

BARNETT HOUSE PAPERS

No. 10

THE STATISTICAL VERIFICATION  
OF SOCIAL AND ECONOMIC  
THEORY

By SIR JOSIAH STAMP

G.B.E., D.Sc., F.B.A.

SIDNEY BALL LECTURE

November 5, 1926

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## THE STATISTICAL VERIFICATION OF SOCIAL AND ECONOMIC THEORY

### I. *Introduction.*

'Science sans expérience  
N'apporte pas grande assurance.'—*Paré's Canon* (1510-90).

ANYONE who reflects upon the troubles and problems of the civilized world to-day must realize that questions are constantly put to economic science to which no clear answer, and sometimes no answer at all, is being given. If we are satisfied that no body of knowledge is worthy the name of science unless it has a capacity for development to meet such new needs, we shall be looking round to see upon what lines growth and change in economics must take place. Will the old methods of inquiry and proof suffice, or must we seek new ones, not necessarily 'better', but more adapted to cope with the fresh problem and to unfold hidden secrets?

It is my view that the analytical method in the line of Ricardo, Mill, and Marshall has, for the time being, at any rate, reached the limit of its usefulness, and that no striking advance can be made thereby from the ground now occupied. I do not think, moreover, that the refinements and extension of it in the mathematical or Cambridge School is likely to lead to important results. Certainly inductions by the Historical School must be so broad, and so lacking in touch with modern data, that they will yield only a small contribution to the constructive problems of the moment.

I believe that for the next advance we must depend upon realistic statistical investigation and verification, and that we stand on the threshold of a new method, which is of general application.

The device of 'sampling'—long suspect as if it led to

mere generalization from the particular—with its recognized tests for a limit of error, and the great assurance which conformity to the 'normal curve of error' may now give, can be applied to a wide variety of economic facts. In the past statistics seemed to be valuable only so far as they dealt with aggregates and told the 'whole story'. But statistics for aggregates are often unattainable, or unmanageable, and indeed more liable to error from extraneous causes than statistics taken at random, but clean from a disturbing element which destroys the value of the particular kinds of inference sought. A sample, unbiased in any other direction, but avoiding the element in question, may afford a clearer view than aggregated statistics. Many detailed investigations have, of course, been made in the past to establish general facts, which are otherwise open to dispute, such as the density of particular populations, the ratio of rent to income, or the distribution of income, but ordinarily no direct use has been made of such facts, when ascertained, to develop theory and generalization of principle. Two types of *ad hoc* inquiry are possible, first that approximating in character to the older type, viz. without any preconceived theoretical rules in mind, to establish a body of facts or relationships, and then to elaborate a theoretical explanation of them which shall supplement, or fit into, the general body of economic theory. Here a theory is made to account for the facts. The second is to take a definite theoretical principle as a question to be answered: Does this really exist in fact? and then to make a careful test from existing data or to collect data specially for the purpose. Here facts are ransacked to see if the theory lies in them.

The new type of special investigation on the social side for the collection of facts was carried to a higher stage of scientific accuracy and usefulness by Mr. and Mrs. Sidney Webb, and on its statistical side by Francis Galton and Charles Booth. Charles Booth was disturbed by the contradictory social theories about the poor:

'In the opinion of some, the great evils to be met were improvidence and self-indulgence. To relieve from the consequences of these was to aggravate the mischief. Yet another view was held, that the selfishness and vice of low lives was the result of the selfishness and vice of high lives; that the first duty of the rich was to produce among their poorer neighbours the physical condition which alone could render decent existence possible. Good air, more room, better clothes, better food and similar advantages would exorcize the demon which ran rife. "Stimulate private charity," said one school. "Relieve the rates." "It is the State-paid pauper who is the source of all harm." "Down with charity," said another set; the very word has become a degradation. Let the State see to it that the toiling millions are fed and housed as they should be." "Toiling millions!" would be replied. "The people who are in want never really toil at all. They are wastrels, lazy, and ill-tempered. No one in England who will work need want." . . . These various views, and many others, were listened to by Charles Booth, and ever more earnestly did he seek an answer to the questions: Who are the people of England? How do they really live? What do they really want? Do they want what is good, and if so, how is it to be given to them?'<sup>1</sup>

In an address to the Royal Statistical Society in 1887 he referred to the 'sense of helplessness' on all sides.

'To relieve this sense of helplessness, the problems of human life must be better stated. The *a priori* reasoning of political economy, orthodox and unorthodox alike, fails from want of reality. At its base are a series of assumptions very imperfectly connected with the observed facts of life. We need to begin with a true picture of the modern industrial organism, the interchange of service, the exercise of faculty, the demands and satisfaction of desire. It is the possibility of such a picture as this that I wish to suggest, and

<sup>1</sup> Charles Booth: *A Memoir* (Macmillan, 1918), by Mrs. Charles Booth, pp. 13-15.

it is as a contribution to it that I have written this paper.'<sup>1</sup>

This is not the place for an assessment of the value of Booth's statistical conclusions from his examination into the conditions of London life, nor their influence in correcting current conceptions of fact and tendency, nor the remoter reactions on social and economic theory. An interesting and sufficient account may be found in Mrs. Sidney Webb's autobiography, *My Apprenticeship*. Booth's questions were hardly those of a theoretical or academic economist, but his method of *ad hoc* inquiry to test theories of social betterment were the forerunners of the specific investigation of later days.

## II. *Modern Developments in Exposition of Theory.*

Professor Marshall's volume on Principles may be taken as the great example of exposition of economic theory on lines new thirty-six years ago and hardly yet superseded. It is interesting to examine its dependence, not so much upon statistical illustration as upon statistical verification. There are several tables (population, growth of wealth, &c.) used to aid the descriptive portions. When dealing with the nature of the 'demand curve' and 'elasticity', he refers to the study of exact lists of demand prices and to the difficulty of interpreting them, but he gives no examples, and indicates diagrammatically how to observe percentage increases over a period of years, or the rate of growth. He thinks that the statistics of consumption published by governments for many commodities are of very little service in helping inductive study, but elaborates the hint given in Jevons' theory 'that traders could further it greatly by analysing their own accounts'. 'If a sufficient number of tables by different sections of society could be obtained, they would afford the means of estimating indirectly

<sup>1</sup> *Condition and Occupations of the People of the Tower Hamlets, 1886-7*, by Charles Booth, 1887, p. 7.

the variations in total demand that would result from extreme variations in price, and thus attaining an end which is inaccessible by any other route.' After analysing the doctrine of maximum satisfaction, and the effect of taxation, he urges the need for more statistics. In dealing with the nature of profits and the element of risk, he deplores the absence of statistics of capital in different businesses. From the statistics of American bureaux, unspecified by him, 'inexact as they are', he says, 'we may conclude that the annual output is less than the capital in industries where the plant is very expensive and the processes very long, but is four times the capital where raw material is expensive and production rapid'. He also draws inferences about the ratio of turnover and circulating capital to the wages bill. But this is virtually the only statistical 'verification' in the book. Elsewhere (p. 492) he deplores that public statistics are not yet properly organized. 'The few, therefore, get their way, although if statistical increases of the interests involved were available, it might prove that the aggregate of the interests of the few was only a tenth or a hundredth part of the aggregation of the interests of the silent many.' He hoped that, in future, statistics of consumption would be so organized as to afford demand schedules to show the amount of 'consumers' surplus', and to give a guide to public action. The idea of testing correlation, or sampling for modal types, is hardly even germinal in his treatment. A superficial observer might imagine that because the volume is full of mathematics and graphs, and because statistical science also employs mathematics and graphs, that his work is statistical. But in fact they are the poles apart; one is the refinement of analysis and the deductive method; the other the advanced spirit of induction.

Writing at the same time (1891) on the *Scope and Method of Political Economy*, Dr. J. Neville Keynes dealt with the method of specific experience, the deductive

method, symbolical and diagrammatic methods, the historical method, and wound up with 'Political Economy and Statistics'. He dealt with concomitant variations and the method of curves, and said the functions of statistics in economic theory are first to suggest empirical laws, and secondly, to supplement deductive reasoning by the test of experience, and thirdly, to elucidate and interpret particular concrete phenomena. As illustrations he gave Malthus' inquiries concerning the marriage rate and fertility rate and his inference that population tends to double in twenty-five years—a statistical rather than an economic inference, be it noted. The tendency of financial crises to recur at periodical intervals was *not* first worked out theoretically, it was disclosed by statistical observations and theories afterwards propounded to account for it. Another simple illustration was the autumnal drain on the money market. As an example of deductive verification, Dr. Keynes cited Bagehot's statistical study testing the legitimacy of the postulate that in modern industrial communities there tends to be movement of labour from the worse to the better-paid localities—the 'tides of people'. He elaborated the dangers present, and the precautions necessary, but concluded that the statistical method easily makes good its claim to rank as a thoroughly effective and reliable instrument of science.

At a later date, nineteen years ago, Marshall said to the Royal Economic Society, 'Qualitative analysis has done the greater part of its work . . . the higher and more difficult task of quantitative analysis must wait upon the slow growth of thorough realistic statistics.' As Professor Wesley Mitchell has said, this is not a pronouncement of antagonisms, for we all practise both, 'shifting our emphasis according to the task we have in hand . . . qualitative analysis cannot be dispensed with, because quantitative work itself involves distinctions of kind, which start with distinctions of quality.'<sup>1</sup>

<sup>1</sup> Address to the Economic Association at Chicago, 1924.

When we come to a similar theoretical work written thirty years later than Marshall's *Principles* and Dr. Keynes' work, Professor Pigou's *Economics of Welfare*, we find a considerable development in the extent to which theoretical findings are 'checked up', so to speak, by reference to statistical investigation. This is partly because the fund of continuous public statistics is fuller, but also because direct inquiries are more available. The coefficient of correlation appears in the actual text. Much of the Malthusian and Ricardian economics turned upon the direct relation between the fertility of the working population and the amount of remuneration, the inevitable tendency for population to multiply up to the level of subsistence, so that any attempt to raise the level of subsistence must be thwarted. The iron law of wages made a dismal science indeed. The progress of the nineteenth century did much under general observation to show that the birth-rate and raised standard were not directly but inversely related. One cannot imagine a doctrine calculated to be more subversive of early economic theory. Even the eighteenth-century doctrine of the incidence of taxation was bound up with this conception. It was said to be impossible to tax the working classes because they were already only on the level of subsistence—encroachment on this would lessen their numbers by starvation and lower fertility, and the supply price of labour would rise to the original level. To some extent, in the then stage of education and civilization, the subsistence doctrine had a substratum of fact. But the trail of it has lain across too long a period of years since, during which the tendency has been reversed. Brentano's contentions are taken up by Pigou and detailed confirmation found in Dr. Heron's statistical study of London, in Bertillon's work, and in papers in the *Statistical Journal* by Dr. Stevenson, Yule, and Dr. Leonard Darwin, from which Pigou concludes: 'The above discussion disproves the suggestion that the beneficial effect on economic welfare of an increase in

the real income of wage earners will be neutralized by an expansion of population.<sup>1</sup>

In his treatment of the useful and well-known doctrines of Economics upon diminishing and increasing returns and their relation to price, monopoly, and taxation, Pigou remarks that they are results in pure theory.

'We have made a number of boxes and sub-boxes, labelled strong increasing returns, weak increasing returns, constant returns, diminishing returns, &c., but they are *empty* boxes and, therefore, some say useless except as toys, for we do not know to which of them the actual industries of real life belong. Statistical technique by itself, in spite of the growing volume and improving qualities of the material available, will not enable us to accomplish this, for statistics refer only to the past. But able business men with a detailed realistic knowledge of the conditions of their several industries should be able to provide economists with raw material for rough judgements. Economists unaided cannot fill their empty boxes because they lack the necessary realistic knowledge, and business men unaided cannot fill them because they do not know where or what the boxes are. With collaboration, however, it is not unreasonable to hope that some measure of success may eventually be achieved. At least, the effort is worth making. It is premature, in impatience at the present shortage of straw, to scrap our brickmaking machinery. It is the better

<sup>1</sup> This had indeed been one of the 'discoveries' from Charles Booth's inquiry. 'To one who had been brought up in the political economy of Malthus, and taught to believe that every increment of income and security would inevitably be accompanied by additional children in working-class families, it was disconcerting to discover that the greater the poverty and overcrowding, and especially the greater the insecurity of the livelihood, the more reckless became the breeding of children; whilst every increment in income, and especially every rise in the regularity and the security of the income in working-class families, was found to be accompanied, according to the statistics, by a more successful control of the birth-rate.'

part to advertise abroad the urgent need for straw and to call for students to produce it.'

No one should fail to read Dr. Clapham's witty and penetrating article on 'Empty Economic Boxes' in the *Economic Journal*, 1922, and Professor Pigou's rejoinder. Says the economic historian, weary of analysis:—

'I myself did not appreciate how completely empty the boxes were until I had given a number of public demonstrations with them. And if more acute minds are not likely to be so misled, the rank and file surely are. Unless we have a good prospect in the near future of filling the boxes reasonably full, there is, I hold, grave danger to an essentially practical science, such as Economics, in the elaboration of hypothetical conclusions about, say, human welfare and taxes in relation to industries which cannot be specified.'

Professor Pigou remarks that Dr. Clapham 'maintains three separate things; first, that his economic boxes, so long as they are empty, cannot have practical usefulness; secondly, that, even if they were filled, they would not have practical usefulness; thirdly, that they cannot be filled'. When he deals with the third he concludes, 'To declare, of a piece of work that has not yet been seriously tackled, that it is impossible, is, in my judgement, at least premature. Something, I believe, might be accomplished if economists would take counsel with leaders of business, expert in particular branches of production. Of course, if Dr. Clapham, or anybody else, goes to them and says, "My dear fellows, an 'analytic' up at Cambridge wants to know if your industries obey the laws of diminishing, constant, or increasing returns", no great illumination is likely to result. But, if he were to ask them to discuss the conditions, as regards the relation between aggregate output and cost, under which various important articles have been and are being produced—which is really asking a great deal more—I for one do not believe that he would always come empty away. Nor need we rely only on the general judgement of people

expect in particular industries. There is already available a certain amount of statistical material—and we may reasonably hope that this material will both grow in quantity and improve in quality—from which students with the requisite mathematical equipment may make rough deductions about the shapes of certain supply schedules. On the side of demand something on these lines has already been accomplished. On the side of supply the task is undoubtedly more difficult. But we need not conclude that it is impossible. The hope of which I have just spoken, that better statistical material may presently be available for study, thus making the inquiry more feasible than it has been hitherto, should itself forbid that. There is indeed a lion in the path; the fact that those people—with the towering exception of Jevons—who have the qualities required for conducting a detailed intensive study of particular industries and writing monographs about them, are not usually well versed either in the more intricate parts of economic analysis or in modern statistical technique; while the “analytics” lack alike capacity and inclination for these detailed studies. For this there is only one real remedy. We must endeavour to train up more men of the calibre of Jevons, who are equally at home in both fields. Till we can accomplish that, the next best thing, for those lesser persons who are moderately qualified for the one sort of inquiry and for the other, is to work together in combination, and not to waste time in quarrelling, perhaps on the basis of an imperfect understanding, with the deficiencies of one another’s methods.’ Dr. Clapham retorts:

‘I had anticipated that the facts and statistics demanded might be, by common consent, at present unprocurable; but I had hoped that they might be specified. And now I am paid with a cheque drawn on the bank of an unborn Jevons. Can no one give us more current coin?’

### III. *Recent comments on the old methods.*

Wesley Mitchell inclines to support the view that quantitative analysis ‘shows no more promise of providing a statistical complement of pure theory’ than it has ever done, if by pure theory we are thinking of analysis according to Jevons and Marshall. And he says it never will, unless we recast the old problems into ‘new forms amenable to statistical attack’, and this involves a change in the content of economic theory. He illustrates this as follows: “In the course of his investigations into economic cycles, Professor Henry L. Moore needed to formulate ‘the concrete laws of demand for the representative crops.’ He approached this task by quoting Dr. Marshall’s qualitative analysis of demand. But with Marshall’s formulation of the problem it was impossible to get quantitative results. For Marshall treated the relation between demand and price on the assumptions (1) that the changes in the two variables are infinitesimal, (2) that the conditions remain constant, and (3) that the shape of the demand curve is known. Professor Moore, on the contrary, had to derive his curves of demand, and to deal with the real world where no factor is known to remain constant, and where changes in demand and price are finite. Attacking his problem by mathematical statistics, Moore obtained equations expressing the relations between the demands for and the prices of corn, hay, oats, and potatoes; he determined the precision of these equations as formulas for predicting prices, and he measured the elasticity of demand for each crop. As he pointed out in concluding the discussion, his results do not solve Marshall’s problem.

But is not Moore’s problem more significant theoretically, as well as more relevant to economic practice? If quantitative analysis can give us empirically valid demand curves and coefficients of elasticity for numerous commodities, shall we not have a better theory of demand than qualitative analysis can supply?



Wesley Mitchell discusses whether the view, held by many statistical economists to-day, that the function of statistics is to provide a statistical complement for economics and not to recast economic theory, is sound. He bases his preference for the most radical suggestion on a very suggestive consideration of what has happened in physics.

'The mechanical view involves the notions of sameness, of certainty, of invariant laws; the statistical view involves the motions of variety, of probability, of approximations. Yet Clerk-Maxwell's "new kind of uniformity" was found to yield results in many physical problems which corresponded closely to results attained on mechanical lines.

'Such a close correspondence between the results based on speculation and the results based on statistical observation is not to be expected in economics, for three reasons. First, the cases summed up in our statistics seldom if ever approach in number the millions of millions of molecules, or atoms, or electrons of the physicist. Second, the units in economic aggregates are less similar than the molecules or atoms of a given element. Third, we cannot approach closely the isolation practices of the laboratory. For these reasons the elements of variety, of uncertainty, of imperfect approximation are more prominent in the statistical work of the social sciences than in the statistical work of the natural sciences.

'And because our statistical results are so marked by these imperfections they do not approach so closely to the results of our reasoning on the basis of assumed premises. Hence the development of statistical method may be expected to make more radical changes in economic than it makes in physical theory.'

The analysts reach their mass generalizations about markets and demand by considering the individual's behaviour and multiplying him into results which can never be precise, whereas the statistician slips the individual stage and treats of masses and modes direct.

'With the fuller reports they are obtaining and the more powerful technique they are developing, properly equipped investigators can study the relations between the actual responses of prices to changes in supply and of supply to changes in prices. They can work out demand schedules which hold empirically within the ranges and periods covered by experience. They can trace the changes in the consumption of commodities by whole communities or by large groups. They can investigate the relations between monetary changes and "real" incomes, between saving and spending, between different forms of economic organization and production.

'With all these fascinating problems and numberless others before them in shape for attack, it seems unlikely that the quantitative workers will retain a keen interest in imaginary individuals coming to imaginary markets with ready-made scales of bid and offer prices. Their theories will probably be theories about the relationships among the variables which measure objective processes. There is little likelihood that the old explanations will be refuted by these investigators, but much likelihood that they will be disregarded.'

But these subjects are not the only ones to which the new methods are suitable and indeed essential, for they apply certainly to some in which the old analysis has been helpless, such as the exact relation, with time changes or constant growth, between different series, output, costs, wages, &c. The quantitative workers cannot content themselves by staying always on the money level of analysis, or always on the commodity level; and 'they cannot pass back and forth between the two levels without realizing what they are doing, as could the classical economists and their followers'. In particular, the influence of changing price levels upon business activity can yield only to this kind of investigation. Again, as Wesley Mitchell says, the quantitative workers will have a special predilection for institutional problems, because institutions standardize behaviour, and thereby facilitate statistical procedure.

IV. *The United States and Britain—a present contrast.*

If we refer to the United States in particular, we immediately see a great improvement in recent years. First, the body of statistical material has grown; secondly, technical methods of statistical analysis are much better; thirdly, the funds and agencies for the laboratory work are much increased, with numerous endowed organizations for research. In this country it would, I think, be true to say that aggregated and national statistics have considerably improved, although we are still without any knowledge of total production, and have not much knowledge of changes in distribution of income and the net product, while our banking statistics are very difficult to interpret and not so complete as they used to be. But in the field of sampled or *ad hoc* inquiry we are very weak indeed, depending on the efforts of individual statisticians and research students, with very little team work. Our technical equipment is just as good in quality as that in the United States, but there are fewer craftsmen. This is partly because in the third respect we are completely behind the United States, and we have practically no endowed statistical inquiry. The London and Cambridge Economic Service has to rely largely on annual subscriptions from business firms. The importance of this difference cannot be exaggerated, for as Wesley Mitchell rightly says, the quantitative method, unlike the qualitative which needs only a thinker and his books and pencil, has to face a heavy burden of routine labour, computers, and field workers.

The list of institutions in the United States which are responsible for co-ordinated team work and diagnosis is now very imposing.

V. *Examples. (a) Time series with published statistics.*

It has been shown that there are two distinct aspects of statistical inquiry. In the *first*, the statistics are gathered and massed for their own sake, uniformities and variations are noticed, and hypotheses constructed

to account for the changes, which hypotheses may rest on physical facts, i. e. sunspots and harvests; or economic theories, i. e. credit control; or a combination of physical fact and economic theory, i. e. aggregate gold production and credit based thereon.

In the *second* aspect, a theory exists, and a statistical inquiry is undertaken *ad hoc* to test it:

- (1) A *new* set of aggregated statistics is collected by public or private authorities to solve a problem,
- or (2) Samples are made on scientific lines for different places or for different times,
- or (3) Existing lines of statistics are put into juxtaposition for the first time and correlation tested, or the ratio of variation ascertained. In other words, it may be ascertained that two series move sympathetically, and we wish to know the regularity and constancy of that sympathy. It is, for example, one thing to say that when set A of facts varies in one direction set B will vary in a specified direction in 95 per cent. of the cases; it is another to determine that when one varies by 10 per cent. from its own average, the other will vary 20 per cent. from its own average.

In 1918 I made the first use in this country of the linear trend, fitted by the method of least squares, to eliminate the common time-growth element from various series of statistics which had different rates of growth, in order that the concomitant variations, free from such constant growth, could be correlated. In the 'Effect of Trade Fluctuations upon Profits',<sup>1</sup> I remarked

'In our pre-war experience when monthly statistics of foreign trade were quoted or the bankers' clearing house figures were referred to, all kinds of inferences were drawn for and against the political features of the day. If the "returns" were up by 10 per cent. on the previous year, it was held to be something for jubilation, if they were down, perhaps the tariff

<sup>1</sup> *Journal of the Royal Statistical Society*, 1918.

controversy took on a new twist. But we do not do increased trade for its own sake—we do it because we hope for increased profits. And there was always the assumption that the up and down movement of trade connoted a related degree of altered “profit” prosperity. (There was the further ethical or teleological assumption, I suppose, that if we all made more profits, then we were all so much happier and better off.) But in many of our minds, as we looked at the figures, there were misgivings and unsettled questions; what would the increase be like if the natural growth of population were taken into account and how far was it a *real* increase in output of commodities, as distinct from an increase due to higher “prices” for the old quantity of goods? If a given measure of trade has risen, say from £100 to £120, and we rejoice in a rise of 20 per cent., it may be either that 120 units have been sold instead of 100 at a regular price of £1 or that 100 units have been sold at a price of £1 4s. each instead of £1, or the result may be a combination of both changed quantity and changed price; it may even be that one factor has actually *diminished*, but that its effect is more than offset by the increase in the other. What is the actual or probable change in profit that accompanies such a change of 20 per cent. according to the cause of the change? Will a like change accompany a second, or third ensuing rise of 20 per cent.? Will the relations found to exist for increases hold also for decreases, or what difference may we expect? These are all questions to which we should like answers, and they suggest many more. For trade in general it may be said that our notions about the relative influence of quantity and price upon profits are very theoretical and abstract.

I used in addition to correlations of the variation from the linear trend, deviations about the moving average and correlation of variate differences, first and second. For the relation between coal profits and tonnage and price I arrived at definite results, showing that the change in profits due to a change in price had been three times as

great as the price change, i. e. that a 1 per cent. fluctuation in price was accompanied by a 3 per cent. fluctuation in profit. But in the case of a unit variation in output, the change in profit was only quite a small fraction over unity. In these investigations all the coefficients of correlation were very high, and it may be said that I was merely putting into elaborate figures conclusions that would be obvious to ordinary reflection. But the real object was to find the ratio of variation and especially to disentangle the effects of price changes and output changes. The fact that demand for coal tends to be very inelastic in the *neighbourhood* of any normal condition is also deducible from the inquiry. I also showed that increase in the prosperity of the coal industry may be symptomatic of increased trade generally, if due to increased output only. The correlation between railway profits and the price of coal was negative, but high and significant, i. e. if the price of coal was markedly increased, then the railway profits were clearly less in the following year—a lag of a year giving the most marked correlation. The deviation of railway profits from the trend was about one-quarter of the opposite deviation of coal prices. Gas profits had a similar negative correlation with coal prices, and the deviation about two-thirds in amplitude. The monthly correlation of the buying price of raw textiles in an Eastern market, and the selling price in London, over seven years, proved to be almost complete (0.97), so that fluctuations in a merchant's total profits were entirely due to volume of trade. Cotton spinning gave an interesting result.

‘There is very little regular relationship between the profits of cotton spinning and either the purchase price of raw cotton or the sale price of yarn, but the changes in the *difference* between the two prices is more indicative of changes in the amount of profits. Of late years the quantity of raw cotton imported has been some criterion of the prosperity of the trade. The purchase price of raw cotton and the sale price of yarn are very closely related indeed.

'The fluctuations in the profits of spinning are very violent. The deviation of the price from the trend of prices is 14.4 per cent. on the average price, but the deviation of profits is 154 per cent. or nearly eleven times as great. (Standard deviation taken.) Reckoned by another method also, it is ten times as great (average deviation). It may fairly be said the fluctuations in profits are ten times as great as fluctuations in prices.'

As regards general profits, I was able to come to such conclusions as the following :

'Speaking for the results of trade as a whole, the statistics of the Bankers' Clearing House and of the railway receipts (or tonnage) have afforded a reliable test as to the *direction* of the movements in profits, and the movement of foreign trade is also a fair but less important criterion.

'The fluctuation in profits has generally been rather less, in *magnitude* or *range* than the fluctuation in statistics of "turnover", such as banking or foreign trade statistics, which reflect both quantities and prices, and it may be taken roughly at two-thirds to three-fourths of such short period changes in trade returns.

'The influence of a change in price level on profits *as a whole* is far less than is frequently supposed by those who base their views upon observations of the striking effect of price changes in particular industries.

'In times of rising prices, increases in profits have been made over and above the amount that would arise upon the increased output that such prices induce, but the additional profit is not usually much greater in proportion than the rise in price, if the period taken is not less than a year. There is no evidence as to the effect of such changes measured over shorter periods than a year.

'Although the increased quantities evoked by increased prices have followed quickly enough to keep profits within such limits, the check has not been permanent, and continually renewed stimulus by the raising of the price level has resulted in increases of profits much greater than could have followed the

ordinary increases in output (due to increasing population) at a constant price level. Similarly the drop in prices from 1880 to 1895 kept profits down considerably below what would have resulted from the *actual* output at a constant price level, and in itself was instrumental in depressing that output.

'The "turnover" of foreign trade had become a *relatively* less important part of the whole trade of the country during the previous thirty years.

'The annual trend of increase in trade freed from all fluctuation has to a great extent been made up of the larger output of existing businesses increasing continually in size, and to a relatively smaller extent of the output added by new businesses.'

The investigation had, perhaps, some value in indicating the kind of pitfalls that have to be avoided in dealing with this class of statistics, and the care that must be taken to make them 'chemically clean', so to speak, before the investigation. Our own Oxford Professor Edgeworth remarked on this paper that there was ever in the class known as the non-statistical reader a

'natural and not altogether unhealthy suspicion of any technical method, any *organon* which seemed intended to supersede the use of common sense. It was Locke, or some one who wrote, like Locke, against the Aristotelian syllogism, who protested that the Almighty had not dealt so very sparingly with the noblest of his creatures as to make them only bipeds, leaving it to Aristotle to make them rational. A similar prejudice on the part of common sense against correlation and other mathematical instruments is to be apprehended.'

#### VI. Examples. (b) Time series without the 'growth' element—Original inquiry.

We can now look at an instance of a personal, as against a collective, inquiry.

Mr. Edgar Smith recently made an investigation, the results of which are given in his book *Common Stocks as*

*Long Term Investments*—what he describes as the record of the failure of facts to support a theory. The theory in question is that, during the period when the prices of goods and services are falling, bonds are better investments than ordinary shares. He traces the effect of investments and holdings over the period of seventeen to twenty-two years and finds that out of twelve tests the investor would have a much higher investment return from ordinary shares than from bonds. In every case except one, the advantage is on the side of ordinary shares, when income and increase in capital value are added together. His investigation is well worth following. The broad conclusion that he arrives at is that, cumulatively, the results tend to show that well diversified lists of common stocks, selected on simple and broad principles, respond to some underlying factor which gives them a margin of advantage over high-grade bonds for long-term investment. There are many reasons why this particular investigation has no universal validity, but it is an interesting example of the verification of theory. A similar test quoted by Mr. Hartley Withers for England, though much more limited, tends in the direction of confirming it, and Mr. Hartley Withers concludes that it has been shown that ordinary shares have advantages which make it impossible to regard them as necessarily so speculative that we ought to feel 'rather ashamed of possessing them'.<sup>1</sup>

Contributing subscribers to the National Bureau of Economic Research, New York, approve the development of 'an organization devoted to exact and impartial investigations in the field of economic, social, and industrial science'. The most recent inquiry is entitled *Business Annals* and gives an analysis of business records of seventeen countries for periods ranging in some instances as far back as 1790. In many cases these are based on description, in others on statistics. A conspectus of business cycles is possible, giving five conventional values

<sup>1</sup> *Hints about Investments*, Ch. XII.

(with diagram shading) for prosperity, recession, depression, revival, and war activity. When the years are arranged vertically and the countries horizontally, the differing shade values appear as irregular but definite vertical ribbons. Some of the inferences drawn may be quoted:

'As one would expect from England's position in international trade and finance, English cycles are more highly correlated with the cycles of other countries, than the cycles of other countries are correlated with each other. The closest agreements are found between English and French or English and German cycles; the loosest agreements are between Austrian and American cycles. . . .

'Thus no country in our list has had fewer business cycles since 1890 than the international pattern calls for; but the majority of countries have had one or two more than that number. These additional cycles seldom result from failure to participate in the international movements of activity and depression, but rather from the intercalation of what we may call domestic recessions between the dates of international recessions. . . .

'Whatever the causes of the recurrent fluctuations in economic activity may be, the annals suggest that these causes become active in all communities where there has developed an economic organization approximating to that of Western Europe. There appears to be a rough parallelism between the stage attained in the evolution of this organization by different countries, and the prominence of business cycles as a factor in their fortunes. . . .

'One characteristic of the type of organization in question is the wide area over which it integrates and co-ordinates economic activities. Bare as they are and short their span, the annals reveal a secular trend toward territorial expansion of business relations and a concomitant trend toward economic unity. . . .

A third example of a time-series test of theory is one I gave recently to show the correlation between fluctuations in real wages, unemployment, and aggregate produc-

tion.<sup>1</sup> My feeling has always been that one can only attempt to secure a real wage higher than the total production justifies by letting the strongest and best organized workers present their claim first and thus leave those who are in a weaker strategic position to 'come to the bag' and find nothing there—in other words, by suffering considerable unemployment in the unsheltered trades. This view finds simple but striking confirmation in a study made by Professor Rueff, of the University of Paris, published recently in the *Revue Politique et Parlementaire*.

For the seven years 1919 to 1925 there is an almost complete correspondence between the fluctuations of real wages—that is, nominal wages, divided by the level of prices to show what those wages will purchase and the trade union percentages of unemployed. It is striking to the eye in the exact parallel of the fluctuating curves, and still more remarkable expressed statistically. The degree of consilience may best be seen from the coefficient of correlation, which I compute as approximately + 0.95 (complete direct correlation being statistically known as + 1.0).

*The Statist* observes that 'when prices fall more rapidly than money wages, unemployment increases'. At such a time, of course, real wages tend to be higher. 'When money wages tend to fall relatively to price movements, the unemployment curve shows a corresponding fall', or, in other words, at times when business is improving and employment increasing, prices are rising faster than wages and real wages tend to be falling. Now this is common theoretical knowledge, which hitherto has not had exact practical demonstration. It is the instantaneous character and continuity of the connexion that is remarkable. It is then impossible for business to improve and bring with it at the same time an improved real wage? The answer depends entirely upon the aggregate production and average output.

If business has become bad, with unemployment, be-

<sup>1</sup> *Financial Times, Special Supplement*, 15 March 1926.

cause the real wage rate that it is attempting to pay has exceeded the real wage rate that is being produced, then it can only become better if these two are adjusted. But if that rise in prices which is partly the inducement to, and partly the effect of, better business, automatically brings about a rise in money wage rates per hour (by adjustment on a cost-of-living basis) without any corresponding increase in output per hour, adjustment is farther off than ever. The only true indication of the valid conditions under which a higher real wage is possible is a sensitive and complete Index of Production on national lines. In the absence of such an official index, I have taken the (private) quarterly production index produced by the London and Cambridge Economic Service, which is the best indication of production we possess, and, finding the extent to which each quarter's figure has fallen below the 100 per cent. pre-war level, obtained what may be called an index of short output. I have plotted the results as a third line upon *The Statist* graph referred to above. Again, the agreement in detailed fluctuation is most remarkable and significant.

At the moment when output is least the rate of real wages is highest, and therefore the number who can be in receipt of such wages is greatly reduced, and increased unemployment is an essential corollary. At the times when output is improving the rate of real wages per person tends to be going down, and, therefore, the total number who can be paid such a wage is increasing. I need not refer to the monetary causes which are probably responsible for the simultaneous movement of all these lines, but I would urge that the pre-war cost-of-living fetish, which may work for or against the worker, according to circumstances, has in the past seven years obscured the exact relationship between the standard of life and the standard of output.

In an address to professional accountants on 'Economics as an Exact Science' <sup>1</sup> I dealt with the possibility of

<sup>1</sup> *Current Problems in Finance*, I.

confidential team work on the business results passing through their hands, and sketched some of the theoretical problems to be illustrated or investigated.

'All students of the subject, and even casual readers, are now familiar with the conception of the "marginal business" which has been derived by deductive reasoning from the generalized conception of the principle of rent, and marginal utility. We know, in the abstract, that price is the figure at which the marginal business can just get a bare return upon the factors of production without going out of production, just as it also tends to be in equilibrium the cost of the last unit of production of other businesses, but I am not going to labour all the consequences of this theory. What I want to point out is that it has received little or no practical verification and examination in practice, though one may feel instinctively that it is true. The verification of it, and the discovery of its consequences in practice rest entirely in the realm of accountancy. Suppose we took all the concerns that are on the margin—i.e. are making an economic rent on their employed capital but no profits above that—and then we take all those in the same trade or industry that are making something more than this rent—i.e. the intra-marginal concerns. Their results could be reduced to a unit cost of production if necessary. In some we should find the amount charged for economic return upon capital higher than in others possibly because the factory was erected in a comparatively disadvantageous position and the costs of erection were unduly high, or the lay-out was not good. Others will be influenced by a disadvantageous position, as shown by transport charges in the revenue account; others by disadvantages in distribution; others by excessive salaries, less efficient machinery, and so on. None of us has the slightest knowledge as to how the differentials of profit for profitable concerns, as compared with marginal concerns, are made up and classified, and to which elements of advantage most profits are due. . . .

'Can any of us at present hazard a guess as to how many marginal businesses there are in a trade, and as to how far profits over and above economic interest arise through disadvantages in situation, excessive capital expenditure, excessive distribution costs, and inferior organization respectively?'

## VII. *Examples—relations between business factors.*

I will now take illustrations closely bearing upon these questions. A very excellent example, first, of what is possible in the field of definite investigation and, secondly, as to the practical position reached in the United States, is the work that is being done in the School of Commerce of the North-Western University under the direction of Professor Secrist. It is impossible for me to give you in detail the various monographs, but I will refer to one or two in particular. The inquiry into the expenses, profits, and losses in meat stores brings together 143 businesses rendering uniform monthly statements. Apart altogether from the valuable statistical constants and limits that are reached, the following conclusions of theoretical interest, which would not be obvious by the light of nature, emerge. The typical or modal ratio of costs to sales ranges from 73 to 78 per cent. The ratio of cost increases as stores increase in size. It is lower for stores making profits than for those incurring losses. Stores making a profit have higher margins than those suffering a loss, i.e. the former 23.85 and the latter 18.89 per cent. of sales. But it has to be noted that moderate margins are much more conducive to the realizations of net profits than those margins which are high. This conclusion might not easily be reached by purely deductive reasoning. The investigation brings out the main causes of high gross margins and low gross margins respectively. It confirms the view that stores which make a profit have lower operating expenses in terms of sales than those which experience loss. There

is a definite relation between the size of the store and the chance of making a net trading profit, proceeding as follows :

One man stores	0.1	net trading profit	per cent. on sales.
Two men stores	2.8	"	" " "
Three men stores	4	"	" " "
Four men or larger	5	"	" " "

In the larger ones losses are rarely incurred. Losses are much more common as the size gets gradually less. Inadequate margins are more responsible for losses than high operating expenses. Profits are most likely to be made when both the margin and expenses are moderate in amount. The single mass explanation of failure to make a reasonable profit is unintelligent competition resulting from poor location, lack of record showing true cost, and easy entrance into the trade.

Another study on *Commercial Rent as an Expense and its Relation to Profits* relates rent to sales and to total expense, and uses the coefficient of correlation, of dispersion and skewness. From 1914 to 1919 for one group and from 1916 to 1920 for another, the precise 'diminution' of the rent element is brought out. When classified by size alone the rent element does not decrease regularly with increasing size of stores, that is, in relation to sales, but in relation to total expenses it does decrease regularly. Surprisingly uniform and persistent tendencies for profits to decrease as rentals increase have been discovered. This must not be taken as necessarily cause and effect. The association is clear, but which is cause and which is effect must be brought out by abstract reasoning.

The study of a seven years' view of the sales and expenses of retail clothiers is an excellent illustration of the problems raised by diminishing costs. Decreases in sales are almost certain to accompany increases in expense in terms of sales. Again, decreases in sales are more certain to increase costs than are increases in sales to reduce them. Both decreases and increases in sales

bring less proportional change in the cost of doing business in large than in small cities.

In another investigation the well-known concept 'representative firm' is discussed with special reference to Marshall's use of the term. Other writers have followed Marshall in the general discussion of this concept—for example, Professor Taussig—but, so far as is known, no attempt has been made by anybody from experience quantitatively to describe the cost conditions of such firms, nor to identify the firms individually. In the whole of the investigation very strong modal types emerge, and there are some striking similarities to the results which the investigations of Mr. W. H. Coates in this country have shown. It was clear that the representative cost area on a five-year basis extended to positions 20 per cent. on either side adjacent to the average, but the actual *identity* of the so-called representative firms, to some extent, changes from year to year. Long run representative costs may be clearly defined and stated, and there is a tendency for them to conform to average costs in the modal area, which is a position somewhat below average costs as trade is now conducted. They conclude that the existence of the 'rent of business ability persistently makes for equilibrium at low-cost positions'. This particular study deals direct with the theoretical concepts of normal prices, marginal costs, &c.

One of the most striking investigations for the purpose of testing economic theory in recent times in this country is that given by Mr. W. H. Coates before the Colwyn Committee on National Debt and Taxation, whose report with this evidence will shortly be published. While holding the post of the Director of Statistics and Intelligence at the Inland Revenue Department, he made an investigation to deal direct with the vexed question of the Incidence of Income-Tax. As you know, economists are practically agreed that, by its very nature, income-tax stays where it is put, and cannot be thrown off in price. If there is no profit there is no tax, and, therefore,



tax cannot form part of the costs of production. But many business men hold strongly the contrary view, and say that a tax on income, like a tax on goods, is diffused over the whole community by means of higher prices. It is obvious that this is an absolutely crucial question in Public Finance and social theory. Mr. Coates took the relation between Turnover and Profit for a very large number of concerns for different years, and extracted from them all the usual statistical constants. He put two main questions for the application of this test.

'First, were typical industries of the country mainly in the hands of representative concerns, whose normal cost of production determines price, so that, by including income-tax in that cost, they could pass on the tax in price, as the business school contends, or, on the contrary, were they in the hands of concerns whose varying circumstances and ability show, in competitive conditions, results varying from losses, or small profits, up to large profits per unit of business? Secondly, how did the rate of profit secured by industry under conditions of high taxation compare with the rate of profit secured under conditions of low taxation? If the contention that high taxation is passed on to the consumer were correct, it seemed clear that the rate of profit (before payment of the tax) would need to be considerably higher when taxation was high than when it was low.'

The first of these questions was answered clearly in the negative. The analysis for each trade group, for each period, showed the familiar distribution common to practically all natural phenomena when treated statistically. 'The practical existence of the marginal concerns postulated by economic doctrine was clearly shown.' In every case there was a material portion of the total business being done at a loss or no profit.

The second question, whether a proportionately higher rate of profit was secured during a period of high taxation, than that obtained in a period of low taxation, was also answered by the statistics in the negative. The

standard rate of tax in 1922-3 was 4.28 times the amount of the rate in 1912-13. If business in 1912-13 yielded a profit of  $Y$  per cent., and that percentage included the tax which had to be paid thereon, then on the theory that those in control of the business will not carry on unless they receive the same net reward for their efforts during the period of much higher taxation, when the rate of tax was 5s. in the £ the gross rate of profit on turnover would require to rise to 1.25 times  $Y$  per cent., or an increase on the original return of 23/90ths of its value. This increase, to be effective in its object, would require to be realized throughout the whole range of the dispersion, and the tables and graphs presented in the memorandum offer no evidence in support of any such increase. On the contrary, the earnings of the industry for these seven groups as a whole, before payment or deduction of income-tax, when related to the unit of the pound of turnover are practically the same in 1912-13 and 1922-3, notwithstanding the increase in the standard rate of income-tax by 328 per cent.

The whole of a large inquiry which I have very briefly summarized, is worthy of close study by economists and statisticians.

### VIII. *The Curriculum of the University.*

As raw material accumulates and as detailed results are co-ordinated, with recognized apparatus and outside support, the new students who are to advance the science must be increasingly directed along the appropriate lines if they are to help in establishing economics as a quantitative science. 'In so far as they accomplish this aim', says Mitchell, 'they will in transforming the subject make obsolete not only the qualitative work of Dr. Marshall and others, but also the crude beginnings of quantitative work which their elders are producing.'

He foresees that the new literature will be numberless papers and monographs, and books will pass out of date more rapidly. No one will get the prestige of Mill and

Marshall, for no one will cover the whole field. The detail that will pour into the research students' library will gradually make it more akin to physical investigation, save that the tests must be repeated from time to time, as no permanence of data in economics is possible. But these, instead of being mere checks *superseding* the past checks, will have a dynamic relation to them, and this change in itself must be the subject of analysis and theorizing, which will again enrich the science on its abstract side.

Moreover, each investigation is a double check upon a particular logical weakness of pure analysis. We cannot think of the result of motives in the mass without individualizing those motives first, and we know how easy it is to go fatally wrong in passing from the particular to the general. It is here that statistical analysis gives a backward check upon all such aggregations of imaginary individuals, each of whom has a slightly different monetary measure of a particular unitary satisfaction or effort, and each of whom responds differently to a slight change, which is even more important.

There is a third way in which the analogy of physics or chemistry must be inexact. The elements behave in the same way whether the human mind is studying them or not, whereas economic tendencies and principles change *because* they are being studied. Obviously so much in them as depends on human psychology, knowledge, or will, undergoes profound change as the human mind becomes conscious of what it is doing. As soon as we have completely discovered and understand what governs the business cycle it will probably almost cease to exist.

How does this University stand towards such a movement as I have illustrated and outlined? I think the syllabus hardly concedes that radical changes are possible or desirable, still less that we should equip students to take part in them. I should be the last to say that somewhere in this country economics should not be studied in particular juxtaposition to history and philosophy, as it is studied in association with mathematics and science in

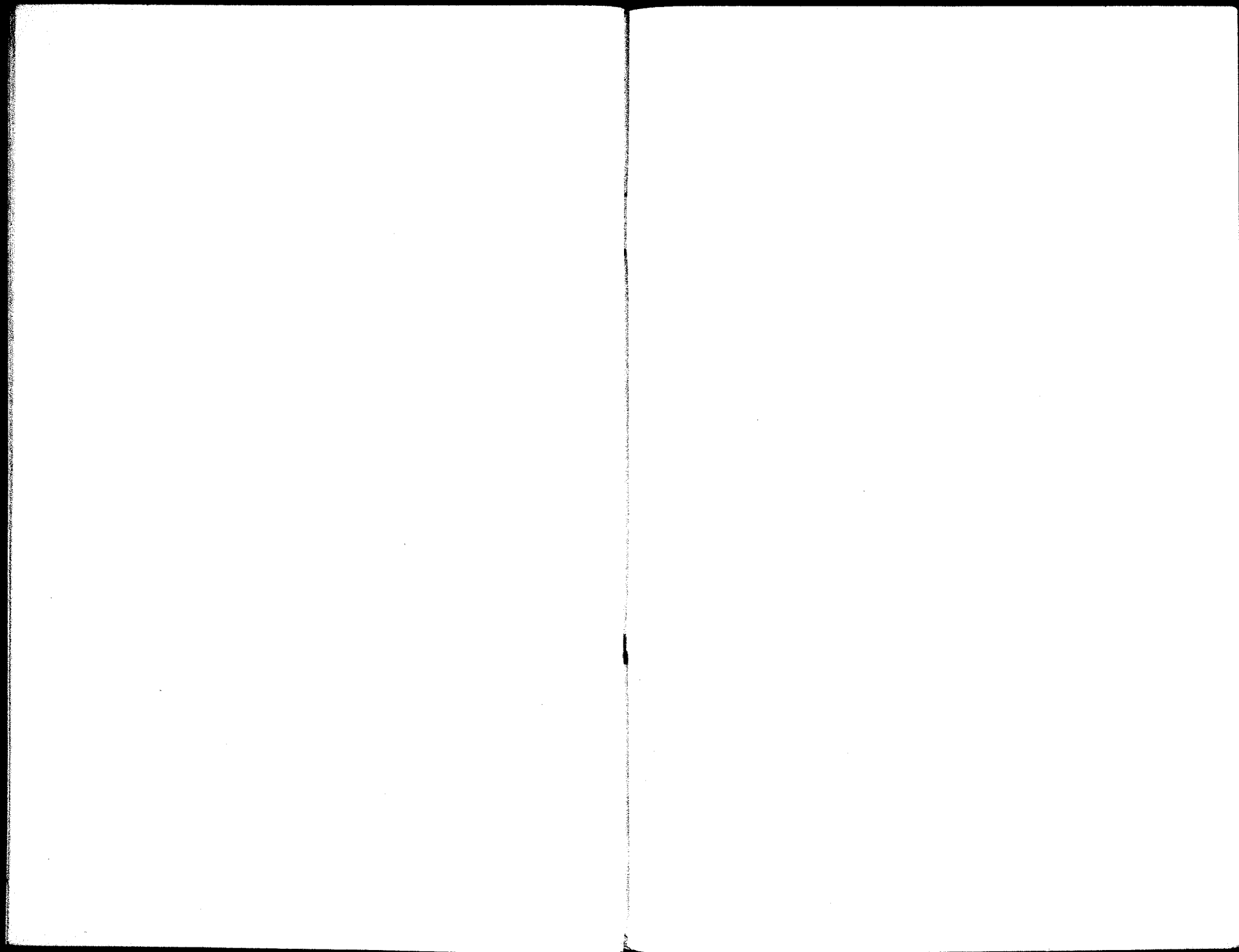
Cambridge, or with administration and commerce in London, and I agree that Oxford is the fittest home for that study. But do we not seem inclined to treat it like a closed philosophical system with Adam Smith as Aristotle, and Ricardo as Plato? An outsider might gain the impression from the curriculum, with the most recent of the set books mentioned therein a work nearly sixty years old, and the latest *date* actually mentioned that given in the heading, 'Labour movements from 1815 to 1895', that it is not respectable to bring economics down to the problems of to-day. Doubtless that impression is erroneous, in practice, but it is abundantly clear that the desirability of some training in published statistical data and in technique is not recognized. If this means that we in Oxford desire to take no part in advancing economic science, and are content with giving a liberal education in past history or modes of thought, it will suffice. But let that be frankly recognized. If, however, we desire to teach a living subject, and to make economists with the practical touch, and not mere historians of economic thought, if, indeed, we are to be really fair to the vast mental energies whose direction is entrusted to us, it will be necessary to give some thought to the new era of economic effort ahead of us.

Not much is needed in fact to give point to the present effort. A full chair in Statistics is not necessary, and if every student is required to take a course in elementary statistical methods, including correlation—without any necessary mastery of the mathematical principles underlying them—the case will be sufficiently met, though of course it is desirable to have available facilities for some more advanced work if possible. The truth is that, without some such equipment, no student will really be an 'economist' in the sense which that term will soon come to bear.

Chaucer, for the twentieth century, will stand:

'In everything, I wot, ther lyeth mesure.'

(*Troilus*, ii. 715.)



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