Balázs Hámori:

The Relevance of New Institutional Economics

Since the publication of Milton Friedman's famous methodological article (*Friedman* [1953] [1986]) there has been consensus among economists about the idea that scientific theories should not be tested on how realistic their *postulations* are. As Friedman himself puts it: 'The more significant a theory is, the less realistic its postulations are.' (*Friedman* [1986] p.27). The relevance of a theory can be tested on whether its *statements* or theorems *can be considered as good approximations to reality* rather than on the true-to-life nature of its postulations. '…the correctness of a hypothesis can only be tested through the reality of its *conclusions* and *predictions*'. *(Friedman* [1986] p. 48) Well, if we accept this starting point, then this criterion can be regarded as the self-criticism of the profession. If we test several acknowledged theoretical achievements according to this criterion we are forced to face the fact that the extremely abstract *postulations* used for building a model do not have much to do with reality (although this, in itself, could be acceptable). There are also problems with their *generalization*, with their *final conclusions* drawn from theoretical examinations and the *predictions* based on them. They do not always stand the test of practise. Let us only mention two examples taken at random to support this. *One year* before the falling down of the Eastern European dominoes and *three years*[1] before the end of the Soviet Union, the complete collapse of the socialist system the founding father of economics calls the economic growth in the Soviet Union since the twenties *impressive* (!) (*Samuelson - Nordhaus* [1988] p. 1101). The final conclusion of the core coursebook taught in every corner of the world is expressly shocking: *'Perhaps the most important lesson to economics is that the plan-command economic system works*.' (p. 1095) Another example of the relationship between predictions and reality is as follows: one of the emblematic economists of the age, *Irving Fisher,* in October 1929 predicted that the decline of stock markets would be over within a few days at most (Goldfarb - Leonard [2002] p. 24)

Mainstream economics has not drawn nearer to reality during the three quarters of the century since the Great Depression or during the 15 years since the change of the regime. While it casts its more and more abstract theories into more and more elegant models, it does not excel in giving answers to real problems. Can theoretical economics, for example, formulate any practicable statement about the fact that after the officially announced end of the American recession - *despite increasing production* - 2 million people lose their jobs? This is contrary to all standard economic wisdom, and yet it is an irrevocable fact. Can statements claiming that the economic system is gravitating towards equilibrium be regarded as good approximations to reality if the wealth of three people or

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[1] The 12th edition of Samuelson's Economics came out in 1985 in the United States, and this was almost immediately followed by the publication of this edition in Hungarian.

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families (the Waltons possessing Walmart chain of stores, the sultan of Brunei and Bill Gates) equals the total GDP of 43 countries (!)? Can't, in the light of all this, be stated that the world is coming nearer to equilibrium? In most cases economists cannot answer questions like this. Apart from some respectable exceptions the representatives of the mainstream are at a loss with the new economic developments which turn our everyday life upside down, and at the same time they make the traditional questions to be addressed by economics alarmingly inadequate. The annoying lack of realistic questions is probably related to the fact that the majority of economists do not try to answer truly serious, unsolved questions but make every effort to generate (pseudo)questions that on the basis of their research traditions they are presumably able to answer.

Is it necessary that economics should be so far from reality? Is it necessary for it to stick still to the stable environment and as a consequence to the predictability of the world (at least on a probable basis) when it formulates its theorems if, in reality, we are surrounded by a complex, rapidly changing environment and we live in constant uncertainty. Is it necessary that in the focus of the profession still be mainly *scarcity* and *limited resources* while the economy is being moved by a theoretically unlimited resource: information (knowledge)? A resource that cannot be scarce as 'the individual cannot lose it by passing it on' (*Arrow* [1979] p.372). Can we fill the pages of journals with more and more elaborate models of equilibrium based on negative feedback without a quiltz conscience, while we can experience positive feedback and chaotic developments more and more often not only in income distribution but also in other processes of world economy? The question is obviously rhetorical.

With the publication of the famous article by Ronald Coase [1937] an economic school appeared on the stage. It is in a much closer contact with reality and the empirical world than traditional economics without rejecting the latter's truly acknowledgeable high theoretical standards. Although the elaborateness of the tenets of the institutional school cannot compete with the theories of mainstream economics which have been refined through centuries, accepting its starting points we can understand the world we live in much better than by relying only on the mainstream: the horizons of new institutionalist economics are much wider those that of mainstream economics, both in space and time and in its methodological arsenal. In several respects mainstream economics is even today based on the Keynesian witticism ('in the long run all of us are dead') when it devotes *disproportionately much space* to short-term market equilibrium while it has almost nothing to say about long-term processes. Beyond dispute, the mainstream is right in stating that in the short run and at micro level a kind of equilibrium always comes about. It would be a big mistake to project this to the long run and to macro level,[2] let alone to global economy. If, for example, we consider the major changes due to the technological revolutions, we cannot talk about equilibrium of any kind. And unless the world is identified with the United States, we cannot talk about equilibrium; if the scope of the applicability of the economic theory is expanded to the globe from Tierra del Fuego to the Spitzbergen, from the Silicon Valley to the Gobi desert. The microeconomic theory undoubtably offers an elegant description of the allocation of resources at a given moment; however, it has no explanation for why there is nothing to allocate in Africa.

[2] Although exactly this is done by the so-called micro-based macroeconomics.

There is no explanation either for why there are more per unit resources today in Ireland than in Great Britain when one and a half or two decades ago this was just the other way round. How could all this be understood if we did not take into account the shifts occurring every ten to fifteen years in technology and corporate government as well as changes in the political and legal environments over centuries? How could we explain, on the basis of models of equilibrium, positive feedback present in technological advancement or the periodical differences in the pace of progress? (The radio took forty years to become widespread whereas the internet took only four.) In other words: neither *technological shifts* nor *history* nor *institutions* can be ignored if we want to surpass the witty experiments of thought - which engage thousands of theoretical economists worldwide. If - according to the intentions of Milton Friedman - we want to draw conlusions applicable in practice (and also verifiable in practice), and we want to make predictions valid longer than a few days, we must go beyond the scope of investigation delimited by the market.

By expanding the boundaries of economics both in time and space, the new institutional economics casts its eyes beyond the neoclassical horizon, but at the same time it keeps the principles of the mainstream, its firm commitment to *rational analysis* and *methodological individualism*. Besides the decisions of individuals, this trend considers the fields connecting individuals to be of great importance (knowledge-sharing networks). It interprets rationalism as limited (rather than extreme) rationalism, taking into account cognitive barriers. "New institutional economics … does not ruin the building of classical economics but it expands it and makes it cosier. So, in this respect it cannot be considered a new paradigm. … Rather the fact is that *new institutional economists*, who, at the beginning, were said to be theoreticians of heretical ideas or the devil's advocates, *have been building the protective ring of auxiliary hypotheses around the inflexible mainstream economics*. This makes it possible for us to expand the validity of our discipline far beyond a narrow, special case (the market described with strict conditions) and also to reinterpret the rational decisions themselves without simplifying them to maximalizing behaviour." (Szabó [2003])

Hungarian economics developed isolated (though not hermetically) from the international trends during the fifty years before the change of the regime. No wonder that in the flourishing era of reform economics the perception of the new institutional discipline was very weak *(Szakadát* [1996]). However, what the most successful research in economics brought to light by analyzing the circumstances in the bygone socialism was not far from the ideas of the new institutional school. It is also for this reason that it cannot be considered an accident that in 2002 the Hungarian Society for New Institutional Economists (HUSNIE) was founded with the active help of International Society for New Institutional Economics (ISNIE). On April 28th and 29th the association embracing the representatives of new institutional economics organized already its third international conference. In this volume the reader can choose from among the high-standard lectures given at the conference in Debrecen between November 2nd and 4th organized jointly by the European Association for Comparative Economic Studies (EACES), the Hungarian Society for Institutional Economics (HUSNIE), the Faculty of Economics of Debrecen University and the Information-economy and Identity OTKA School of Science. The significance of the conference is marked by the presenters of the plenary session on November 3rd: Daniel Danianu, the Romanian president of EACES, Silvana Malle, the director of OECD or László Csaba, a professor at CEU, the leader of the PhD school at the Faculty of Economics, University of Debrecen. Professor Xaviet Richet (Sorbonne), a leading personality of EACES, also joined the discussion. Participants could also listen to the talk given by a Malaysian economist, a self-proclaimed institutionalist. Perhaps even more important is the fact that there were a good number of young people: PhD students and researchers at the beginning of their careers. The fierce debates after the talks were also reported about by the journal Közgazdasági Szemle. After gleaning in the conference material the reader might also come to the conclusion that this introduction closes with: although in Hungary new institutional research programmes do not have a significant history, they inevitably have a future.

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