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Institutional Economics

Gunnar Myrdal

Having been invited to give a public lecture toward the end of my pleasant term as Visiting Professor at this university, I can find no more appropriate topic than to give my views on the type of nonconventional economic thinking which I have become accustomed to calling institutional economics, but which other authors with the same critical bent of mind sometimes refer to as evolutionary or political economics.

I have in the course of my life often been in the United States, sometimes for a prolonged period, as when I wrote *An American Dilemma,* and I have frequently visited American universities. But it so happens that I have not been in Madison since I spent one month here in the early spring of 1930. Alva Myrdal and I had then a year in the United States as Rockefeller fellows, and I had been advised to go to the University of Wisconsin, where John R. Commons and several of his younger colleagues, among them Selig Perlman, had formed a center of institutional economics. Another such center was at Columbia University, where Wesley Mitchell was the dominant leader. Generally speaking, this was in America a time of an almost aggressive advance of institutional economics, held together as a rebellion against the conventional neoclassical school. In Madison I was received most generously by Commons and his friends in the faculty, and I remember he taught me bowling.

But I was not converted to what was then called "the new economics." I was in the "theoretical" stage of my own scholarly development. I be-

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lieve the first thought of forming the Econometric Society, then conceived of as a defense organization against the advancing institutionalists, was born at a Sunday luncheon in Irving Fisher's home in New Haven, Connecticut, to which Ragnar Frisch and I had been invited.

When the further rise of institutional economics, which I then saw as a danger, was averted, I believe this was an effect of the Great Depression. In the macroeconomic neoclassical school there were those of us who could master the problem of decreasing demand and deflation and give relevant policy advice to governments, while the institutionalists remained disoriented.

In Sweden we happened to be in advance of this theoretical development, as young economists could build on Knut Wicksell's works, written around the turn of the century. When the Labor Party assumed power in 1932, an unbalanced expansionary budget with public works that built upon the new theory was prepared and presented the next year, and it was later accepted by Parliament. In fall 1932 I wrote the theoretical motivation appended to the new budget proposal. I will discuss later how the present stagflation-depression is different in character from that of the 1930s and cannot so easily be handled by the theoretical tools inherited from and then further developed in the conventional neoclassical tradition.

Meanwhile, I had become an institutional economist. In Sweden I became involved in social equality problems, which I found could not be handled scientifically except by broadening the approach to all human relations. In 1938 I accepted responsibility for a study of race relations in the United States, and this took me farther away from conventional economics. I found myself writing a book about the entire American civilization, with stress on various aspects relevant from the viewpoint of the largest of the disadvantaged groups there.

From then on more definitely I came to see that there are no economic, sociological, or 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 are regularly also political and, moreover, must be seen in historical perspective.

To recognize the duty of the student to transgress the inherited boundaries among disciplines, created and adhered to by tradition and for practical reasons of specialization upheld in teaching and research, became the essence of my conversion to institutional economics. In my later work on the development problems of underdeveloped countries I have continually been under the intellectual compulsion to broaden my approach far outside the models restricted to the so-called economic factors.

But this transgression should not be permitted to decrease the demand on the student for expertise. I have never felt that difficulty overwhelming. The rules for truth-seeking are identical, whatever one is studying. But these rules certainly make institutional research more difficult and time consuming.

Returning to the institutional economics becoming prevalent in the United States in 1930, this was an almost isolated American development. Reference was regularly made to Thorstein Veblen, Commons, and Mitchell, of whom the latter two were still living and active. I remember that I said at the time that no country can be so provincial as a very big one.

Indeed, most of the writers in the classical and neoclassical line, from Adam Smith to Alfred Marshall, could be characterized as institutional economists in the sense that they did take into account all sorts of factors besides the economic. And this was still more true in regard to authors who fell outside that main line, for example, the German historical school and individual writers such as Karl Marx and, one generation earlier, Friedrich List.

It was only after World War II that conventional economists narrowed and hardened their isolation from the other social sciences. This isolation made it possible, for example, for a group of economists in the 1950s virtually to claim as a discovery the idea that education should be stressed as important 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 space for education in his writings, had expressly warned against.

What Is Institutionalism?

After these introductory remarks, the personal tone of which will be excused in friendly Madison, I now proceed 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. The conscious and systematic institutionalists are in a tiny minority, which permits me to refer to the others as ordinary economists.

The reasons for the broader approach of the institutionalists I will formulate in logical terms. My presentation must be brief and concentrated. And it cannot be original, as I and others have more fully stated our views before in books and papers.

The most fundamental thought that holds institutional economists together is our recogni ɔn that even if we focus attention on specific problems, our study must take into account the entire social system, including everything else of importance for what comes to happen in the economic field. Foremost, among other things, is the distribution of power in society and, more generally, economic, social, and political stratification; indeed, all institutions and attitudes. To this must be added, 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. Those secondary changes in their turn will cause new changes all around, even affecting the condition whose change we assumed initiated 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; 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, although not always, changes which are reactions to a more primary change tend to move in the same direction. To give an abstract example: Improved nutrition among poverty-stricken masses in an underdeveloped country will raise labor productivity and, in turn, increase the opportunity to improve production and nutrition further. This is why circular causation normally will have *cumulative effects*. Through feedback regularly 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 change impulse of one or several conditions.

Those initial changes, which in this model are defined as exogenous, that is, the policy interventions, under a wider perspective are 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. In this model of circular causation with cumulative effects, these exogenous changes may be kept separate in order to allow freedom for an analysis in terms of planning, that is, policy deliberations and decisions conceived of as not entirely restricted and determined by the existing conditions and ongoing changes.

Because the system is in constant movement, 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 nonresponsiveness of one or several conditions to changes in some sets of conditions, all 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 often must end in tentative generalizations and mere plausible hypotheses, built upon limited observations, discernment, and conjectural judgments. 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 endeavor, 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. That model must be holistic, even when focused on particular economic problems. I believe that the common denominator among institutional economists is their tacit acceptance of a master model which encompasses the movement of the whole social system, within which there is causal interdependence. Thus, while studying an economic problem they will include noneconomic factors in their economic analysis, selected according to their relevance to what happens.

Conventional Economics

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

Confining the variables to only a few that can be quantified permits the use of impressive mathematical models, which regularly presuppose a sharp restriction of vision. Almost the entire social system is kept out of sight. This methodology should at least require a clear *statement of assumptions* with respect to conditions and determinants not considered. Such an account is regularly not given. Most of the time the need is not consciously perceived.

I should add that, in recent decades, when some economists (but more often sociologists) have attempted to account for and measure the importance of factors other than the "economic" ones (for example, a vital index), it most often has been done in a similarly restricted way in regard to all other conditions in the social system. And, again, assumptions have not been spelled out in clear terms, and still less often has there been an attempt to integrate their findings into a broader framework. Our journals yield a crop of ever more minute studies, lacking in attempts toward that integration into a view of the whole social system which is the sine qua non of the institutionalists.

Institutional economists so often apply their analyses to fields in which, for reasons already hinted at, quantitative precision is not yet possible that much of our research is facilely characterized 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 W. S. 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 ignoring a whole world of relevant things. But as we gradually have become accustomed and trained to treat matters that, although relevant, cannot easily be represented by numbers, we have generally developed a more critical scrutiny of statistics. Especially when conventional economists discuss practical and political problems, but also in their abstract models, they all too often use aggregate figures for economic matters (such as gross national product or unemployment) with great carelessness. When dealing with numbers, they do not show the same concern for clear concepts and for estimating uncertainty of measurements as, for example, has become standard in demographic research.

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

Political Economy

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

When our discipline originated in the eighteenth and early nineteenth century, economists firmly believed it was part of their duty to draw policy conclusions and to do so on rational grounds, that is, as logical inferences from their knowledge about facts. As refined and clarified by John Stuart Mill in his early methodological essays, the study of economic problems was a "moral science." He stressed, however, that economists' "theory"—the prototype of present-day closed models of ordinary economists—was not broad enough to permit them to draw policy conclusions; in order to do this, they had to add knowledge from a wider field. As I have pointed out, this led them very generally to broaden their field of study, so that we modern institutionalists can reckon among our early precursors many in the classical and neoclassical tradition, at least until Alfred Marshall.

Crucial to the conception of being political economists, however, was the belief that there were objective values, that these could be known as facts, could be observed and analyzed, and so serve as basis both for the study of other facts and for rational and objective policy conclusions. This value basis for economic study was the prevailing moral philosophy—initially natural law philosophy and later utilitarian philosophy, the latter, as I have shown, being only a variation and reformulation of the former.

The neoclassical authors refined this approach and, in particular, its foundation in hedonistic associational psychology. As a matter of fact, the marginal utility theory of about a century ago stands out as the final refinement of the moral philosophy of utilitarianism. Many of the great economists of the era, such as Henry Sidgwick, also figure prominently in the pantheon of philosophers in that line of thought.

This development occurred at about the same time that utilitarian moral philosophy and, in particular, the basic hedonistic associational psychology were abandoned by the professional philosophers and psychologists. The very apparent isolation of economic science from the other social sciences and from philosophy dates from that time.

Welfare Theory

Modern conventional economists have retained the welfare theory that developed a century ago but have done their best to conceal and forget its foundation in a now obsolete moral philosophy and an equally obsolete hedonistic psychology. They have thus succeeded in pursuing what appears as an amoral economic theory, and they are proud of stressing this as "professionalism."

I thought I had finally disposed of the modern welfare theory some fifty years ago in a book from my early youth. In English it bears the title, *The Political Element in the Development of Economic Theory.* There I demonstrated the superficiality and logical inconsistency of this welfare theory, but it grows in conventional economics like a malignant tumor. Hundreds of books and articles on the topic are produced every year. But if that theory is not entirely meaningless, it has a meaning only in terms of a forlorn hedonistic psychology and utilitarian philosophy.

The disastrous neglect of the history of economic science in the curricula of our universities permits the ordinary economist to grow up unaware of the roots of his own thinking. Those earlier economists who originally developed the hedonistic and utilitarian welfare theory—foremost among them Sidgwick and F. Y. Edgeworth—could work with conviction and in clear terms, for they knew what they were doing. They were not apt to overlook the basic psychological and philosophical assumptions implied in welfare theory. The contemporary "welfare" theorists mostly lack the historical perspective which they would gain by intensive study of their predecessors and the awareness of where the basic difficulties lie.

I may note in passing that the recent flourishing of welfare economics is closely related to the growing predilection for overly abstract theoretical models. Among the many implicit and not sufficiently scrutinized assumptions of these models, and sometimes even in their explicit structure, the objectified welfare conception almost always plays a role.

Institutionalism

In one sense, institutionalists are generally political economists in the great tradition that began in the eighteenth century. For all of us, so far as I know, economics is a "moral" science in Mill's meaning of the term. While ordinary economists, like most other social scientists, now are what in the history of philosophy is known as "naïve empiricists," having convinced themselves that they are simply dealing with observable facts, we institutionalists have all been involved in the problems of how to account for the role of human valuations in research. Now that we no longer can accept as valuational basis for our research the outmoded moral philosophy and hedonistic psychology of our classical and neoclassical predecessors, we must account for what other valuational basis we do have.

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 approaches to a problem, the definition of our concepts, the choice of models, the selection of our observations, the presentation of our conclusions—in fact, the whole pursuit of a study from beginning to end. If we remain unaware of the valuational basis to our research, this implies that we proceed to reason with one premise missing, which implies an indeterminateness that opens the door for biases.

In this context I have argued for, and in my own research from *An American Dilemma* onward have tried to observe, the necessity in any scientific undertaking of stating, clearly and explicitly, the value premises which are instrumental. They are needed for establishing relevant facts, not only for drawing policy conclusions. Upon all the intricate problems of how to proceed in selecting and utilizing such a volitional element in research, I will not enter here.

In regard to this primary methodological problem I cannot speak for institutional economists as a group, except to assert that we are all political economists and therefore have all been involved in the problems of how to account for the role of human valuations in research and in policy prescriptions. We have also 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 virtually absent among the writings of ordinary economists. Looking back, these influences are more obvious. 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 above.

One common bias even in contemporary research 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.

A Broader Analysis

I have been attempting to argue the case for institutional economics in terms of logic. But I believe that in the near future it is destined to win ground at the expense of conventional economics, and not primarily because of the strength of its logic. Institutionalism will become more prevalent because a broader approach will be needed for dealing in an effective way with the practical and political problems that are now towering above and threatening to overwhelm us. I believe that much of present establishment economics, and in particular its very abstract theoretical constructs, which until now have enjoyed highest prestige among economists, will be left by the wayside as irrelevant and uninteresting.

Long before the oil crisis, indeed, almost since the end of World War II, most of the Western world was experiencing a rather persistent and on the whole accelerating trend of price inflation. For orderly countries in peacetime this was a new experience. Economists had generally adjusted their thinking to this new trend as a natural one; they had not inquired much about what causes lay behind it, and they had been satisfied to deal with the practical and political issues in terms of a trade-off between price inflation and unemployment.

The oil crisis, the concomitant short-lived raw materials boom, and the increase in food prices, caused by bad crops in large regions of the world, then suddenly brought about a very large rise in the price level. Despite the contractionist economic policies clamped down in most countries, this shock left as a legacy "stagflation," which meant an unprecedented, continuing, and rapid price inflation, concomitant with a high level of unemployment and a low rate of growth, if any. The persistent and extraordinarily rapid price inflation is a new phenomenon that cannot be explained by the higher cost of oil.

I cannot here embark upon an analysis of this very high rate of price inflation in the face of similarly unprecedented high rates of unemployment. I must restrict myself to expressing my considered opinion that, behind this new constellation, there have been changes of institutional and attitudinal conditions which, if observed at all, have not been fully integrated into our thinking.

As early as the 1930s the phenomenon of "administered prices" was observed and commented upon by institutional economists. The rapid trend toward consolidation of production, commerce, and finance into huge corporations implies the growth of the sector in which prices and wages become administered and are not closely dependent on a market. When, at the same time, these corporations become transnational and take the nature of conglomerates, an increasing proportion of economic and financial transactions takes place within these corporations themselves and can escape statistical or other registration and the control of governments.

And there are other such institutional trends, the effects of which on the economy are not easy to incorporate into the closed models of economic theorizing. Partly related to all this, there might be deeper ongoing changes in people's inclinations and patterns of behavior, about which as yet we have only scant information. A study of this whole field of institutional and attitudinal trends of change will imply a broadening of economic analysis.

The Underdeveloped Countries

The decolonization hurricane that swept the globe after World War II suddenly drew a large number of economists into the study of planning for development of underdeveloped countries, and I have been one of them. This was rather a new field, for until that time much interest had not been devoted to the economic problems of the so-called stagnant regions, where there were then no independent countries. It was natural that ordinary economists would use the models they were accustomed to use for the analysis of developed countries, without many qualms about whether they fitted these different conditions.

The concepts used—unemployment, income, consumption, savings, investments, supply and demand in markets, and so on—were inadequate for describing reality in the underdeveloped countries. Hecatombs of statistics were compiled and utilized that were not worth the paper on which they were printed. Their deficiency is not due merely to the inferior quality of the statistical services in underdeveloped countries, although that is important, but more fundamentally to the false categories under which facts are observed and recorded.

I have already complained about the carelessness in using statistics by ordinary economists. In studies of underdeveloped countries this carelessness reached a peak, and unfortunately this criticism is still valid for most of the economic literature on the subject. I am not concerned about the criticism of my book, *Asian Drama,* on the basis that it contains so few figures, although it contains much criticism about figures and the way they are compiled and utilized.

My basic criticism of the conventional approach to the study of development problems of underdeveloped nations is directed against the uncritical use of models worked out for developed countries. To give an example: In the developed nations, where relatively high levels of living prevail, even for the poorer classes, partially sustained by systems of social security, it may be permissible to use, as an approximation, development models that do not include consumption. In underdeveloped countries, where masses of people suffer severely from nutritional and other insufficiencies of consumption, which is an important factor in labor productivity, it is not permissible. This analytical error becomes accentuated when it is assumed, as is often the case even in some recent writings, that increased savings, even by the poor masses, would increase the possibility of development.

In the past, development was commonly understood as simple economic growth, regularly accounted for in terms of very questionable statistics on gross national product or income. Social reforms in underdeveloped nations to increase equality in these ordinarily very inegalitarian societies were reckoned as expensive and as hampering development. They should wait for a rise of production. In recent times, distribution has been given more attention, and the compatibility of growth and increased equality has been questioned and sometimes denied. Nevertheless, the result seldom has led to more fruitful conclusions for the dynamic problem of planning. The poor do not need a little more money. In any case, tax evasion among those better off is usually colossal in underdeveloped countries, and more money could hardly be provided without spurring inflation that hurts the poor. What the poor do need are radical institutional reforms on a scale far outside the field enclosed by reasoning about growth and distribution accounted for in terms of money.

In agriculture, what is needed is a changed relation between man and land, including not only land reform but also organized collective work to make the many investments of labor needed for raising yields. In education, the problem is not only one of increasing the number of children who attend school, but also of changing goals and methods of teaching. At present, much school education is actually antidevelopmental in its effects, and not only because it fosters a contempt for manual work. What I have called the "soft state" must be overcome. In particular, corruption must be fought effectively.

The important thing about these types of reforms is that they are needed to achieve both economic growth and greater equality, two goals that in underdeveloped countries are inextricably joined, much more so than in developed countries.

Economists carry their share of responsibility for the neglect of institutional reform because their analyses usually have disregarded "noneconomic" factors. Hundreds of books and articles are still published which make no mention of such important facts of life as widespread corruption, which even seems to be increasing in most underdeveloped nations. Problems related to land reform, the need to strengthen administration and redirect education, and the organization of health work have seldom been the focus of conventional economic analysis and have not been dealt with from the perspective of both growth and decreased inequality. This biased view is in tune with the inclinations of the tiny economic, social, and political elites which, rather independent of constitutional arrangements, dominate most of the underdeveloped nations and hinder or distort efforts to institute these reforms.

Conclusion

My discussion has focused on the logic of institutional economics. When I say that it will gain ground in the near future, it is, as I said, not for the reasons I have outlined above, but because of the failure of ordinary economics to come to grips with the bewildering, new, and momentous policy problems with which the world is faced. Ordinary economists, in their search for solutions to these problems, will have to broaden their approach and seek for causes outside the customary confines of economic research.

For the last century, as I pointed out, economic science has tended to become isolated from the other social sciences, and this isolation has hardened in the postwar period. In our journals and at our universities economists have worked in a purified model world, and even their empirical studies have borne the mark of self-sufficiency and sometimes sectarian arrogance. Because economic conditions are so important, and because economists have had a "theory" for two hundred years, we have enjoyed prestige, and there has even been a tendency in some other social sciences to devise similar closed models.

When economics becomes institutional, however, that isolation will be broken, to the advantage, I hope, of all social sciences. There will be less opportunity for doctrinaire approaches when, in principle and in practice, we all are led to an holistic approach.