

Economic Sociology in East- Central Europe: Trends and Challenges*

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Five years ago in Dubrovnik (before the Inter University Center was battered down), Richard Swedberg first gave us a lecture on new economic sociology. His arguments were convincing and covered an extensive body of knowledge. However, I still have a vivid memory of a critical remark made by one of our colleagues, Witold Morawski, who claimed that the lecture was about American economic sociology rather than economic sociology in general. The years that have passed have brought with them significant changes in society and important scholarly studies; anyone who still questions the legitimacy of the term economic sociology would be convinced by the arguments in the *Handbook* edited by Smelser and Swedberg (1994), and time has come to revisit the notion.

Thus, my initial intention was to ask Richard Swedberg for yet another summary (all the more since I know he is working on one), and to encourage Jonathan Gershuny to outline Western European ideas, while I would attempt to describe some Eastern European trends. The result was half-success, half-failure. Understandably, Jonathan refused on the grounds that he would rather talk about what he is currently interested in. And, despite years of contact with a relatively large number of Eastern European economic sociologists, I began to realize that my own knowledge was not sufficient for a summary. Nevertheless, I have recognized some problems which I would like to share.

First of all, although it is true that since the 1950s the most important workshops of economic sociology have been confined to the American continent, Eastern-Europe is not lacking in achievements. On the contrary, too much has been written or researched for me to explore it all thoroughly. At the risk of overgeneralizing, I will refer to certain problems instead of presenting an extensive bibliography.

The fact that Károly Polányi, his brother, Mihály, George Katona, or the economists Tibor Scitovsky and John Harsányi all have their roots in Eastern-Europe is quite well known in the profession, as are the regional ties of Maria Hirszoicz, Iván Szelényi or

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János Kornai, since their frame of reference is Eastern Europe. Do they have anything in common? The most striking clue is negative: it was not in their homeland that they won fame. That they left their country and that they achieved international reputation are not automatically related facts. Many emigrated and few became famous. For instance, Oszkár Jászi was at home a foremost figure in the sparkling intellectual life at the turn of the century, yet met with indifference at a small American university. And most of them (though not all), emigrated not to seek a positive scholarly or financial alternative but to escape a negative existential one.

In the twentieth century, Eastern European social sciences lack internationally acknowledged authorities with solely a domestic heritage. We do not have a Weber, Durkheim, Pareto or Spencer; we have a Polányi and a Szelényi, both of whom had to emigrate and gained international fame abroad. In this they may have differed from German emigrants between the world wars, most of whom already had an established name in their field at home. The intellectual baggage of these scholars is not easy to determine. As secretary of the Galilei Circle formed by radical students, Polányi was evidently influenced by the Hungarian liberal movement early in the century and also by the ideas of *Huszadik Század* (Twentieth Century), a journal edited by Oszkár Jászi, who was himself in contact with Durkheim and Spencer. Polányi's attitude expressed criticism towards the remnants of feudal institutions in a society with a strengthening middle-class and a social sensibility. With unusual solemnity, Polányi wrote in a memoir: "What I have become, I became on Hungarian soil. Hungarian lives have given meaning to my own. Whatever mistakes I may have made were paid for by others. Whatever good I have ever meant should reach perfection here. What little I have given the world should return here" (Polányi 1960, quoted by Gyurgyák 1981). Nevertheless, it seems that, for the most part, Polányi's intellectual influences from home may not have encompassed much more than value choices, friendships, and perhaps features of thought—that is, a certain style. (This style is also apparent in the works of Katona, Gerschenkron or Szelényi.) His theories tend to reflect the influences of Max Weber and, particularly, Karl Bücher, rather than those of Hungarian thinkers.

However, one quality that may be regarded truly as an intellectual legacy is one shared for the most part by both an Eastern European and a German sociological heritage: *a sensitivity to historical asynchronousness*. To elaborate on this idea, let me refer here to two Hungarian authors who were never part of the international scholarly scene. (In fact, the pattern for Eastern Europeans looks like this: if you emigrate, your chances of becoming known are one to a hundred. If you do not emigrate, your odds are of ten-thousand to one that you remain unknown. With his posthumous reputation, Chayanov is the only exception known to me.)

SIMULATED CAPITALISM AND DUAL SOCIETY

One of the authors whose work displays a sensitivity to historical asynchronicity was a strange Hungarian thinker, from the turn of the century, Lajos Leopold. He was a landowner, thus an amateur thinker which at the time hardly suited the sense of professionalism in domestic social sciences. His work on prestige, however, proved Leopold to be a thinker of great insight rather than a dilettante. Even in Hungary, generations have been

unaware of his achievements. His essay, "Simulated Capitalism" – which he started to write during the First World War and which went into print as part of a collection of economic-political articles in 1917 after his discharge from the army – was published again in the eighties in the university journal *Medvetánc*.

Leopold makes a distinction between two types of capitalism: one in which a legal framework and a capitalist economy are both present, and the other, in which serfdom and guilds have been abolished, the workforce is independent and private property is dominant but lacks a capitalist economic content. Such is the relationship between *organic* and *simulated* capitalism. Some countries are made up of either one or the other, but in most cases the two are combined: even "the City of London has pieces of simulated capitalism." (Leopold 1988: 323)

What the author calls the *content*, the *idea* or, elsewhere, the *logic* of the economy coincides with the inner motives of the capitalist – that is, aiming for the most effective financial existence and destroying whatever social structures impede growth. This aim is missing from simulated capitalism, explaining the increased presence of parasitic attitudes.

Wherever it is the dominant form, simulated capitalism emerged under political, economic and ideological pressure by Western countries. Therefore, the internal inequalities in the capitalist system were also reproduced within international relations. Badly in need of Western loans and markets, Eastern European societies were in a position analogous to the dependence of workers in the West. Make-believe societies emerged with a capitalist legal system, a disproportionately large bureaucracy and armaments, and an aimless diplomacy. Hungarian development was characterized by the fact that its creditors were its immediate geographical neighbors. Another feature of simulated capitalism was the paralysed concentration and regional centralization of the workforce and capital. This led to the creation of an urban banking and industrial elite who monopolized positions and held onto them through nepotism – a dominance which was impossible to counter, particularly in the countryside. Furthermore, a significant part of the land was parcelled out or transferred to tenants, which served the purposes of short-term profiteering but did not encourage the establishment of long-term capitalist agriculture.

In simulated capitalism, the division of labor is unbalanced, with part-time tradesmen outnumbering skilled workers. In order to sustain a living, tradesmen in the countryside were forced to deal with peasant workers as well. The logic of simulated capitalism encourages a technique of negotiating favorable contracts and manipulating economic policy. While techniques of central financial affairs and foreign loans advanced by leaps and bounds, elements of an internal capitalist structure failed to keep in step. Even as state funds generated social needs, these public funds were controlled by officials engaged on the basis of party merit, and were therefore insensitive to the potential economic effects of their actions.

Leopold's tone is self-assured, unlike that of many dilettante rural thinkers. At certain points the influence of German sociology is obvious, but his critical social analysis is trenchant. He has a rich store of experiences and seeks a way to explicate them more systematically than in a political essay. He focuses on problems, but also finds appropriate analytic viewpoints and categories. While his elaboration is evidently instructive even today, there is something else that makes me cite it in such detail. What renders it East Central European is not only this critical social acumen, even though it is a significant

tradition; more important still is the problem-orientation, the need to concentrate on issues that are socially relevant.

The other thinker, Ferenc Erdei, who came from a peasant background and led a controversial life, was the most prominent young representative of Hungarian sociology and socio-critical sociology in the interwar period. Later, like many of his predecessors, he took an active political role – both in the crude collectivization of the fifties and in the preparation of the agrarian reforms of the sixties – but this fact has no relevance for an evaluation of his early theoretical work. At the core of his argument, which was conceived during the war years and first presented at a radical third-party meeting but only published posthumously in the seventies, is a thesis of a dual social structure (Erdei, 1988 [1974]).

Erdei's starting point is similar to Lajos Leopold's: while in the West, the evolution of productive technology and social structure was intrinsically motivated and synchronous (though individually coloured), East European societies became capitalistic through external influence in the second half of the 19th century and thus developed asynchronously. Production and trade developed following the capitalist model in every sector, but society as a whole failed to transform accordingly. On the one hand, feudal social groups remained unchanged and were superimposed upon the new economic structure. On the other hand, new social groups emerged, which differed from those in the West in that they were not the continuation of historic middle-class social formations but were newly arisen.

Erdei argued that in the interwar years, a "historic-national" society, the successor to the society of estates, was the dominant society in Hungary. The social institutions that had existed in feudal society – large estates, the state administration and the church – were patterned upon historical models. However, at the same time, industry, trade, and financial affairs were modelled on bourgeois patterns, and moreover, the population of these sectors was made up mostly of foreigners. This dual society had two consequences. The first was related to urbanization, during which the lifestyles of the petty bourgeoisie and the working strata reached a modern level of civilization, while that of the peasantry, at the bottom of historic society, did not. Secondly, though the bourgeoisie's conception of progress tailored to western models was opposed to the national conservatism of the historic society, it proved too feeble, and the new bourgeoisie tended to assimilate to a gentry lifestyle.

Another interwar development was that the state's role increased to become the third independent factor of power besides large estates and big capital, especially in the depression years. Traditionally, this bureaucracy provided careers for the nobility with the consequence that the gentry, whose mentality opposed the threat of totalitarianism, at the same time obstructed professionalization.

The other pillar of historic society was the large estate, which between the two world wars, though having lost several former privileges, still preserved feudal elements in regulating labor relations and maintaining a high number of servants. Its levels of mechanization were low and work organization limited. By nature, the large estate reproduced the feudal structure, meaning that the large estate owner still rose highest in society between the two world wars. At the same time, the lowest stratum of the peasantry consisted of servants in large estates. Estates thus comprised the greatest social differences.

Not having developed gradually, capitalist entrepreneurship, the basic pillar of modern bourgeois society, could not utilize formerly accumulated expertise and instead pen-

etrated society through foreign capital and mass labor. Under these new circumstances, the organs protecting the working strata's interests were late to develop, but even so they proved more effective than the organizations safeguarding the interests of the peasantry. Despite the economic recession and lack of domestic capital accumulation, industrial capacity considerably increased in the interwar years. Erdei noted that at this juncture the traditional gentry society began to blend into the capitalist elite, while the mass agrarian population drifted towards the working classes. Thus, the walls partitioning historical and bourgeois societies began to crumble. Nonetheless, up to the second world war, these processes barely modified, let alone put an end to, the basic formula of the dual social structure which had earlier been solidified.

DOES THE THEORETICAL LEGACY HAVE ANY EFFECT?

The postwar turnover to state socialism seemed to neglect questions of both simulated capitalism and a dual social structure, not only on account of the predominance of a teleological ideology's vision of society, but also because in the early years of the planned economy this ideology proved to be inadequate to explain social phenomena. From the late sixties, however, criticism of state intervention appears to have influenced the endeavors of historians such as Iván T. Berend, Péter Hanák, and György Ránki, while the thesis of the dual structure reemerged in the works of sociologists Iván Szelényi and Tamás Kolosi. The new conceptual framework emerged, not as a reinterpretation of the duality of historical and bourgeois societies, but as a scheme to explain the structuring roles of the first and second economies or the types of income-generating activity directly controlled by the state and those outside state control. This scheme was related to interpretations of Polányi's concept of redistribution, whose influence was most powerful upon critical sociology (first of all via György Konrád's and Iván Szelényi's book on intellectuals) and upon economic thought (as is indicated by János Kornai's distinction between bureaucratic, market, ethical, and aggressive coordinating mechanisms). It can be seen then that certain elements of Hungarian sociological tradition have had some influence upon approaches and conceptualizations, though without direct explanatory force. This suggests that there is a sort of cumulative dynamism in East European social sciences.

That said, one is even more justified in asking how successfully we can invoke this theoretical tradition under the circumstances of present-day social transformations. In Hungarian economic sociology, the theoretical legacy has not been reconsidered, nor have alternative theoretical frameworks been created. Instead, sociologists analyze a relatively wide spectrum of problems focusing on new economic actors, (such as entrepreneurs, economic elites, the unemployed), while methodologically well-formulated panel studies try to plot changes in the behavior of households and enterprises. Subtle analyses point out the emergence of historical schemes of embourgeoisement and entrepreneurship (Szelényi 1988), but most native works are less systematic elaborations of randomly chosen problems.

May one venture a conclusion that the period we live in is ill-suited to synthesizing? Social phenomena are often amorphous, concepts are vague, facts rapidly outdated; furthermore, so many phenomena are encountered that are relevant, but at first incomprehensible, that we can rarely apply ready-made conceptual schemes to explain them. This

should, in turn, be even more challenging for theories. Some attempts have been made at applying theoretical frameworks (Csontos, Czakó-Sik), but only the first steps have been taken.

Another reason East European economic sociology is underconceptualized and chiefly descriptive-reflexive stems from the hunger for information by the political arena and the public. This extraordinary demand for data and information requires rapid and plausible interpretations. Furthermore, science cannot aristocratically brush aside these demands, for the information that scholars possess might contribute to the early recognition of possible negative consequences of certain decisions and to the mitigation of social tensions. For these reasons, participants in the Hungarian household panel study inform the MPs annually of their research findings, and young Russian specialists have met with the president directly to reveal their views (Radaev 1994).

Our Czech colleagues have also reflected upon the public demand for information (Vecernik, Mateju), as their work highlights economic successes and inequalities, income-generating strategies and labor market positions. In Slovakia, the elite paradigm is applied, while in Croatia interdisciplinary research is conducted to explore the relationship between human capital and entrepreneurship (Szomolányi, Stulhofer). In Bulgaria, one of the central research topics of economic sociologists is the attitude towards privatization (Nonchev, Stoyanov, Kostova). In Romania, investigations also enlarge upon entrepreneurship, unemployment and modes of subsistence, with case studies and qualitative anthropological field work taking a large role in research. In Russia, the questions of the second economy and, more recently, corruption are a focus of interest for economic sociology. In Serbia, the war gives special emphasis to crisis, techniques of survival and the black economy, but there are investigations into the survival of the old elite and the spread of entrepreneurship (Lazic, Bolcic).

Perhaps it is Polish economic sociology that demonstrates the greatest need in this region for transcending direct reflection. A sign may be the large amount of foreign language publications, meant by authors to be placed within the framework of the international professional community instead of the domestic public (Rychard, Morawski, Federowicz, Adamski, Kolarska-Bobinska, Kaminski). This may be merely in keeping with tradition, however, since Polish sociology, unlike the rest of Eastern Europe, has produced foreign language publications regularly in harder times as well. (One such series, named *Sisyphus*, earned credit for explicitly dealing with the crisis and even at the time of martial law, an issue featuring selected interviews with Gdansk strikers was available.) One finds that a drive for theoretical conceptualization is stronger in these works. It yet holds true that they are not paradigm- but problem-oriented, meaning that their concepts are adjusted to the questions posed by empirical phenomena.

We are talking of an "East European" economic sociology, but we must be aware that up to the most recent times, this tradition has been fragmented. National sociologies in the region rarely reflected upon each other. As Antonin Kaminski noted in a personal conversation, there is something paradoxical in knowing more about the current developments of western sociology than that of our immediate neighbors. This has been somewhat modified recently through conferences, workshops and some joint research, and the conscious effort to publish overviews of sociological trends in the countries of the region in professional journals. Yet the fundamental tendency has not changed. What has changed is the relationship of the sciences to politics. After the regime change, politics

drew off many intellectuals, and not the worst of them. Hungarian economic sociologists Bálint Magyar, Pál Juhász, Gyula Tellér, and economists Márton Tardos and Károly Attila Soós became MPs. They were acknowledged specialists, similarly to the historians Miklós Szabó, Iván Pető and György Szabad. Their choice to enter politics must have partly been motivated by the strong tradition of social criticism in the social sciences. Not unlike Oszkár Jászi, who undertook a ministerial position during the short-lived democratic government of 1918, several prominent personages of contemporary scholarship have been prompted by a sense of public responsibility to assume political roles. Some have held that the scholarly role of these new politicians was only a disguise masking their formerly inhibited public aspirations. This may apply to a few, but not to the majority. Their decision was more likely the combined result of the nature of the challenge, a tradition of social criticism, and the compatibility of scientific and public roles.

SOCIOLOGICAL IMAGINATION LOST

In Mills' definition, sociological imagination is the significant capacity "that enables us to grasp history and biography and the relations between the two within society." (Mills, 1980 [1959] p.12). This is an early and precise formulation, by an author now out of fashion, of the problems of the micro-macro link – the necessity of investigating the dynamic interaction between personal and social changes. It also implies a promise that social science will contribute to the general understanding of everyday social phenomena.

Taking Mills' statement further, one may pinpoint two elements defining the sociological imagination. One is related to the researcher's habitus and might be termed the capacity for *wonderment*: an openness and sensitivity to unexpected social phenomenon, or an ability to perceive crucial and intriguing social phenomena, allowing them to call into question pre-existing models. This ability prompts the researcher to perceive phenomena outside the interpretative field of a paradigm; to be interested in and ponder over problems formulated by others, even if they cannot be immediately captured by conceptual schemes – to see the world at large, as a whole and not only through the paradigm. In other words, the sociological imagination allows the researcher to sew the button to the coat, not the coat to the button.

Secondly, a sociological imagination today also means the recognition of what James Coleman called the unwanted side-effects of "constructed organizations." I suspect that a greater part of relevant social problems coincide with these phenomena. Not all relevant social phenomena can be derived from the unwanted side-effects of constructed organization, but all the side-effects of constructed organizations are relevant social phenomena.

Paradoxical as it may seem, in Eastern Europe the development of spontaneous organizations takes place amidst the framework of these constructed organizations, and are conditioned by them. Perhaps this paradox is not so unique, since in Coleman's interpretation, contrary to Hayek's, a similar development characterized the West. What fascinates the sociological imagination in Eastern Europe is the speed at which the mixture of constructed and spontaneous organizations is emerging, and the immense social pressure that precipitates it. It is easy to see what increased possibility there is for the production of unwanted side-effects and how great are the risks this implies in an instable medium.

It is not the job of the social scientist to promise a better future – and East European colleagues are indeed most averse to such a job, nor could they find a less popular role in view of the past decades. However, meaningful predictions might be formulated, as exemplified by demography, of social and economic institutional changes, and the social conditions and effects of economic changes (Illner 1993).

PARADIGMS AND PROBLEMS

We must not forget to be aware of social problems – not only problems in the sense of indicating danger and generating tensions (Coleman's unwanted side-effects included), but problems in another, broader sense: phenomena that are not easy to grasp at first, that are a source of wonder. In other words, problems as a set of side-effect syndromes are fewer than startling social phenomena, but, being unwanted side-effects, are sure to be hard to understand and not always easily traced back to their origin.

The difference between paradigm-oriented and problem-oriented (or exploratory) research is not merely a difference between Eastern and Western sociological traditions. It is also a difference between researchers' attitudes. One is aimed at constructing a theory, implying an effort to work out a set of exact concepts suitable for explanation and free from blurred or slippery notions. In the other case, one is faced with a momentous social phenomenon that calls for interpretation. When does it arise, what causes it, what effects does it have? What one often misses from paradigmatic thought is an ability to wonder at social phenomena and a reality-centered thinking. To be more exact, there are some relevant social problems at the fountainhead of nearly every contemporary paradigm, but as the conceptual stock gets richer and richer, these are replaced by ever more artificial problems. The spectator may have the impression that the paradigm devours reality, and reality shrinks to become an artificial illustration.

After Giddens' critical analysis, one may bracket Kuhn's normal science concept and the conclusion that social science has no dominant paradigm (Giddens 1993). It does have sets of concepts, taken-for-granted understandings, hermeneutically grasped descriptions, that is, frames of meaning (Giddens 1993: 149) which are shared by smaller or larger communities of contemporary scholars. Somewhat off Kuhn's line of thought but in line with reality, one might call these social science paradigms. This comes close to Merton's conception, who understood paradigms to be presuppositions, concepts, procedures and the relations between them. It is quite right that Merton regards the screening of vague presuppositions and uncodifiable conclusions as the main function of a paradigm. At the same time, he warns of a worrisome aspect gradually forgotten by now: a paradigm may generate intellectual laziness, the sociologist may ignore data that do not fit the paradigm (Merton 1980).

The primary problem raised here is not the relationship between theory and practice, nor the social usefulness of social science. (This is also a legitimate issue and I agree with James Coleman who claimed that social science had much to reconsider in this regard.) The principal problem is that given a variety of alternative paradigms, each of these sets of concepts can only be applied to a narrow segment of reality. When I asked the late James Coleman in 1991 about the empirical feasibility of the rational choice approach, he said that the weak point of the approach was that it fails to break away from the circle

of illustrative empirical applications. Even if it is only a question of time, (and the exponents of network analysis or time budget analysis have another opinion of their own paradigm), I am afraid one may find several relevant social phenomena that do not relate to any currently prevalent paradigm, that is, cannot be explained completely through them.

THE CRITERION OF SOCIAL RELEVANCE

Contemporary American sociology faces with a similar need for problem-oriented research. Most recently, the reviewer of a series of books for university students suggested that nowadays sociological texts are all but targets of ridicule in jokes about senseless training. The reason is that sociologists constitute a more and more closed reference group, talking to each other and not participating in public discourse, not observing criteria of social relevance (O'Brien 1995).

For economic sociology, social relevance and public discourse are indeed two key categories. Let us pause here for a moment: the two categories do not necessarily overlap. Some of the questions raised in public discourse are indeed socially relevant, most having been exposed and formulated by science and supplied with the necessary conceptual instruments required in public discourse. The majority of phenomena are well-known and possibly easily understood, for unlike natural science, we are working on themes experienced by everyone, themes in which everyone is more or less a specialist, which is an advantage rather than a disadvantage. What we are talking about is something everyone has an opinion about, for they feel they know something, even though no one knows much beyond their subjective experience.

However, a significant part of the questions raised in public discourse are not necessarily socially relevant. It suffices to take a look at the dominant channels of public discourse – television, radio and the daily press – to realize that a great part of the information, though interesting, is not socially relevant. This is the trap of the public's spiritual consumption: the daily wrangling of politicians, the vanity of stars, the efforts of interest groups or the spiritual shallowness of the media, determine the topics of interest that rush aside the problems of everyday life, the distressing concerns of our narrower or broader environment, the questions that require mental effort.

How does one recognize socially relevant questions then, if science enlarges upon socially irrelevant issues, if the majority of problems raised in public discourse have little social relevance? It is true that I have stated that if science develops paradigmatically, it does little good for a sense of reality. Science, however, exposes and interprets socially relevant questions as well. The irrelevant topics of public discourse are not only an outcome of the dictates of the media, but also public discourse picks up on and mediates problems thematically defined by science, with a time lag. Science is the avant-garde of public discourse, as it were.

But, in order not to evade the question: "What is a socially relevant problem?" one may at first suggest that every global or social phenomenon significantly influencing the reproductive processes of the whole or the principal groups of society are socially relevant. Obviously, such phenomenon are mediated by the dominant channels of public discourse as well, let alone the marginal channels and everyday communication. This is

the point that may be of help in working on a theme that is part of everyone's experience. This, however, is only a corollary statement: the criteria of social relevance is not that it has to be a subject of everyday conversation.

On the other hand, a social phenomenon cannot be explored like a natural fact. In a certain sense, there is nothing new for a social scientist because every fact he studies has been lived, experienced, possibly known and interpreted by people. Scientific experience differs from this first-hand experience by being more systematic, suitable for comparison and a deliberation of broader consequences, thus for predictions. Anyone who first identifies a social phenomenon, assesses its weight and effects, discovers something and thus carries out a scientific achievement. The implied ability to translate it into everyday knowledge helps define social relevance.

ECONOMICS AND SOCIOLOGY

A considerably sharp dividing-line has existed between economics and sociology in the development of western sciences, until recent decades. According to the convincing arguments of Boudon and Bourricaud (R. Boudon-F. Bourricaud, 1989), this line is the product of the circumstances under which the two disciplines emerged: economics was born at the zenith of the philosophy of the Enlightenment and utilitarianism, while sociology came to life during the romantic reaction to the former.

The historical and ideological differences implied by rigid hypotheses began to decrease only recently – though there have always been diverging views – because, I think, the ideas shaped in the pace-setting workshops and paradigm centers of economics and sociology have drawn closer to one another. This rapprochement is exemplified by the joint research seminar by Becker and Coleman on the sociology of markets. Prior to it, economics used analyses based on utilitarian assumptions to successfully explain phenomena which were earlier subsumed under the heading of sociology. This was regarded by many as a sign of economic imperialism (Swedberg, 1990).

Another precedent was that via the crystallization of capital theories, sociology had reached a phase facilitating the formation of paradigms, and it proved through the sociology of markets that it could contribute not only to the neo-classical but also to the institutionalist concept of the market. Earlier, the prevailing opinion – succinctly summarized by the following saying attributed to James Duesenberry – was that “economics is all about how people make choices; sociology is all about how they don't have any choices to make” (quoted by Bowles, 1995), absolutizing the individualistic-rational viewpoint of economists and the holistic viewpoint of sociologists. What is more important here, as Bowles rightly notes, was that economics was traditionally interested in horizontal relations between actors. It is no exaggeration to say that sociology, on the other hand, was interested in hierarchy rather than the market, in power rather than ‘truck, barter and exchange.’ Since then, by virtue of neo-institutionalism and economic imperialism, economics has taken an interest in several topics that fall beyond the sphere of competitive, horizontal and symmetric relations. Sociology, on the other hand, having studied networks of relations, trust and alternative modes of money use, spotted phenomena that are related to the contract but cannot be derived either from utilitarian assumptions or from the theory of transaction costs. The main point here is that after lasting opposition and

the two disciplines' perception of problems, the languages of their trail-blazing representatives have drawn closer to each other. Though there are reviews that regard the mutual influence meager and only a promise of the future (Baron-Hannan, 1993), it is apparent that economists have begun to dirty their hands and sociologists have made attempts to clean up their models.

What characterized the relationship between the two disciplines in Eastern Europe? I must restrict my speculations chiefly to Hungary since I suspect that generalization would lead us astray and disciplinary limits seem to have been drawn more markedly in Polish and Czech intellectual life. By contrast, in Budapest, and also Sofia, Moscow and Novosibirsk, economic sociological research was most likely carried out at universities and institutes of economics. In Hungary, the two disciplines were far less separated than in the West. One reason certainly is that these areas which involve only a few hundred specialists have not been completely professionalized. For lack of regular training, former generations of sociologists were recruited from other disciplines: law, philosophy, economics. Some sociologists were thus formally trained in economics, and the outlook of generations of young economists starting from the sixties was shaped by being taught sociology by András Hegedűs, Iván Szelényi or István Kemény. The Hungarian institutionalist school (Mihály Laki, Erzsébet Szalai, Mária Csanádi, Éva Várhegyi, Éva Voszka) and those absorbed in the economics of human resources (István R. Gábor, Péter Galasi, Gábor Kertesi, János Köllő, Gyula Nagy) – whose works are perfectly relevant for sociology as well – are characterized by this outlook and sense of reality, by the need to launch an investigation on the basis of empirical processes.

Finally, there must have been another reason why the two disciplines remained so close. From the second half of the sixties, the dominant manner of expression in both disciplines was reform discourse (which eventually facilitated the resuscitation of sociology, handled with some ideological suspicion). In these years, all the highly popular economists were either preoccupied with reform suggestions, the mental sphere of the blueprint, such as Márton Tardos, László Antal or Tibor Liska – famous for his verbal radicalism –, or immersed in research, such as János Kornai of outstanding international prestige, Éva Ehrlich, Ferenc Jánossy or András Bródy, although they also declared their views on reform from time to time. Reform was that area of questions that involved them regularly in discussion and debate with the sociologists András Hegedűs, Iván Szelényi, Teréz Laky, Pál Juhász, Tamás Kolosi, Zsuzsa Ferge and Rudolf Andorka, in the course of which mutual contacts and respect evolved. This kind of discourse was typical among younger experts as well (Major 1982). Though there were sometimes quarrels between economists who beliered in the market and socially sensitive sociologists, distinct ideas were not formulated at the breaking points of the two disciplines (Miklós 1988). There was a complex of questions that kindled the interest of both economists and sociologists: the second economy and related questions of the labor market and social stratification.

I am afraid, however, that the reform discourse that contributed to down-to-earth standpoints and disciplinary tolerance did little good for theoretical performance. Hungarian sociology, to be sure, is underconceptualized and works that create models are chiefly recent attempts at interpretation.

This partly explains the increased interest in the work of those who move in the border areas of Western and Eastern research traditions. They are not former sovietologists or re-

gional experts, but sociologists par excellence who are able to formulate theoretical questions appealing to western sociologists and which confront them with an awareness of reality characterizing East European research traditions. They include researchers of social structure, conflicts and economic elites (Iván Szelényi, or of the younger generation, Ákos Róna-Tas and József Böröcz). Others that may belong here include those whose interest drove them to case study investigations of enterprises, a strong tradition in Eastern Europe. Apt examples are Michael Burawoy and David Stark, who carried out participant observation and case studies in the eighties and established in the meantime an intricate network of contacts with the local professional community. Others include Steven Sampson, Nigel Swain and Wim Swaan whose studies testify to a harmony between questions of paradigm and a keen perception of reality. More recently similar research have been launched with the technique of case studies by Magoroh Maruyama as well as Richard Whitley and Jeffrey Henderson, who offer the perspective of and comparisons with Far Eastern business systems.

TWO KINDS OF ECONOMIC SOCIOLOGY?

Let me briefly review the differences and similarities I am inclined to recognize between western and East European economic sociology. Because of the nature of a comparison, the emphasis is on differences, which is not to say that there are no common concerns or that the differences are insurmountable (see Table 1).

Is one to acquiesce to the fact that there are seemingly two economic sociologies, a paradigm-oriented type in the West and a social-economic problem-oriented type in the East? Despite signs to this effect, it is certain that in regard to scientific standards and methods, there are not two sociologies. Lack of knowledge is no virtue, be it scientific knowledge or an awareness of reality. In Eastern Europe the tradition of criticism and direct reflection on problems has been stronger, with somewhat fewer theoretical achievements – and with even fewer existing theories becoming known. In the West, sociology is more attracted to constructing theories, even if it gives rise to alternative paradigms, and even though sociologists relate to various concepts as they relate to a toothbrush – to quote Weber's apposite simile: they are averse to using that of others.

Thinking without paradigms might render Coase's words about the American institutionalist tradition valid here, too: it "led to nothing. . . Without a theory, they had nothing to pass on except a mass of descriptive material awaiting a theory or a fire" (Williamson in Smelser and Swedberg 1994: 78).

Paradigmatic thinking cannot, and need not, be eluded. However, it has its unwanted side-effects, which must be recognized. It is not useful for the paradigm to dictate the choice of problems. The paradigm must be set within the frame of a sociological imagination and the perception of problems must precede the paradigm. The social embeddedness of economic phenomena – indisputably the crucial point of economic sociology – has more to it than simply illustrating paradigms. It is the task of the paradigm to provide a frame of meaning, a set of concepts for the understanding of social phenomena. Always to be considered is what kind of social phenomena a given paradigm can cover, lest we get lost in the maze of scientific conceptualization.

The East European sociological tradition and the challenges of the ongoing changes may help find the lost sociological imagination and put paradigms in its service.

Economic sociology in the West and in Eastern Europe – A comparison

	West	Eastern Europe
focus of analysis	paradigm-oriented	problem-oriented
relationship to the sociological imagination	few contact points, more efforts for systematic analysis	frequent contact points, few systematic efforts
relation to theories	starting with theories, rich theoretical implications	casual conceptual frameworks, poor theoretical implications
reference groups	groups of sociologists	sociologists, politicians, media experts
relation to social reality	illustrative empiricism, poor awareness	starting with empirical problems, ability for 'wonderment' rich awareness
relevance of discussed social problems	mixed, sometimes little	mixed, sometimes outstanding
relation to public policy	weak connections, casual implications	hunger for data, strong connections
relation to public discourse	gradually separated from -	progressively connected with -
international recognition	yes, especially those who are in the paradigm cores	no, except a few of those who left left for the West
boundaries between economics and sociology	sharp boundaries, getting closer to each other	diffuse boundaries, vivid intellectual discourse between reform economists and sociologists
intellectual tradition	Marx, Weber, Durkheim, Spencer, Sombart, Bücher, Schumpeter, Pareto Polányi, Parsons/Smelser	as in the West plus fragmented local traditions

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