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Class 28 (and last): Fictionalism and Plenitudinous Platonism

I. Face-value interpretations and fictionalism

I had mentioned that the fictionalist wants to say that 'The square root of two is irrational' is false.
Jonathan mentioned that 2 looks like a name, and there looks to be a problem of failure of presupposition.
That is, the fictionalist will want to say that 'The square root of two is rational' is also false.
If we re-cast the sentence, a la Russell, so that we have a definite description in the place of the name, we can call both of them false because of the non-existence of 2.
We are still taking the sentence at face value, even if we re-cast the semantics, I suppose.
But, a lover of names might want to reject excluded middle, and look to a three-valued logic.

II. A couple of other items up to clean

Another thing to clear up is the quote from Burgess and Rosen that I mangled in our last class, about the likelihood of developing new nominalist techniques.

> "As a consequence of nominalism's being mainly a philosopher's concern, this open research problem is...one that has so far been investigated only by amateurs - philosophers and logicians - not professionals - geometers and physicists; and the failure of amateurs to surmount the obstacles is no strong grounds for pessimism about what could be achieved by professionals." (Burgess and Rosen (1997) p 118)

A last thing was my dissatisfaction with my response to James in last class, about the proximity of conservativeness and consistency.
I suppose it has to do with the way Field proves conservativeness.
He shows that adding a consistent theory mathematical theory to a nominalist theory results in a new, consistent theory.
But, this proof doesn't really work, since it doesn't show that the new consistent theory is not an extension of the original nominalist one.
(I have some work on this in my dissertation, pp 37 et seq.)
Still he calls conservativeness a generalized version of consistency, in *RMM*, p 72.
See also Field (1980) p 12 et seq., for discussions of conservativeness.

III. Balaguer

We were talking last class about the Nepalese villages argument. One quick objection seems to be whether I can imagine any possible mathematical objects. Perhaps the argument is a non-starter. That is, I can imagine all possible Nepalese villages, but can I imagine any mathematical objects? Certainly not, in the strict sense of imagine: I can not form a picture of them.

Balaguer says that we need not have thick beliefs about mathematical objects, merely thin ones. That is, we can have thin beliefs about non-existent objects, like Santa Claus, so it would seem that having thin beliefs about mathematical objects is not such a challenge. I'm not sure that his response suffices, but I may just be stubborn.

Kara had raised some questions about truth, in FBP. Truth (simpliciter) is supposed to apply only to statements that are true in every model. Is choice true? There is a universe in which it is and another universe in which it is not. So, it is neither true nor false.

Consider the parallel postulate, p 64. Balaguer is saying that we could develop intuitions about set theoretic universes to parallel (sic) our intuitions about geometric spaces.

There is also truth as the intended model. There are non-standard models. Mathematicians are mainly concerned with the standard, or intended model. They are not concerned with problems of modeling. Balaguer proposes to explain the interest in the standard model sociologically, pp 64-5.

This is the central problem with FBP. There are too many consistent theories, and we need some explanation for our interest in certain ones. Balaguer can explain our interest in the standard model, for limited mathematical theories, but their applicability to empirical science. But, once mathematical theories become purely theoretical, or un-applied, this justification is no longer useful. Alternatively, Balaguer can try to explain our interest in the standard model on the basis of sociological considerations. Perhaps our interest is just the result of community standards. Still, one has to ask about the origins of those community standards.

Explaining the interest in the standard model was not the challenge for Balaguer, though. The challenge for Balaguer was to explain the reliability of our mathematical beliefs. Nelson's paper, p 2-3, is right about deflecting the internalist challenge.

[In fact, I don't really get the objection that Balaguer discusses: 'If FBP, then T truly describes some part of the mathematical realm' seems like a reasonable response to Benacerraf. Maybe Balaguer was just trying to set that up. This part seems poorly written by Balaguer.]

Compare the internalist challenge to the KK thesis. No one believes the KK thesis. So, we can be externalists about justification. Still, does FBP put platonism on par with the theory that claims that there is an external world? See pp 56, 69. Nelson discusses the internalist/externalist question on p 3.

IV. Tymoczko

1. Explain the three major characteristics of proofs. Which are the "deep factors" (61)? Why?

Comments:

They are convincing (psychological, or "anthropological" (61)). They are surveyable (epistemic). They are formalizable (logic). "Formalizability idealizes surveyability" (61). This just seems wrong, doesn't it? I mean, the more we formalize, the less a proof is surveyable. Take Coulomb's law:

Less regimented:

$$F = k\ |q_1 q_2| / r^2$$

More regimented:

$$\forall x \forall y\{(Px \wedge Py) \supset (\exists f)[<q(x), q(y), d(x,y), k, F> \mid F = (k \cdot |q(x) \cdot q(y)|) / d(x,y)^2\}$$

2. How is formalizability a local characteristic?

Comments:

Gödel's theorem shows that mathematical truth can not consist in proof within a single formal system.

3. How do formal proofs outrun surveyable proofs?

Comments:

4CT is too long to survey, in principle.

4. How does the proof of 4CT introduce empirical elements into mathematics? Why is this worrisome? Consider the electron microscope analogy. What does the computer do that is not merely calculation?

Comments:

See pp 73-4. The key to Tymoczko's claim is that there are factors about the reliability of the machine and the program. He claims that these factors are similar to those on which we rely in empirical experiments, like ones using an electron microscope. Are these different in kind from the reliability of the human calculator? In fact, the machine is more reliable, right?

On 69, Tymoczko says that the computer is not merely calculating. But, I'm not sure why. Walter, step 15.

5. How can we establish that 4CT has a surveyable proof by fiat? What is wrong with this approach?

Comments:

We can just say call appeals to computers surveyable; see p 70.

Isn't the machine table of the computer, in theory, surveyable? It is certainly formalizable. So, why isn't it like Gauss's proof?

Computers do what they are told to do; Walter p 4.

6. Compare the justifications "Simon says" and "by computer".

Comments:

The Martian mathematicians trace to Putnam's article, "What is mathematical truth?". See pp 61-3, especially.

The assumption, of course, is that Simon never says, "This statement is false," as James suggests he might.

7. Are all necessary truths known a priori? Explain.

Comments:

Traditionally, a necessary truths had to be known a priori, since we can not know from experience how things must be, but only how they are. Conversely, contingent truths could not be known a priori.

Kripke argues that there are necessary a posteriori truths, like that water is H_2O, and that hesperus is phosphorus. Similarly, he argues that there are contingent a priori truths, like that the standard meter is one meter long.

Tymoczko thinks that 4CT is necessary, but a posteriori.

8. Consider the new paradigm proposed on p 81. What makes it new?

Comments:

Hasn't guessing always had a central role in theorem proving? Where's the new?