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LOGIC IN SERVICE OF PHILOSOPHY OF SCIENCE

REPLY TO ISABELLA BURGER AND JOHANNES HEIDEMA

In order to localize the paper by Isabella Burger and Johannes Heidema, and to express my appreciation for it, I start by quoting the first part of the introduction I wrote for the proceedings of the special symposium on "Current interfaces between logic and philosophy of science," held at the 10th Logic, Methodology and Philosophy of Science (LMPS) international congress in Florence, 1995.

> Logic and philosophy of science have had many alliances since the rise of modern logic. The Wiener Kreis was even based on the idea of permanent cooperation. However, the ties have as yet not culminated in a strong interdiscipline, as for instance logic and linguistics have in logical semantics. To be sure, logicians and philosophers of science feel to belong to the same family, as is for instance clear from the very happening of the 10th LMPS international congress. But, curiously enough, besides the section General Methodology, there is not yet a section called Logical, or Formal, Methodology.
>
> One reason for the non-existence of an interdiscipline maybe Suppes' well known complaint that the logical study of science started to imitate metamathematics rather than mathematics. Another reason certainly is a general one, always threatening interdisciplinary cooperation. As Zandvoort (1995) has demonstrated, successful cooperation usually has an asymmetric means-end nature: one program plays the role of guide program, it poses and determines the leading problems, the other plays the role of supply or service program by helping to reformulate and solve these problems. However, when the potential supply discipline is well developed, as in the case of logic, the temptation is strong to create a problem area vaguely suggested by the guide discipline and to proceed by developing appropriate tools for that area. This may well be a useful strategy leading to interesting results, but not necessarily for the intended guide discipline, i.c. philosophy of science. In sum, whereas philosophy of science has the lasting problem of keeping in touch with science, the logical study of science has the problem of keeping in touch with science or at least with philosophy of science. (Kuipers 1997, p. 379)

The paper by Burger and Heidema is not only evidently a paper of a logical nature, it is also strongly guided by problems in philosophy of science or, more generally, by epistemological problems. As is clear, from the beginning to the very end, they succeed in keeping in touch with these problems. Incidentally, the same is true for the paper by Thomas Mormann, which may even be seen

as a "geometric" variation on the theme of Burger and Heidema's section on structural similarity. Regarding Burger and Heidema, I would like to make some remarks about "naive sorting," followed by some thoughts about the definition of a theory being false.

The Janus Character of Improving Naive Sorting

In ICR (p.152) I merely write about a "tension," whereas Burger and Heidema aptly write about the Janus character of trying to improve (naive) "sorting," where 'sorting' is their favorite term for what a theory does. If T is the target (set of models of a) theory, and X (the set of models of) a given theory, the naive challenge is to narrow down the symmetric difference between X and T. This may be done by trying to apply successfully, without knowing T (!), two operations in an arbitrary order: enlarging the set of models, with the safe aspect that no T-models are lost, but the unhappy aspect that *non-T*-models are introduced, and shrinking the resulting set, with the safe aspect of not introducing *non-T*-models, but the unhappy risk of losing T-models. Note that a similar Janus character applies in the refined, structural similarity approach. Presupposing my favorite type of structural similarity, of which it is not evident that it fits in the approach by Burger and Heidema, starting from a certain theory, a similar problem situation is generated by idealization followed by concretization, or vice versa.

I agree of course with the general claim (below *Definition* 2) that it makes a great deal of sense to generalize the terminology of truthlikeness, for example, 'Y is closer to the truth (expressed by) T than X', to the sorting terminology, in particular 'Y sorts closer to T than X'. However, I am surprised about the fact that an asymmetric (T-biased) general formulation is given, whereas the naive definition evidently is symmetric between T and X. For example, 'Y's sorting is between that of X and T' would be neutral between X and T.

Some Reflections on the Definition of a "False Theory"

It may be interesting to compare my own definition of a false theory with that of Burger and Heidema in Section 2.2.2. They follow the standard definition in model theory (cf. Przełęcki 1969, p. 19). Relative to a certain favorite or target theory T, they call an arbitrary theory X T-true iff all T-models are X-models, T-false iff no T-model is an X-model, and T-uncommitted otherwise, that is, when some T-models are X-models and some are not. Although in ICR I essentially use the same definition of being T-true, I call a theory already false

when it is not *T*-true, that is, when some *T*-models are not *X*-models. Of course, one may ask, what's in a definition? But the point is that the standard logical terminology is not very attractive from the nomic point of view in philosophy of science. According to this view, the target set *Mod*(*T*) contains all and only the nomic possibilities. This nicely fits in the definition of '*T*-true', a theory *X* is *T*-true when the claim "all *T*-models are *X*-models" is true, with the consequence that *T* is the strongest true theory, for which reason I call it "the truth." However, in this view it is then also plausible to let being *T*-false correspond with the situation that this claim is false: a theory (about nomic possibilities) is false when it does not leave room for all nomic possibilities. Of course, it is possible to formalize the intended nomic interpretation within a modal language, but it is much easier to leave the intended interpretation informal. Unfortunately, Burger and Heidema do not pay attention to the nomic interpretation. If I am right, the only place where they touch upon the distinction between actual and nomic truth approximation is when, near the end of Section 2.2.4, they deal with "[t]he very special case when we know exactly which world is the actual world, i.e. the case when T has one model only." This suggests that calling a theory *X* *T*-uncommitted when it shares some models with *T*, without including them all, is essentially based on the "actualist" interpretation: if *T* contains more than one model, we do not yet know "exactly which world is the actual world." But in the nomic interpretation, *T* will contain more than one model and is nevertheless the strongest truth there is to know about the target. Hence, from the philosophy of science point of view, we need actual and nomic definitions: nomically true or false versus actually (or instantially) true or false.

Note that the problem of how to define what a "false theory" is, is not specific to the nomic point of view. The standard definition is more generally a problem for the structuralist approach. Przełęcki (1991) considers besides the standard "uni-referential conception of a theory's intended interpretation" also a "multi-referential" one, that is, a structuralist one. In line with structuralist practice, he carefully talks about the "empirical claim" of the theory, that is, "all intended applications are *X*-models," as being either true or false. In ICR I "officially" did the same in terms of nomic possibilities, by talking about the (weak) claim of the theory. However, I certainly forgot to do so all the time, and for good reason. Scientists are used to talking about true or false theories in some generic sense and about theories being true or false in particular cases. Explicating that terminology should do justice to that practice in a way that is as simple as possible. In general, although in the logical study of science logic should certainly determine the rules of the game, it should try to keep the terminology of the field intact, whenever possible. For a further elaboration of

this point, see my reply to Zwart's "fourth problem," which was later written than the present reply.

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