Justifying Path Induction

An Inferentialist Analysis of Identity Elimination in Homotopy Type Theory

Patrick Walsh

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Department of Philosophy
Carnegie Mellon University
Pittsburgh, Pennsylvania
United States

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Abstract

Homotopy Type Theory (HoTT) has recently been challenged as to its foundational status. James Ladyman and Stuart Presnell have contested that HoTT's presentation is not sufficiently 'pre-mathematical'. I argue that indeed the presentation is not pre-mathematical and that's a good thing. I give a *mathematical* justification for the rule of inference in HoTT they find particularly problematic: Path Induction. I provide a categorical justification of both the internal structure and external representation of Path Induction. This justification, although partially mathematical, has its philosophic roots in philosophy of logic and language. Particularly, I draw from Prawitz style *inferentialism* and the notion of *Harmony* championed for example by Michael Dummett. This constellation of ideas is unified through categorical logic. In the end, I hope to have justified Path Induction in response to valid philosophical concerns.

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1 Justifying Rules of Inference

1.1 History

The history of justifying rules of inference is a long and rich one. Justification itself relies on implicit or explicit rules of inference and it has been at the center of the philosophical enterprise from the beginning. The *reasons* someone can give for a claim are often supposed to *entitle* them to that claim. I take entitlement to a norm and I believe this sort of norm-based view of inference rules is most appropriate in natural language, but I will also use it for the logical and mathematical case. A rule of inference can be expressed in formal frameworks as a sequent $\Gamma \vdash \Delta$ where both Γ and Δ are sets of formulae. Assuming for the moment that we have a language rich enough to capture any sentence we might want to express, we can read this sequent as stating a certain rule of the language game: If you establish (assert, know, believe, etc.) those sentences in Γ, then you are 'allowed' to establish (assert, know, believe, etc.) some formula of Δ. The most natural cases usually have Δ as a singleton $\{\phi\}$ so we can assert a sentence ϕ based on Γ. This is a sequent-style natural deduction presentation that goes back to Gentzen.

But in studies of proof in mathematical logic we usually find it more convenient and general to use sequents themselves as the antecedent and consequent of rules of inference. We represent inferences with a line

$$\frac{\Gamma_1 \vdash \Delta_1 \qquad \Gamma_2 \vdash \Delta_2 \quad \dots \qquad \Gamma_n \vdash \Delta_n}{\Gamma \vdash \Delta}$$

And we say that we have established the sequent below the line if we've established all the sequents above the line. This is for finite premises and we could extend this to the infinite case but will avoid doing so for our current project. We will also require the sequents to be single-succedent sequents;[1] this is where Δ_i is a formula. An intuitive example is that of $\wedge$-introduction:

$$\frac{\Gamma_1 \vdash A \qquad \Gamma_2 \vdash B}{\Gamma_1, \Gamma_2 \vdash A \wedge B}$$

[1] The antecedent is to the left of $\vdash$ and the succedent is to the right.

which we read as saying that if you can prove A with formula in Γ_1 and B with formula in Γ_2, then you are allowed to assert $A \wedge B$ with the sentences in Γ_1 and Γ_2.

Note the pattern based way rules of inference are stated. We are allowed to replace A and B with any sentences or propositions we like. But we also don't restrict the sort of formulas in the antecedent of the sequents so this rule is a schema for a potentially infinite number of inferences.

We use similar, if not so formal, reasoning in empirical matters as well. What are the conditions under which saying "that patch is red" is warranted? The answer certainly integrates quite a few implicit assumptions and if we could flesh these out, they'd go into our rule as antecedents. The way to think about the antecedents can vary based on context. The two ways I think are most appropriate are as follows:

1. The speaker (who aims to assert the conclusion) must make sure he or she is justified in asserting each sequent above the line before they're justified in asserting the sequent underneath the line.

2. The listener can infer from the speaker's assertion that the speaker must have justification for their antecedent committments.

Of course there isn't much different between these two perspectives. But it should be noted that a prominent reason why logic developed in this way was to make problem solving an *interpersonal* project. So it shouldn't surprise us that the speaker's commitments and the listener's entitlements are the same. There is some messiness in (2) since there may be many possible configurations that justify the speaker's assertion. We might ask, is the listener to assume a *canonical* way of justifying the speaker's assertion?[2]

The way this is formulated here usually suggests a foundational picture. After all, if the justification of the speaker depends on their justification of the antecedents and those are justified by futher antecedents and so on, to avoid both circularity and infinite regresses, there needs to be something like axioms or first principles. I do not take this 'suggestion' and indeed find it to be overreaching on the intuition given by the sequent style reasoning. Although we do have axioms and unjustified primitives, I don't think we need to have such stringent requirements on our justification story. I will not argue for a different program of

[2]The notion of canonicity of justifications can be though of in two ways. In the discursive view of inference, we think a justification is canonical when it fits some standardizing norm of the language game. In logic, we select a type of proof as canonical to serve as a precise notion of 'standard'. One purpose of canonicity is to allow the speaker to assume that there is a canonical proof of the heard claim without having to worry about how the speaker herself justified the assertion.

justification, but something more coherentist and pragmatic would be more in line with my view. I take the foundational view as a special case, or an extension of the norms of justification, of mine. It is not so much wrong as it is too strong. In fact, I will not be supposing a foundational story in the analysis below since it is not required for justification to make sense. I would certainly like to ward against any reductionist justifications of mathematics or type theory. I think the general set of objections to empiricist reductionist programs to be largely generalizable to anti-reductionist arguments in mathematics. But I shall not take up this thread here, although hints of it were given in the previous chapter.

Before diving in to the history, I should note that similar to Dummett in [36], I treat axioms or 'logical laws' as akin to rules of inference. In our sequent presentation, an axiom φ can be expressed as the rule

$$\frac{}{\vdash \varphi}$$

with or without an antecedent of the sequent. In Gentzen presentations we can prefer the sequent axiom rule for any proposition P

$$\frac{}{P \vdash P} \cdot$$

Since we can make many axioms or logical laws into assumptionless rules of inference, the problem of justifying rules of inference includes the problem of justifying axioms.

Clearly, the role that logical inference rules are supposed to play will impact how we justify them. For the philosopher of science, there are particular standards that seem most attractive. From Peirce's leading principles, we have materially true premises and materially true conclusions related in an inductive or probabilistic way. But for an initial justification of logical rules, we turn to Carnap's work in [25]. Traditionally, rules of inference are judged by their semantic rather than syntactic features. We can view this in part as an artifact of the prevailing interest of rules of inference in the philosophy of science especially for induction. This is because we have an empirical world, and ideas about that world, that we feel should be represented validly in our inferences. Although my project is not directly related to the philosophy of science, we use the empiricist as a good starting point for the sort of considerations that inform what we think it takes to justify a rule of inference. Carnap proposes reversing the traditional semantic order of explanation:

> Up to now, in constructing a language, the procedure has usually been, first to assign a meaning to the fundamental mathematico-logical symbols, and then to consider what sentences and inferences are seen to be logically correct in accordance with this meaning. Since the assignment

> of the meaning is expressed in words, and is, in consequence, inexact, no conclusion arrived at in this way can very well be otherwise than inexact and ambiguous. The connection will only become clear when approached from the opposite direction: let any postulates and any rules of inference be chosen arbitrarily; then this choice, whatever it may be, will determine what meaning is to be assigned to the fundamental logical symbols. By this method, also, the conflict between the divergent points of view on the problem of the foundations of mathematics disappears. For language, in its mathematical form, can be constructed according to the preferences of any one of the points of view represented; so that no question of justification arises at all, but only the question of the syntactical consequences to which one or other of the choices leads, including the question of non-contradiction. [25, p. xv]

Carnap's views developed significantly over his career and so we do not pretend to ascribe a fixed position here to Carnap the man. Indeed, Carnap's attention to semantics as well as pragmatics was such that the above view might not fit with his later views even in the same work. We will not be using 'pragmatic' below in the way he used it in [25, p. 454] where he says "Our considerations belong, strictly speaking, to a biological or psychological theory of language as a kind of human behavior, and especially as a kind of reaction to observations." For us, this is the 'language-entrance' move that we will not be discussing in detail. For now, we use Carnap's view as a jumping off point for our program of justifying rules of inference.

The classic response from Prior [119] showed why the rules can't be "chosen arbitrarily" as Carnap wished. Prior introduced a connective called *tonk* that has the introduction rule of disjunction and the right elimination rule of conjunction. If we denote tonk by $\star$, then the rules are

$$\frac{A}{A \star B}\,{\star}\mathrm{I} \qquad \text{and} \qquad \frac{A \star B}{B}\,{\star}\mathrm{E}$$

The succession of $\star$I followed by $\star$E would allow the derivation of $A \vdash B$ for all A and B: the runabout inference ticket. Each rule *by itself* seems perfectly legitimate since $\star$I is the same as $\vee$I and $\star$E is the same as $\wedge$E. Prior's point is that their *combination* necessarily factors in into whether they are legitimate or workable rules of inference.

So not just *any* inference rule and associated connective would work, given certain norms of logic like the avoidance of inconsistencies. That is, there's more to be said than what Carnap has indicated. The received view that Carnap opposes in this quote we might call the semantic approach to rules of inference. On this view, we have a prior meaning in mind when we construct connectives. Carnap's view then would be called syntactic. Of course we could view the tonk objection as the demise of Carnap's syntactic approach. We might, in the end, need to reason semantically. This would block tonk, for it does not represent any previously accepted concept. Stevenson, for instance, in [148], advocates going back to the order of explanation that Carnap was opposing. But we need not abandon our project quite yet.

Belnap's response [12] in turn was to differentiate good connectives and rules from bad ones by considering *conservative extensions*. In brief, if $\mathfrak{L}$ is our language and rules and we add a connective with some rules to get $\mathfrak{L}^+$, $\mathfrak{L}^+$ is a conservative extension of $\mathfrak{L}$ if all the propositions stated in $\mathfrak{L}$ do not change provability in $\mathfrak{L}^+$. This is a well established concept in logic now. It does block things like tonk. For A and B would be in $\mathfrak{L}$ and if $\mathfrak{L}$ was consistent $\mathfrak{L} \nvdash A \to B$ for some A and B.[3] But when we add tonk, we get $\mathfrak{L}^+ \vdash A \to B$ for all A and B. Thus a non-theorem of the original language has become a theorem in the supplemented language. So tonk is out.

Although we can convince ourselves, perhaps, of the necessity of this conservative extension requirement, it isn't obviously sufficient for inference rules. The condition really only says that the new connective, with its rules, has to be relevantly independent of the initial logic. It is not allowed to interact with the connectives already there in any unsavory way. But is this enough? If it is, then the internal structure of the rules of inference is of little to no consequence. Belnap's suggestion would amount to giving mostly external reasons for accepting a rule. And this, I claim, is problematic. We want at least part of the inference rules to be self-justifying. This is in line with a more syntactic approach to logic. Syntax is internally justified, and not referentially or externally so, as semantics seems to be.

Prawitz pursues exactly this sort of self-justifying program. He wants the introduction rules to be self-justifying.[4] The elimination rules, in contrast, are justified only by reference to the introduction rules. The criterion, for Prawitz, for the elimination rules is normalizability, or the ability to locally reduce and expand the appropriate formula in a derivation. The natural deduction calculus must be locally reducible with the new rule and connective. Clearly tonk ruins any hope of normalizing the derivations in the logic. Detour formulas would not be eliminable in general. For example, we can systematically show that occurrences of $\wedge$I followed by one of the $\wedge$E rules can be eliminated in favor of more direct

[3] E.g. a consistent system will not model $\top \to \bot$.

[4] The word "self-justifying" might put many readers on guard. I only mean that the introduction rules have no constraints to check. If we take an inferentialist perspective, we might say that this is not a case where 'asking for reasons' is appropriate. Relatedly, we might even say that the introduction rules are arbitrary, like Carnap wanted.

reasoning:

$$\dfrac{\dfrac{\begin{matrix}\mathcal{D}_1 \\ A\end{matrix} \quad \begin{matrix}\mathcal{D}_2 \\ B\end{matrix}}{A \wedge B}\,{\scriptstyle \wedge I}}{A}\,{\scriptstyle \wedge E1} \quad \text{reduces to} \quad \begin{matrix}\mathcal{D}_1 \\ A\end{matrix}$$

But it should be clear that there is no hope of reducing similar detours of tonk:

$$\dfrac{\dfrac{\begin{matrix}\mathcal{D}_1 \\ A\end{matrix}}{A \star B}\,{\scriptstyle \star I}}{B}\,{\scriptstyle \star E} \quad \text{reduces to} \quad ?$$

where in general we don't have a proof of B ready to hand.

We might be concerned that this too feels like an external requirement for rules, much like the conservative extension requirement. And although this is partially true, Prawitz only applies the normalization requirement on the elimination rules. The introduction rules can be included in our logic without any worry about conservative extension, for the only changes to the logic by adding introduction rules are those sentences with the new connective — so there are no changes in the original language. But the elimination rule must maintain normalizability with respect to the introduction rule. This may feel like a compromise between Belnap and Carnap. Whereas we allow arbitrary introduction rules, we restrict the elimination rules to be meta-mathematically well-behaved. I take this to be an attractive position; one that we will explore more.

This normalization requirement is *prima facie* equivalent to the idea that the elimination rule has to be inverse to the introduction rule. This will be covered in Chapter 5 and we'll see what sense of 'inverse' we mean here.

1.2 Dummett

Michael Dummett's analysis of logical laws and inference rules matches up nicely with this approach. There are some differences to note although I take my program to be quite similar to his. The largest difference is the context in which we take our meaning theories to be. Where Dummett's *verificationism* is supposed to hold in general, for language, my inferentialism is restricted to mathematical and logical contexts. This creates a sort of tension with respect to the rules considered. However, enough is said in Dummett about the logical case that I will elaborate on exactly what I take from his work.

In [36], Dummett discusses the plausibility and justification of logical laws and we'll include the inference rules there for convenience. Dummett makes an important distinction in his analysis between compositional and holistic theories of meaning. The meaning of expressions (in whatever units you please) in a holistic theory depends on the meaning of all the other expressions in a given set. We can be global holists with respect to meaning where the meaning of a sentence depends on the entirety of the language. Although this seems an incredible claim, Dummett points out that its bite comes from a denial that a meaning theory is possible. At least it denies the sort of meaning theory that compositional theories would exemplify. Compositional theories, as the name suggests, assert that the meaning of a sentence, for example, is a product of the meaning of its constituent parts. This allows for analyzing wholes in terms of parts instead of the other way around as with the holist.

But where there is opposition of this kind, there are sure to be intermediates. These intermediate theories would fall into something like a *molecular* theory of meaning.[5] That is, there are clusters of language that act as the wholes with respect to which the meaning of the parts are specified. For purposes of this paper, I take it that mathematics is mostly an autonomous language game. There is always danger is using a word like “autonomous” when it is clear that in many respects mathematics is not autonomous. The sense of autonomy that I want to use here only says that the meaning of mathematical statements only depends at most on all other mathematical statements. The cognitive and historical fact that we enter mathematical language from non-mathematical language does not contradict this. But the meaning of mathematical concepts does not *only* depend on the meaning of non-mathematical concepts. So perhaps we can be molecularists in our meaning theory and take mathematics as a molecule.

A major theme of this work is a resistance to externalist programs of justification. I take this externalist requirement to be the underlying problem in justificatory framework used in [73] It is also exemplified in many accounts of mathematical foundations, especially with respect to categorical foundations as in [84] and [42].[6] I resist deriving our criterion of justification from a privileged vocabulary of intuitive and *pre-mathematical* notions. It is rather widely assumed that an analysis of the foundations of mathematics requires establishing some supportive structure that is only *extra*-mathematical. Although my account includes plenty of non-mathematical considerations, they are of a broadly methodological flavor in the study of mathematics. That is, where [73] requires the entire justification of our axioms and rules to be non-mathematical, I think mathematics may well play a role in justifying a foundations. The circularity worry looms large in considerations of foundations and it is understandable to wish for a purely external justification. But the common mentality of “we have to start somewhere”

[5] My position is largely in line with that expressed in [145] with respect to the meaning theory.

[6] Indeed when revisiting Feferman’s [42] in a special issue of the *The Review of Symbolic Logic* [43], the foundational concerns of the participants were often externalist, as I’d characterize them.

with respect to the axioms of logic, thought, or perception often lead to assuming there is a foundations to be found or established. And yet the desire to always have justification at hand for the axioms of one system in terms of another system contradicts this dictum. A molecularist or holistic approach to linguistic meaning allows the justification story to be without foundation. It may require more in the way of explanations on how we learn the language and how it was constructed in the first place; it does *not* require a special story with respect to the axioms or logical laws.[7] As Dummett points out,

> A holistic conception of language therefore does not merely *allow* an arbitrary stipulation of the logical laws to be regarded as holding: such a conception is *demanded* by the claim that they may be arbitrarily stipulated. [36, p. 230]

Given our discussion in the previous section, we might wonder how we are to reconcile this claim with our goal of letting the introduction rules be arbitrary but the elimination rules constrained. If holism is the only option with arbitrary logical laws, but we want something like semi-arbitrary laws, can we safely land in molecularism? I think we can. To see how this is possible, we give a more detailed look at the type of meaning theory this project relies on. Dummett called the meaning theory that located the meaning of a sentence in the set of canonical warrants for its assertion *verificationism*.[8] That is, the meaning of a sentence is the 'standard', or canonical, way of verifying that sentence's truth. He called the related meaning theory that takes the set of consequences of an assertion as constitutive of its meaning *pragmatism*. This is what the listener can infer from the assertion of a sentence. The program described in the next section can be seen as the combination of Dummett's verificationism and pragmatism. And it is to this program we now turn.

1.3 An Inferentialist Treatment

In this section, I explain the inferential basis on which the next chapters will be based. We can take inferentialism as the philosophical underpinning for the category-theoretic work and conceptual analysis to follow. I focus on two aspects with respect to inferentialism: The relevance to the current project and the way it will justify my analysis of identity in Chapter 6.

[7] Neil Tennant has explored such an explanation on how we arrived at logic through evolutionary processes in [153].

[8] I think Martin-Löf best expresses the distinction between Dummett's *verificationism* and the verificationism of the 1930s and the logical positivists. In Dummett and Martin-Löf's theory, "what is equated with verifiability is not the meaningfulness but the truth of a proposition, and what qualifies as a method of verification is a proof that the proposition is true" [95, p. 26].

First, however, I outline what inferentialism asserts as a meaning theory. Roughly, inferentialism says that the content of a concept is determined by the inferential role that concept plays in the language. This is in contrast to *representationalist* theories that require the meaning of a concept to be in reference to something (mostly) language independent. We can imagine a theory that says the meaning of a concept is its denotation or reference. The canonical example is usually taken to be physical properties. If we want to know the content of the concept "redness", we might naïvely hope that it is sufficient to group all the red things together and say that "redness" is that which all elements of this class have in common. We might further require that this thing they have in common has to be somewhat explanatory of their having this property. It wouldn't do, it seems, if all the red things happened also to be round things. It would be a mistake in this case to say that what we now call roundness is actually part of redness by an *accident* of the extension of "redness".

This harkens back to an essentialism about properties. Essentialism in this sense would say properties are those things which classes have in common. Naturally, this strong and simple sort of essentialism runs into rather large problems. But it serves as an example of the sort of external reference that representationalist theories exemplify. I do not think it would behoove us to go into too much detail about rival meaning theories here. I take it as sufficient to explain my position without too much in the way of contrast. Of course, I cannot avoid this completely.

Inferentialism plays on a larger pragmatic theory of meaning. Often cited for this theory is Wittgenstein's idea of "meaning as use" [164]. This does little in the way of defending a pragmatic position, but it gives a very broad idea of where I stand. The idea, as opposed to an externalist view, is that linguistic expressions have meanings only explicable in terms of their use in a language game. Moves in a language game provide the framework in which the meaning of expressions is fixed. What it means to fix a meaning will be dealt with below, but for now it is interesting to note a consequence of a pragmatic meaning theory. We need not require meanings to *represent* some preconceived notion of that concept. We do not need to know redness in order to check whether our concept "redness" captures it correctly. There need be no such check in a pragmatic meaning theory.

This will help us avoid what I take to be the downfall of the argument in [73] and [97]. Specifically, it allows us to skirt the question of whether the identity type captures a *pre-mathematical* notion of identity. We may be interested in such a question for certain other purposes but the justification of the identity type does not require it. Although not explicit in their work, it seems that the *pre-mathematical* requirement expresses a desire that the mathematical language game be representative of some non-mathematical language game. This is precisely how I understand representationalist theories. In general, they require the meaning or justification of concepts in one language game be expressed with reference to another, sometimes privileged, language game. It is here that I depart from [73] by endorsing a pragmatic meaning theory instead. Thus the analysis and justification of a concept is internal to a language game and the requirement that it be reducible or 'grounded' in another seems to me to miss the point. It would at least require a preference over languages so that mathematics is necessarily dependent on basic, pre-mathematical intuitions. It is this requirement that is left unargued for; I'd suggest that we leave it so.

The nature of what this 'inferentially determined conceptual content' is and what is intended by 'inferential role' need to be further explicated. It seems question begging to define conceptual content to be that which is fixed by inferential roles. Although this is not itself the argument, it is not far off. The inferentialist asks what more could be 'in' a concept than the grounds for and the consequences of using that concept in language. If there is meaning to a concept which is not used in the warrant or a consequence of its ascription, we might wonder how we are to find this remainder. If we are talking about inferentialism for natural languages, the common point of contention is with empirical language-entry and behavioral language-exit moves. But we are not talking about the broad context here; we are talking about mathematical language.

It is an interesting question whether mathematics has similar language-entry and language-exit moves. Tennant gives three accounts that he finds possible situations for mathematics' entry and exit moves in [152, p. 9]. Since language-entry moves are perception or observation, in the empirical case, we might see the analogue in mathematics as 'perceiving' a proof as a proof. This entry move requires a concept-wielding mathematician to use. The exit move would be to use the proof in an assertion. Tennant specifies that it would be a "speech act of *assertion backed by proof*". This option has some merit, but I find the exit move a bit odd given the entrance. Should it not be the presentation of a proof, for others to identify or not *as* a proof?

A second option is that mathematics, being closely related to the empirical sciences, has similar entry and exit moves. In sympathy with Field's [45], Tennant outlines a scientific analogue of the entry and exit moves for mathematical language. The expression of scientific laws in mathematics would be the entry and the testing of mathematically (or merely deductively) derived predictions in empirical tests would be the language-exit moves. Although I share little sympathy with Field's project, these are serviceable analogues for the language of mathematics. It might not be appropriate for all of mathematics, but mathematical science and scientific mathematics might benefit from such an analysis.

The last option is that there are no language-entry or language-exit moves. This would be amenable, Tennant says, to Prawitz and Dummett in that it allows mathematics and logic to be extended to language fragments that require language-entrance and exit moves. I don't think this is much needed for the Prawitz or Dummett line of argument, but it still remains my favorite option. At the very least I plan to remain agnostic as of now to the fleshing out of the inferentialist moves that are not language-language moves.

1.4 Meaning and Truth

The concepts of meaning and truth have both found themselves at the center of contemporary analytic philosophy. Long before even the 'linguistic turn' [18] in philosophy, a preoccupation with meaning and truth has been a certain norm. No philosophy can wholly avoid the question and neither do I here. I will say that although my remarks will be relatively summary, they are supposed to support the work on identity that can be found in the next chapters.

I take as influential the works of Michael Dummett, especially in *The Logical Basis of Metaphysics*. Although my program is not whole-cloth taken from Dummett's work, it would be beneficial to indicate the relevant parts for the current project. The relevant parts are perhaps unsurprisingly with respect to meaning. The verificationist and inferentialist programs are, in a direct sense, contrasted with Tarski semantics. Where Tarski's schema fixes meaning to evaluate truth predicates, a Dummettian or inferentialist analysis will fix truth (or truth preservation) to explain meaning.

It is a relevant bit of sociology that professional mathematicians generally equivocate truth with (having a) proof, especially in non-formal systems. We can make sense of this by affirming that meaning depends on truth, not the other way around. Both proof-theoretic semantics and the inferential program used here are ways in which to express this relationship. When we take inferential moves — which implicitly require a notion of validity and truth — as the basis for our meaning theory, we find that meaning can be fixed in a mathematically appropriate way. Mathematical logic, and proof theory, give us the tools we need to talk about 'inferential roles', 'inferential moves', and of course 'proof' in a formal system. In Chapter 6, I use the categorical representation of mathematical logic to pin down more general notions of inference.

In order to explicate how meaning can be determined (or 'fixed') via inferential roles and moves, I need to first explain the success criterion for my theory. One of the first criterion that a meaning theory traditionally needs to satisfy is some sort of reason why the analysis is that of *meaning* at all. This is a significant challenge for representationalist and similar programs. There should be a way to indicate that whatever philosophy of meaning one holds is indeed about *meaning*. Under representationalist or platonic philosophies, a challenge much like the naturalistic fallacy can be levied. They may give us elaborate ways to think of meaning and show painstakingly that it coincides with our intuitive ascription of meaning to expressions. But we are left with the question of whether what the representationalist (or other non-pragmatic philosophies) posits *is* meaning or simply correlated with it.

This challenge is only well-formed when we imagine the meaning to be in a sense independent of the use of the expression. Explaining that the theory matches our use is only a part of the program for representationalists; it is to give credibility to the theory, not necessarily justificatory merit. If we opt for a pragmatic view of meaning, we immediately avoid such a challenge. For it does not make sense to follow up the statement of a pragmatic meaning theory with a question of whether it really captures *meaning*. For the pragmatic theory, there is no meaning that is not dependent on use. There is no independent meaning that our analysis has to 'capture', and so the criterion is lifted.

Of course we must then ask why the pragmatic theory can hold that meaning is so dependent and neatly avoid this criticism. But this too presupposes an anti-pragmatic view of language. As if meaning is independent of use in some respect and the pragmatist has to glue this remainder back into use. I take it as an unconvincing challenge that pragmatic meaning theories fail to account for meaning 'proper'. At the theoretic level, then, a pragmatic theory of meaning seems question begging. If the justification relies on a denial of the independence of meaning and use while at the same time an affirmation of the dependence of meaning and use, we seem to have a confusion. But the denial of this sort of independence is a meta-theoretic claim.

The sorts of questions we allow in our metatheorizing exclude those of the foundationalist and the representationalist. So while it is true that meaning-as-use as a thesis can seem question begging to the representationlist, I find no reason to avoid this consequence. To put it even more polemically, I'd side with certain 'therapeutic' views like those of Wittgenstein and Rorty [128] when it comes to the appropriate questions asked of a meaning-theory.

A criterion that I think *does* need to be satisfied is an explication of identity and non-identity of meaning between expressions. The inferential programs do so. Two expressions are identical when they have the same inferential role. Two expressions are different when their inferential roles are different. For the identity of meaning, then, we pass the buck to the identity of inferential roles.

I believe this definition of the identity of meaning would not work in the full natural language setting — or at least I think it is a substantially different project.[9]

[9] Of course there are prominent challenges to theories that claim to fix synonymy. Something like the analytic/synthetic distinction might well creep in. I do take the challenges of Quine [123] seriously and add that by changing the discussion to logical synonymy mathematics narrowly,

As our context is helpfully constrained to mathematics and type theory, it is perhaps more reasonable that synonymy be better defined. Likewise, the motivation for considering inferential roles to constitute the criterion of identity among (the meaning of) expressions stems from the fact that mathematics has an amenable 'space of reasons'. The question of meaning then becomes how we are to fix positions in such a space of reasons.

The space of reasons[10] for Sellars, does rely on a logic of reasons in the language game and so we need a logic in which mathematics is discursive. It happens that mathematics has a ready logic of reasons: proof. It is tricky to pin down 'reasons' in natural language because we get subtleties of explanation and justification, but for mathematics if someone asks why X asserts Y, we can simply point to X having a proof of Y. In this way, we get relatively more grip on where concepts are located in the space of reason.

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we avoid the charges expressed there.

[10] Sellars coins this lovely phrase in *Empiricism and the Philosophy of Mind* with "The essential point is that in characterizing an episode or a state as that of knowing, we are not giving an empirical description of that episode or state; we are placing it in the logical space of reasons, of justifying and being able to justify what one says" [139, § 36].

Appendices

A Appendix

The following proof that Path Induction holds in certain hyperdoctrines is due to Steve Awodey.

Let $(\mathbb{C}, \mathcal{P})$ be a hyperdoctrine. We have a functor

$$L : \ \mathbb{C}/X \to \mathcal{P}(X)$$

defined on an arrow $\alpha : A \to X$ by

$$L(\alpha) := \exists_\alpha \top_A \quad \in \mathcal{P}(A)\,.$$

We define the *comprehension functor* R_X as a right adjoint.

$$\mathcal{P}(X) \underset{L_X}{\overset{R_X}{\underset{\leftarrow}{\overset{\rightarrow}{\top}}}} \mathbb{C}/X\,.$$

This adjunction has a unit η at any $\alpha : A \to X$,

$$\begin{array}{ccc} A & \xrightarrow{\eta} & A' \\ & {\scriptstyle\alpha}\searrow & \downarrow{\scriptstyle RL(\alpha)} \\ & & X \end{array} \quad .$$

For convenience, we denote $F := R \circ L$.

Fact A.1. *The following are equivalent:*

1. *R is full and faithful*

2. *For all predicates $P \in \mathcal{P}(X)$, the counit $\varepsilon_P : LR(P) \to P$ of the adjunction $L \dashv R$ is an isomorphism.*

3. *F is idempotent, i.e. $F^2 \cong F$, via*

$$F^2 \underset{\eta_F}{\overset{\mu}{\underset{\longleftarrow}{\longrightarrow}}} F \quad .$$

There will be two axioms that act as conditions on the hyperdoctrine. The first is as follows:

Axiom 1. *$F^2 \cong F$ as in Fact A.1.*

Definition A.1. Let

$$\mathcal{F}_X \hookrightarrow \mathbb{C}/X$$

be the full subcategory that is the image of R. That is, let the objects of $\mathcal{F}_X$ be the collection of

$$\alpha : A \to X \quad \text{such that} \quad \begin{array}{ccc} A & \xrightarrow[\eta]{\sim} & A' \\ & \searrow^{\alpha} & \downarrow F(\alpha) \\ & & X \end{array} .$$

The second axiom asserts that the $\mathcal{F}_X$ are closed under composition.

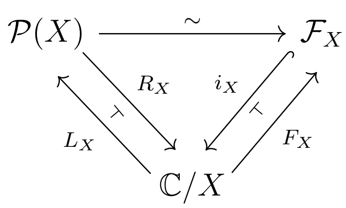
Axiom 2. *If $\alpha : A \to X \in \mathcal{F}_X$ and $f : X \to Y \in \mathcal{F}_Y$, then $f \circ \alpha : A \to Y \in \mathcal{F}_Y$,*

$$\begin{array}{ccc} A & \xrightarrow{\alpha} & X \\ & \searrow^{f \circ \alpha} & \downarrow f \\ & & Y \end{array} .$$

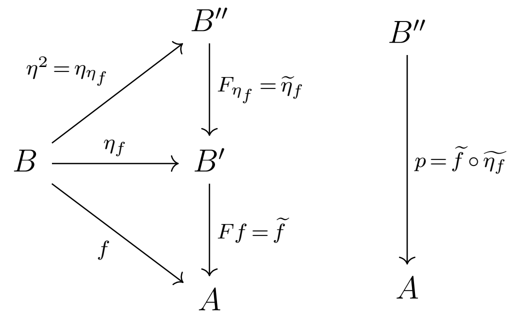
When needed, we may denote the inclusion map of $\mathcal{F}_X$ into the slice $\mathbb{C}/X$ with i_X

$$\mathcal{F}_X \underset{F_X}{\overset{i_X}{\underset{\leftarrow}{\overset{\hookrightarrow}{\top}}}} \mathbb{C}/X$$

which gives us the following set-up:



Now, consider the unit of a unit η, as in the following diagram:



And let $p := F(f) \circ F(\eta_f)$ as indicated. By axiom 2, $p \in \mathcal{F}_A$ and by axiom 1, we know that

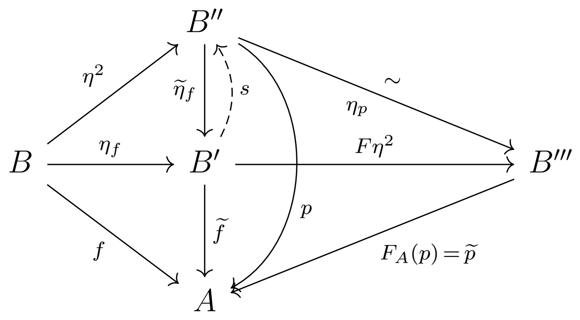
$$\eta_p : p \tilde{\to} Fp$$

is an isomorphism. This leads us to our main claim.

Theorem A.2 (Main Claim)**.** *There exists $s : B' \to B''$ such that*

1. $s \circ \eta_f = \eta^2$,
2. $\widetilde{\eta_f} \circ s = 1_{B'}$.

Proof. Define s by first applying F_A to η^2, regarded as a map $\eta^2 : f \to p$, as indicated in:



Then set:

$$s := \eta_p^{-1} \circ F\eta^2$$

For (1):

$$\begin{aligned} s \circ \eta_f &= (\eta_p^{-1} \circ F\eta^2) \circ \eta_f \\ &= \eta_p^{-1} \circ \eta_p \circ \eta^2 \\ &= \eta^2 \,. \end{aligned}$$

For (2), we require:

$$\widetilde{\eta_f} \circ s \overset{!}{=} 1_{B'} \,.$$

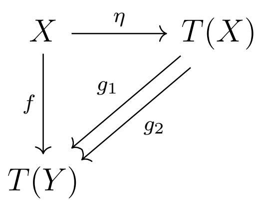
It suffices to show:

$$\widetilde{\eta_f} \circ s \circ \eta_f = \eta_f \,,$$

since η_f is universal, in the sense recalled below. Now:

$$\begin{aligned} \widetilde{\eta_f} \circ s \circ \eta_f &= \widetilde{\eta_f} \circ (\eta_p^{-1} \circ F\eta^2) \circ \eta_f \\ &= \widetilde{\eta_f} \circ \eta_p^{-1} \circ \eta_p \circ \eta^2 \\ &= \widetilde{\eta_f} \circ \eta^2 \\ &= \eta_f \ . \end{aligned}$$

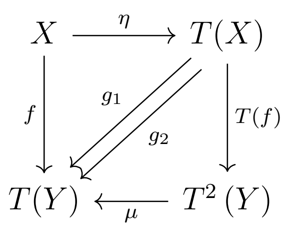
The sense in which η_f is universal is as follows: given an idempotent monad T, and a diagram



then

$$g_1\eta = f = g_2\eta \ \text{ implies } \ g_1 = g_2 \,.$$

This is so because



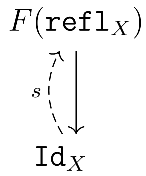
shows us that $f = \mu \circ T(f) \circ \eta$ and so

$$\begin{aligned} g_1 &= \mu \circ T(g_1 \circ \eta) \\ &= \mu \circ T(f) \\ &= \mu \circ T(g_2 \circ \eta) \\ &= g_2 \,. \end{aligned}$$

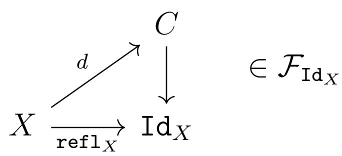
Finally, for Id-Elim, take:

$$A = X \times X$$
$$B = X$$
$$f = \delta : X \to X \times X$$
$$B' = \mathtt{Id}_X$$
$$\eta_f = \mathtt{refl}_X \,.$$

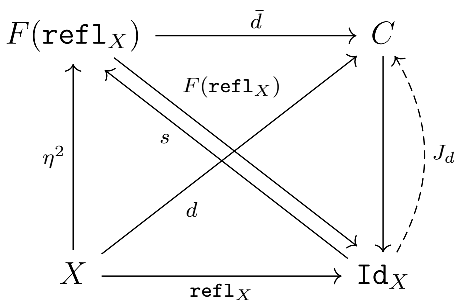
Then the Main Claim shows that the type $F(\mathtt{refl}_X)$ over $\mathtt{Id}_X$ has a section $s : 1 \to F(\mathtt{refl}_X)$



For Id-Elim, take any $C \in \mathcal{F}_{\mathtt{Id}_X}$ and $d : \mathtt{refl}_X \to C$ in $\mathbb{C}/\mathtt{Id}_X$:



to get $\bar{d} : F(\mathtt{refl}_X) \to C$ over $\mathtt{Id}_X$ as $\mu_C \circ F_{\mathtt{Id}_X}(d)$,



and compose with s to get

$$J_d = \bar{d} \circ s \,.$$

□

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