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What's in a Taxon?

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Whether taxometrics yields inferential knowledge to something latent is partly but not wholly a semantic question. Although the single variables are manifest indicator scores of individuals, the statistics computed from them via postulates of the formalism are not mere data summaries and will be incorrect or meaningless if the structural conjectures are false. The unidirectional derivability from postulates to data relations supplies the taxonic inferences' surplus meaning that constitutes conventional psychometric meaning of latency (e.g., latent class analysis). Surplus meaning beyond the purely mathematical may be provided by interpretive text attributing unobserved attributes or causal origin to taxon members.

For more than thirty years, colleagues and I have discussed informally the meaning of *taxon* and the concept of taxonicity, and at times I have attempted to expound it systematically (Meehl, 1992, 1999; Meehl & Golden, 1982). Like its more familiar associates *category*, *kind*, and *type*, the term taxon has a broad pre-analytic extension, rendering a precise verbal explication difficult. In ordinary language, the range of categories includes natural kinds (planet and moon, gopher and chipmunk), organic diseases (mumps, measles), social institutions (monarchy, dictatorship), ideologies (Trotskyist, Stalinist), religions (Sikh, Quaker), human artifacts (spoon, fork). In some domains taxa are sharply defined by conjunctions of clearly demarcated properties (there are no sporks in kitchen drawers, there are no gophmunks in the zoo). In the realm of human behavior, such clear delimitation may occur (Tay-Sachs disease, Trotskyist ideology) but it is rare. Fortunately, as is often true in empirical sciences, a precise verbal definition of the word 'taxon' is not necessary. We accept taxonicity as an open concept (Meehl, 1972; Pap, 1958) and tailor any verbal explication to the fact domain being investigated. The important point is to be clear about our research aim, what we are trying to find out about a domain when we raise the taxonic question. Thus, whether a rough but adequate verbal explication when we are investigating mental illness would be an appropriate guide if we were working in, say, geology or political science is of no scientific interest (although a philosopher might find it fun to imagine why open concepts can be useful in certain domains).

When we ask the "taxonic question," we are asking whether the underlying (latent, unobserved) situation is a single distribution or composed of two or more groups (each with its own distribution). To formulate the taxonic decision problem as categorical *versus* dimensional is somewhat misleading, because in psychometrics the manifest indicators are almost always thought of as generated by latent factors that are dimensional. Thus the question is whether the distribution of the indicators is composite in the sense of mixture analysis, which usually means that the distributions of the underlying psychometric factors are composite. For this reason, my conjecture is that under favorable circumstances taxometrics and mixture model analysis should yield similar results.

It is important to realize that the use of taxometrics does not require that the investigator entertains a taxonic conjecture. Taxometrics may be used in that way, analogously to confirmatory factor analysis. But a researcher with no theoretical opinion can properly use taxometrics as a decision procedure, with one possible outcome being that the latent structure is nontaxonic. Further, there is no reason to prejudge the proportions of taxonic and nontaxonic decisions that will result from the use of taxometrics. For example, I do not believe that most of the DSM (*Diagnostic and Statistical Manual of Mental Disorders*, Fourth Edition, text revision—APA, 2000) rubrics represent real taxa, but I do conjecture that the majority of hospitalized psychiatric patients belong to a small group of valid taxa; the truth of my conjectures will be found in future taxometric research.

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Usually there is indicator overlap between the taxon and its complement class, contributing to imperfect pairwise correlations between indicators. Even in organic medicine, where a well understood disease entity is explicitly defined by a conjunction of pathology and etiology (Meehl, 1973, diagram, p. 287), two-way pathognomic signs are rare so that the sorting of individuals is probabilistic; if the indicators had perfect validity it would not be probabilistic.

In researching personality and mental disorder, even a syndromically loose taxon may be sharply defined at a lower level if it arises from a specific etiology (germ, gene, vitamin deficiency, trauma; cf. Meehl, 1973, 1977, 2001). Absent that possibility (i.e., a specific etiology does not exist or we haven't found it yet), I have proposed adoption of a formal-numerical concept of taxonicity: A group of individuals has a taxonic structure if certain mathematical relations obtain among observed indicator scores or inferred latent factors (Meehl, 1992, 1999; Waller & Meehl, 1998). Whatever the taxometrician asserts beyond these formal-numerical relations depends on the state of knowledge. This minimalist interpretation I have labeled with my initials as *taxon*$_{PM}$, not out of narcissism or egocentricity, but to emphasize that I do not try to impose my semantic convention on others.

What is latent that can emerge from a taxometric analysis and how can we justify our inferences? Robert Golden and I had many conversations about this in the 1970s, and we were unable to reach a clear formulation. I do not claim to now have a demonstrative formulation (except as to the mathematics), but I can offer some clarifying remarks to help prevent certain kinds of mistakes about what taxometric analyses can and cannot show.

It is helpful to distinguish purely statistical latency from the additional latency provided by an interpretive text. I shall take latent (contrasted with manifest) to mean the existence of Reichenbach's (1938) *surplus meaning*. Reichenbach showed that statements concerning inferred entities (those not directly observed, called by him *illata*) are not deducible from the class of observational statements, that a projective construction differs from a reduction, and that a set of statements about an inferred entity says something more than the conjunction of the observation statements that are evidentiary with regard to the entity but which are not semantically equivalent to it. This distinction is not a matter of taste nor of one's philosophy of science but, as Reichenbach showed, it is a logical and semantic difference.

If the latent situation is taxonic, then the conjectured latent structure is given by the General Covariance Mixture Equation,

$$\mathrm{cov}(yz) = P\,\mathrm{cov}(yz)_t + Q\,\mathrm{cov}(yz)_c + PQ(\bar{y}_t - \bar{y}_c)(\bar{z}_t - \bar{z}_c)$$

where:

$\mathrm{cov}(yz)$ is the (manifest, observed) covariance of y and z in the total (i.e., mixed) sample;

P is the taxon base rate in the total sample, and $Q = 1 - P$;

$P\,\mathrm{cov}(yz)_t$ is the weighted indicator covariance in the taxon (t) class;

$Q\,\mathrm{cov}(yz)_c$ is the weighted indicator covariance in the complement (c) class;

$PQ(\bar{y}_t - \bar{y}_c)(\bar{z}_t - \bar{z}_c)$ is the weighted cross-product of the latent mean differences; it is called the *validity mixture* term, because if there is no separation on the indicators, this term will be zero.

Define the validity constant K as the product of the separations,

$$K = (\bar{y}_t - \bar{y}_c)(\bar{z}_t - \bar{z}_c)$$

In the hitmax interval, defined by the conjectured latent structure, $p = q = ½$; hence we can infer the value of K from the covariance in the hitmax interval,

$$\mathrm{cov}(yz)_{hitmax} = (½)\,(½)\,K$$
$$= ¼\,K$$
$$K = 4\,\mathrm{cov}(yz)_{hitmax}$$

For any given interval, the estimated taxon membership probability is

$$p_i = \frac{K \pm \sqrt{K^2 - 4K\,\mathrm{cov}(yz)}}{2K}$$

Figure 1. Illustration of the use of manifest and latent variables in MAXCOV.

Surplus Meaning of Formal Taxa

When I use taxon$_{PM}$ to mean formal-numerical taxa identified by my taxometric method, the claim is that a taxon identified by coherent cut kinetics is not merely a statistical summary of observations, although it is inferred from the observations. Nor can the mathematical statements regarding a taxon detected by my coherent cut kinetics procedures be deduced from the observations (although there is a search algorithm). How is this possible?

Consider the MAXCOV procedure (see Meehl, 1973; Meehl & Yonce, 1996). From the data we compute the *yz* covariances of cases lying in successive *x* intervals. These numerical values are manifest in that they are simply statistical summaries of the observations. But what motivates these calculations? Why would anybody want to plot a graph of the *yz* covariances in *x* intervals? It is motivated by a postulated latent mathematical structure stated in the General Covariance Mixture Theorem. It is the conjecture that there are two classes of individuals, whatever they are and however they got that way, for which there are two latent distributions on these three manifest variables.

The theorem, shown in Table 1, holds (ignoring sampling error) both for the sample and for any subsets of it (e.g., as defined by the x intervals). On the basis of that postulated structure we predict the shape of the plotted covariances (and that prediction will fail if the postulated latent structure is incorrect). Finding the maximum covariance, we multiply it by 4 to infer a latent constant, K. Where does this multiplier 4 come from? It comes from the latent equation that says the maximum observed covariance occurs in the x-interval where the taxon probability $p = ½$ (inferred, not observed). The constant K is conceived as the product of the y and z separations, which are not observed by us but which are yet another feature of the postulated latent structure. Interpreting K as the product of differences of latent means, none being computable as observational summaries, we use K as two of the coefficients of quadratics to find the taxon proportion p_i in each x interval. This proportion is, again, not computable from the observed yz covariances except via the postulated latent situation that motivates the quadratic. If that conjectured latent structure does not exist, the inferred p_i values do not denote anything, nor does the taxon base rate $\hat{P}$ obtained from them by summing over intervals. The entire procedure deals with latent values inferred via a postulated structure.

Although the manipulations are performed on the observed quantities, the rationale for these manipulations and the numerical values of these latent quantities inferred from the results of the manipulations are all predicated on the postulated latent structure. The General Covariance Mixture Theorem as a statement in the formalism is not mathematically derived from the observations; rather, it is postulated (conjectured), and from it are deduced statements that warrant the algorithmic search procedure for inferring such things as latent classification rates, hitmax cut location, and base rate. Hence, it is correct as a purely formal statement to say that the various expressions and numbers that occur in the taxometric analysis have surplus meaning in Reichenbach's (1938) sense. They are statements about something latent because their content is not merely summaries of the observations such as a mean, standard deviation, or correlation coefficient would be. Thus, using the dispersion of $\hat{P}$ as a consistency test to detect pseudotaxonicity, one properly says that when this consistency test is failed (by the base rate estimates scattering too widely), the formal notation $\hat{P}$ *designates* but does not *denote*—it has a meaning, but it lacks a referent. If there is no taxon, there is no base rate, hence the notation $\hat{P}$ and its numerical value do not refer to anything.

The distinction made here does not depend on a particular philosophy of science (e.g., positivist, fictionist, social constructionist) or theoretical preference (e.g., cognitive, psychoanalytic, hereditarian, behaviorist); it is formulable in purely logico-mathematical terms. A psychometric procedure is latent if the formalism employs variables that are not observable, statistics that are not simply summaries of observable variables, or constants that are not observable. The fact that we can write a computer program to calculate certain values does not mean there is no latency involved. In taxometrics, while indicator variables x, y, z are observables, their latent means are not mere data summaries, the taxon rates p_i in each interval are inferred, and the constant K used in MAXCOV is inferred from the observed statistics via the postulated latent structure but not directly deducible from the data simply by the definition of a mean or variance. Clearly, Reichenbach's (1938) surplus meaning exists here.

Surplus Meaning of Interpretive Text

In most psychometric contexts, formal and interpretive surplus meanings are associated, but they are neither necessary nor sufficient for one another. A taxometric analysis is always latent mathematically, but a minimalist interpretation attributes no properties to the taxon members other than their indicator scores. However, it seems odd on the purely formal-numerical interpretation to ask whether an individual in an interval is a taxon member or not when no other attributes are assigned; but if that were meaningless, it would also be meaningless to speak of the taxon probability p in the interval, which is a necessary part of the formalism and implicitly defined by it.

Considering the *embedding text* that empirical science associates with a calculus, I distinguish the *operational text* from the *interpretive text* (Meehl, 1990a, 1990b). The operational text is in observational language and defines or connects portions of the formalism with observational predicates and functors. This part of the embedding text provides the "upward seepage" component of empirical meaning to a scientific theory. Some say this provides a complete explication of the implicit definition of theoretical terms and that is all there is to it. I think this is an inadequate analysis. There is also an interpretive text that is not a direct data linkage but that characterizes the theoretical entities in some way that interprets the formalism. For instance, in my theory of schizophrenia, coordinating variable *x* in the formalism with fine tremor as measured by the Dunlap apparatus is operational text, contributing to the implicit definition of the open concept schizotaxia (Meehl, 1962, 1977, 1986, 1990c). Explaining the schizotaxic syndrome in terms of aberrated signal selectivity at the synapse (hypokrisia) belongs to the interpretive text; schizotaxia could exist without hypokrisia as its causal explanation.

Someone critical of the explanatory value of taxometrics might object that, although a taxonic finding has a surplus meaning in the formal sense, such minimal “latency” is not latency in a theoretically interesting sense, but that something else by way of interpretive text is needed. That more stringent meaning of the term *latent* is a departure from conventional usage in psychometrics and general mathematics that would call for justification.

Without arguing semantics, one can recognize a distinction between a minimalist interpretation of a taxonic finding (in which the only surplus meaning lies in the mathematical fact of one-directional deducibility) and the addition of further semantic content by the interpretive text. This additional interpretation would consist of attributing nonindicator (theoretical, unobserved) properties to the members of the taxon, or of a causal inference explaining the taxon’s origin, or both.

It might seem that a causal inference presupposes added meaning about attributes; however, although that would usually be the case, it need not be. How much and what kind of surplus meaning obtains from a clear taxonic finding depends on the amount and kind of information available and the taxometrician’s risk-taking proclivities. For example, suppose I am provided with nothing but a set of indicator scores labeled *x*, *y*, *z*, *u*, *v*. If I get a clearly taxonic result with good satisfaction of the consistency tests, I am able to make formal-numerical statements only: that there is a taxon having base rate $\hat{P}$, that there are such and such separations, the location of hitmax cuts on each variable, the valid and false positive rates achieved when individuals are classified by those cuts, and the Bayes classification probabilities.

If I am now told the nature of the indicators, that they are such soft neurological signs as saccadic inhibition, subclinical Romberg, and regular fine tremor, I infer that something about the central nervous system is involved because I know such behavior phenomena are controlled by the nervous system rather than by the stomach or by the kidneys.

Perhaps I may not be told the nature of the indicators, but told only that the sample is composed of siblings and parent pairs of an unspecified group of probands, with individuals identified as each proband’s father, mother, or sibling. To draw a plausible genetic conclusion it is only necessary to know the basic concepts of genetics as revealed in the fruit fly or in the mouse, to know about dominant and recessive genes, penetrance, and the like. Given the family relations, I may conclude that it is a hereditary condition (not necessarily an illness) and is produced by either a dominant gene or a polygenic factor with a threshold at 50%. I could apply some of the theorems proved in Golden and Meehl (1978), such as that the covariance of parent pairs for each indicator should, on the hypothesis of a dominant gene, be equal to $-1/4$ times the square of the separation (which has been estimated by taxometric analysis). If those point predictions are correct within tolerance, I would confidently assert that we are dealing with something that is heritable in this way.

Given knowledge of both the nature of the indicators and the family relations, I reject the threshold theory because the parent pair covariance prediction would require a sizeable negative assortative mating, and I assume that people do not select their spouses on the basis of soft neurology.

If, finally, I am told that the families to be studied were chosen because they contained a schizophrenic proband, but only a small proportion of the sibs and parents were themselves schizophrenic, I conclude that the soft neurology is not a psychosomatic by-product, but that schizophrenia is apparently a rare complication of the inherited neurological disorder.

The epistemological point here is simple and not in dispute. Although there is no algorithm for inductive logic (except in special cases like Bayes’ Rule), there is a negative principle of empirical knowledge, namely Carnap’s (1950) Total Evidence Rule, that in making theoretical interpretations of facts it is not permissible to ignore evidence that is logically germane to the theory. That does not mean one must be able to explain every fact, especially in the life sciences where theories are always incomplete. It does not even mean that no fact can contradict the interpretation, because there can always be problematic auxiliaries (Meehl, 1997, 2002), and although a theory may be literally false, it may yet have sufficient verisimilitude that we can hope to improve it. In this respect, taxometrics is like other statistical methods which present relationships among facts open to alternative theoretical interpretations.

In writing about taxometrics, I have repeatedly emphasized that no statistic is self-interpreting. Although this maxim is not universally correct, it is almost always true even for simple summary statistics, such as a mean or standard deviation, because in those cases we are almost always attempting to generalize to a population value. This movement from sample to supply raises questions about distribution shape, the central limit theorem, the randomness of the sampling procedure, temporal stability of the population parameters, contribution of measurement unreliability to the individual differences variances, and so on. However, that is not the question involved here, where the manifest–latent distinction can be formulated on the idealized assumption that we know the parameter values of the indicators. Thus a more careful formulation of the maxim would be that no statistical procedure involving formally surplus meaning in the mathematics is self-interpreting.

The Total Evidence Rule is so obvious a principle of rationality that it may seem too general and trivial to serve as a methodological tool in thinking about taxometrics; but the schizotaxia example shows otherwise. If we ask what can be legitimately inferred about the nature of a taxon or its causal origin, the answer depends on what information the taxometrician has. Given nothing but the individuals' indicator scores, numbers without embedding text, the taxometrician can only decide whether there is a taxon and, if so, may estimate the latent numerical values (e.g., base rate, hitmax cut, valid and false positive rates). What further inferences are reasonable with varying confidence depends on which subsets of information are provided.

To object that a taxometric finding "does not imply [textual interpretation]" is misleading because, strictly speaking, a set of observed facts never implies their explanation. One must always distinguish a substantive scientific theory *T* from a statistical hypothesis *H* that the theory implies and by which the theory may be appraised (Meehl, 1978, 1997). All fact-to-theory inference is a matter of inductive, not deductive, logic; used in a deductive fashion, the result is a logically invalid syntactical form. The question is what it is reasonable to infer, where infer has the weaker sense of inductive logic. In the example given, presentation of all of the information could properly lead the taxometrician to infer that we have an asymptomatic neurological taxon inherited as a Mendelian dominant of complete penetrance, of which only a minority develop florid schizophrenia. In making such inferences, one relies in addition to the specific study data on background knowledge, including here the concepts and laws of genetics, fallible signs and symptoms of disease, specificity and sensitivity in epidemiology, classical psychometrics (e.g., unreliability, factor loadings), and the like. It is simply that what one can reasonably infer from statistical findings depends on what sorts of things one already knows.

If the background knowledge is considerable and well confirmed, for a taxometrician to steadfastly resist interpretation becomes unduly cautious, as it would be in the schizophrenia example. One must always distinguish between research strategies and questions decidable by mathematics and logic; although an inductively cautious investigator cannot be coerced to be more conjectural, it can become unreasonable not to draw an inference as evidence accumulates.

Taxometrics was invented to help in appraising a theory (schizotaxia), and I prefer that approach to a sheer inductive scanning (analogous to exploratory factor analysis). Conjecturing the existence of a latent taxon, however loosely or tightly the theory is articulated, one selects diverse candidate indicators whose construct validity is plausibly supported by theory, clinical experience, nontaxometric findings of content, predictive, and concurrent validity, and internal psychometric structure.[1] I do not, however, oppose exploratory taxometrics that is not theory driven, as shown in Meehl (1999) where the TAXSCAN procedure is briefly described and illustrated. In that procedure one starts with a set of diverse variables that might serve as valid indicators if taxa exist in the population, identifies clusters by pairwise correlations, and analyzes such provisional clusters with multiple taxometric procedures and consistency tests to decide whether they are taxonic or not. Because of our emphasis on consistency tests as essential, the process, although initially exploratory, has the epistemic character imputed to confirmatory factor analysis, which makes analogy and disanalogy with factor analysis somewhat complicated. It is incorrect to view taxometrics and factor analysis as competitive procedures.

[1] Cronbach and Meehl (1955) erred in listing the four kinds of validity almost as if they were on the same level, whereas construct validity is evidenced by the other three kinds along with internal psychometric structure (Loevinger, 1957). In our defense I would say that the full text makes it clear that we took internal psychometric structure into consideration. Omission of it in the summary list of validities by Cronbach (inventor of coefficient alpha) and Meehl (knowledgeable about implicit definition in philosophy of science) is puzzling.

Wherever indicators are valid for a taxon, factor analysis will yield a factor. That predictable result means factor analysis cannot yield a decision as to the taxonic question.

Finally, it is not helpful to compare taxometrics with cluster algorithms here. Cluster analysis procedures vary as to imputed latency and what kind. Ward's method derives from a postulated latent structure, whereas other cluster methods do not; and cladists and pheneticists differ about theoretical aims even when using the same search algorithm. In addition, cluster analysis procedures lack consistency tests.

A crucial feature of the coherent cut kinetics method of taxometrics is reliance on consistency tests to provide multiple lines of evidence about the latent structure. I have always advocated that taxometricians should use multiple taxometric procedures and consistency tests, and I have called my taxometric method *coherent* cut kinetics to emphasize that the results will be in reasonable agreement if the underlying situation is a certain structure. If the latent structure is taxonic, one sort of coherent picture will emerge; if it is non-taxonic, a different sort of picture will emerge; if what emerges is unclear, judgment should be suspended until more evidence is examined.

If consistency tests are so important, why are they not mentioned as such in the more developed sciences? Sometimes they are. Whewell (1847/1966), inventor of the word *scientist* and author of the first formal treatise on philosophy of science as it is practiced, attached pivotal importance to what he called consilience of inductions. For scientists such as astronomers, chemists, geologists, and geneticists, convergence of distinct lines of evidence for the existence of a theoretical entity and its numerical values is simply how one does science, no special word is needed. Social scientists seem often not to understand this and, perhaps because of our emphasis on statistics, mistakenly tend to suppose that having a maximum likelihood estimation of a hypothesized value is more important than having several inferential paths to it converging within tolerance.

Summary

A statistical procedure is wholly manifest if it merely summarizes the observational data. The inferential step from statistic to parameter (pure sampling error) is not relevant in discussing latency inherent in taxometrics; sample mean $\bar{x}$ and population mean μ_x are both manifest in the present context.

A taxometric analysis is latent in the conventional sense that factor analysis, multidimensional scaling, latent class analysis, latent trait theory, path analysis, and structural equation modeling are latent. This formal latency exists because the mathematical expressions employed to infer numerical values from the observational statistics are not deducible from the data but are postulated formal features of a structure neither directly observed nor deducible from data-summarizing statistics. The one-way derivability (from postulated latent structure to data relations but not conversely) provides the epistemological surplus meaning that marks formal latency.

Formal-numerical latency may be supplemented by an interpretive surplus meaning in which unobserved properties are attributed to the taxon members or a causal origin of the taxon is imputed. How much of this interpretation (beyond minimalist taxonic inference) seems warranted is a judgment call depending largely on the risk-taking style of the investigator. Lacking an algorithm for inductive logic, we can only rely on the logician's Total Evidence Rule. Every germane fact and plausible competing theory must be taken into account; they need not all be satisfactorily explained—Kuhn's "puzzle solving" is a legitimate job in normal science—but they should not be ignored. The strategy and tactics of taxometrics is basically the same as in other scientific procedures, and there is no mechanical algorithmic substitute for a researcher having factual information, fruitful mathematics, perspicacity, and good luck.

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