

Committee II
Theoretical Empiricism: A General
Rationale for Scientific Model-Building

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Discussion Paper

on

Claes Fornell's

**THE BLENDING OF THEORETICAL AND EMPIRICAL KNOWLEDGE
IN STRUCTURAL EQUATIONS WITH UNOBSERVABLES**

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Abstract: Professor Fornell's paper discusses the relation between theory and data by analyzing recent statistical techniques from the point of view of modern philosophy of science. The relation is discussed on three levels: philosophy, methodology and through concrete implementation. In this comment it is argued that Fornell's ambitious paper is of interest not only for statisticians, but also for philosophers, as it nicely exemplifies certain much discussed problems in recent philosophy of science. It is also argued that some minor difficulties are due to a few more basic inconsistencies of the paper; esp., Fornell did not succeed in conjoining the levels of abstraction in an agreeable way. The paper would have profited from a more careful elaboration of the philosophical and methodological sections, and a more stringent unification of these sections with the example.

My task as a critical discussant is complicated by the circumstance that I sympathize with so much in professor Fornell's paper: in particular, its ambitious aim. Statistical techniques have more often been spared from philosophical analysis, and when analyzed, this has been done from the point of view of older philosophies of science. Fornell has set himself the difficult and important task of combining modern philosophy of science with recent achievements in the field of multivariate analysis. This has resulted in a paper that is multi-faceted, sometimes sketchy, but which explores new avenues for inquiry into the assumptions and deeper meanings of new statistical methods.

From the point of view of philosophy of science, the merit of the paper is that it attempts to illuminate the relationship between allegedly theory-neutral data and theoretical assumptions. Therefore, the paper should be related to the modern debate of the nature of science, a debate that to a great extent concerns the question of the rationality of science. Today, this question is often put as the problem of theory-choice, i.e. how is it possible to choose between two rival theories or methods that claim to solve "the same" problem, if there is no rational standard of evaluation? Philosophers of science have become increasingly aware that science cannot be understood through abstract and idealized reconstructions of its logic, but that we first and foremost need more examples of actual problems and anomalies characterizing different sciences. I will return to this issue below, and try to say how Fornell's contribution might be fitted into this debate.

Fornell's paper is centered around the relationship between theory and observation. This relation is discussed on at least three levels: philosophy, methodology and through concrete implementation. The main theses of these parts are:

1. It is not possible to draw a distinction between theory and observation. Reason: observations must always be interpreted.

2. Concepts have both abstract and empirical meaning. The abstract need not and should not be reduced to empiri, need not be operationalized, but has and should be given a certain autonomy.

3. Implementation: with examples it is shown that a given dataset can be weighted against theoretical hypotheses in different ways, through different statistical strategies/procedures, which produce different results. This supports the theses of parts 1 and 2.

1. Philosophy. The hallmark of logical positivism is the so-called criterion of verification, i.e the postulate that only assertions that can be reduced to elementary propositions about sense-data are meaningful/scientific. Verification and falsification require that there exist theory-neutral observations against which theoretical assertions can be put to test (verified/falsified). By drawing on results of gestalt- and perception psychology, plus the history of science, recent philosophers of science have cast severe doubts on the possibility of neutral observations. It seems that the reverse is true; all observation is by necessity theory-dependent. This conclusion has enormous consequences for our understanding of the dynamics of science. According to the most radical interpretation, the thesis of theory-dependence implies that we can no longer conceive science as an interplay between theory and observation, and verification/falsification cannot constitute objective arbiters of theories; all research is theoretical, "Nature" cannot say "No"; science is an autonomous theoretical discourse.

Let us imagine the debate concerning the theory-dependence of facts as a scale with two poles, where one pole is represented by the crudest version of positivism ("naiv realism"), and the other by, say, Feyerabend's "theoreticism". From his general remarks, Fornell's position is not difficult to determine. He rejects positivism, and he rejects the possibility of "a

distinction between the observational vs the theoretical", his reason being that an observation must always be interpreted, and interpretation is always made on the basis of theory. Further, he concludes part 1 by holding: "in other words, all the information collected by a researcher is conditioned by the context into which research is placed" (p 4, my emphasis). This seems to place Fornell near the "theoreticism-pole". However, as will be shown, at this point he does not manage to maintain a consistent position.

2. Methodology. In this part, Fornell attempts to show two things: how concepts obtain abstract and empirical meaning. and how theory is related to data - in particular, the epistemological direction between theory and data. A concept F obtains complete abstract meaning if three criteria are satisfied: (a) definition of F, (b) knowledge of the antecedents, determinants and causes of F, and (c) knowledge of the consequences and implications of F. Empirical meaning is obtained through rules of correspondence, where the concept is related to reality through a series of empirical measurements.

In the following, I shall raise a series of objections to Fornell's views. Some of these are minor, some really only questions. I put them because I suspect they reflect a few more fundamental problems, which I shall summarize in parts 4 and 5.

i) Abstract meaning. Utopically, Fornell is of course right; the more we know about a phenomenon, the more we understand its meaning. But in practice, Fornell's criteria will very often be too strict, or pedantic. When satisfying his criteria, we will on the one hand get a lot of redundant information, on the other sometimes also contradictions. The antecedent or earlier meaning of a concept may be diametrically opposed to the present, and that also goes for its consequences. "Revolution" today means a change to something qualitatively new, but it has meant a return to an earlier

stage, an action backwards. Even if this may be interesting, I doubt that the scientific use and meaning of the concept will improve more than marginally by this knowledge - in any case, it is not motivated to introduce the antecedents of a concept as a separate criterion of meaning, especially in view of the fact that the stipulative definition is the most characteristic of science.

According to Fornell, one major form of definition, i.e. criterion (a), is the dispositional. This mode of definition encompasses the capability and potentiality of a concept, "the ability or power of the concept to undergo change and to produce change in other concepts" (p 7). Likewise, criterion (c) purports to "supply information as to where a phenomenon is going, what it can lead to, and/or what influence it has" (p 8). Thus we find a considerable overlap, not to say identity, between criteria (a) and (c). Furthermore, we find an ambivalence as regards what is being defined: is it the meaning, consequences etc of a concept F, or the phenomenon the concept refers to? Most often, Fornell writes that it concerns the consequences of the concept. But the consequences of a scientific concept deals primarily with future research. How is this to be established in advance, and why?

ii) Abstract and empirical. On the whole, I have difficulties in understanding why these three criteria are needed in order to identify abstract meaning. This leads to a related problem, viz. what Fornell really means with the term "abstract", as opposed to "empirical". Abstract meaning is primarily established through attributional and dispositional definitions, according to Fornell. But are these definitions abstract? The classical example of a dispositional definition is of magnetism, which may read: "if iron filings are put close to x, and they are attracted to and cling to x, x is magnetic". "Attributional" definitions very often consist of enumerations of utterly concrete properties, such as colour, weight, length etc.

Fornell's own example (market, p 5-6), also indicates that his use of "abstract" is strange. So what does he put into this concept?

I get the impression that "abstract" is equalled to "theoretical", and that these two concepts are equalled to "unobservable", from expressions such as "there is basic agreement about the abstract or unobservable status /of theoretical terms/" (p 4). But this does not accord with the examples (market, magnetism), nor with the definitions of abstract meaning, as they contain observables.

It is therefore possible that Fornell by abstract meaning simply means "not operationalized" meaning. But this does not hold water either, as dispositional and attributional definitions very often are subtypes of operational definitions.

Later, Fornell adds that "in the definition of abstract meaning, empirical criteria also enter the picture" (p 8). Fine, but confusing, because what is then the difference between abstract, or abstract/empirical, and empirical?

I guess we have to conclude that "abstract meaning" remains a very diffuse concept, which is unfortunate, as it is so crucial - actually, Fornell's parts 2 and 3 build on the possibility of an autonomous identification of abstract or theoretical meaning.

iii) Direction. What is then the relationship between abstract and empirical meaning? Fornell provides a general answer to this question. The answer is based upon an inclusion of the deductive and inductive modes of reasoning into a general model (figure 3). According to Fornell, there are two principal relations between theory and empiri. In the "deductive case we take the observations as dependent upon the abstract theoretical model, whereas in induction the theoretical variables are taken as dependent upon the observed variables" (p 11). In the deductive case, observation is "reflective" upon the theoretical model, in the case of induction, observations

"make up" the theoretical variables, they are "formative" of the theoretical model. Later these two modes are used to explain different strategies concerning the connections between theory and observations of reality (monopoly power and exit/voice).

Fornell's presentation is illustrative, but not more than a metaphor, and possibly a misleading one. Firstly, deduction and induction are modes of inference, and does not deal with causality. (Socrates is not mortal because all men are mortal.) Secondly, deduction and induction does not necessarily proceed from the theoretical or unobservable to the empirical or observable, but most often (but not always) connect the more and the less general. "All swans are white" is in principle not less observable than one swan (even though there admittedly are a few practical obstacles...). Thirdly, this coupling is mechanical, it only adds theoretical to empirical and does not take the real problems into consideration. But the interesting question is: how can theoretical and empirical meaning be combined, amalgamated? Think for instance of our standard example, intelligence. We can define it in the abstract, but then we cannot use it in empirical research. Or we can define it in accordance with existing measurement methods, but then we lose the full meaning of the concept. In other words, theoretical and empirical meaning often stand opposed - and this is the problem that constitutes the raison d'etre of operationalism! We do not solve problems such as these by "deducing" observations from e.g. "intelligence". Fourthly, if the relation between abstract and observable were of the kind Fornell describes, then we would not have had any scientific problems at all; theory and observation would have agreed. To quote a famous scholar: if essence and appearance were the same, science would have been unnecessary.

iv) Dialogue and autonomy. What, then, is the dynamic in this model of science? Fornell concludes part 2 by stating that knowledge is produced by "a continuing dialogue between theory and data". But a dialogue can hardly

exist between knowledges that stand in deductive or inductive relations to one another, as they follow from each other. Rather, scientific problems emerge, are elaborated and sometimes solved because theoretical and empirical meaning are produced by different, relatively autonomous discourses. Fornell also says that "the context of a specific situation must determine what should be regarded as unobserved or observed and what the linkage should be" (p 11). This indicates an internal inconsistency in Fornell's reasoning. Either there are two autonomously defined meanings, which is the main claim of part 2. If so, a dialogue is possible. Or else, the empirical is theory-dependent, and the distinction between theoretical and empirical is conventional, arbitrary, dependent upon context - but then we seem to come up with a monologue, and science must be motored by something very different from a dialectics between theory and observation.

3. Implementation. In this part Fornell compares different statistical methods, and demonstrates that different ways of treating "the same" dataset on the one hand produce different results, on the other build on different methodological and philosophical presuppositions. The discrepancies are due to different ways of combining theoretical and empirical variables. By applying the PLS-approach on Hirschman's theory of monopoly power Fornell shows that you obtain one result with reflective indicators, another with formative indicators. With the first, you are faithful to your theory, with the second, you lean more to the data-side.

I leave out the technical aspects, and note that Fornell's to my mind very convincing demonstration should be of great interest to today's philosophy and sociology of science (esp. the so-called "post-Kuhnian sociology of science"). The example seems to illustrate a very topical question, viz. that there are no rational criteria for choosing between the proce-

dures, no way of making "a crucial experiment". Fornell has rejected verification and falsification. What remains?

After his demonstration Fornell raises this question (p 17), but does not really try to answer it. He says that it has to do with the objective of the model, which may be interpreted as a very trivial or else a quite interesting answer. Does it concern internal or external objectives? Are personal or perhaps even social factors playing a role here? Are the methods really incommensurable, or possibly complementary? (They should be incommensurable. If not, I am afraid part three is reduced to the triviality that data can be manipulated in different ways.) Elaboration of questions such as these would fit Fornell's example into the today rapidly growing collection of illustrations of scientific "irrationalities" (?)

4. The relations between the parts. Much as a consequence of the problems mentioned, a more general view of the paper displays a few inconsistencies between the three parts. Briefly:

i) The relation between parts 1 and 2. This has been touched upon above: The crucial problem regarding the relation between these parts is that Fornell has had different aims with them, aims that are opposed. On the one hand, he claims that it is not possible to draw a distinction between theory and observation. On the other hand, part 2 assumes, builds on, exactly this distinction. To the extent he succeeds in showing the one, the other will cease to have meaning, and vice versa.

ii) The relation between parts 1 and 3. In part 3, Fornell wants to illustrate the thesis of the theory-dependence of observation he has presented in part 1. As we saw, Fornell's position is near the "theoreticism-pole". However, we should distinguish between two types of theory-dependence, two types of criticism of empiricistic philosophy. The first is "the underdetermination of theory by facts", which says that many theories can

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be confirmed by, and simultaneously explain, the same dataset; there is no logical or isomorph relation between theory and data (the Duhem-Quine thesis, today developed by Hesse and others). The second concerns the "theory-ladenness" of observations, i.e. by their nature, observations are by necessity theoretical and "partial"; there exists no sotonomous "observational core" (Feyerabend, Hanson, Toulmin, Kuhn). In part 1, Fornell presents the latter thesis. However, the implementation in part 3 does not concern this thesis, but rather the thesis of underdetermination, the conventional relation between theory and data.

iii) The relation between parts 2 and 3. One problem here is that the rules and criteria for the determination of abstract meaning presented in part 2 (antecedents, definitions etc) are not at all employed for the delineation of abstract concepts such as "monopoly power". On the contrary, different versions of monopoly power are defined from the point of view of data, and from the possibilities and limitations of statistical measurements - i.e. rather an operational definition!

For these reasons, I find it doubtful whether it is possible to say that part 3 is an implementation of parts 1 and 2.

5. Concluding remarks. Several of the objections raised are of minor importance, and may seem like fault-finding, and certainly misunderstandings from my side. But to the extent I am right, they point at a few more fundamental problems in Fornell's presentation. I think Fornell's excellent example would have profited from a slightly different frame of reference. In part 2, it would probably have sufficed to build on the common distinction between operational and lexical or nominal meaning, and not complicate the picture by introducing difficult philosophical problems such as observability, abstract-empirical etc. The framework of philosophy of science should, I think, have concerned the Duhem-Quine thesis, and perhaps also the issue of rationality and theory-choice. This would

have created a more consistent relation between the parts of the paper.

As Fornell points out, the choice of statistical procedure connected to question of existence, i.e one further, ontological level. When combining theoretical and empirical, the vital point is of course what weight or status you give these two entities. And this is not something that can be accomplished by some technical innovation or statistical method, nor with "substantial theory". On the contrary, it is a problem that to a great extent is answered at the borderline of science, i.e. what we call "philosophy", which in its turn borders on "faith". Fornell's illustration exemplifies this dilemma: how to choose between reflective and formative indicators? This problem is mirrored in the history of philosophy, in the battles between rationalism and empiricism, but, if you allow me to speculate a bit, there is another classical opposition that better agrees with Fornell's: the dispute between realism and nominalism as regards the status of universals. To realists, universals exist before the objects (universalia ante res). To nominalists, only single objects exist, universals denote nothing, or are artificial conceptualizations (ante res). (Does the law of gravity exist? If so, in what sense? Is the law of gravity the cause of the effect that objects fall?) In the last, this must also be Fornell's problem. Does monopoly power exist? In this case, it is justified to use reflective indicators. A nominalist, on the other hand, would display a predilection for formative indicators. The path from philosophy over epistemology and methodology to implementation could thus perhaps be reconstructed along the following two parallel lines: realism ---> rationalism ---> faithfulness to theory ---> PLS-approach with reflective indicators, and nominalism ---> empiricism ---> faithfulness to data ---> PLS-approach with formative indicators.

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