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The 'Must' and the 'Heptahedron'. Remarks on Remarks

Mario GÓMEZ-TORRENTE

ABSTRACT: I offer some brief remarks in reply to comments and criticisms of my earlier work on logical consequence and logical constants. I concentrate on criticisms, especially García-Carpintero's charge that my views make no room for modal intuitions about logical consequence, and Sher's attempted rebuttal of my critique of her theory of logical constants. I show that García-Carpintero's charge is based on misunderstandings, and that Sher's attempted rebuttal actually reveals new problems for her theory.

Keywords: logical consequence, logical constant, modality

1. The 'must'

As I note in my introductory piece (2003), a widely shared intuition about the concept of logical consequence tells us that if a conclusion follows logically from some premises then that conclusion follows by logical necessity from those premises. The concept of logically necessary consequence is not as transparent as we might want, but it is linked somehow to the modal concepts of analytical implication, a *priori* implication and implication by necessity (*tout court*). I share the widely shared intuition, and I have emphasized the links of the concept of logical consequence with all these modal concepts in several of my writings (see, e.g., (1998/9), (2000a)). In my view, a fair requirement on the adequacy of a technical notion of logical consequence (such as the model-theoretic one) demands that no argument which is an instance of that technical notion be a (definite) instance of logically contingent implication, whatever 'logically contingent implication' ought to be understood to mean. Or in other words, I demand (i):

(i) all model-theoretically valid arguments are instances of implication by logical necessity.

Manuel García-Carpintero (2003) endorses this requirement of purely extensional adequacy, but he insists that a stronger requirement is needed. He endorses a requirement he locates in Etchemendy, that a good technical notion of logical consequence be "a conceptual analysis" of the pre-theoretic notion. According to García-Carpintero, then, we also need (among other things) something of the form of (ii):

(ii) Necessarily (all model-theoretically valid arguments are instances of implication by logical necessity),

where the 'necessarily' means something as strong as 'it is conceptually or analytically true that'. I think (ii) is certainly too strong if interpreted this way; hardly any notion intended as a technical or scientific version of some common concept has ever been

proposed as a conceptual analysis of the common notion, and there is nothing wrong in that. On the other hand, if 'necessarily' means something like 'it is metaphysically necessarily true that', (ii) is somewhat more reasonable, since often the technical or scientific version of a common concept turns out to be (approximately) metaphysically necessarily coextensional with the common notion (even if the metaphysical necessity of coextensionality was not explicitly intended). But I don't think even this weaker reading of (ii) provides a mandatory requirement on the model-theoretic notion of logical consequence. Accidentally coextensional technical concepts which are theoretically fruitful and worth exploring may be hit upon by the philosopher or scientist; their exploration cannot be banned, or even denied philosophical import.

García-Carpintero's main reason for imposing (ii) as a requirement appears to be epistemological. He says that "a 'merely material' generalization ... is the sort of generalization which we are *in a position to assert* only after checking all cases" (my emphasis); he also says that proposals of technical notions "*establish* (if anything) some modality-involving relationship between the property captured by the pre-analytical concepts and the precisely defined ones. Any purported *justification* of this kind, if it is *justification* enough and is provided for a true proposition, *justifies* a claim having a modal force absent in merely material generalizations" (my emphases). The idea seems to be that if we are to *be able to assert*, or to *know* something like (i) we cannot at the same time fail to have come to be able to assert, or to know something stronger, like (ii), for accidental generalizations cannot be known or asserted by themselves if they quantify over an infinite number of cases (as (i) does). I think this is not a good line of argument. First, it is far from clear that one cannot come to possess minimally appropriate justification, e.g. of an inductive sort, for an accidental generalization without having examined all the cases the generalization quantifies over. Second, and more important, generalizations like (i) are often put forward as theses or hypotheses without more than an inductive justification (which may or may not be sufficient to allow us to "assert" them); a justification for their necessitations (things like (ii)) need not have been provided, and for all we know need not exist. Significantly, I think, the two examples García-Carpintero mentions (the technical concepts of logical consequence and of a computable function) are cases where technical concepts are employed in the absence of more than a basically inductive justification for merely non-necessitated generalizations like (ii), codifying minimal adequacy conditions for those concepts¹.

This is not to deny the importance of finding arguments allowing us to establish (i) (or the stronger (ii), suitably interpreted) more conclusively. But García-Carpintero appears to have misunderstood me as being unsympathetic not merely to imposing (ii) as a requirement, but also to imposing (i). On the contrary, I am sympathetic to (i),

¹ In the case of model-theoretic logical consequence, I think there is a conclusive argument for (i) as restricted to first-order languages, which is basically an extension of an argument of Kreisel; but no conclusive argument exists for (i) in general. (See my (2003) and (1998/9).) In the case of a technical concept intended as a scientific version of the informal notion of a computable function, the generalization analogous to (i) for which we basically do not have more than empirical evidence is the generalization 'all functions computable in the informal sense are computable in the technical sense', known as 'Church's thesis'.

and I insist that if it were falsified then we would have a serious argument against the model-theoretic notion of logical consequence. García-Carpintero's misperception seems to arise from two sources: one is his running together and indistinctly the two modal requirements (i) and (ii), and the other is my purely exegetical view that Tarski (in the key passages of (1936) discussed by García-Carpintero) did not endorse (i) as a requirement, but merely (iii):

- (iii) all model-theoretically valid arguments are arguments such that no argument with the same logical form has true premises and a false conclusion.²

The condition on model-theoretically valid arguments embedded in (iii), *being an argument such that no argument with the same logical form has true premises and a false conclusion*, is what Tarski calls 'condition (F)'. (F), and hence (iii), unlike (i), does not contain any modal concept whatsoever. Of course, the fact that I hold that Tarski only demanded (iii) should not lead anyone to believe that I side with him on this. But the piece of exegesis is hardly disputable, despite a common, but fortunately waning misreading of the relevant passages originating in Etchemendy's work. I won't go again into the details of my exegesis; for this, I refer the reader to my (1998) and (2000b).

But García-Carpintero suggests that even the exegesis as such is wrong. According to him, Tarski explicitly enunciated (i) as a requirement. I will briefly rebut what I take to be García-Carpintero's most significant arguments for saying this.

One of these arguments appeals to his distinction between *exegesis* and *mere biography*. Allegedly, my main or only reason for claiming that Tarski demanded just (iii) is his documented skepticism towards modal notions, but not any objective exegetical ground to be found in the relevant passages of (1936) (in which Tarski would supposedly be embracing modal requirements like (i)). I could not disagree more, and I am confident that the reader of my historical papers will agree that I have fundamentally and essentially attempted to provide textual arguments for my exegetical claims. These claims concern fundamentally what the texts explicitly say, not what Tarski thought but did not say.

Not surprisingly, García-Carpintero has misunderstood some of the purely textual considerations for my exegesis that I take to be definitive. For example, he says that I have offered persuasive reasons for interpreting Tarski's use of the word 'must' in his own formulation of condition (F)³ as signaling a certain kind of universal quantification, but he suggests that this is compatible with that quantification being one over possible worlds or models existing in other possible worlds, hence a modal quantifica-

² Actually, Tarski employs a somewhat different notion of model than the one common nowadays, so 'model-theoretically valid' means something different for him than for us, but I'm glossing over this difference for present purposes. See my (2000b) for an explanation of the difference.

³ Tarski's formulation is, I recall, this: "(F) If, in the sentences of the class *K* and in the sentence *X*, the constants - apart from purely logical constants - are replaced by any other constants (like signs being everywhere replaced by like signs), and if we denote the class of sentences thus obtained from *K* by '*K*', and the sentence obtained from *X* by '*X*', then the sentence *X*' must be true provided only that all sentences of the class *K*' are true" (Tarski (1936), p. 415).

tion. This fails to take into account the fact that my only textual reasons for thinking that (F), and hence (iii), make merely non-modal quantifications (over *arguments* or *replacements*) are incompatible with the quantifications being modal. One of these reasons is that Tarski says that (F) can be formulated without problems with the help of “scientific semantics”, but obviously he could not have said this if (F) contained a modal quantification—regardless of his exact views about modality. Another textual reason is that Tarski claims to have a proof of his requirement that he doesn’t bother to give, and such a proof is clear enough under the assumption that his requirement is (iii) and that our model theory postulates some reasonable models (in fact, in my (2000b) I have noted that Tarski gave a similar proof in writings other than (1936)); nothing similar can be said of (i), even if understood as making a quantification over possible worlds.⁴ Further, there is the straightforward consideration that the ‘must’ in (F) is most naturally read as emphatic and non-modal. All readings of which I am aware, except the one just put forward by García-Carpintero, see (F) as entirely non-modal, as a condition quantifying merely over all arguments of a certain class. This is true of Etchemendyan and anti-Etchemendyan readings alike, and it should not be confused with the matter whether Tarski demanded (i)—as the Etchemendyans say—or merely (iii).

García-Carpintero claims also that if Tarski were imposing a non-modal requirement, then this would trivialize the attainment of the goal of justifying the appropriate adequacy claim; there would not be a sufficient “contrast” between the model-theoretic concept of logical consequence and the condition embedded in the adequacy claim. It is easy to see that this claim is false, and even the modest (iii) illustrates this falsity. We simply do not have a proof of (iii) for arguments of second- and higher-order languages when we take (as usual today, but probably not in Tarski’s 1936 view) our models to be set-theoretical. A convincing argument for (iii) in this case is bound to be, if it exists, highly non-trivial.

Can we expect a conclusive proof of (i)? Maybe. At least I’m fairly certain that no conclusive counterexamples to (i) have been provided. But it might be thought that such counterexamples could be extracted from Ignacio Jané’s (2003) considerations. I don’t think they could; it is worthwhile to explain why. (I’m not claiming that Jané means to use his considerations in this way. In fact, he is explicit that his worries are mainly about what is the most fruitful mathematical notion of model-theoretic higher-order validity, and not so much about capturing a pretheoretic notion.)

⁴ A curious exegetical error of García-Carpintero concerns a passage in which Tarski says that his “proposed treatment of the concept of consequence makes no very high claim to complete originality. The ideas involved in this treatment will certainly seem to be something well known, or even something of his own, to many a logician who has given close attention to the concept of consequence and has tried to characterize it more precisely. Nevertheless it seems to me that only the methods which have been developed in recent years for the establishment of scientific semantics, and the concepts defined with their aid, allow us to present these ideas in an exact form” (Tarski (1936), p. 414). The italicized text has been taken by García-Carpintero to be an indication that Tarski is referring to a modal requirement like (i), a requirement that any logician could take as “something of his own”. But the quotation in context makes it textually transparent that the “ideas” referred to are informal ideas about models and satisfaction, which can only be “presented in an exact form” with the help of Tarskian semantics.

There is a second-order sentence α such that the statement ‘ $\text{Val}_T(\emptyset, \alpha)$ ’ (in the terminology of my introductory piece (2003)) is (set-theoretically) equivalent to the continuum hypothesis; similarly with another sentence β , the statement ‘ $\text{Val}_T(\emptyset, \beta)$ ’ and the negation of the continuum hypothesis. This means that either α is a model-theoretic consequence of the empty set of premises or β is. If (i) is true, then, α is a logically necessary consequence of the empty set of premises or β is. Does this discredit (i)? It might seem so, but in fact it will be so only under one further assumption:

- (iv) all arguments (\emptyset, X) (with no premises, and conclusion X) which are instances of logically necessary implication are such that the (set-theoretic) statement ‘ $\text{Val}_T(\emptyset, X)$ ’ is not set-theoretically equivalent to a mathematically “strong” proposition.⁵

Why accept (iv)? I have no idea, and as I mentioned in my introductory piece (2003), I think there are reasonable counterexamples to (iv) (see my (1998/9)).

But aside from this, it is worthwhile to insist that a certain possible reason for accepting (iv) is *bad*. If we accepted or expected something like (ii), with ‘necessarily’ understood as ‘it is conceptually true that’, then virtually anything false of the notion of logically necessary consequence would be false of the model-theoretic notion of validity. So if we accepted (ii), and we accepted, for example, that the claim that a sentence X follows by logical necessity from the empty set of premises is not (set-theoretically equivalent to) a mathematically “strong” claim, then it would follow that the statement ‘ $\text{Val}_T(\emptyset, X)$ ’ is not (set-theoretically equivalent to) a mathematically “strong” claim. So we would have (iv). Do we have (iv), then? No, because there is no reason to accept (ii) to begin with. Compare the case of a Tarskian defined truth predicate, ‘ True_T ’. If this predicate and the pretheoretical truth predicate were conceptually related in a way analogous to that in (ii), then presumably, since the claim that a sentence X is true does not refer to sets, we would be forced to conclude falsely that the claim ‘ $\text{True}_T(X)$ ’ does not refer to sets. It is clear that the corresponding version of (ii) is to be rejected in both cases.

2. The ‘heptahedron’

The extension of the model-theoretic notion of logical consequence depends essentially on the assumed class of logical constants. What is this class? It is reasonable to say that the proponent of the model-theoretic notion ought not to answer something like “The (biggest) class of constants that makes my proposal extensionally correct”. This would make his proposal suspiciously unfalsifiable (or nearly so). An independent characterization of the class of logical constants is needed by the Tarskian. But characterizing the class is not easy. I have conjectured that it is a hopeless task if one

⁵ We saw in my introductory piece how Etchemendy used tacitly a related assumption in order to reject the extensional correctness of the model-theoretic notion of higher-order consequence. See also my (1998/9).

wants to do it without using pragmatic notions, and in particular without using more than the resources of Tarskian semantics, and in (2002) I have attacked a number of characterizations of this and other kinds. Gila Sher's (1991) characterization uses only the resources of Tarskian semantics, and in fact is a refinement of a proposal made by Tarski himself. In her (2003) she has attempted to rebut my criticisms. The failure of her rebuttal is illuminating.

Sher's proposal is, in essence, that a logical constant is one whose extension is invariant under isomorphisms of models, and that it must be assigned an extension in all models. I had claimed that predicates with an empty extension over all models, like 'unicorn', 'heptahedron' and 'male widow' satisfy this condition, but are not traditional logical constants. And if we take them as logical constants, then the sentences ' $\forall x(\sim \text{unicorn}(x))$ ', ' $\forall x(\sim \text{heptahedron}(x))$ ' and ' $\forall x(\sim \text{male widow}(x))$ ' will come out model-theoretic logical consequences of every set of sentences (along with many other sentences which are intuitively not logically true)⁶. This is really bad news for Sher's proposal⁷.

Sher's first counterobjection is that these predicates are not defined over all models, that, e.g., 'unicorn' is not defined over universes containing "non-zoological, or at least ... non-biological, objects—numbers, thoughts, planets, etc". This sounds odd; if anyone asks me if it is defined whether a planet is a unicorn or not, I will say without hesitation that it is, and that it is defined that it is not. More importantly: when I took it that Sher would make the natural assumption that 'unicorn', 'heptahedron' and 'male widow' are defined over all models, I was following her own natural usage in (1991), where all sorts of terms from natural language are considered as being defined over all models⁸. Even more importantly: if Sher insists that 'unicorn' is not defined over all models, the critic can simply create a new term, 'unicorn#', by merely stipulating that it has the same extension where 'unicorn' is defined and the empty extension elsewhere; my points go through for 'unicorn#' (and 'heptahedron#' and 'male widow#').

Sher seems to feel that the shaky claim that the predicates I gave are "undefined" over some models will sound *ad hoc*, or even that it will just postpone the difficulty (as I just noted). She proposes to adopt the stipulation I just mentioned. However, somewhat confusingly, she adopts it not for new terms, but for 'unicorn', 'heptahedron' and 'male widow' themselves. What is her defense then? In the case of 'unicorn', her defense is that the universe of some models includes a nonempty subset of unicorns. This will be so in models with "fictional" unicorns. To this I have no objection, except that her original presentation of her theory contained no indication that she

spoke of 'models' in a sense different from the one usual in model theory and especially in discussions of model-theoretic logical consequence, i.e., models as certain set-theoretical entities built out of actually existing urelements. (Much of the recent philosophical discussion over model-theoretic logical consequence is simply unintelligible without this tacit assumption.)

So Sher's models contain non-actualized elements. Her real theory is then not adequately described in (1991) and is instead closer to a theory proposed some years earlier by Timothy McCarthy, according to which a logical constant is one whose extension is invariant under isomorphisms of *possible* models (where 'possible' may mean 'conceptually possible', 'epistemically possible' or 'metaphysically possible', among perhaps other things). It was in order to undermine *this* theory that I proposed the examples 'heptahedron' and 'male widow'. Their extensions are empty in all possible models, however one looks at the matter, and so they are invariant under isomorphisms of possible models. What is Sher's defense now?

She fiddles a bit with the meaning of 'heptahedron', claiming that it is ambiguous even as I defined it, and that some of its acceptations are after all compatible with the supposition that it might not have an empty extension. She claims, for example, that some possible one-sided surfaces can be called 'heptahedra' without doing violence to the meaning I gave to 'heptahedron'—"regular polyhedron of seven faces". Of course Sher's claim turns on how we understand 'regular' and 'polyhedron'. But suppose we grant the claim. In reply I only have to insist on focusing on the intended acceptance, according to which an 'heptahedron' ought to be a two-sided surface. (Significantly, she does not develop a similar counterobjection for 'male widow'.)

Her substantive counterobjection is designed to deal with the case in which 'heptahedron' has the intended meaning. But the substantive counterobjection is puzzling. She says that the problem is that her stipulation about how to assign an extension to 'heptahedron' (in the models over which it's not defined) fails after all to capture its meaning; then she seems to suggest that without the meaning having been captured, the question whether 'heptahedron' is a logical constant in her technical sense simply does not arise. Why is the meaning of 'heptahedron' not captured by the stipulation mentioned above? Because under this stipulation the class-function assigned to 'heptahedron' that maps models into extensions drawn from those models (the surrogate "meaning" of 'heptahedron') is the same as that of other words with a different intuitive meaning, such as 'non-self-identical'.

This reply fails for two reasons. One of them is simply that, if Sher demands that the appropriate class-functions assigned to two words with different meanings be different, then there will be vast numbers of expressions to which Sher's theory will be inapplicable. For example, surely the predicate ' $x \neq x$ ' gets the same class-function as the predicate ' $\exists y(x=y \ \& \ x \neq y)$ ', but Sher would obviously want her theory to be applicable to them (and to count the two predicates as logical).

The second reason is that the objection to Sher, as noted above, can be run with 'heptahedron#'. Here we don't intend to capture a preexisting meaning, we just define the meaning of the expression *ad hoc*. If the theory of class-functions over models that Sher's theory relies on is not fine-grained enough to provide different surrogate mean-

⁶ I take this opportunity to correct an unaccountable typo in my discussion of these matters in my (2002).

On page 19 of that paper, please replace all occurrences of ' $\forall x\forall y$ ' with occurrences of ' $\forall x\exists y$ '.

⁷ William Hanson's (2003) critique of Sher is essentially the same, although my way of putting it is perhaps simpler. Notice that ' $\forall x(\sim \text{unicorn}(x))$ ' is presumably synthetic and knowable only *a posteriori*, so if it came out as a model-theoretic logical truth, we would have a serious reason to doubt the truth of (i). I agree with most of the points made in Hanson's substantial and yet pellucid discussion.

⁸ I would have thought that only artificial terms stipulated not to be defined over some models would fail to meet the condition in question.

ings for 'heptahedron#' and 'non-self-identical', the problem is a problem for the theory, not for intuitive distinctions. The theory is directly applicable to 'heptahedron#', and it yields the undesirable result that it is a logical constant.

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