
The main thesis of the book is in its title: for sociology to become scientific it has to make predictions. Predictions refer to assertions about yet unexamined facts, be those in the past, present or future. The authors, distinguished sociologists, are not naïve. They are painfully aware of the many difficulties and complexities of their chosen path and try to address many of them. (Curiously, they completely ignore similar arguments made by economists including Milton Friedman's famous 1953 essay on the methodology of positive economics.) Along the way they pick fights with an impressively varied set of schools of thought: interpretative sociology, postmodern sociology, Marxist sociology, historical sociology, ethnography, quantitative sociology based on multivariate statistical analysis and functionalism to name a few. Karl Marx, Max Weber, Erving Goffman, Talcott Parsons, Otis Dudley Duncan, Nicos Poulantzas, Judea Pearl, and Joe Feagin are some of the scholars they call on the carpet. This is an ambitious and, in its own way original book, that aspires to redirect the entire sociological enterprise.

Their point of departure is that by the standards of Newtonian and Einsteinian physics, sociology falls short as science and worse, recently many sociologists explicitly abandoned the scientific aspirations of the discipline taking a turn that is ideologically driven, empirically vacuous, and rooted in a relativistic epistemology. The book proposes an alternative. Sociology is about empirically testable assertions or predictions about clearly defined units (people, cities, countries, events etc.). Of any two theories the one that predicts better should be preferred. To assess predictive power, they list no fewer than seven dimensions of comparison. Testability means the theoretical statement can be translated in some agreed way into something that is measurable. The authors are aware not just that the measurement process is complex and rarely unambiguously determined by custom or the theory, but repeated tests can give different results. Predictive accuracy is assessed by some measure of association that can gauge its direction, form and degree. Acknowledging that measurement problems can affect accuracy and that the comparison of the accuracy of predicting ordinal vs. ratio/interval (as well as continuous and categorical) variables may be difficult, they throw up their hands and write that “correct predictions though only as to the direction of the association [...] should be an occasion of wild celebration” (p.40). The troubles don’t end there. What if the relationship is non-monotonic? What if there is a threshold below or above which the relationship doesn’t exist? What if the prediction involves several variables in a complex pattern? Here, as in so many places, a rhetorical flourish is offered in place of an answer. Within the framework of multivariate statistics there are answers to these questions, but the authors take a dim view of multivariate analysis to which we will return. Range refers to the breadth of the kinds of units the prediction is about, while scope is the variety of dependent variables the prediction covers. Then we get to what I consider one of the book’s more interesting contributions, the authors’ insistence on the importance of what they call time-space specificity. Our sociological theories tend to be vague as to where and when they apply. Worse, most theories are fuzzy about the temporal distance between the factors they connect. If growth in inequality predicts a rise in crime, does that happen everywhere? Just in the U.S.? Just in large cities? Does that happen always? Throughout the 20th century? Only in the 1990s? And does the growing income gap result in more crime immediately? Only after a year? Or a decade? While it runs counter to the range criterion, without being very specific about space and time, we cannot evaluate predictions. Discriminatory power is a bit opaque but my best understanding is that it is the theory’s ability to correctly distinguish among its own theorems (conclusions) through the
evaluation of repeated predictions. Finally, parsimony is the ratio of theorems to premises in a given theory.

The book also offers us a method of “formal theory construction.” While the authors at many places lament that sociology does not use the language of mathematics, in their three-part phrase “formal” refers to “theory construction” rather than to “theory.” (There are no equations in the book.) To construct theory first we have to clarify our unit terms (what is usually called “unit of analysis”). The authors are aware that these units always require some definition that are theoretically loaded (what is a city?), but they set that aside as the “extrinsic part” of the theory. They also understand that the unit terms can change over time. The unit terms have characteristics that are similar to variables, except the authors point out that the concept of the variable falsely suggests that the characteristics are independent of the unit terms they refer to. Residential density, for instance, means something entirely different when the unit is an apartment, a city block or a city, so they prefer the phrase characteristic terms. There are three types of characteristic terms: constructs (ideas not intended for measurement), concepts (clearly defined and measurable) and referentials (operationalized concepts). Characteristic terms are linked by relational terms (e.g., greater... less). There are also additional words in theories stated in natural language they call residual terms. (Confusingly, they list logical operators “and,” “or” as examples.)

Theories then have axioms, postulates and theorems, all used here differently than in mathematics. Axioms are assertions of relationships that feature constructs while postulates link only concepts. If concepts are translated into referentials, postulates are transformed into testable theorems. One suggestion of the authors is that if we start with a construct and we link it to more than one concept, each of which we subsequently convert into a referential, we can logically deduce a set of theorems. They call it the “sign rule.” Suppose we have the “nebulous” construct of class conflict. (Unlike the authors, I don’t think class conflict is necessarily nebulous. One can measure it by counting strikes, workplace sabotage, political demonstrations etc., but that is not my point here). If we can relate class conflict to the concept of income inequality by stating that (1) more income equality goes with less class conflict, and to intergenerational occupational social mobility by stating that (2) more mobility too begets less class conflict, we can deduce from these two “untestable” axioms a third testable theorem that (3) more mobility will go with more equality. (The authors add a third concept, crime, to mobility and equality, but I will keep it simple.) Once we transformed those two concepts (mobility and equality) into well-measured referentials, (and properly specified the units, time and space etc.) we can test the theory by making predictions like this one: industrial countries in the second part of the 20th century with more intergenerational occupational mobility will have more equality of personal income. Alas, these predictions do not follow from the two axioms about class conflict. Imagine that mobility and equality are two alternative paths to decreasing class conflict. There will be countries like Denmark, with lots of mobility and equality, and miniscule class conflict. In others both mobility and equality will be tiny, and class conflict will be rampant. Finally, countries with small mobility and large equality and those with great mobility and little equality will be somewhere in between. If the countries are evenly distributed among these four types, the relationship between mobility and equality will be zero. If the last two types dominate, the mobility and equality will have a negative relationship, not the positive one suggested above. Take another construct: discrimination. Being a woman and being African American are both positively related to discrimination but gender and race are unrelated.
This takes me to the book’s curious crusade against explanation and causation. We are told we only *read* explanations into (predictive) theories. The same prediction will allow for many stories. Fixation on causation that is impossible to establish anyway is just an enabler of explanations, the scourge of scientific sociology. Yet the authors themselves cannot get away from the asymmetry of relationships implied by causation. They talk about dependent and independent variables, use “because” to link concepts, and more importantly, their predictions must decide temporal order (time-specificity).

Another odd battle they choose to fight takes on multivariate statistical analysis. This is strange because multivariate models are all about prediction. Their best argument against these models is that they often cover up complicated relationships by averaging patterns across the sample. There are ways to deal with that problem in multivariate models but the authors simply ignore them. As a result, in the authors’ world predictions are based on bivariate relationships occasionally disturbed by a third variable, a form of averaging of its own kind.

This is not an easy book to read. Abstract discussions are peppered with unexpected colloquialisms, oratorical hand waving is frequently substituted for arguments. In the end, we learn more about why prediction is difficult than how to do it successfully. Yet, at a time when sociology is challenged both by new areas of scientific expertise, such as sociogenomics, socio-biology and data science armed with powerful new technologies, and by aggressive political attacks hostile to expertise of any kind, sociologists cannot take a pass on trying to find the best way to settle disputes as good scientists should: with empirically grounded, rational arguments.

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