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POLITICAL AND INSTITUTIONAL ECONOMICS

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Political and Institutional Economics

The honour of being invited to give this year's Geary Lecture here in Dublin is highly appreciated. When brooding over what topic to choose for my lecture, I finally decided not to take up any special research problem this time but to try to account in the most general terms for the methodological conception of economic science I have arrived at after more than half a century of work in the field. My audience will understand and excuse that my lecture then occasionally takes on a more personal tone than an ordinary scholarly presentation.

I

We all know that the academic discipline now commonly referred to as "economics" was called "political economy" equally commonly a couple of generations ago. The first Chair I held at the University of Stockholm from 1933, as successor to Gustav Cassel, was named "Political Economy and Financial Science".

What did economists in earlier times mean by inserting the adjective "political"? And why was it dropped? Was the change only meant to be a rationalising abridgement without logical significance? Or can there be a deeper significance motivating the change?

As an idiomatic alteration of term, from "political economy" to "economics", when it gradually occurred, it was seldom, if ever, discussed as a problem or even noted as signifying an important change in emphasis or direction of our work. Looking backward it seems to me, however, to have been important as a sign of a change in the pursuit of our work. It pertained to a fundamental difference in approach when studying the economy.

II

Already from the beginning in the eighteenth and early nineteenth centuries, economists unhesitatingly saw themselves as political economists. They firmly believed it was part of their duty to draw policy conclusions. And they held that they were entitled to do this on rational grounds, i.e., as logical inferences from their knowledge about the facts.

It is true that in the classical line, at least from the early part of the nineteenth century, the more sophisticated economists often emphasised that their "theory" did not permit them to draw policy conclusions.

What they referred to as not possible to serve as basis for policy conclusions was, however, their "theory" in a restricted sense. From Ricardo and on, that theory had become a very simplified deductive analysis built upon a few abstract assumptions; Senior reckoned four. Even though the attempt was to make the assumptions realistic as far as possible, the more thoughtful writers were, in principle, aware of the need to encompass much detailed data on how various other social conditions actually were, in order to formulate valid policy conclusions. In that sense they were, in principle, institutionalists. To this I will return.

But, in addition, they needed a valuation basis for these policy conclusions. In the meaning clarified by John Stuart Mill in his earliest works, the study of the economy is a "moral science", as in his view are all social sciences. Crucial for this view, which from then on expressively established economic science as political economy, was the conception that there were objective values, that could be known as facts, could be observed and analysed, and so laid as a basis both for the study of all other facts and for rational policy conclusions.

This value basis for economic study was provided by the moral philosophy of that time, initially by the natural law philosophy and later by the utilitarian philosophy which, as I have shown, was only a variation and reformulation of the old natural law philosophy.

The neo-classical authors then refined this moral philosophy and, in particular, its foundation in the hedonistic associational psychology. As a matter of fact, the marginal theory of value from the 1870s stands out as giving the final finish to the moral philosophy of utilitarianism. Many of the prominent economists of that era, for instance Sidgwick, also figure prominently in the pantheon of philosophers in that line of thought.

The "welfare theory" developed by the first generation of neo-classical authors had thus its logical foundation in the utilitarian moral philosophy, which, in turn, rested on the hedonistic associational psychology. This development happened at about the time when both the utilitarian moral philosophy and, in particular, the basic hedonistic associational psychology, lost its self-evident character, if not abandoned entirely by the professional philosophers and psychologists. The very apparent isolation of economic science from the other social sciences and from philosophy, as these disciplines developed, dates from that time.

III

Modern establishment economists have retained the welfare theory but have done their best to conceal and forget its foundation in a particular, and now obsolete, moral philosophy and an equally obsolete psychology. They have then succeeded in pursuing what appears to be an amoral economic theory, and they are proud of stressing this. They are not programmatically political economists as were our forebears.

By demonstrating the superficiality and logical inconsistency of this modern welfare theory almost fifty years ago – in a book published in Swedish in 1931, but first translated into English in 1953, *The Political Element in the Development of Economic Theory* – I thought I had finally disposed of it. But it grows like a malignant tumour. Hundreds of books and articles are produced every year on "welfare economics", reasoning in terms of individual or social "utility" or some substitute for that term. But if the approach is not entirely meaningless, it has a meaning only in terms of a forlorn hedonistic psychology, and a utilitarian moral philosophy built upon that psychology. I have always wondered why the psychologists and philosophers have *left the economists alone and undisturbed in their futile* exercise.

The trend toward narrow "professionalism" in contemporary establishment economics in regard to training, reading and, indeed, awareness of everything outside the narrow field they have staked out for their work, protects them from being disturbed by much knowledge about modern psychology and philosophy. The relative neglect we can now find in the curricula at most universities of the study of the history of economic science helps them to have an exaggerated belief in the novelty of their own contributions to welfare economics. In particular, it protects them from grasping that, what they are attempting is normative economic theory, but in disguise, as they are not prepared to call themselves political economists.

Those great economists who, a century ago, originally developed the hedonistic and utilitarian welfare theory – among them, Jevons, Sidgwick and Edgeworth – could work with conviction and in clear terms, since they were aware of what they were doing. They were not apt to skip *over the basic psychological and philosophical assumptions* implied in welfare theory. The contemporary welfare theorists mostly miss the historical perspective they would gain by intensive study of their predecessors and, at the same time, the awareness they could get by such studies of where the basic difficulties are buried.

Few attempts have been made by contemporary establishment economists to study, empirically and in terms of modern psychology and sociology, people's behaviour as income earners, consumers, savers, investors and as members of interest organisations and political parties. Those attempts that have been made outside our fraternity to carry out realistic psychological and sociological research about economic behaviour, free from the assumptions of the old and new welfare theory, have been disregarded in establishment economics. The deeper reason for this neglect is, of course, that the results of such research cannot possibly be integrated into the conceptual framework of a welfare theory of the inherited, and still dominant, type.

IV

In one fundamental sense, a student who is prepared, as I am, to call himself a political economist – and thus recognises that economics is a moral science – works in the great tradition that became established already in the eighteenth century and more definitely formalised in the nineteenth century. When, at the same time, he discards the moral philosophy of utilitarianism and its implied hedonistic psychology, that traditionally was laid as a basis for political economy – and is a hidden and repressed assumption even for welfare theory in modern economics – he has then to account for what other valuational basis he has for his work.

Valuations are always with us. Disinterested research there has never been and can never be. Prior to answers there must be questions. There can be no view except from a viewpoint. In the questions raised, and the viewpoint chosen, valuations are implied.

Our valuations determine our approach to a problem, the definition of concepts, the choice of models, the selection of observations and, at the end, the presentation of research results – in fact, the whole pursuit of a study from the beginning to the end. When we remain unaware of the valuations basic to our research, this implies that we try to reason with one main premise missing, which results in an indeterminateness opening the door for systematic biases.

The old book of mine that I mentioned was devoted to finding out, by immanent analysis, the hidden common biases in different epochs by different main authors. As a logical critique of economic science, as it developed within the classical and neo-classical line, I hold it still valid. The book did not enlarge however, upon the psychological and sociological problems of how the space of indeterminateness, due to the avoidance of spelling out more specifically the underlying valuations, has actually come to be utilised for reaching the specific biased views.

While we are studying intensely all sorts of groups in society and their behaviour, we have preserved a taboo about our own behaviour as researchers. But we are certainly not automatons like some of our modern research facilities. What we actually do in our research is dependent, not only on our inherited personality traits, but also on the strong traditions in our craft and the influence of the surrounding society. Except for a few dissenters, the forces working on all economists at a given time are apparently strong enough to result in substantial conformity in shaping the dominant views in an epoch.

When I have urged the development of a psychology and sociology of social science and scientists, it is also for the practical reason that it would be important for avoiding biases by sharpening our awareness of the problem and heightening our efforts to avoid biases. The term bias, and the reality behind it, is shunned by economists in the establishment line.

V

Valuations are always implied in research, as I said, but they should not be brushed under the carpet. Instead they should be made conscious. They represent a volitional element in research, which for the sake of honesty and clarity should be explicitly stated. They are needed already for establishing relevant facts and not only for drawing policy conclusions.

In this way alone can we aspire to "objectivity in research" in the only sense this term can be given any meaning. The prevalent endeavour to be like the natural scientists, for whom valuations do not play the same role in research, and to believe that what we are doing is simply observing and establishing the facts and factual relationships, is an illusion.

The use of explicit value premises serves three purposes. It determines in a rational way the statement of problems, the approach and the definition of concepts used in a study. It further lays a tenable, logical basis for reaching policy conclusions by rational reasoning. And it helps to purge, as far as possible, the scientific investigation of distorting biases.

By working with specific and explicit value premises, we are not simply "expressing our own biases", as is often suggested. For biases are ordinarily not conscious to the researcher and are thus not under his control, which opens the door for arbitrariness. Loose declarations of personal biases are no substitute for a rational procedure.

The value premises cannot be chosen arbitrarily. They have to be relevant and significant for the society studied, logically consistent, and feasible. The difficulties of realistically basing our value premises on prevalent valuations in the society we are studying should not be concealed, although in this lecture I shall not elaborate that point.

I do insist, however, that when a researcher has placed himself under the discipline to spell out, in as definite terms as he can, a set of instrumental value premises – however they have been reached and whichever they are – and if he allows them to determine his approach, the definition of concepts and his formulation of models and theories, this represents an advance in the effectiveness of research, and particularly towards protecting himself against biases.

By so spelling out the role certain valuations have played in his research, he has also aided his critics. Anyone willing to challenge his choice of value premises is relieved of the cumbersome task of discovering, through immanent criticism, the otherwise only implicit valuations, and the way they have steered research.

VI

When in many fields of study I have tried to apply this insight, and felt compelled to state my value premises explicitly and justify their selection, I feel myself, as I said, in one sense to be working in the great tradition that began in the eighteenth century. Since it implies that economic policy conclusions can rationally be inferred from these value premises and from the facts ascertained from the viewpoint of the same value premises, economic science has been restored to a moral science in the meaning of John Stuart Mill – though deprived of the reliance placed by the old political economists on the existence of an objective system of values.

In another respect also I feel aligned to the old tradition. When the writers in the classical and neo-classical line observed that their abstract theory could not permit them to draw policy conclusions, this was, as I pointed out, a recognition of the need for a much fuller knowledge of the society they were studying. Only by widening the horizon could they pretend to be political economists.

But while, as I have shown, the value theory always remained central and was finally worked out with considerable thoroughness by the first generation of neo-classical economists a hundred years ago – in their welfare theory – the other prop under their pretension of being political economists, viz. the inclusion in their study of all relevant conditions besides the economic factors in their theory, was never systematically worked out as a principle of research.

It is true that without such a methodological clarification many of the writers in the classical and neo-classical line, from Adam Smith to Alfred Marshall, did stretch their analysis far outside the realm of economic factors. This was, of course, still more true in regard to authors outside that main line as, for instance, the German Historical School and individual writers like Karl Marx and, one generation earlier, Friedrich List.

It was only after the Second World War that conventional economists narrowed and hardened their isolation from the other social sciences. This isolation has, for instance, made it possible for a group of economists in the 1950s to almost aspire to have made a discovery by stressing education as of importance in development. But they retreated immediately to deal with it as an "investment in man" together with physical investment in the capital/output ratio, an approach that Marshall, who, of course, also made room for education in his writings, had expressly warned against.

VII

If, with the indulgence of my friendly audience, I be permitted a brief autobiographical note to this point, I should confess that it was not simply the logic, as I am presenting it today, but my actual research experiences in the type of problems I worked on that brought me to clarity in regard to the two methodological issues I deal with in this lecture.

The old book of mine already mentioned contained a critical review of the development of economic theory from its early beginning. By immanent logical criticism I demonstrated how in every earlier epoch, and also at the time I wrote my book, the economists in the classical and neo-classical line had fallen into the trap of biased views. As a critical and historical analysis I believe it is valid today.

But throughout the presentation lurks the idea that if only purged from biases there would remain a solid and objective economic theory from which, by adding value premises, rational political conclusions could be inferred. That valuations enter already in the ascertaining of the facts, making necessary explicit value premises as early as at that stage of a study, had then not become clear to me.

At the end of the 1930s I became committed to making a comprehensive and objective study of the Negro problem in America. As I faced this task, that problem was not only immensely complicated, involving practically all phases of American civilisation, but it was charged with violent emotions, manifesting themselves also in opportunistically distorted views about reality. In this situation even the scholarly literature was heavily influenced and was very clearly biased in different directions. That this was so, was part of the reality I was studying. I found myself quoting books, not simply to try to establish how race relations actually were, but to demonstrate those other facts of how different categories of writers were perceiving and thinking about this reality.

In this situation I felt compelled to explain from what valuational angle I was ascertaining and analysing what I could observe and substantiate of actual race relations. I chose as instrumental value premises what I called the American Creed, the set of ideals and moral commitments, which in America during the Revolution, and later in the efforts to make a nation of immigrants from so many different countries and cultures, had been given a currency and consciousness unmatched in any other country.

I had to specify my chosen value premises for the several categories of problems and to develop in methodological appendices the general reasons why value premises were needed in such research, and how they should be selected. After this experience I have, in all my work, been careful to spell out my value premises and how I have arrived at them.

Particularly in my work on the planning problems of underdeveloped countries which has taken up the major part of my years since then, I have felt the need for explicit value premises as logically pressing as in my work on *An American Dilemma.*

The second influence of my work on that book was to make me a fully-fledged institutional economist. That there was a close interrelation between the economic situation of Negroes in America and all other conditions of their life, and that of all others, was only too apparent. Nothing of scientific importance could be ascertained except by transgressing the boundaries between our inherited disciplines. In this respect also I have adhered to this principle in my later work.

I came to see that in reality there are no economic, sociological, psychological problems, but just problems, and they are all mixed and composite. In research the only permissible demarcation is between relevant and irrelevant conditions. The problems have also to be seen generally in historical perspective.

VIII

I proceed now to attempt to give a more systematic account of what I mean by institutional economics. The borderline is somewhat blurred as some economists of the conventional school sometimes venture to take a broader approach to practical problems. The conscious and systematic institutionalists are, however, in a tiny minority. The reasons for the broader approach I will now formulate in logical terms.

The most fundamental thought that holds institutional economists together, however different they are otherwise is our recognition that even if we focus attention on specific economic problems, our study must take account of the entire social system, including everything else of importance for what happens in the economic field: foremost, among other things, the distribution of power in society and, generally, economic, social and political stratification and indeed, all institutions and attitudes. To this we have to add, as an exogenous set of factors, induced policy measures, applied with the purpose of changing one or several of these endogenous factors.

The dynamics of this social system are determined by the fact that among all the endogenous conditions there is *circular causation*, implying that, if there is change in one condition, others will change in response. These secondary changes in their turn will cause new changes all around, even reaching back to the initial condition, the change in which we assumed began the process, and so on in further rounds.

So the whole system will be moving in one direction or another, and it may even be turning around its axis. There is no one basic factor, but everything causes everything else. This implies *interdependence* within the whole social process. And there is generally no equilibrium in sight.

One important aspect of this process is that most often, though not always, changes which are reactions to a more primary change tend to go in the same direction. To give an abstract example: improved nutrition among poverty-stricken masses in an underdeveloped country will raise the productivity of labour which, in turn, will increase the opportunity to improve production and nutrition further. This is why circular causation normally will have *cumulative effects*. Through feedbacks normally causing more primary changes to have repercussions *in the same direction*, the results, for good or ill, may, after some time, be quite out of proportion to an initial impulse of change in one or several conditions.

Those initial changes, which in this model are defined as exogenous, (that is, the *policy interventions*,) are under a wider perspective also dependent on the endogenous conditions and their changes, to which they are reactions, and which also in many ways constrict and influence their scope and direction. When kept separate in this model of circular causation with cumulative effects, this is done in order to preserve room for an analysis in terms of planning, i.e., policy deliberations and decisions conceived of as not entirely restricted and determined by the existing conditions and ongoing changes.

As the system is moving, partly under the influence of policy measures, the *coefficients of interrelations* among the various conditions in circular causation are ordinarily not known with quantitative precision. Elements of inertia, time lags, and in extreme cases, the total non-responsiveness of one or several conditions to changes in some set of other conditions, raise problems about which precise knowledge is seldom available. This is largely true even in developed countries with their more complete accounting for all social conditions and their more perfected statistical services. But it is particularly true in underdeveloped countries.

Consequently, our analysis of development problems must often end in tentative generalisations and mere plausible hypotheses, built upon limited observations, discernment, and conjectural judgements. Even in developed countries the widening of the perspective, implied in this institutional approach, will regularly destroy the neat simplicity of both analysis and conclusions in conventional economics.

Our endeavour, of course, must be to develop concepts which more adequately grasp real conditions and their interrelations, and to direct empirical study to ascertain the quantitative coefficients of those interrelations. But we should be aware of the huge area of less reliable, complete and precise knowledge.

These remarks are offered as hints toward the master model of institutional economics, which must be holistic, even when focused on particular economic problems. What I believe is common to institutional economists is that they all have in the back of their minds the master-model of the movement of the whole social system within which there is causal interdependence. While studying an economic problem they will therefore come to include in their economic analysis non-economic factors, selected by the criterion of relevance to what happens.

IX

In calling the holistic approach the fundamental principle of institutional economics, I imply that our main criticism of ordinary economists is that they work with narrowly closed models, limiting the analysis to too few conditions. These are traditionally chosen from conditions called "economic factors," which usually are more susceptible to quantification, although even this quality is often opportunistically exaggerated, and this is so not only in regard to underdeveloped countries.

Holding down the variables to only a few that can be quantified makes possible the use of impressive mathematical models. They regularly presuppose a sharp restriction of vision. Almost the entire social system is kept out of sight. This should at least have required a clear *statement of assumptions* with respect to conditions and determinants not considered. Such an account of implied assumptions about what is left out is generally not given. Most of the time it is not even consciously perceived.

I should add that when in recent decades some economists, but more often sociologists, actually attempt to account for, besides the "economic factors," the importance of one or another condition which they can measure – for instance a vital index – it is most often done in a similarly restricted way in regard to all other conditions in the social system. And again it is done without spelling out clearly in terms of assumptions, all that is not considered, and still less attempting to integrate their findings into a broader framework. In our journals we are getting a crop of ever more minute studies, which lack even an attempt towards that integration into a view of the whole social system, which is the demand of the institutionalists. For that reason I find them irrelevant and, therefore, uninteresting.

The institutional economists will so often stretch out their analyses into fields where, for reasons already hinted at, quantitative precision is not yet possible. This easily leads to a facile characterisation of much of our research as "qualitative" instead of "quantitative." But we are equally, or more, intent upon reaching quantitative knowledge as soon, and as widely, as possible. We are in fundamental agreement with Jevons's old dictum, that more perfect knowledge is attainable only when we can measure conditions and changes of conditions.

The seemingly greater precision in conventional economic analysis is only attained by leaving out a whole world of relevant things. But as we institutionalists became accustomed and trained to treat matters that, though relevant, cannot easily be represented by figures, we have generally developed a more critical scrutiny of statistics. Particularly when conventional economists turn to discuss practical and political problems, but also in their abstract models, too often they are using aggregate figures for gross national product, or unemployment, or other economic matters within their view, with great carelessness. In regard to their dealing with figures, ordinary economists do not show the same urge for clear concepts and the same concern for estimating uncertainty of measurement, as, for instance, has always been standard in demographic research.

I should add that when institutional economists have to be critical of the closed models of our conventional colleagues, this does not, of course, imply that we are hostile to models and theories. But we want the models and theories – by us normally conceived as logically integrated systems of questions to the empirical reality around us – to be more adequate to this reality.

X

I should, at last, point out that institutional economists generally are, at the same time, political economists. For all of us, as far as I know, economics is a "moral science" in Mill's meaning of the term. While ordinary economists, like most other social scientists, are what is known in the history of philosophy as "naïve empiricists," having convinced themselves that they are simply dealing with observable facts, we institutionalists have been involved in the problem of how to account for the role of human valuations in research.

We are all utterly sceptical towards the welfare theory of ordinary economists. Since we cannot accept as the valuational basis for our research the outmoded moral philosophy and hedonistic psychology of our classical and neo-classical predecessors, we have to account for what other valuational basis we have for our research.

We have thus, generally, had our eyes open for prevailing biases in research when valuational assumptions are concealed, while the very idea of that type of opportunistic distortion is an almost forbidden thought in conventional economics. Studies of how influences from the surrounding society have conditioned economic research are almost entirely missing in the writings of ordinary economists. Looking back, these influences are more apparent. I suspect the unwillingness to be aware of the problem of prevalent and systematic biases in economic research may be one of the explanations for the disinterest in the history of economics, about which I complained.

One common bias among most ordinary economists is the more or less explicit assumption of market rationality and optimality, while actual markets are becoming less and less perfect and in some areas disappearing altogether.

XI

I have so far been attempting to argue the case for political and institutional economics in terms of logic. When, however, I believe that it is destined to gain ground at the expense of conventional economics in the near future, it is not primarily because of the strength of its logic, but because it will be needed for dealing effectively with the practical and political problems that are now towering over, and threatening to overwhelm, us. Much of present establishment economics, and in particular, those very abstract theoretical constructs enjoying up till now the highest prestige among economists will, I believe, be left by the wayside as irrelevant and uninteresting.