Probability and Depreciation: A History of the Stochastic Approach to Index Numbers

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Introduction

Early in this century economists began using the new statistical techniques of correlation and regression developed in biometrics. Thus began a period of uninterrupted growth in what became known as econometrics.[1] There were precedents. Long before, Cournot and Jevons had proposed several "econometric" projects. The statistical methods underpinning these projects had originated in astronomy.[2] One of the projects, the estimation of demand laws, was reinvigorated to become the paradigm econometric investigation of the twentieth century. Another never became part of the canon of econometrics although it had received the full higher statistics treatment in the nineteenth century. This project—which involved the use of probabilistic reasoning in the analysis of price data—is my subject.

At the end of the nineteenth century, this project may well have seemed more promising than the estimation of demand laws. In the 1860s, Jevons failed to estimate "laws of variation of price" but succeeded in applying probabilistic reasoning to the depreciation of gold. In the 1880s, Edgeworth continued the work, combining research in statistical theory with applications to economics in a way that became

1. For the consequences of this development see Epstein 1987 and Morgan 1990.

2. See Stigler 1986 and Porter 1986 for accounts of this background.

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routine only with the rise of modern econometrics in the 1940s. However, despite the flying start, the project did not survive—it was a "circle-squaring expedition," "a will o' the wisp," or so thought Keynes, who laid the project to rest.[3]

The project has been labeled the stochastic approach to index number theory, but it has little to do with modern conceptions of index numbers and should not be thought of as modern index number theory done badly.[4] The aim was to learn about the *causes* of price changes, either to isolate the cause of a price change or to measure the force of a particular cause, the monetary cause.[5]

Two closely related arguments were involved. The *probability argument* was an application of the "probability of causes," which involved the use of Bayes's Theorem, and the *averaging argument* was an application of the "theory of errors" which involved the use of methods related to least squares.

The prices of commodities are known at two dates. For the purposes of the probability argument, it is assumed that at the second date prices are uniformly higher. The probability argument evaluates the probability that the cause of the change in prices is something acting on money rather than something acting on commodities. In a world in which prices are regulated by cost of production and money is a commodity, it means evaluating the probability that the cost of production of money had changed, with other costs unchanged. For the purposes of the averaging argument, it is assumed that the price changes have not been uniform. The averaging argument evaluates the contribution to each price change of the general monetary cause and the specific commodity cause.

The history of these arguments provides a record of changing conceptions of probability, of what probability arguments could achieve, and of the justification required for their use in economics. It is also a record of a partnership between economic theory and probability in which the economic theory component was constantly renewed, as conceptions changed of how best to analyse the value of money.

3. John McDonald has pointed out to me that the approach is *not* dead. It survives—or has been revived—in work such as Clements and Izan 1987.

4. The term "stochastic" was introduced by Frisch (1936) and adopted by Allen (1975) to describe Edgeworth's analysis. I apply the term more widely.

5. The modern point of view is expressed very clearly in Walsh's (1901) great survey of index number analysis, "In measuring quantities we must bear in mind that we are not concerned with the causes of their constancy or of their variations" (22). In writing this, he was rejecting a large part of the index number literature—including the part reviewed here.

Ricardo and the Measure of Value

Jevons was the first to apply the probability and averaging arguments to real data, but the arguments were not new. They had been devised at least as early as the 1820s and 1830s. The probability argument appears in the third edition of Ricardo's *Principles* (1821). Suppose that behind an alteration in the rate of exchange between two commodities is a change in the conditions of production of at least one commodity; then, "If we compare the present value of one [commodity], with shoes, stockings, hats, iron, sugar, and all other commodities, we find that it will exchange for precisely the same quantity of all these things as before. If we compare the other with the same commodities, we find that it has varied with respect to them all; we may then with great probability infer that the variation has been in this commodity, and not in the commodities with which we have compared it.(17).

This argument appears incidentally in the course of a criticism of Smith and Malthus, but, in the unpublished essay, "Absolute Value and Exchangeable Value," Ricardo criticized using the "mass of commodities" as a measure of value.

> By many Political Economists it is said that we have an absolute measure of value, not indeed in any one single commodity but in the mass of commodities. If we wanted to ascertain whether in the case just supposed of the cloth and the gold the variation had been in the one or in the other we could immediately ascertain it by comparing them alternately to many other commodities and if the gold preserved the same relation as before with these commodities then the cloth had varied, but if the cloth remained as before we might safely conclude that gold had varied. This measure might be an accurate one on many occasions but suppose that on such a comparison I found that with respect to a great number gold had altered in value and with respect to another large number it had not altered in value, but cloth had; how should I determine whether the cloth or the gold had varied? Suppose further that with respect to any twenty or thirty with which I compared them the results were the same, how should I know that the commodities to which I thus compared them had not themselves altered in value? If it be admitted that one commodity may alter in absolute value, it must be admitted that 2, 3, 100, a million may also, and how shall I be able to say whether the one or the million had varied. (1823, 400)

So here Ricardo rejected the argument against coincidence. It would not be decisive when the reference commodities changed amongst themselves; it could never be conclusive because the coincidence was always possible.[6]

These arguments were peripheral to Ricardo's work on the invariable measure of value—and even more so to the modern successor literature. Invariability first appeared in Ricardo's work as a requirement for a perfect money. In the *Principles,* he showed how natural candidates, such as gold, are "disqualified."[7] His disqualification analysis has been the starting point for much modern work, most notably Sraffa's (1960) account of the "standard commodity." Indeed, modern commentators have been less interested in the original monetary problem than in the disqualification analysis.

Senior and Steadiness in Value

Senior thought that absolute, real, or intrinsic value was nonsense and criticized Ricardo for confusing value with the cause of value. Yet he saw nothing nonsensical about intrinsic and extrinsic *causes* of value. "The causes which give utility to a commodity and limit it in supply may be called the *intrinsic* causes of its value; those which limit the supply and occasion the utility of the commodities for which it is to be exchanged may be called the *extrinsic* causes of its value" (1836, 16). Unlike Ricardo, Senior put as much emphasis on utility as on supply.

Like Ricardo, Senior held that "the general value of any commodity, that is the quantity of all other objects of exchange which might be obtained in return for a given quantity of it, is incapable of being ascertained"(96). Yet there was nothing wrong with speaking of "steadiness in value."

> The fluctuations in value to which a commodity is subject by alterations, in what we have called the extrinsic causes of its value, or in other words, by alterations in the demand or supply of other commodities, have a tendency, like all other extensive combinations of chances, to neutralize one another. While it retains the same utility and is limited in supply by the same causes, a given quantity of it,

6. See the discussion in Stigler 1982 where this passage is contrasted with that from Jevons (1963, 58) quoted below.

7. See section 6 of chapter 1 of the *Principles* (1821) and the accounts in Blaug 1958 and Hollander 1979.

> though it may exchange for a greater or a less quantity of different specific commodities, will in general command the same average quantity as before of the general mass of commodities; what it gains or loses in one direction being made up in another. It may be said without impropriety, therefore, to remain steady in value. (20)

Faith in the neutralization and compensation of causes was an important part of popular thinking on statistics, associated in particular with Quetelet.[8] Thus a few years after Senior's *Outline*, neutralization was prominent in the account of the "elimination of chance" given in Mill's *Logic* (1843).

Senior had a corresponding definition for "unsteadiness."

> But the rise or fall in value which a commodity experiences in consequence of an alteration in its utility, or in the obstacles to its supply, is, in fact entirely uncompensated. It is compensated only with regard to those commodities of which the utility or the supply has also varied at the same time and in the same direction. And as quite as many are likely to experience a similar variation but in an opposite direction, there is really *no* compensation. A commodity, therefore, which is strikingly subject to such variations, is properly said to be unsteady in value. (20)

He noted that "the scarcity and durability of gold and silver . . . makes them peculiarly unsusceptible of alteration in value from intrinsic causes" (97).

These considerations are from the *Outline of the Science of Political Economy* (1836). The notion of extrinsic causes of value seems not to be used elsewhere in his writing. Thus in his lectures on the value of money (1829–40), he considered only the intrinsic causes of the value of money. This is not surprising, for the point of segregating intrinsic from extrinsic causes of value was to allow the treatment of a commodity in isolation.

Cournot: Absolute Versus Relative Changes

Keynes states that the stochastic approach originated with Cournot, "the great parent of so many brilliant errors based on false analogies

8. Senior and Quetelet were friends. Laplace is cited as an authority in the *Outline*'s discussion of the influence of the probability of success on the wages in an occupation (1836, 209).

between the moral and the physical sciences" (1930, 71). Cournot was a specialist in probability, and there was a seriousness about his discussion of the application of probability to value that was lacking in Ricardo and Senior.[9] In fact, he published Jevons's method twenty-five years before Jevons.

For Cournot "relative changes" in value were observable but not "absolute changes." Yet it was possible to go from the former to the latter. "Among the possible hypotheses on the absolute changes which produce the observed relative changes, there are some which the general laws of probability indicate as the most probable" (1838, 19). In view of the close similarity to Jevons's much more elaborate work, I will not examine Cournot's proposal in detail. However, for the same reason, we should consider why Cournot did *not* foster the approach he had fathered. Cournot relates the probability argument *before* he expounds the theory of wealth. The point is that the probability argument represented the best that could be done without economic theory—and that was not much.

> But in general such calculations of probability, in view of the absolute ignorance in which we would be of the causes of variation of values, would be of very slight interest. What is really important is to know the laws which govern the variation of values, or in other words, the theory of wealth. This theory alone can make it possible to prove to what absolute variations are due the relative variations which come into the field of observation; in the same manner (if it is permissible to compare the most exact of sciences with the one nearest its cradle) as the theory of the laws of motion, begun by Galileo and completed by Newton, alone makes it possible to prove to what real and absolute motions are due the relative and apparent motions of the solar system. (18)

The great parent abandoned the child at birth for he had a greater favorite among his children—the mathematical theory of wealth.

Jevons and the Depreciation of Gold

Jevons took the decisive step of applying the theoretical arguments to real data in his pamphlet, *A Serious Fall in the Value of Gold Ascer-*

9. See Stigler 1986 and Porter 1986 for discussions of Cournot's statistical work.

tained and Its Social Effects Set Forth (1863). He wrote a history of prices, using the index number as a method of "reduction," to support his claim that the cause of the recent price increases was something acting on the side of gold. This was empirical work on a grand scale. Series on 112 commodities were used and price indices constructed both for the entire set of commodities and for groups of commodities. Tooke had argued in his *History of Prices* (1838) that the price variations of the Restriction Period were as much the result of specific causes as of a general monetary cause. To explain the changes in prices, Tooke referred to harvest fluctuations and other specific causes. By averaging, Jevons claimed to neutralize all the specific causes and expose the one general cause, the change to gold. It is difficult to know whether Jevons was influenced by Ricardo or Senior. He certainly shared with them a way of thinking about price changes—in terms of intrinsic and extrinsic causes—which gave meaning to the exercise. But he saw calculation as his important contribution and there is no hint of this in their work.[10]

In Cournot's case, there is no scope for speculation about possible influence, for Jevons discovered the method about ten years before he discovered Cournot. When Jevons (1879, xxx) reported Cournot's discussion and conceded priority for the "logarithmic method of treating price changes," he did not mention Cournot's reservations. It is unlikely that he saw the same conflict between probability and theory, for he used the theory of value in conjunction with statistical analysis. Jevons was not in "absolute ignorance" of the possible causes—there had been a revolution in the production of gold, an event that, according to the theory, would have an effect on the value of gold.

In Jevons's version of the theory of wealth, changes to the value of gold could be caused by "an increased supply of gold or a diminished demand for it" or from an "increased demand for one or more commodities not accompanied by a corresponding demand for gold, or a diminished supply not accompanied by a corresponding change of supply of gold" (21). It is worth remarking that Jevons's innovations in

10. For the "problem," citation would have been superfluous because it was such an obvious problem. Jevons saw *calculation* based on a *mass of commodities* as his important contribution, and this led him to identify as forerunners calculators like Lowe and Porter. He mentions Ricardo only once (1884, 297). In the *Proposals for an Economical and Secure Currency*, Ricardo had criticised the proposed use of the mass of commodities in a measure of value. Jevons regretted that he gave no source for the proposal.

the theory of value contributed nothing to his most ambitious piece of applied value theory. The innovations concerned the foundations of supply and demand, and his applicable theory was no different from Senior's.[11]

Like Senior, Jevons repudiated any notion of intrinsic value: a "fall in the value of gold" meant a general rise of prices—to which Cairnes objected,

> As employed by Mr. Jevons, a depreciation or fall in the value of gold expresses simply the fact, that the relation of gold to commodities has been altered in a particular direction, without *reference to the cause of the alteration*. . . . I hold that, in discussions concerning variations in value, whether of gold or commodities, we shall only convey just notions by using these terms with constant reference to what Adam Smith, Ricardo and Mill call "natural value"—that is to say, value in its relation to cost. (Quoted in Black 1960, 219)

For Jevons, the connection between a change in the conditions of gold production and a "fall in the value of gold" was causal, not definitional. Jevons identified two distinct problems: ascertaining that there had been a depreciation and ascertaining its causes, a problem of measurement and a problem of causation. However, they were conceptualized in closely related ways. Put in modern statistical terms, measuring the extent of the depreciation was estimation and locating its cause was testing.

A Serious Fall (1863) begins with a discussion of the problem of locating the cause of a change in the rate of exchange between commodities. When only two commodities are involved there is nothing to indicate "from which side the change comes." With more than two commodities, the outlook is more promising.

11. The only place in Jevons's work on money where a link is made between the value of money and utility is in his book, *Money and the Mechanism of Exchange* (1875). After making the familiar case that a change in the value of money will redistribute income but not reduce it, he argues that there will be a utility loss nevertheless, "to take any sum of money from one and give it to another will, on the average of cases, injure the loser more than it benefits the receiver. A person with an income of one hundred pounds a year would suffer more by losing ten pounds than he would gain by an addition of ten pounds, because the degree of utility of money to him is considerably higher at ninety pounds than it is at one hundred and ten" (39).

> [If] the same quantity of A purchases less of each of B, C, D, E than it used to do, this may arise either from causes affecting A only, or from causes affecting each of B, C, D, E. . . . *It is more likely that the alteration should have arisen on the side of A than on the side of B, C, D, E* because one cause affecting A will suffice to explain the change, whereas four separate but concurring causes respectively affecting B, C, D, E will be needed on the other side. The odds, then, are four to one in favour of the cause of the alteration being in A and not in B, C, D, E. (18)

The problem conformed to the pattern of "common cause versus coincidence" that was central to Jevons's thinking about probability.[12] The events to be accounted for are changes in price. The common cause is a change occurring on the side of gold, while goods remain stationary; and the coincidence, gold remained stationary, while changes occur on the side of goods.

In the argument, Jevons puts the commodities A through E on an equal footing and assume that the probability of an alteration is the same for all commodities. This argument appears to be based on an assumption of ignorance, or absence of information, about the commodities, and it corresponds to Cournot's appeal to the "general laws of probability." However, when information about commodities is taken into account (i.e., the "theory of wealth" is used) the conclusion that the change has been on the side of gold is strengthened.

> It is seen to be more likely that any considerable and general change of prices should arise from a single circumstance affecting the demand or supply of gold only, than from a variety of circumstances separately affecting all or most other commodities. Joined to the fact that circumstances have occurred in the production of gold which would probably cause a considerable rise of prices, it is hardly to be doubted that any general elevation of prices which we may discover is for the most part due to such circumstances. (22)

The probability argument remained at most a heuristic argument, for Jevons could not make it operational. However, he could use it to demonstrate the importance of considering a large number of commodities:

12. There is a detailed account of this argument and, more generally, of Jevons's use of probability in Aldrich 1987.

"the fact of an alteration may be ascertained with a continual approach to certainty, by examining its value in terms of more and more commodities" (19).

Jevons never denied that it was possible that the general cause was operating on commodities.

> Any variation of demand or of supply affecting most commodities, to the exclusion of gold and in a greater degree than gold, or on the other hand affecting gold to the exclusion of other commodities, or in a greater degree than those commodities, may be the complete or partial cause of the alteration. Numerous circumstances might be called in as contributing causes; but all facts I am aware of are so inconsiderable compared with the great discoveries of gold, that it is impossible not to treat these discoveries as the substantial cause of the depreciation. (59)

The estimation, or measurement, side of the study was apparently more satisfactory, for Jevons could use his data to produce an estimate of the extent of the depreciation. The technique adopted was averaging. He described its power in terms very similar to those used by Senior. "In determining the average variation of value with respect to A, of a sufficient number of articles B, C, D, E, etc. we may always ascertain the common alteration probably due to A. For the distinct and contrary variations peculiar to B, C, D, etc. will destroy each other more or less completely in drawing the average, and only that common variation which all equally suffer in being measured against A will remain undiminished" (Jevons 1863, 20).

Tooke and Cournot thought an investigation of the circumstances of each commodity—given the right interpretation—to be essential. Jevons made an appeal to the efficacy of "wide averages."

> It may seem to some persons that the best and perhaps the only way to ascertain whether and why prices have altered, is to examine the circumstances of demand and supply of each article. I do not hesitate to say that the whole inquiry would be thrown into confusion by any such attempt, and that for the particular purposes of our inquiry it is better not to know the details concerning the articles. If you are able to explain the fall of one commodity by circumstances unconnected with gold, and throw it out of the inquiry, you must do the same with others or else the impartial balance of the inquiry is overthrown. Now there is not a single article but is affected by many cir-

> cumstances besides the alteration in gold. A searching inquiry into the conditions of supply and demand of every article would result in every one being thrown out as unworthy of reliance as a measure of the value of gold. It is only by ignoring all these individual circumstances and trusting that in a wide average, such as that of 118 articles, all individual discrepancies will be neutralised, that we can arrive at any conclusion in this difficult question. (1863, 58)

Jevons clearly thought that "trusting" to a wide average was reasonable. As his work contains no indication of when it would *not* be reasonable to be so trusting, the passage is less an argument than a declaration of faith.[13]

Jevons knew he wanted to take an average, but there remained the matter of choosing the right one. In *A Serious Fall* (1863), he considered the possibility of using the arithmetic mean of the price ratios but rejected it as "totally erroneous." When he reconsidered the question in his second paper (1865), he was more flexible. On that occasion he gave a number of reasons for favoring the geometric mean. The reasons were diverse—and rather unconvincing: that the geometric mean was between the arithmetic and the harmonic means, and that the use of the geometric mean was facilitated by the use of logarithms. But there was a third reason which reveals more about his interpretation of price changes:

> It seems likely to give in the most accurate manner such general changes in price as is due to a change on the part of gold. For any change in gold will affect all prices in an equal ratio; and if other disturbing causes may be considered proportional to the ratio of the change in price they produce in one or more commodities, then all the individual variations of prices will be correctly balanced off against each other in the geometric mean, and the true variation in the value of gold will be detected. (1865, 122)

Some formalism may be useful.[14] Suppose there are n + 1 commodities including gold or money (commodity A); p_i is the price (in

13. This passage is discussed in Stigler 1982 (361) and 1986 (5).

14. Using this formalism, the simplest Ricardian analysis can be expressed as follows. Suppose that prices are proportional to labor contents

$$p_i = \frac{l_i}{l_A} \quad \text{and} \quad \pi_i = \frac{\lambda_i}{\lambda_A}, \qquad i = 1, \ldots, n$$

gold) of the i-th commodity in the initial period and π_i is its price in the second period. $p_A = 1 = \pi_A$, the price of gold, is unity in both periods. A price changes because there is a demand or supply shock (with intensity ε_i) to the commodity and/or a shock (with intensity ε_A) to money:

$$\pi_i = p_i \frac{\varepsilon_i}{\varepsilon_A}$$

$$\frac{\pi_i}{p_i} = \frac{\varepsilon_i}{\varepsilon_A} \quad \text{and} \quad \Pi\left(\frac{\pi_i}{p_i}\right)^{1/n} = \Pi\left(\frac{\varepsilon_i}{\varepsilon_A}\right)^{1/n} = \left(\frac{1}{\varepsilon_A}\right)\Pi(\varepsilon_i)^{1/n}$$

The geometric mean equals the product of the gold factor, $(1/\varepsilon_A)$, and the commodity factor, $\Pi(\varepsilon_i)^{1/n}$. When Jevons claims that all the individual variations of prices "will be correctly balanced off against each other," he seems to mean that $\Pi(\varepsilon_i)^{1/n}$ is very close to unity (i.e., there is no general influence acting on commodities). He is never clear about the status of this proposition—whether it follows from the averaging process or it is a reasonable inference from "all the facts of which I am aware" (quoted above; 1863, 59).

Cairnes took a different view of the average, for he took a different view of the facts. He thought Jevons's estimate of the depreciation of gold (in Cairnes's sense) was an underestimate. There *had been* a general influence on commodities, technical progress in their production; and Jevons's estimate of the change in the value of gold confounded the effects of the change to commodities with the change to gold.

In *not* using the arithmetic mean, Jevons was departing from the theory of least squares, but in other ways the theory strongly marked his thinking, especially on the question of "weighting." The term itself came from least squares theory. "Ought we to take all commodities on an equal footing in the determination? Ought we to give most weight to those which are least intrinsically variable in value? Ought we to give additional weight to articles according to their importance, and

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where l_i is the labor content of the i-th commodity in the first period, and λ_i is its labor content in the second period.

Define ε_i as the change in labor content, or change in *natural value:*

$$\lambda_i = l_i\varepsilon_i$$

If $\varepsilon_i = 1$, the i-th commodity would be an *invariable measure of value* and a change in relative price would indicate a change on the part of the other commodity.

the total quantities bought and sold?" (1863, 21). The leading consideration in the discussion of weighting was whether more precise estimates of the change in the value of gold could be obtained by weighting. The theory of least squares suggested weighting commodities more heavily if they were less intrinsically variable. As Jevons interpreted this criterion, commodities which had experienced less price variability in the period under study should be weighted more heavily. In practice, such reweighting made little difference, and Jevons did not insist upon it.

Jevons tried to rationalize weighting by "importance" in terms of the theory of least squares; in a very obscure argument, he presented such weighting as a way of dealing with a "certain interdependence" between the prices of different commodities. The concept of correlation was not yet formalized, but it would not be misleading to say that Jevons was aware that price changes would be correlated and that this problem would greatly affect the analysis.[15] Jevons had no solution to the weighting problem, and he showed that his results were robust to different selections of commodities and to crude reweighting.

It is easy to read *A Serious Fall* without noticing its probability backbone, and most of its readers appear to have done so. That backbone was clearly displayed in a postscript published six years later, *The Depreciation of Gold* (1869). The argument has two parts; first, the fact of depreciation is established beyond any doubt, and then the common-cause conclusion is drawn. The calculations now take into account the magnitude of the price changes, which are treated as independent observations on a common mean.

> To gain some notion of the degree of probability of conclusions on this subject, it has occurred to me to apply the ordinary methods of the theory of probabilities to the results stated in my pamphlet on the value of gold. . . . Regarding each of these thirty-six commodities as a separate and independent measure of the alteration in the value of gold, I first took the average rise of prices, namely 16 per cent, as the most probable estimate which these thirty-six measures give,

15. Jevons mentions the problem of correlation in connection with the prices of substitutes, such as different types of grain (see below; 1869, 156). In the *Theory of Political Economy*, he opened his discussion of substitutes with the lament that "much confusion is thrown into the statistical investigations of supply and demand by the circumstance that one commodity can often replace another, and serve the same purpose more or less perfectly" ([1871] 1910, 127).

> and then proceeded to calculate by the ordinary method of least squares the probable error of this result. The probable error proved to be just 2½ per cent—that is to say, *it is as likely as not that the true alteration of gold lies within 2½ per cent of 16 per cent,* or between 13½ and 18½ per cent. From this result we can readily calculate the probability that gold is depreciated in some degree, or that the true result if it not be 16 per cent is above 0 per cent rise. The probability proves to be so near certainty that the tables required in the calculation do not go sufficiently far to enable me to give it exactly. (1869, 156)

Having established that there had been an "alteration of gold," the next problem was to identify the cause or causes. Here the coincidence-versus-common-cause analysis was used to brilliant effect; the probability value obtained was overwhelming.

> It may be safely said that the odds are 10,000 to 1 in favour of a real depreciation of gold. The meaning of this is, that the chances are 10,000 to 1 against a series of dis-connected and casual circumstances having caused the rise of prices—one in the case of one commodity, another in the case of another—instead of some general cause acting over them all. It is true that as the commodities do not all vary independently, different kinds of corn, for instance, generally varying together, the improbability is not so great as stated; but if we reduce it ten times, to 1000 to 1, it is great enough for my purposes. (157)

Jevons did not claim that the alteration was necessarily due to something on the side of gold. It would be consistent with the result, that there was a general cause, for the common cause to operate elsewhere, but given the increase in the supply of gold and in the absence of any plausible alternative, he felt satisfied that the cause had operated on the side of gold.

The article is a tour de force; Jevons had solved both the measurement and the causation problems. He had married Senior's and Ricardo's arguments and put them to work to explain a complicated set of data. *The Depreciation of Gold* did not tie up all the loose ends, and it even created some new ones. For on this one occasion, he calculated the arithmetic mean of the price changes; had he applied the "ordinary methods" of least squares to the logarithm of the price changes, it would all have been coherent. It is not clear why he did not—was it an oversight, or did he think that only the arithmetic mean could figure in the least squares argument, or was there some other reason?

This article closed his series on gold, and, in later years, Jevons only touched on the problem. He discussed proposals for a "tabular standard of value" in his textbook, *Money and the Mechanism of Exchange* (1875). His first reaction to the suggestion by Lowe and Scrope, that indexing of contracts would eliminate much of the nuisance associated with changes in the value of money, had been sceptical, "Such a proposal, though scarcely practicable, is interesting, and perhaps sound in a theoretical point of view" (1865, 123). He now thought that "the practical difficulties are not of a serious character" (1875, 330). He adopted their objective but substituted his own idea of an index number. For the tabular standard, he recommended the geometric mean based on prices of, perhaps, 100 commodities to be "chosen with special regard to the independence of their fluctuations one from another" (1875, 332). Jevons knew of different index number formulae, and wrote, "it is probable that each of these is right for its own purposes when these are more clearly understood in theory" (1865, 121), but he did not discuss the differences in purpose behind the formulae. Scrope (1833, 406), for instance, thought it "quite indifferent" whether the change which he wished to correct had been brought about by "circumstances immediately affecting the production of gold or of goods."[16] Jevons wanted to settle such questions of causation.

Edgeworth and the Indefinite Standard

Jevons introduced index numbers because he needed them in his empirical work. Edgeworth was more interested in the arguments behind the formulae. He brought to them a very subtle mind and a command of a wide range of economic and statistical notions. He changed the probability approach—it became much more sophisticated and much more narrowly focused.

Edgeworth's first essay on index numbers (1883) gives little sign of sympathy for the stochastic approach.[17] For the utility theorist, the

16. Cournot (1877, 121) criticized Jevons for confusing two quite distinct tasks when he proposed his index number as the basis for a tabular standard.

17. Edgeworth made a number of telling points against the analogy between index numbers and means or, rather, the points tell against Jevons's reasons for favoring the geometric

only interesting issue was the change in income that would return an individual to the level of utility enjoyed before a change in prices. However, Edgeworth's interests were extending beyond utility theory, to statistical theory and monetary theory.[18] He became perhaps the most powerful statistical theorist ever to work on economic problems. Jevons's posthumous collection of essays, *Investigations in Currency and Finance* (1884), had a strong influence on Edgeworth and he spent much of the 1880s extending its analysis. Indeed, all of his contributions to statistical economics were in the field of money.

In his review of the *Investigations,* Edgeworth drew on an astronomical analogy to legitimize the stochastic approach, "Mr. Jevons infers that, so to speak, the movement must have been in gold, not in the commodities, by a stroke of the calculus of probabilities like that which may be applied to prove the proper motion of the sun among the stars" (1884, 38). Edgeworth's main contribution to index number analysis was contained in three memoranda on the problem of "ascertaining and measuring variations in the value of the monetary standard" (1887, 1888 and 1889). He tried to make sense of the burgeoning literature on index numbers. There was a "plurality of purposes" served by index numbers; there had been too much "vain controversy," when "one party makes a good stroke at billiards and thinks he has scored off another player who is playing at chess" (1888, 347).[19]

I should also mention the probability analysis developed in the second memorandum, which applied the theory of measurement errors to index numbers. Edgeworth considered a non-stochastic index, such as a cost-of-living index, and the errors to which it was subject—in particular, the use of inappropriate weights. The analysis was purely statistical and most easily interpreted as a variation on the theme of the performance of different generalized least-squares estimators. The message was that the index was insensitive to weighting—provided there were enough observations (commodities). I will not pursue the

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mean since Jevons hardly made a case for the analogy itself. The only sign of future developments was in the excessive references to the technical probability literature: excessive, for, as yet, there was no technical analysis! Edgeworth's first statistical paper appeared in 1883.

18. See Stigler 1978 and 1986 and Porter 1986 for reviews of Edgeworth's statistical work. Creedy's (1986) account of Edgeworth's economic work does not cover his monetary analysis nor his index number work.

19. The third memorandum consisted of afterthoughts to the first memorandum.

details of the analysis nor the qualifications to the message as they have little to do with my story.

Returning to the primary probability theory, Jevons and the other writers discussed above had been interested in the value of gold but there was nothing essentially *monetary* about their method of analysis. Had it been necessary to know whether the change in the rate of exchange of nutmeg for other goods was due to something on the side of nutmeg or to something on the side of other goods they would have used the same argument. Without comment, but in line with contemporary thinking, Edgeworth shifted the topic of the value of money from value theory to the quantity theory of money.

The type of index number, for which the stochastic treatment was suited, was needed "not so much for any practical object as for the more general purposes of Monetary Science" (1887, 200). He called this index number the "indefinite standard." "Monetary Science" was concerned with "causes which operate upon all goods whatever" (233) as in the thought experiment in which everyone wakes to find his stock of money doubled.

In real world experiments, there were influences specific to individual commodities as well as general causes acting on all prices. The cause did not have to be a change in the quantity of money; there were other possibilities, but the pervasiveness of the cause *was* essential. "It is sufficient for our purpose that there should be a circle of commodities, including money, such that the equilibrium of exchange between them should continually be readjusted by a comparatively frictionless play of market forces" (1887, 234).

The problem of measuring the effect of a general cause was familiar from the physical sciences. In his discussion of weighting, Edgeworth likened the measurement of the force of this cause to measuring the force of gravity.

> The displacement from the vertical constituting the required measurement might be found by taking a mean of the displacements suffered by all the pendulums. Now, *from what we know of the action of gravity,* there is no reason to think that the displacement of a larger mass gives in general a better measure of the common disturbing agency, the gravitation force, than a smaller does. Hence, in taking the mean of the displacements, there is no propriety in assigning more importance to the displacement of the more massive pendu-

> lum. If we do assign preferential importance, it should be on other grounds, namely, that the proper disturbance of some pendulums are apt to be less serious than those of others. (1887, 235)[20]

I shall return to the question of interpretation after considering the statistical side of the analysis.

For Edgeworth, it was a basic principle that the choice of index be determined by the nature of the behavior of price changes. Examination of the distribution of price changes replaced Cournot's appeal to "general laws of probability" and Jevons's jumble of reasons for preferring the geometric mean. In his *Principles of Science* (1874), Jevons used the law of error, or normal distribution, to justify the use of the arithmetic mean. Although one of his arguments for the geometric mean vaguely suggests the kind of argument used to prove the law of error, the overall impression from his writing is that the use of the geometric mean was divorced from any consideration of the distribution of price changes. For Edgeworth, the form of the distribution of price changes settled the choice of mean, and the approach became less a priori.

Galton and Macalister's (1879) treatment of the lognormal distribution influenced Edgeworth's thinking about means. Their work was significant in three ways; it emphasized that laws other than the normal occurred naturally and were to be found in phenomena similar to those of interest to economists, that the geometric mean was the appropriate mean for a certain kind of distribution, and, more generally, that the choice of mean depended upon the distribution of the observations.

20. Edgeworth followed Jevons in taking the point of weighting to be obtaining more precise estimates. Although he liked to tease his readers with the claim that the price of nutmeg or that of pepper might serve as well to indicate the value of money as those of cotton or iron, he also wondered whether weighting by importance could be justified by appeal to the theory of errors. He thought it might be that "each price which enters into our formula is to be regarded as the mean of several prices, which vary with the differences of time, of place, and of quality; by the mere friction of the market and in the case of "declared values," through errors of estimation it is reasonable to suppose that this heterogeneity is greater the larger the volume of transactions. On this account . . . greater weight should attach to the prices of these commodities where quantities are larger" (1887, 247). The choice between weighted and unweighted averages might be settled by an exhaustive study of market statistics, but there was no prospect of obtaining the necessary information; and whatever could be done was necessarily crude. Of course, he could warn against treating as independent changes that were not, but he could not do any more than Jevons to bring ideas of correlation to bear upon the problem of index number construction.

In his first memorandum (1887), Edgeworth advanced a number of reasons why the distribution of price changes would resemble a log-normal distribution. The use of empirical evidence was crude—this was fifteen years before the χ^2 goodness-of-fit test—but good enough to show that the data little resembled a normal distribution. Besides the empirical evidence, there were theoretical arguments that were as various as those Jevons had advanced for the choice of mean. For instance, there was an argument that demand was in some way analogous to the psychophysical phenomena exhibiting lognormality. Though arguments were given—for those who want reasons—they were not offered very seriously; they were taken as a demonstration that there was no a priori presumption against the supposition of an asymmetric distribution for price changes. The arguments cannot be taken as a very large step towards an economic explanation of an empirical distribution, but they were a step.

A number of data sets were presented, and all except one exhibited an asymmetrical lognormal-like pattern. Edgeworth was satisfied that in the exceptional case the conditions for the central limit theorem were satisfied, and he did not consider the exception damaging. Edgeworth's examination of this price-change data was perhaps the first instance in economics of fitting distributions to data. Implicit in Jevons's common-cause-versus-coincidence analysis was a null-hypothesis distribution representing randomness. However, Jevons was concerned only to show that the null hypothesis was false; he was not interested in describing the actual distribution.

The geometric mean was not, in fact, Edgeworth's own preferred mean. He favored the median—the (weighted) median was the "author's method." The median was studied intensively by Edgeworth in the 1880s and, though he was following lines established by Laplace, he derived many new results including extensions to regression. His reasons for favoring the median were its ease of calculation and its robustness; it was only slightly inferior to, (i.e., less efficient than) the mean when the data was normal, but it was much better for non-normal data. Edgeworth saw the object of the quest as "a figure such that if we took any ware at random, that figure would be more likely that any other to be equal to the relative price of the selected ware" (235). Thus the "quaesitum" was the mode of the distribution; it was at this that the geometric mean or the median were aiming.

So far I have treated only the implications of the shape of the distribution for the choice of mean. These relate to the details of the investigation, but Edgeworth also had much to say about the interpretation of the results, and, in particular, about the interpretation of the "distribution" of price changes and about the circumstances in which it would be justifiable to claim that a "true mean" had been found. These were aspects of the subject that Jevons simply did not consider.

Edgeworth noted that, in practice, the prices used in the construction of the index formed only a sample of those ruling at any particular time, but, that even were they comprehensive, they would form only a sample from the universe of possible prices. Edgeworth's interpretation of the index number as based on a sample of prices from a hypothetical infinite population was thus linked to Venn's (1866) limiting-frequency interpretation of probability. "It is even allowable to imagine series of statistics still longer, namely, those which would ideally occur if we could go on and on multiplying observations under unchanged conditions" (287). The model for Edgeworth's construction was Quetelet's "typical mean," although with Galton's modification that the distribution might as well be lognormal as normal, or even follow some other distribution. The index number recorded a feature of a distribution and did not correspond to any external entity; it did not resemble so much the measurement of the position of a star as that of the average height of Italians.

Jevons took it as a principle of statistical analysis that it was legitimate to pool observations (see 1863, 58; quoted above). Edgeworth was not prepared to take for granted the existence of a typical mean—there might be no distribution, no type. Given a set of data, a mean could always be calculated, but it might not correspond to a true mean. The possibility that disturbed him was that, instead of a single distribution of price changes, a mixture of distributions was involved. A true mean existed if the observations were governed by a single general cause and independent specific causes. There would be no true mean if there were pockets of prices isolated from each other and governed by independent causes. Edgeworth discussed the claim that the recent price history of the United States was a case in point. He took the view that should a thorough empirical examination confirm this suggestion, the calculation of a mean would be inappropriate; there would be no "type" to support a typical mean. He does not say what a quantity theorist should do.

There were some pilot investigations in the first memorandum (1887). Edgeworth considered evidence of heterogeneity in the prices of three groups of commodities. The criterion was whether there were significant differences between the average price of the various subcategories, and he used the method for comparing two means expounded in his paper,"Methods of Statistics" (1885). He found "a real, yet not enormous difference; not greater than the differences in stature which exist between the sub-classes of a nation contributing to a perfect type" (245). When the entire set of 312 observations is plotted, "it will be difficult to resist the impression that here we have a *typical mean* as perfect as any represented in concrete statistics" (245). He was secure in going on to argue that "the evidence that the general average rise for the whole group of 312 articles, namely from 100 to 118, is no mere accidental appearance, but indicative of a real agency, is mathematically estimated by odds of trillions to one" (246). Although he gives the probability that a "real agency" had been at work, the emphasis is always on measurement, on the averaging argument, rather than on the probability argument.

Edgeworth's work on index numbers was part of a concentrated effort in general monetary analysis. Although his exposition of monetary theory is highly compressed, the analysis is impressive. The equation of exchange provided the framework, but Edgeworth made a special point of emphasizing credit and, in his formulation, the equation of exchange accommodated credit. He devised a mathematical theory of banking to explain the relationship between the volume of credit and the cash reserves needed to sustain it. This analysis involved modeling the stochastic pattern of withdrawals and was much more sophisticated than the fixed reserve ratio later used by Fisher.

Besides theoretical analysis, his studies included a review of existing empirical work, and he contemplated empirical analysis on the lines later followed by Fisher in his *Purchasing Power of Money* (1911). Like Cournot, Edgeworth felt that the index number analysis was unsatisfactory. "Why rest satisfied with a type if there exists a more substantial *quaesitum*? . . . Why not penetrate beneath the superficies of shifting prices to the real relations between the quantity of money and commodities?" (1885, 251). He believed that "insurmountable statistical difficulties" blocked a full analysis (253). It was not a need for theory but for data. There was no prospect of measuring the variables appearing in Edgeworth's monetary theory.

The construction of a typical mean was the best that could be done, and unfortunately the answer had to be that "it seems that in the present state of science we must abandon the sort of realism which seeks an additional entity behind the phenomena of varying prices. We must resign the fond idea of finding in the mean variation of price, any measure of its cause verifiable by an independent statistical investigation" (254).

The "indefinite standard" was a curious invention. Although Edgeworth was immensely more careful and sophisticated than Jevons, his construct remains an enigma. Edgeworth conscientiously illustrated all his theoretical analysis with realistic numerical examples, but he did no genuine applied work, and it is unclear how he thought his invention should be used in practice. Was it a makeshift that would become superfluous as the state of science advanced? Did the *Purchasing Power of Money* (Fisher 1911) make it obsolete?

After the Memoranda

In the early 1890s Pareto reviewed economic theory, rewriting it as he went along. The turn of the probability argument—from Cournot via Walras—came in 1893.[21] Suppose (as above) that the same quantity of A purchases less of each of B, C, D, and E than it used to do, but the realative prices of B, C, D, and E do not change. The alternative hypotheses are that A has remained stationary while B, C, D, and E have moved upwards *or* that B, C, D, and E have remained stationary while A has moved downwards. Let τ_b be the probability that a cause exists affecting B, and σ_b be the probability that if this cause exists it makes B move upwards. Assuming independence, the probability that A remains stationary while B, C, D, and E move is

$$\pi_1 = \tau_a(1 - \sigma_a)(1 - \tau_b)(1 - \tau_c)(1 - \tau_d)(1 - \tau_e),$$

while the probability that B, C, D, and E remain stationary and A moves is

$$\pi_2 = (1 - \tau_a)\tau_b\sigma_b\tau_c\sigma_c\tau_d\sigma_d\tau_e\sigma_e.$$

Hence the probability that the change is due to a change in A is $\pi_1/(\pi_1 + \pi_2)$. Pareto emphasized that the magnitude of this probabil-

21. Sanger 1895 summarizes Pareto's analysis.

ity depends on the underlying probabilities. If we take all the probabilities equal to 1/2, then the odds in favor of the change in A are 8 to 1. He shows how the analysis would be changed if there were a cause, like a change in transportation costs, affecting several commodities. Having given the first really thorough treatment of the formal side of the problem, Pareto dismissed the analysis as being of no practical value, for he saw no possible basis for grouping the commodities and assigning the probabilities. In the *Cours* (1896, 266) he mentions the argument and says it is plausible, if at all, only for very short-term changes.

Edgeworth wrote expository articles (see 1887 and 1889a) and spent forty years defending the approach—though without extending the analysis in any significant way. He insisted that the probability approach be given its due as one of the valid approaches to index number analysis. In Britain, the stochastic approach to index numbers found a modest, but seemingly secure, place in the economics of the early twentieth century. Bowley wrote it into his *Elements of Statistics* (1901), the first statistics textbook for economists in English. In his surveys of index number theory (1919, 1928), he accepted the basic Edgeworthian principle that, for some purposes at least, the choice of index formula rested on the properties of the distribution of price changes. Flux (1907) attempted to combine Edgeworth's emphasis on the need for background distributional analysis with Pearson's distributional theory. In America, Fisher (1902) saw some merit in the approach and so did Mitchell (1915). Though most economists ignored it, there were enough competent people who accepted the stochastic approach for its prospect of survival to seem good.

Of the critics, two were particularly notable—C. M. Walsh and J. M. Keynes. In his monumental work on index numbers, the *Measurement of Exchange Value* (1901), Walsh overlooked the indefinite standard and mentioned probability only in connection with a crude version of the probability argument.[22] Edgeworth's review (1901) shows how this snub displeased him. The differences with Walsh rose to an epic level with Walsh's book (1921) and Edgeworth's articles in reply (1923, 1925a, 1925b). However I will concentrate on Keynes's criticisms because they had greater significance for the fate of the stochastic approach. Index numbers were important to Keynes. They had a significant place in his monetary economics, from the Adam Smith

22. The historical discussions in Walsh's book are still very valuable.

prize essay of 1909, *The Method of Index Numbers,* to *The General Theory* nearly thirty years later. In 1905 when he began work on what became the *Treatise on Probability* (1921), the only important application of probability-based statistics to economics was Edgeworth's index number analysis. The stochastic approach was the only piece of statistical economics with which Keynes ever became thoroughly familiar, and his experience with it helped form his celebrated antipathy towards mathematical and statistical economics. He made the stochastic approach and the hunting down of Edgeworth's "mythical creature," the indefinite standard, his own special business.

The indefinite standard is the subject of chapter 8 of the Adam Smith essay, is mentioned in the *Treatise on Probability,* and is covered in the review of index number theory in volume 1 of the *Treatise on Money*—where it draws the full power of Keynes's rhetoric. As it is often argued that there is a close relationship between Keynes's views on probability and his economic ideas, I begin by considering the relationship between Keynes's philosophy of probability and his index number theory.

On the philosophy of probability and statistics, there were interesting points of both agreement and disagreement among Keynes and Jevons and Edgeworth. In his conception of probability, as degree of rational belief, Keynes followed Jevons. He interpreted Edgeworth's philosophy of probability as frequentist and, thus, fundamentally unsound. Yet, though he criticized Edgeworth's foundational work, he held that much of the analysis that most interested Edgeworth would remain intact after proper reexamination.[23] In particular, Edgeworth's index number analysis was not necessarily vitiated by weak probability foundations.

Compared to Jevons, Edgeworth was very self-critical in the use of probabilistic analysis, and Keynes took this critical stance further. His critique of modern statistical theory, as it appeared in part 5 of the *Treatise on Probability* does not rest on its dependence upon a false doctrine of probability, but in accepting as highly confirmed, inductions that were patently not so. For Keynes there was too little atten-

23. In his obituary of Edgeworth, Keynes gave the following account of their differences. "I have often pressed him to give an opinion as to how far the modern theory of statistics can stand if the frequency theory falls as a logical doctrine. He would always reply to the effect that the collapse of the frequency theory would affect the *universality* of application of statistical theory, but that large masses of statistical data did, nevertheless, in his opinion satisfy the conditions necessary for the validity of statistical theory, whatever these might be. I expect that this is true" (1926, 260).

tion to varying the instances, too uncritical an acceptance of the existence of a relationship or distribution. Keynes had learned Edgeworth's lessons very well, for it was Edgeworth who required evidence that the data conformed to a type, that the population was not a mixture of distributions.

In Keynes's discussions of the probability approach to index numbers there is never a suggestion that the approach fails *because* it is probability based; when the approach is described as "root and branch erroneous," it is not because it is built on probabilistic foundations but for other reasons. Keynes was as much a disciple of Edgeworth as Bowley was; and Keynes's arguments against Edgeworth's index number work were not based on a philosophical challenge to Edgeworth.

The Adam Smith essay discussed both the theory of the indefinite standard and the error analysis of index numbers (1909, chap. 5). On the latter, Keynes accepted Edgeworth's mathematical argument but questioned its applicability. Keynes thought misuse of Edgeworth's conclusion about the unimportance of weighting was widespread. In his review of Fisher's *Purchasing Power of Money,* he wrote, "The conditions for the application of the theorem . . . have often been misapprehended, but seldom to the extent of applying it to a case where *three* commodities only are the subject of weighting" (1911b, 380). His treatment of the indefinite standard is also remarkably faithful to the general framework devised by Edgeworth. Indeed, the appendix, originally published as "The Principal Averages and the Laws of Error Which Lead to Them" (1911a), was based on Edgeworth's principle that "a law of probable dispersion is the only *kind* of valid ground which we can give for selecting, for instance, the geometric mean or the median" (Keynes 1909, 109).

For Keynes as for Edgeworth, the "origins of the method are to be found in the doctrines of the quantity theory of money" (105), and Keynes did not dispute that it was meaningful to "*separate* that part of the fluctuation, which is due to changes on the side of money, from that part which is due to changes on the side of commodities" (110). The essay criticized the stochastic approach very much in its own terms.[24]

24. Keynes (1909) thought Jevons's analysis confused. "We cannot speak of 'diminished demand,' but only of . . . diminished demand relative to the demand for other things" (106). However, he wrote how Jevons "had to solve the problem of index-numbers from the begin-

Keynes's main objection was that Edgeworth's investigation was directed to discovering the probable variation of prices—a "useless goal" (111), when what was needed was the probable variation of the general exchange value. "How then are we to discover the law of probable variation of the exchange value of commodities? We can only do so, I think, if we suppose ourselves to know already the change in the exchange value in a number of preliminary cases" (112). After pursuing this suggestion, Keynes concluded, "The conclusion of the argument shows that the method of probabilities is not truly practicable, except in the roughest possible way. It turns essentially upon our obtaining reasonably reliable laws of probable variation of general exchange value, and there seems no means, in the present state of knowledge, of obtaining such generalisations" (119).

The discussion in the *Treatise on Money* has a different tone. As in his essays on Marshall and Edgeworth, Keynes emphasized the error in treating economics like a physical science. In his discussion, he avoided the technical details and concentrated on the view that there were two sets of forces governing relative prices and the absolute price level.

> It makes the mistake of assuming that there is a meaning of the price level, as a measure in some sense or another of the value of money, which retains its value unaltered when only *relative* prices have changes. The abstraction between the two sets of forces, which seemed momentarily plausible when we made it, is a false abstraction, because the thing under observation, namely the price level, is itself a function of relative prices and liable to change its value whenever, and merely because, relative prices have changed. The hypothetical change in the price level which would have occurred *if* there had been no changes in relative prices, is no longer relevant if relative prices have in fact changed—for the change in relative prices has in itself affected the price level. (1930, 77)

This criticism was in line with the argument of the *Treatise on Money* that the relationship between different price levels was the key to the phenomenon of the credit cycle. It was part of Keynes's rejection of the quantity equation as a useful approach to monetary problems.

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ning; and it is scarcely an exaggeration to say that he made as much progress in this brief pamphlet as has been made by all succeeding authors put together" (1936, 120).

Retrospect

From the early nineteenth century, ideas about probability and statistics were part of the intellectual atmosphere in which economists worked. We have seen how some of these ideas were applied by Jevons and Edgeworth to an important economic problem, discovering the causes of price changes and assessing the relative importance of those causes. There seemed to be a good match between economists' ideas on how prices change and the assumptions behind the statistical methods. There was also a body of observations to which the methods could be applied.

Jevons's work was both a failure and a great success. He established the technique of index numbers where Scrope, Lowe, and other writers had failed and demonstrated the big result—that gold had depreciated. Yet very few of those who adopted the technique used it for the purpose for which it had been developed. Only Edgeworth took seriously the basic probability argument.

The stochastic approach had ceased developing long before the arrival of the new approaches to index number analysis of the 1920s—the test approach of Fisher (1922) and the economic theory of index numbers of Konüs (1924). One of the pioneers of the latter approach was Edgeworth—in his first paper on index numbers. In Frisch's authoritative survey of index number theory (1936), the economic theory was given central place, and Keynes's (1930, 76) judgement that the stochastic approach was "root-and-branch erroneous" was endorsed. That judgement has more or less stuck.

Although some of the leading economists of the nineteenth century contributed to the stochastic approach, its influence on economics was very limited. It treated a narrow technical problem and was seen neither as a great example nor a great warning. Its statistical analysis may have been decades "ahead" of that used in other branches of economics, but later statistical economists did not look to it for inspiration and example, and it had little influence on twentieth-century econometrics.

The great success story of statistics during this era was the correlation analysis associated with the work of Galton and Pearson on heredity. Edgeworth developed new statistical theory in conjunction with his index number work, but the statistical work seemed only to repeat established themes. The contrast with correlation analysis was obvious. The viability of the basic ideas was not important. The genetic ideas associated with the new statistical methods were soon rejected, but the methods had a profound impact, not least on the rising generation of statistical economists.

One of the problems left unresolved by the history of the stochastic approach was the relationship between statistical analysis and economic theory. Our protagonists had most diverse views on the subject, and there is little sign of any real development. Cournot presented a purely statistical theory—to show the gain from having an economic theory. For Jevons, the statistical approach could be used in conjunction with economic theory and other information. However, when Pareto formalized the argument, he concluded that application of the probability argument was impossible. For Edgeworth and the young Keynes, the statistical argument was founded on the quantity theory; though Keynes, like Pareto, thought application of the argument impractical. For the mature Keynes, the approach was to be rejected because the associated economic theory was defective.

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