The schmidentity strategy

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November 2, 2021

This is a paper about a prominent style of argument in the philosophy of language, which Nathan Salmon has called 'the schmidentity strategy.' The style of argument has great intuitive force, and has been used to defend theses as diverse as the classical theory of the 'is' of identity, the Russellian analysis of definite descriptions, and the Millian theory of proper names. It is thus of considerable interest to determine whether the strategy is a good one, and how widely it can be employed.

1 The schmidentity strategy in action

The schmidentity strategy derives its name from the following well-known passage in *Naming and Necessity*:

> 'It is, for example, thought that if you have two names like 'Cicero' and 'Tully' and say that Cicero is Tully, you can't really be saying of the object which is both Cicero and Tully that it is identical with itself. On the contrary, 'Cicero is Tully' can express an empirical discovery . . . And so some philosophers, even Frege at one early stage of his writing, have taken identity to be a relation between names. Identity, so they say, is not the relation between an object and itself, but is the relation which holds between two names when they designate the same object. . . .

If anyone ever inclines to this particular account of identity, let's suppose we *gave* him his account. Suppose identity *were* a relation in English between the names. I shall introduce an artificial relation called 'schmidentity' (not a word of English) which I now stipulate to hold only between an object and itself. Now then the question whether Cicero is schmidentical with Tully can arise, and if it does arise the same problems will hold for this statement as were thought in the case of the original identity statement to give the belief that this was a relation between the names. If anyone thinks about this seriously, I think he will see that therefore probably his original account of identity was not necessary, and probably not possible, for the problems it was originally meant to solve, and that therefore it should be dropped, and identity should just be taken to be the relation between a thing and itself. This sort of device can be used for a number of philosophical problems.'[1]

In this passage, Kripke considers an objection to the semantic hypothesis that 'is' in sentences like 'Cicero is Tully' expresses the identity relation. The objection is that sentences of this form 'can express an empirical discovery.' Roughly, the idea is that in some cases speakers may need to resort to empirical evidence to discover whether sentences of this kind are true.

Why is this thought to be an objection to semantic hypothesis in question? The thought is that if the hypothesis were true, sentences of this form would be trivial if true, and hence would never, in the above sense, 'express an empirical discovery.' So, if the hypothesis were true, it would not be the case that speakers would sometimes need to resort to empirical evidence to discover whether such a sentence is true. But they do. So the hypothesis is false.

Kripke's response is an argument against the conditional claim about what would be the case were the hypothesis true. We begin by stipulating that 'is schmidentical' expresses the relation of identity. We then notice that, if we introduced this predicate into our language, 'the question whether Cicero is schmidentical with Tully can arise.' That is, speakers would sometimes need to resort to empirical evidence to discover whether this sentence is true. But 'is schmidentical' was stipulated to express the identity relation. So if 'is' in English sentences like 'Cicero is Tully' expressed identity, presumably the same would be true of 'Cicero is Tully.' But then the conditional claim on which the objection relies is false, and the objection fails.

Kripke lays out this form of argument more explicitly in 'Speaker's Reference and Semantic Reference':

'I propose the following test for any alleged counterexample to a linguistic proposal: If someone alleges that a certain linguistic phenomenon in English is a counterexample to a given analysis, consider a hypothetical language which (as much as possible) is like English except that the analysis is *stipulated* to be correct. Imagine such a

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[1]Kripke (1972), 108.

hypothetical language introduced into a community and spoken by
it. *If the phenomenon in question would still arise in a community
that spoke such a language (which may not be English), then the
fact that it arises in English cannot disprove the hypothesis that the
analysis is correct for English.*'[2]

In each of the cases we'll consider, we have a semantic hypothesis H and a piece
of evidence E which is proposed as an objection to H. The schmidentity strategy
is a way of defending H against the objection.

A face-value reading of the passage just quoted would suggest that an instance of the strategy would consist of a defense of the relevant instance of the
following claim:

> PREDICTION   In the nearest world in which H is stipulated to be
> true, E would be true.

The strategy also seems to assume that all instances of the following schema are
true:

> NO EVIDENCE   If in the nearest world in which H is stipulated to be
> true, E would be true, then E is not evidence against
> H.[3]

Given that NO EVIDENCE seems, at a first glance, plausible,[4] a natural thought
is that the question of when the schmidentity strategy can be employed boils
down to the question of what instances of PREDICTION are true.[5]

In the original case, we have

> H: A predicate R expresses classical identity

> E: Sentences of the form $\ulcorner N \ R \ M \urcorner$ are sometimes both true and
> such that some speakers who understand the sentence can't know
> whether the sentence is true without empirical evidence.

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[2]Kripke (1977), 265. Emphasis in original.

[3]This is closely related to what Ramachandran (1995) calls the 'counterexample principle.'

[4]It is worth noting that, even if it is plausible, NO EVIDENCE is certainly not trivial. On one
reading it is an instance of the more general claim that if $P \boxright Q$ then it is not the case that
Q is evidence against P. On a probability-raising conception of evidence this amounts to the
claim that $P \boxright Q$ entails that it is not the case that $P(Q|P) < P(Q)$. But some would reject
this sort of connection between probabilities and the truth conditions of counterfactuals. I set
worries of this general sort to the side for purposes of this paper.

[5]To be sure, there is a reading of Kripke's discussion of these topics which makes the
intended argument more ambitious than the one just outlined. In the passage quoted from
Naming and Necessity, Kripke says 'If anyone thinks about this seriously, I think he will see
that therefore probably his original account of identity was not necessary, and *probably not
possible*' (emphasis added). What does Kripke mean when he says that the original account
is, in addition to being unnecessary, 'not possible'? One reading is that he thinks that the
schmidentity example is not just a defense of the hypothesis that 'is' expresses identity against
the objection that identity sentences can be informative but also a positive argument against
the competing hypothesis that it expresses a relation between names. I set this idea to the side
here. For discussion, see the criticism of the 'schmidentity attack' in Ramachandran (1995),
§§III-IV.

For the reasons Kripke gives, the instance of PREDICTION for these values of H and E seems true.

This is, intuitively, an extremely compelling form of argument. And it would appear to have wide application. Let's briefly look at a few other well-known applications of the strategy.

The first is Kripke's defense of a Russellian theory of descriptions against an objection based on the existence of referential uses of definite descriptions. In response to this objection, Kripke has us consider

> 'a language in which sentences containing definite descriptions are taken to be abbreviations or paraphrases of their Russellian analyses: for example, 'The present king of France is bald' means ... 'Exactly one person is at present king of France, and he is bald,' or the like.
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> Would the phenomenon Donnellan adduces arise in communities that spoke these languages? Surely speakers of these languages are no more infallible than we. They too will find themselves at a party and mistakenly think someone is drinking champagne even though he is actually drinking sparkling water. ...they will say, 'The man in the comer drinking champagne is happy tonight.' They will say this precisely because they think, though erroneously, that the Russellian truth conditions are satisfied. Wouldn't we say of these speakers that they are referring to the teetotaler, under the misimpression that he is drinking champagne? And, if he is happy, are they not saying of him, truly, that he is happy? Both answers seem obviously affirmative.'[6]

Here the relevant hypothesis and evidence seem to be as follows:

> H: Russell's theory of descriptions is true.
>
> E: On some occasions in which a speaker utters a sentence ⌜The F is G⌝ we will take the speaker to have referred to o, and said something true about o, even though o is not in the extension of F.

Again, for the reasons Kripke gives, it seems plausible that the relevant instance of PREDICTION is true.[7] But then if, as seems plausible, the relevant instance

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[6]Kripke (1977), 265-6. This is what Kripke calls the 'intermediate Russell language. The differences between this language, and the weak and strong Russell languages, won't matter much for present purposes.

[7]Devitt (1981), §6 objects that this instance of PREDICTION is false:

> 'Russell English differs from English in that definite descriptions are not de- vices in it for the designational mode of identifying reference. However, other de- vices for that mode remain: demonstratives, pronouns, even names. So if the speaker has a particular object in mind (the teetotaler) and means to refer to him, he will use one of those devices, not a definite description like 'the man in the corner drinking champagne'. Speakers of Russell English will behave differently from speakers of English.' (520)

(Here 'Russell English' is a language like ours but for the fact that Russell's theory of de-

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of No Evidence is true, it follows that the existence of referential uses of descriptions is, not just weak evidence against Russell's theory, but no evidence at all.

Next, let's consider Nathan Salmon's use of the schmidentity strategy to defend the Millian theory of names against the objection that substitution of co-referring names in the complements of attitude ascriptions can seem to change the truth-value of the ascription. Salmon claims that this phenomenon

> would arise even in a language for which it was stipulated — say, by an authoritative linguistic committee that legislates the grammar and semantics of the language, and to which all speakers of the language give their cooperation and consent — that [Millianism] is correct. Suppose, for example, that such a committee decreed that there are to be two new individual constants, 'Schmesperus' and 'Schmosphorus'. (I am deliberately following the genius as closely as possible.) It is decreed that these two words are to function exactly like the mathematician's variables 'x', 'y', and 'z' as regards information value, except that they are to remain constant . . . the constant value of the first being the first heavenly body visible at dusk and the constant value of the second being the last heavenly body visible at dawn. Suppose further that some English speakers — for example, the astronomers — are aware that these two new constants are co-referential, and hence synonymous. Nevertheless, even if our character Jones were fully aware of the legislative decree in connection with 'Schmesperus' and 'Schmosphorus', he would remain ignorant of their co-reference. Jones would dissent from such queries as 'Is Schmesperus the same heavenly body as Schmosphorus?' Would those who are in the know — the astronomers — automatically regard the new constants as completely interchangeable, even in propositional-attitude attributions? Almost certainly not.

> . . . speakers who agree to abide by the legislative committee's decree about 'Schmesperus' and 'Schmosphorus' and who recognize that these two terms are co-referential — especially if these speakers do not reflect philosophically on the implications of the decree — might for independent pragmatic reasons be led to utter or to assent to such sentences as . . . 'Jones believes that Schmesperus is Schmesperus, but he does not believe that Schmesperus is Schmosphorus.' The astronomers may be led to utter the latter sentence,

scriptions is stipulated to be true.) But this argument, I think, neglects the fact that there may be pragmatic reasons for speakers of Russell English to use definite descriptions rather than these other referential devices. Suppose, for instance, that we know of no name for the teetotaler, and that our conversation is taking place after the party, when he's no longer around to be demonstrated. Then it will be very natural for a speaker of Russell English to use the definite description as a way of picking out the teetotaler, and it will be very natural to describe the speaker as referring to the teetotaler even if he is not drinking champagne. It is not hard to come up with other cases of this kind. For more discussion of Devitt's criticism, see Ramachandran (1995) and Ramachandran (1996).

for example, in order to convey (without knowing it) the complex fact about Jones that he agrees to the proposition about Venus that it is it, taking it in the way he would were it presented to him by the sentence 'Schmesperus is Schmesperus' but not taking it in the way he would were it presented to him by the sentence 'Schmesperus is Schmosphorus'. The astronomers would thus unknowingly speak in a way that conflicts with the usage to which they have agreed. This, in turn, would lead to their judging such belief attributions as 'Jones believes that Schmesperus is Schmosphorus' not only inappropriate but literally false, and to the unmistakable feeling that substitution of 'Schmosphorus' for (some occurrences of) 'Schmesperus' in such attributions as 'Jones believes that Schmesperus is Schmesperus' is logically invalid. Insofar as the same phenomena that give rise to Frege's puzzle about identity sentences and to the appearance of substitutivity failure would arise even in a language for which [Millianism] was true by fiat and unanimous consent . . . these phenomena cannot be taken to refute the theory.'[8]

The pattern is (by design, of course) just the same. The relevant instances of H and E are as follows:

H: Millianism about names, along with some compositionality assumptions, is true.

E: Competent speakers of the language sometimes judge that attitude ascriptions have different truth values despite those ascriptions differing only in the substitution of names which those speakers know to be coreferential.

Again, it seems quite plausible that the relevant instance of PREDICTION is true: as Salmon says, even speakers who were aware of the relevant linguistic decrees would exhibit much of the linguistic behavior used as evidence against Millianism. This linguistic behavior is typically regarded as the most significant argument against a Millian treatment of names. Nonetheless, if this instance of the schmidentity strategy is sound, it follows that this linguistic behavior is no evidence at all against Millianism. This is a striking result. Could it really be true that speaker intuitions about substitution failures are no evidence at all against the Millian theory of names? [9]

More recently, Salmon has deployed the same strategy to undercut the motivations for the view, which Kit Fine calls 'semantic relationism,' that sentences like 'Cicero is Cicero' and 'Cicero is Tully' differ in meaning despite 'Cicero' and 'Tully' not differing in meaning.[10] On this view, in the former type of sentence

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[8]Salmon (1990), 14-15. See also Salmon (1989), §II.

[9]To be sure, Salmon only makes the weaker claim that intuitions about substitution failures 'cannot be taken to refute' Millianiam. But, for reasons already given, it looks like the considerations Salmon gives in favor of his conclusion also support the stronger conclusion that speaker intuitions about substitution failures are no evidence at all against Millianism.

[10]Fine (2007). Related views are defended in Putnam (1953); Richard (1983); Taschek (1995). For criticism, see Salmon (2012), Salmon (2015), and Chapter 6 of Soames (2015).rama

the pair of names represent the object denoted as the same. Using Fine's terminology, let's say that sentences of this type express *coordinated* propositions whereas the latter do not. The fact that sentences like 'Cicero is Cicero' and 'Cicero is Tully' seem to differ in meaning is taken to be strong evidence against non-relationist versions of Millianism. So here we have the following values of H and E:

> H: Non-relationist Millianism is true.

> E: Competent speakers of the language judge that pairs of identity sentences of the form ⌜N is N⌝ and ⌜N is M⌝ systematically differ in meaning.

To argue that these intuitions about differences in meaning are not in fact evidence against non-relationist Millian views of names, Salmon imagines some semantically sophisticated speakers stipulating that their language should express no coordinated propositions. So, in the new language, 'Cicero is Cicero' and 'Cicero is Tully' are stipulated to express the same uncoordinated proposition.

But as Salmon points out, these linguistic stipulations will neither remove their knowledge that every name refers to the same thing as itself nor remedy their ignorance of the fact that certain pairs of distinct names refer to the same thing. So, reasoning similar to that given in the passage just quoted suggests that, even after the stipulations, there will be a felt difference in meaning between sentences like 'Cicero is Cicero' and sentences like 'Cicero is Tully.'[11] But that suggests that the relevant instance of PREDICTION is true. And that, given the relevant instance of NO EVIDENCE, implies that the central piece of evidence used to support semantic relationism against its non-relationist rivals is no evidence at all against the latter.

2   The strategy extended

We have already seen that the schmidentity strategy has wide application. But a plausible case can be made that the strategy can be extended yet further, in ways which might seem to cast some doubt on the strategy itself.

Let's consider first the linguistic hypothesis that the semantic contents of sentences, and the denotations of that-clauses, are sets of possible worlds. A very well-known objection to this view is that it makes semantic contents too coarse-grained to be the objects of propositional attitudes. One way to press the objection is to point out that speakers are often inclined to regard a pair of attitude ascriptions as differing in truth-value despite the fact that the complements of the ascriptions are true in just the same possible worlds.[12] On its face, the fact that speakers are strongly inclined to regard pairs of ascriptions of this sort as sometimes differing in truth value is evidence against the idea that

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[11]Salmon works out the example in considerably more detail in Salmon (2018), §1.

[12]For a well-known presentation of the objection, see Soames (1988).

the semantic contents of sentences, and denotations of that-clauses, are sets of worlds.

Might the schmidentity strategy come to the aid of the proponent of possible worlds semantics? It seems that it can. Here we have the following values for H and E:

> H: The semantic contents of sentences, and the denotations of that-clauses, are sets of possible worlds.

> E: Competent speakers of the language sometimes judge that attitude ascriptions have different truth values despite those ascriptions differing only in the substitution of sentences which those speakers know to be true in just the same possible worlds.

It seems that here we can follow Salmon's lead, and imagine that the speakers of a language give their assent to a governing linguistic committee. We can further imagine that the committee, being very impressed with the beauty and simplicity of intensional semantics, decrees that the semantic contents of sentences are to be sets of possible worlds. (We can imagine this to have been accompanied by other decrees with respect to subsentential expressions of various sorts.) We may imagine that speakers of the language agree that, from this point forward, there are to be no differences in meaning without a difference in truth conditions. This would then be a world in which H is stipulated to be true.

We can now ask whether, in this world, E would still be the case. And it seems that it would be. For consider the sort of stock example often used in arguments against the possible worlds view of propositions. Let Violet be a mathematically advanced middle school student. She will, of course, assent to such sentences as '2+2=4' and '9x7=63.' Now suppose that Violet is asked, 'Are there infinitely many prime numbers?' Violet might well understand what a prime number is, and so understand the question perfectly well. But she might also think that, because prime numbers become less and less common as numbers get larger, they must run out at some point; so she might say 'No.'

Now, those who are both familiar with the intensionalist linguistic decree and know more about prime numbers than Violet will be well aware that '2+2=4' and 'There are infinitely many prime numbers' mean the same thing in the language which they and Violet speak. But they might, for 'independent pragmatic reasons,' be inclined to say things like 'Violet knows that 2+2=4, but doesn't know that there are infinitely many primes.' They might do this to convey facts about (e.g.) the mathematical sentences which Violet is disposed to accept. They would thus, like Salmon's astronomers, 'unknowingly speak in a way that conflicts with the usage to which they have agreed.' And we can expect that this habit, in time, might lead them to judge sentences like 'Violet knows that there are infinitely many primes' to be not just misleading or inappropriate, but literally false.

And so we find that, even in a language in which the possible worlds view of content is stipulated to be correct, speakers will be inclined to judge that some

attitude ascriptions differ in truth value despite differing only in the substitution of sentences which those speakers know to be true in just the same possible worlds. But that, it seems, is enough for the relevant instances of PREDICTION and No EVIDENCE to be true. And that allows us to derive, via the schmidentity strategy, the highly surprising result that speaker intuitions about the truth conditions about attitude ascriptions are no evidence at all against the thesis that the semantic contents of sentences are sets of worlds.

One may have one two responses to this example. On the one hand, one may take this to be unexpectedly good news for the possible worlds semanticist. But on the other hand, one may take this very unexpected piece of good news to be an indication that something has gone awry with our application of the schmidentity strategy.

I want now to consider another example, which I think suggests that the second response is more likely to be correct. Impressed with the success of their intensionalist decree, we can imagine that our friends on the linguistic committee from the previous example decide to simplify the semantics of their language yet further. They might therefore decree that the semantic contents of sentences, and the denotations of that-clauses, be not sets of possible worlds but truth-values. (We can further imagine that they make corresponding decrees with respect to subsentential expressions; they decree, for instance, that the semantic contents of monadic predicates will be, not functions from worlds to functions from individuals to truth-values, but simply functions from individuals to truth-values.) We can further suppose that the speakers of the language consent to the decree.

Despite the assent which this linguistic stipulation receives, would speakers of the language still sometimes judge that attitude ascriptions have different truth values despite those ascriptions differing only in the substitution of sentences which those speakers know to have the same truth-value? Despite the fact that these ascriptions would, given the linguistic stipulation, have just the same semantic content, the line of reasoning present in our various instances of the schmidentity strategy seems to show that they would. And that seems to be enough to show that the relevant instance of PREDICTION is true.

The problem is that the result to which this leads — that speaker intuitions about the truth-values of various attitude ascriptions are no evidence against our now extremely coarse-grained semantics — seems quite implausible. The schmidentity strategy seems to generalize so easily that it can be used to defend virtually any semantic hypothesis which makes semantic values comparatively coarse-grained against speaker intuitions which would seem to cut in the opposite direction. But surely speaker intuitions of this kind can sometimes be evidence against semantic hypotheses.

So the examples discussed in this section leave us with a kind of dilemma. On the one hand, the relevant instances of both PREDICTION and No EVIDENCE look quite plausible. On the other hand, they seem to deliver incorrect results when applied to the hypotheses and objections just considered.

3 'Stipulated to be true'

Both PREDICTION and NO EVIDENCE include the phrase 'stipulated to be true.' One diagnosis of our current dilemma is that it is based on a conflation of two interpretations of this phrase.

3.1 Stipulations within worlds

On the first interpretation, to consider the nearest world in which a certain linguistic hypothesis is stipulated to be true is to consider the nearest world in which speakers of a language make a certain sort of overt linguistic stipulation. We might thus imagine, as Salmon does, some sort of committee issuing decrees of certain sorts.

3.1.1 Efficacious vs. inefficacious stipulations

To go this route is to imagine the stipulation taking place within the relevant possible world. The problem with this route is that linguistic stipulations are not always successful; the fact that someone, or some committee, stipulates that a class of expressions has some linguistic property is no guarantee that the expressions have that property.[13] Let's say that a linguistic stipulation is *efficacious* iff the relevant expressions have, after the stipulation, the semantic properties they are stipulated to have. Once the possibility of inefficacious stipulations is noticed, it is fairly clear that what the proponent of the schmidentity strategy needs to consider is not the nearest world in which some linguistic stipulation is made, but the nearest world in which that stipulation is made and is efficacious. Otherwise the relevant instance of NO EVIDENCE would lose all plausibility. The fact that some bit of linguistic behavior would occur in a world in which a semantic hypothesis is false but claimed by some speakers to be true would not, on its face, imply much about whether that linguistic behavior is evidence against the hypothesis.

The proponent of the schmidentity strategy might reasonably concede this point without thinking that it matters much for the success of the strategy. For surely there is a presumption that linguistic stipulations are successful unless the world fails to cooperate in some unexpected way. Sure, it can happen that my stipulation that 'Vulcan' refer to the planet causing perturbations in the orbit of Mercury is inefficacious because there is no such planet. But it is not as though we have attempts to name children and pets constantly going south; in the ordinary case, linguistic stipulations are efficacious.

This is, I think, an entirely fair point about the eponymous use of the schmidentity strategy. There is little doubt that the identity relation exists, and little doubt that we are capable of having thoughts about this relation. Given these facts, it is entirely obscure why we should be incapable of introducing a term to stand for this relation. Sure, identity is more abstract than my dog; but it

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[13]A related worry is raised in passing in Ramachandran (1989), 468.

is hard to see why that fact should make introducing terms for these things all that different.

But our later uses of the schmidentity strategy differ both from this initial use and from ordinary linguistic stipulations in two respects. The first is a difference in the linguistic property the stipulation is about. While the distinction is marked using different vocabulary in different approaches to natural language semantics, virtually all such approaches recognize a distinction between the semantic contents of expressions and what those expressions designate or refer to. I'll use the term 'semantic value' for the latter. Of course, it is true that some theories — like Millianism — hold that some expressions have their semantic values as their semantic contents. But proponents of these views still endorse the distinction between semantic content and semantic value, even if they think that occasionally one and the same entity can play both roles.

Garden variety linguistic stipulations are stipulations of semantic values. When I introduce a name, I stipulate what the name is to stand for; I don't, in the ordinary case, make any stipulations with respect to its semantic content. Of course, if my stipulation is efficacious, I will have succeeded in giving the name a semantic content as well as a semantic value. But this is, as regards the stipulation, a somewhat secondary effect. The name introduced comes to have whatever semantic content is needed to secure the wanted semantic value.

But consider, by contrast, the linguistic stipulation employed as part of Salmon's defense of Millianism. There the stipulation is not that a certain expression designate a certain entity; rather, the stipulation is that certain expressions have a certain sort of semantic content. The same is true of Kripke's defense of the Russellian analysis of definite descriptions, and the imagined defense of possible worlds semantics in the previous section.

The second difference between ordinary linguistic stipulations and the stipulations imagined in instances of the schmidentity strategy is a difference in scope. Familiar linguistic stipulations concern only one expression, like 'Rufus' (or 'schmidentity'). By contrast, many of the examples discussed above concern more general linguistic stipulations, which are directed at, not just individual linguistic expressions, but broad categories of linguistic expressions (for example, definite descriptions).

So the stipulations in question are stipulations with respect to the types of semantic contents certain categories of linguistic expressions have. The problem is that there is reason to think that at least some linguistic stipulations of this kind are bound to fail.

3.1.2 Modally stable semantic properties

The reason is that certain facts about the semantic properties of expressions in a language seem to be, in the following sense, modally stable:

> F is a modally stable property of natural language iff if one natural language has F, then all actual and nearby possible languages also have F

The definition is obviously vague. But this will be enough to make clear the main points that follow.

If a certain property of a natural language is modally stable in this sense, then it it is plausible that it is not sensitive to relatively local facts about linguistic usage. Among the relatively local facts about linguistic usage are facts about the linguistic stipulations speakers of the language have made. Given this, it seems plausible that if a language has a modally stable property F, and we imagine speakers of the language making a stipulation which, if efficacious, would make it the case that the language lacks F, the nearest world(s) in which that stipulation is made are worlds in which the stipulation is inefficacious. Let's say that stipulations which are inconsistent with some modally stable property of a language *conflict* with that property.

Which properties of languages are modally stable? Consider the view of a structured propositions theorist who thinks that the semantic contents of sentences must be more fine-grained than possible worlds. It would be very odd for this theorist to adopt this view of English, but not of (say) Bengali. Surely if the semantic contents of sentences of English are more fine-grained than sets of possible worlds, the same goes for the semantic contents of sentences of other natural languages.

And if this is true of other actual natural languages, it is presumably also true of languages spoken by communities in nearby possible worlds. After all, just the same considerations which count in favor of the view that sentences of English express structured propositions would seem to count in favor of the view that the sentences of languages spoken by merely possible agents who are similar to us do the same. So, it seems plausible that the property expressed by the open sentence

> sentences of L have structured propositions as their semantic content

is modally stable. The same, of course, goes for the opposed view that sentences have sets of worlds as their contents. If this view is true of one natural language, it would seem to hold for all.

Note that the proponent of the view that properties of this sort are modally stable need not think that it is metaphysically impossible that languages lack the relevant property. They need only think that speakers of a language which lacked the property would have to be quite different than, and therefore relatively modally remote from, actual speakers.

Nor is this only true of sentences. It seems plausible that this holds for expressions of any syntactic category. It would, for instance, be bizarre if the subsentential expressions of English had Russellian contents whereas the expressions of German had Fregean contents. Certain facts about the form of the correct semantic theory for a language seem to be modally stable.

3.1.3   Modal stability, names, and descriptions

The discussion in the preceding paragraph is enough to show what is wrong with the imagined uses of the schmidentity strategy discussed in §2: if the semantic

contents of sentences of our language are in fact more fine-grained than sets of worlds, then this property of our language seems to be modally stable, and hence not the sort of property which can be undone by linguistic stipulation. So the defender of the idea that propositions are more fine-grained than sets of worlds would seem to have a principled reason for rejecting the relevant stipulation as inefficacious: it conflicts with what that theorist regards as a modally stable property of our language. This gives the structured propositions theorist a principled reason to think that the relevant linguistic stipulation is inefficacious, and hence to reject the relevant instance of NO EVIDENCE.

Because the dialectical situation here is a bit complex, it is worth pausing a moment to think about how this line of argument works. We can imagine a theorist undecided as between the hypotheses (H) that the denotations of that-clauses are structured propositions and (H') that they are sets of worlds. The theorist then encounters some evidence E — in this case, judgements about the truth conditions of certain attitude ascriptions — which seem to favor the former hypothesis over the latter. On the basis of E, they come to believe that H rather than H' is correct.

In reply, we imagine the sets of worlds theorist employing the schmidentity strategy, and arguing, on the basis of the relevant instances of PREDICTION and NO EVIDENCE, that E does not favor H after all, since if H' were true, E would be true. But the argument for this conclusion relies on the assumption that a stipulation that H' is true would be efficacious. But because H and H' are both modally stable properties of languages, this relies on the assumption that H' is in fact true. Because our theorist believes on the basis of E that H' is false, they have reason to reject this assumption, and hence reject the relevant instance of NO EVIDENCE.

Is there something objectionably circular about this line of thinking? I don't think so. There is no obvious incoherence in believing both that E favors H and in believing the material conditional that if H' is true, E does not favor H.

An analogy in another domain may help. Suppose that we are considering the question of whether theism or atheism is true. Suppose further that we come to believe that the existence of evil favors atheism, and on that basis come to believe that atheism is true. It is of course fair game for the theist to reply by arguing that evil does not in fact favor atheism. But suppose that the theist's argument for that conclusion uses theism itself as a premise. It is perfectly reasonable for the atheist to be unmoved by this argument, even if all of the other premises are correct. The atheist can endorse the material conditional that if theism is true then evil does not favor atheism without giving up their view that in fact evil does favor atheism.

This gives an outline of the reasons why a particular instance of the schmidentity strategy might fail. With this in mind, let's turn to the applications of the schmidentity strategy discussed in §1.

As already mentioned, the considerations just laid out would seem to pose no problem for the original use of the schmidentity strategy. Containing or failing to contain an expression with a certain semantic value is not, in general, a modally stable property of a language. So there would appear to be no modally stable

property of our language which conflicts with the stipulation that 'schmidentity' have the identity relation as its semantic value.

How about Kripke's defense of the Russellian analysis of definite descriptions? Here, unlike the 'schmidentity' case, we have a stipulation with respect to the semantic contents, rather than semantic values, of a class of expressions. This might lead one to assimilate this case to the case of the possible worlds view of propositions. But in this case the stipulation is that definite descriptions have the same meaning as another expression type in our language; a sentence ⌜The F is G⌝ is stipulated to mean the same thing as ⌜Exactly one thing is F and that thing is G⌝. Stipulations of this kind — in which one expression is stipulated to abbreviate, and so have the same semantic content of, another — are a somewhat special case. Here the language is already agreed to have an expression with the relevant semantic content. So there is no reason to think that the stipulation that another expression have that content should conflict with any modally stable property of the language. So our present discussion seems to give us no reason to doubt that the relevant instance of NO EVIDENCE is false.

Let's consider next Salmon's defense of Millianism against objections based on apparent substitution failures of coreferential names in the complements attitude ascriptions. Is there a principled reason for the anti-Millian to view the linguistic stipulation involved in this instance of the schmidentity strategy as in conflict with some modally stable property of our language?

Here there is, I think, no one answer. It depends on the sort of anti-Millian view one has. Consider first a Fregean of a fairly traditional stripe, who takes the semantic contents of expressions to be abstracta which are individuated by Frege's criterion of difference for sense based on distinctions of cognitive significance. This sort of traditional Fregean will regard objects like 'Mont Blanc with its snowfields' to be altogether the wrong sort of thing to be the semantic content of a linguistic expression.[14] To be sure, one might give all sorts of arguments against this kind of view. But the proponent of this view would seem to have a principled reason for thinking, not just that our language contains no Millian terms, but also that this absence of Millian terms is a modally stable property of our language. Salmon's use of the schmidentity strategy would thus — on the 'stipulation within worlds' interpretation – seem to have little force in blocking the use of Frege's puzzle as a motivation for this sort of view. The orthodox Fregean has reason to think that the would-be Millian linguistic stipulation is inefficacious, and thus that the relevant instance of NO EVIDENCE is false.

There are other less wildly anti-Millian views which would seem to have the resources to make the same move. Consider, for example, the Russell of 'Knowledge by Acquaintance and Knowledge by Description,' who agrees with the Millian that some singular terms 'merely and solely name' objects.[15] But because Russell both held that we can introduce these Millian terms only for

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[14]The example, as is well known, is from Frege's correspondence with Russell. See Gabriel et al. (1980).
[15]Russell (1911), 121.

the objects of acquaintance and had a restrictive view of the possible objects of acquaintance, he thought that 'I' and 'this' were the only Millian names for particulars. It is fairly clear that Russell thought that these views about the relationship between naming and acquaintance and about the scope of acquaintance are not simply true due to accidental features of our language. Someone with views like Russell's would, like the orthodox Fregean, therefore seem to have principled reasons for thinking that Salmon's imagined linguistic stipulations with respect to 'Schmesperus' and 'Schmosphorus' conflict with a modally stable property of our language, and hence are inefficacious.

But these two examples don't mean that Salmon's argument is without force; this sort of reply to Salmon's use of the schmidentity strategy would not seem to be open to all non-Millians. Consider a theorist who endorses the (plausible) view that the semantic content of a variable relative to an assignment is the value assigned, but denies Millianism about ordinary proper names. Presuming that the theorist has no idiosyncratic views about the sort of things which can be the values of variables, it is hard to see what principled reason they could have for resisting the idea that stipulations like those in Salmon's example could be efficacious. It is thus hard to see how — presuming that they agree with Salmon that the relevant instance of PREDICTION is true — they could take apparent substitution failures in attitude ascriptions to be evidence against Millianism.

Consider now Salmon's use of the schmidentity strategy to undercut the motivations for semantic relationism. Would the stipulations in this case be efficacious? Salmon thinks that it is clear that they would be. After all, classical semantics makes no distinction between coordinated and uncoordinated propositions. But surely, Salmon argues, it is possible to speak a language which fits classical semantics in this respect; as Salmon says, 'There are no semantic gods to forbid such a language, no semantic police who will hit the back of our hands with a ruler if we set up such a language.'[16]

This is not so obvious to me. As above, we can consider the hypothesis that semantic relationism is true of English, and ask whether, given this supposition, it is plausible that semantic relationism is true of every other natural language. It seems overwhelmingly plausible to me that this is so. Just as it would be bizarre to think that English sentences have structured propositions as their semantic contents whereas Bengali sentences have sets of worlds as their contents, it would be bizarre to think that semantic relationism was true of English but not of Bengali. Given this, it seems plausible that being a language of which semantic relationism is true is a modally stable property.

What, according to the proponent of semantic relationism about English, does it take for semantic relationism to be true of a language? A plausible answer would seem to be that it is true of any language in which it is a linguistic convention that multiple uses of a name all have the same semantic value, and in which it is possible to understand distinct coreferential names without knowing that the names corefer. But those conditions are satisfied by the post-stipulation language that Salmon imagines.[17] This suggests that there

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[16]Salmon (2018), 221.
[17]As Salmon notes:

are principled reasons for the semantic relationist to resist Salmon's use of the schmidentity strategy by denying the relevant instance of No Evidence. They should concede that post-stipulation speakers would exhibit the relevant linguistic behavior, but should deny that the stipulation is efficacious, and for that reason should deny that the post-stipulation existence of the linguistic behavior implies anything about whether that behavior is evidence against non-relationist Millian views of names.

In this section, I've offered a number of preliminary thoughts about the success of various instances of the schmidentity strategy. But the primary moral of this discussion is methodological. The moral is that, if we think of the relevant linguistic stipulations as happening within the relevant possible worlds, the key question is whether these stipulations would be efficacious. And that, I've suggested, depends largely on the question of whether there are any modally stable properties that our language actually has but would lack if the stipulation were efficacious. If one has reason to believe that there are such modally stable properties, that provides reason to deny the relevant instance of No Evidence.

3.2 Stipulations about worlds

A proponent of the schmidentity strategy might reply to the preceding line of thought by saying that the version of the schmidentity strategy discussed in §3.1 takes talk about speakers making linguistic stipulations much too seriously. Adapting another well-known lesson from *Naming and Necessity*, they might say, "Possible worlds' are *stipulated*, not *discovered* by powerful telescopes.'[18] There's thus no reason to consider possible worlds in which speakers make various overt linguistic stipulations and examine those worlds with a semantics-scope to see whether the stipulations are efficacious. Instead, we can simply stipulate 'from the outside' that a given world is one in which the semantic hypothesis in question is true. Wouldn't that be a simple way around the difficulties canvassed above?[19]

Perhaps. But there is a worry that the move simply shifts the locus of our difficulties from No Evidence to Prediction. If the linguistic property which conflicts with the relevant stipulation is modally stable, then the nearest world in which the stipulation is true may be one in which users of the language are quite different than us. And in that case, intuitions about what those speakers would think or do are bound to be much less reliable.

'It should be noted here that their agreement to speak Schmenglish does not eradicate their knowledge or ignorance, as the case may be, concerning whom the names "Cicero" and "Tully" name. Those who knew that the two names are co-designative in English know that they are equally co-designative in Schmenglish. In particular, their agreement to speak Schmenglish does not obliterate their knowledge that every proper name that designates at all in Schmenglish is exactly synonymous with itself, and hence co-designative with itself.' (Salmon (2018), 218)

[18]Kripke (1972), 44. Emphasis in original.
[19]This second interpretation of the schmidentity strategy arguably fits better with Kripke's presentation of the argument.

To see the force of this, consider again Salmon's defense of Millianism against arguments based on apparent substitution failures in attitude ascriptions, and the stipulation that 'Schmesperus' and 'Schmosphorus' are Millian names. What would a linguistic community have to be like for this stipulation to be true of it?

From the point of view of a Millian, the answer is clear: it would be just like our actual linguistic community. But from the point of view of a Fregean who accepts Frege's criterion for difference of sense based on distinctions of cognitive significance, it seems that the speakers of a Millian language would have to be quite a bit different than us. On this view, for a language to contain distinct co-referential Millian names n and m it would have to be impossible that (roughly) a speaker understands the names and, on reflection, is unsure whether a pair of sentences differing only with respect to substitution of n and m have the same truth-value.[20] But presumably speakers who satisfy this condition would not ever be inclined to regard attitude ascriptions which differ only in the substitution of coreferential Millian names as differing in truth value. But that falsifies the relevant instance of PREDICTION.

This is, of course, just the difficulty raised in §3.1 in a new form. When a stipulation conflicts with a modally stable property of language, we can either consider the nearest world in which the stipulation is made, or the nearest world in which it is true. If we take the former route, there will often be principled reasons for thinking that the stipulation is inefficacious, which will falsify NO EVIDENCE. If we take the latter route, there will often be principled reasons for thinking that E is false at that world, which will falsify PREDICTION.

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The schmidentity strategy is a powerful form of argument. Given the significance of the conclusions it has been used to defend, it is surprising that it has not received more explicit discussion. My aim has been less to argue for or against the strategy as such, but rather to advance the discussion of what conditions must be satisfied for the strategy to be successful.[21]

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[20]This is just a first pass. A better formulation would, for reasons related to cases like Kripke's (1979) example of 'Paderewski,' have to be more careful about the distinction between expression types and expression tokens. It is not obvious whether this can be done. For some of the difficulties, see Speaks (2013).

[21]Thanks to Nathan Salmon for helpful discussion of the issues discussed in this paper.

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