Some Notes on the History of Probabilism in Economics

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Abstract
A tension has long existed between the idea of economics as a science of society and economics as a science of individuals in society. Throughout the nineteenth century this tension often took the form of an ambivalent attitude toward the development of probability and statistics.

This paper traces the history of statistics in economics and its implications for some important theoretical debates in the early twentieth century.

From the beginnings of political economy in the eighteenth century, economists, like other social scientists, have sought to do for the study of the social order what physics had done for the study of the natural order. This is why many of its metaphors imitate physics: constrained maximization of utility, equilibrium, self-regulating mechanisms, and even Marx's "laws of motion" of capitalism.[1]

At the beginning of the last century, motivated by a desire for social change and the realization that measurement of social phenomena would be required to argue successfully for such change, social scientists developed techniques to analyze statistical data (Kendall 1972, Menard 1980). Methodological difficulties disappointed the hopes of early proponents. The reaction was a retreat to the axiomatic method, since this seemed to be the source of strength for classical Newtonian physics. At the same time, toward the end of the century, social sciences, especially economics, began to play a more conservative role. Embracing the then-current non-probabilistic paradigm of mathematical physics provided an underpinning for maintaining the status quo (Menard 1988, Mirowski 1989).

Since at least the time of Walras, mathematical economists have dominated the profession by enforcing a belief that "pure" theory of this particular kind is superior to empirical investigation. Statistics are meaningful in this view, only to the degree that they elucidate the variables deemed pertinent by economic theory, properly conceived.

This paper traces some nineteenth century attempts to apply probability and statistics to economic theory, and suggests that these attempts were rejected in the 1930s and 1940s by both neoclassical and Keynesian economists.

The Changing Uses of Probability

Mathematical probability theory was developed in the eighteenth century as a method to quantify and measure degrees of uncertainty or reasonable belief. Some of the earliest attempts to apply probability concerned rules for weighing evidence in courts of law (Daston 1987). In keeping with the Enlightenment view that human behavior is governed by mechanistic laws not unlike Newton's laws of motion, rational thought was viewed as the outcome of a mechanical process and hence capable of mathematical description. Because the subjects of rational thought were intelligent men(!) exercising moral judgement, it was further thought that probability theory could be used to codify both legal procedure and statecraft.

In the early part of the following century, utilitarian philosophers conceived a similar calculus of social policy based on mathematical aggregation of individual utility. But individuals are too numerous for such a quasi-democratic project to be carried out directly; a more promising starting point for the formulation of public policy was observation of group behavior. At the same time, a potential source of information on group behavior was conveniently available from emerging nation states with a zeal to collect data on their citizens. Such large quantities of data, or statistics, encouraged some to think of imitating the experimental method of the natural sciences. In Britain, concern for social problems led to the founding of the Royal Statistical Society in 1833 (Cullen 1975, p.77). Together, the law of large numbers and the liberal ideal of creating social policy became the basis of a project for the social sciences to discover social "laws."[2]

Adolphe Quetelet, a Belgian mathematician and astronomer, and an early correspondent with members of the Royal Statistical Society, exemplifies the search for this ideal in his hope to use statistical information to reveal underlying regularity. He first became interested in applying probability theory to social statistics in the 1820s while designing census procedures. In effect, he proposed to estimate population by sampling births and deaths from a subset of locations within a country (Stigler 1986, pp.163-167). As he continued to compile demographic data, Quetelet observed that many of these variables, like observations of celestial objects taken at different times, followed a normal distribution. By analogy, Quetelet reasoned that social variables could be understood as determined by a combination of stable traits or "penchants" in a population plus an error term. The set of means of these physical and social traits then described what he dubbed *l'homme moyenne*, or average man, an actually existing ideal that can be revealed through statistical investigation. Quetelet recognized that observations over space in a census enumeration were analogous to observations over time in astronomy, and that the penchants of *l'homme moyenne* could be inferred from observation of aggregate data rather than on pure theoretical grounds.

Despite the promising appearance of the normal distribution, Quetelet's work did not lead directly to the application of probability theory to social statistics. Two major difficulties were pointed out by others and acknowledged by Quetelet himself: First, any subset to be sampled would always differ in terms of variables that were known to affect population changes. Faced with a myriad of potentially important variables and no knowledge of which are truly important, many researchers, including Quetelet, were reluctant to make the assumption of homogeneity. And second, it was argued that to elevate statistical averages to the status of objective truth was to hypostatize a mathematical construct.

Probabilism in Natural Science and Economics

Probabilism on the theoretical level took root and developed in the natural sciences in the nineteenth century. In physics, both James Clerk Maxwell and Ludwig Boltzman proposed that gas laws be formulated in statistical terms in view of the impossible complexity of performing calculations of momentum for millions of independent particles (Porter 1986).

A similar shift in focus took place in biological science. At the beginning of the century there was no science of biology; instead, separate areas of study included medicine, botany, and natural history. By the end of the century, there was a shift away from efforts to generalize from individual specimens and toward theories where the species or population was the center of attention and the individual appears as a derivative concept.

Emile Durkheim provides a good example of the point reached by Quetelet's research program at the end of the nineteenth century. *L'homme moyenne* now reappears as *"l'ame collective,"* the collective will. In this move, Durkheim appealed to organic concepts drawn from biology:

> The living cell contains nothing but mineral particles, as society contains nothing but individuals, Yet it is patently impossible for the phenomena characteristic of life to reside in the atoms of hydrogen, oxygen, carbon, and nitrogen.
>
> Let us apply this principle to sociology. If, as we may say, this synthesis constituting every society yields new phenomena, differing from those which take place in individual consciousnesses, we must, indeed, admit that these facts reside exclusively in the very society itself which produced them, and not in its parts, i.e., its members (Durkheim 1897, p.xxxix).

Durkheim thus opened the way for probabilistic thinking in sociology by focusing on society as a whole rather than individuals within society.

In each of these cases, there is a move away from mechanistic determinism. Plants and animals, including humans, are no longer investigated as clock-like machines; societies are not viewed as a collection of rational individuals. Each individual in a population is unique and not simply a departure from a Platonic essence.

The move away from determinism was not always straightforward. In biology, advocates of mechanistic determinism, clinging to the paradigm of Newtonian physics, insisted that organisms could be understood purely in terms of the known laws of physics and chemistry. To account for living processes, an invisible force, *vis viva* was invoked, justified as analogous to Newton's invisible force of gravity. (Or, in a different context, analogous to the invisible energy of utility.) Biologists eventually accepted the view that although the postulation of an unknowable external "life force" is unnecessary, organisms are far too complex to be reduced to laws of physics. Modern biologists speak of living processes but do not attempt to define "life" as a real substance.

The Twentieth Century: Tinbergen vs. Keynes, Koopmans vs. Vining

In economics, by contrast, there has been resistance to any movement away from determinism. The basis of economic analysis remained the behavior of individuals as members of groups rather than the behavior of groups themselves. And the paradigm for this behavior was, and remained, Newtonian mechanics.

Thus when probability theory and statistical information about economic affairs were joined in the early years of the twentieth century, the kind of movement that took place in other disciplines failed to materialize. At times, as with Wesley C. Mitchell's investigations of business cycles, econometrics bordered on an attempt to discover the empirical "laws" Quetelet had in mind.

But while economists rejected probability in theory construction, they often accepted it at the level of inference. (Physics, by contrast, whether classical or quantum, never accepted stochastic inference.) This was the issue in the well-known debate between Keynes and Tinbergen. Tinbergen's report to the League of Nations was an early attempt to construct and test an economic model. In the spirit of Walras and Marshall, Keynes regarded economics as "a branch of logic," and "essentially a moral science and not a natural science" dealing with "motives, expectations, psychological uncertainties" (Keynes 1938, pp. 296, 297, 300). He regarded Tinbergen's method as "one neither of discovery nor of criticism. It is a means of giving quantitative precision to what, in qualitative terms, we know already as the result of complete theoretical analysis" (Keynes 1938, p. 308). In this respect, he remained in the determinist tradition and rejected the possibility of induction in social science, a position that had been noted in Keynes' *Treatise on Probability* by Frederick Mills, who accused Keynes of "throwing empiricism overboard" (Mills 1924, p. 62).

But Keynes reason for rejecting of Tinbergen's work—that social systems are organic rather than atomic—is also one that removes him and his followers from neoclassical economic thought altogether. The methodological point Keynes makes against Tinbergen is that not all probability is numerically measurable.[3] Unless this is the case, then the kind of inductive inference that Tinbergen (and later Haavelmo) employs is not valid, and this is not the case in economics because of the organic context of human action. In economics, observation does not provide a ground for induction, but merely a reinforcement of probability arrived at *a priori*. This is the sense in which we "know already" certain things through theoretical analysis. Put another way, when the object of a science exhibits the organic complexity of economics, *mathematical* statistics is limited to the field of description; it is methodologically incorrect to extend its role to one of inference.

Less than a decade later, Tjalling Koopmans labelled the work of Burns and Mitchell as "measurement without theory," criticizing the two for falling short of the Newtonian ideal and remaining instead in the "Kepler stage" of economics (Koopmans 1947, p.161). In a response, Rutledge Vining took Koopmans to task for insisting that the individual be the unit of analysis, rather than group behavior expressed in the business cycle, as Burns and Mitchell do. In the ensuing debate

Vining and Koopmans appear to talk past each other. Koopmans, relying on the justification for statistical inference introduced by Haavelmo in his 1944 *Econometrica* article, argues that the outcome of "the behavior of any group of individuals, and the outcome of any production process, is determined in part by many minor factors, further scrutiny of which is unrewarding" (Koopmans 1947, p.169). Vining in turn notes that Koopmans' reason for this position is based on estimation considerations rather than interest in an underlying reality.

The debate can be interpreted as one concerned with mathematical technique, but a more fundamental point of contention is whether economic analysis should be grounded in individuals, as Koopmans and Haavelmo do, or in the behavior of groups, as Burns and Mitchell do.

In developing their respective positions, Koopmans implicitly (if obliquely) argues that economic theory is equivalent to atomistic and deterministic Newtonian physics, while Vining continually makes references to biological analogies. Vining's position is not a defense of measurement without theory, but of measurement without *neoclassical* theory. In his view probability theory is "fundamental as a guide to an understanding of the nature of the phenomena to be studied and not merely as a basis for a theory of the sampling behavior of estimates of population parameters the characteristics of which have been postulated" (Vining 1949, p.85).

Lawson (1989) compares Vining to Keynes for emphasizing the organic, rather then the molecular level in his analysis. But Vining insists that individuals may not always be the appropriate level at which to begin analysis, while for Keynes, the individual decision-maker alone is appropriate. In criticizing econometric methods, Keynes was defending an orthodox position about measurement with origins in the eighteenth century. This is why, like Walras, he views economics as a moral science. It also accounts for his use of the term "legal atoms" with its strong juridical overtone that harkens back to early applications to rules of evidence (Carabelli 1988, Daston 1987).

Conclusion

From Quetelet to Burns and Mitchell, attempts to develop an empiricist economics have foundered at the point of applying probability theory to statisitcal data. The result of the Keynes-Tinbergen debate of the 1930s together with the Koopmans-Vining debate of the 1940s was to reinforce the dominant view of statistics among economists established in the last century: they are meaningful only to the degree that they confirm pre-conceived theories that are essentially deterministic.

For orthodox neoclassical economists, this reinforcement was established by Haavelmo's 1944 publication of "The Probability Approach in Econometrics," which Mary Morgan has called a "probabilistic revolution" (Morgan, 1987, 1990). While Haavelmo did provide a new justification for applying probability theory as a tool for data analysis, Morgan's phrase seems too strong if it is meant to imply the type of paradigm shift referred to by Thomas Kuhn (1962) and others.

Post-Keynesian economists such as Davidson (1982, 1991) have taken pains to distance themselves from what they call the "ergodic assummptions" required by orthodox econometrics. The notion of economics as a moral science with an organic logic does distinguish Keynes from neoclassical economists, as many writers, including, most recently and extensively, Carabelli (1988) have pointed out. However, it remains clear from his criticisms of Tinbergen that Keynes shares with neoclassicals the idea that economics is about the behavior of decision makers within larger frameworks. In this respect, Keynes remains in the determinist tradition and does not extend his methodology to theory construction.

In this respect, too, economics continues to lag behind the natural sciences; a striking outcome, since many of the advances in the latter are the result of developing tools originally conceived for the former.

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Notes

1. See Mirowski, 1984, 1987, for additional examples.
2. For example, Malthus' laws of population and Marx's "natural laws of capitalist production."
3. "By saying that not all probabilities are measurable, I mean that it is not possible to say of every pair of conclusions, about which we have some knowledge, that the degree of our rational belief in one bears any numerical relation to our rational belief in the other" (Keynes, 1921, p. 37)

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