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Idiosyncratic reflections on economics as a science

Assar Lindbeck,
Stockholm University, Sweden

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1. Introduction

"Is it really possible to make solid research on economic issues"? I have often been confronted with this question by colleagues in the natural sciences. My answer has usually been: "Yes it is possible, but it is not as easy as in the natural sciences" – a serious answer rather than a joke. What I mean is, of course, that economics, like other social sciences, study highly complex systems that differ across geographical areas and change over time – in some cases even as result of research itself. Generally speaking what, then, is the main achievement of economic research, and what are its major shortcomings? Let me make a few unpretentious reflections on these rather pretentious questions. I illustrate with a number of contemporary examples.

Doubts about the possibilities to pursue solid research on economic issues reflect, at least to some extent, a misconception about what economic research is all about. Many non-economists seem to believe that research in economics mainly deals with *forecasts* of the future economic development, and that the quality of economic research can therefore be judged on basis of the accuracy of such forecasts. But research and forecasting are quite different things. The purpose of economic research is to clarify *economic mechanisms.* On basis of insights about such mechanisms, economists also make conditional predictions about the consequences of exogenous events, such as changes in technology, preferences or government policies. In contrast, forecasts about the future economic development require guesses about future exogenous events. Such guesses are often much off the mark, and so are therefore often also forecasts about the future economic situation. In fact, many, perhaps most, major changes in the economic development in rich countries in the past were unexpected – not unlike earthquakes and volcano eruptions. A contemporary illustration of the difficulties in making forecasts is the diverging view among economists about the future unemployment prospects in rich countries – with diverging policy recommendations as a result. Some economists argue that the working-age population in developed countries will be too small to finance the expanding population of elderly. Others hypothesize that robotization will result in mass unemployment. The former group often proposes a higher retirement age or increased immigration of young individuals, while the second group often proposes a lower retirement age and/or increased transfer payments from the government to all individuals who choose not to work ("citizen's salary"). Both groups cannot be right. But both can, of course, be wrong.

2. Basic contribution

What is then the main contribution of mainstream economic analysis? Different economists would probably give different answers to this question. For instance, when Paul Samuelson was asked this question many years ago, he referred to the theory of Comparative Advantage. The rationale for his answer was, of course, that this theory explains a fundamental aspect of all societies, namely the division of work across nations, firms and households – as well as within such institutions. With full respect for Samuelson's choice, I would emphasize another, even more general, contribution. I refer to clarifications of *indirect effects* of decision-making of

different economic agents – in many cases in the form of complicated *interactions* of these effects across different markets. Such effects are highlighted not only in the context of formalized Partial and General Equilibrium models but also in concrete applications of economic analysis. Indeed, it is tempting to say that analyses of different types of indirect effects, and interactions among decisions of different agents, are both the most characteristic and the most important contributions of economic analysis.

Concrete examples are probably the best way of clarifying this point. My own favorite example is rent control. It is easy for the layman to see the direct effect of rent control, namely reduced housing costs for households with first-hand contracts for tenement apartments. In contrast, the indirect effects are often neglected by laymen, including politicians. I refer, for instance, to the emergence of housing shortage, to the inefficient use of the existing housing stock when barter and black markets replace traditional markets, and to a fall in both private investment and the maintenance of privately own tenement houses. Many years ago, I tried a drastic formulation of this message: "Next to bombing, rent control is the most efficient technique for destroying cities". After 73 years of rent control in Sweden, the waiting time in the official queues for a tenement apartment in Stockholm is today over one decade – i.e., about the same length of time as for Trabant cars in East Germany. As we know, rent control also has important consequences for the functioning of other markets, in particular the labor market where the mobility, and hence the flexibility, is harmed.

The importance of emphasizing indirect effects and interactions across markets is also demonstrated in the analysis of short-term macroeconomic fluctuations. I do, of course, refer to the importance of studying interactions across aggregate markets for products, labor, money and financial assets, although Keynesian-oriented and New Classical Macroeconomic models still differ on the best way of analyzing these interactions. However, it seems that most contemporary macroeconomists today accept the traditional Keynesian view that a large fraction of unemployment may be *involuntary* and that models that do not reflect this feature of macroeconomic fluctuations are defect. Indeed, for a long time, there have also been long solid microeconomic foundations for the existence of involuntary unemployment – efficiency-wage models, monopoly-union models and insider-outsider models.

3. Some limitations and pitfalls in social science

Like other social sciences economics is, of course, plagued by a number of limitations and pitfalls. Some of these are shared with other social sciences, while others are more specific for economics. One example of the former is the complex relation between formal modeling and intuitive reasoning. It is probably generally agreed among researchers in the social sciences that it is important to combine formal model-building with verbally formulated intuition – presumably much more important than in the physical sciences. Intuition is important both to generate hypotheses and to interpret the results of formal studies (theoretical as well as empirical). Indeed, it seems that producers of research in the social sciences are consistently required to provide intuitive interpretations of their results. This observation reminds me of

another point made by Paul Samuelson: "All economic regularities that have no common-sense core that you can explain to your wife will soon fall".

But intuition has severe limitations. In particular, it may be too flexible. This is illustrated by the ease of finding an intuitive explanation *afterwards* for conclusions that we have reached through formal analysis. Let me take an extreme example. Assume that a researcher concludes from formal analysis that an increase in a parameter, α, generates a fall in the variable x – and that the researcher is able to provide convincing intuition for the result of the formal analysis. But suppose that the researcher, later on, discovers that he/she has made a mistake in the formal analysis, and that the sign of the effect of α turns out to be the opposite. In such cases, it is often possible for the researcher to find a new intuition that fits the corrected result. (I have myself been in this situation several times.) This illustrates what I mean when arguing that intuition is too flexible to be fully reliable. This example makes me think about a well-known formulation by Groucho Marx: "I have a strong principle on this issue – but if you do not like this principle, I have another one". Intuition is sometimes as flexible as Groucho Marx' principles.

Strong *priors* of the researcher may also create problems. Suppose that a scholar, through formal analysis, has arrived at results that conflict with his/her original priors. It is then tempting for the researcher to take a new look at the analysis, asking if the results could possibly have been generated by some mistake in the specification of the model, by some deficiency in the data set or perhaps by a weakness in the statistical design. In principle, the rationale for taking a new look at the result of formal analysis is, of course, equally strong if the results actually conform to the researcher's intuitive priors. But the personal incentives of an author to scrutinize the results are probably weaker in this case, which may then generate biased results. Of course, referees and editors of academic journals have, in particular in recent decades, tried to mitigate such bias, for instance by requiring the authors to make numerous robustness tests of their results.

Moreover, when a researcher has invested in a particular analytical framework for several years, it may be tempting (perhaps simply for convenience) to apply the same framework even when analyzing issues for which it may not fit very well. The researcher may, to some extent, become a victim of what Daniel Kahneman (2011) has called *theory-induced blindness*. An important example is the broad acceptance among new generations of macroeconomists in the 1970s and 1980s to accept models with equilibrating labor markets also in short-term macroeconomic analysis, i.e. models without involuntary unemployment. The risk of being the victim of such "blindness" is accentuated if the researcher has strong ideological preferences. One reason is that ideologies are mixtures of values and views of the world – and that the values may color the view of the world, and therefore also the choice of model.

4. Specific criticism of economics

The limitations and pitfalls of research discussed in the previous section prevail in all social sciences – indeed in some cases in research in general. Economic research is, however, often also criticized for specific limitations and pitfalls. For instance, economic analysis is often

criticized both for exaggerating *the rationality* of economic agents and for assuming *too narrow motives* (mainly selfishness) behind economic behavior. How should we look at these criticisms? My own experience is that economic analysis based on the assumption of rationality and selfishness often gives reasonably realistic answers to important questions. Examples are questions on how the allocation of resources and the distribution of income are influenced by changes in technology, preferences, competition, taxes, subsidies, or price regulations. Indeed, in my discussion earlier of rent control. I saw no compelling reason either to constrain the assumed rationality of economic agents or to include a richer set of behavior motives than selfishness.

A model based on full rationality and/or narrow behavior motives functions as a flashlight on a theatre scene. The part of the stage that is sharply lit up comes out quite clear, but the rest of the stage looks even darker than without the flashlight, which means that a wider spread of light may sometimes be advantageous for the viewer. Translated into economics: in some cases, assumptions about more modest levels of rationality among economic agents may increase the realism of the analysis. I refer, for instance to assumptions about overconfidence, overoptimism, limited will-power and wishful thinking. In many cases, we may also increase the realism of the analysis by allowing a broader set of motives than selfishness behind individuals' behavior. Potentially important examples are altruism, trust and social norms. Indeed, recent work by so-called "behavioral economists" has already enriched economic analysis by allowing such behavior motives.

Moreover, at the same time as economists in recent decades have imported insights from other social sciences, models and methods in economics have increasingly been applied to issues that have traditionally been studied only in other social sciences. As a result, there has recently been some convergence of analytical methods among social scientists. In this connection, it has also been easier to study interactions between traditional economic and traditional social mechanisms. An important contemporary example is studies of causes and consequences of mass migration of low-educated individuals from poor to rich countries. In particular, it is likely that the strong resistance among many natives in rich countries to such migration does not only depend on economic losses among some individuals in host countries but also on *social tensions* in neighborhoods with a particularly large and speedy migration of this type – partly reflecting clashes between individuals with diverging human capital, income and values.

In spite of pronounced improvements of the quality of economic research in recent decades, we should not expect to achieve the same precision and robustness as in physics. Indeed, already John Stuart Mill (1836) emphasized that asserted economic mechanisms are statements of "tendencies" rather than tight laws. In a similar vein, Friedrich von Hayek has argued that economic analysis can only achieve what he calls "pattern predictions", which I interpret as broad, mainly qualitative insights about economic mechanisms. However, I am somewhat more optimistic than these scholars about the possibilities to further increase the precision and robustness of economic research – in many cases as a result of methodological advances in quantitative economic analysis, i.e. econometrics. We have to wait and see.

5. The researcher as policy advisor

How should we then judge researchers' role as economic-policy advisors – either as formal advisors to the government or as a participant in the open policy discussion? In both cases it is, of course, important that the advisor is explicit not only about the objectives but also about the drawbacks of the recommended policy. Moreover, the recommendations should ideally be based on what in medicine is called *evidence-based treatment*. But in some cases, there may not be much solid research to draw on. The advisor then has to rely on a combination of established theory and whatever fragmented empirical studies that exist – weighted together by common sense and hence intuition. Take, for instance, situations when a country with a fixed exchange rate runs into an overvalued exchange rate as a result of more rapidly rising unit labor costs than in competing countries. Should the economists in such a situation recommend deflationary policies, a discretionary devaluation or a shift to a floating exchange rate – even when there is not much serious research evidence about which strategy that is most proper for this particular country at a particular point in time. Now, such complications are not unique for social science. Within medicine, treatment may, according to generally accepted principles, be based not only on research but also on accumulated experience.

An even more delicate issue is whether a policy advisor should take into account the expected *ability* of politicians and public-sector administrators to implement suggested policies. In other words, should the advisor base his/her advice on his/her beliefs of how politician and public-sector administrators are likely to react to his advice? For instance, should he/she suggest policy measures that require a very competent implementation? Or should he/she instead recommend policy measures that are likely to make some good even if they were not very competently implemented – what may be called a "robust policy strategy"?

Now, it is difficult to know how large an influence academic economists have had on actually pursued economic policies. John Maynard Keynes is often quoted for his formulation that "practical men who believe themselves to be quite exempt from any intellectual influence, are usually the slaves of some defunct economists (Keynes, 1936, p.387). Keynes' own influence is a striking illustration. However, in reality, the interaction between the advisor and the government may be problematic. Some politicians may simply choose advisors who are expected to give academic legitimacy to positions that they themselves have already taken. Or some politicians may use the *arguments* provided by the advisor even if the basic *motives* behind their actual political decisions are quite different. Motives and arguments do not always coincide – a phenomenon not confined to politics.

Finally, to give advice to governments without at the same time informing the general public about the recommended policy is like writing in the sand of an ocean: the next wave in public opinion will erase the writing. Therefore, it is important that the advisor helps the government explain how a suggested policy package is likely to function. After all, in a democracy it is important that the actually pursued policy is in reasonable harmony with public opinion.

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