Probability, Statistics & Political Economy in Mill's *Logic*

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Abstract
J. S. Mill is often cast as an opponent of the use of probabilistic reasoning in science. His attitudes were more complex and it is necessary to distinguish his position as a general methodologist of science from his position as a methodologist of political economy and of other special sciences. As a general methodologist Mill found a role for probability in justifying conclusions. He also considered the use of statistical reasoning valid in some special sciences. His successors, Jevons and Edgeworth, disagreed with him for they believed that the conditions of those special sciences also obtained in political economy.

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Introduction

By the late nineteenth century the "rise of statistical thinking" as Porter (1986) calls it, was beginning to change economics. Jevons and Edgeworth were analysing quantitative data using methods based on the theory of probability.[1] Did this development find any support in the "connected view of the principles of evidence and the methods of scientific investigation" given in John Stuart Mill's *System of Logic*? The question is pertinent because the *Logic* (first edition 1843, eighth edition 1872) was probably the most widely read of nineteenth century accounts of scientific method. It is even more pertinent to the case of economics for there Mill's views had a profound and long-lasting influence.

The rise of statistical thinking was no single simple development and the relationship between the *Logic* and the development of probability and statistics is complex. This issue or, rather, cluster of issues, has not been treated thoroughly in the literature.[2] Despite this, there seems to be a general sense that Mill was hostile to statistical and related thinking. Cartwright (1989, 176) asserts that Mill was "opposed" to the statistical laws of Quetelet's social physics. Cohen (1987, 321-2) places him with Comte as an "opponent of statistical based science or knowledge", saying that he takes a "stand ... against statistical arguments or the improper use of probability in science or social science." Such views are not confined to recent commentators. Jevons (1883, 200) wrote, "it passes my conception how two such men as Auguste Comte and J. S. Mill could be found depreciating [the theory of probability] and vainly questioning its validity." J. M. Keynes (1921, 298) pointed to Mill's "complete failure to grasp with any kind of thoroughness the nature and importance of the theory of probability."

The observations behind these judgements have some validity but, it will be argued below, they should not be accumulated into one big observation that Mill was hostile to statistical thinking. Mill's position was complex, unlike Jevons's wonderfully simple position: "to eulogise the theory [of probability] ought to be as needless as to eulogise reason itself." (1883, 200).[3] Mill was no eulogist but neither was he an undiscriminating opponent. He criticised the conventional treatment of the foundations of probability and some of the standard applications. Yet he discussed probability because he gave it a central role in science, unlike his contemporaries Whewell and Herschel who confined it to technical questions of measurement. Mill saw the power of statistical arguments but, when he came to identify the social sciences to which such arguments were relevant, they were not the ones he cultivated himself. In particular, as Peart (1995 and 2001) emphasises, Mill found no use for statistical arguments in political economy. It will be argued that when successors like Jevons or Edgeworth found a use it was not primarily because their understanding of statistical arguments was any different but because they were concerned with different questions in economics. They took arguments that Mill recognised as valid when applied to, say, criminal statistics and applied them to price statistics.

This is the plan. Sections 1-3 review relevant features of Mill's philosophy of science in preparation for the discussion of his account of probability in Sections 4-6. Sections 7-9 examine Mill's views on the applicability of statistical reasoning in social science. Section 10 considers the Mill legacy, or rather the Mill legacies.

1. Laws

We need some background before we consider Mill's account of probability and statistics. To begin with, what did Mill understand by a law and establishing a law? [4] The world is subject to laws of nature and it is the object of science to ascertain these. There are "only two modes by which laws of nature can be ascertained: deductively and experimentally, including under the denomination of experimental inquiry, observation as well as artificial experiment." (1843, 865). Mill was less concerned with how discoveries are made than with identifying different types of evidence and the conclusiveness associated with them. His aim was to characterise the circumstances in which each mode—the deductive and the different varieties of the experimental—would be conclusive in establishing a law and also the type of law that would be established.

The circumstances of subjects were very different. Of the "moral sciences", psychology was to be studied by the experimental method, ethology and political economy by the concrete deductive method and history and statistics by the inverse deductive method.

Laws of nature are "the uniformities which exist among the phenomena of nature, when reduced to their simplest expression." (318). The most basic of these are laws of causation; these are invariable and unconditional connections between antecedents and consequences. There are also laws of coexistence. Besides these fundamental laws there are derivative laws that follow from them with certain "collocations of causes." Kepler's laws follow from Newton's laws of motion and a collocation of causes relating to the sun and planet.

Suppose there is enough evidence to establish a law. It may be an ultimate law or a derivative law. Laws with doubtful status are called empirical laws:

> Scientific inquirers give the name of Empirical Laws to those uniformities which observation or experiment has shown to exist, but on which they hesitate to rely in cases varying much from those which have been actually observed, for want of seeing any reason why such a law should exist. (516).

Thus the "periodical return of eclipses" 1had the status of an empirical law until accounted for by the "general laws of the celestial motions."

Empirical laws are suspected of being derivative laws, depending upon more basic laws and usually upon specific collocations of causes. An empirical law has only limited coverage and cannot be expected to hold in circumstances very different from those in which it had been observed to hold. Change the collocation of causes and the law may break down:

> we cannot be certain that it will be true under any variation in the mode of co-existence of these causes. (519),

For this reason Mill emphasised the need for care in the use of empirical laws. The derivation of an empirical law from the relevant basic laws and collocation of causes will show the scope of the law.

Empirical laws are significant in a discussion of statistics for Mill believed that laws disclosed by statistics could not claim to be anything more than empirical laws. Mill's "empirical laws" reappear in the Frisch-Haavelmo ontology as "confluent relations." Koopmans's "identification problem"—see Koopmans, Rubin & Leipnik (63) —is concerned with distinguishing a basic law, a structural equation, from a derivative law of the same form. Cohen's point, that Mill was opposed to statistically based science, is valid in the sense that for Mill laws disclosed by statistics would not be basic laws—but they would be laws.

2. The Experimental Methods

We now sketch the "experimental" mode of ascertaining laws. Much of this material is familiar but a brief review is necessary because important points have been overlooked. The fundamental methods were the method of difference and the method of agreement.[5] The former was relevant to controlled experiment and able to establish laws of causation, the latter to passive observation and only able to establish empirical laws.

> The method of difference is based on the maxim:
>
> If an instance in which the phenomenon under investigation occurs, and an instance in which it does not occur, have every circumstance in common save one, that one occurring only in the former; the circumstance in which alone the two instances differ is the effect, or the cause, or an indispensable part of the cause, of the phenomenon. (391).

Mill believed that the method of difference was decisive in establishing laws of causation. The most secure knowledge is obtained in this way but, because controlled experiments are seldom feasible, most knowledge has to be obtained by inferior methods.

> One such inferior method is the method of agreement based on the maxim (390):
>
> If two or more instances of the phenomenon under investigation have only one circumstance in common, the circumstance in which alone all instances agree, is the cause (or effect) of the given phenomenon.

If it is *known* that there is only one possible cause of the phenomenon under investigation, the method of agreement will isolate it.

The method is rendered "uncertain" when the effect can be produced in several different ways—when there is a "plurality of causes" —for, when two

instances are compared, the causes may be among the uncommon factors. In some fields this uncertainty is a serious weakness. In the "phenomena of politics and history ... plurality of causes exist in almost boundless excess, and effects, are for the most part, inextricably interwoven with one another." (452).

Commentaries on Mill's methods often stop at this point but Mill has more to say on the method of agreement.[6] It is at this point that he advances on Herschel. Mill held that when there are sufficient and sufficiently varied observations the possibility of a coincidence can be ruled out. It is at this point of his argument that probability enters and we shall see what it leads to in the next section. The law will be no more than an empirical law.

The application of "experimental" methods to social policy was Mill's special target in his methodological polemics.[7] He was irritated by:

> those clamorous appeals we daily hear, on the most complicated questions of state policy, to what are termed facts, that is to history and statistics, by men whose knowledge of facts in history and statistics ... is like an insect's knowledge of the great earth: and their inductions, very like an oyster's conjectures of the laws which govern the universe. (1832, 412)

We leave to Section 3 the question whether the experimental methods must necessarily fail in this field.

3. The Deductive Method

The basic laws in any science were obtained by the experimental methods but the more advanced portions of the sciences were generally obtained by the deductive method, Newton's method:

> We suspect that, when any more comprehensive views shall be arrived at, or any greater certainty shall hereafter be attained, in the sciences, physical, moral, or political, than we at present enjoy, it will be in some way as Newton's rather than by the road [of observation]. (1832, 1415).

Mechanics, in particular celestial mechanics, had a powerful influence on Mill's conception of science. According to Mill's understanding of the history of astronomy, there was an empirical stage which culminated in Kepler's work on the orbit of Mars and the establishment of the laws of planetary orbits. The deductive stage began when Newton showed how these empirical laws could be deduced from laws of motion (which had been established by means of experiment) —plus, of course, the appropriate collocations of causes.[8]

Mill described the application of the method to the problems of society as follows:

> [The practical philosopher] must analyze the existing state of society into its elements, not dropping and losing any of them by the way. After referring to the experience of individual man to learn the law of each of these elements, that is to learn what are its natural effects and how much of the effect follows from so much of the cause when not counteracted by any other cause, there remains an operation of synthesis; to put all these effects together, and from what they are separately, to collect what would be the effect of all the causes acting at once. (1836, 336).

Why should this process of analysis and synthesis work? Essential to Mill's account of science was the distinction between situations in which the combined law can be deduced from the separate laws and those in which it cannot. In the

first case, laws for complex 'objects' could be built up from the laws of components. In the second case each level has laws of its own: the laws of water cannot be deduced from those of hydrogen and oxygen. The notion that laws could be added underlies the treatment of the deductive method both in the physical and in the social sciences.

In Mill's terminology, situations in which laws could be combined were characterised by the "composition of causes":

> I shall give the name of the Composition of Causes to the principle which is exemplified in all cases in which the joint effect of several causes is identical with the sum of their separate effects. (371).

The composition of causes, based on the metaphor of the parallelogram of forces in mechanics, was very important in Mill's thinking. Yet it is hardly a well-formulated concept for it is not clear how to compose causes, nor how to sum effects.

The Newtonian method could be used in political economy and in the science of society generally because the composition of causes obtained:

> Human beings in society have no properties but those which are derived from, and may be resolved into, the laws of the nature of the individual man. In social phenomena the Composition of Causes is the universal law. (879)

The deductive method was no a priori method for empirical evidence entered at two points: the basic laws were obtained by direct induction and applicability of the argument to the new situation had to be verified by comparing the conclusions of the ratiocination with what actually occurs.[9] In the *Logic* Mill emphasised that after the "synthesis" came another stage: "verification by specific experience" or comparison of the results of the synthesis with what actually occurs—in astronomy

the comparison of the model orbit with the actual orbit of Mars. The purpose of verification was to expose oversights in the basic premisses and errors of reasoning.

4. The Role of Probability in Science

The leading contemporary works on the philosophy of science, Herschel's *Preliminary Discourse* (1830, 214-9), and Whewell's *Philosophy of the Inductive Sciences* (1840, vol. i, 550-9), treated probability only in relation to techniques of measurement. Mill saw a more basic role for probability; it underpinned the method of agreement.

The association of an event *a* with the circumstance *A* may be the expression of a law or it may be the result of "chance". He explains:

> we may say that two or more phenomena are conjoined by chance ... meaning that they are in no way related through causation; that they are neither cause and effect, nor effects of the same cause, nor effects of causes between which there subsists any law of coexistence, nor even effects of the same collocation of primeval causes. (526)

Phenomena "conjoined by chance", though not causally connected are caused. Mill was a determinist: "whatever happens is the result of some law".

Although Mill would have considered a probabilistic law of causation an incoherent notion, he does not confine himself to perfect associations when discussing the problem of sorting regularities into chance coincidences and regularities indicative of causation. For he recognised that less than perfect associations may have great practical value.[10] In appraising regularities, the "question is not whether the coincidence occurs often or seldom, in the ordinary sense of those terms; but whether it occurs more often ... than might rationally be expected

if the coincidence were casual." The point is illustrated with a nice account of what would be involved in demonstrating a connection between rainfall and wind direction[11]:

> In England, westerly winds blow during about twice as great a proportion of the year as easterly. If therefore, it rains only twice as often with a westerly as with an easterly wind, we have no reason to infer that any law of nature is concerned in the coincidence. If it rains more than twice as often, we may be sure that some law is concerned; either there is some cause which in this climate, tends to produce both rain and a westerly wind or a westerly wind has itself some tendency to produce rain. (528).

Any such connection cannot be an "actual law" since it is not invariable.

5. The Analysis of Coincidences

Mill tackled the problem of distinguishing "coincidences which are casual from those which are the result of law" by means of Laplace's sixth principle, or Bayes' formula, as it is now usually called. Mill (1843, 543, 1145) states this as follows.

> Given an effect to be accounted for, and there are several causes which might have produced it, but of the presence of which in the particular case nothing is known; the probability that the effect was produced by any one of these causes *is as the antecedent probability of the cause, multiplied by the probability that the cause if it existed, would have produced the given effect*..

Before considering how Mill used this theorem we ought to examine his interpretation of probability. In the first edition of the *Logic* Mill criticised Laplace's way of assigning probabilities to events: events are equi-probable when

one and only one of the events will occur and we have no reason for expecting that it will be one of these events rather than another. Mill required that events be treated as equi-probable *only* when experience has shown that the events occur equally often. Mill (1142) suspected any claim to spin knowledge out of ignorance:

> It would indeed require strong evidence to persuade any rational person that by a system of operations upon numbers, our ignorance can be coined into science; and it is doubtless this strange pretension which has driven a profound thinker, M. Comte, into the contrary extreme of rejecting altogether a doctrine which, however imperfectly its principles may sometimes have been conceived, receives daily verification from the practice of insurance, and from a great mass of other positive experience.

What was wrong with much probabilistic reasoning was its inadequate statistical base. Mill thought that "to render [probability] applicable there must be numerical data, derived from the observation of a large number of instances". (1147). When the number of instances was small, the conclusions yielded by the method of agreement "are of no real value except as in the character of suggestions." (436).

After coaching from Herschel, Mill withdrew his objection to Laplace's subjective interpretation of probability.[12] Yet the change made almost no difference to what he considered reasonable applications of probability. Mill (545-7, 1148-50) is so good at bringing out its difficulties that his "application" of Laplace's sixth principle to the problem of distinguishing laws from coincidences is almost a convincing demonstration that the principle is *not* applicable. His discussion is confused but it exposes the central difficulty, that of evaluating the prior probability that a law holds.[13]

To follow Mill's account it is useful to set down Bayes' formula (Mill does not do this) for the case of two possible "causes"—the observations are the result of a chance coincidence or of a law:

$$P(Law|Data) = \frac{P(Law)P(Data|Law)}{P(Law)P(Data|Law) + P(Coincidence)P(Data|Coincidence)}$$

In his reflections on the application of this principle Mill sometimes confuses the prior probability $P(Coincidence)$ with the likelihood $P(Data|Coincidence)$. Thus, when the data comprises m independent occurrences of an event which occurs by chance with probability $1/n$, Mill sets $P(Coincidence)$ at $(1/n)^m$.

Mill (545-6, 1148-9) considers the case of a run of aces. Then it may be possible "to form a conjecture" as to the prior probability of the alternative to the coincidence from "the character of the parties concerned" but even in this case, it will be impossible to estimate it with "anything like numerical precision".

When judging between coincidence and law Mill (546) points out how things are even harder:

> though the probability of a casual coincidence may be capable of appreciation, that of the counter-supposition, the existence of an undiscovered law of nature, is clearly unsusceptible of even an approximate valuation. In order to have the data which such a case would require, it would be necessary to know what proportion of all the individual sequences or co-existences occurring in nature are the result of law, and what proportion are mere casual coincidences.

Yet, Mill goes on to observe, the "detection of an unknown law of nature ... is no uncommon event" and so provided the number of instances is sufficiently large the coincidence can be accepted as the "effect of causation". He (547) adds, "Further than

this, in point of precision, we cannot go; nor in most cases, is greater precision required for the solution of any practical doubt." When Edgeworth discussed Mill's treatment in his *Metretike* he (1887, 82-3 & 108) found fault with the argument but agreed with this conclusion.

For Mill, probability seemed to be part of the rational reconstruction of science rather than part of its everyday practice and he did not expect every inference—or even very many—to have a probability attached to it. A rather similar position was adopted by Keynes in his *Treatise on Probability*; see Aldrich (2008) for an examination of Keynes's views. Mill's Bayesian analysis with empirically based priors has not survived and Venn and Jevons, Mill's most important immediate successors as commentators on the role of probability in science, were dissatisfied with it. (See Section 10 below.) However it had some viability for it was accepted by Edgeworth and Pearson, the leading statistical theorists of the late nineteenth century. See Edgeworth (1884, 229) and Pearson (1892, 143) and for discussion Dale (1997) and Aldrich (2007).

6. The Elimination of Chance

Herschel and Whewell discussed averaging as part of the technique of measurement but Mill attached special importance to it. He described averaging, or the "elimination of chance", as an "additional rule of experimental inquiry. " (530). It is applicable when "the effects of casual conjunctions of causes are habitually blended in one result with the effects of one constant cause", a particular case of the composition of causes. The "elimination of chance" is the isolation of the constant cause by eliminating not "any one assignable cause, but the

multitude of floating unassignable ones. " As noted in Section 1, phenomena to which the composition of causes apply are also amenable to the deductive method.

Averaging for Mill was very much a large sample technique: the "general result" is disclosed only in large samples and probability details need be considered only when small samples are being treated. The doctrine of chances dealt with such questions as:

> what are the extreme limits of variation from the general result which may occasionally be expected as the result of some smaller number of instances. (533).

But though Mill formally recognised the use of probability arguments for assessing the reliability of estimates, he seems not to have treated it as part of the standard apparatus of the elimination of chance. He does not seem to have thought of the methods as self-policing for he warned continually against the use of inadequate sample sizes in social science applications which would be attended by exaggerated impressions of precision. One reason may have been that he thought that observations were seldom sufficiently homogeneous to justify pooling.[14]

As well as ascertaining how much is attributable to a given cause, the technique of eliminating chance can also be used for disclosing the presence of a constant cause, i.e. it can be used as a method of discovery. In this context Mill writes of the "discovery of residual phenomena by eliminating the effects of chance" (532) drawing a parallel with one of the experimental methods, the "method of residues" by which effects are eliminated by means of a theoretical argument.

Mill's connected treatment of the method of agreement, empirical laws, probability and the elimination of chance is an impressive feat of synthesis. It is easy to locate the origins of the elements of the synthesis but hard to know where the idea of the synthesis came from. It is particularly difficult to reconstruct the course of Mill's thinking about probability for he scarcely mentions it outside the *Logic*,

Laplace's *Philosophical Essay on Probabilities* was the only probability work referred to in the first edition of the *Logic* and Mill was very selective in what he took from it. He accepted the mathematical theory, the applications to "natural philosophy" and to the duration of life and insurance. He accepted the applications which were based on large numbers of instances, but rejected the applications that were not. Thus he rejected the application of probability arguments to the "credibility of witnesses, and to the correctness of the verdicts of juries". These were amongst the most striking applications of probability and the most relevant to the reform of institutions. It was in this context that Mill made his famous remark about probability having become the "real opprobrium of mathematics". (538).

Herschel's *Preliminary Discourse* treated several of the topics with which Mill linked probability. Besides the experimental methods themselves, Mill took from it the conception of an "empirical law". However Mill thoroughly reworked the material, integrating Laplace's ideas on probability. His treatment is on a different scale—the method of agreement occupies chapters rather than just sentences. But it is not just the scale of the treatment that is different. Herschel says here are the procedures—Mill is concerned to understand what makes them work, if indeed they do work. When Mill described his work as "little more than expansion & a more scientific statement" of what Herschel had written he was underplaying his own contribution. When "Mill's experimental methods" became established in the

curriculum of logic and scientific method, Mill's main contribution was all but forgotten.

Some of the themes of the *Logic* were rehearsed in a review from which we have already quoted. The work reviewed was T. J. Todd's *Book of Analysis*. Todd suggested methods for eliciting laws from tabular summaries of data. After detailing the difficulties Mill conceded they might have some value.

> Though [the methods] may never directly lead to scientific knowledge, they often, on subjects of empirical and probable evidence, afford an approximation to it, sufficient to be of practical use. They afford data for what is called the calculation of chances. (416).

Probability (i.e. the calculation of chances) is associated with genuine knowledge, though it is only second class knowledge.

7. Statistical Reasoning in Social Science

We now turn to the last and most important part of the *Logic*, book VI, on the "Logic of the Moral Sciences". Mill's treatment was brief for he saw the book as only an appendix, "since the methods of investigation applicable to moral and social science must already have been described". (835).

There is no chapter headed "probability and the elimination of chance in the moral sciences", but there is discussion of the merits of the elimination of chance as a method in social science. In the case of political economy and ethology the discussion is confined to showing why it cannot be used and why they must be studied deductively: the samples are too small.

Mill thought the appropriate method for "ethology", or the science of character formation, was the same as for political economy—the deductive

method. He explained why in ethology "there is hardly one current opinion respecting the characters of nations, classes or descriptions of persons, which is universally acknowledged as indisputable" as follows:

> in proportion as the differences are slight, it requires a greater number of instances to eliminate chance; it cannot often happen to any one to know a sufficient number of cases with the accuracy requisite for making the sort of comparison last mentioned; less than which, however, would not constitute a real induction. (866)

A Galton or a Jevons might have gone out and got the data.

Political economy belonged to a class of subjects where the "object is to determine the effect of any one social cause among a great number acting simultaneously". The typical economic question was "the operation of restrictive and prohibitory commercial legislation upon national wealth " (881) which he treated by the deductive method in his economic writings. Mill considered how well different methods could cope with the question. We pass over his discussion of the experimental methods and concentrate on the possibilities of statistical reasoning.[17] His description of the nature of the subject suggests that the "elimination of chance" might work. It does not:

> specific experience can at most only show that on an average of a great number of instances, the cases where there were corn laws exhibited the effect in a greater degree than those where they were not. Now the number of instances necessary to exhaust the whole round of combinations of the various influential circumstances, and thus afford a fair average, never can be obtained. Not only can we never learn with sufficient authenticity the facts of so many instances, but the world itself does not afford them sufficient numbers, within

> the limits of the given state of society and civilisation which such enquiries always presuppose. (909).

Mill's (908) verdict on political economy was that "specific experience affords nothing amounting to empirical laws". Sample sizes were too small for the establishment of empirical laws. Perhaps there was also the matter of restricted variation. In his discussion of the application of the method of difference to the same problem Mill argued that it was impossible to obtain two countries identical in every respect except that of commercial policy for the difference in commercial policy would itself be caused by other factors. As these factors are themselves likely to influence prosperity, commercial policy will be confounded with them.

In his methodological writing about economics Mill took a narrower view of what was an economic question than he did in his actual economic writing. Are there applications of the elimination of chance in his economic writing? The notion of an average is appealed to in his account of value:

> Besides their temporary value, things have also a permanent, or, as it may be called, a Natural Value, to which the market value, after every variation, always tends to return; and the oscillations compensate for one another, so that on the average, commodities exchange at about their natural value. (*Principles*, 478).

However when it came to applying the notion to answer the important question of whether the improvement of agriculture was prevailing over population growth, he saw only difficulties [18]:

> Which of the two conflicting agencies is gaining upon the other at any particular time, might be conjectured with tolerable accuracy from the money price of agricultural produce (supposing bullion not to vary materially in value), provided a sufficient number of years could be taken, to form an average independent of the fluctuations of the seasons. This, however, is hardly practicable, since Mr. Tooke has shown that even so long a period as half a century may include a much greater proportion of abundant and a smaller number of deficient seasons than is properly due to it. A mere average, therefore, might lead to conclusions only the more misleading for their deceptive semblance of accuracy. There would be less danger of error in taking the average of only a small number of years, and correcting it by a conjectural allowance for the character of the seasons, than in trusting to a longer average without any such corrections. (704).

There seems to be nothing in Mill's economic work to contradict the verdict of the *Logic* —the elimination of chance cannot be effected in economic data.

8. The Inverse Deductive Method

The *Logic* was written at a time of great enthusiasm for statistics; the statistical societies of London and Manchester had only recently been founded. Was Mill reacting suspiciously to a new source of information and results about social problems—especially one sponsored by the leading critics of Ricardian economics, Jones and Whewell? However there is no obvious association between Mill's analysis and the work produced by the societies. [19]

Mill must have been familiar with the ideas of Quetelet and Guerry on criminal and other statistics.[20] He did believe that there were areas of social science where

adequate samples existed and laws could be based on "specific experience." To reach these conclusions took a major reorientation of Mill's methodology of social science—for which he gave Comte the credit:

> In a merely logical point of view, the only leading conception for which I am indebted to him is that of the Inverse Deductive Method, as the one chiefly applicable to the complicated subjects of History and Statistics: a process differing from the more common form of the Deductive Method in this, that instead of arriving at its conclusions by general reasoning and verifying them by specific experience (as is the natural order in the deductive branches of physical science), it obtains its generalisations by a collation of specific experience, and verifies them by ascertaining whether they are such as would follow from known general principles. This was an idea entirely new to me when I found it in Comte: and but for him I might not soon (if ever) have arrived at it. (*Autobiography*, 219).

Mill never defines "statistics" and in this passage he refers to a "subject" while in the *Logic* he refers only to the material. Mill does not discuss the nature of the subject—whether it is a matter of method or a particular type of fact. The material consists of numerical facts relating to human affairs, typically large quantities of such facts. Yule's (1911) famous definition of the material, "By statistics we mean quantitative data affected to a marked extent by a multiplicity of causes", captures one aspect of Mill's thinking abut statistics, the aspect relevant to the elimination of chance.

Mill coupled history and statistics, both as sources of facts on society and as subjects trying to do something with those facts. Yet there was never any question of

parity of treatment for history and statistics. The provision of proper foundations for a science of history was one of the main goals of Book VI of the *Logic* while the existence of statistical laws was just a commonplace fact.

What made statistics and history "complicated" subjects was not the number of laws involved but the complicated collocations of causes. It was not feasible to directly deduce behaviour from the laws of human nature—to work from causes to effects—because it was impracticable to obtain information on all the causes and impossible to do the calculations. Study had to be start from the empirical laws of the effects. Mill expected that these empirical laws would be connected in some way with the laws of human nature but as there was no prospect of gathering the information about the collocation of causes nor of being able to do the calculations, the deductive component in the inverse deductive method was rather weak.

Of course Mill needed to distinguish the new inverse deductive method from the old a posteriori, experimental, or "chemical", method that he had always argued was inapplicable to social affairs. Both methods obtained their generalisations by "collation of specific experience" but there was the difference that the laws found by the inverse deductive method did not masquerade as anything more than empirical laws. "Proof" only followed when the empirical law was derived from laws of human nature.

Mill argued that the two deductive methods corresponded to two types of enquiry, viz. "in which the question proposed is, what effect will follow from a given cause, a certain condition of social circumstances being presupposed" and "what are the causes which produce, and the phenomena which characterize, States of Society generally". (911). Political economy, it seems, was concerned exclusively with the former — e.g. with the effects of a free trade policy—although Mill scarcely

respected this restriction in his own economic work. History and statistics were concerned with second type of question. The "characterization" part involved finding empirical laws and the "causal" part involved deduction from "known general principles".

Being "complicated subjects", there was the possibility that history and statistics would prove completely intractable. Fortunately this was not so:

> there is a kind of sociological inquiries to which, from their prodigious complication, the method of direct deduction is altogether inapplicable, while by a happy compensation it is precisely in these cases that we are able to obtain the best empirical laws; to these inquiries, therefore, the Inverse Method is exclusively adapted. But there are also ... other cases in which it is impossible to obtain from direct observation anything worthy of the name of an empirical law; and it fortunately happens that these are the very cases in which the Direct Method is least affected by the objection, which undoubtedly must always affect it in a certain degree. (585)

In the case of history Mill tried to explain how this "happy compensation" came about but he did not make the effort for statistics. For statistics it was just a fact. In the *Logic* Mill asserts that in statistics "it is evident that empirical laws may sometimes be traced." (907) In the newspaper "now lying before me" he found an example of an empirical law: in bankruptcy cases misconduct is the most common cause of failure. Mill did nothing to establish that this was an empirical law and the whole discussion is very casual. Statistical laws are lying around everywhere but evidently they do not have much scientific interest. In line with his earlier discussion of empirical laws he warned of the dangers of relying on such

generalisations and insisted on the need for deduction to "stand sentinel" over the process.

9. Mill on Buckle

That is all that the discussion of statistics amounts to in the early editions of the *Logic*—just a few hundred words. However, the treatment was greatly expanded in the 1862 edition when Mill discussed the statistical ideas in Buckle's *History of Civilization in England*. The *Logic* has a new chapter with the title "Additional Elucidations of the Science of History".

Although the treatment is expanded and, although rather different points are made, there was no evidence of any basic change in Mill's interpretation of statistics. The part of Buckle's work that interested Mill was the popularisation of Quetelet's ideas, especially the notion of statistical regularity. These were quite old ideas which already had a long history of popularisation. Thus De Morgan (1838) had treated similar issues. Indeed some of Buckle's examples were used in Laplace's *Essay* which Mill had used for the first edition of the *Logic*. Among the examples were the annual number of suicides in London and the association between the number of marriages and the price of food. The second example came from Porter's *Progress of the Nation* and later studies of the interrelationships between economic and demographic variables would be amongst the earliest applications of correlation analysis in social science. Of course such regularities were taken for granted in Mill's discussion of population in the *Principles*.

Mill accepts the genuineness of the statistical laws discussed by Buckle and uses them to reinforce old points of his own—e.g. that human behaviour is caused—and also to dispute some of Buckle's interpretations, in particular his view of the

unimportance of moral influences. The discussion also reveals Mill's own views about the nature of statistical laws. Regarding the stability of the murder rate, he explained how "these most capricious events" could produce a degree of regularity "approaching the mathematical":

> This singular degree of regularity en masse, combined with the extreme of irregularity in the cases comprising the mass, is a felicitous verification a posteriori of the law of causation in its application to human action, every murder, for instance, is the concurrent result of two sets of causes. On the one part, the general circumstances of the country and its inhabitants ... On the other part the great variety of influences special to the individual ... If we now take the whole of the instances which occur within a sufficiently large field to exhaust all the combinations of these special influences or, in other words, to eliminate chance ... we may be certain that if human actions are governed by invariable laws, the aggregate result will be something like a constant result. (933).

The difference between this situation and the study of the effects of free trade was that here there was a "sufficiently large field" 1to eliminate chance. Even here, though, there are problems of changing conditions:

> the period of a year is too short to include all the possible combinations of partial causes, while it is, at the same time, sufficiently long to make it probable that in some years at least, of every series, there will have been introduced new influences of a more or less general character. (933)

What can be concluded from this review of Mill's reflections on statistical thinking? From his discussion of the elimination of chance, which went unchanged through all the editions of the *Logic*, he could be classed an enthusiast for what we would now call statistical thinking. As a philosopher of social science, he took the method of eliminating chance seriously and tried to explain the circumstances in which it would work. He recognised the existence of statistical laws without claiming any originality or seeming to think that any elaborate justification or explanation was required. As philosopher of political economy and ethology he expressed only doubts about the elimination of chance. He saw statistics as a going concern but was not very interested in it. Mill was not opposed to statistical thinking.

10. After Mill

We have extracted three small topics from Mill's huge *System of Logic*. For twentieth century economists these may make a natural enough group of topics but few of Mill's nineteenth century readers could have thought so.[21] There were exceptions—Jevons and Edgeworth are the obvious ones—but the influence of the *Logic* on later discussions was mainly a matter of separate influences. What were they?

Bain (1882, 67) wrote that the *Logic* "has been about the best attacked book of the time, and the author has in successive editions replied to objections and made extensive amendments". Although the material on Buckle was a big addition, the material on empirical laws and the elimination of chance was not revised nor does it

seem to have been much attacked. His contemporaries and immediate successors found those topics less interesting than, say, the foundations of probability.

Venn developed the relative frequency interpretation of probability which was very much in keeping with Mill's initial restriction of probability to cases where there were very large numbers of instances. Venn (1866, xiii) praised Mill's analysis of probability: "almost the only writer who seems to me to have expressed a just view of the nature and foundation of the rules of Probability is Mr. Mill". However Venn was far more radical in his resistance to probability; he saw *no* connection between induction and probability.

> The common opinion therefore which regards Inductive formulae as composing a portion of Probability ... cannot, I think, be maintained. It would be more correct to say, as stated above, that Induction is quite distinct from Probability, but yet co-operates with almost all its inferences. By the former we determine, for example whether we can safely generalise the proposition that four men in ten live to be fifty; supposing such a proposition to be generalised, we hand it over to Probability to say what sort of inference can be deduced from it. (173)

Mill had helped form that "common opinion"; he told Venn how he could not subscribe to all of his arguments:

> you seem to go farther in rejecting the doctrines of the mathematicians on the subject than even I do. If I understand you rightly, you attach little value to the rule for determining the probability by which of several causes a known event has been produced, which rule seems to me to rest on solid grounds, and to be quite reconcilable with the principle that all evaluation of probabilities must depend on appropriate statistics. (972, iii, 1361)

Venn disagreed with Mill but praised him. Jevons agreed with Mill but damned him. Or rather Jevons made much of their disagreements and very little of their agreement. Mill's criticisms of the conventional treatment of probability were wrong. There was no big problem in assigning probabilities to non-recurrent events. Why could not Mill see that probability was involved in all uses of evidence and that the certainty promised by the method of difference was illusory? Jevons did not seem to notice that Mill used probability in establishing empirical laws—in his commentary on Mill's discussion of the experimental methods he (1878, 267) complained of the absence of probability.

Mill's aim was to analyse the processes by which the science of his time was justified and to adjudicate on processes that were doubtful. Usually he exhausts the reader with realistic examples from the natural sciences illustrating each methodological principle. Probability, by contrast, seemed to live far back in the justification of inferential procedures for Mill gives *no* examples of the scientific use of probability calculations. Of course, the probability scene in the 1840s was quite different from what it became even thirty years later. There was no probabilistic theoretical science and Mill's treatment of probability does not anticipate it. Yet he gave probability an important role in science, one that was elaborated in the logical probability of Jevons and Keynes, Jeffreys and Carnap and in the statistics of Edgeworth and Pearson. From his own viewpoint J. M. Keynes's complaint about Mill's lack of "thoroughness" was justified yet Mill deserves a place in any history of those who set out to probabilise induction.

We turn to the place of statistics in economics. Mill's influence on methodological thought in economics was very strong. Indeed it is easy to read its

history up to the second world war as little more than a series of footnotes to Mill. To indicate what could be done from inside the Millian structure we will glance at the two great nineteenth century reviews of methodology, Cairnes's *Character and Logical Method of Political Economy* and Keynes's *Scope and Method of Political Economy*, and at the work of Jevons and Edgeworth, the great statistical economists of the nineteenth century. The discussion of economic methodology in the *Logic* is brief and extremely schematic. Cairnes provides a book-length exposition of Millian methodology with plenty of realistic examples of economic research to illustrate both good and bad practice. It gives a more accurate account of the role of statistics in political economy.[27]

Cairnes treats explicitly the "place of statistics in economic reasoning" and found it in the operation of the concrete deductive method. Statistics played its part in the "verification" stage [24].

> it is by availing ourselves of this systematized method of observation that we can most effectively check and verify the accuracy of our reasoning from the fundamental assumptions of the science; while the same expedient offers also by much the most efficacious means of bringing into view the action of those minor or disturbing agencies which modify, sometimes so extensively, the actual course of events. (97)

Cairnes had no sympathy for the project of constructing political economy with statistical laws as foundations, the view that political economy is identical with the "statistics of wealth and population":

> if this view, however, is to be accepted, the pretensions of the study, as a means of analyzing and explaining the causes and laws of which the facts

> presented by statistical results are but the results, must be given up. We may indeed give to the empirical generalizations which are to be found at the bottom of our statistical tables, and which are "founded on a plurality of instances to the same effect", the sounding title of "laws of our social system"; and if such empirical generalizations are to be regarded as ultimate facts, if every attempt at further analysis is to be met by ridicule of the ideas of causes in "habitual antagonism", and by simple re-assertion of the complex phenomenon to be explained, then however we may persist in retaining the forms and phrases of science, the scientific character of the study is gone; and Political Economy has no longer any claim to be admitted among those departments of knowledge of which the business is not only to observe, but to interpret nature. (219)

For Cairnes this project meant abandoning the goal of finding ultimate laws and remaining satisfied with derivative laws. Jevons showed how far statistical analysis could be taken in political economy while respecting the deductive structure of the science. Some questions could be settled only by the concrete deductive method:

> [the beneficent results of Free Trade] could hardly be proved to exist a posteriori; they are to be believed because deductive reasoning from premises of almost certain truth leads us confidently to expect such results, and there is nothing in experience which in the least conflicts with our expectations. (1879, 88)

On the other hand, much of Jevons's statistical work was directed to showing that in political economy specific experience does afford something amounting to

empirical laws. His work on seasonal variation and on business cycles was concerned with tracing empirical laws—and, when he tried to explain the regularities he obtained, he was merely following the precepts of the inverse deductive method.

Jevons's theoretical innovations also imposed new priorities: "if we could tell exactly how much people reduce their consumption of each important article when the price rises, we could determine, at least approximately, the variation of the final degree of utility—the all important element in economics." (1879, 174). Such a function could be obtained from price/quantity data, although it "would doubtless be a purely empirical one". Cairnes (125-9) also considered the possibility of obtaining a law relating price to quantity but argued that, as the factors on which such a relationship depends (preferences and incomes) are not known, then it is impossible to frame such a law. For Cairnes, the impossibility of a deductive argument yielding the law closed the matter. For Jevons, the law would just remain—an empirical law.

Jevons used the inverse deductive method and not surprisingly some of Mill's interpretations of statistical laws reappear in Jevons's economics. The explanation of the stability of murder rates reappears in Jevons's exposition of the practical application of his theory of consumer behaviour:

> Provided that we have a sufficient number of independent cases, we may then detect the effects of any tendency however slight. Accordingly questions which appear, and perhaps are, quite indeterminate as regards individuals may be capable of exact investigation and solution in regard to great masses and wide averages. (86).

This is just what Mill meant by the "elimination of chance." Marshall (1890, 152) had a similar confidence in "sufficiently broad' averages."

We have seen Mill's reservations about the applicability of the elimination of chance. Jevons did not share them. His instinct was for activity: samples were large enough to make it worth going ahead and doing the calculations. Stigler (1986, 5) has argued that the problem of overcoming the "inherent diversity of their material" was the principal conceptual obstacle to the acceptance of probability-based statistical inference by social scientists and that Jevons relied on bluster rather than good arguments to overcome it. Indeed there was not sufficient understanding on either side for the debate to

Edgeworth, Jevons's heir in statistical economics, had more of Mill's caution about statistical arguments.[25] However his way of settling doubts about the validity of a statistical argument was to use more delicate probabilistic reasoning. His earliest research in statistics was a rigorous reappraisal of Jevons's swashbuckling studies in statistical economics. Although in his taste for elaborate probabilistic arguments Edgeworth was moving away from Mill, he had much more respect for Mill's views than Jevons and used them as reference points for the new statistical doctrines.[26] The other principal figures in the creation of modern statistics do not seem to have been influenced by Mill in any obvious direct way. Edgeworth was set apart by his background as teacher of logic, his admiration for Mill's economic work and his strong sense of the continuity of intellectual effort.

By the end of the century there was a new methodological rule-book for economics, Neville Keynes's *Scope and Method of Political Economy*. Keynes gave further recognition to the place of statistics as a method applied to economics. He wrote the Jevonian innovations into the methodological canon.

> Political economy ... has a special tendency to become on its inductive side statistical, just as on its deductive side it tends to become mathematical. (343)

There were empirical laws in economics that could be uncovered by statistical analysis: e.g. "the tendency of financial crises to recur at periodical intervals was not first worked out theoretically; it was disclosed by statistical observations." (345).

Neither Mill nor his successors in the central deductive tradition held that economics could be based on statistics in the sense that the fundamental laws could be statistical generalisations. Yet, from Jevons onwards, the tradition had to accommodate very different assessments of the role of statistical analysis—there were the same differences between Robbins (1933) and Schultz (1938), say, as between Cairnes and Jevons. Both assessments could find backing in Mill's system—but in different parts. The Jevons-Schultz branch of the deductive tradition had as much right to claim descent from Mill as the Cairnes-Robbins branch. Indeed the lineage can be extended to the econometrics of the 1940s—to Haavelmo (Cartwright, 175) and Koopmans (de Marchi and Hirsch, 127).

To return to our starting point: how congenial was the *System of Logic* to the "rise of statistical thinking" The question was misposed. We have seen that there was no single cause of probability or statistics to advance or retard and that there was enough material in Mill's system to support a very wide range of positions on the different issues involved.

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Notes

1. See Stigler (1982) and Aldrich (1987) for reviews of Jevons's work and Stigler (1985, chapter 9) and Porter (1986, 255-69) for reviews of Edgeworth's statistical work. Aldrich (1992) gives a detailed account of their work on index numbers, their major excursion into statistical economics.

2. Mill's views on probability are discussed *passim* by Porter (1986) and by several contributors to Kruger et al (1987). The only detailed account of Mill's probability is the very nice study by Strong (1978); this is narrower in scope than the present paper but also much briefer. Stigler (1986) is excellent for general background

3. For Jevons, probability, statistics and political economy made a very potent combination. A number of studies have focussed on this combination; there are references in Aldrich (1987).

4. There is a huge literature on Mill's methodology. There is a useful bibliography in Redman (1997). Cartwright (1989) defends an updated version of Mill's philosophy.

5. The full set is: method of difference, method of agreement, joint method of agreement and difference, method of residues, method of variations. The method of concomitant variations has attracted some attention from historians of economic

thought (Cf. Hollander (1985, 101) and Whitaker (1977)) because it most closely resembles the modern economist's favourite statistical technique, regression analysis. However Mill does not discuss it either in connection with probability or with political economy.

6. Losee (1973, 151) does not overlook the probability component but most commentators do. It is easily missed for the exposition meanders through Book III of the *Logic*. The discussion of the method of agreement starts in chapter VIII, continues in chapter X on the "plurality of causes", extends to the discussion of "empirical laws" in chapter XVI to chapters XVII and XVIII on "chance and its elimination" "calculation of chances". There is also a Chapter XXIII on generalisations, and probable evidence—and more probability in Chapter XXV on "grounds of disbelief".

7. Cf. the treatment of the advocates of the "chemical method" in the *Logic* (879-886).

8. Mill's "deductive method" is not the same as the deductive-nomological model of explanation, associated with Hempel and Popper in the twentieth century for Mill's deductive method depended not just on deductive subsumption under laws but also on the possibility of adding partial laws. The confusion goes back at least as far as Jevons (1879, 87).

9. Cf. Hollander's (1985, ch. 2 passim) insistence on the experiential basis of deductive arguments in political economy.

10. Mill has a chapter on "approximate generalisations" and acknowledges their usefulness, particularly in social science. (603)

11. This discussion is rather remarkable as a systematic body of technique for analysing associations was only developed at the beginning of the twentieth century and first appeared in textbook form in Yule (1911).

12. In the Toronto edition of the *Logic* the original 1843 chapter on the calculation of chances is reproduced as Appendix F. Strong (1978) has a good account of what Herschel taught Mill.

13. The problem of evaluating the prior probability of the existence of a law of nature has been the main obstacle in attempts to develop a Bayesian confirmation theory. See Dale (1997) and Fienberg (2006) for histories of Bayesian reasoning.

14. See Mill's discussion of "seasonality" quoted on p. 22 below.

15. Strong (35) conjectures that Mill's original critique of Laplace was influenced by Prevost and L'Huillier whose views reached him via Dugald Stewart.

16. Mill certainly did not take his favourable opinion of probability from Todd who managed to hit probability and medicine with a single insult: "It was probably the same view of medical evidence which led the celebrated La Place to convey that

severe censure on the practice of medicine, of proposing to submit it to the mathematical doctrine of chances." (111)

17. Hollander (1985, 94-104) reviews this discussion.

18. De Marchi (1970, 275) points out that Mill makes this point about the effects of the seasons "in no less than three separate places" in the *Principles*. Tooke (1838, 84) suspected that "a series of 100 years at least is requisite to reduce to a fair average the inequalities of the seasons." On that hint of specious accuracy in the last sentence of that passage from the *Principles*, compare the following from the *Logic*:

> But where observation and experiment have not afforded a set of instances sufficiently numerous to eliminate chance, and sufficiently various to eliminate all non-essential specialities of circumstance, to attempt to calculate chances is to convert mere ignorance into dangerous error by clothing it in the garb of knowledge. (first edition, 1148).

19. Hilts (1978) and Cullen (1975) discuss the origins of the London Statistical Society and Hollander (1983 and -5) discusses Mill's relationship (or lack of it) with the "Cambridge Inductivist Critics". Unfortunately none of this work sheds any light on Mill's attitude to statistics. Hilts mentions a progress report on the publications of the London Statistical Society published in the *London and Westminster Review* that is critical of their work. Mill controlled this periodical and one may conjecture that the article (Robertson (1837)) broadly reflects his views. Robertson was particularly hard on the Society's programme of collecting facts without preconceptions.

20. Surprisingly Mill does not mention Quetelet in any of his writings. He reports a meeting with Guerry to Harriet Taylor: "the man whose maps of France ... shewing the state of crime, instruction &c. in each department you may remember." (*Later Letters*, vol 1, 5). For Guerry, see Hacking (1990) passim.

21. There was a tradition of logician-economists to which Mill and Jevons belonged and which continued with Johnson, the Keyneses and Ramsey. Johnson, the younger Keynes and Ramsey all made major contributions to the philosophy of probability.

22. Jevons faced Mill along a wide front that included philosophy of science, formal logic and economic theory.

23. Cairnes's work had Mill's approval: "its view of the logic of Political Economy is thoroughly sound and philosophical, and expressed in clear and precise language." Mill (1973, ii p. 554).

24. Cairnes upheld Tooke as an exponent of the right way of using statistics in political economy. Mill himself thought highly of Tooke's work and there is no reason to suppose he would have disagreed with Cairnes. Blaug (1956) has a section headed "Cairnes's Attack on Statistics" and gives rather a one-sided account of Cairnes's views.

25. Cf. Edgeworth's wonderful statement: "The pure theory of Probabilities must be taken cum grano when we are treating concrete problems. The relation between the mathematical reasoning and the numerical facts is very much the same as that which

holds between the abstract theory of Economics and the actual industrial world-a varying and undefinable degree of consilience, exaggerated by pedants, ignored by the vulgar, and used by the wise." Quoted by Porter, p. 256.

26. The Millian influence on Edgeworth can be seen from the synopses of Edgeworth's lecture courses reprinted as an appendix to Stigler (1985). Edgeworth even brought Mill to bear on Yule's (1926) interpretation of "nonsense correlations"; see Aldrich (1995). Edgeworth had a good deal more respect for Mill than Jevons; the case of logic and methodology is paralleled in economic theory—see Aldrich (2000).

27. From accounts such as the one in Fisher (1922), it would seem that Venn's criticisms played an important part in the eclipse of Bayesian methods. Zabell (1989) and Aldrich (1997) dispute this.

28. For discussion of these points, see Aldrich (1989 and -94).

29. This influence lasted far beyond Mill; see e.g. Koopmans (1947).

30. See Aldrich (1996, 187) for a discussion of this point.

31. In his "Law of Interchange" Mill (1844, p. 237) considered framing a rule for the determination of international values. However the "inclinations and circumstances of consumers" could not be "reduced to any rule".