Wittgensteinean Notions of Uniformity and Kripkensteinean Skepticism*

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The threat of semantic skepticism that Kripke (1982) extracts from Wittgenstein's remarks on rule-following is as trenchant as it is troubling. Still, Kripke's reading of Wittgenstein as grappling with such skepticism is on shaky exegetical grounds.[1]

My point of departure here is a reading of Wittgenstein I defend elsewhere on which he never engaged with semantic skepticism in his texts.[2] While this reading distances Wittgenstein from Kripke, an intriguing indirect connection between their work remains. Certain concepts like *regularity*, *constancy*, and *(qualitative) sameness*—what I will call "notions of uniformity"—play a significant role in addressing questions in the foundations of semantics for Wittgenstein. If Wittgenstein's appeal to these notions is legitimate, they may also be of use in diffusing semantic skepticism.

1 Notions of Uniformity in the *Investigations* and a Naive Reply to the Skeptic

Let me *very* briefly sketch the reading of Wittgenstein I am working from. On it, Wittgenstein asks and addresses two logically distinct questions in his rule-following remarks.

The first of Wittgenstein's questions is (roughly) in semantic epistemology. Imagine an interlocutor who is liable to systematically misinterpret any overt definitions or

*Some of the material in this essay appears, sometimes in a slightly altered form, in Chapters 4 and 10 of Shaw (forthcoming).

[1] See, e.g., the exegetical criticisms coming on the heels of Kripke's work in Baker & Hacker (1984), McDowell (1984), McGinn (1984), Anscombe (1985), Goldfarb (1985), and Tait (1986). I give my own reservations in Ch.3 of Shaw (forthcoming).

[2] Shaw (forthcoming). From here on out, "Wittgenstein" is best read as "Wittgenstein as I read him."

examples you give that are supposed to explain how some words (like "add 2") are to be applied to new cases. Wittgenstein's first question is:

> The Justificatory Question: What justification did you possess when meaning your words, such that if a bizarre misinterpreter were to possess that justification, even they could have reapplied the words as you would to new cases?

"Justification" here is used in a special sense, largely divorced from its role in contemporary epistemology. Wittgenstein takes model justifications in non-deviant cases to be things like ostensive definitions or like schematics, diagrams, or images that—engaged with in certain processes—can be used to rectify misconstruals of verbal instructions.[3] So the question is asking whether we can produce something *like those* that will avoid bizarre misinterpretation. Wittgenstein's technique here is one of trial-and-error: run through possible justifications (especially 'internal' mentalistic ones), and see whether any can do this work. And Wittgenstein's conclusion is that none can. We possess nothing on the model of what Wittgenstein calls a "justification" such that, if the bizarre misinterpreter were only to have had that justification, their applications would have coincided with ours. Note that however surprising that conclusion may be, no radical concerns about meaningfulness (e.g. threats of non-factualism or non-truth-conditionality) are immediately pressed by it.

The second of Wittgenstein's questions is (roughly) in the foundations of semantics. I call it the "Grammatical Question" for reasons we needn't get into here. It suffices for now to note that Wittgenstein almost always frames his foundational semantic questions metalinguistically (not: "what is meaning?", but: "how do we use the word 'meaning'"?).

> The Grammatical Question: When 'everything is open to view,' what conditions influence your willingness to say "such-and-such was meant" ("such-and-such a rule was followed," etc.) on a particular occasion?

The methodology Wittgenstein uses to address this question is one he reserves for family resemblance terms—that is, the methodology of case studies. These often take the form of thought experiments involving contrast cases: a 'good,' clear case of meaning or rule-following, and a case where some single feature from the good case is removed.

[3] This construal of justification is made clearest around Wittgenstein (1958, p. 14)—though Wittgenstein there uses the (for him interchangeable) language of reasons.

We are then asked to see if removing the relevant feature makes a difference to our willingness to apply semantic terms in the context imagined. His conclusion from a series of such case studies is that there is an assortment of different things that can matter, in different contexts, to how we use semantic terms (the presence of training, the presence of repeated usage, the presence of 'ordinary human behavior', the presence of regularity in usage, etc.). But no set of features provides anything like necessary and sufficient conditions. This last idea is, of course, bound up with a treatment of meaning, rule-following, etc. as family resemblance concepts.

The Justificatory and Grammatical Questions are related for Wittgenstein. He thinks the presupposition of the Justificatory Question (that there is some justification we have that our imagined perverse interlocutor lacks) stands in relationships of mutual support with a tempting misconception of meaning as a 'local' event—where the criteria for meaning a word in a particular way centers around the happenings at the time and place the word was meant. The grammatical investigation aims to dislodge that misconception, by bringing to the foreground non-local criteria (training that precedes acts of meaning, regularity that extends beyond it, etc.). But, again, nowhere is it ever countenanced that there is a problem for meaning attributions of the kind Kripke raises (e.g., in the truth-conditionality of such attributions).

The above is the barest sketch of a reading of the rule-following remarks, and is compressed even as such. Still, it can serve to frame the significance of a thought experiment engaging with the Grammatical Question at §207 of the *Philosophical Investigations*.[4]

> Let's imagine that the people in [an unknown country] carried on the usual human activities and in the course of them employed, apparently, an articulate language. If we watch their activities, we find them intelligible, they seem 'logical'. But when we try to learn their language we find it impossible to do so. For there is no regular connection between what they say, the sounds they make, and their activities; but still these sounds are not superfluous, for if, for example, we gag one of the people, this has the same consequences as with us: without those sounds their actions fall into confusion—as I feel like putting it.
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> Should we say that these people have a language: orders, reports, and so

[4] Henceforth *PI*. I employ the translations from Wittgenstein (2009), sometimes with minor modifications of my own.

on?

PI §207

This passage exhibits the methodology of contrast case studies I alluded to earlier: Wittgenstein takes the good case (a community of ordinary speakers) and subtracts exactly one feature, namely regularity of usage, and finally asks if this makes a difference to whether we say meaningful language is present. One way to imagine the people Wittgenstein has in mind is to take an extended span of interactions from a group of actual human speakers and imagine a counterfactual circumstance differing only in that usage of words is chaotically permuted (perhaps along with anything needed to effect the permutation—e.g., some causal bases for the chaotic usage). Outwardly, by hypothesis, this counterfactual people looks exactly like the original group of ordinary language users up to the sounds they emit or the symbols they inscribe. Does this people speak a language? This thought experiment is a rare case where we have an unequivocal verdict from Wittgenstein.

There is not enough regularity for us to call it "language".

PI §207

I agree. It can help to see the point by focusing on just one 'word.' Supposing the original, un-permuted speech was English, a word like "knight" might be used by one of the permuted users in a setting where they are testing avocados for ripeness at the supermarket, by another when they are selecting paint colors for a bedroom, by another in the middle of what looks like a counting procedure, and so on. The word does not occur in any discernible pattern. If this word is part of a language, it should have meaning. But what could its meaning be?

If there is no regularity in the use of "knight" whatsoever, then there can be no words of our language to say directly what its meaning is. We could try to define the meaning indirectly as 'whatever people seem to be using it for (or are disposed to use it for) on each occasion.' But this presumes we can first figure out what "knight" is used for on *any* occasion, which on reflection is completely unclear. For example, imagine there's a case where some speakers seem to treat the word "knight" as having a possible application, but one that they are as yet ignorant of (e.g., an astronomer appears to conjecture tentatively with "knight", observing a point of light through a telescope). How do we settle a correct application, which should be possible if "knight"

has a meaning here? There's no help from actual usage, actual dispositions, or even 'ideal' dispositions—how are we ever going to pick out ideal dispositions for these completely bizarre agents? And even if we could identify ideal dispositions, what if on such dispositions we find speakers are not disposed to use "knight" in ideal circumstances (e.g., where they have better eyesight, more astronomical experience, etc.)? Is that due to the word failing to apply to (say) the heavenly body, or is it due to the oddly permuted linguistic usage of this people, which appears to be sensitive to *any* change of circumstances?

Due to concerns like this, I'm inclined to agree with Wittgenstein that even though *all* other features of meaningful linguistic use are present, the absence of regularity loosens our grip on how this people could be construed as speaking a language.

More could be said to bolster this lesson from *PI* §207 and to bolster the exegetical significance of regularity and other notions of uniformity (like 'sameness', etc.) for Wittgenstein.[5] But for now, let's just take this as a given: Wittgenstein thinks regularity of usage is one of the features that influences our willingness to say words have meanings, rules are followed, etc. And for him that means that regularity of use is something that sometimes helps 'constitute' the presence of meanings (at least in the whatever sense of constitution his grammatical, family resemblance methodology allows).

Wittgenstein, as I alluded to before, draws out many more concepts than just regularity in investigating which features play a role in attributing meaningfulness. These include: the presence of training, repetition, general harmonious 'agreement' in good cases, similar harmonious reactions to 'justifications', and so on. The reason I focus here on regularity is because it appears to have a distinctive power against the kinds of skeptical considerations Kripke adduces on Wittgenstein's behalf.

Kripke's skeptic asks of Kripke's past use of the word "+": "what fact it is (about [Kripke's] mental state) that constitutes [his] meaning plus, not quus,"[6] where "quus" denotes a function that deviates from addition for arguments greater than the greatest number figuring in Kripke's past explicit computations. The definition of "quus" ensures Kripke's past usage doesn't distinguish between addition and quaddition. So the question is in effect: what *else* could do the work of settling that Kripke meant the

[5] To give one other example, at *PI* §237 Wittgenstein says we could not count a procedure in which someone produces one line on the basis of another as following a rule if there is no discernible regularity in how the features of the first line relate to the second.

[6] Kripke (1982, p. 11).

first function rather than the second? Kripke adduces an impressive array of arguments seeming to show that no fact seems suited to the task.

However, if we help ourselves to the use of notions of uniformity like regularity in addressing questions in the foundations of semantics, as I've claimed Wittgenstein does, then it seems like we have the following relatively straightforward reply.

> Naive Reply
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> What determines that I meant addition by "+" rather than quaddition? While there may be no general answer as to what facts determine someone's meaning addition by their words, we can point to many features of past use and its circumstances (ordinary human responses of approval and disapproval, expectation and frustration, and so on) that help us determine a range of 'core, good' applications of "+" by myself and other calculators that accords with a substantial (albeit 'gappy') initial segment of the addition function. Of course, as Kripke notes, all this usage accords just as well with many quaddition-like functions. But *addition is the most regular and uniform continuation* of the core, good applications we make. Indeed, it is the most regular and uniform continuation of even the smallest segments of addition that we use in elementary instruction. And that is one of the factors that influences our willingness (that is, my willingness, and that of ordinary speakers like me) to say that addition is meant by users of "+", including my past self. It is not that it is impossible to mean a quaddition-like rule, but rather that its irregularities must be manifest either in original use or dispositions for use, in order for us to be willing to say that it was meant on an occasion. Otherwise addition, because the most regular continuation, wins by default. And that is just a feature of how we use the word "meaning".

What can we say about this somewhat simple-minded attempt to head off the skeptic's arguments?

2 Lewisian Natural Properties

To those familiar with the literature on meaning skepticism the Naive Reply should look familiar. With some fiddling it could be viewed as a variant on the reply offered

by Lewis (1983). (Indeed, I've borrowed the language of 'naivité' from him.) For this reason, Lewis's views serve nicely as a contrast for the 'Wittgensteinean' position we're exploring.

Here is the barest sketch of Lewis's position. Lewis accepts a broadly 'interpretationist' picture of mental content, on which such contents are assigned to a mental state via principles of functional fit designed to optimize the rationality of the agents to whom the mental contents are ascribed. But (even independently of Kripkensteinean worries) Lewis thinks there will be many unrelated contents that satisfy principles of functional fit for any given thinker. So to avoid radical underdetermination, we need to introduce what he calls 'principles of humanity': "*a priori*—albeit defeasible—presumptions about what sorts of things are apt to be believed and desired."[7]

Lewis says that to wield these principles, we need a metaphysically privileged class of properties: the natural properties.

> It is here that we need natural properties. The principles of [humanity] will impute a bias toward believing that things are green rather than grue, toward having a basic desire for long life rather than for long-life-unless-one-was-born-on-Monday-and-in-that-case-life-for-an-even-number-of-weeks. In short, they will impute eligible content, where ineligibility consists in severe unnaturalness of the properties the subject supposedly believes or desires or intends himself to have.
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> (Lewis, 1983, p. 375)

So, the content of a mental state (which, for Lewis, will be a property) is determined by a combination of the content's functional fit and metaphysical naturalness. This, familiarly, is supposed to help with meaning skepticism.

> The naive solution [to Kripkensteinean skepticism] is that adding means going on in the same way as before when the numbers get big, whereas quadding means doing something different; there is nothing present in the subject that constitutes an intention to do different things in different cases; therefore he intends addition, not quaddition. We should not scoff at this naive response. It is the correct solution to the puzzle. But we

[7] Lewis (1983, p. 375).

> must pay to regain our naiveté. Our theory of properties must have adequate resources to somehow ratify the judgement that instances of adding are all alike in a way that instances of quadding are not. The property of adding is not perfectly natural, of course, not on a par with unit charge or sphericality. And the property of quadding is not perfectly unnatural. But quadding is worse by a disjunction. So quaddition is to that extent less of a way to go on doing the same, and therefore it is to that extent less of an eligible thing to intend to do.
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> (Lewis, 1983, p. 376)

Some clarifications: I follow Schwarz (2014) in thinking that Lewis is not claiming that the addition function is more natural than the quaddition function. He is rather claiming that the property of being an adder is more natural than the property of being a quadder. Second, Lewis initially glosses his resolution by saying that adding is "going on in the same way." But "same" here had better not mean *numerically identical*. That is tantamount to saying that we were always adding which would beg the question against the skeptic. "Same" is better construed as expressing qualitative sameness—that is, sufficient similarity in certain respects. This comes out more clearly when Lewis talks of instances of adding being "all alike" in ways that instances of quadding are not. Qualitative sameness and likeness are, of course, notions of uniformity.

Seen in this way Lewis's naive response and the Wittgensteinean one sketched in §1 run parallel. Lewis's functionalism is the souped up, modern, mentalistic counterpart to Wittgenstein's seemingly more behaviorist and linguistically focused methods of establishing 'core applications.' Then both appeal to notions of uniformity to smooth out remaining cases. The key difference at present, of course, is that Lewis posits a metaphysical distinction of naturalness to 'ratify' (as he puts it) our judgments of uniformity. Wittgenstein would posit no such distinction. Of course Wittgenstein thinks some things are more similar than others, some continuations of patterns are more regular than others, and so on. But the notions of uniformity are just ordinary, intuitive concepts. There's no concern to 'ground' or 'ratify' them in Wittgenstein, even when they surface in foundational semantic investigation, and certainly not with the help of a metaphysical distinction.[8] So the contrast between Wittgenstein and Lewis forces us to

[8] As one might anticipate, Wittgenstein is in fact hostile to such a distinction—see, e.g., I.X of Wittgenstein (1974).

confront the question: what work are universals doing for Lewis? I believe that probing this question reveals that Lewis's appeal to universals is not only superfluous, but in fact counterproductive.

A first, simple concern is that it is far from obvious that Lewis's proposal gets us the right results. Lewis takes perfectly natural properties to be those figuring in fundamental physics like spin, charge, and spatiotemporal distance. It might also be worth noting what kinds of properties seem to be excluded from the category of perfect naturalness: being exactly alike, or alike in some respect, set and class membership, and so presumably more generally properties of numbers.[9]

So Lewis's claim seems to be something roughly like this: when we write up a definition of the properties of 'being an adder' and 'being a quadder' in terms of things like spin and charge, we will find the former involves (say) a shorter definition than the latter.[10] But complexity in the definition of a property applying to all and only (actual and possible) adders in terms of spin, charge, etc. seems like it will exhibit an absurd degree of complexity. What does a young child grouping together apples and an accountant in the midst of furiously mashing on a keyboard share in terms of fundamental properties like charge and spin? And how can we extend this to cover all actual *and merely possible* adders, human or otherwise?

Grant for the moment that there are such definitions. What do we know about them—in particular about their lengths? Do we even know that they are finite? Couldn't it be that the definitions in question exhibit some 'surprising' results—for example that the quadders form a more natural class than the adders? And what importance would this have for how we use the term "intends to add"? Would we say, in the face of this surprising result, that such talk referred to quadders all along? I find this incredible.

These worries for the role of naturalness may seem to be exacerbated by the fact that Lewis's candidate natural properties (spin, charge, etc.) are distantly removed, by chains

[9] Lewis (1983, p. 345). See discussion of this point at Schwarz (2014, p. 24).

[10] Lewis develops his solution by appealing to a relative notion of naturalness, which he never developed in great detail. He sometimes suggested (Lewis (1986, p. 61), Lewis (1984, p. 66)) that we can ascertain the relative naturalness of two properties by comparing the lengths of the definitions of those properties in terms of perfectly natural properties, which is the provisional understanding I work with here given Lewis's talk of the property of intending to quadd being "worse by a disjunction." See the citations in Schwarz (2014, p. 25) for some alternative characterizations that may or may not be equivalent. I do not think these differences in characterization of naturalness help with the general concern developed here.

of definition, from intuitive applications of notions of uniformity to rule-followers or the rules they follow. But the core of the objection above actually has little to do with definitional distance. And this is important, since the interpretation of Lewis I've been working with is controversial.[11] Some philosophers have attributed to Lewis a much simpler account of the role of naturalness in semantics.[12] On this view, naturalness arbitrates directly between possible *linguistic* contents for an expression (rather than indirectly influencing linguistic contents by first arbitrating between the contents of mental states). Naturalness does this by privileging a subset of the contents compatible with certain actual privileged uses of particular words. Call a view of this form *Reference Magnetism*. Sometimes philosophers see Lewis as holding not only a magnetic view of linguistic reference, but also holding that naturalness is applicable directly to mathematical entities like functions, so that (say) the property of being the addition function would be more natural than that of being the quaddition function.

Whether or not this view is properly attributable to Lewis isn't of great concern here. One can still wonder: wouldn't this view escape the worries I've given above? Couldn't it help reveal more clearly how fundamental metaphysical facts could be of great importance to the meaning of "meaning"?

But actually the objection to this sketched simple form of Reference Magnetism is essentially the same as the objection against the appeal to naturalness that I originally attributed to Lewis. In fact, I think a decisive objection to both appeals to naturalness can be found (with minor variations) in a footnote of Kripke's.[13]

In the footnote in question, Kripke considers an attempt to revitalize a dispositionalist reply to semantic skepticism by appealing to a notion of simplicity from computer science. The idea is that perhaps addition would come out to be a simpler function than quaddition along some metric computer science would supply, and that this could arbitrate which of several dispositions we count as manifesting on a particular occasion. Kripke's main response to this idea invoked a hypothetical: suppose (after developing the relevant technical simplicity metric) that on investigating the structure of the brain we found the simplest program instantiated in it, to our surprise, was computing quaddition. Kripke asked: "Would this show that I did not mean addition by '+'?" Would

[11] E.g., for some criticisms of Schwarz's reading of Lewis, which I've been following here, see Janssen-Lauret & MacBride (2020).

[12] Cf. Weatherson (2003), Sider (2009).

[13] Kripke (1982, p. 39 n.25).

it show that I meant quaddition instead? The answer to both questions, obviously, is "no." Kripke thought this sufficed to show that any technical notions offered by computer science are not part of the *actual* foundations of our practices of attributing meanings in linguistic usage.

It is important to recognize that the force of Kripke's objection here is not hostage to the future development of science. The objection to the use of simplicity metrics would not have been met by doing computer science and developing a suitable notion of simplicity that showed that programs computing addition were generally simpler than those computing quaddition. It would not have been met even if we could wield this hypothetical characterization of simplicity to show that quaddition was a relatively less simple program instantiable in our brain structure. The point of Kripke's hypothetical was not to raise a likelihood, but to make a conceptual point: that we regard the outcome of the project of developing a notion of simplicity in computer science and applying it to our brains with relative indifference when it comes to actually ascertaining our intentions. The failure of that project to vindicate our intuitive judgments would not dislodge them. So, correlatively, the success of the project can in no way support them.

Similarly the objection I raised above for appeals to naturalness—which involved raising the hypothetical outcome of metaphysical inquiry turning up the 'surprising' result that quaddition is more natural than addition—is not an objection which banks for its efficacy on a bet about the development of metaphysics. It is not met by successfully defending a metaphysics on which (say) addition really is more natural than quaddition. The point of the hypothetical is to show that on reflection we view the result of technical, metaphysical investigation with indifference. We use the term "meaning" without any regard for it. The failure of the metaphysical project to vindicate our intuitive judgments about meaningfulness would in no way dislodge or alter them, and so the success of the metaphysical project in no way supports them either.

The same points go for my imagined reference magnetist. Maybe it is less in doubt that their notion of naturalness, which applies directly to functions, will make an addition function (as opposed to the property of being an adder) more natural than its competitors. The objection is not that the result of metaphysical investigation won't pan out. The objection is that the investigation's possible failure (even if this possibility is merely conceptual) would do nothing to influence our application of "means". And that shows that it is not part of the actual foundations of that word's use. The Kripkean

point that our ordinary notions of meaningfulness are not in the first instance responsive to any sophisticated technical analogs of notions of uniformity stands, whether those analogs come to us from computer science or metaphysics.

For reasons like this, I think that Lewis's foray into distinctly metaphysical foundations for semantics was a mistake. (It is worth noting that this foray was new to Lewis at the time—beforehand he seemed happy to take the relevant principles of humanity as unjustified givens.) It is abundantly clear from the examples Lewis gives that he wants the *deliverances* of the application of naturalness to be ordinary, intuitive notions of similarity, regularity, and the like. Trying to ground those appeals in a metaphysical distinction provides no extra support, when it is clear that it is the ordinary notions that are doing the real work. Better to let the ordinary notions play foundational roles themselves, as Wittgenstein frequently seems to do.

So much for the relation between Wittgenstein and Lewis. What can we say about how Kripke's skeptic might view the Naive Reply?

3 Skeptical Responses to the Naive Reply

Notions of uniformity appear sufficient to respond to Kripkensteinean semantic skepticism provided three claims hold.[14]

(i) Present language of uniformity can be used to state (truth-conditional) facts.

(ii) Notions of uniformity privilege non-skeptical continuations of actual usage.

(E.g., the total function that is the most regular extension of the partial function picked out by past privileged use of "+" is in fact addition.)

(iii) It is dialectically permissible to appeal to the language of uniformity while engaging with the skeptic.

[14] I am here glossing over the extremely complex issue of whether there are distinctive normative or epistemic dimensions to the form of skepticism Kripke develops. I tend to think that Kripke's epistemic and normative language can be 'deflated' (cf. Fodor (1990, pp. 135–6,n.35)). Whether or not this is what Kripke intended, I think any formulation of skepticism that incorporates substantive epistemic or normative constraints can and should be dealt with simply by rejecting the constraints. I recognize this requires much more defense than I can supply here. But even if I am wrong about this point, it would be interesting to show that the Naive Reply is sufficient to cope with the 'purely metaphysical' formulation of semantic skepticism. I focus on that issue here without further comment.

If (iii) holds, we are free to use the language of uniformity in the dialectic with the skeptic. Then if (i) and (ii) hold, we can use these notions in stating facts which privilege actual meanings of our words over bizarre skeptical interpretations roughly as the Naive Reply seeks to do. Given this, where should the skeptic put up resistance?

Significantly, as far as I know, there is *no* explicit discussion in Kripke's text that bears on *any* of (i)–(iii). Kripke discusses issues relevant to notions of uniformity at three places in his text, but none of these discussions cast doubt on the claims needed for the Naive Reply to succeed.

The footnote discussed in §2 is an example. There, we saw Kripke criticize an appeal to a simplicity metric from computer science for being at a conceptual remove from actual practices of meaning attributions. But while this objection carries over to metaphysical correlates, it does not carry over to ordinary notions of uniformity. These are not technical notions belonging to a specialized domain like computer science or metaphysics, but are mundane concepts that even a grade schooler is expected to understand and be capable of deploying in elementary instruction. Not only this, but these notions seem directly conceptually tied, in ways evinced by those kinds of instructional settings, to the continuations that yield up reapplications of words consistent with how they are meant.

For example, if we want a student to learn the "even" numbers, it is a normal first attempt to show them the first few evens and, after noting some features of the series, to tell the student to 'continue on *like* I did.' When a student makes a mistake, a common way of showing them why is to demonstrate an *irregularity* in their continuation, and describe it as such. We might also, for example, say that they didn't continue in the *same way* as they were before. In short, the ordinary notions of uniformity (unlike those of computer science or metaphysics) are directly conceptually tied to the continuations of usage that we regard as giving the meanings of our words. So the style of objection Kripke raises in the footnote discussed in §2 cannot be extended to rule out their foundational relevance.

A second place Kripke comes close to considering something like the Naive Reply occurs in a second footnote given below.

> Few readers, I suppose, will by this time be tempted to appeal to a determination to "go on the same way" as before . . . Some followers of Wittgenstein. . . have thought that his point involves a rejection of 'absolute identity'. . . I do not see that this is so, whether or not doctrines of 'relative'

> identity are correct on other grounds. Let identity be as 'absolute' as one pleases: it holds only between each thing and itself. Then the plus function is identical with itself, and the quus function is identical with itself. None of this will tell me whether I referred to the plus function or quus function in the past, nor therefore will it tell me whether to apply the same function now.
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> (Kripke, 1982, p. 18 n. 13)

Here Kripke notes that appealing to *absolute identity* is of no help in resisting the skeptic. Despite a superficial appearance of relevance to the position of §1 in attacking an appeal to 'going on the same way,' Kripke's remarks here are not germane to the Naive Reply since numerical identity (absolute or relative) is not a notion of uniformity like qualitative identity or similarity (or at any rate, numerical identity is not a notion of uniformity used in the Naive Reply).

Of course, one might wonder whether Kripke's point here might be *extended* to notions of uniformity. Kripke claims that a past *intention* (which is roughly what I take him to mean by "determination") to go on in the 'same way' would be ineffective. Isn't that right whether the sameness in question involves numerical or qualitative identity? Indeed Kripke does make the analogous point in a third footnote—the final passage that comes closest to engaging with the Naive Reply.

> …it is important to see that, even if 'absolutely similar' had a fixed meaning in English, and 'similar' did not need to be filled in by a specification of the 'respects' in which things are similar, the sceptical problem would not be solved. When I learn 'plus', I could not simply give myself some finite number of examples and continue: 'Act similarly when confronted with any addition problem in the future.' Suppose that, on the ordinary meaning of 'similar' this construction is completely determinate, and that one does not hold the doctrine that various alternative ways of acting can be called 'similar', depending on how 'similar' is filled out by speaking of a respect in which one or another way of acting can be called 'similar' to what I did before. Even so, the sceptic can argue that by 'similar' I meant *quimilar*, where two actions are quimilar if…
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> (Kripke, 1982, p. 59, n. 46)

Kripke notes that notions of uniformity like regularity, similarity, etc., can be subjected to a key maneuver in the skeptic's toolkit: that of skeptical reinterpretation. Past overt use of "regular" is compatible with meaning *schregular*, where schregular continuations conform to explicit past applications of "regular" but diverge in irregular ways beyond them. This means that replies to the skeptic cannot appeal to *past* usage of the language of uniformity, including past intentions taking concepts of uniformity as objects, to circumvent skeptical paradox.

While I agree with Kripke on this point, it has no bearing on the Naive Reply which makes no use of theses about past usage of the language of uniformity, nor past intentions to use terms uniformly, but only applies *present* standards of uniformity to arbitrate between competing meanings. These present standards are fair game, and not subject to skeptical reinterpretation at the first stages of the skeptic's dialectic (when the Naive Reply is intervening).[15]

Lewis recognized the importance of this point as well, when he said: "there is nothing present in the subject that constitutes an intention to do different things in different cases; therefore he intends addition, not quaddition." It is in contexts where we find the *absence* of further intentions—not the presence of intentions to 'do the same' or 'do what is natural'—in which naturalness gets to play its arbitrating role for Lewis. Notions of uniformity function analogously in the Naive Reply. So the last footnote of Kripke's just discussed has no force against it.[16]

I mention these footnotes not to point to a deficiency in Kripke's arguments. On

[15] Kripke (1982, p. 12)—I elaborate this point a little, further below.

[16] In this footnote Kripke also seems concerned about whether sameness (and so perhaps other notions of uniformity) exhibit a relativity to respects (e.g. sameness in color, rather than shape, etc.), though he does not explain why this would matter. As far as I can see it would not, as long as the respects are not themselves semantic. As long as the respects are not semantic in nature, the facts stated by suitably relativized notions of uniformity could still not be disqualified as tools in rebutting skepticism. I suppose one might also worry that respects will sometimes vary from case to case. That could impede the statement of a precise, general recipe specifying meaning-constituting facts in necessary and sufficient terms. But this only seems problematic if we are precluded from treating meaning and rule-following as family resemblance concepts, as Wittgenstein does. For in that case it would be perfectly natural to expect that we would not specify respects for all terms, but rather to elaborate elements of a network of usages bearing various relationships to each other. The skeptic might claim that treating semantic terms, or the notions of uniformity underlying them, as family resemblance concepts somehow lands us in skepticism anyway. I find it hard to see why this would be so. (There is no indication that Wittgenstein, for example, thought that family resemblance terms were as such precluded from stating truth-conditions or facts.)

the contrary, I agree with Kripke on all the points he makes. I have merely emphasized that their force is directed against positions other than that offered by the Naive Reply. So these passages motivate a rather different conclusion about Kripke's relation to the Naive Reply : that he never seemed to consider anything like it in his text. This is already a noteworthy fact, especially given the strikingly broad range of positions Kripke does engage with. I think it is interesting to ask *why* this simple reply never gets considered (though I won't be able to explore that question here).

In any event, any reply on the skeptic's behalf seems like it will have to go substantially beyond Kripke's explicit argumentation. Given this, which of (i)–(iii) should the skeptic target?

I find it hard to see how (iii) can be rejected. As alluded to above, at the first stages of the dialectic the skeptic gives us access to all our present language with its standard interpretation. As Kripke puts it: "[the skeptic] is not questioning my *present* use of the word 'plus'; he agrees that, according to my *present* usage, '68+57' denotes 125. Not only does he agree with me on this, he conducts the entire debate with me in my language as I *presently* use it." (Kripke (1982, p. 12), emphases in the original)

Granted, the skeptic *eventually* wants to call into question present usage as well. But doing this requires first casting sufficient doubt on past meaning facts, and this is precisely what the Naive Reply would block.

Could some variation on the technique of skeptical reinterpretation be applied here? Well, the skeptic calls into question whether we can find facts that underly *all past* meanings, and then leverages our eventual failure to further call into question *all present* meanings. But perhaps the skeptic shouldn't first call into question past meanings for all symbols, including those like "+", and should instead first specifically attack our ability to ground the meanings of more fundamental terms like "regular" or our other past expressions of notions of uniformity. If the skeptic could undermine facts governing the past use of the language of uniformity first, perhaps he could leverage that into a case that there is no fact about what we mean by such terminology in the present. Once we are deprived of these present resources, the skeptic could go *back* to undermine the existence of past (and then eventually present) facts underlying the meanings of other terms like "plus" without fear from the Naive Reply.

But it is not obvious how the skeptic can execute the first stages of this plan successfully. Here is how I imagine the skeptic trying to formulate the challenge.

> Consider your past use of the word "regular" or even "regular continua-

> tion of a series", and consider some series to which you didn't yet apply these terms. For example, since you only applied these terms to finitely many cases in the past, there is some number *n* such that you never considered a 'regular continuation' of a series whose first few members count by *n*. We can say, for the sake of argument, that *n* is 5, so that you never considered the initial segment given by "5, 10, 15, 20, 25, 30, 35". I will grant you that your *present* use of "regular continuation" is one on which the most regular continuation of this series follows with the number 40. But what fact determines that your past use accords with that present one?

This is a new challenge. The problem is that meeting it doesn't call for new argumentative resources. For the series of past applications of the term "regular" *itself can admit of more and less regular continuations by present standards*. We are *given* (say) that some past use of "regular" applies to the continuation "14, 16, 18…" of "2, 4, 6, 8, 10, 12" and to the continuation "21, 24, 27…" of "3, 6, 9, 12, 15, 18", and to the continuation "28, 32, 36…" of "4, 8, 12, 16, 20, 24". We can ask, in the *present* use of the word "regular" (which the skeptic must grant us): "what would be the most regular way of continuing to use the past word 'regular', when confronted with the initial segment '5, 10, 15, 20, 25, 30, 35'?" It would obviously be a slightly irregular (in the present, accepted sense) continued use of the past word "regular" to go on to apply it to the continuation "45, 55,…" or the continuation "36, 37, 38…". It is not that these represent impossible ways for us to mean the word "regular" in the past. It is rather that to mean it in those ways would require special circumstances, just as would be required to use a word to express quaddition. Absent such circumstances, "regular" as used in the past will take on the meaning corresponding to the most regular continuation of its past privileged use. And that is the continuation that corresponds to present use of the term "regular"—i.e., one on which the most regular continuation of "5, 10, 15, 20, 25, 30, 35" is given by "40, 45, 50,…". In short, *present* applications of notions of uniformity can be used to salvage *past* ones via the Naive Reply. So the skeptic makes no progress by trying to target those notions specifically.

Given that the Naive Reply appeals to present usage that the skeptic grants, the only other reason I can see for trying to forbid appeal to the language of uniformity would be to claim that it is used to state *semantic* facts. Semantic terminology, presently used or not, is dialectically ruled out because we are supposed to answer the skeptic with *non*-semantic facts to ground the semantic ones, lest we lapse into a form of semantic

primitivism.

But notions of uniformity plainly state non-semantic facts. Water can drip from a leaf in a more or less regular pattern. That fact about the dripping is hardly a semantic one. The colors of two flowers can be more similar to each other than they are to the color of a third flower. This is a fact about colors, and their relationships, not about meanings. There would be innumerable regularities, irregularities, similarities, dissimilarities, etc. present in a world with *no* physically realized semantic properties—no beings that spoke or inscribed their thoughts, for example. So there is no hope of claiming that notions of uniformity are out-of-bounds for smuggling in semantic concepts.

For reasons like this, I feel the skeptic will have a hard time denying (iii). Denying (ii) is perhaps even harder. It is clear that ordinary speaker judgements involving concepts of uniformity track the extensions of past usage that match the interpretations we would assign to words. If you give even a young student the first eight evens and ask them to continue the series in the "most regular" way it is perfectly clear what counts as satisfying that command. For the same reason virtually any other series of past applications of terminology will have a 'most regular' extension, or one which applies to 'most similar' things, etc., that lines up with the terminology's standard interpretation. This is probably why Lewis hardly elaborates how naturalness—insofar as he presumed it would track ordinary notions of uniformity—will give us the results we need to avoid skepticism. He took this to be obvious. What needed defense, he thought, was the legitimacy of appealing to the notions in the first place.

If we also grant (i) (on which more in a moment), then the skeptic requires some kind of error theory for these judgments. He must acknowledge that there are facts about which continuations of past usage are most regular or most similar, and then deny what appear to be obvious, ubiquitous claims about what those facts are. I do not know what the basis for this error theory could be. It seems desperate and ad hoc.

Given all this, I think the skeptic's best response to the Naive Reply is to try to deny (i): to say that notions of uniformity aren't usable to state facts. In what sense of 'fact' must the skeptic claim this? Well, in whatever sense they want to deny there are 'meaning facts' in the first place.

Here we run up against a tricky issue of how to interpret the nature of the skeptic's desired threat. While there are subtleties here,[17] I don't think we need to enter into

[17] E.g., Kripke eventually countenances a 'skeptical solution' on Wittgenstein's behalf that may end up deflating the notions of fact or truth along the line of a redundancy theory (Kripke, 1982, 86)). See

them. This is because there is a strong *prima facie* case that the language of uniformity is fact-stating, and usable to state truth-conditions, in just the way that any other area of apparently factualist discourse does. What I take to be essential for a predication to express truth-conditions (even broadly inflationary ones) is that there be 'conditions under which' what was said or expressed through the predication is true. That is to say, there is a partition among possibilities into those that accord with what was thereby said, and those in which are not in accord with it. And (again *prima facie*) notions of uniformity meet that standard. Notions like 'similar' (perhaps relative to a respect) partition the world into pairs, notions like 'regular' partition processes and events, and so on. True, there will be vagueness and ambiguous cases. But this is fine as long as there is parallel vagueness and ambiguity in the semantic case (which, to all appearances, there is).

Here is another way to put the matter. There was no special reason *prior* to skeptical inquiry to doubt that the language of uniformity was usable to state facts or truth-conditions in a manner like any other clearly factualist form of discourse. And there do not appear to be any reasons *post* skeptical inquiry to do so either. What I've been arguing so far is that the kinds of techniques Kripke deploys on behalf of the skeptic (like skeptical reinterpretation) cannot *establish* the non-factuality of the relevant sphere of discourse. Rather, if anything, the success of the skeptical techniques *presupposes* this independent form of non-factualism.

This leaves open that the skeptic could construct an inventive case against the factuality of the language of uniformity. I won't try to prejudge the existence of such a case here. I merely want to emphasize that it would have to be novel. Nothing in Kripke's text gives us any reason to doubt the factuality of the language of uniformity, since the skeptical arguments for forms of non-factualism can only succeed *given* that the language of uniformity is not fact-stating. Surely, given that dialectical situation, the burden of proof here is on the skeptic to kick-start the non-factualist argumentative machinery. What could motivate an independent skepticism distinctly for notions of uniformity I am not entirely sure.

There is obviously more to be said on all the matters I've been discussing. I certainly haven't explored all possible worries one could raise for the Naive Reply. But I think it is interesting to see how resilient this loosely Wittgenstein-inspired reply can

Byrne (1996), Wilson (1998) for a discussion of the resulting complexities in trying to attribute a form of non-factualism to Kripke's Wittgenstein.

be in a confrontation with the semantic skeptic especially if Wittgenstein in fact never countenanced any such form of skepticism. Wittgenstein was exceedingly cautious in formulating his response to foundational semantic worries, and in spite of this made open use of notions of uniformity in spelling out his views.[18] Those notions appear to have a power against the skeptic that Kripke never really considered. Whether or not something can ultimately be said to firmly shore up the skeptic's position in light of this attempted reply, there is good reason to think we will have something interesting to learn from the confrontation.

References

Anscombe, G. E. M. 1985. "Critical Notice: Wittgenstein on Rules and Private Language." *Canadian Journal of Philosophy*, vol. 15 (4): 103–9.

Baker, Gordon P. & P. M. S. Hacker. 1984. "On Misunderstanding Wittgenstein: Kripke's Private Language Argument." *Synthese*, vol. 58 (3): 407–450.

Byrne, Alex. 1996. "On Misinterpreting Kripke's Wittgenstein." *Philosophy and Phenomenological Research*, vol. 56 (2): 339–343.

Fodor, Jerry A. 1990. "A Theory of Content II." In *A Theory of Content*, Jerry A. Fodor, editor. MIT Press.

Goldfarb, Warren. 1985. "Kripke on Wittgenstein on Rules." *Journal of Philosophy*, vol. 82: 471–488.

Janssen-Lauret, Frederique & Fraser MacBride. 2020. "Lewis's Global Descriptivism and Reference Magnetism." *Australasian Journal of Philosophy*, vol. 98 (1): 192–198.

Kripke, Saul. 1982. *Wittgenstein on Rules and Private Language*. Harvard University Press.

Lewis, David K. 1983. "New Work for a Theory of Universals." *Australasian Journal of Philosophy*, vol. 61 (4): 343–377.

—. 1984. "Putnam's Paradox." *Australasian Journal of Philosophy*, vol. 62 (3): 221–236.

—. 1986. *On the Plurality of Worlds*. Wiley-Blackwell.

McDowell, John. 1984. "Wittgenstein on Following a Rule." *Synthese*, vol. 58 (3): 325–364.

McGinn, Colin. 1984. *Wittgenstein on Meaning*. Blackwell.

Schwarz, Wolfgang. 2014. "Against Magnetism." *Australasian Journal of Philosophy*, vol. 92 (1): 17–36.

Shaw, James R. forthcoming. *Wittgenstein on Rules: Justification, Grammar, and Agreement*. Oxford University Press.

Sider, Theodore. 2009. "Ontological Realism." In *Metametaphysics: New Essays on the Foundations of Ontology*, David Chalmers, David Manley & Ryan Wasserman, editors, 384–423. Oxford University Press.

[18] To be fair, Wittgenstein *did* qualify the use of notions of uniformity in important ways—notably at *PI* §208. But, as I read him, he is there qualifying the relevance of notions of uniformity for the *Justificatory* Question, and so not qualifying their relevance to skeptical inquiry as I have construed it.

Tait, William W. 1986. "Wittgenstein and the 'Skeptical Paradoxes'." *Journal of Philosophy*, vol. 83 (September): 475–488.

Weatherson, Brian. 2003. "What Good Are Counterexamples?" *Philosophical Studies*, vol. 115 (1): 1–31. doi:10.1023/A:1024961917413.

Wilson, George M. 1998. "Semantic Realism and Kripke's Wittgenstein." *Philosophy and Phenomenological Research*, vol. 58 (1): 99–122.

Wittgenstein, Ludwig. 1958. *The Blue and Brown Books*. Harper and Row.

—. 1974. *Philosophical Grammar*. University of California Press.

—. 2009. *Philosophical Investigations*. Wiley-Blackwell Publishers, Oxford, fourth edn. Trans. G. E. M. Anscombe, P. M. S. Hacker & Joachim Schulte.