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

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Only

DISCUSSION PAPER

by

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on

Claes Fornell's

The Blending of Theoretical and
Empirical Knowledge in Structureal
Equations with Unobserables

and

Jan-Bernd Lohm"oller's

The Basic Principles of Model Building

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COMMENTS ON DR. FORNELL's PAPER.

(by Francisco Azorín,
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I think Dr. Fornell's paper will be read with much profit, for several conceptual and methodological reasons. As I am especially interested in terminological matters I would like to make the following comments:

(i) On definitions.

Concerning what Dr. Fornell calls the attributional definition of concepts (p.5), which can also be extended to objects in general -it should be noted that the terms: attributes, characteristics, properties or descriptors, are often used with two different meanings.

First, as the names of variables - quantitative or qualitative.

Secondly, denoting the particular values taken by these variables for a particular concept, or object.

For instance, using the first meaning, in the example given by Fornell, p.5, on the definition of a market, it would be said that the number of actors in a market, is an attribute, property, etc, of the concept "market" (first of the meanings indicate above).

On the other hand, it can be said, for a particular case, that the market has the attribute, property, etc. of having two actors (second meaning).

The same term - or terms, are thus used for a variable, and for any one of its values.

In my opinion, even if in most cases there would be no confusion on which is the meaning taken by the term, it would be more advisable to distinguish explicitly between the two meanings.--- This could be done by selecting an appropriate name for each of them from the collection of seemingly equivalent terms: attribute, characteristics., property, descriptor,... (or picking a different term, e.g. predicate). For instance, character could be used for the general meaning, and the value or modality for the second meaning.

Another possibility would be to indicate by (1) the first, general meaning, e.g. a "variable", and by (2) the second, particular meaning, e.g. the "value of the variable".

Now, it seems to me that homonyms and synonyms, which give richness and beauty to literary language, should better be avoided, as much as reasonably possible, in scientific prose, for the sake of clarity and the attenuation of the risk of misunderstandings. Thus, instead of using indiscriminately the above mentioned terms with the same meaning, (1), or (2), I would prefer different definitions to be assigned, as follows:

Attribute should be used, as it is often made in statistical practice, to denote qualitative characters, or characteristics, sometimes also called categorical variables (another synonym). That is, characters, or characteristics (general meaning),

would be divided into attributes or qualitative characters, and variables (variables proper, or numerical) or quantitative characters. We have not been able to avoid absolutely the use of synonyms, for the time being, but just pointing the way to its reduction or, if possible or reasonably, to its eventual elimination.

For the particular states or modalities of an attribute, or for the particular values ofa variable, the term property could be used. The term descriptors, could indicate the characters chosen for the description of a concept, or object, according to some rule, or satisfying a given objective, or target.

So much for what the author calls atomistic-analytical attributional definitions. As to holistic - contextual attributional definitions (p. 5), it is not clear to me why, according to the author, mention is made of elements and relations (p. 6, line 5). It is true that there is a totally comprehensive (exhaustive) definition as in the case of a market (lines 10-12), but then, speaking of elements could lead us to the same difficulty as is found before with the atomistic - analytical attributional definition. In fact there are potentially infinite elements, as there are potentially infinite characters to be observed in any object.

Another point, worth mentioning, is that characters -attributes or variables-can be crisp or fuzzy. For instance, when we say that a market has two actors, we are giving the value of the variable "number of actors". This is a property. But when we say that a market has "many actors" we are giving a fuzzy property. There are not clear-cut frontiers between "many" and

"not many". We found another possible source of fuzziness (in p. 6,line 7), when saying that "some properties are essential or necessary to a definition". These adjectives (essential, necesary) may not be as crisp as they sound. Substantives, like "actors" of a market , can also be fuzzy; for instance, when there are "actors" with "fleeting" activities, or with"feckless" or "ineffective" actions. There is no time now to enter into the important problems of "feature selection" (notice another possible synonym for character) and of the weights to be attached to characters in some cases (the so called Adansonian principle postulates equal weights for all characters, but those not included have already been given weight zero).

With respect to dispositional definitions, or the "description of capacities, tendencies, or dispositions of a concept" (p. 6), to influence or being influenced, to change in attitude and behaviour. These "cognitions and evaluations" (line -3) could be themselves subject not only to change, but also to ambivalence and plurivalence, especially when dealing with emotions ("mixed feelings", etc.).

(ii) On Directionality of the Relationship between Theory and Data (p. 10).

As the author says,knowledge is produced by a continuing dialogue between theory and data (p.11). But it is difficult to indicate where we stand in each particular dialogue with respect to a theory or hypotesis and to the corresponding data.. Fornell describes Partial Least Squares (PLS), a method developed by Herman

Wold, which is available when the analyst is unwilling to depart too far from the data and wants to obtain a proper balance between theory and observation (p. 14, lines -8, etc). This comes after considering the trade-off between overriding the data with the abstract model specification, or being excessively faithful to observation. It is, of course, difficult in many occasions to strike a "reasonably", or "justifiably", or "adequate" balance. In PLS iterative estimation procedure, the local criteria are treated as fixed in one cycle and relaxed in the next cycle and viceversa, until convergence (p. 15) is attained.

The example developed by Fornell in p. 16 is illustrative of the difference between what he calls formative (mode B in Wold's terminology) and reflective (mode A in Wold's terminology) indicators. That is, according to the author's terminology (p. 11), that in a relationship between observables and unoservables as implied by a deductive approach, the observations are reflective of the theoretical model. Whereas in an inductive approach, the observations "make up" the theoretical variables, and the observations are formative of the theoretical model. The value of Indicators could be taken as the result of indirect measurement, or of measurement of 2nd degree.

It is important to emphasize that the author's position, while giving due attention to observations and to methods which would not lead to them being superseded by theory, accepts the impossibility to obtain absolutely "theory-free" data (p. 21). The philosopher E. Levinas, following Husserl, deals with sig-

nificance and sense, and says that data are always set in a certain horizon, in a context. Significance, the observer's point of view, and theory, would thus precede and illuminate the data. Fornell, in the line of Achinstein, concludes that all the information collected by a researcher is conditioned by the context in which the research is placed (p. 4, line -4).

(iii) Reliability and Validity.

The author states that research conclusions are highly dependent on how we specify the theoretical model, and the relationships between model and data (p. 21, line -5, -6). Then he adds that "it makes little sense to follow the common practice of assuring quality of measurement (via various reliability and validity test) (p. 21, line -4) in isolation of the theory to which the measures are related, and before they are used in a substantive context". Thus, reliability and validity are used here in the sense which is generally admitted in statistics (by the way, reliability is another example of homonymy in statistics, it being used for life duration, dependability, etc. of a system, or of materials, etc.).

With respect to validity, the author says also (p. 17, line -7): "even though concentration ratios are fallible indicators of monopoly power, they probably contain some information about such power. The task is then to separate valid information (variance) from that which is not relevant". Now, this opposition between validity (validation?) and relevancy is not satisfactory, in my opinion. In a paper presented elsewhere I made a list of

desiderata (and evitanda) with respect to statistical data, and estimators. Among these desiderata, or list of desirable properties, the following are mentioned: (i) Accuracy, i.e. both Reliability and Validity, or in a more operational way, Precision and Unbiasedness; (ii) Relevancy; (iii) Timeliness; (iv) Efficiency.. I think that it would be better to give different meaning to each of these terms. The author mentions the "accuracy" of a theory (p. 2, line - 10), I suppose in the sense of "proximity to truth" i.e. of being both "reliable and valid". Perhaps this double meaning could be made more explicit when it concerns theory rather than data, or estimators.

PLS method (soft modelling) is an exceedingly valuable instrument for finding a way out of the dilemma: "specification and fertility" versus "robustness and freedom of assumption". It may be relevant to mention two others, non independent issues: the estimation of prpbability density functions without a previous specification or "parametric hypothesis", and the controversy between: (i) "orthodox" samplers, who base their inferences on the deliberately introduced randomization of the sampling frame, and (ii) "users of superpopulations" , or models, including Bayesians, "Royallists", and others, when working in the theory and practice of sampling surveys.

These controversy has lead to suggestions by the second group to define two types of inference: "scientific inference", based on previous specification, and "statistical inference", limited to the estimation of unknown constants of a finite population.

Finally, as to the author's use of the term"fallibility" , in "ratio measures are very fallible measures of monopoly power" (p. 17, line 13), I think that it should better be considered as a synonym for unreliability ("fallible" as prone to failure, as not reliable, etc.), in the second sense of reliability, mentioned above. This is a nice example of synonymy with homonymy.

I think that the " unended and unending quest" for "accuracy" and "validity" in the use of scientific language requires a careful scrutiny of the terms used, as scientists in general and as statisticians and X - metricians, even at the risk of being considered as addicts to hair splitting. For me, it is certain that in discussions concerned with the unity of science, we should make all efforts to avoid linguistic confusion and misunderstandings.

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On "THE BASIC PRINCIPLES OF MODEL BUILDING", by Dr. Jan-Bernd Lohm"oller.

(Discussant, F. Azorín, Univ. Autónoma, Madrid, Spain).

Dr. Lohmöller's introduction is a terse and effective description of model validation. I would like to make a few comments on this persuasive paper.

First, on his "steps of model building".

I think that it would be relevant to note that model specification and estimation should be now taken in a very comprehensive sense, even if as the author says " we will focus on models which require a statistical treatment". Perhaps it would be better to describe this preliminary step as a statement of the inductive or inferential specific purpose. In many cases this purpose would be the estimation of parameters [illegible] following a classical parametric specification, or of finite population unknowns. In other, as quoted by the author, the specification of manifest or latent variables. Still in other cases, the construction of spline models, etc. And perhaps it should be mentioned as well a special kind of estimation or "automatic estimation", the so called imputation process, to assign values to missing or rejected data in Censuses and Surveys.

Secondly, following H. Wold (e.g. "Model Construction and Evaluation," 1979) one should always keep clear the distinction between testing and evaluation: "testing confronts the model with the data under analysis; in evaluation, it is compared

with other models as applied to the same data. Hence the key facto is the criteria of evaluation". The same happens with evaluation procedures; e.g. when a census is evaluated through a post-censal survey, both applied to the same population.

Finally, with respect to what the autor calls "the blind-foldin device of model evaluation", it seems to me that a wide and syste- matic comparison between re-use methods would be extremely useful. Boot-strapping, jackknifing, etc., "per se" or as auxiliary devices for measuring precision in cross-validation should be considered, as well as the trade - offs between contradictory desiderata. This is by no means an easy task even if great advantage could be taken from theoretical and practical results by S. Wold and others, by analytical and simulation methods.

I think the conclusions could be rewarding, in a field where there is wide room for exploration and systematic evaluation.

Madrid, August, 1984

Some brief comments to Dr. Noolan's paper.

In his Abstract (line -10, -9), he says that " the strategy of hierarchically structuring the latent variables ... leads to ... greater complexity but makes the model easier to handle conceptually". I liked this distinction, because it deals with the age old justification for introducing stochastic "complications" in models, and more recently (about 20 years) for the introduction of fuzziness, in variables and models. How to integrate, as far as possible, soft modelling and the fuzzy sets approach is still an open field for investigation.

His statement that theory is required at all stages of research starting from sampling design (p. 2, line 2), agrees with the position established by Fornell, and others.

Madrid, August, 1984.

R E F E R E N C E S

1 K.G. Joereskog & H. Wold, Systems Under Indirect Observations. Causality * Structure * Prediction, Part II, Chapter 1, North Holland, Amsterdam 1982

2 J.-B. Lohmöller, LVPLS 1.6 program manual: Latent variables path analysis with partial least squares estimation (Forschungsbericht 81.04 Fachbereich Paedagogik). Muenchen: Hochschule der Bundeswehr. 1981