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Lange and Keynes

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Lange and Keynes

Jan Toporowski[1]

Abstract

Lange engaged with Keynes in the matter of Keynes's macroeconomics, and over econometrics. Lange's interpretation of the Keynesian macroeconomic model laid the foundation for the neo-classical synthesis. But it was limited in its monetary economics. Lange also intervened in the Keynes-Tinbergen dispute over the scope and significance of econometrics. Lange collaborated in a paper that conceded Keynes's main point that empirical investigation could not determine theory, but insisted that theories could nevertheless be tested empirically. The paper was never publish and Keynes, unconvinced, planned to recruit Kalecki to his critique of econometrics.

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Oskar Lange was unusual among Marxists in his openness to the ideas of non-Marxist economists and in his willingness to engage with their analysis. Among those non-Marxist economists was John Maynard Keynes. Lange became one of the key interpreters of Keynes's enigmatic General Theory of Employment, Interest and Money, and one of the founders of the Neo-Classical Synthesis of Keynesian and pre-Keynesian ideas, that was to dominate macroeconomics for thirty years after the death of Keynes. Lange, like Hicks and Samuelson, attempted to adapt Keynes's ideas to his (Lange's) own pre-Keynesian notions of money, saving and investment. This is shown in the first part of this chapter, which discusses Lange's interpretation of Keynes's General Theory and the concept of money and interest within which Lange framed Keynesian macroeconomics. Significantly, however, Lange chose to confront Keynes not over his macroeconomics but over Keynes's critique of econometrics.

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1. The interpretation of Keynesian macroeconomics

The publication of Keynes's *General Theory*, arguably the first fully worked out theory integrating money, finance and economic activity, caused Lange to revise his own views on money and the rate of interest. However, his response was perhaps more to develop his theory to accommodate Keynes's own analysis, rather than to adopt Keynes's theoretical and philosophical precepts, bearing out Willard Quine's view that we do not change our minds as much as we adapt our preconceptions. In 1938 Lange published a paper in *Economica* on 'The Rate of Interest and the Optimum Propensity to Consume', in which he tried to show that Keynes's theory was a special case of a Walrasian general equilibrium. Lange argued that the rate of interest is determined in the *real* economy, this being now defined as the relationship between desired *real* money balances, and real consumption and investment, all measured in wage units. He seems to have held an essentially Ricardian conception of money and banking, what Schumpeter called a 'monetary theory of credit', in which money is a means of exchange, and banking consists solely of the intermediation of saving.

In his paper, Lange argued that the rate of interest still tended to become equal to the marginal productivity of capital (as he interpreted Keynes's marginal efficiency of capital). Liquidity preference came into the analysis because the rate of interest on real money balances was determined by real income and the supply of money. Equilibrium is achieved when the rate of interest with a given real income and liquidity preference function is consistent with the rate of interest that gives a particular excess of real income over real consumption, and real investment that combines with consumption into that given real income.

Lange distinguished two specific cases of this general model, which he argued could be inferred from Walras. One was what he called the 'traditional' view, in which the demand for real cash balances is inelastic with respect to the rate of interest, and therefore the demand for money is purely determined by real income. The other was the case he attributed to Keynes, in which changes in the marginal efficiency of investment and in the propensity to consume do not affect the rate of interest. Lange concluded by arguing that his more general, Walrasian, theory is preferable because it can be used to give an 'optimum' level of saving (the corollary of the 'optimum propensity to consume' of the paper's title, that does not depress investment through either inadequate saving, causing a higher rate of interest, or excessive saving, causing under-consumption.) In Keynes's case, according to Lange, consumption does not affect the rate of interest, so that the optimum consumption is that level of consumption at which the marginal efficiency of capital (in the sense of marginal capital productivity) does not rise any more. This is where the elasticity of supply of al factors of production falls to zero, and any further expenditure on consumption raises prices but not output. In the 'traditional case' any decrease in consumption and increase in saving reduces the rate of interest and stimulates investment, so that saving can never be excessive. For the general case the 'optimum' propensity to save is defined by, among others, a given quantity of money. Over-saving can therefore be counteracted by loosening monetary policy.

In 'The Rate of Interest and the Optimum Propensity to Consume' Lange used Walrasian general equilibrium to define his variables and establish the relationship between them. But he was sufficiently 'Austrian' to believe that this general equilibrium does not exist in the real world. He used his general system of equations also to show how if the rate of interest was not the equilibrium rate, the total of real consumption and real investment would change the liquidity preference function to give a new rate of interest. This would give a new level of consumption and investment. The resulting change in total income would give a new liquidity preference function, at the given money supply. 'This process of *mutual adjustment* goes on …until equilibrium is attained.' In a footnote he pointed out that if lags are involved then cyclical fluctuations rather than equilibrium would ensue, and gave Kalecki's business cycle theory, as expounded in the latter's 'A Theory of the Business Cycle' which had been published in the *Review of Economic Studies* (Kalecki 1937) as an example. (Kalecki was not impressed. In a revised version of that paper, he explicitly referred to Lange, along with Hicks and Meade, as obtaining spurious Keynesian equilibrium positions because they did not distinguish between investment and investment *decisions*, which cause lags, and because they ignored the effect of investment on the capital stock. [Kalecki 1939, pp. 139-140]). In his later review of Schumpeter's *Business Cycles*, Lange explicitly criticised the latter's presumption, in the course of Schumpeter's criticism of Kalecki, that the rate of interest would automatically bring about an equilibrium between saving and investment. Furthermore, he argued that the existing monetary policy would fail to make interest rates sufficiently flexible, in the face of uncertainty and inelastic expectations (Lange 1941).

The view of Keynesian macroeconomics that Oskar Lange put forward view is essentially pre-Keynesian. Pre-Keynesian here does not mean published prior to Keynes' *General Theory*, or in some sense failing to come to terms with some essential Keynesian innovation. Rather it indicates a 'monetary theory of credit' and a notion of interest that is rooted in some excess of the value of real production over its cost, in a capitalist system of production. This may be contrasted with a 'pure monetary' theory in which interest is derived from relations in money and financial markets. Whether this was Keynes's fundamental innovation in monetary economics is debateable. ('Keynes… in intent at least, established a monetary theory of interest, according to which interest is not derived from, or expressive of, anything that has, in whatever form, to do with the net return on capital goods.' Schumpter in his *History of Economic Analysis* 1954, p. 1178). What is indisputable is that in market capitalist economies from the late nineteenth century onwards, the rate of interest has been determined in financial markets, rather than in and among firms engaged in the production of goods and services. In 'The Rate of Interest and the Optimum Propensity to Consume', Lange came close to a pure monetary theory of interest, but concluded that it could only be temporary if it was not reinforced by an equivalent marginal productivity of capital. This perhaps reveals his reading of Wicksell. Lange moved beyond the general equilibrium categories that he used in his macroeconomics, to allow that a disequilibrium rate of interest could be a key factor in economic dynamics.

Keynes called the approach to macroeconomics, in which business cycles are driven by differences between saving and investment, 'neo-classical' and indicated his dissent from it (Keynes 1936, p. 177). However, Lange's interpretation was taken up by Dennis Robertson, in a note which he sent to the *Economic Journal* entitled 'Mr. Keynes on "Finance"'. Keynes, as editor of the *Economic Journal* sent the note for comment to Austen Robinson, with a request that Austen or his wife Joan Robinson might care to comment. Joan Robinson responded in a letter to Keynes dated the 23 March 1938 in which she wrote: 'Dennis appeals to Lange. Lange's article tho' silly is formally quite correct, and if Dennis really accepts his argument he has given away everything.' (Keynes *Collected Writings* Vol. XXIX, pp. 168-9).

2. Differences over Econometrics

The dispute that John Maynard Keynes conducted in 1939 with Jan Tinbergen over the scope and significance of quantitative economics is well known. Less well known is the criticism of

Keynes's position that was put forward by Kalecki's friend Oskar Lange together with Jacob Marschak, who had championed Kalecki in Oxford. Much more obscure is the reaction of Oskar Lange to that dispute. Research has now shown that as Kalecki was leaving Cambridge for Oxford, Keynes was urging that Kalecki undertake a full technical criticism of Tinbergen's work. Oskar Lange was already known for his Walrasian formalisation of Keynes's *General Theory* in his paper 'The Rate of Interest and the Optimum Propensity to Consume'. He was an accomplished statistician and a leading figure in the trend towards formalism and mathematics in academic economics. Moreover, from his Marxist background he derived a strong interest in economic philosophy that was to form the basis of the reasoned critique of Keynes's views on method. Following the publication of Keynes's review of Tinbergen's *A Method and Its Application to Investment Activity* in the *Economic Journal* in September 1939, Lange and Jacob Marschak decided to write a response to Keynes' criticisms of Tinbergen. A seventeen-page typescript emerged, dated 1939, under their joint authorship and the title 'Mr. Keynes on the Statistical Verification of Business Cycles Theories'. Keynes appears not to have thought highly of their joint critique. But he was clearly keen to put more technical expertise behind his critique of Tinbergen. As Kalecki's research at Cambridge came to an end, Keynes urged him to turn his critical attention to Tinbergen's work. Kalecki, who was close to Lange and Marschak, did not take up this suggestion.

According to the author of the editorial notes in volume five of the Collected Works of Oskar Lange, the joint paper was sent to Keynes with a view to publication in the *Economic Journal* (Ulatowska 1976, p. 1072). However, in the meantime, Tinbergen responded to Keynes's review of his book. That response was published in the *Journal* together with Keynes's reply to Tinbergen's note. Possibly with a sense that the issues between Keynes and Tinbergen had been dealt with in the published exchange between them, Lange and Marschak refrained from seeking publication of their paper. A typescript corrected in hand by Lange is among Lange's papers (ibid.). A summary of this paper provided by Irena Ulatowska in the editorial notes in volume five of the Collected Works of Oskar Lange. David Hendry and Mary Morgan published the draft of the paper that Mary Morgan found among the papers of Jacob Marschak in 1995 (Hendry and Morgan 1995). However, since Lange was the corresponding author of the paper, Hendry and Morgan did not have access to the correspondence with Keynes over the paper, and the version in the Lange archives contains Lange's small but significant corrections to the paper.

There is no mention of Marschak and Lange's paper in the *Collected Writings* of Keynes. The only mention of Lange are references to his paper 'The Rate of Interest and the Optimum Propensity to Consume'. There are only two rather curious references to Jacob Marschak in the *Collected Writings*, both of them in Volume XIV. The first appears in a letter to Keynes dated 31 August 1938 from Alexander Loveday at the League of Nations. The letter was written further to his original request to Keynes for a review of Tinbergen's book. Loveday wrote:

'I think that most of the questions you have raised (on statistical methodology in an earlier letter to Loveday - JT) are those which a number of us have had in mind throughout the whole course of Tinbergen's labours and for this very reason I had in fact arranged for two or three meetings of economists and statisticians to discuss the whole matter. Also, Dennis Robertson has been good enough to spend a good deal of time in advising Tinbergen, although he is of course not responsible for the results. I think the net result of the meetings, which have included such persons as Bowley, Marshak, Anderson, Harrod and a number of other economists, has been that we ought certainly to go ahead with the work, although probably each one of them had reservations on this or that point – possibly the statisticians less than the economists.' (Moggridge 1973, p. 290).

Marschak, like Lange, was a statistician as well as an economist. Roy Harrod had no doubt as to the camp in which Marschak belonged. In an earlier letter to Keynes, dated 6 July 1938, Harrod had expressed his irritation at Tinbergen's suggestion that 'the facts' do not support the 'acceleration principle' which was a feature of Harrod's dynamic theory. Harrod commented further:

'We have a sort of minor Tinbergen here in the form of Marschak… He tells (me) that my theoretical work is entirely divorced from the facts. He himself is content to with what you once called wisecracks, very good ones I think. But he happens to be a shrewd person.' (Moggridge 1973, p. 298). In fact, Marschak was at that time the Director of the Oxford Institute of Statistics. His stay in the United States, on prolonged leave of absence from the Institute during 1939, not only provided the opportunity for him to collaborate with Lange on a response to Keynes's attack on Tinbergen. It also set in train a series of changes at the Oxford Institute that led to the appointment there of Lange's compatriot, Michał Kalecki.

Keynes's critique of Tinbergen is too well-known to warrant any more than the briefest summary here. Keynes argued that statistical methods cannot disprove theories, because there will always be a possibility that particular correlation tests were wrongly specified. Furthermore, such methods cannot take into account non-quantitative factors, and they rely on the independence of causal quantitative variables. He criticised Tinbergen for using linear functions in his study of investment. Such functions, in Keynes's view, were an inappropriate foundation for recurrent business cycles. Tinbergen's choice of time lags and trends was arbitrary, and he failed to allow for structural changes, such as innovations or changes in tastes, in the economies he was studying (Keynes 1939).

2.1 The Lange-Marschak Critique of Keynes

Lange and Marschak commenced by pointing out the most obvious flaw in Keynes's argument, namely that he have reviewed on the first volume of the Tinbergen study, which should have been considered together with its second volume. Lange and Marschak would not accept that statistical methods cannot disprove theories. They gave as an example Hayek's theory of the trade cycle. This postulates that the capital-intensity of production rises during economic booms, and falls in economic depressions. Kaldor had shown in his paper 'Capital Intensity and the Trade Cycle', published in *Economica* in February 1939, that in fact the reverse is the case: Capital intensity falls during a boom and rises in a depression. According to Lange and Marschak, Keynes himself used such methods (Ulatowska 1976 pp. 680-681). Kaldor in fact did not use any statistical proof in his paper, but used logical, theoretical analysis taking into account various arguments on the subject. In a review of Schumpeter's monumental *Business Cycles*, which Lange wrote shortly afterwards, he referred to 'confronting some implications of the theories of Mr. Kalecki or Mr. Kaldor with the facts. Both theories imply net disinvestments of capital during the depression. As far as our statistical knowledge goes such disinvestments does not happen as a rule (an exception: the USA in 1931-35).' (Lange 1941).

Lange and Marschak agreed with Keynes that non-quantitative factors also influenced the movement of variables. But they suggested, given a proper understanding of such qualitative factors, quantitative techniques could still be appropriate. Very often, they suggested, such variables can be proxied by binary variables, or indices. Indeed, they argued that it was essential to understand non-quantitative factors because such an understanding can then indicate where a correlation is affected by two variables off-setting each other's influence on some dependent variable. In other words, the statistical investigator had to have an understanding of the *economic* significance of variables in order then to be able show their statistical correlations. These would often be revealed in partial correlations between variables. From this point of view, Lange and Marschak argued that Tinbergen had very carefully established the economic significance of his correlations. Structural changes can be accommodated by appropriate and careful manipulation of data, although they conceded that, where these changes caused major shifts in variables, this can cause difficulty.

Lange and Marschak also pointed out the obvious flaw in Keynes's assertion that linear equations could not result in cyclical movements of variables. This was a point which Tinbergen had rebutted, pointing among other examples to Kalecki's business cycle models based on linear functions. However, the example Lange and Marschak proposed was a simple 'cobweb' model using linear supply and demand functions to produce cyclical equilibrium price movements.

Regarding the choice of lag periods and trends, Lange and Marschak accepted that this was a problem. But they argued that the treatment of time in general is a common difficulty of all empirical work. In many cases, the investigator has to rely on 'additional sources of information', often qualitative data or induction from other sources. This may not provide definitive answers, but it limits the conclusions that may be drawn from *a priori* systems of analysis. They agreed with Keynes that further analysis of sub-periods was called for by Tinbergen's data. They also agreed with his view that correlation analysis cannot pick up the influence of factors which do not change in the period under analysis. But they argued that this is well-known in economics. The geographic and historical limitations of conclusions is recognised when making empirical observations.

Lange was also taken by Keynes's suggestion that the constancy over time of statistical relationships may vary, and that therefore date over a period may need to be broken down into sub-periods to check on the stability of coefficients between the sub-periods. Marschak was less taken by this suggestion, adding a marginal note that 'this is not new. I think it is too kind to Mr. Keynes.' (Hendry and Morgan, 1995 p. 397.) Lange pointed out that such methods had been first introduced by the German political economist and statistician

Wilhelm Lexis. They had been developed in the analysis of variance since his Lexis' death in 1914.

Lange then pointed out much broader and more important limitations of statistical methods arising from the issue of variance. He linked these limitation clearly with his own Marxist approach to the scope and methods of economic theory. In the first place:
'…nothing can be done to investigate the influence of factors which remain relatively constant during the period under consideration. But this limitation applies to <u>any</u> economic proposition whether formulated statistically or not. Therefore all empirical generalizations are bound to have a historically and geographically limited validity… Their validity is limited historically and geographically by a number of factors such as given social institutions, given motivations of behaviour (e.g., entrepreneurs aiming at maximum money profit, and not at carrying out certain rules directed towards the maximization of social welfare, as they would under Socialism), etc. It is only within the framework of given and constant social institutions and historically conditioned patterns of behaviour that most of the "laws" of economic theory are valid. Even the validity of natural laws is subject to the restriction "other things being constant". The historical character of the empirical material of the social sciences subjects the regularities discoverable in the social world to much narrower limits of time and space within which they hold…in no case need this difference (between the natural and social sciences – JT) lead us into giving up the very attempt to narrow down, by empirical study, the bewildering variety of theories, all equally plausible on <u>a priori</u> grounds. The statistical methods of such verification may be crude and as yet unsatisfactory, but it is by a refinement of these methods and by their coordination with theoretical analysis rather than by their wholesale condemnation that we can expect to contribute to the further development of economic knowledge.' (The quotation is from the version of the paper found, together with his correspondence with Keynes, among the Lange papers in the archives of the Polish Academy of Sciences, file number PAN, the Polish initials of the Polish Academy of Sciences, III-309/29. It may be found on p. 397 in the Hendry and Morgan edition.)

There was one other condition necessary, according to Marschak and Lange, that a theory requires. This is that it needs to be formulated 'so as to show that it is neither over nor underdeterminate (loc. cit, Morgan and Hendry, 1995 p. 398). This would obviously limit economic 'laws' to such situations in which a determinate system of statistical relations could be obtained. But then such 'laws' would also limited by the choice of variables, and the possibility that a different, or extended, set of variables may give different 'laws' applicable over different time-spans, incorporating the shorter time-spans in which some initial 'laws' obtained.

Thus, Lange and Marschak accepted the validity of many of Keynes's comments, insofar as they limited the inferences that may be drawn from statistical studies. Although current methods of empirical verification were still primitive, they argued that progress in economics would come from improving statistical methods, and coordinating them with theoretical analysis, rather than rejecting them. Lange was absolutely convinced that empirical study could eventually sort out those theories which are valid, from those which have no basis in the real world. In his review of Schumpeter's *Business Cycles*, he concluded:

'The choice …(between various business cycle theories) …can be made only on the basis of empirical investigation. It is necessary to find the concrete functions involved and their parameters, then to investigate what periods, amplitudes. Damping etc. are to be expected from the different theories, and to confront these expected values with empirical data. Only in this way is it possible to choose the "true" theory from among those theoretically admissible. It is possible, even likely, that the "true" theory will prove more complex and will have to combine elements of the different *a priori* theories developed (this is suggested by Professor Tinbergen's work and has also been made clear by Professor Haberler).'(Lange 1941, p. 193).

2.2 Keynes's response

Lange sent off their joint paper from the University of Chicago to Keynes on the 15 February. (The covering letter is among the Lange correspondence deposited in PAN III-309/22). In the absence of a reply from Keynes, on the 30 April, Lange wrote again to check 'that the article did not become lost in the mail.'(PAN III-309/22). In fact Keynes had received the article, and had written back on the 10 April an extensive reply to Lange.

In the first place, Keynes pointed out that he now had 'an extensive rejoinder' from Tinbergen, which had reached Keynes before the Lange-Marschak paper had arrived. Since Tinbergen's response was going to be published, Keynes had an editorial decision to make about whether also to publish the Lange-Marschak rejoinder. He referred the matter to Arthur

Pigou, the President of the Royal Economics Society, which publishes the *Economic Journal*. Pigou advised that, in view of Tinbergen's response, which was to be published, the Lange-Marschak paper should not be published. Nevertheless, Keynes felt reluctant to close a discussion that he felt 'is an extremely important one.' He therefore suggested that the co-authors should read Tinbergen's response, and then consider if there were any 'special points…not dealt with by him which you would like to pick out for special emphasis.' Keynes suggested that he might find room for a four or five page manuscript on those points. In any case, the co-authors would have to review their arguments in the light of Tinbergen's own response.

On the substantive issues raised by Lange and Marschak, Keynes had three points to make. First of all, he denied arguing that a business cycle theory cannot be tested statistically. He was, he said, 'dealing solely with Tinbergen's very special method of analysis.' (In a return to the characteristically waspish rhetoric that had marked his attack on Tinbergen, Keynes concluded this paragraph with the remark: 'I emphasis this because, whilst it is sometimes useful to have a controversy about something one has said, it can never be of interest to the general public to have a controversy about something which the author himself does not admit to having said and which, however that may be, he certainly does not believe.' PAN III-309/22).

Keynes then grudgingly conceded that it may be possible to obtain cyclical variation from linear functions. ('I think it very possible indeed that there may be something in what you say.') But he could not resist pointing out to Lange that Lange and Marschak had made their case using merely one example, the 'cobweb' price model, and this example could give perverse results. Calculating the price series backwards, according to the formula given by Lange and Marschak, would give a negative price. This obviously indicates that the model is incorrect.

Finally, Keynes questioned whether admitting the possibility of variation of coefficients between sub-periods contradicts Tinbergen's assumption that 'the same formula is valid over a long period of years? If this is seldom or never the case, is it worth while to bother about the details of the method?' (PAN III-309/22).

2.3 The Resort to Kalecki

The general view is that Keynes did not come out well from his critique of Tinbergen, with the implication that essentially he did not understand the new mathematical economics that Tinbergen represented, and that Lange and Marschak championed ('Introduction', Hendry and Morgan 1995). Keynes's apparent failure to pursue the matter into a methodological study that was more systematic than incidental criticisms of a particular author, have generally been taken to indicate a less than serious approach to the matter, caused by his own frail health and the emergency of war. However, there is now evidence that Keynes was somewhat more persistent in his view than might be inferred from his published exchange with Tinbergen. He appears to have decided that his critique could best be pursued by a more technical approach. He identified Kalecki as a possible ally in this.

Kalecki was, by his later account relayed through the recollections of his widow, becoming disillusioned with Cambridge. His work there had been subject to methodological criticism. His funding from the National Institute for Economic and Social Research appeared secure. With the continued absence of Marschak from Oxford, the Institute of Statistics there was leaderless, and offered a clear opening for Kalecki to continue the statistical research that he was doing in Cambridge. Keynes continued to support Kalecki at meetings of the NIESR Council. He put forward there his suggestions for Kalecki's research agenda at Oxford.

Lange had the advantage of being able to join Keynes's dispute with Tinbergen on economic method without the need to justify a specific body of quantitative work, as Tinbergen was obliged to do. Lange was therefore able to concede points on which Keynes's view was (probably) incontrovertible. At the same time, Lange, was unusual in combining a strong interest in statistical and mathematical economics, with an equally strong commitment to economic philosophy and methodology. This gave him a stronger position from which to challenge Keynes's philosophical and methodological objections to Tinbergen. The writer of the editorial notes to Lange's Collected Works therefore makes perhaps too modest a claim that the published exchanges between Keynes and Tinbergen covered the main points in Lange and Marshak's contribution. One can understand that two writers who were making their careers promoting a statistical and mathematical approach to economics would not wish to prolong a public dispute over a methodological approach to which they had nailed their academic colours. But the issues which they raise continue to haunt economics (see, e.g., Hendry and Morgan 1995, Toporowski 2002) if only because when the respective parties to these methodological disputes enter into dialogue, as they did in the case of Lange, Marschak and Keynes, the terms they used did not have the same meaning for each of the parties. For the statisticians, economic theory meant relations between variables. For Keynes, economic theory meant the linguistically unambiguous specification of concepts and the relations between them. Lange and Marschak were quite happy to concede to Keynes the possibility of structural change within a period covered by statistical data. But structural change meant in this context changes in model coefficients, whereas Lange clearly suggested that the more interesting structural changes are the ones in the institutions that make up a particular mode of production. Keynes was clearly keen that these issues should be more thoroughly explored. Hence his attempt to place the critique of Tinbergen onto Kalecki's research agenda at Oxford. In fact, despite his technical facility, Kalecki was perhaps the last person who could have undertaken such a fundamental analysis. He was not a trained statistician, and his knowledge of statistical theory was practical rather than methodological. In any case, there is no evidence that Kalecki took up Keynes's suggestion, although in later years he attempted a more methodological and institutional criticism of econometrics, as his contribution to the *festschrift* for Oskar Lange (Kalecki 1964).

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