giulia because when thinking I was tokening an expression of some public language that referred to her.

Now, this is only an oversimplified sketch, and I have not offered any argument in favor of it here. Of course, it is not meant to persuade anyone. However, this should be enough to convince the reader that accepting an account of the relationship between language and thought along these lines considerably constrains a theory of reference: if one wanted to explain the intentional properties of mental states in terms of the semantic properties of linguistic expressions, one should not explain the semantic properties of linguistic expressions in terms of the intentional properties of mental states.[[4]](#footnote-4) So, given that I accept such an account, here is the desideratum that I think a theory of reference should satisfy: it must not appeal to the intentional properties of mental states in a non-eliminable way.

In order to make things clearer, let me give an example of an account of reference that does not satisfy this desideratum. Some pages after the first sketch of his “picture,” in order to avoid a problem that I will discuss later, Kripke proposes some refinements. Indeed, he writes:

An initial ‘baptism’ takes place. Here the object may be named by ostension, or the reference of the name may be fixed by a description. When the name is ‘passed from link to link’, the receiver of the name must, I think, intend when he learns it to use it with the same reference as the man from whom he heard it. If I hear the name ‘Napoleon’ and decide it would be a nice name for my pet aardvark, I do not satisfy this condition. (Kripke 1972: 96)

Kripke himself notes that this way of putting things is somewhat unsatisfying. As he acknowledges, “the preceding outline hardly *eliminates* the notion of reference; on the contrary, it takes the notion of intending to use the same reference as a given” (97). Indeed, this is perhaps the main reason why Kripke limited himself to offering a picture rather than producing a theory: he did not find a way to avoid this circularity. However, this is not what I take to be the real problem. Perhaps, some other, non-circular, way of characterizing (what Kripke takes to be) the crucial intention can be found. But still an *intention* it remains. Kripke is appealing to the intentional properties of certain mental states to account for reference. If this is so, even if one found a way to avoid the circularity that worries Kripke, the resulting theory would not satisfy my desideratum.

Of course, no one is forced to buy the desideratum. Those who have more orthodox views than I have about the relationship between language and thought will not feel under any pressure to accept it. So, I do not want to insist on it. I mentioned it only because my quest for a theory of reference was driven by it: it is the quest for a theory that satisfies the desideratum. I hope, however, that there is agreement on this at least: if a theory can be offered that seems to work and does not rely on intentions, it certainly deserves attention, whatever views about the relationship between language and thought one has.

Let me just add a final remark concerning this. At the very beginning of this chapter, I qualified as quite convincing the answer to the metasemantic question about reference that one can get from Kripke’s first sketch of his picture (“Someone, let’s say, a baby, is born …”). In *that* sketch, however, no mention whatsoever was made of intentions. Thus, if we are to develop *that* answer – the answer that emerges from *that* sketch – into a theory, why should we appeal to them?[[5]](#footnote-5) All the more so, if the specific problem that induced Kripke to refine his picture by appealing to intentions can be solved, as I will try to show below, without appealing to them.

It is now time to state (a first version of) my theory. Here it is:

(2) ∀x ∀y (x refers to y ↔ ∃z (x is a repetition of z ∧ z was introduced for y)).

Before trying to defend it, I need to say something to elucidate it. First of all, what about the relata of the relation that my theory is a theory of? The metasemantic question I started out from was a question about certain linguistic particulars and their reference. So, it is important to note that I am concerned here with what makes it the case that certain linguistic particulars refer, and do not aim to account for what speakers *do* when they are referring to something. In other words, I am concerned here not with *speaker’s reference* but with *semantic reference*, to say it with Kripke (1977: 263-4). Or, to say it with David Kaplan instead, “not with the vagaries of actions, but with the verities of meanings” (1989: 585). Once we possess an account of semantic reference, we can almost certainly use it to account for speaker’s reference and so to contribute to speech-act theory, but the relationship between the two notions is not straightforward.[[6]](#footnote-6) So, for example, I am ready to concede that a linguistic particular whose semantic reference is *a* can be produced by someone who is not referring to *a*, or even not referring to anything.[[7]](#footnote-7) For this reason, I find the following considerations by Kripke quite misleading:

More exact conditions [for reference to take place] are very complicated to give. They seem in a way somehow different in the case of a famous man and one who isn’t so famous…. If … the teacher uses the name ‘George Smith’ – a man by that name is actually his next door neighbor – and says that George Smith first squared the circle, does it follow from this that the students have a false belief about the teacher’s neighbor? The teacher doesn’t tell them that Smith is his neighbor, nor does he believe Smith first squared the circle. He isn’t particularly trying to get any belief *about the neighbor* into the students’ heads. He tries to inculcate the belief that there was a man who squared the circle, but not a belief about any particular man – he just pulls out the first name that occurs to him – as it happens, he uses his neighbor’s name. It doesn’t seem clear in that case that the students have a false belief about the neighbor, even though there is a causal chain going back to the neighbor. (Kripke 1972: 95-6)

Kripke seems to suggest here that the linguistic particular that the teacher produces does not refer to George Smith, the teacher’s neighbor. Why it should not do so, however, is far from clear. In this passage, Kripke seems to confuse speaker’s reference with semantic reference. Perhaps, by producing that linguistic particular, the teacher is not performing the speech act of referring to anyone (though I doubt even this), but it is difficult to deny that the linguistic particular that is produced refers to Smith. Indeed, the name that the teacher pulled out is, as Kripke recognizes, precisely a name *of* him. And, while it is true that linguistic expressions are often used by speakers to “inculcate” beliefs about their referent and even more often they originate beliefs about their referent in hearers, this does not seem to be a condition for semantic reference to take place. Quite the opposite, I would surmise: it is the fact that semantic reference takes place that is a condition for the inculcation and origination of beliefs via linguistic exchange to take place.[[8]](#footnote-8)

I need also to make it clear that I am not purporting here to offer a general theory of reference for linguistic particulars. In fact, my limited aim is to hint at how Kripke’s picture of the reference of proper names may be developed into a theory. I think that my theory can quite easily be extended to account for the reference of other linguistic particulars (tokens of terms for substances and natural and artifactual kinds, and possibly of all other referring common nouns, adjectives, verbs, and adverbs). It may also accommodate any referential tokens of definite descriptions, if only we see them as proper names introduced in special contexts, as Keith Donnellan’s (1966: 282 and 302-4) assimilation of them to Bertrand Russell’s logically proper names may suggest. The case of indexicals and demonstratives, however, is more delicate. But, perhaps, their so-called *character* might be seen as something like a general instruction for introducing linguistic particulars of a given form (e.g., “this”) for other, as a rule non-linguistic, particulars. If this were so, even they would be covered by my theory (see my formulation (3) later in this chapter).

Now, I must say something about the two notions that are employed in my theory, those of *introduction* and *repetition*. In their joint book, Michael Devitt and Kim Sterelny summarize nicely the problems that have to be dealt with to develop Kripke’s picture into a theory:

How is a person able to use ‘Einstein’ to designate a physicist he has never met and whose theories he does not grasp? This problem divides in two.

1. How do we explain the introduction into our language of ‘Einstein’ as a name for Einstein? We need to explain how people were first able to use that noise to designate a certain individual. This requires a theory of *reference fixing*….

2. How do we account for the social transmission of the name ‘Einstein’ within the linguistic community? None of us had anything to do with the introduction of the name but can use it to designate Einstein because we have gained the name from others. To explain this we need a theory of *reference borrowing*. (Devitt and Sterelny 1999: 66)

If these are the problems, we may say that the notion of *introduction* that I employ in my theory addresses the first, that of reference fixing, while the notion of *repetition* addresses the second, that of reference borrowing.

As to introductions, though, I must admit that I do not have much to say.[[9]](#footnote-9) At first glance, this seems to be the part of Kripke’s picture that is easier to develop into a theory. Indeed, none of Kripke’s doubts have to do with it. The typical, though certainly not the only (as we shall soon see), case of introduction is that of a *dubbing*. If this is so, one may object that in appealing to the notion of introduction in my theory I am blatantly not satisfying my desideratum, given that dubbings require intentions on the part of the dubbers. However, a reasonable response – which would obviously need to be carefully worked out – is that, while it has to be conceded that as a rule intentions are at play in a dubbing, it does not follow that they are what makes it the case that the linguistic particular thereby produced and something else instantiate the relation of introduction.[[10]](#footnote-10) Indeed, in Kripke’s refinement of his picture that we have already discussed and criticized, intentions are called for not to account for “the initial ‘baptism’” (concerning which Kripke limits himself to saying that “here the object may be named by ostension, or the reference of the name may be fixed by a description”) but to characterize what happens afterwards, “when the name is ‘passed from link to link’.” I am not suggesting that accounting for introduction is easy (it is not!). On the contrary, I will speculate at the end of this chapter that a better understanding of it is exactly what is necessary for dealing with what I take to be the major problem for my theory. But for now I have nothing interesting to say about this, so let me move on to what was really troubling Kripke and drove him to search for some refinements of his initial picture: the fact that “*of course not every sort of causal chain reaching from me to a certain man will do for me to make a reference*” (1972: 93, emphasis mine). It was precisely the quest for a solution to this problem that induced Kripke to let intentions play a role in his account of reference.

My claim is that this problem can be dealt with by appealing to the notion of *repetition*, rather than to that of intention. I take this notion from Kaplan’s “Words,” where it is used to account for the metaphysics of words. I propose to use it to account instead for the reference of those linguistic particulars that may be seen as proper name tokens. In short, my claim is that repetition is what allows for the phenomenon that Devitt and Sterelny aptly call *reference borrowing*.

The problem Kaplan wants to solve is this:

Suppose a particular word is transmitted from you to me. Now at some point I make a transmission to someone else. Question: Am I transmitting that very word? (Kaplan 1990: 101)

He reframes it in the following manner:

Some word was transmitted to me by way of utterance or inscription. I transmit some word by way of utterance or inscription. We can phrase the question in this way: Take the utterance or inscription received and the utterance or inscription transmitted. *What makes it that the transmission is an utterance or inscription of the same word as that received*? We can thus turn the question into a question about the relationship between input and output utterances or inscriptions. (101, emphasis mine)

Note that now the problem concerns a relation between two linguistic particulars. What I want to suggest is that, whenever the utterances or inscriptions at issue are proper name tokens, Kaplan’s answer to his question (whether or not it is a good answer to that question),[[11]](#footnote-11) also serves as the right answer to the following question: What makes it the case that the transmission is an utterance or inscription (i.e., a linguistic particular) that is co-referential with the one received (i.e., that refers to the thing the received utterance or inscription referred to)?

Kaplan’s answer is simply this: Repetition. To justify it, Kaplan appeals to a “thought experiment”:

I say the name of an individual, possibly a name known to the person to whom I am speaking. The subject is to wait for a count of five, and then repeat the name. I say a name, then the subject says the name. I say the next name, then the subject says the next name. So, if I say “Rudolf”, the person says “Rudolf”; “Alonzo” – “Alonzo”; “Bertrand” – “Bertrand”, and so on. Because we have to worry that the subject may be, in Kripke’s term, reticent, if he succeeds in repeating the name, we reward him with a dollar, or, if he has tenure already, a thousand dollars, enough at any rate to motivate him. I think that if we set up the story in this way – the subject is highly motivated, he is sincere, he is not reticent, he is reflective – whatever that means, then we are very strongly inclined to say that when this person speaks, he is repeating the very name that he heard. I’m not saying what’s happening inside the black box, I’m not saying *how* he does it, I’m just saying that from the description of the case it’s clear that we would agree to describe his output as a *repetition* of *that* name. This notion of repetition is central to my conception….

Contrast this with a wealthy mischievous subject who has decided that he’s going to play a trick on us and instead of repeating the names as he hears them, ignores the input and just utters names at random (or, he may have prepared his own list ahead of time which he recites in order). Even if, by happenstance, the sounds that come out in these two cases equally resemble the sounds that went in, the first case is a case of repetition and the second is not. (102-4)

Now, note that, characterized this way, repetition is a *causal* relation.[[12]](#footnote-12) Indeed, here is what happens in the case of the first subject, but not in the case of the second, according to Kaplan:

There is a physical transmission of my output utterance, which is the subject’s input utterance, to the subject’s black box, then there is a psycho-physical (or, better, a physico-psychological) transition where those sound waves hit the ear and something goes up and is put into what we call memory, and then, after five seconds, *it* (that something) is called out of memory, goes through a psycho-physical transition in the vicinity of the mouth and throat and the output utterance appears. (103)

Obviously, the notion of repetition has to be couched in a way that allows for repetitions taking much longer than five seconds. What is fundamental here is storage into *memory* (see also Kaplan 2011: 513). Unfortunately, things do not always proceed smoothly, as Kaplan acknowledges:

When the word goes through the black box, when the word is received from one person and stored for passage on to the next person, it isn’t, of course, put into the pocket in the way in which a coin can be stored in its passage from person to person. The coin is put into the pocket and there it is located. There is a definite answer, whether we know what it is or not, as to whether the lucky coin that your coach gave you is really the very one that his coach gave him .... In the case of the word, we feel that the comparable question doesn’t have the same very straightforward answer, because it isn’t put into the pocket, it is put into memory.... This form of storage, *in the mind* (rather than in the pocket), makes the continuity much harder to trace. (Kaplan 1990: 106)

One problematic case occurs when someone, “taking two names to be one,” links and stores them “in a single location” (109). Concerning this, Kaplan writes:

[This] error is a short circuit – two different circuits got wired together – zap, the whole thing goes up in smoke. I am inclined to think that when two different common currency words are wired together in this way in a given black box, which then pulls from that common source and transmits, nothing whatsoever is being said. Is it transmitting the first word? Is it transmitting the second word? I think there is just no answer to that question. The two words have been co-mingled in such a way that there is just no answer. (109)[[13]](#footnote-13)

These problems notwithstanding, I claim that, if we take the relation of repetition to be transitive (so that if the linguistic particular *a* is a repetition of the linguistic particular *b* and *b* is a repetition of *c*, *a* is a repetition of *c*), we have found precisely what Kripke was searching for: which, among the many causal chains reaching from a speaker’s use of a name to objects, makes it the case that the linguistic particular produced refers to the object at its end. So, if we couple an account of repetition with an account of introduction, we arrive at what we were aiming at, a theory of the reference of proper names. Furthermore, note that Kaplan’s outline of what repetitions are does not appeal to any intentional property of mental states.[[14]](#footnote-14) In the end, repetition seems to be a matter of *mechanics*: parrots repeat, and repeating machines can be manufactured. As we saw a moment ago, the mechanisms involved in repetitions produced by human beings are rather complex, and for a better understanding of them we may have to wait for developments in psychology and neurophysiology. This, however, should not bother us too much, as this is what almost invariably happens with notions appealed to in causal theories that are not expressed in the language of physics. So, for now we can leave it at that and note that, if a characterization of introductions can be found that does not appeal to any intentional property of mental states either, the resulting theory of reference satisfies my desideratum. Let me restate it:

(2) ∀x ∀y (x refers to y ↔ ∃z (x is a repetition of z ∧ z was introduced for y)).

Is this theory true? It seems to me to work quite well for most of those linguistic particulars that may be seen as proper name tokens. Take my initial question: What made it the case that the sound with which my earlier utterance of the sentence “Giulia is not in this room” began referred to Giulia? Well, precisely the fact that that sound was a repetition of a linguistic particular that had been introduced, more than trwenty-five years ago, for Giulia.

Let me mention now a couple of non-standard cases that may seem problematic but can easily be accommodated by my theory.

First, what about those linguistic particulars that refer but do not seem to be repetitions? I have in mind here the case of the first use of an expression as a proper name whose reference is fixed not by a previous stipulation but precisely by that use (as often happens with nicknames). Perhaps, as I have already suggested, referential tokens of definite descriptions may also be seen as falling within this category. Here, the obvious solution is to change the open formula on the right side of the biconditional to a disjunction. My theory thus becomes:

(3) ∀x ∀y (x refers to y ↔ (x is introduced for y ∨ ∃z (x is a repetition of z ∧ z was introduced for y))).

Second, what about those linguistic particulars that seem to be repetitions but seem not to co-refer with what they seem to repeat? I have in mind here cases such as the one that directly motivates Kripke’s appeal to intentions. We have already encountered the relevant passage:

When the name is ‘passed from link to link’, the receiver of the name must, I think, intend when he learns it to use it with the same reference as the man from whom he heard it. If I hear the name ‘Napoleon’ and decide it would be a nice name for my pet aardvark, I do not satisfy this condition. (Kripke 1972: 96)

In a footnote, Kripke adds:

I can transmit the name of the aardvark to other people. For each of these people, as for me, there will be a certain sort of causal or historical connection between my use of the name and the Emperor of the French, but not one of the required type. (96 n. 43)

But to account for this case, it is certainly not necessary to call intentions into the picture. Whoever names his aardvark “Napoleon” introduces a linguistic particular for the aardvark. Now, the following seems to me to be a very plausible principle: a linguistic particular does not count as a repetition of another linguistic particular if it counts as an introduction. To put it in Kaplan’s terms (1989: 602), we can play the role of “language *consumers*” (in which case we are repeating) and we can play the role of “language *creators*” (in which case we are introducing), but we cannot play both at once. If this is so, the fact that “the causal or historical connection” between Kripke’s (fictitious) use of “Napoleon” and the Emperor of the French is not of the “required type” is easily explained: the linguistic particular purportedly produced by Kripke is not a repetition of one that was introduced for the Emperor of the French.

Now, on to the major problem for a theory like the one I have outlined. As a matter of fact, the problem is not at all new. It was highlighted more than forty years ago by Gareth Evans in arguing against “those who, ignoring Kripke’s explicit remarks to the contrary, supposed that the Causal Theory could provide them with a totally *non-intentional* answer to the problem posed by names” (1973: 24).[[15]](#footnote-15) I do not know whom exactly Evans had in mind, but certainly his simple observation constitutes a serious challenge to my theory:

Change of denotation is … decisive against the Causal Theory of Names. Not only are changes of denotation imaginable, but it appears that they actually occur. We learn from Isaac Taylor’s *Names and their History* (1898): “In the case of ‘Madagascar’ a hearsay report of Malay or Arab sailors misunderstood by Marco Polo … has had the effect of transferring a corrupt form of the name of a portion of the African mainland to the great African Island.” A simple imaginary case would be this: two babies are born, and their mothers bestow names upon them. A nurse inadvertently switches them and the error is never discovered. It will henceforth undeniably be the case that the man universally known as ‘Jack’ is so called because a woman dubbed some other baby with the name. (11)

The relation between this observation and my theory can be represented as a trilemma: either (a) my theory is false, or (b) the observation is false, or (c) contrary to all appearances, the token I produce of the name “Madagascar” does not count as a repetition of tokens of it preceding Marco Polo’s travels. So, if I want to avoid (a), I have to go either for (b) or for (c). But (b) does not seem to be a viable option (it would have the wildly implausible consequence that the token of “Madagascar” I produce refers to a portion of the African mainland, or that no tokens of “Madagascar” ever referred to a portion of the African mainland). Hence, we are left with (c), but then, either (c1) the token I produce of the name “Madagascar” does not count as a repetition of the tokens of it produced by Marco Polo, or (c2) the latter do not count as a repetition of the tokens of it produced by whoever passed the name to him. In either case, the only not completely *ad hoc* way to defend the idea that we are not faced with a repetition is to appeal to the principle that I have already mentioned: a linguistic particular does not count as a repetition of another linguistic particular if it counts as an introduction. Therefore, we should probably count one of the tokens produced by Marco Polo as an introduction. But note how different this case is from the case of “Napoleon.” If in Kripke’s fictitious case repetition is out of the question, here Marco Polo takes himself to be just repeating what he received. If he made an introduction, he did so quite without it being reflected in his “psycho-physical transitions,” to use Kaplan’s terminology. Probably, what we should say here is that something is a repetition if it is the result of given psycho-physical transitions, *unless* something else happens. We may call this something else an *introduction*, if we want. This, however, would require an understanding of introduction that is deeply different from the quite naïve understanding that I – but concerning this I believe that I am in good company – have had till now.

I want, however, to close on a note of optimism. While these last considerations show that we do not yet possess a satisfying theory of reference, they may help us to see where the problem really lies, and hence to reshape the research. In fact, all those who find Kripke’s answer to the metasemantic question I started out from convincing should focus on reference fixing, not on reference borrowing.[[16]](#footnote-16) Once we have an account of the former, extending it to a full theory of reference is much less difficult than is often supposed, or so it seems to me. If I am right, repetition is the key notion.[[17]](#footnote-17)

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1. I made this point, incidentally, in a survey I wrote on reference (Bianchi 2011: 264). See Michael Devitt’s contribution to this volume for a reaction. As a matter of fact, I find it rather surprising that, despite the enormous success of Kripke’s picture, the task of giving “more exact conditions for reference to take place” has not received the attention it deserved in the philosophical community. As far as I know, Devitt is the only person who seriously tried to develop Kripke’s picture into a theory (see Devitt 1974 and especially 1981: chs. 2 and 5). However, Devitt’s way of filling in some of the details is somewhat unsatisfying. See McKinsey 1976 for an early, in my opinion quite convincing, criticism. This is not the place to discuss the issue, but let me note that, in a later book written together with Kim Sterelny, Devitt himself hedged (mainly because of what he calls the “*qua*-problem”): “We rather doubt that the sort of pure-causal theory we have presented so far can supply the ultimate explanation of reference that we need” (Devitt and Sterelny 1999: 93; see also p. 61). But see his contribution to this volume for improvements. [↑](#footnote-ref-1)
2. I am not suggesting that Kripke thinks otherwise. But if he does not, then his mention of concepts and philosophical analysis to justify his (perhaps not too serious) skepticism is rather unfortunate. [↑](#footnote-ref-2)
3. On this, see Bianchi (2005, 2007). [↑](#footnote-ref-3)
4. This is not entirely correct. Since the intentional properties ofperceptual states can certainly not be explained in terms of the semantic properties of linguistic expressions, one is entitled to appeal to them to explain the latter. Moreover, one could explain the intentional properties of *some* non-perceptual mental states in terms of the semantic properties of *some* linguistic expressions, then explain the semantic properties of some *other* linguistic expressions in terms of the intentional properties of those mental states. All this, however, is irrelevant to what follows. [↑](#footnote-ref-4)
5. For some further considerations concerning this, see Bianchi (2012: 86-8). [↑](#footnote-ref-5)
6. I elaborate on this in my “Speaker’s Reference and Semantic Reference: A Theoretically Useful Distinction?,” in progress. [↑](#footnote-ref-6)
7. To understand what follows, one should keep this point in mind. Later, for example, I will suggest that parrots repeat, and among the things they repeat are referring linguistic particulars. Hence, my theory commits me to saying that sometimes they produce linguistic particulars that refer. It is important to realize, however, that it does not commit me to saying that parrots themselves refer. Probably, they don’t: they seem to lack the cognitive structures that are required to accomplish an act of this nature. [↑](#footnote-ref-7)
8. As an aside, let me note that the beginning of the “George Smith” passage is also rather perplexing. Why on earth should the conditions for reference to take place be “different in the case of a famous man and one who isn’t so famous”? For some interesting considerations concerning this, see Holliday (ms). [↑](#footnote-ref-8)
9. Let me be clear, however, about my use of “introduction,” which might lead to some misunderstandings. In fact, I non-standardly take a linguistic particular rather than an expression type to be the first relatum of the relation. Moreover, I do not take “x is introduced for y” as implying that a new expression type has been created (if this were the case, certainly my theory would not be able to cover the case of indexicals and demonstratives) but only that a new semantic relation has been instituted (reference has been *fixed*, in Kripke’s, and Devitt and Sterelny’s, terminology). Basically, a linguistic particular is introduced for something if and only if it refers to it and is not a repetition of any linguistic particular that refers to it. But, obviously, introductions should be accounted for without appealing to reference and, for reasons that will become clear at the end of this chapter, to repetition. [↑](#footnote-ref-9)
10. On this, see also Martí’s contribution to this volume. [↑](#footnote-ref-10)
11. Kaplan’s answer to his question has been criticized by Herman Cappelen (1999) and by John Hawthorne and Ernest Lepore (2011) among others. I need not and will not take a stand on this here. Since as far as I can see my theory does not presuppose *any* answer to Kaplan’s question, I prefer to steer away from issues concerning the individuation of words. This also explains why I am putting things in terms of linguistic particulars rather than tokens or occurrences of expressions. [↑](#footnote-ref-11)
12. *Contra* Kaplan (2011: 514) (“[T]he continuity involves mental activities that I am methodologically reluctant to characterize as causally determined.... The important thing is that there *be* a link, not that it is causally determined. So, a chain? Yes. A continuous chain with active and inactive links? Yes. A causal chain? Who knows!”). [↑](#footnote-ref-12)
13. From my perspective, this is a case where reference is *indeterminate*. [↑](#footnote-ref-13)
14. Admittedly, this is at odds with some influential reconstructions of Kaplan’s account. Cappelen, for example, calls Kaplan’s “an *intentional* theory of words” (1999: 92) and claims that “[a]ccording to Kaplan, repetition is a *purely intentional notion*,” since “[w]hether someone repeated depends *only* on the intentional states of that person” (93). And Hawthorne and Lepore write that “Kaplan stresses ... the importance of *intentional repetition*: what is important is that a speaker intends to *repeat* a word” and ascribe to him the following principle: “*Constitutive Role of Intention*: If someone intends to produce the same word *w* as that used in a particular performance, then whatever comes out of his mouth (or from his pen) is a performance of *w*” (2011: 460). See also Bromberger (2011: 488). Recently, in his rejoinder to Hawthorne and Lepore, Kaplan himself gave some support to these reconstructions (Kaplan 2011: 520-1 and 526-7). Now, it is true that at one point Kaplan in his original article mentions intentions and seems to assign them an important role:

The identification of a word uttered or inscribed with one heard or read is not a matter of resemblance between the two physical embodiments (the two utterances, the two inscriptions, or the one utterance and one inscription). Rather it is a matter of intrapersonal continuity, a matter of intention: Was it *repetition*? We depend heavily on resemblance between utterances and inscriptions ... in order to divine these critical intentions. If it sounds like “duck”, it probably is “duck”. But we also take account of accent and idiolect and all the usual clues to intention. It is the latter that decides the matter. (1990: 104)

This late appearance of intentions in Kaplan’s account, however, seems to me rather unfortunate. In fact, the relevant contrast is the one between resemblance and *intrapersonal continuity*, and intrapersonal continuity is a matter of *causal connection* (“psycho-physical transition”) rather than intention, as all the discussion preceding this passage – discussion that is in fact totally ignored both by Cappelen and by Hawthorne and Lepore – made clear. Note, moreover, that the relevant processes were described by Kaplan at a *subpersonal* level (he writes: “I’m imagining an analysis in the language of psychology, not brain physiology; an analysis in programming, software terms, not in electronic circuitry, hardware terms” (106)). This is as it should be. So, I suggest that we had better intentionally ignore the above passage on intentions (whatever Kaplan’s intentions were). [↑](#footnote-ref-14)
15. See also McKinsey (1976: 235-7). [↑](#footnote-ref-15)
16. For some converging remarks, see Hinchliff (2012). [↑](#footnote-ref-16)
17. I presented versions of this chapter at two conferences in Parma (*Reference*, September 2010, and *Reference and Proper Names*, February 2012) and at one in Dubrovnik (*Mental Phenomena: Philosophy of Linguistics*, September 2012). I am grateful to all those who gave me feedback on those occasions, and especially to my discussant in the second conference in Parma, Giuseppe Spolaore, who provided a lot of good insights. I would also like to thank Joseph Almog, Antonio Capuano, Luca De Ioanna, Michael Devitt, Wolfgang Huemer, Elisabetta Lalumera, Paolo Leonardi, Ernesto Napoli, Paul Nichols, Jessica Pepp, Jessica Rett, Daniele Sgaravatti, and two anonymous referees for Oxford University Press for their comments on earlier drafts, and Marco Santambrogio for joint teaching on the topic. [↑](#footnote-ref-17)