What has mainstream economics learnt from transition? Some evolutions, paradoxes and challenges.

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Abstract

As a foremost and encompassing socioeconomic phenomenon, post-socialist transformation might have prompted major evolutions within the field of economic science. Yet, the learning effects have been impressive but uneven and very ambiguous: the feedback process related mainly to peripheral assumptions without reaching the theoretical and methodological core of economic theories. The aim of the paper is to assess briefly and sketch the reasons of these developments. A tentative framework to analyse ‘transitology’ is suggested here. It centres on epistemological obstacles to theory testing in economics and on the social and scientific construction of ‘transitology’. “Facts”, indicators and methodologies are not neutral but theory-laden. The lack of consent on the facts, their selection and their interpretation, opens the way to manifold immunization strategies (insertion of ad hoc exogenous explanatory factors, neutralization of objections through superficial integration). Besides, the organizational and institutional arrangements of economic science are crucial too: the marginal position of ‘transitologists’ within economics, the compartmentalization of economics contributed to limit de facto the “lessons” of transition to partial learning process.

JEL-Codes: A11, B 50, B 52, P 30

EAEPE-Codes: COMES, IHEC, INSEC, METHO

Keywords: Transition economics, economic methodology, history of present economic thought, institutions.
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Introduction¹: Putting economics to a test?

Post-socialist transformation is one of the greatest economic experiments in the 20th century. Virtually the transformation process concerned every field of economics – from macro to micro –: firms, markets, labour, industrial organization, trade, growth, the whole range of economic policies, institutional change etc. Furthermore, it affected and still affects about 1.7 billion people and twenty-nine countries. As a process of all-encompassing change, transition provides “out of steady state” observations. Such observations should tell us a lot about arrangements and interactions between the constitutive parts of economic systems. As a foremost and encompassing socioeconomic phenomenon, post-socialist transformation may have triggered major evolutions within the field of economic science. How did economics come to stand before the bar of this experience?

The latest theoretical learning effects seem impressive: an institutional and political turn going hand in hand with a new embeddedness of economics in society, is apparently taking place. Nevertheless, these evolutions remain uneven and often ambiguous. The feedback process related mainly to peripheral assumptions without reaching the theoretical and methodological core of economic theories. This paper aims at assessing briefly and sketching the reasons of these developments, with a view to pinpointing some emerging paradoxes and challenges. The neoclassical tradition being the tone-giving school of thought, we focus primarily on mainstream economics, leaving the evolution of various strands of heterodoxy aside². Of course, these issues are very controversial and highly complex and no straightforward and definitive answers can be made: merely some tentative schemes can be proposed here.

An exploratory framework to analyse transitology is suggested. Relying on a kind of “political economy of knowledge production”, it centres on the social and scientific construction of transitology, on methodological and epistemological obstacles to theory testing in economics³. “Facts”, indicators and methodologies are not neutral but theory-laden. The lack of consent on the facts, their selection and interpretation, opens the way to immunization strategies (insertion of *ad hoc* exogenous explanatory factors, neutralization of objections through superficial integration). Besides, the organizational and institutional arrangements of economic science are crucial too: the marginal position of transitologists within economics, the extreme division of labour, the compartmentalization of economics contributed to a limitation of the learning effects.

1. Analysing a paradox: a great experiment, ambiguous learning effects

Both for actors and experts, the fall of the Berlin Wall was to a great extend a surprise and very few preestablished theoretical building blocks were available to enlighten the transition process to market economies. Yet it did not happen in an ideological and theoretical vacuum, but in the specific historical momentum of the conservative ‘counter revolution’ and the Washington consensus which emerged in the 1980s (Williamson, 1990). The influential transition schemes were set in this context. As we shall see now, the transition experimentation has indeed fostered striking evolutions in economics and amplified trends already at work but these developments are often ambiguous.

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¹ The author wish to thank Bernard Chavance, Jaime Marques-Pereira and participants to the conference “What has transition taught to economics?” held in Amiens in June 2006 for their precious remarks on an earlier draft of this paper. The usual caveats applies.

² A comparison of mainstream and heterodox intellectual paths would be the matter of another paper.

³ We will not deal with the political element in the development and comparative success of economic theories. A systematic analysis of the interaction between politics and science in transitology is still to be done.
1.1. A great social experiment, striking evolutions but no scientific revolution

The early debate on transition was dominated both in high-ranking journals and in international institutions by the so-called shock therapy approach. Dissenting views relating to an institutional-evolutionary approach (Murrell, 1992, 2005) with a more gradual and encompassing view of change were present from the very beginning. Yet, except for a few outstanding exceptions, they were chiefly influential in heterodox circles (Boyer, 1993) and in the non-economic literature (Stark, 1992; Stark & Bruszt, 1998). We shall be brief on these points, which are well-known.

Transition schemes and transition surprises

Although partly built on different pieces, much of the policy advice was derived from basic textbook economics. It enables us to sketch some stylized features of the dominant doctrine at the beginning of the 1990s:

- It is a transition model from one equilibrium state to another. The transition period is suspended between the economic order of the past (a suboptimal plan equilibrium) and the economic order of the future (an optimal market equilibrium), each conceived as stable and homogenous.
- To reach the new equilibrium, every legacy from the past has to be abolished (tabula rasa) through a big bang approach to economic reform. Thus, the so-called shock therapy aimed at implementing a complete and coherent market speedily, with strong emphasis on liberalizing prices, tight monetary policy and balanced budgets to stabilize the macroeconomy, and privatizing state-owned enterprises in order to induce profit-maximizing behaviour (Washington consensus). Institutional issues are ignored overall.
- It is a one-size-fits-all policy model. The approach to be applied to Eastern Europe was developed in other contexts (Latin America, Africa). It was supposed that a single theoretical framework could encompass all possibilities. No wonder: mainstream economics is indeed largely insensitive to historical and geographic diversity.
- Since transition is settled in replicating a standard market model, it is understood as a mere convergence process toward a unique market economy.

Clearly, this initial transition canon has a strong neoclassical core:

- It relies on equilibrium thinking and static efficiency criteria. This notion of efficiency is extremely narrow, and concerned only with the allocation of a given quantity of resources. Dynamic efficiency is not truly taken into account in the standard view. In addition, the performance of one element is measured in isolation of other elements. Such a view can be contrasted with the theory of institutional complementarity (Amable et al., 2005) in which the performance contribution of any one element may be dependent upon the presence of other elements.
- Change is viewed as path-independent: it is end-state driven. In such a teleological perspective, the initial conditions and the history of the economies at stake do not matter. Irreversibility, cumulative and circular causation are out of the picture.

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4 By dynamic efficiency, we mean here that, in the short term, the efficiency of one element may be negative but that it can turn out highly positive when some mutations and learning processes have taken place. It is different from dynamics in neoclassical terms: “By dynamic efficiency neoclassical economics means the existence, stability and uniqueness of equilibrium. Dynamic analysis is shorn of any substance and asks simply whether an economy in equilibrium (existence) subject to an exogenous shock will return (stability) to its original position (unique).” McCartney (2004: 2).

5 Balcerowit (1994: 17) puts this universalism and the associated blindness to historical conditions very well: “One common fallacy is that of country-specificity: ‘Ukraine is different; what worked or did not work elsewhere is not relevant here.’ Some countries may indeed have specific economic problems (the huge military-industrial sector in Ukraine), but economic theory is clear about which policies are best for basic problems: macroeconomic stabilization as the appropriate response to hyperinflation; price liberalization and liberalization of supply are the prescribed therapy for massive shortages, etc. […]”.
The underlying conception of economic agents is the one of the *homo oeconomicus*. Individual preferences are supposed to be context independent, transitive and continuous. No creative interactions between economic agents are envisaged (methodological atomism). In this world of Robinson Crusoe before the arrival of Friday, individuals are abstracted from organizations, networks or institutions. Economics is not embedded in society. Institutions do not matter or do not belong to economic preoccupations.

Economics is viewed as a separate and exact science. Separate from the social sciences, it is supposed to enjoy a scientificity comparable to the one of physics.

The deductive-nomological scheme (deduction from a predefined set of universal hypotheses of testable statements followed by quantitative empirical testing) is then a key element of economics as a hard science. According to this, there is a simple testability of economic models. Thus, economics should produce theoretical and empirical knowledge that is subject to a process of testing and feedback (hypotheses ought to be revised in light of new evidence).

Interestingly, the transition process showed unexpected developments compared with the convenient textbook stereotypes of the transition doctrine:

- Far from the “magic of the market”, a ‘transformational recession’ (Kornai, 1994) or ‘post-socialist great depression’ (Chavance, 1994: 176) took place in Eastern Europe and Russia. The length and the depth of the recession – many countries in 2003 remain far below their ante-transition level of output – contrast with the early optimistic anticipations on the growth that should follow automatically the freeing of market forces.

- The Russian crash (Sapir, 1998) and the emergence of clan capitalism in Russia (Kosals, 2006), the spectacular growth of organized crime belong to the transition surprises too;

- The Vietnamese and Chinese trajectories (Chavance, 1997, 2000; Paquet, 2004) seem to contradict widely the big bang approach. Gradual and cumulative reforms lead to rapid growth without transition recession. The reforms differed diametrically from the Washington consensus (role of collective and state ownership, poorly defined property rights, dual-track organization of transition processes and of labour markets, weak financial markets, gradual and partial opening of the economy, etc.).

- Contrary to the pervasive scheme of convergence, a high variety of trajectories – be it at the national, infranational or supranational levels – appeared in transition countries (Chavance & Magnin, 1996; Chavance, 2002).

**Learning (or not) from transition: an institutional and political turn?**

Following these surprising outcomes, following other disappointing experiences in Latin America or Africa, and critics by heterodox economists, some proponents of the transition canon have modified their initial views. A kind of amended transition doctrine emerged as exemplified by the World Bank evolution and the work by authors like Stiglitz (1998, 2000). In the modified version of the transition canon, market is not enough: institutions, society, culture, politics and history matter too. For instance, one can read in a recent World Bank report (2005: 10-11): “Perhaps most important, while reforms in the 1990s focused on increasing the role of markets and decreasing the role of the state, they tended to neglect the role of institutions […]. The experience highlights the importance of the investment climate, and of providing predictable conditions for investors and other economic agents. It also highlights that growth entails more than the efficient use of resources. Growth entails structural transformation, diversification of production, change, risk taking by producers, correction of both government and market failures, and changes in policies and institutions. It is also a process of social transformation: people will change activities and live in different places. […] These changes take place over time, alongside changes in institutions that render them possible. Any growth strategy needs to include actions, both on the policy and the institutional front, that address and support this process of change”.

Moreover, according to this new worldview, a variety of viable transition paths is likely. “While there are some functions that institutions need to perform in any society, the form through
which institutions can perform these functions can vary considerably (Virmani, 2004). Most of the empirical work on the importance of institutions leaves open the question of how to improve institutional performance. Merely adopting some other country’s laws and formal regulations is no guarantee of achieving the same institutional performance” (World Bank, 2005: 6). Gobind Nankani, the World Bank vice-president writes in the preface of the book, “The central message of this volume is that there is no unique universal set of rules… [W]e need to get away from formulae and the search for elusive ‘best practices’ ” (World Bank, 2005: xiii). It is well worth noticing that proponents of historical institutionalism (Thelen & Steinmo, 1992), since they take into account historical specificity (Hodgson, 2001a), worked out the idea that there is no unique universal and optimal set of rules very early. The farewell to institutional mono-cropping is even in some cases logically followed by a shift from a Planglossian view (Gould & Lewontin, 1984; Boyer, 1997) towards a critic of functionalism, a central point in historical institutionalism: “Functionalism also provides the biggest pitfall in trying to understand institutional change. The functionalist view is that institutions change by necessity in response to a change in their environment that diminishes the efficiency of existing institutions; it does not explain the mechanisms by which change takes place or fails to take place” (Roland 2004: 118).

Even the notion of efficiency seems to evolve toward a more complex approach including complementarities and interdependencies between different elements of economic systems: “economies do not operate in mechanical ways and dynamism in one sector can offset the cost of inefficiency in others.” (World Bank 2005: 9).

As Rodrik (2006: 3) remarks sharply about the World Bank’s Report on Economic Growth in the 1990s, “occasionally, the reader has to remind himself that the book he is holding in his hands is not some radical manifesto, but a report prepared by the seat of orthodoxy in the universe of development policy”. However, has this new openness to institutional, cultural, political and social issues contributed to a real change in economic thinking? Is there a new consensus on institutions? Is it a real comeback of institutional economics leading to a new embeddedness? Is it a farewell to the transplantation of “best-practice” institutions, to the maxim ‘one theory fits all’ and, last but not least, to equilibrium thinking? Or is it simply a declamatory exercise?

**The problem: obvious blockages in the feedback process**

Obviously, there are numerous limits to this seeming institutionalist turn in economic thought. First, the evolution towards an amended doctrine is not consensual at all. One could contrast the World Bank intellectual path with an “IMF path” in which the Bretton Woods institution simply sticks to the initial transition doctrine. For instance, according to the IMF, the problem was not with the approach taken to reform, but that it did not go deep and far enough (Rodrik, 2006: 9). As we shall see later, to follow such a path, immunization strategies to the transition surprises are required.

Second, even in the case of the amended doctrine, the evolution did not go very far and the conclusions and lessons cannot be taken at face value. The economists’ praxis has remained much the same. “It would be naïve to think that the World Bank’s practice will change overnight. There is little evidence that operational work at the Bank has internalized these lessons to any significant extent. […] One thing we have discovered is how difficult it is to win the Bank’s country economists away from the Washington-Consensus, laundry-list, best-practice approach to reform.” (Rodrik, 2006: 7-8). The tools, indicators stay behind the generous discourse and remain encroached in a neoclassical framework (for illustrations of this point, see the next part).

The turn remains superficial not only at the operational level but also at the theoretical level. It had no real effects on the methodological core of economic theory. Clearly, the amendments to the initial theory are incompatible with the original theoretical core. For instance, admitting that institutions matters and mould the behaviour of economic agents is inconsistent with the assumption that the individual preferences are context-independent or with methodological atomism. In one word, it is not logically possible to mix an institutional model with the *homo oeconomicus* together. A consistent consequence of the transition lessons would have been to switch from *homo oeconomicus* to *homo institutionalis*, from equilibrium thinking to processual thinking, from an axiomatic to a historically grounded theorization. Historical specificity and its correlate – the variety of economic organizations in time and space – is then not a disturbing factor for economic theories. It may be taken seriously and integrated into the core of economic theories (Hodgson, 2001a). Economic actors follow
rules that are historically specific, situated in a peculiar social and political context. The early transition doctrine exhibited a high coherence between the neoclassical core and the applied theory of transition. It is not the case any more. In the present state of the art, the modifications at the level of applied theories without corresponding amendments at the more abstract level of economic theories, led to inconsistent theoretical ensembles. Development strategies were partially rethought but general theories and methodologies remain so far largely untouched (see figure 1).

1.2. Analysing transitology: a tentative institutionalist and evolutionary framework

The Popperian ideal seems not to work without friction in the transitologists’ world: the outcomes of the transition experiment contradict deeply the standard transition model but there is no rebuff of the underlying neoclassical paradigm. The preceding decades have shown that falsification do not lead to the rejection of economic theories: the repeated experimental falsification of hypotheses regarding the homo oeconomicus and its preference structure (Kahneman & Tverski, 2000) have been so far of no consequence to economic modeling so long as econometric tests manifest a close enough fit. There is often a strategic shift from a Popperian view to an instrumentalism à la Friedman (1953). Yet, this alternative doesn’t work either: if accurate predictions – rather than realistic hypotheses – were the genuine test of economic scientificity, then the transition surprises pose a big problem to standard theories. There are obviously shortcomings in the traditional epistemological positions. Therefore an alternative framework is needed to analyze more realistically the impact of the transition experiment on economics, especially the observed blockages. We suggest applying an institutional and evolutionary approach reflexively to economics (Labrousse, 2006) and especially to the field of transitology. This framework emphasizes the social and scientific construction of reality.

The active role of the observer: preconceptions, system of relevances and theory-ladeness

In mainstream economics, the methods, datas, the treatment of the subject are supposed to be independent from the observer (pure scientific objectivity). An alternative view is suggested here. The subject of analysis is not independent from the inquirer. The access to “reality” is filtered by the preconceptions (Veblen, 1899-2000), by the system of relevances (Schütz, 1932) of the observer, by its cognitive maps. He selects relevant “facts” in a complex reality. Observation is not fully neutral. Preconceptions guide and shape the process of observation and interpretation. Thorstein Veblen emphasized early the crucial role of preconceptions in economic science. Take for example the competition equilibrium and the homo oeconomicus model: “Of course, this perfect competitive system, with its untainted ‘economic man’, is a feat of scientific imagination, and is not intended as a competent expression of fact. It is an expedient of abstract reasoning; and its avowed competency extends only to the abstract principles, the fundamental laws of the science, which hold only so far as the abstraction holds. But, as happens in such cases, having once been accepted and assimilated as real, though perhaps not as actual, it becomes an effective constituent in the inquirer’s habits of thought, and goes to shape his knowledge of facts. It comes to serve as a norm of substantiality or legitimacy; and facts in some degree fall under its constraint, as is exemplified by many allegations regarding the ‘tendency’ of things. This competitive ideal, therefore, affords the test of absolute economic truth. The standpoint so gained selectively guides the attention of the classical writers in their observation and apprehension of facts, and they come to see evidence of conformity or approach to the normal in the most unlikely places. Their observation is, in great part, interpretative, as observation commonly is. What is peculiar to the classical economists in this respect is their particular norm of procedure in the work of interpretation” (Veblen, 1899-2000, emphasis added).
Figure 1: Putting economics to a test? Obvious blockages

**The theoretical and methodological core**
- The deductive-nomological scheme
- The *homo oeconomicus*
- Methodological atomism
- Equilibrium thinking
- Economics as separate and exact science
- A simple testability of economic models

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**An applied theory: transition economics**
- The transition model (from a planned equilibrium to a market equilibrium)
- Convergence, one-size-fits-all policy model
- *Tabula rasa* of the past
- Implementing speedily a complete and coherent market (Washington consensus)

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**Amended transition economics**
- Market is not enough
- Institutions, society, culture, politics and history matter too
- A variety of transition paths are possible
- These amendments to the original theory are incompatible with the theoretical core

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**Surprising outcomes**
- Transition recession
- The Chinese and Vietnamese paths etc.

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**Poor feedback**
Immunization strategies

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**No real feedback to the theoretical core**
Why?

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Observers with different beliefs and theoretical backgrounds “see” or “experience” different things. Scientists are producing social constructs (Hacking, 1999). There is not such thing as a neutral observation language. The descriptive language of economics is theory-laden. Indicators are a more or less a hidden crystallization of theories. As Veblen stated, observation is always interpretative. Thus, observation by itself can not settle disputes between rival systems of belief, between different theoretical frameworks. It challenges two influential conceptions of economic explanation: The first one is based on the assumption that the aim of economics is either to test existing theories along “hard”, fully objective facts, uncontaminated by theory; The second one assumes that the aim is to accurately describe the economic world in a theory-neutral language and, having accomplished this descriptive task, to construct the correct economic explanation of these economic facts by recourse to economic theory.

Observation is context-dependent (Kahneman, 2003) and thus historically situated. It depends on the historical environment of the observer, on his subjective history and the one of the group(s) he is embedded in. Pragmatism that deeply influenced old American institutionalism (Hirsch, 1998) builds on Dewey’s notion that the acquisition and the production of knowledge is a social activity—a group undertaking. Similarly, Alfred Schütz (1932) delineates various zones of interests, or relevances, and their dynamic. He considers how subjective knowledge becomes embodied in a social stock of knowledge and how the latter influences the former. In Schütz’s words, the ordinary person, acting in the world, is in a biographically-determined situation, doing what he or she does in line with the system of relevances that enables him or her to select from the environment and from interactions with others, those elements that make sense for the purpose at hand. The social scientist, on the other hand, is operating on the basis of a scientifically-determined set of relevances, choosing those aspects of the situation that are appropriate for the objectives of the research. In behaving in this way, the researcher develops models of human action, with the postulate of logical consistency, whereby the objective validity of the scientist’s constructs are guaranteed and are distinguished from the constructs of everyday life. The procedures, the norms of enquiry are just crucial in this respect. They allow the researcher to escape from a pure subjective view to produce reliable and consistent knowledge (Labrousse, 2006). This goes hand in hand with a procedural rationality.

The embedded scholar: academic actors, organizations, networks and institutions

Following Simon (1997), the model of bounded rationality is applied to common economic actors as well as to scholars in the process of scientific discovery (Langley, 2004). The rationality of the researcher can be characterized as bounded, subjective, procedural, interpretative and socially embedded.

- It is a subjective rationality: each observer, as we have already dwelled upon, develops a specific point of view.
- It is a bounded rationality: the economist cannot scrutinize exhaustively every aspect of his subject; he can only deal with a fragment of reality.
- It is a procedural rationality: in order to cope with these cognitive limits, researchers develop routines, procedures of inquiry that are constructed socially and historically and aim at the production of reliable knowledge.
- It is an interpretative and deliberative rationality: the research project evolves through trial and error; the project is discussed in scientific communities and arenas.
- It is a situated and embedded rationality. The economist does not work alone. He is embedded in scientific networks and organizations and follows canons of knowledge evolving in time and space. “The spiritual attitude of a given generation of economists is therefore in good part a special outgrowth of the ideals and preconceptions current in the world about them” (Veblen, 1999a).

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6 For instance, the study on privatization and corporate governance by Frydman et al. (1996) was conducted on the back of prior studies on the US system, i.e. through the lenses of an American governance model: effective ownership was defined as the presence of a few investors who hold an optimal number of shares.
The academic actor is involved in academic organizations and institutions. By institutions, we understand the academic formal and informal rules of the game which are widespread in the discipline (canons of knowledge, career valuation rules etc.). To paraphrase North (1990), academic organizations (research institutes, universities, journals and the like) are the players of this game. Some players are more powerful than others: they have a leadership in academic debates. For instance, the Harvard Institute for International Development (HIID) from the Harvard economic department, the MIT and the Quarterly Journal of Economics were tone-giving in transition analysis. They defined the relevant issues at stake, the normal way to deal with them etc. – be it for supporters or opponents to their approach. Configurations of knowledge production matter: internal organizational structure\textsuperscript{8}, degree of compartmentalization and openness (structuration in small worlds for instance), proximity between individual and collective actors etc. Far from being atomized individuals, academic actors are linked by various personal and professional networks. Kogut and Spicer (2004), using numerical citation analyses, document the strong ties among a core group of economists subscribing to the shock therapy approach (for more details, see the last section of the present text). This group had strong connections to the international financial institutions and the US government, and was able to spread its views in reforming countries under the auspices of these powerful organizations (Murrell, 2005). Scientific actors are part of scientific but also policy networks, which are sometimes intricate together. One could think once more of the dubious role of the Harvard boys in the political economy of the Russian transition (Sapir, 2001). Scholars follow strategic behaviours within and sometimes outside academic rules.

In such a perspective, the evolution of economics is not characterized by a linear progress but by a complex path-dependent process with locked-in trajectories and bifurcations. According to Veblen (1899) “the canons of knowledge are of the nature of habits of thought, and habit does not break with the past.” Yet, there is a process of partial adaptation to an evolving experience –empirical twists and turns– through learning processes, through trial and errors\textsuperscript{9}. Cumulative alterations can lead to a bifurcation point. It should be stressed at this point, that the perspective sketched here is not at all a relativistic one: three main criteria are crucial to value a theory: its fruitfulness in explaining reality, its internal coherence and the robustness of the procedures used to process and produce reliable knowledge.

2. The theory-ladeness of « facts », indicators and methods in transitology:
epistemological obstacles to theory testing and learning

In the field of transitology as in other areas of social sciences, holders of rival theories construct, select and interpret the ‘data’ differently, each in the light of his or her own theory, as we shall see now.

2.1. The theory-ladeness of indicators and methods

Theory-laden indicators: the example of transition indicators

The way indicators are constructed matters. Let us illustrate this point with the example of the EBRD transition indicators. About ten years after the beginning of transition, the authors of the EBRD Transition report seem to go away from their previous functionalist and teleological approaches: “One important lesson from the experience of the past decade has been that there is no unique process or ‘time-line’ of transition from central planning under communism to a unique, easily identifiable, familiar form of market” (EBRD 2000, 3-4). Nevertheless, they remain de facto locked-in in previous preconceptions of a teleological transition process toward a given end-state equilibrium because of the very construction of their indicators. As Bernard Chavance (2002, 5) remarks: “After this welcome

\textsuperscript{7} Although entailing a strong international dimension, these rules are rooted in specific national academic systems.
\textsuperscript{8} For instance, the presence of an Operations Evaluation Department in the World Bank, meant independent and reporting directly to the Bank’s Board of Advisors (Kogut and Spicer 2004), probably alleviated the emergence of an amended doctrine within the World Bank.
\textsuperscript{9} Wladimir Leontief (1979) spoke nicely of ‘trial and error economics’.
clarification, the Report extensively discusses the evolution of EBRD’s ‘transition indicators’, where the advance of each country has been measured since 1994 on a scale from 1 to 4+ in a few dimensions (private sector share of GDP, price liberalization, financial institutions reform...). National economies thus implicitly appear as more or less advanced or late on this unidirectional scale.” Such indicators embody the very idea of a single “one best way”. These criteria do not allow for enlightening the very diversity of transition paths. Variety only appears as a lack, a deficit in relation to a normative model. Relying on equilibrium thinking and comparative statics, they hide the processual dimension of transition, hybridization process between endogenous and exogenous elements (Labrousse, 2003). In short, these indicators are incompatible with the introductory statements of the report and turn out to be a major impediment to the learning process. Furthermore, such instruments are widely spread in the academic world and are often the only one available. This in turn poses a big problem for heterodox economists who most often have not the financial and organizational means to build indicators and large databases of their own. They must in many cases use neoclassical inspired indicators with their shortcomings. The use of indicators has become in some circumstances a crude end in itself, regardless of the construction of ‘facts’ and the bias it implies.

Cross-country growth regression and the assumption of universal, ahistorical laws
Not only indicators are theory-laden but methods too. Let us take the example of cross-country growth regressions, a usual tool in economics. Far from being neutral, this method entails an underlying neoclassical assumption that growth follows universal laws. In a recent contribution, Matthew McCartney put this very nicely forward: “In order to run large cross-country regressions researchers are tightly constrained to the assumption of universalism. Conventional growth analysis assumes that growth parameters are identical across countries. Far from being a positivist statistical exercise, cross-country growth regressions are bound to an underlying neo-classical assumption – that the growth process is universal. Each individual country in cross-section according to this view will provide evidence that can be used to elucidate the one underlying universal economic relation” (McCartney, 2006). An increase in the private sector share for example is hypothesised to have the same effect on growth in all countries from Russia to China. The problem is that the effect of one variable may be dependent of other elements, of institutional complementarities and that a mere quantitative view of change is often too narrow. For instance, mass privatization programs in the Czech Republic and Russia were a success in terms of speed, but resulted in disturbing configurations in terms of distribution of ownership or corporate governance (Chavance, 2002). The economy is a complex web of interdependencies that does not allow us to predict the final outcome of a change in a single variable. A rise in interest rates may well lead to an appreciation of the national currency, but the contrary is just as possible (Orléan, 2001). The efficiency of reforms is often time-dependent: for instance, the mixed forms of ownership in China proved helpful in the first phase of mutations. This does not mean that economists have to throw the baby of growth regression with the neoclassical bath, but that this tool has to be applied carefully in relation with more qualitative material. To quote McCartney again: “Comparative and historically informed case studies allow researchers to question the assumption of universality rather than be forced to assume it true a priori. A heterodox or post-autistic economist should begin with case studies and only then proceed to cross-country growth regressions with all due caution.”

The dominance of the deductive-nomological scheme: an hindrance to understanding novelty
Another example of the theory-ladeness of methods is the deductive-nomological (D-N) scheme. It implies a specific way of scrutinizing reality. The hypothetical-deductive model functions in a closed system. It defines a priori the relevant aspects and leads to putting unforeseen, startling events aside that are then regarded as irrelevant abnormalities. Because of the dominance of the DN-scheme in economics, transition was practically a new field of application of existing hypotheses not an opportunity to build new ones fitting with the specific historical conditions of transition countries. Transition economists using the D-N scheme call to mind the parable of the drunken man who had lost his key in front of the doorstep but was searching under the streetlight because it was the only place where there was light. Its use is enforced by the canons of economic reasoning. As in the case of
indicators, there is a kind of path-dependency in the use of economic methods and ways of thinking. The problem is that, as stressed by Roy Bhaskar, the D-N scheme is inadequate in analysing complex, open systems in which conditions are changing and give rise to endogenously engendered novelty, which is obviously the case in economics in transition.

Yet, there are alternatives to an exclusive deductive scheme. Many economists imagine that induction and deduction exhaust our ways of doing science. However, there is a third inference mode: abduction. Many scientists use abduction without realizing it. Abduction aims at discovering the reasons for particular events. It starts from the observation of surprising facts, contradicting our preconceptions and for which we need an explanation. It is the process of reasoning\(^{10}\) in which explanatory hypotheses are formed and evaluated. “The object of reasoning is to find out, from the consideration of what we already know, something else which we do not know” (Peirce, 1877: 9).

Abduction is a way to deal theoretically with novelty and change which are crucial in the transition process. In abduction, we deploy specific known relations and particular assumptions to formulate propositions (testable hypotheses) with the intent of explaining those particular events. In economics, it can begin with the observation of stylized facts – as suggested by Kaldor (1961) – to establish a preliminary diagnosis. Then, “we have to form one or more explanatory hypotheses, select one of those, and continue our process of inquiry, testing the hypothesis” (Kruijfft 1997: 5). In this iterative process, the economist builds a provisional theoretical framework permitting to deduce the consequences of the selected hypothesis that are then tested and lead to a renewed exploration of reality. The process ends provisionally with the building of a theory delivering a satisfying explanation. From this heuristical point of view, it is interesting to start from surprising phenomena like transition recession or from the Chinese case often neglected in the standard approach. Murrell (1995: 174) in his review of a standard book (Blanchard et al., 1994) by prominent tenets of the standard approach notes: “Are there not observations that recur, a pattern worthy of note that fits the majority of the essays? The non-fulfilment of expectations is certainly a persistent element […]. They are somewhat troubling, when viewed against the backdrop of perceived unanimity on dramatic reform programs, on grand schemes of socioeconomic redesign incorporating the same judgment that inform those expectations”. Salient facts can orient theorizing, surprise is significant. It is not argued here that the whole economists’ community should adopt abduction and leave aside the D-N scheme. To control better the perverse effects of the theory-ladeness of methods, it would be necessary to use a greater variety of methods and to become more aware of the manyfold bias implied by each scientific construction of economic reality, be it neoclassical or heterodox.

2.2. No agreement on the facts, their selection and their interpretation

No agreement on facts: the example of transformational recession

The transition depression was a big surprise for market economists who had promised rapid growth following the market implementation. This disappointment was a triggering element in the evolution of the World Bank. Yet some economists reject this point of view and consider the fall of GDP in Eastern Europe to be a mere myth and not a salient feature at all. It is the case of Anders Aslund (2001) in his paper *Myth of output collapse after communism*: “both communist and post-communist statistics are deeply flawed. First, everywhere the decline in output has been much smaller than perceived, and a few countries grew immediately rather than contracted. Second, the Soviet economy was in far worse shape than generally understood. Third, even after revising the official statistics, the differences between failures and successes remain vast—and the correlation between structural reform and economic performance becomes much stronger. Fourth, flawed statistics misled policy makers in post-communist transformation, inciting them to adopt inefficient gradual reforms, which reinforced rent-seeking and prolonged stagnation. And fifth, economic welfare has diminished far less than output.” Notably, policy advisors to the Russian government often promoted such interpretations when

\(^{10}\) According to Peirce (1903, vol. 5, paragraph 189), an abductive argument is of the form:

1. The surprising fact, C, is observed:
2. But if A were true, C would be a matter of course,
3. Hence, there is reason to suspect A is true.
the Russian performance was hardly to be praised. Yet, such an absence of agreement on basic facts impedes the feedback process and blocks it at the level of applied theories.

No agreement on the relevant facts

Furthermore, there is no agreement on the scope and reach of relevant facts. Fields and models of comparison are moving with theories. Authors pick up facts in a complex reality, facts that fit with their theoretical preferences. For instance, there have been “rolling transition models”. The transition doctrine has been keen of success stories that were supposed to exemplify the beneficial consequences of its proposals. However, according to Chavance (2002), “these stories were often modified or reversed, producing surprising changes of assessments. We first observed the Polish exemplary shock therapy resulting in an unexpectedly slow privatisation and a surprising electoral comeback of reformed communists, we have witnessed the bold Russian liberal attempt praised by various authorities or specialists (Aslund, 1995; Layard & Parker, 1996) tragically give rise to a grand failure, we have seen the initial ‘Czech miracle » (good macro-economic performance, fast mass privatisation, low unemployment) reversed after 1997 in the very example of what ought not be done,…”

The scope of comparison moves with theoretical preconceptions too. For instance, the Asian post-socialist paths are often excluded from analysis according to an alleged radical specificity of Asian developments. Sachs and Woo (1997) reject the relevance of the Chinese case: they suggest that the agricultural roots of the Chinese economy went hand in hand with a higher growth potential. Yet it embroils such analysis in a contradiction: if specificity is so important, if initial conditions are so significant, then they have to be taken explicitly into account in economic theory. The same could be said for the East German experience, an extreme case of transition through institutional blueprint that is almost never taken into account in comparative transition analysis and which exhibited surprisingly and interestingly poor results (Labrousse, 2003). It is indeed difficult to define, without theoretical a priori, the field of comparison to put theories on test.

No agreement on the interpretation of facts

There is more or less a factual consent on the Chinese economic growth. Nevertheless, even if the fact is not very controversial, its interpretation is. Has the growth taken place because or despite gradual reforms? According to institutionalist authors, gradual reforms in China have highly contributed to the absence of transition recession and to the spectacular Chinese growth (Chavance, 2000). But according to standard economists like Woo (1999: 118) “China’s rapid growth has come despite gradualism, in areas of the economy characterized by radical rather than gradual reforms. China’s ability to grow rapidly despite gradual reforms reflects China’s particular economic structure. China’s gradualist strategy is therefore not transferable to Eastern Europe”. It is always possible to attribute the explanation of a fact to other elements. The Duhem-Quine thesis asserts that any empirical evaluation of a theory is in fact a composite test of several interconnected hypotheses (Cross, 1982). This leads to a problem in the evaluation of disconfirming evidence: is it the individual hypothesis or the network of hypotheses that is refuted? This epistemological problem has a strategical counterpart: it is possible to escape from disturbing facts in ascribing them to alternative factors. These conflicts of interpretation are not just perspectives from Sirius but also part of strategies and the product of institutional configurations and interests. It is time now to give some flesh to these epistemological questions and have a brief look on these strategies and configurations.

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11 One could add the rise and fall of the Albanian model. From 1992 on, Albania followed truthfully the principles of the Washington consensus. It was even called “the Balkan Tiger” by the IMF until the collapse of the Albanian financial pyramids in 1997 buried the model into oblivion.
3. Actors strategies and institutional configurations: immunization, balkanization as obstacles to a theoretical feedback

3.1. Immunization strategies: from ad hoc factors to catch-all theories

Though often combined in the economic discourse, three variants of immunization strategies can be distinguished: in the first one, it is “never soon enough” to confront the theory with real outcomes, in the second one the reforms went “never far enough” and in the third one, “the truth is out there”: exogenous factors explain the gap between models and reality. These strategies can lead to catch-all theories including superficially some elements developed earlier by critical proponents to standard transition theory.

Postponing the empirical test: the “never soon enough” argument

A first way to deal with disturbing facts is to postpone the empirical test assuming that in the long run reality will converge toward the theoretical model. Then, it is “never soon enough” to confront the model to empirical reality. For instance, Russia was long declared on the verge of success (just a short while before its 1998 crash). Since “Russia became a market economy” (Aslund, 1995), its poor economic results could only be transitory and had to lead to “the coming Russian boom” (Layard & Parker, 1996). Similarly, advocates of the big bang approach predict China future inevitable “convergence” with Eastern Europe (Sachs & Woo, 1994, 2000) and stress that depression in Eastern Europe is only a short-term collateral effect that shall not undermine the long-run positive impact of big bang. The problem with such a strategy is that theories can never be refutable. Furthermore, there is a striking similarity with the soviet rhetoric according to which the future was deemed brighter than the present and the transition always longer than originally thought. Such a changeable way of dealing with empirical outcomes questions the robustness of scientific procedures.

Implementation failures: reforms went “never far enough”

The next way to cope with the poor outcomes of Eastern transition in the 90’s is the implementation failures argument: the reforms went “never far enough”. For instance, the IMF report starts from the same basic premise as the World Bank report – growth has been disappointing – but its essential argument is utterly different. The problem was not with the approach taken to reform, but that it did not go deep and far enough. Using the report’s own words, “reforms were uneven and remained incomplete”. To quote Stiglitz (2000): “The medicine was right; it was only that the patient failed to follow the doctor’s orders”. In such a view, there is no need to question the accuracy of the electroshock cure. Neoclassical economics is a theory of the normal case: the reality –here the depth and impact of market reform– is always abnormal compared to an implicit or explicit market ideal (Veblen, 1899). The economist matches events against that quixotic market yardstick and diagnoses as failure any deviation from design. The dissonance between model and reality is then due to the unruly patient, its lack of willpower, its nefarious political opposition, its dilatoriness etc. Here again, we find a strong similarity with the soviet way of thinking. The lag between the Marxian-Leninist model and real existing socialism was due to counterrevolutionary defectors and renegades: many soviet patients were faulty and did not even deserve the socialist treatment. In the mainstream transition literature of the second phase, not economics but society and politics –“the grabbing hand: government pathologies and their cures” (Shleifer & Vishny, 1998) – were pronounced guilty. This immunization strategy could be termed: “The truth is out there”.

\[12\] Shleifer forgets here the “grabbing hand” of economist experts: “In 1997, the Harvard Russia Project was suspended and ultimately terminated after the USAID Office of Inspector General uncovered evidence that Shleifer and his second in command, Jonathan Hay, were making investments in Russia and assisting their wives in establishing businesses in Russia. This included using their influence with Russian government officials to obtain favorable licensing, funding, and other benefits for themselves and their wives in violation of the terms of the agreement between USAID and Harvard” (http://www.usaid.gov/press/releases/2004/pr040730.html). The United States pursued a civil action for this conduct against Harvard University, Shleifer and Hay. It led to the following settlement: “the total payments will exceed $31 million. Specifically the settlement calls for Harvard to pay $26.5 million; Shleifer to pay $2 million; and Hay to pay between $1 million and $2 million (depending upon his earnings over the next 10 years)” (http://www.usaid.gov/press/releases/2005/pr050803_1.html).
Introducing ad hoc external factors ex-post: “the truth is out there”

Politics but also past and culture often appear in the economists’ opera as dei ex machina. They provide nice exogenous explanations of the embarrassing gap between models and reality. The past is seen as a mere pathology holding back modernization, never as a resource. Culture is viewed as a crude resistance factor to the market implementation. It is an essentialist, mechanistic and poor conception of the role of history and culture, largely ignorant of standard works on the subjects by ethnographers or sociologists. Culture and history are viewed as immutable monoliths and not as repertoires of conduct that are selectively used and interpreted according to situations. One may ask if past and culture in transitology were not the “measure of the economist’s ignorance” to make a parallel with the unaccounted-for growth in works of Denison and other economists using growth accounting methods to estimate the sources of economic growth: Moses Abramovitz (1993, 218) considered this residual as “a measure of our ignorance”. It is to be sure a weighty residual. Besides, one can presume that the argument has been developed ad hoc: economists that denied in the early 1990 a role to legacies from the past or of cultural norms use it now. “Many Western economists argued that the Soviet legacy did not matter. In their view, the rapid privatization of state enterprises both solved the problem of committing the government to the market—because a mass privatization would be difficult to reverse—and ensured a political constituency for institutions that would support the market. They argued that there was no ‘Soviet man’, only ‘economic man’, and given democracy and privatization, those ‘economic men’ who benefited from privatization would create an automatic and irresistible lobbying force for the rule of law” (Hoff & Stiglitz 2004: 239). As stated by Veblen (1899), “wherever a departure from this normal course of things occurs, it is due to disturbing causes, that is to say, to causes not comprised in the main premises of the science”. Yet, introducing ex post past and culture in this way, as ad hoc exogenous factors, raises more questions than it provides genuine elucidations. First, it is contradictory with the basic premises of the neoclassical paradigm defended by these authors. Second, the model is said to be true ceteris paribus, if past and culture were not out there as disturbing factors. Hutchison, familiar with the work of the logical positivists and of Karl Popper, complained that statements qualified with vague ceteris paribus clauses cannot be refuted (Hausman, 1998). It seems to be the case here.

Nevertheless, even if one lets these points aside, other difficulties appear. If standard economics rely deeply on entirely extraneous theoretical frameworks for the description of the empirical phenomena it explains, then an explicit theorization on how culture and history influences economic behaviours and outcomes is de rigueur. Since these various spheres are interrelated, standard economics should aim for integrated theories and model the relevant interactions between history, culture, politics and economics. It is still a missing link and a factor of inconsistency. Yet, it remains questionable if these factors can be integrated in the standard framework without a paradigm change, without considering economics as a truly historical, social and political science. This issue is particularly acute when it comes to neoclassical theoretical ensembles integrating such factors as institutions and politics.

Neutralizing critical elements: catch-all theories

According to Murrell (2003: 1): “Transition has been coterminous with a remarkable change in emphasis within economics. In 1989, it was a distinctly minority enterprise to highlight the importance of institutions. Now, institutions are at the heart of both research and policy discussions. Similarly, while many early influential analyses of transition virtually ignored institutions, current discussions place them at the centre”. As indicated in the review by Kogut and Spicer (2004, 15), “many of the themes that later came to dominate the economics literature, and the World Development Reports, were actively discussed in the first half of the 1990s”. What Murrell calls the institutional-evolutionary paradigm underlined the diversity of capitalist models, the role of institutional complementarities, the embeddedness of economic issues from the very beginning.

Yet, the late recognition by numerous mainstream economics that institutions, history and politics matters did not lead to a switch to the institutional-evolutionary paradigm. It led instead to a kind of catch-all theory. Institutional issues were “swallowed up” without integrating them fully to the theoretical core. These strategies led to cosmetic amendments at the surface without in-depth rejuvenation of the methodological and theoretical body. Beyond the rhetorical gloss, the newly
transplanted elements are incompatible with the initial and unchanged theoretical foundation. These adjustments are part of immunization strategies. For example, Sachs et alii reversed trickily institutional issues to safeguard their previous a-institutional approach (!): “In a recent debate about relative merit of gradual versus shock therapy approaches to the transition, the gradualist view was overwhelmingly dominant (see Roland, 2000 and Sachs & Woo, 1999). This is partly due to the lack of constitutional thinking among economists. Some economists who are in favour of gradualism easily jump to the conclusions by looking only at the short-term economic effects of different approaches to the transition […] First, the long-term effects of the changes in the constitutional rules on economic performance are not always consistent with their short-term effects. […] It is argued that assessment of reform performance might be very misleading if it is not recognized that economic reforms are just a small part of large scale of constitutional transition.” (Sachs, Woo & Yang, 2000: 2-3). Beyond the institutional façade, one can identify the “never soon enough” as well as the “truth is out there” rhetoric. The same could be said of the amalgamation of political elements in economic models. The argument is not simply to add new variables into an old model, but instead to re-examine the model in toto in order to draw the logical consequences of the amendments. This is a challenge.

3.2. Barriers to learning: the growing compartmentalization of economics

In analysing actors’ strategies to cope with the unexpected developments in transition countries, we have so far focused on intentional aspects. However, they are other barriers to learning that have no intentional character but are linked with organizational and institutional configurations of economics.

Transitology: a marginal and disintegrating field of economics?

After an initial period of general interest, transitology became gradually a kind of “area studies”. Yet, area studies are not noble and pure science. Because of their geographical limitation and their applied character, they are very low in the economists’ hierarchy. The evolution is going further and transitology is deemed to become a subfield of economic history, which is no better for its intellectual status. Another trend is a trend towards normalization. Gradually research on transition simply split up into the usual fields of specialization: macroeconomics, industrial organization, finance, trade, etc. and breaks off further into narrower subfields. The interdependencies between the constituent elements of economic systems are lost. Transition appears as a mere context and is not analysed per se. The division of labour between applied and pure theory and the increasingly marginal position of transitology within the field explains why the feedback process on general economics is very limited. This fragmentation, this ‘balkanization’ of East European Studies was accompanied with a westernization of transitology.

A ‘balkanization’ but no Eastern-Europeanization of economics

The funding and delineating of research programmes were to a large extent dominated by western actors and institutions who saw the eastern reality with western lenses, with systems of relevances commonly disconnected from post-socialists contexts. For instance, the fundamental ‘transforming’ function of new forms of social organization in China that did not match Anglo-Saxon conceptions of public and private ownership was poorly understood by mainstream economists. The same could be said of the developmental role of family firms or mixed property firms in transition countries as a whole. Furthermore, the western scholars were themselves dominated by newcomers to the field. A large body of knowledge on the socialist economy had been generated by several generations of Sovietologists and comparative economists. The scholarly knowledge developed by that literature is impressive compared to the caricatural description of socialism that one often finds in the transition literature. Yet, it was largely ignored by mainstream transitology. The field of Sovietology and its accumulated knowledge was considered out-of-date, since the new laws of the market were to reign immediately in the East. Consequently, initial conditions have been widely neglected, a loss of local

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13 It is well worth noticing that this was not the case in sociology, a discipline more sensitive to history and to grounded theory. According to Kogut & Spicer (2004: 2): “In economics, the Soviet Studies community became irrelevant. In sociology, it split into two camps, one with a Marxist framing, the other developing outlines of a sociological theory of ‘transformation’.”
knowledge occurred. This loss of inside knowledge is dramatic because knowledge on economic systems and organizations is locally distributed to a great part; it is grounded as shown by Simon and Hayek. Knowledge does not exist disembodied from the context of its discovery and use, it is “knowledge of the particular circumstances of time and space” (Hayek, 1937). Ironically, it is the socialist calculation debate that led Hayek to develop its theory of knowledge through a distinctive ‘knowledge-based’ critique of socialism. It is because local knowledge is simply not abstract and objective and thus incapable of being used by planners outside of that context to plan the large-scale organization of society, that socialism was largely inefficient. It is maybe because local knowledge was widely neglected in the standard approach to transition planning that many reforms were largely inefficient.

*Information segmentation and information overload*

Economics is facing a problem of both information segmentation and information overload. Alec Nove coined the concept ‘information overload’ to describe the incapacity of planning authorities to process the huge masses of information collected for the planning process. Indeed, the Gosplan was responsible for about 12 million planning indicators (Nove, 1979). When the volume and the heterogeneity of information flows is too high, it is highly difficult to find out which information may be reliable, relevant and essential and to connect it to other potentially interesting pieces of information, as exemplified for instance by intelligence services dealing with the terrorist planning of September 11th. A similar problem appears in economic science. The economist with its bounded rationality cannot cope with the exploding number of scientific journals and other publications. Therefore, it is increasingly difficult to keep up-to-date in any subdiscipline, let alone in a whole subject like transition. To manage the growing flows of literature, he focuses on prestigious, most-quoted papers and insulates himself in subfields. Economics is indeed subdividing endlessly into a rising number of subdisciplines (Hodgson, 2001b). Thus, they are missing links between and within subfields: the retroactions on the discipline of new findings in one speciality are often poor. For instance, the studies in economic literature on China were long viewed as distinctive from transition studies, contrarily to sociology were the division of labour was less marked (Kogut & Spicer, 2004: 8). It afforded the deep public resonance of the Chinese miracle and the clamour of some strong dissenting voices –like the Nobel-awarded one of Stiglitz– to bridge partly the gap.

*Small worlds: the intellectual weakness of strong academic ties*

Moreover, economics is largely structured in small worlds and schools of thoughts hardly communicating with each other. It limited the openness to debate. For example, the alleged consensus among economists on “shock therapy” (Summers, 1994) had strong geographic and organizational bounds: Cambridge, Massachusetts with the Harvard economic department and the MIT. This was nicely put forward by Murrell in his “Transition according to Cambridge, Mass.” (1995). “The Quarterly Journal of Economics is managed by the economics faculty of Harvard University. In 1989, one of the three editors was Lawrence Summers, later chief economist of the World Bank, undersecretary and later secretary of the Treasury, and now president of Harvard. In his role at the treasury, one of Summers responsibilities was to approve the lending by US and international financial institutions to Russia. In 1990, Andrei Shleifer became editor of the QJE and also grew to play a primary role in the transition debate […] Andrei Shleifer, Robert Vishny, Stanley Fischer and Jeffrey Sachs leads the group [of the most published authors in the sample]. Former Professor at Chicago and MIT, Fischer became the chief economist of the World Bank before Summers, then moved into this position at the IMF. Shleifer, Vishny, and Sachs all played direct policy roles as advisers in Russia” (Kogut & Spicer: 42). It should be added that Shleifer had been a student of Summers, and that Summers and Sachs had been students of Fisher. Small worlds can tend to insularity. Here is maybe

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14 Interestingly, the organization in small worlds seems to be a way to cope with problems of information and knowledge circulation in a context of bounded rationality. According to a recent model by Cowan & Jonard (2004: 1557), “the performance of the system exhibits clear ‘small world’ properties, in that the steady-state level of average knowledge is maximal when the structure is a small world (that is, when most connections are local, but roughly 10 percent of them are long distance). The variance of knowledge levels among agents is maximal in the small world region, whereas the coefficient of variation is minimal. We explain these results as reflecting the dynamics of knowledge transmission as affected by the architecture of connections among agents”.
the weakness of strong ties: isolation from dissenting views. In contrast, contestation was fragmented and often located in less prestigious economic institutions or outside the realm of the discipline. On the other hand, it should be stressed that this academic compartmentalization, akin to the role of compartmentalization in biodiversity (Kolasa, 2005), permitted to preserve some intellectual diversity. Alternative views were developed in intellectual niches and survived outside the high-ranking academic journals (Murrell being an exception). This lack of communication between schools of thought is all the more pregnant regarding communication with other disciplines.

Missing links: a lack of interdisciplinarity

According to Hodgson (2001b): “the crucial result [of the increasing academic specialization] is that wide-ranging critical reflection and interdisciplinary conversation are increasingly impaired. It is ever more difficult to take a more general view, and make an impact across the disciplines. Generalists of the orientation of Marx, Mill, Marshall, Durkheim, Pareto, Weber or Schumpeter would find it difficult to obtain a foothold in the modern university. Today, as the grand view is more difficult to obtain, the big questions fall out of favour. The disciplines narrow down on relatively minute technicalities. Sadly, the grand vista is lost”. As a process of system-wide change, transition is indeed a big question. The narrow standard economic focus mutilated its exploration. The conclusions by Kogut & Spicer (2004, 4-5) regarding the World Bank may be generalized: “The lessons from the experience of advising transition economies suggest that academic communities are not organized for the generation of policies that by necessity cross traditional disciplinary boundaries. Our main recommendation is that the World Bank should develop an institutional capability that fosters research on development by external academic that represent multiple disciplines and perspectives”. Without institutional (changing the rules of the game regarding career or article evaluation) and organizational support (founding system, creation of truly pluralistic and interdisciplinary institutes) such a claim will remain oratory. This is another challenge for economics.

4. Concluding remarks

“Whatever situation or course of events can be shown to express these [neoclassical] postulates without mitigation is normal; and wherever a departure from this normal course of things occurs, it is due to disturbing causes, that is to say, to causes not comprised in the main premises of the science, -- and such departures are to be taken account of by way of qualification. Such departures and such qualification are constantly present in the facts to be handled by the science; but, being not congruous with the underlying postulates, they have no place in the body of the science. The laws of the science, that which makes up the economist’s theoretical knowledge, are laws of the normal case. The normal case does not occur in concrete fact. These laws are, therefore, in Cairnes' terminology, 'hypothetical' truths; and the science is a 'hypothetical' science. They apply to concrete facts only as the facts are interpreted and abstracted from, in the light of the underlying postulates. The science is, therefore, a theory of the normal case, a discussion of the concrete facts of life in respect of their degree of approximation to the normal case. That is to say, it is a taxonomic science”.    Thorstein Veblen (1899)

The post-socialist experiment has fostered evolutions in economics and amplified trends already at work. It brought new issues to the foreground, it broadened its interest and local learning processes took place. Nevertheless, these developments are partial and display at least ambiguous and even perverse outcomes.

These developments are partial because the institutional and organizational framing of economics did not allow for enough knowledge confrontation and circulation and put high barriers to learning and feedback. They are partial because various immunization strategies took place. The roots of such a phenomenon lie deeply in the neoclassical “way of thinking” as defined above by Veblen. It has a proclivity to ascribe the gap between model and reality to disturbing causes lying out the main theoretical premises, without putting the latter into question. Its taxonomic bias becomes visible: The economist matches mechanically real worlds against a virtual market yardstick and diagnoses as failure any deviation from neoclassical design, without wondering whether the yardstick itself might not exhibit shortages.
The superficial adjustments that followed these immunization strategies led to widely inconsistent catch-all-theories. These enlarged theories are incoherent at the lower level of empirical tools and at the upper level of the methodological and theoretical “core”. Moreover, these amendments are seemingly blurring off the frontiers between orthodoxy and heterodoxy, adding further to confusion. The impact of transition on economics may even turn out perverse since a renewed kind of economic imperialism seems to be emerging. The economist applies its usual tools and ways of thinking to new fields (politics, institutions etc.) without taking into account decades of developments and achievements in sociology or political science. The economic ‘canons of knowledge’ are indeed enduring habits of thought in which mainstream economics is largely locked-in. Normative indicators, best practices approaches or equilibrium thinking remain more powerful than ever, even though the initial theoretical framework from which such tools derive has starkly evolved and broke up into manifold “small” theories. The neoclassical methodology, reinforced by cumulative institutional factors, is largely path-dependent. The newborn institutional turn falls back on a “benchmarking paradigm” (Chavance 2006), which is all the more influential now as we are living in an economic world where consulting and finance are getting hold of mounting positions.
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