@# pandoc "Study of 'There's Something about Goedel.txt" References [1] There's Something about Goedel. [2] Symbolic Logic, Syntax, Semantics. [3] Sweet reason, a field guide to modern logic. [4] Stanford Encyclopedia of Philosophy, Model Theory. @url p://plato.stanford.edu/entries/model-theory/

@p.48 synt. consistency, synt. completeness, sem. soundness, sem. completeness @?.1 "A (semantically) sound system is a formal system that proves only truths: it is never the case that S proves α, when α is a false formula." - proves how? syntactically? if yes then how is this a semantic property? @!.1 proves syntactically. sem. soundness relates the synt. and the sem. despite the fact that (@[2].264) sem. truth (at least for predicate logic unlike the simpler propositional logic) in RL 'truth is determined relative to a model'. @?.2 I thought that Tarski's definition of semantic truth determines truth relative to the formal language! @?.3 What is the point of the truth coming from the semantics!? @!.3 The point is that the goal is to formalize and that is all. The truth has been already 'informally established' and hence we already know the semantic truth, or at least some core of it which we wish to preserve intact after formalization. @?.4 What makes a sentence semantically true within Tarski's definition of semantic truth @!.4 Informal proofs @?.5 How can a proposition be true in 'all models'? (after http://mathoverflow.net/questions/24350/what-does-it-mean-for-a-mathematical-statement-to-be-true) @.5 Tarski allowed for a 'calculus of translation' between two specific kinds of languages. @.5 Tarki's definition of truth is no more than 'book-keeping' between the two sides of the 'translation'. @quote[http://web.mat.bham.ac.uk/R.W.Kaye/logic/tarski]: " Tarski himself seemed at this time to regard his definition as methodological book-keeping, and not as useful mathematics, though it clearly is an important step before the question of completeness of first-order logic can be stated let alone proved. (The first proof of the Completeness theorem for first-order logic is due to Gödel in 1930.) In much more advanced work and certain specific circumstances (for instance the theory of satisfaction classes in models of Peano Arithmetic) this definition of truth has genuine mathematical importance, but in most contexts it really is just book-keeping, and so the discussion is relegated to these web pages. "

@?!.5 https://books.google.de/books?id=9Iw33PRksdQC&pg=PA101&lpg=PA101&dq=what+makes+a+sentence+semantically+true+in+tarski's+sense&source=bl&ots=HtMnMIsMUj&sig=zOSkaBc1-IlY*Ur*63jUINzOKB0&hl=en&sa=X&ved=0CE0Q6AEwCGoVChMIptG-sNDHxwIVwbcUCh1*Dwl0#v=onepage&q=what%20makes%20a%20sentence%20semantically%20true%20in%20tarski's%20sense&f=false @?!.5 proof of the existence of semantically sound formal languages https://en.wikipedia.org/wiki/Formal*system https://en.wikipedia.org/wiki/Model_theory This reduces to a proof of soundness as in: https://en.wikipedia.org/wiki/Soundness Here is is for first order logic, let us study it next: http://web.mat.bham.ac.uk/R.W.Kaye/logic/soundness

of note in chinese: http://jpkc.fudan.edu.cn/picture/article/235/42/cd/67a7af5f4a31b24df6c4360889a0/98a37476-547e-4edb-95d5-fd5b1169d62d.pdf @?.6 Is not truth in the semantic language something that can be chosen at will? Is there not a demand of semantic consistency that @p.48 forgets to mention? @!.6 This seems the contain the answers to most of our many questions arising from @p.48: @quote[Mathematical Models in Linguistics, p202] " A formal system is consistent if it is not possible to derive from its axioms both some statement and the denial of that same statement. An inconsistent system cannot have a m odel, since no actual statement can be simultaneously true and false; hence one way to show that a system is consistent is to exhibit a model for it. It is useful that we have both a syntactic and a semantic characterization of consistency known to be equivalent, since one is easier to apply in some cases and the other in others, In particular, when a system is inconsistent, it's usually easier to demonstrate that syntactically than semantically. That is, it's usually easier to derive a contradiction from the axioms than to prove by a meta-level argument that the system has no models Conversely, when a system is consistent, it's usually easier to show that semantically, by finding a model, than to demonstrate that it's impossible to derive a contradiction from the given axioms. When one doesn't know the answer in advance, it may be necesary to try both methods alternately until one of them succeeds. " This then puts us on a potentially better track and leads to the next two questions: @?.7 "we have both a syntactic and a semantic characterization of consistency known to be equivalent" contradicts all our ideas. Why is this true? @?.8 Why does finding a model imply consistency? This probably indicates that we do not understand what a model is. @.8 to answer this we distinguish absolute and relative consistency, noting how exactly relative consistency functions from: @quote[http://web.mnstate.edu/peil/geometry/C1AxiomSystem/AxiomaticSystems.htm] " A model of an axiomatic system is obtained if we can assign meaning to the undefined terms of the axiomatic system which convert the axioms into true statements about the assigned concepts. Two types of models are used concrete models and abstract models. A model is concrete if the meanings assigned to the undefined terms are objects and relations adapted from the real world. A model is abstract if the meanings assigned to the undefined terms are objects and relations adapted from another axiomatic development.

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Consistency is often difficult to prove. One method for showing that an axiomatic system is consistent is to use a model. W
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" Note that even absolute consistency is conditional on the assumption that the 'real world is consistent'. However, we have doubts about this quote since it does not seem to be echoed anywhere, however, we have a quote that could also help: @quote[3.p399]@tag[*] " There are two fundamentally different definitions of implication, and the discussion of the last section dramatizes that difference. One definition is that A implies B iff there is a deduction of B from A using the introduction and elimination rules. This definition is often called syntactic because only the syntax or grammar of the language is needed to formulate the definition. The other definition of implication is that A implies B iff in every structure which makes A true, B is true. This kind of definition is often called semantic because it appeals to meanings or interpretations of wffs, specifically, to structures and concepts like "true." Similarly, we have two definitions of a valid wff, one syntactic and one semantic. A wff is syntactically valid iff there is a deduction of it without premises. A wff is semantically valid iff it is true in all structures. We can focus our attention on either implication or valid wffs because the implication A, B, C .\ D is the same as the validity of the wff (A&B&C)=>Din both cases. Thus the two definitions agree on the wffs that each declares valid iff they agree on the implications or valid arguments they accept. As we saw in the last section for thousands of years a major goal of mathematics was to find a deduction of Euclid's fifth postulate from the first four—in other words, to show that postulates 1 through 4 imply postulate 5 syntactically. In fact, this goal is impossible. The discovery of this impossibility, however, did not come syntactically. Instead, it was shown that the first four postulates do not imply the fifth semantically. But why should the discovery that the first four postulates could be true in a structure even though the fifth was false discourage the search for a formal deduction of the fifth from the other four? The brief answer is the "completeness theorem," which itself was not proven until the twentieth century. The completeness theorem states that syntactic implication is equivalent to semantic implication: Both the method of deductions and the method of structures yield the same answer to every question of the form "does A imply B ?" Sometimes the claim that syntactic implication is contained in semantic implication is called the consistency theorem. It assures us that the precise formulation of our introduction and elimination rules is correct; those rules can never lead us from truth to falsity. The inverse claim that semantic implication is contained in syntactic implication is occasionally also called the completeness theorem. It assures us that our rules are complete; thus they enable us to prove all purely logical truths. The completeness theorem is important for a number of reasons. One is convenience. Syntactic implication is much easier to show than semantic implication (especially when the premises and conclusions are somewhat complex). It is generally easier to come up with one deduction than to survey all structures and realize that a given conclusion is true in every structure in which given premises are. Furthermore, semantic implication is much easier to refute than syntactic implication. It is generally easier to come up with one structure that refutes an implication than to survey all possible deductions and realize that none show a given conclusion follows from given premises. " We have to continue at [3.p197] which starts with the very promising @quote[3.p197]" In order to interpret predicate languages, we need a more powerful analogue of truth tables- The basis for the analogy consists of structures (or interpretations or models), A structure for a predicate wff is like a line in a truth table for a Sentential wff. The basic idea is that a structure for any predicate wff determines a truth value for that wff, just as a truth assignment for the sentence letters determines a truth value for a wff of Sentential " @note. We found out of topic books that echo our translation theory more closely, and show that our 'theory' is not related to mathematical model theory at any but the most remote level. Those are: "Systems thinking: Concepts and notions", "Method, Model and Matter". @note. Once one considers the 'triviality' of propositional logic, and then that first order logic is almost predicate logic, it begins to feel very amazing that mathematics can be expressed using first order logic. @note. Even in natural language, self-reference does NOT happen by substitution. 'this document is bad'. @note. Another possible resolution for many of our problems is @quote[http://math.stackexchange.com/questions/45781/model-existence-theorem-in-set-theory] " You are asking for the completeness theorem of first-order logic, proved by Kurt Gödel in 1929.

There are various ways to state the completeness theorem, and among them are the following two assertions:

Whenever a statement φ is true in every model of a theory T, then it is derivable from T.

Whenever a theory T is consistent, then it has a model.

These assertions are easily seen to be equivalent, by the following argument. If the first holds, and a theory T has no model, then false holds (vacuously) in every model of T, and so T derives a contradiction; so the second holds. If the second holds, and φ holds in every model of T, then T+¬φ has no models and so is inconsistent by 2, so by elementary logic, T derives φ; so the first statement holds. " ¶ @note. Answers from [http://plato.stanford.edu/entries/model-theory/] ¶¶@!. model-theoretic truth is what we seem not to have understood at all so far. This is due to the fact that, even though we encountered exactly this text before, and similar ones, the emphasis is as usual wrong in the presentation since it all sounds trivially fine when one is unsuspecting. mode-theoretic truth tries to tackles the problem, on a single sentence level, that one can, all things being equal assign any meaning to any sentence if one wishes so. This is not very fruitful and constraints have to be put in place such that only certain 'interpretations/structues/models' are allowed; how exactly this is restricted so that not 'anything goes', we hope to understand soon. It is very important here, and (strangely) for the first time, to note the difference between interpretation and model. This difference stands out in this @quote " Sometimes we write or speak a sentence S that expresses nothing either true or false, because some crucial information is missing about what the words mean. If we go on to add this information, so that S comes to express a true or false statement, we are said to interpret S, and the added information is called an interpretation of S. If the interpretation I happens to make S state something true, we say that I is a model of S, or that I satisfies S, in symbols ‘I ⊨ S’. " " so model-theoretic truth is parasitic on plain ordinary truth, and we can always paraphrase it away. " So one of the sources of our confusion was identifying 'interpretation' and 'model' while trying to motivate the model-theoretic definition of inconsistency and hence of consistency, which simply turns out to mean 'the mere existence of a model'. Consistency therefore means 'the existence of an interpretation where the sentence is true' and for a whole 'system' the 'existence of an interpretation where all sentences are true'. As to how this guarantees that that interpretation does not also contain a sentence with both true and false implementations is another question <@?.9 Notice how we are only focusing on the 'true' sentences, but by dichotomy this should prove sufficient (?). @rel[@np.10]

@----------< Before we continue, we proceed to a survey of critique of STT, that is, Tarski's semantic theory of truth. @refs Brentano's criticism of the correspondence conception of truth and Tarski's semantic theory The Enlightenment Project in the Analytic Conversation https://books.google.de/books?id=SAoDCAAAQBAJ&pg=PA386&lpg=PA386&dq=critics+of+tarski's+semantic+truth&source=bl&ots=ldVrvQurJi&sig=M_WXwYa9yTKTy1vXhIzTaELWO4c&hl=en&sa=X& IN DEFENSE OF THE SEMANTIC DEFINITION OF TRUTH @ref{http://logic.sysu.edu.cn/Soft/UploadSoft/200803/20080322162125620.pdf} @note. Most of these have the problem of assuming (just like Tarski), that what he did is more than what it is. This happens, as usual, because of very libral usage of words like 'true' in a manner that allows to draw totally unwarranted conclusions, especially in relation to the real world. @note. Of significance may be: @quote

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@-----------< While searching for 'meta-meta-mathematics', thinking about what Tarski and co are doing by making informal rules (by imposing a certain skeleton on what mathematics is, in effect, dictating some 'relations/functions' on the 'words' of meta-mathematics) and attributing (by talking about them) meaning to what meaning means, @note. This seems to be very nice and 'in english': @ref{http://arxiv.org/pdf/math/0404335.pdf} @note,vquote. This looks like an extremely validating vquote with a nice way to put things: @quote{https://existentialtype.wordpress.com/2013/07/10/constructive-mathematics-is-not-meta-mathematics/} " The concept of proof relevance in HoTT seems to have revived a very common misunderstanding about the nature of proofs. Many people have been trained to think that a proof is a derivation in an axiomatic theory, such as set theory, a viewpoint often promoted in textbooks and bolstered by the argument that an informal proof can always be written out in full in this form, even if we don’t do that as a matter of course. It is a short step from there to the conclusion that proofs are therefore mathematical objects, even in classical set theory, because we can treat the derivations as elements of an inductively defined set (famously, the set of natural numbers, but more realistically using more natural representations of abstract syntax such as the s-expression formalism introduced by McCarthy in 1960 for exactly this purpose). From this point of view many people are left confused about the stress on “proofs as mathematical objects” as a defining characteristic of HoTT, and wonder what could be original about that.

The key to recognize that a proof is not a formal proof. To avoid further confusion, I hasten to add that by “formal” I do not mean “rigorous”, but rather “represented in a formal system” such as the axiomatic theory of sets. A formal proof is an element of a computably enumerable set generated by the axioms and rules of the formal theory. A proof is an argument that demonstrates the truth of a proposition. While formal proofs are always proofs (at least, under the assumption of consistency of the underlying formal theory), a proof need not be, or even have a representation as, a formal proof. The principal example of this distinction is Goedel’s Theorem, which proves that the computably enumerable set of formal provable propositions in axiomatic arithmetic is not decidable. The key step is to devise a self-referential proposition that (a) is not formally provable, but (b) has a proof that shows that it is true. The crux of the argument is that once you fix the rules of proof, you automatically miss out true things that are not provable in that fixed system.

Now comes the confusing part. HoTT is defined as a formal system, so why doesn’t the same argument apply? It does, pretty much verbatim! But this has no bearing on “proof relevance” in HoTT, because the proofs that are relevant are not the formal proofs (derivations) defining HoTT as a formal system. Rather proofs are formulated internally as objects of the type theory, and there is no commitment a priori to being the only forms of proof there are. Thus, for example, we may easily see that there are only countably many functions definable in HoTT from the outside (because it is defined by a formal system), but within the theory any function space on an infinite type has uncountably many elements. There is no contradiction, because the proofs of implications, being internal functions, are not identified with codes of formal derivations, and hence are not denumerable. " @about{internal}. The above also provides the hint of the so-far elusive meaning of 'internal'. @note. Also of interest is this quote which hooks us to the 'most modern' mathematics, actually quite easily by the vquote above, if 'types' are our 'fixed structures' (vs fixed data). " The most obvious manifestation of proof relevance is the defining characteristic of HoTT, that proofs of equality correspond to paths in a space. Paths may be thought of as evidence for the equality of their endpoints. That this is a good notion of equality follows from the homotopy invariance of the constructs of type theory: everything in sight respects paths (that is, respect the groupoid structure of types). More generally, theorems in HoTT tend to characterize the space of proofs of a proposition, rather than simply state that the corresponding type is inhabited. For example, the univalence axiom itself states an equivalence between proofs of equivalence of types in a universe and equivalences between these types. This sort of reasoning may take some getting used to, but its beauty is, to my way of thinking, undeniable. Classical modes of thought may be recovered by explicitly obliterating the structure of proofs using truncation. Sometimes this is the best or only available way to state a theorem, but usually one tends to say more than just that a type is inhabited, or that two types are mutually inhabited. In this respect the constructive viewpoint enriches, rather than diminishes, classical mathematics, a point that even the greatest mathematician of the 20th century, David Hilbert, seems to have missed. " @concretely, this is a book we should skim through, it might be relevant to our ultimate goal of destroying mathematics for humans <@ref{http://homotopytypetheory.org/book/}. @----------->

@-----------< Book "Retrospective Survey of Calculi, volume I." @free< - We finally know better what syntactic consistency really should be called: calculus correctness. A guarantee that we cannot derive A and also derive ~A (~A not taken semantically! but formally). Nothing more. - State that we will provide multiple 'mechanically correct, undebatable' (that is syntactically consistent) calculi (to showcase the richness of formal systems). - The calculi will be provided by first stating 'demands of such and such' and showing that the calculi fit. Note that the major problem of mathematics is that it is still done in the ever confusing method of natural language which is dangerous because the implicit implementations cannot be taken out of the calculus no matter how much one tries. - At some point one will show the technically more demanding calculus to satisfy complete ordered fields. - It will be mentioned that all these calculi are apriori useless, can be used at will at own risk and with no guarantee, while welcoming texts that specifically try (in vain) to map them to the real world in way that is undebatable. - Is this exactly what Bourbaki did? time to check! @free> @-----------< Bourbaki This is very interesing! @quote[https://en.wikipedia.org/wiki/Nicolas_Bourbaki] " Heinz König: The traditional abstract measure theory which emerged from the achievements of Borel and Lebesgue in the first two decades of the 20th century is burdened with its total limitation to sequential procedures and its neglect of regularity. The alternative theory due to Bourbaki which arose in the middle of the century was able to relieve these burdens, but produced new ones. In particular its fundamental turn to inner regularity, based on the profound role of compactness, was done with the inappropriate weapons from the outer arsenal, which subsequently enforced that unfortunate construction named the essential one. All this produced serious obstacles against a unified theory of measure and integration, for example for the notion of signed measures, the formation of products and for the representation theorems of Daniell-Stone and Riesz types. " @note. This is extremely interesting!!! and gets more so as it goes! [The Ignorance of Bourbaki] {https://www.dpmms.cam.ac.uk/~ardm/bourbaki.pdf} @ppl[A.R.D. Mathias]{https://www.dpmms.cam.ac.uk/~ardm/}. Also beautiful: https://www.dpmms.cam.ac.uk/~ardm/inefff.pdf (A Term of Length 4,523,659,424,929 Synthese 133 (2002) 75--86 .ps .pdf

(A calculation of the number of symbols required to give Bourbaki's definition of the number 1; to which must be added 1,179,618,517,981 disambiguatory links. The implications for Bourbaki's philosophical claims and the mental health of their readers are discussed.))

and https://www.dpmms.cam.ac.uk/~ardm/slim.pdf

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@note. Search for 'transfinite induction a solution to self-reference' we find @quote[Truth and provability,Herman Ruge Jervel] {http://www.uio.no/studier/emner/matnat/ifi/INF5800/v11/undervisningsmateriale/hrj-TruthProvability.pdf} " Transfinite induction is given by $PROG(F) : \forall x. (\forall y < x.Fy \to Fx)$ $TI(\alpha,F) : PROG(F) \to \forall x < \alpha.Fx$ Let us now assume that we have a system for first order arithmetic with ordinals and one special predicate letter F. We have shown that we are able to prove $TI(\alpha,F)$ where $\alpha < \varepsilon 0$ In this section we shall prove that we cannot do better. To do this we introduce a new system with the following infinitary rule, called progressionrule, for each ordinal α $F0$ $F1$ ... $F\beta$ ... $F\alpha$ where $\beta < \alpha$ In this system $PROG(F)$ is derivable. Furthermore the system with ω-rule and progressionrule admits cut elimination and the estimates of height are the same. The only place where the new rule can be involved in cuts are with atomic cuts and cutformula F. Assume now that we have proved in the ordinary first order theory $TI(\alpha, F)$ Then we can imbed this derivation into the system with ω-rule and progres- sionrules. There we can derive $F\alpha$ This derivation is of height $< \omega 2$ and of finite cut rank. The cut elimination gives a cutfree derivation of height $< \varepsilon 0$. Now look at such a cut free derivation of $F\alpha$. This derivation can only contain progressionrules and we see that α must be less or equal to its height. Therefore $\alpha < \varepsilon 0$ This is interesting. It gives us a concrete G¨odel sentence. The sentence $TI(\varepsilon 0,F)$ is not provable in first order arithmetic — and we can state Gentzens result. Theorem 35 (Gentzen) $\vdash P A\ TI(\alpha) \Leftrightarrow \alpha < \varepsilon 0$ The two sides combine different worlds Left: Provability in first order arithmetic Right: Ordertype of α is less than iterated lexicographical ordering And the proof involves ⇒: The complexity of a direct proof of $TI(\alpha)$ ⇐: Provability of transfinite induction is closed under exponentiation Gentzens result is an improvement over G¨odels incompleteness. It analyzes provability in a system using the mathematical statement $\alpha < \varepsilon 0$. " @quote[] We also find this nice direct snippet " We now want to replace the semantical assumption “only true sentences are provable within the theory” with some weaker syntactical notion. G¨odel con- sidered two notions • A theory is consistent if$\nvdash \perp$. • A theory is ω-consistent if there are no Fx with $\vdash \exists x.Fx$ and $\vdash \neg Fp$ for all pairs p We could also have formulated consistency as demanding that there are no formulas F with both $\vdash F$ and $\vdash \neg F$. Then we see that ω-consistency implies consistency. Instead of ω-consistency it may be more perspicuous to use the following consequence Theorem 6 If the theory is ω-consistent and provability is $\Sigma 1$, then $\vdash PF \Rightarrow \vdash F$ " @note. 'Relative consistency' is no consistency at all. This falls into 'SCW' (Stupid Choice of Words). As we said, this should be called 'translation correctness'. In any case, here is a validator, that also includes the kind of garbage faith common amongst imitators, breaking the 'NCR'. @quote[http://math.stackexchange.com/questions/183243/what-is-actually-relatively-consistent] " This is what a relative consistency result means: it is a result that says the consistency of one theory implies the consistency of the other. Kurt Gödel proved that if ZF is consistent, then so if ZFC+V=L (and also that V=L implies GCH). Paul Cohen proved that if ZF is consistent, then both ZF+¬AC and ZFC+¬CH are consistent. More fundamentally, ZF implies the consistency of PA. These are fundamentally relative consistency, and cannot be improved to straight consistency results. This stems from Gödel's Second Incompleteness Theorem, which basically implies acceptance of the consistency of ZF(C) is an article of faith (though one I'm happy to believe in). " But the quote does leads us to new information: @quote[Gödel's Second Incompleteness Theorem Explained in Words of One Syllable] {http://www2.kenyon.edu/Depts/Math/Milnikel/boolos-godel.pdf} " Gödel's Second Incompleteness Theorem Explained in Words of One Syllable First of all, when I say "proved", what I will mean is "proved with the aid of the whole of math". Now then: two plus two is four, as you well know. And, of course, it can be proved that two plus two is four (proved, that is, with the aid of the whole of math, as I said, though in the case of two plus two, of course we do not need the whole of math to prove that it is four). And, as may not be quite so clear, it can be proved that it can be proved that two plus two is four, as well. And it can be proved that it can be proved that it can be proved that two plus two is four. And so on. In fact, if a claim can be proved, then it can be proved that the claim can be proved. And that too can be proved. Now, two plus two is not five. And it can be proved that two plus two is not five. And it can be proved that it can be proved that two plus two is not five, and so on. Thus: it can be proved that two plus two is not five. Can it be proved as well that two plus two is five? It would be a real blow to math, to say the least, if it could. If it could be proved that two plus two is five, then it could be proved that five is not five, and then there would be no claim that could not be proved, and math would be a lot of bunk. So, we now want to ask, can it be proved that it can't be proved that two plus two is five? Here's the shock: no, it can't. Or, to hedge a bit: if it can be proved that it can't be proved that two plus two is five, then it can be proved as well that two plus two is five, and math is a lot of bunk. In fact, if math is not a lot of bunk, then no claim of the form "claim X can't be proved" can be proved. So, if math is not a lot of bunk, then, though it can't be proved that two plus two is five, it can't be proved that it can't be proved that two plus two is five. By the way, in case you'd like to know: yes, it can be proved that if it can be proved that it can't be proved that two plus two is five, then it can be proved that two plus two is five. George Boolos, Mind, Vol. 103, January 1994, pp. 1 - 3. "

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