

Reflections on *Naming and Necessity*

by

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Abstract: This paper celebrates the fortieth anniversary of *Naming and Necessity* by gathering together some reflections I have made on that work over the years. The paper focuses on aspects of the book that I think have been misunderstood, overlooked, mistakenly rejected, or simply merit more emphasis. The paper starts with the philosophy of language, first discussing Kripke's most powerful argument against description theories of reference: the "ignorance and error" argument. It then considers Kripke's causal-historical "better picture"; his discussion of rigidity; and finally, "direct reference" an implausible doctrine that has been wrongly attributed to Kripke. The rest of the paper is concerned with essentialist doctrines that Kripke has urged about biological kinds, chemical kinds, and individuals. These doctrines have been trenchantly criticized by some philosophers of science. I think that the relevant sciences support Kripke and do not support the critics.

Keywords: description theories, causal-historical theories, rigidity, direct reference, biological essentialism, chemical essentialism, individual essentialism

1. Introduction

This paper celebrates the fortieth anniversary of Saul Kripke's now-classic book, *Naming and Necessity* (1980).¹ But it is more than 50 years since I first experienced the ideas in that book. It was September 1967, and I had just arrived at Harvard University in the PhD program. I audited a small undergraduate class given by Kripke in which these ideas were presented for the first time.² A later version of most of the ideas were delivered as lectures at Princeton University in 1970, published as the paper, "Naming and Necessity" (1972), and finally published as the book of that name.

These lectures – so clear, so brilliant – had a profound effect on me. In particular, Kripke's view of proper names was the main inspiration for my dissertation, "The Semantics of Proper Names: A Causal Theory" (1972).³ In that work, and

1 The paper was commissioned by Kepa Korta to be translated into Basque by him and then appear in *Gogoa*. It appears here in English for the first time.

2 About half those attending were graduate students. My notes of these lectures are in the archives at the Saul Kripke Center.

3 Bianchi (2020a) argues that my theory of names "should ... be considered as a development of the alternative causal picture offered by Donnellan" rather than of that by Kripke. I do not think so (2020: 19.2.3). Certainly, it was Kripke who *influenced* my theory: my dissertation was virtually complete before I read Donnellan's paper (1972) on proper names. But my view of definite descriptions (1972; 1974; 1981b; 2004) was very influenced by Donnellan (1966, 1968).

later ones (1974, 1981a, 1981b, 2015), I tried to develop a “naturalistic” causal–historical theory of singular reference. Now, of course, Kripke is no naturalist: he is a great “armchair” philosopher, famous for relying on his intuitions. Yet, from my naturalistic perspective, he got so much right! That is an implicit theme of this paper. I have often thought that Kripke has “privileged access to reality.”

In this paper, I shall gather together some reflections on *Naming and Necessity* that have mostly been scattered through my work.⁴ I shall focus on aspects of the book that I think have been misunderstood, overlooked, mistakenly rejected, or simply merit more emphasis.⁵ In section 2, I shall discuss Kripke’s most powerful argument against description theories of reference: the “ignorance and error” argument. In section 3, I turn to the causal–historical alternative and reference borrowing. In section 4, I consider rigidity and description theories, and in section 5, direct reference. These sections are thus primarily about the philosophy of language. The remaining ones are about essentialism: biological kind essentialism in section 6, chemical kind in section 7, and individual in section 8.

2. Description Theories and Ignorance and Error

It is common to think that a term’s referential relation to reality is the core of its meaning. In the case of a proper name, the relation in question is to a certain object. This raises the fundamental question: *In virtue of what* does the name refer to that object? A theory of reference attempts to answer this question. Until the 1970s, all popular answers were description theories of one sort or another. Thus, according to the “classical” Frege–Russell theory, the reference of a name is determined by a description associated with it by competent speakers; for example, in the case of “Gödel” by the description, “the person who proved the incompleteness of arithmetic”; in the case of “Jonah” by one capturing the high points of the biblical story of Jonah. Kripke made seemingly devastating criticisms of a variety of description theories, including the classical one. And he proposed an alternative “better picture” (94)⁶ according to which the reference of a name is determined by a series of “reference borrowings” stretching back to the name’s introduction; reference determination is a “causal-historical” matter. Thus began the revolution in the theory of reference.

4 They have mostly appeared as passing comments on Kripke – a footnote or paragraph or two – in 15 or so works that had other concerns. Thus, the main novelty of this paper lies in gathering these comments together in one place as reflections on *Naming and Necessity*.

5 So I shall not be discussing Kripke’s vivid demonstration of the importance of distinguishing what is necessary from what is (allegedly) known a priori. I take it that this importance is now generally accepted.

6 All such unexplained citations are to *Naming and Necessity*.

Kripke's refutation of description theories of names consists in three arguments which Kim Sterelny and I, in our textbook, called "Unwanted Necessity," "Lost Rigidity," and "Ignorance and Error" (1999, pp. 48–59). Kripke's Ignorance and Error argument, and a similar argument by Donnellan (1972), were totally novel. Kripke's Lost Rigidity argument was near enough so.⁷ Immediate responses to Kripke's refutation focused on Lost Rigidity, but it always seemed to me that Ignorance and Error was the really powerful argument. Indeed, it is as close to a "knock down" argument as can be found in philosophy, and one of Kripke's two enormous contributions to the theory of language. I shall start with it. I shall have some comments on Lost Rigidity in section 4. I will mention Unwanted Necessity only in passing.

Description theories are naturally construed strongly as theories of meaning: the *meaning* of a proper name is expressed by the description that competent speakers associate with the name, a description that *identifies* the referent. A weaker construal makes no claim about meaning: the associated description simply identifies the referent. Kripke showed, with a range of *actual* cases, that most users of a lot of names are not able to supply descriptions that are adequate to identify what are intuitively the referents. Associated descriptions thus do not determine the referent of a name. Therefore, they do not express a meaning that determines the referent. Thus, most users of the names of Cicero, Catiline, Feynman, and Einstein are too ignorant to give identifying descriptions of these people (80–3). Furthermore, many users of the names of Peano, Einstein, and Columbus associate descriptions that are false of those people (84–5). Yet, Kripke's intuitions are that these users nonetheless succeed in designating these people with their names.

It is important to note that these cases are *humdrum*: they are not counterfactual, hypothetical, fictional, or in the least bit fanciful (as is, e.g., Kripke's Gödel case [83–84]⁸ and, even more so, Hilary Putnam's "Twin Earth" [1973, 1975]). Inspired by them, it is *very easy* to come up with countless cases, each just as humdrum and each yielding the intuition that a speaker's use of a name

7 The Unwanted Necessity argument against the modern "cluster" description theory was novel but the one against the classical theory was already familiar and was one of the motivations for the cluster theory; see Searle (1958). The familiar problem for the classical theory was that any single description we associate with, say, "Aristotle" picks out a property that Aristotle might not have had. Kripke's novel point against the cluster theory was: "Most of the things commonly attributed to Aristotle are things that Aristotle might not have done" (61).

8 So it is a pity that Kripke's critics, for example Machery et al. (2004), have tended to focus on the Gödel case: "intuitions about the humdrum cases play a much more important role" in the Ignorance and Error argument (Devitt, 2011b, p. 421).

designates an object despite the speaker's ignorance or error about the object.⁹ These intuitions are very powerful because *to reject them is to rule that many names out of just about every mouth fail to designate what they should*. And, so far as I know, no philosopher has rejected them.¹⁰

This is not to say that philosophers gave up on description theories of names in the face of this argument. Many tried to come up with novel description theories that avoided Ignorance and Error. Raatikainen (2020) has recently given a critical summary of some these, including this most notable one:

“[C]ausal descriptivism” favored by Lewis (1984), Kroon (1987), and Jackson (1998) ... speakers associate with a name “*N*” a description of the form “The entity standing in relation *R* to my current use of the name ‘*N*,’” and this description determines the reference of ‘*N*.’ The relation *R* here is drawn from the rival non-descriptivist (e.g., the causal-historical chain picture) theory of reference. (2020: 90)¹¹

The theory is “ingenious, but fishy” (Devitt and Sterelny, 1999, p. 61), not least because it is parasitic on the causal–historical theory.

One objection to the theory is that it requires every competent speaker to associate “*N*” with the above “*R*-description” and hence, in effect, to know that “*N*” stands in relation *R* to the referent, where *R* is the relation specified by the causal-historical theory. Note that causal descriptivism does not require that speakers know *that very theory*, know that it is *in virtue of* such associations/knowledge that “*N*” refers to its referent, but it does require that speakers have the knowledge on which the theory is built. This is highly implausible given that not even *causal theorists* should be so immodest as to suppose that they have discovered the *R* that determines reference! Although causal descriptivism was designed to avoid ignorance and error problems, it has raised a big one.¹²

Kripke would surely raise this objection to causal descriptivism. And he might well approve of the following one which strikes at the parasitic heart of the theory (Devitt and Sterelny, 1999, p. 61). According to causal descriptivism, reference is determined by the *speakers' association of the R-description with “N.”* But that association must be redundant. In claiming that the referent of “*N*” is the object the *R*-description identifies, the causal descriptivist accepts that “*N*” stands in the

9 Donnellan has a nice example that shows just how easy it is to come up with humdrum cases. A child is gotten up from sleep at a party and introduced to a person called “Tom”. “Later the child says to his parents, ‘Tom is a nice man’ ... nothing the child possesses in the way of descriptions, dispositions to recognize, serves to pick out in the standard way anybody uniquely” (1972, p. 364). Yet the child is talking about that very person he was introduced to.

10 And these intuitions have recently been confirmed in experimental tests of usage; see Domaneschi et al. (2017), Devitt and Porot (2018).

11 I first heard of causal descriptivism from Robert Nozick at Harvard in 1970 in response to my graduate student talk proposing a causal theory. Kripke (88 n. 38) also first heard the theory from Nozick.

12 Jackson (2020) is unimpressed with this objection. I have responded (2020: 19.2.5.1).

distinctive relation R to that referent. Then R alone is sufficient to explain reference. Requiring the speaker to associate the R -description does no theoretical work.

The power and reach of Ignorance and Error arguments is great and insufficiently recognized. *Any* description theory of *any* term faces the Ignorance and Error challenge. This is not to say that all such theories *fail* the challenge. For example, maybe a description theory of the social-kind term “bachelor” does not fail: perhaps all competent speakers do associate “bachelor” with “adult unmarried male.” But what about “artifactual”-kind terms like “computer”? Can the folk accurately describe a computer? And Kripke’s famous arguments against description theories of many “natural” kind terms like “tiger” and “gold” (116–43, 156–7), arguments that are more Unwanted Necessity than Ignorance and Error, show that those theories fail the challenge. Consider also Putnam’s (1975) lovely example of “elm”: most apparently competent users of this term could not uniquely describe elms or pick one out in a crowd of trees.

Ignorance and Error reaches even to Paul Horwich’s “use theory of meaning” (1998), inspired by Wittgenstein. The very same arguments that show that competent users of many terms are often ignorant or wrong about the referent show that the uses of those terms vary greatly in the speech community. Horwich neatly describes the problem for his use theory: “members of a linguistic community typically mean exactly the same as one another by a given word, even when their uses of it diverge” (pp. 85–6). He has tried to solve the problem by appealing to “deference” (1998, 2005, 2020) but, I have argued, this solution fails. Indeed, Ignorance and Error counts against the very idea of a use theory of meaning (2002, 2011a, 2020: 19.3.5).

I have also used Ignorance and Error (1985, pp. 225–7) against Gareth Evans’ “Russell’s Principle” (1982, pp. 69–79), according to which to “think about an object, one must *know which* object is in question (p. 65); and (2004, pp. 297–303) against Russellian attempts to handle “incomplete” definite descriptions like “the table.” Furthermore, Ignorance and Error is seriously detrimental to the thriving field of “lexical semantics,” yet it seems to have gone unnoticed there for 50 years.¹³ Finally, in the face of Ignorance and Error (and reference borrowing below), it is time to abandon the still-common assumption that competent users of an expression must *know about* its meaning.

3. The Causal-Historical “Picture”

“Deference,” as Horwich and many others understand it, is a bastardized version of Kripke’s other enormous contribution to the theory of language: his positive

13 Thus, Ignorance and Error, along with Kripke, Donnellan, and Putnam, go unmentioned in the comprehensive discussion of the lexicon in Pustejovsky (1995).

theory of reference borrowing. How can people who are so ignorant and wrong about the reference of names, and perhaps many other terms, nonetheless succeed in using them to refer? Kripke provided a large part of the answer: people “borrow” the reference of the terms in communication situations: reference determination is a *social rather than individual* matter. And borrowing is an epistemically undemanding causal process that does not require any capacity to identify the referent. This radical idea is the crux of Kripke’s “better picture” of reference (94).

The bastardized version, deference, arose from a common misunderstanding of Kripke’s proposal. Kripke claims that a person in borrowing a name “must ... intend when he learns it to use it with the same reference” (96). So Kripke is saying that a borrower must *at the time of borrowing* intend to use the name with the same reference as the lender.¹⁴ Yet, this claim is standardly misunderstood as requiring that the borrower, *whenever using the name thereafter*, must intend to use it with the same reference as the lender. Thus, Searle (1983, p. 234) quotes Kripke’s claim but then immediately misinterprets it as follows:

[E]ach speaker must intend to refer to the same object as the person from whom he learned the name. So this does give us some Intentional content associated with each use of a name “N” in the causal chain, viz., “N is the object referred to by the person from whom I got the name.” (pp. 235-36)

Searle thus takes Kripke to require a speaker, *at the time of using* a name, to have an intention directed at the reference lender,¹⁵ and that is what people typically have in mind in talking of the speaker “deferring” to the lender. But Kripke does not require such a “backward-looking” intention at the time of use. Indeed, such a requirement is quite alien to his discussion and the contrast he draws between his and Strawson’s (1959) *description* theory of reference borrowing. Strawson’s theory fails precisely *because* it requires competent users of names to have knowledge of reference lenders (92–93).¹⁶

14 I urge (1974; 1981a; 2015) a more brute-causal and less intellectualized account of this borrowing.

15 The first example of this misunderstanding that I know of is by Dummett (1973, pp. 147–51).

16 Keith Donnellan’s proposal, similar to Kripke’s (*pace* Bianchi and Bonanini, 2014), does not require a backward-looking intention either. Did any “causal theorist” require it? (1) Perhaps Putnam did in his discussion of the “division of linguistic labor”. His introduction of this idea does not suggest the requirement (1973, p. 705) but his later discussions might be seen to: “my denotation may be, by general consent, the denotation assigned by persons distant from me in space and even in time, but linked to me by relations of cooperation”; he thinks he is thus “giving up my right to be the authority on the denotation of my own words” (1975, p. 274); “speakers defer to experts for the fixing of reference in a huge number of cases” (1978, p. 114). (2) It rather looks as if Kaplan did. He talks of the (non-creative) use of a proper name being associated with “the intention to use the word with the meaning given it by the person from whom you learned the word.” His subsequent talk of “one who uses a proper name with [this] intention” (1989a, p. 559) implies that the intention accompanies each use rather than simply being present at the time of learning.

The successful use of a name that has been acquired by reference borrowing is a use that is caused by an ability with that name that is, *as a matter of fact*, grounded in the bearer via that borrowing: *the efficacious ability must have the right sort of causal history*. That is all that is required. Contrary to what Blackburn (1988, p. 184) claims, we need not require a borrower to recognize or acknowledge this history. And contrary to what Mercier (1999) claims, we need not require that a borrower's successful use of a name be accompanied by an intention to defer.¹⁷ The speaker exploits an ability that is, *as a matter of fact*, borrowed, but the speaker need not defer to the lender. Indeed, the speaker need not know who the lender was or even that she *has* borrowed the name.¹⁸

Just how extensive is reference borrowing? Kripke took it to be a feature not only of names but of "natural" kind terms. And Putnam went even further with his "division of linguistic labor" (1973, 1975). Raatikainen believes that "any sort of term can be borrowed," even "bachelor" (2020: 77-78). I wonder. Consider "artifactual"-kind terms. It does seem plausible that terms like "sloop" and "dagger" can be borrowed. But if so, are they, like proper names, covered by a "pure-causal" theory of borrowing? Sterelny and I pointed out that maybe not (1999, pp. 93–101): although you can gain "sloop" without associating it with "boat having a single mast with a mainsail and jib," maybe you cannot gain it without associating it with "boat." And what about the more basic "artifactual" terms "boat" and "weapon"? Is it really plausible that they can be borrowed? The first problem with these questions is that we do not have strong intuitions. The second problem is that here, more than anywhere in the theory of reference, we need the support of more than intuitions. We need evidence from usage (2011b, 2012a, 2012b). One wonders what Kripke thinks of that.¹⁹

As a result of reference borrowing, we can all succeed in designating Aristotle with his name in virtue of a chain of reference borrowings that takes us back to the original users who fixed the name's reference in Aristotle. But how did the original users do that? How was reference fixed in the first place? Kripke did not say much about this in his 1967 lectures, but he seemed to be favourably disposed to a description theory of some sort: a person fixes the reference in an object by a description of the object in a dubbing. Howard Wettstein thinks this

17 Where did this talk of "deference" come from? The first such talk in this context seems to have been by Evans (1973, p. 205-6) in the process of *criticizing* the causal theory and presenting his own.

18 See Devitt (2015, p. 116-8) for a recent development of this idea.

19 The practice of basing theories of reference on the referential intuitions of philosophers has been severely criticized by some "experimental philosophers," Machery et al. (2004). This led to a lively debate including, *inter alia*: Ludwig (2007), Martí (2009, 2012), Machery et al. (2009), Weinberg et al. (2010), Devitt (2011b, 2012a, 2012b), Sytsma and Livengood (2011), Ichikawa et al. (2012), Machery and Stich (2012), Machery (2012a, 2012b), and Machery et al. (2013).

was Kripke's "paradigm" (2012, p. 115).²⁰ In his book, Kripke talks of "fixing a reference by description, or ostension" (97) but does not offer an account of ostension. Set aside "descriptive names" like "Jack the Ripper"²¹ and consider *paradigm* names like "Aristotle." In my view, the reference of a paradigm name is fixed in an object, directly or indirectly, by the causal link between a person and that object when it is the focus of that person's perception.²² This is what I call a "grounding" and was central to my attempt to "naturalize" reference (1974, 1981a, 2015).²³ So on this view of reference-fixing, the original users have their ability to designate Aristotle by "Aristotle" in virtue of their causal link to him and then we inherited this ability to designate him by reference borrowing. We should not suppose that Kripke would approve of this view of reference-fixing.

4. Description Theories and Lost Rigidity

Kripke's claim that some terms are "rigid" was another notable feature of the revolution in the theory of reference. He defines a rigid designator as one that designates the same object in every possible world in which that object exists. If a designator does not do this, then it is non-rigid (48). Kripke then argues that a proper name is rigid but descriptions, of the sort alleged by description theories to express the name's meaning, are not. For example, "Aristotle" is rigid but Frege's suggestion for its meaning, "the pupil of Plato and teacher of Alexander the Great," is not. So the name is not synonymous with a description and the description theories are wrong. That is the Lost Rigidity argument. I have several points to make about this argument.

(I) *Saving Description Theories.* As Kripke notes, the argument counts against description theories construed as theories of what a name *means*, as they naturally are, but not against ones construed as only theories of *what determines the reference* of a name. But the latter construal leaves many problems (Devitt and Sterelny, 1999, p. 53). A better way to save description theories is to modify them to accommodate Kripke's point: the meaning of a name is given by a different

20 And that is Hilary Putnam's view: "even in externalist theories of reference descriptions play a key role: the original dubber or dubbers identify or have the capacity to identify what they are talking about by definite descriptions" (2001, pp. 496–7).

21 These have to be treated differently in my view (1974, pp. 195–6; 1981a, pp. 40–1; 2015, pp. 124–5, 135–8).

22 A different story is obviously needed for the names of abstract objects like numbers.

23 Names are typically *multiply grounded* in their bearers (1974, p. 198). This enables a causal-historical theory to explain reference *change*, as in Gareth Evans' famous example of "Madagascar." In brief, the reference of a name changes from *x* to *y* when the pattern of its groundings changes from being in *x* to being in *y* (1981a, pp. 138–52; 2015, pp. 121–4). Nonetheless, Evans' mistaken idea that cases of reference change are "decisive against the Causal Theory of Names" (1973, p. 195) persists (Searle, 1983; Sullivan, 2010; Dickie, 2011).

sort of description, a *rigidified* one. Thus, instead of Frege's suggestion for the meaning of "Aristotle," the modified theory says that the meaning is given by the rigid description, "the person *in the actual world* who was the pupil of Plato and teacher of Alexander the Great."

This is an effective response to the Lost Rigidity argument. There is no such response to the Ignorance and Error argument.

(II) *Metaphysical Intuitions*. One might be tempted to think that Kripke's refutation of description theories rests on *referential* intuitions.²⁴ And this is true enough of Ignorance and Error, but it is not true of Unwanted Necessity and Lost Rigidity: these arguments rest on *metaphysical* intuitions about *modality* and *essence*; any intuitions about what "*N*" refers to in a counterfactual situation are derived from intuitions about which object is or is not *N* in that situation. This is obvious with Unwanted Necessity²⁵ but may not be with Lost Rigidity. We can distinguish four versions of that argument:

(A) Kripke claims that "although someone other than the U.S. President in 1970 might have been the U.S. President in 1970 ..., no one other than Nixon might have been Nixon" (48). This is a modal claim arising from the view that being U.S. President in 1970 is not essential to being Nixon but being Nixon is. Based on this modal intuition, Kripke claims that "Nixon" passes "the intuitive test" for rigidity, whereas "the U.S. President in 1970" fails (48).

(B) Kripke claims that since Hesperus is Phosphorus, necessarily Hesperus is Phosphorus. But substituting descriptions, say, "the planet seen in the evening" for "Hesperus" and "the planet seen in the morning" for "Phosphorus," turns this truth into a falsehood. The names, unlike the descriptions, are rigid (3–5, 98–102). This is based on modal intuitions about identities.

(C) Modal sentences like the following have a scope ambiguity:

- (a) It might have been the case that the last great philosopher of antiquity was not a philosopher.
- (b) It might have been the case that Aristotle was not a philosopher



The scope of "the last great philosopher of antiquity" in (a) and "Aristotle" in (b) can be wide or narrow. But, Kripke claims, the two readings of (b) are equivalent but those of (a) are not. This equivalence is a mark of the rigidity of "Aristotle" (10–12, 62). The argument rests on the view that being a philosopher is not an essential property of Aristotle.

²⁴ See Machery et al. (2004), for example.

²⁵ It is particularly obvious in the discussion of natural kind terms (116–43, 156–7).

(D) Finally, consider what seems to me the most important version of the Lost Rigidity argument.²⁶ It first appeared in the Preface of *Naming and Necessity*. Commenting on

(1) Aristotle was fond of dogs,

Kripke says,

Presumably everyone agrees that there is a certain man – the philosopher we call “Aristotle” – such that, as a matter of fact, (1) is true if and only if *he* was fond of dogs. The thesis of rigid designation is simply ... that the same paradigm applies to the truth conditions of (1) as it describes *counterfactual* situations. That is, (1) truly describes a counterfactual situation if and only if the same aforementioned man would have been fond of dogs, had that situation obtained. (6)

Take the counterfactual situation to be as follows. The person who actually grew up to be the famous philosopher we call “Aristotle” died as a teenager before studying any philosophy. In those circumstances, the last great philosopher of antiquity would be someone else, say Plato. Kripke’s intuition is that whether *Aristotle* was fond of dogs, *hence whether (1) is true*, depends on whether *that teenager* was fond of dogs not on whether *Plato* was. So, in that counterfactual situation, “Aristotle” in (1) refers to the teenager, who is our actual Aristotle, and not to Plato. So, “Aristotle” is rigid. Kripke’s intuition is about what is essential to Aristotle: being that teenager is essential, being the greatest philosopher of antiquity is not.²⁷

So Kripke’s views about what is and is not essential to an individual underlie the Lost Rigidity argument. We shall have more to say about individual essentialism in section 8.

(III) *Natural Kind Terms and the Work of Rigidity*. Kripke argues that certain “natural kind” general terms are like proper names. He clearly thinks that one way in which they are like is in being rigid (127–36). The terms he has in mind are

various species names, whether they are count nouns such as ‘cat’, ‘tiger’, ‘chunk of gold’, or mass terms such as ‘gold’, ‘water’, ‘iron pyrites’ ... certain term for natural phenomena, such as ‘heat’, ‘light’, ‘sound’, ‘lightening’, and, presumably, suitably elaborated, ... corresponding adjectives - ‘hot’, ‘loud’, ‘red.’ (134)

26 In discussing (A) to (D) in an earlier work, I wrongly allowed that this version (D) “*does rest on intuitions about reference*” (2011b, p. 422).

27 I agree with Kripke’s intuition that paradigm names like “Aristotle” are rigid. This rigidity can be explained by a causal theory of their reference (2000, p. 145). But what about a non-paradigm “descriptive” name like “Jack the Ripper,” where no such explanation is possible? Nathan Salmon thinks that “Kripke has persuaded the angels and all right-minded philosophers” that these are rigid too (2020: 241). I have my doubts about their rigidity (2015, pp. 136–7; 2020: 19.3.2), despite being named after a top angel.

Kripke does not offer a new definition of rigid designation for these terms, and so one might suppose that the old one should apply to them. But we wonder how it could. “Aristotle” and “the last great philosopher of antiquity” are singular terms designating a certain concrete entity and so it is easy to see how they can be rigid or nonrigid. But the terms Kripke has in mind here are not singular. What could they rigidly designate? Several philosophers proposed that these terms rigidly designate *abstract* objects of some sort, kinds, properties, or attributes.²⁸ I have criticized such views (2005), as have many others.²⁹ But I do not go along with a common theme of these criticisms, a theme vigorously urged by Schwartz (1977, 1980, 2002, 2020). According to Schwartz, an acceptable notion of rigidity must do the theoretical work of distinguishing natural kind terms from nonnatural ones like “pencil,” “hunter,” and “bachelor.” The abstract-object views fail to do this work.

I very much agree with Schwartz that a notion of rigidity must do theoretical work but that is not the work it must do. The primary work that Kripke gave to rigidity, as we have just seen and I have argued (2005), was featuring in Lost Rigidity arguments against description theories of meaning. That is the work that an acceptable notion of rigidity must do. And one problem with abstract-object views is that they fail to do that.

It is true that Kripke’s favourite examples of rigid kind terms are (arguably) natural kind terms. Still, Kripke does not claim that all natural kind terms are rigid or that all non-natural ones are non-rigid. Indeed, he thinks that “presumably, suitably elaborated ... ‘hot’, ‘loud’, ‘red’” (134) are among the rigid ones; see also the discussion of yellowness (128n). Furthermore, we should wonder how rigidity *could* have a *theoretically interesting* task of marking out the class of natural kind terms. It is trivial that a term is a natural kind term iff it refers to a natural kind. So, if we mark out the kinds, we mark out the terms; and vice versa. And we should proceed by marking out the kinds and thus the terms: we should “put metaphysics first,” as I like to say (2010). Indeed, it is somewhat preposterous to think that we should proceed in the other direction, marking out natural kinds by way of a semantic thesis about natural kind terms. I am confident Kripke would agree.

A theoretically useful notion of rigidity for natural kind terms must feature in Lost Rigidity arguments. I have argued that the following notion, *rigid application*, will do this work nicely:

28 Mondadori (1978), Donnellan (1983), Boer (1985), and LaPorte (2000). There is some support for the proposal in Kripke: he talks of “the physical property” yellowness being rigidly designated by ‘yellow’ which “in this respect resembles the natural kind terms” (128n).

29 For example, Cook (1980) and Macbeth (1995).

[A] general term ‘*F*’ is a rigid applier iff it is such that if it applies to an object in any possible world, then it applies to that object in every possible world in which the object exists. Similarly for a mass term. (2005, p. 146)

Schwartz (2002, 2020) disagrees. I have responded (2020: 19.3.3).

5. Direct Reference

Kripke showed that the meaning of a proper name could not be descriptive, but then what was it? Kripke has been widely misunderstood to endorse the answer given by “direct reference”: a name’s meaning (“semantic value,” “semantic content”) simply is its bearer. Yet, in fact, Kripke was noncommittal on the matter in the Preface (20–21) and has resisted all attempts to get him off the fence since.

Direct reference, as I am understanding it,³⁰ is simply a resurrection of the “Millian” theory. It thus faces the familiar, and apparently overwhelming, problems (e.g., identity statements, negative existence statements) that led Frege and Russell to abandon Millianism long ago in favour of description theories. Because of these problems, I did not, for many years, entertain direct reference as even a *candidate* theory of the meaning of a name. But then, by the mid-80s, direct reference’s popularity made it impossible to ignore.³¹ Indeed, by that time the received view, even among counter-revolutionaries, seemed to have become that direct reference was a *consequence* of a Kripkean nondescriptive theory of a name’s reference.³² The history of this is curious.

But before telling it, we should consider this question: Why did people, friend and foe alike, think that the revolution implies direct reference? The answer is clear: people thought there was no alternative. Yet there is one. The apparently overwhelming problems for Millianism gave Frege a good reason for holding that a name has a meaning that is its mode of referring to its bearer. He proposed that the mode is a description of the bearer. Kripke has shown that this cannot be right. Yet, a name obviously has *some* mode of referring. Kripke’s “better picture” provides the clue: a name has a causal-historical mode of some sort. So *that* is the name’s meaning. I urged this from the start (1974, p. 204).³³ It was only much later that I came to realize that this idea, so natural to me as to seem almost

30 “Direct reference” is often understood in other ways. I have summarized the various “direct reference” theories and their histories elsewhere (1989, pp. 206–12; see also 1996, p. 170n).

31 See Barwise and Perry (1983, p. 165), Almog (1984, 1985), Salmon (1986), Soames (1985, 1987), and Wettstein (1986). For criticisms, see my (1989, 1996, 2012c). For a recent exchange, see Braun (2020) and my (2020): 19.3.1. In my view, direct reference is theoretically unmotivated.

32 See, for example, McGinn (1982, p. 244), Baker (1982, p. 227), Lycan (1985), Block (1987, pp. 660, 665), Lepore and Loewer (1987, p. 60), and Wagner (1986, p. 452).

33 My (2015), pp. 130–3, is a recent elaboration.

uninteresting – the initial presentation was in one brief paragraph with no fanfare – struck many as what I later called “A Shocking Idea about Meaning” (2001). And that, I assume, is why this alternative was overlooked.³⁴ Shocking as it may be, it solves the meaning problem for names and seems to have nothing against it but a misguided attachment to traditional ways of thinking.

Now to the curious history of direct reference. An early statement of the view that direct reference was indeed part of the Kripkean revolution was by Brian Loar in a paper that was *critical* of the revolution (1976, p. 355). Loar went further, claiming that direct reference was “the chief philosophical interest of causal theories of referring” (p. 368). Yet, as I pointed out in a response (1980, pp. 272–73), direct reference is not to be found in the only papers that Loar cites, Kripke (1972) and Donnellan (1972). And we should note that Schwartz, writing at roughly the same time as Loar, lists the “main features” of what he calls “the new theory” of reference without even mentioning direct reference (1977, pp. 20–34). Loar’s claim led me to say that direct reference was “too implausible to be attributed to anyone without evidence” (1980, p. 273). Other *critics* of the revolution, Schiffer (1979) and Ackerman (1979), soon joined Loar in misattributing direct reference to Kripke.³⁵ But they are on much firmer footing in finding the view in Donnellan (1974).³⁶ Schiffer also claims that David Kaplan’s famous “Dthat” (1978) “clearly implies” the view (1979, p. 73 n. 5). I see no such clear implication in “Dthat” (but see below for more on Kaplan). Was there anyone back in the 1970s, other perhaps than Donnellan, who was led by the revolution to *embrace* direct reference?³⁷ The only publication I could find is by Tye (1978). A little later, there was one by McKay (1981). Both, like the critics, misattribute the view to Kripke (Tye, 1978, p. 220; McKay, 1981, p. 301n).³⁸ And McKay claims that the view “has quickly become a commonplace” (p. 287).

34 The reaction to this alternative from the two “hard men” of direct reference is interesting: Salmon describes it as “ill conceived if not downright desperate ... wildly bizarre ... a confusion, on the order of a category mistake” (1986, pp. 70–1); Soames (2002) simply pretends that it does not exist.

35 Schiffer claims, without argument, that it is “difficult, if not impossible, to see how [Kripke] could reject” direct reference (1979, p. 63). Ackerman cites two passages to support her attribution to Kripke, while allowing that it is “less clear-cut” than the attribution to Donnellan (1979, p. 68: n. 6). The citations are not convincing.

36 I have earlier (1989, p. 209) doubted that direct reference was to be found in Donnellan (1974), however, thanks to Andrea Bianchi, I now think I may have been wrong about this. In a later work, Donnellan certainly embraces direct reference (1989, pp. 275–76).

37 Marcus (1961) had embraced the view before the revolution.

38 The misattributions continue to this day: “But Kripke still goes a long way with Mill. For the name user down the chain, the name is fully Millian – no descriptions or modes of presentation, not even in a reference-fixing capacity.” (Wettstein, 2012, p. 117).

Thus, by the end of the 1970s direct reference was well and truly in the air despite a mostly mistaken view of where precisely it had come from.³⁹

But what about Kaplan? This is a fascinating story. Although the direct reference theory of names is not clearly in “Dthat,” it is in the influential paper “Demonstratives” (1989a) that circulated widely in the 1970s, long before it was published. This paper introduced the *term* “direct reference” but used it primarily in the discussion of indexicals and demonstratives. Only in “the most hastily written section” (p. 558) does Kaplan, somewhat tentatively, adopt the direct reference theory of names (p. 562).⁴⁰ And, in a final footnote, Kaplan briefly raises the possibility of a view with some similarities to mine (563n). Such a view is further explored in his “Afterthoughts,” although his considered position is direct reference (1989b, pp. 574–77, 598–99). Yet recently, and remarkably, Kaplan (2012) has urged much the same view as mine, and for much the same reason as I did, apparently unaware that the view has been around for forty years⁴¹ and that it is generally regarded as shocking.

6. Biological Kind Essentialism

Kripke argues that the nature or essence of some “natural” kinds, biological kinds like tigers and cats, chemical kinds like gold and water, is not constituted by the “external appearances” of members of the kind, hence not by the properties we commonly use to identify members; for example, tigers are *not essentially* large carnivorous quadrupedal felines, and so forth, as described in the OED. This metaphysical view is the main basis for Kripke’s rejection of the description theory that the reference of a “natural” kind term is determined by a description of such external appearances. What then *is* the essence of such a kind? According to Kripke, it is (at least partly) a certain underlying “internal structure” (119–121). Thus, anything that lacked the “atomic structure” of gold “would not be gold” (124).

I shall consider biological kinds in this section, chemical kinds in the next.

It would be hard to exaggerate the scorn that biologists and, particularly, philosophers of biology, have for Kripke’s view of tigers, cats, and species in general. His view is thought to smack of “Aristotelian essentialism” and reflect a

39 Thanks to Nathan Salmon for scholarly help with this paragraph.

40 I have discussed the argument that leads him to this (1989, pp. 213–15).

41 Aside from my own proposal, Panu Raatikainen has drawn my attention to a note in which Kripke briefly mentions something like this idea: “Hartry Field has proposed that, for some of the purposes of Frege’s theory, his notion of sense should be replaced by the chain which determines reference” (1972, p. 346, n. 22; the note does not re-appear in the 1980 book). (Field also attended Kripke’s 1967 lectures. In the months that followed, we talked countless times about the issues raised by the lectures. The idea of a name’s meaning as a causal mode of referring surely first emerged in those discussions.)

naive and uninformed view of biology that is incompatible with Darwinism. Thus, Michael Ruse places Kripke, along with Putnam and Wiggins who have similar essentialist views, “somewhere to the right of Aristotle” and talks of them showing “an almost proud ignorance of the organic world” (1987, p. 358n). John Dupré argues that the views of Putnam and Kripke are fatally divergent from “some actual biological facts and theories” (1981, p. 66). The view of species held by the consensus is totally at odds with Kripke’s sort of intrinsic essentialism. Samir Okasha captures the consensus well:

[V]irtually all philosophers of biology agree that ... it simply is not true that the groups of organisms that working biologists treat as con-specific share a set of common morphological, physiological or genetic traits which set them off from other species. (2002, p. 196)

On this matter, according to Sarah-Jane Leslie, “there is a degree of consensus among philosophers of biology (and indeed biologists) that is almost unprecedented in philosophy at large” (2013, p. 132).

Philosophers of biology therefore think that the nature or essence of a species like tigers is *not* something intrinsic. What then do they think it *is*? They think that the essence is relational: for an organism to be a member of a certain species, it must have a certain *history*. As Kim Sterelny and Paul Griffiths put it in their textbook, *Sex and Death*, there is “close to a consensus in thinking that species are identified by their histories” (1999, p. 8).

Now it may be that Kripke would agree to there being an historical *component* to the essence of a species as well as an intrinsic one. For, consider this passage:

Past experience has shown that usually things like [tigers], living together, looking alike, mating together, do form a kind. If there are two kinds of tigers that have something to do with each other but not as much as we thought, then maybe they form a larger biological family. If they have absolutely nothing to do with each other, then there are really two kinds of tiger. This all depends on the history and on what we actually find out. (121)

Still, Kripke’s claim that that there is an intrinsic internal-structural component to the essence is clearly inconsistent with the consensus.

Philosophers of biology think that Kripke’s intrinsic essentialism, based on “armchair” intuitions that reflect ignorance of the biological facts, are simply wrong. In contrast, I have argued, in effect, that Kripke’s intuitions are right and supported by the biological facts. The initial argument was in “Resurrecting Biological Essentialism” (2008); I argued that species (and other taxa) “have essences that are, at least partly, intrinsic underlying properties” (p. 346) These properties “are probably largely, though not entirely, genetic” (p. 347). Not surprisingly, this received criticism from the consensus (Barker, 2010; Ereshefsky, 2010; Lewens, 2012; Leslie, 2013; Slater, 2013). I have responded (2021).

My line of argument is briefly this. (1) Why do African rhinos have two horns and Indian rhinos, one? The question seeks a “structural” explanation (Kitcher, 1984) of the developmental mechanisms in these rhinos. And the answer rests ultimately on the underlying intrinsic natures or essences of these two species of rhinos. These natures *cause* these species of rhino to have their respective number of horns. (2) Why, in this group of animals, does *that one* have stripes? Because it is a zebra and it is the nature of zebras to have stripes: some underlying intrinsic property of that animal made it a zebra and caused the stripes. So, the essence or nature of a species (or other taxon) simply *is* whatever underlying state causes members of that species, in their environment, to have their phenotypic properties; the essence explains the place that those organisms have in the causal nexus just because they are members of the species; the essence is “superexplanatory,” in Mallozzi’s (2021) neat terminology.⁴² Elliott Sober rightly insists that the essence of a species must be “a causal mechanism that acts on each member of the species, making it the kind of thing that it is”; the essence must explain why the members “are the way they are” (1980, p. 250). In sum, the demands of biological explanation show that Kripke is right and the biological consensus wrong about intrinsic essentialism.

I later argued along analogous lines that there is indeed *also* a historical component to the essence of a species. But what is that component? I argue against answers suggested in the literature and for the view that the historical component *requires* an intrinsic component; the relevant history of a species is of organisms of *a certain intrinsic kind* evolving into organisms of *a certain other intrinsic kind* until we reach the species in question. This thus adds to the case for intrinsic essentialism (2018a).⁴³ Perhaps Kripke would agree. This argument attends to the demands of “historical” explanations (Kitcher, 1984) of how members of a species came to have their developmental mechanism.

7. Chemical Kind Essentialism

Paul Needham has a long-running campaign against Kripke’s and Putnam’s “microessentialism” about water; see Needham (2011) and the earlier papers cited. This campaign is based on a detailed and illuminating presentation of the scientific facts about water. Nonetheless, I do not think it succeeds. I shall consider its bearing on Kripke only.

(1) Needham is critical of the talk of “H₂O” as the essence of water:

42 See also Godman et al. (2020).

43 Godman and Papineau (2020) disagree, arguing along different lines for a purely historical essence; I have responded (2020: 19.5.2).

[T]he expression “H₂O” provides a characterisation of water without resorting to microstructure, and anyone wanting to give a microdescription of water who simply offers “H₂O” fails miserably. (2011, p. 9)

But it is central to Kripke’s position that we should look to *empirical science*, not to philosophers like himself to *discover* essences/natures:

[T]he scientists have investigated the nature of gold and have found that it is part of the very nature of this substance, so to speak, that it have the atomic number 79. (124)

scientific discoveries about what this stuff is...present scientific theory is such that it is part of the nature of gold as we have it to be an element with atomic number 79. (125)

Whether science can discover empirically that certain properties are necessary of cows, or of tigers, is another question, which I answer affirmatively. (128)

It certainly represents a discovery that water is H₂O. (128)

Therefore, Kripke’s talk of the nature of water being “H₂O,” talk which is quite standard in philosophy anyway,⁴⁴ should be seen as nothing more than a philosopher’s hand wave toward the scientific facts. Needham’s discussion is a helpful guide to what those facts may be. He is persuasive that the H₂O-talk is inadequate, but a recent paper by another philosopher of chemistry, Hendry (n.d.), suggests that it may not be.

Needham does not convince me that Kripke is wrong to think that the essence is *entirely micro*, but even if Kripke is wrong and the essence is partly, even entirely, macro, that alone would not be much of a blow to Kripke’s metaphysics. Kripke surely already accepts that many scientific kinds have macro essences; *predator* is a likely example. What *would* be a blow would be the discovery that the essence of water is constituted entirely by the macro properties *picked out by descriptions associated with the word “water” by its ordinary users*. For, *that* is the consequence of the description theory of reference that Kripke and Putnam are mainly challenging: in the clever words of Putnam’s slogan, the meaning of “water” “just ain’t in the head.” Needham does not attempt to undermine that challenge.

(2) Needham charges that “no argument has been given for the *micro-essentialist* claim that we must turn to microscopic features to obtain an adequate characterisation of a substance” (2011, p. 4). Here Needham is on firmer footing: Kripke’s essentialism rests ultimately on intuitions. It just seems intuitively clear to him – and, we should note, to many other philosophers – that the properties commonly associated with water, gold, and the like, of the sort listed in a dictionary, are not essential to them and that some “atomic” “internal structure” is (119–129). Still, if this reliance on intuition is a sin, sin is everywhere in philosophy, particularly in metaphysics. In any

44 It was common in discussions of “the identity theory” of mind, for example.

case, Kripke's intuitions can be argued for along the same lines as the argument for biological essentialism briefly described in section 6: it is in virtue of having some underlying microscopic property that water has those commonly associated properties and plays its causal role in the world; *that* property is superexplanatory. And this, in effect, is what Hendry (n.d.) *does* argue. This argument for microessentialism rests on an empirical hypothesis, of course. I think we should see such a hypothesis as underlying Kripke's essentialist intuitions.

(3) Needham claims that "a characterisation of water which would seem to be a stereotypical one is easily given by specifying its melting point" (2011, p. 6). "It is not necessary, then, to resort to microscopic features in order to obtain an adequate characterisation of water, distinguishing it from other substances" (2011, p. 7). But what Needham has in mind as a "characterization," a term that Kripke does not use, seems much weaker than what Kripke has in mind as an essence. Kripke is seeking the very nature of water, implicitly, I think, the *explanatory* property that gives water its causal role. He would not be satisfied with a property that simply distinguishes water from other stuff.

Finally, Leslie's (2013, pp. 143–52) discussion of chemical kinds reflects the influence of Needham but makes a different criticism. Kripke's discussion is all about gold and water, as her Kripke quotes, and mine above, indicate. Yet, Leslie thinks that it seems "reasonable to infer" from these passages a view of "the *same substance* relation" (p. 143), a view that she then goes on to criticize at length. But this inference is not reasonable at all. It is one thing to say what it is to be gold or water, and it is quite another to say what it is for gold or water or any other stuff to be a "substance," hence to say what it is for stuffs X and Y to be "the *same substance*." Analogously, it is one thing to say what it is to be a tiger or a cat, and it is quite another thing to say what it is for tigers or cats or any other taxon to be a "species," hence to say what it is for taxa X and Y to be "the same species."⁴⁵ I see no evidence that Kripke has taken a clear stand on the substance issue (or the species issue). Nor, contrary to another critic, Tahko (2015), do I see evidence that Kripke has taken a clear stand on what it is for water or gold to be "chemical" or "natural" kinds.

8. Individual Essentialism

What is the essence of an individual? Kripke offers an answer for biological individuals and some others. That answer – his so-called "origin essentialism" – has received much attention from metaphysicians (e.g., McGinn, 1976;

45 The former is Ernst Mayr's "taxon problem," the latter, his "category problem" (1982, pp. 253–4). Mayr's distinction is well-established and yet, as I demonstrated (2008), its significance has been standardly overlooked in dismissals of intrinsic essentialism about species.

Salmon, 1979; Forbes, 1986; Robertson, 1998). Attention from philosophers of biology has been limited to one significant aspect of Kripke's answer, "*Essential Membership*." This is the doctrine that if an individual organism *O* belongs to a taxon, particularly to a species, it does so essentially. Joseph LaPorte, a philosopher of biology, aims to approach the issue "in the light of biological systematics" and charges that "essentialists have tended to be rather naïve on scientific matters" (1997, p. 97). LaPorte's approach leads him to reject *Essential Membership*. Okasha (2002) endorses LaPorte's rejection.⁴⁶ Thus, LaPorte and Okasha both urge, *from a biological basis*, a view of what is *not* essential to *O*. But neither they nor, so far as I can discover, any other philosopher of biology or any biologist, have seriously addressed the issue, broader than *Essential Membership*, of what *is* essential to *O*. It seems that this issue, much discussed by metaphysicians, has entirely escaped the attention of philosophers of biology. I have recently given it some attention, also from a biological basis (2018b).

Considering one particular organism, Kripke famously asked: "could the Queen ... have been born of different parents from the parents from whom she actually came?" Taking her parents to be "the people whose body tissues are sources of the biological sperm and egg" that led to the Queen (112), Kripke answers in the negative: "anything coming from a different origin would not be this object" (113). He moves on to raise a similar question about a particular table: "[C]ould *this table* be made from a completely *different* block of wood, or even of water cleverly hardened into ice ...? (113). Once again he answers in the negative (114). So he is urging "the principle that the *origin* of an object is essential to it" (114 n. 57). What about the principle "that the *substance* of which it is made is essential"? Kripke accepts this principle too: the wooden table in question could not "have been made of anything other than wood" (114–5, n. 57). Finally, Kripke notes a relationship between the two principles. Supposing, as Kripke does, that it is essential to the block from which the table was made that it was a block of *wood*, then it follows from the origin principle that it is essential to the table that it is made of wood (115, n. 57).⁴⁷

According to the origin principle, it is essential to the Queen that she came from certain parents and from certain gametes. Those gametes united to form a zygote. According to the origin principle it is also essential that the Queen came from that particular zygote. What we have said about the Queen applies, of course, to her parents, their parents, and so on. Thus there is a historical

⁴⁶ I infer from the form of LaPorte's argument that he thinks that philosophers of biology up to that time had *implicitly* rejected *Essential Membership*. Since then, Griffith (1999), Levine (2001), Haber (2012), Leslie (2013), and Witteveen (2015) have done so explicitly.

⁴⁷ Kripke claims also that "(roughly) *being a table* seems to be an essential property of the table" (115 n. 57). I have argued that this cautious suggestion is mistaken (2005, p. 156).

component to *O*'s essence constituted by the actual individual people, gametes, and zygotes that make up that ancestral history, that family tree.

Kripke does not apply the substance principle to the Queen but we can do so. Just as the table must come from a block that is essentially wooden material, the Queen must come from a zygote that is essentially human material. In the spirit if not the letter of Kripke, we should surely go further with the substance of the Queen: what is essential to her zygote, hence to the Queen herself, is not simply that the zygote is constituted of human material of *some sort* but of *the particular sort in that zygote*. Therefore, we have the Kripkean proposal that the essence of *O* is partly *O*'s origin in a certain zygote formed from certain gametes, hence from certain parents, and partly intrinsic properties of *O*'s zygote.⁴⁸ So, on this proposal, the essence of *O*, like the essence of its species proposed in section 6, has both an historical component and an intrinsic one.

Combining these two proposals, one about the individual essence of *O*, one about the kind essence of its species, I have argued (2018b) for *Essential Membership* (it is not hard): having the historical and intrinsic components of the essence of *O* entails having the historical and intrinsic components of the essence of *O*'s species (2018b).

The support for Kripkean individual essentialism has come so far from intuitions. This raises a methodological question. Okasha claims that it is "widely held" that "claims about individual essence," unlike those about "kind essence," "are not responsible to empirical science"; they are matters "for the armchair metaphysician" to be handled by "consulting ... modal intuitions" (2002, p. 193). In a moment, I shall very briefly describe an argument that shows that Kripke's individual essentialism is also "responsible to empirical science." This is just as well because, as already noted, LaPorte and other philosophers of biology reject *Essential Membership* on biological grounds. Furthermore, some of Kripke's essentialist intuitions are not shared by all metaphysicians.

The influential metaphysicians, Forbes (1986) and Robertson (1998), agree only partly with Kripke about the historical component of his essentialism. On the Kripkean view I have presented, at least three relations are essential to the Queen: (a) she must come from a certain zygote; (b) that zygote must come from certain gametes; (c) those gametes must come from certain parents. Forbes and Robertson do not resist (a) but their modal intuitions count against (b) and (c). Forbes argues as follows:

Suppose *z* is a human zygote that is formed by fusion of a sperm *z* with an egg *e*. Then one can conceive that scientists synthesize a zygote by building it nucleotide by nucleotide, and happen to use exactly the actual matter of *z* in exactly its actual *z*-configuration. In such a world, *s* and *e* do

48 This is only true of sexual organisms, of course.

not exist, or so we can consistently postulate, but it is hard to deny that z exists...So z exists but does not originate from s and e , since they do not exist. (1986, p. 7)

Robertson agrees and claims that others who write on the topic do too (1998, p. 732 n. 5). I do not agree: it seems to me that the synthesized zygote is not z precisely because it lacks the right history.⁴⁹ But we need more than intuitions.

I offer arguments for Kripkean individual essentialism that are analogous to those for taxon essentialism described briefly in section 6. The historical component of O 's essence is demanded by "historical" *explanations* of O , the intrinsic component, by "structural" *explanations*. These arguments support Kripkean origin essentialism against Forbes and Robertson and, when combined with species essentialism, support *Essential Membership* against the philosophers of biology (2018b).

9. Conclusion

The early sections of the paper were concerned with *Naming and Necessity*'s discussion of the philosophy of language. In section 2, I discussed Kripke's Ignorance and Error argument against description theories. This argument is devastating for description theories of names, including causal descriptivist ones. But it challenges all description theories, the use theory of meaning, and quite a lot else. Its reach and power have not had the recognition it deserves.

In section 3, I turned to the alternative "picture," causal-historical theories. Kripke's theory of reference borrowing is his other enormous contribution to the theory of language. It is widely misunderstood as requiring a speaker, in using a term, to "defer" to the person from whom she got the term. Such an epistemically demanding "backward-looking" intention has no place in the Kripkean "picture" of reference. It remains to be seen just how widespread reference borrowing is in language.

In section 4, we noted that a description theory can avoid Kripke's Lost Rigidity argument by rigidifying its descriptions. Still, the argument is powerful against all traditional description theories. It is important to note that its intuitions about reference are derived from metaphysical intuitions about what is essential and necessary.

"Rigid" is defined for singular terms and yet Kripke applies it also to other sorts of terms. There is controversy over what notion of rigidity is appropriate for that. I have argued against Schwartz that the work we want a notion to do is not

49 This avoids what Robertson and Atkins (2016) nicely call, "The Recycling Problem": if all the matter that constitutes z were recycled into something that was qualitatively identical to z it would still not be z because it would lack the right history.


to distinguish “natural” kind terms from others but rather to feature in Lost Rigidity arguments. My notion of *rigid application* does this nicely.

Section 5 discusses direct reference. This popular but highly implausible theory that a name’s meaning is its referent is widely misattributed to Kripke who is in fact noncommittal. The reason for this misattribution seems to be the idea that there is no alternative once descriptivist views of a name’s meaning have been rejected. But there is an alternative, one that I have been urging over the years: a name’s meaning is its *causal* mode of reference. I related the history of direct reference, climaxing with the story of Kaplan. He, unlike Kripke, *is* a source of the direct reference theory of proper names and yet he has recently urged what is, in effect, my old alternative.

The final three sections addressed Kripke’s views on essentialism. These views have come in for trenchant criticism from philosophers of science. His view, discussed in section 6, that biological kinds like tigers have an essential “internal structure” has been dismissed by philosophers of biology as reflecting ignorance of biology. And his similar view, discussed in section 7, about chemical kinds like water has been similarly dismissed by Paul Needham, a philosopher of chemistry. Finally, in section 8, I considered the Kripkean view that an individual like the Queen has an essence that is partly historical, partly underlying intrinsic. When combined with Kripke’s kind essentialism, this leads to the doctrine of *Essential Membership*: if an individual organism *O* belongs to a species, it does so essentially. This puts Kripke again sharply at odds with philosophers of biology. And some metaphysicians disagree with him over the “principle of origin.”

In response to these objections to Kripkean essentialism, I have gestured toward arguments, which have mostly been made elsewhere. These arguments start from the premise that essences explain the observed properties of kinds or individuals and their places in the causal nexus. Find whatever plays that causal role and one has found the essence. My empirical hypothesis is that Kripke has gotten it pretty right about essences.⁵⁰

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