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## Response to Marcel Weber's Comment

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I want to thank Marcel Weber for his incisive and constructive criticism of my paper and I would like to use the space below to address the major worry he raises about the very intelligibility of the proposal defended in the paper. Before I turn to it, I will make a brief comment about his second major criticism, viz. that I use a derivational conception of reduction. I simply want to acknowledge that I do so in this paper – to a large extent for the purposes of an argument. I quite agree that we may want to talk about reductive relations between theories that are not formulated in ways that would allow for a derivation and I by no means think that derivability is essential to reduction. The larger project [Paprzycka, forthcoming], of which this paper is a part, attempts to cast bridges between Salmon's [1984; 1989] and Nowak's [1980] theory of science. As is well known, Salmon has been hostile to requiring any derivability conditions in the account of explanation and probably would be in an account of reduction. Still, I believe that it is not necessary to take such a radical stance, and while we should learn the lesson Salmon teaches us—that relevance matters—this is no reason to think that 'controlled' derivability relations cannot be understood as showing us something important. In the Idealizational Conception of Science, derivability occurs only against the background of essentiality relations. Without going into too much detail, let us only remind ourselves of the footnote 33 problem to which pure derivability conceptions of reduction are subject, i.e. the problem of generating spurious reductions (derivations) of a theory  $t$  from a conjunction of  $t$  and an arbitrarily chosen other theory  $T_X$ . What blocks spurious derivations in the Idealizational Conception of Science is the fact that the derivations are to manifest deeper essentiality relations—as long as theory  $T_X$  does not investigate the same factor as  $t$ , and as long as its space of essential factors does not at least partially overlap with the space of essential factors of  $t$ , we cannot even begin to think that a reduction is involved.

Let us then turn to the problem of having the identity-theory cake and eating it with an anti-reductionist pudding. Since *I do* mean factor identities to be factor identities and not just equalities among the values that different factors take, Weber points out that the following objection arises: If it turns out that the factors of two true theories can be identified with each other in such a way that the essential structure of the factors will not be preserved under the identification, why should

we not conclude that one of the theories is simply wrong about the essential ordering of factors rather than thinking that they are equally good but irreducible to one another?

Two points must be appreciated before we can see how one could defend the intelligibility of suggestion that irreducibility can be based on essential incompatibility. (1) We have been assuming thus far that the factors of the two theories can be identified one-by-one with one another. It is highly unlikely, however, that the factors of mature psychology could be identified on a one-by-one basis with the factors of physics or neurophysiology. We will thus waive this assumption allowing for the possibility that higher-level factors be more complex constructs composed of lower-level factors. (2) The second point concerns the question how to understand the notion of essentiality. On one (the accuracy) interpretation of the degrees of essentiality [Paprzycki and Paprzycka, 1992; Paprzycka, forthcoming], scientists order factors depending on the degree of accuracy that the inclusion of a factor affords.

With these thoughts in mind, let us consider the following schematic example. Let us assume that we are dealing with two true independently proposed theories that explain apparently different phenomena. Let us assume that theory  $t$  proposes the following essential structure of  $C$ :

$$S_C : \begin{array}{l} M \\ M, m_1 \\ M, m_1, m_2 \end{array}$$

The essential structure of factor  $D$ , which is the investigated factor of theory  $T$  looks thus:

$$S_D : \begin{array}{l} N \\ N, n_1 \\ N, n_1, n_2 \\ N, n_1, n_2, n_3 \\ N, n_1, n_2, n_3, n_4 \\ N, n_1, n_2, n_3, n_4, n_5 \\ N, n_1, n_2, n_3, n_4, n_5, n_6 \end{array}$$

As it turns out, the two theories have been in fact investigating the same factors:

$$C = D$$

and that the remaining factors of theory  $t$  can be identified as constructs of the factors of theory  $T$ , so that the following identities hold:

$$\begin{array}{l} M = n_1 \wedge n_2 \wedge n_4 \\ m_1 = N \wedge n_5 \\ m_2 = n_3 \wedge n_6 \end{array}$$

The relation between the factors is demonstrated by Fig. 1, where in particular the degree of essentiality of factors is shown.

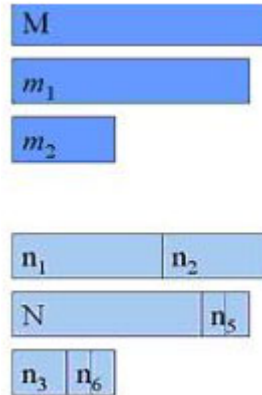


Figure 1. The relation between the factors of two essentially incompatible theories  $t$  and  $T$ . The areas represent the accuracy afforded by the inclusion of a given factor. Further explanation in text.

The diagram makes clear why a theory that uses factors  $M, m_1, m_2$  would consider them to be essential to the investigated magnitude in that order. The fact that it is possible to identify those factors with  $n_1 \wedge n_2 \wedge n_4$ , and  $N \wedge n_5$ , and  $n_3 \wedge n_6$ , respectively, does not provide a reason to undermine their ordering—quite to the contrary it actually allows us to vindicate it. A theory that uses factors  $M, m_1, m_2$  must use them in this—and no other—order. In this way, the assumption that factors of theory  $t$  are complex constructions of factors of theory  $T$  together with the accuracy interpretation of essentiality provide a justification why the essential structure of theory  $T$  need not lead to a change in the essential structure of theory  $t$ . This suffices to establish the intelligibility of the suggestion that essentially incompatible theories will be irreducible to each other.

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