

For a modest and heterodox mainstream economics: an academic manifesto

Luiz Carlos Bresser-Pereira

Paper to be presented to the EAEPE's 2011 Annual Conference, "Schumpeter's Heritage: The Evolution of the Theory of Evolution", Vienna, 27-30 October 2011.

Abstract. The hard core of neoclassical economics (general equilibrium, rational expectations macroeconomics, and endogenous growth models) is essentially mistaken because it adopts a hypothetical-deductive method that is suitable for the methodological sciences, whereas a substantive social science requires an empirical or historical-deductive method. Although Marshallian microeconomics is also hypothetic-deductive, it is a major achievement because it actually founded a methodological science: economic decision-making, later completed by game theory. As deductive thought allows for mathematical reasoning, the resulting models are apparently scientific and constitute the core of mainstream economics. But often they are economic "reasonings", not real theories able to predict and orient. This fact became obvious in the 2008 global financial crisis. Now is the time to change the mainstream; and the present paper is an academic manifesto in this direction. We need a modest and pragmatic economic theory – a Keynesian-structuralist economics that takes into consideration not just agency but structures and institutions too.

Key words: mainstream, orthodoxy, heterodoxy, theories, reasonings, method

JEL Classification: B400, B500, C020

Suddenly, in the aftermath of the 2008 global financial crisis, we all realized that the king was naked. Neoclassical economics, dominant since the late 1970s in universities and in policymaking, repeated the fiasco of 1929: it proved once again unable to explain and predict the behavior of economic systems or to orient policymaking. Based on the assumptions of *homo economicus* and of rational expectations, and adopting a hypothetic-deductive method, neoclassical economics maintained that markets were efficient and self-regulating. They were not. It also maintained that existing market failures were essentially minor, and it did not question the logic of that revealed truth. To ensure financial and price stability,

Luiz Carlos Bresser-Pereira is emeritus professor at Getulio Vargas Foundation, in São Paulo, and doctor honoris causa by the University of Buenos Aires.
www.bresserpereira.org.br bressepereira@gmail.com

fast growth, and fair income distribution, it would be enough to guarantee markets, to protect property rights and contracts, and to keep the public finances in balance. But this, too, was false. Although, according to hypothetical-deductive economic models, markets distribute income according to the marginal productivity of factors, they didn't; although market competition was supposed to cause fast growth automatically, historical experience consistently demonstrated that catching up requires active state action; although deregulated financial markets were supposed to guarantee financial stability, the enormous increase in asset bubbles and financial crises after the collapse of the Bretton Woods agreement proved that this was just not true. Nevertheless, the fall of the Berlin Wall in 1989 and the victory of market economies over the central command economies obscured these simple truths. A triumphant neoliberalism saw in these events the confirmation of the victory of fully free markets over fully regulated markets – as if such extreme opposites bore any relation to reality.

In the academic realm, the prospect of building sophisticated and relatively consistent mathematical models using the hypothetic-deductive mathematical models (rejected by Keynesian and classical or structuralist economists that use the historical-deductive method), made neoclassical economics increasingly attractive to the economics departments of the main universities. Neoclassical economists saw in the abstract models derived from the hypothetical-deductive method a practical device for distinguishing graduate from undergraduate teaching. Their mathematical nature was the “proof” that they were “scientific”.

In the 1990s, while Nobel prizes were awarded to economists in recognition of their mathematical efforts, the dream of perfect markets came to be seen as embodied in the Anglo-Saxon model of capitalism. Also in the 1990s, in the heyday of the neoliberal hegemony, an ideologue of capitalism could confidently assert that all countries would have to adopt the same model of capitalism – a “golden straightjacket”. The criterion distinguishing good from evil administrations, the competent from the “populist”, the backward from the modern, was very simple: whether they were market-friendly or “reformist” as opposed to favoring state regulation.

Yet it is true that such claims were being systematically falsified by the facts: the economic performance of the 30 Neoliberal Years of Capitalism (1979–2008) was substantially inferior to that of the previous 30 Golden Years of Capitalism (1949–78); financial crises had almost disappeared in the Bretton Woods years in which Keynesian economics was mainstream, whereas from the 1980s they increased dramatically in frequency and in destructiveness. It is true that the 2008 global financial crisis was principally the outcome of the deliberate deregulation of financial markets as “scientifically” justified by the efficient markets hypothesis of financial theory. It is true that this crisis was not as severe as Great Depression of the 1930s because governments everywhere adopted heterodox monetary and fiscal policies to counteract it, and because the new developmentalist policies adopted by large middle-income countries like China or India proved effective in bringing back high rates of growth and in sustaining growth in the rich countries. It is true that two developing countries’ political leaders who in the 1990s were portrayed as heroic “reformists” – Boris Yeltsin in Russia and Carlos Menem in Argentina – led their countries into major economic crises. It is true that the successful control of inflation was not principally the outcome of inflation-targeting schemes, but was rather the outcome of rapidly rising productivity in China and the fall in the prices of the manufactured goods that it exports. It is true that the 2008 global financial crisis and the preceding crises in developing countries were the consequence not principally of profligate administrations, but of the irresponsible indebtedness of families, business enterprises, and banks; it was not fiscal populism (the state spending in excess of its revenues, so incurring high budget deficits) but exchange-rate populism (the nation consuming more than it produces, so incurring high current account deficits) that proved highly destabilizing. It is true that in the international financial agencies orthodox economists consistently supported current account deficits or the policy of financing growth with foreign savings. It is also true that behavioral economics repeatedly demonstrated in experimental research that the assumptions of *homo economicus* on which the hypothetical-deductive method is based were just a fiction.

All this is true. Everything condemned neoliberalism and neoclassical economics.

Everything indicated that economists should be less arrogant and ambitious in terms of economic theory, and more down-to-earth in understanding or interpreting economic systems. Everything suggested that in order to achieve growth we should go back to the more modest historical-deductive models developed by the classical economics and by structuralist development economics, and that in order to achieve full employment, stable prices and financial stability we should return to the macroeconomics of Keynes, Kalecki and Minsky.

Yet for 30 years neoclassical economics legitimized a radical free-market ideology, while laypersons in economics (the immense majority of citizens) deferred to a body of knowledge which had a major impact on their lives but which they felt unable to understand because of its mathematical sophistication. A financial crisis as deep as that of 2008 was necessary to make politicians and citizens lose confidence in orthodox economics and policymaking, and to make an increasing number of economists ask about the real “foundations” of their science.

Actually what is required today is a radical critique of neoclassical economics and a less radical but effective critique of orthodox policymaking. Less radical because orthodox policymaking is less unrealistic than neoclassical macroeconomics. What is necessary is not just one theoretical alternative, but several – a plurality of alternatives. Not as formal and fully encompassing, but more modest (and less plagued by certainty) historical-deductive alternatives that open the way for sensible interpretations of, and reasonable predictions about, how economic systems work.

In this paper, I argue in favor of a new mainstream – a mainstream that is *modest* with respect to the truth, *plural* because it is open to different approaches to a very complex and changing reality, and *heterodox* because whereas heterodoxy is no guarantee of accuracy, orthodoxy is always evil because it implies intolerance.

The neoclassical core

Mainstream economics – the economics that has been taught in the graduate programs of the more prestigious universities since the late 1970s – is today a

varied and often contradictory constellation of knowledge. Neoclassical economics remains the *core* of this thought, but in order not to completely lose contact with reality its supporters do not hesitate to coopt – so legitimizing its teaching in graduate economics programs – many schools of thought such as behavioral and experimental economics, new institutionalism, and game theory, which are strictly inconsistent with the neoclassical core. John B. Davis (2007) observes that “the new approaches all maintain fundamental assumptions at odds with neoclassical orthodoxy, and, thus, *should* be seen as heterodox”. Nevertheless, these approaches are part of the mainstream. This process of cooptation – or chain of compromises – went so far, and the contradictions within the mainstream became so deep, that David Colander (2000) felt justified in announcing “the death of neoclassical economics”.¹ He was wrong; it is still alive, and probably will be for some time, in academia, in so far as academic economists insist on building a science as mathematical as physics or even mathematics itself. It is not by asserting that a neoclassical orthodoxy no longer exists that we will defeat it.

What do I mean by “the neoclassical core”? Essentially it includes the Walrasian general equilibrium model, rational expectations macroeconomics, and neoclassical growth models. I exclude Marshallian microeconomics from this core, not because I have forgotten it, but because, according to my view of economics, microeconomics as well as game theory are not part of economics but of a different science: economic decision-making theory. If I had to nominate only five major economists, Alfred Marshall would be on the list because of his diagrammatic theory of markets. Yet, contrary to what he intended and to neoclassical thinking, his microeconomics, combined with Leon Walras’s general equilibrium model, did not provide the “microfoundations” for macroeconomics, but was the foundation of a methodological science called economic decision-making.

Indeed, in so far as I reject the hypothetical-deductive character of economics, I automatically reject the concept of microfoundations. It is not necessary. I don’t

¹ Colander (2000: 127) additionally weakens his claim by adding: “what I am sentencing to death . . . is not the content of neoclassical economics... What I am declaring dead is the term”.

need a model of man in order to develop economics or political economy. Instead of such a useless foundation, Marshall developed an extraordinary method of analyzing markets and making economic decisions. What he presents in his graphic analysis is not *how* economic systems work. Given his passion for historical analysis, he knew well his theory's limitations when it came to understanding trade and industry. Yet unintended consequences may be wonderful. Marshall developed a hypothetical-deductive system of reasoning that is legitimate because it is methodological, because it does not say how economic systems work (this is what a substantive science such as economics is supposed to do), but offers a way of reasoning and making market decisions. It is no coincidence that in the wake of his major contribution many economists, beginning with Lionel Robbins (1932), decided to call economics "the science of choice". It is not; economics is the science of economic systems; but Robbins's definition shows how strong that Marshall's influence was. On the other hand, it is no accident that since the 1970s microeconomics textbooks have devoted many pages to game theory; in so doing their authors have been setting microeconomics alongside an overt branch of decision theory.

The neoclassical core is made up of a cluster of hypothetical-deductive models that aim to offer a closed and all-embracing view of a timeless economic system. In the same way as mathematicians and statisticians depart from some axioms in order to develop their methodological science, the core neoclassical economists depart from the assumptions of perfect rationality or self-interest and of competitive markets in order to deduce the whole economic system. The main outcome of such methodological individualism has been the general equilibrium model. This is an incomplete model, an economic system where there is no money; it is just a nice abstraction. To have it as the core of economics hinders rather than helps the understanding of economic systems. Marshall was wise enough not to adopt it. But the model was rational and consistent. It was enormously attractive to economists searching for the perfect model.

In the 1930s, due principally to the contributions of Michael Kalecki and John Maynard Keynes, a new and powerful model explaining economic systems emerged – macroeconomics – using a historical or empirical approach to the

understanding of economic systems similar to that adopted by the classical political economists. For that reason – and also because it concluded that permanent state regulation of markets was needed to achieve stability and full employment – neoclassical economists rejected it. They rejected it for its lack of precision and for ideological reasons. “This model lacks microfoundations!” they exclaimed. And they proceeded to search for the Holy Grail – a macroeconomic model consistent with microfoundations or with individual rational behavior. In the 1970s Robert Lucas was the “hero” of this quest. On the basis of his model, in which rational expectations played a major role, it was possible to build a macroeconomics endowed with microfoundations, and also to demonstrate that economic policy is ineffective because it is neutralized by the expectations of economic agents.

But there was one problem still to be solved: making the model dynamic and endogenous. Robert Solow had devised a growth model that was consistent with the neoclassical assumption of the full substitutability of labor for capital and vice versa, but in his theory technological progress was exogenous. In 1986 Paul Romer, followed by Robert Lucas, mastered the formidable mathematics that made technological progress an integral part of the growth model – an endogenous variable. It is true that, before them, Smith, Marx and Schumpeter had already come to this conclusion and included it in their theory of economic development... Well, but they had not demonstrated it formally, mathematically – and this is the only thing that counts with neoclassical economics.

After these two additions, economics was complete, “all problems had been solved”. I never heard this claim in relation to the endogenous models, but in relation to macroeconomics I personally heard Robert Lucas say it in the 1980s, during a visit to São Paulo. And now that macroeconomics was “complete” because he had resolved all macroeconomic theoretical problems, he had changed his focus to growth theory...

The new ideas took hold in the universities; they produced the New Classical Economics School at the University of Chicago and the New Keynesian Economics School at the MIT and Harvard University. Although the New Keynesian Economics

School is less orthodox or less radical in so far as it takes into consideration the market failures that are essentially ignored by the New Classical Economics School, both schools are within the core of neoclassical economics, and both develop and teach axioms-based mathematical economics. The debate among their members may be interesting, but it is domestic and ultimately irrelevant.

This is the neoclassical core. Essentially, it is a hubristic castle in the sky, without empirical legitimacy; a product of Platonism – the absurd belief that rational ideas exist independently from reality; an intolerant truth; a new version of medieval scholastics. In truth, the neoclassical core is a non-falsifiable model that cannot and need not be empirically or historically demonstrated. The implicit *truth criterion* is not conformity to an empirically verified reality, but internal coherence, logical consistency – the criterion of the methodological sciences. If reality is not in conformity with the model, this does not mean that the model is wrong. It just means that the market is wrong, and when the market failures are solved, reality will faithfully reflect the true and flawless model...

Do I mean to say that all neoclassical macroeconomics theorizing is useless? Yes. Or, in the words of Willem Buiter (2009: 1) who adds technical competence to having been an external member of the Monetary Policy Committee of the Bank of England, neoclassical economics is “inward-looking distraction at best”. In his words:

Most mainstream macroeconomic theoretical innovations since the 1970s (the New Classical rational expectations revolution associated with such names as Robert E. Lucas Jr., Edward Prescott, Thomas Sargent, Robert Barro etc., and the New Keynesian theorizing of Michael Woodford and many others) have turned out to be self-referential, inward-looking distractions at best.

Or, in the words of Narayana Kocherlakota, President of The Federal Reserve of Minneapolis (2010: 1):

I believe that during the last financial crisis, macroeconomists (and I include myself among them) failed the country, and indeed the world.

For sure, many neoclassical macroeconomists resisted the “purity” of rational expectations macroeconomics and tried to be more empirical. Edward Prescott made an apparently more “successful” attempt in this direction, but his “real

business cycle” theory that became dominant in the universities from the late 1990s is just a new version of rational expectations reasoning. Crises are not related to the business cycle, but derive from exogenous technological shocks that were modeled or simulated with the help of sophisticated mathematical instruments using real data. As for unemployment, it remained the outcome of a rational choice on the part of workers...

More successful in separating itself from the neoclassical core was the “New Economics”, related to the works of Paul Krugman and Joseph Stiglitz, which emphasized imperfect market competition, asymmetric information, and increasing returns of scale. But, as William Milberg (2004: 6) remarks, “the New Economics did not cause an abandonment of choice mathematical modeling”. Yet, in policymaking, these two distinguished economists were able to fully distinguish themselves from orthodox analysts and policymakers.

Other branches

Within the mainstream there are other serious and respectable branches. I have already referred to Marshallian microeconomics and game theory, which actually belong to a distinct science, that is, economic decision-making theory. The same applies to New Institutional Economics, in so far as the institutional economist limits himself to understanding activities that are not economically well coordinated by markets using for that the concept of transaction costs. In this case, New Institutional Economics is just a new and valuable development of the reasoning on market failures; it is part of the science of economic decision-making. Yet, if the new institutionalist economist is more ambitious, if he starts with the absurd assumption that “in the beginning was the word”, and intends to explain the existence of organizations and the state itself by using the concept of transaction costs, he goes astray, and his reasoning turns into just a part of the ideological justification of markets.

Another different science within the mainstream that is also not in its core and should not be rejected is econometrics – an essential tool for economic research. Like microeconomics, it is also a methodological science. Thus, complex

mathematics is fully legitimate. We know that it is a limited tool, that it is full of pitfalls. But we cannot dispense with it. And – what is more important – we should not confuse it with the neoclassical mathematical economics that is in the core of the present mainstream.

Besides these two auxiliary sciences, within the mainstream there are three other large branches that are part of economics but should neither be confused with the neoclassical core nor excluded from a new, heterodox, mainstream. First is the Applied Microeconomics School, which is often confused with the New Economics group but should be distinguished from it because it is substantially more empirical and less theoretical. It is subscribed to by an immense and ever-increasing number of economists who make specific studies trying to correlate some variable with another with the help of econometrics: growth with capital accumulation, or with technological progress, or with institutions; inflation with the money supply, or with the budget deficit, or with previous inflation; educational performance with expenditures on education, or with a specific education method, or with the education of parents; etc., etc. I call these economists the empirical econometric Applied Microeconomics School because they are essentially and legitimately ad hoc. There is no real theory behind it, just some specific and often reasonable hypotheses. As Colander (2000: 139) observes, “modern applied microeconomics consists of a grab bag of models with a model for every purpose”. Most research just demonstrates the obvious, but some of it may be quite interesting and help in policymaking. Its practitioners who today are largely dominant in the universities believe that their studies are based on neoclassical economics, partly because they don’t clearly distinguish neoclassical economics from “general economics”, i.e., from the sum of knowledge that the economics profession shares on how economic systems work independently of the school of thought, and partly because they were taught that no econometric study should be made if there is no theory behind it to explain what is being researched. Thus, they make a regression analysis correlating growth with education, and say that they are “using the Solow model”. They are not. They are just, and often legitimately, correlating two variables that are probably correlated.

Second, we have the simulation models that seek to simulate economic systems through a system of equations. They are called general equilibrium models, but are not really based on the Walrasian general equilibrium model. Instead, they are based on a useful planning tool, Leontief's input-output table. The same applies to the stock-flow models based on the work of Robert W. Clower. These simulations are always precarious, but may be useful if the specific models or partial theories behind them are good, if the data are reliable, and, above all, if they are treated with caution and used with prudence.

Third, within the mainstream but not in the neoclassical core, we have the economic policymakers and their analyses and propositions. Policymakers and policy analysts are numerous: they work for government, for multilateral organizations, for banks, for big business enterprises, for newspapers and specialist magazines. The radical rejection of the neoclassical core that I am proposing and the formation of a new mainstream will make sense only if we distinguish economic policymakers and analysts from the theoretical ideas that they espouse. Among them there are many competent professionals who make competent economic analyses and propose or adopt sensible economic policies. How can they perform relatively well, how can they often be right, if the theory in which they were trained in postgraduate economics programs, and which they assume to form the basis of their reasoning, is wrong? The explanations of this apparent paradox are simple. First, most of these economists are highly intelligent; PhD programs in economics are very selective in their enrollments. Second, the programs don't teach them sensible economics, but do teach them to think abstractly; mathematics and micro-decision theory are very helpful in this respect. Third, they don't apply the absurd macroeconomic and growth models that they were taught in the graduate programs, but the much more reasonable and modest economics that they have learned in good undergraduate textbooks. The macroeconomics in these textbooks remain essentially Keynesian, despite the monetarist and – what is much worse – rational expectation biases that appear here and there in the more recent ones.

The fact that economic policymakers do not necessarily use neoclassical macroeconomics in their work has long been clear to me. I was always in

disagreement with the orthodox economists who offer a panacea for all problems, namely, “to cut public expenditures” (and I also always rejected the opposite tendency of vulgar Keynesians proposing “to increase government expenditures” as a cure for all evils); I was often in disagreement with the IMF’s excessive severity in adjustment processes; since the early 1990s I have been a critic of the Washington consensus on how to promote growth and stability in developing countries. But many of the mistakes involved in such policies derived from ideological preconceptions rather than from theoretical claims. Besides, my disagreement in relation to these policies was often a question of degree. The policies are not always and essentially wrong, as is the case with the neoclassical core.

The neoclassical core is relevant to policymakers not because they based their policies directly on neoclassical macroeconomics, but because neoclassical economics was the “scientific” foundation for the neoliberal ideology that was hegemonic in the 30 years between 1979 and 2008. My personal experience in the policymaking area helped me to understand that. In his excellent book on the independence of central banks, Alan Blinder (1998) wrote that, unlike his fellow central bankers, he believed in macroeconomic models, but that he always used “four of them”, which meant that he didn’t put his trust in “the one and only rational model”. This is a good indication of how precarious economic theory is, and of how important in policymaking are intelligence, an open mind, a wide scope of reasoning, and prudence. But the definitive demonstration of my long-standing conviction (which is also a fascinating “confession” of the failure of the neoclassical macroeconomic core by one of its more well-known contributors) is the revealing paper of Gregory Mankiw (2006), “The macroeconomist as scientist and engineer”. The Harvard professor begins saying that for two years he was in Washington (in the two first years of the G. W. Bush administration he was President of the Council of Economic Advisers), and, to his surprise, he realized that in Washington economic policymakers did not use the “scientific models” that academia had developed:

The sad truth is that the macroeconomic research of the past three decades has had only minor impact on the practical analysis of monetary or fiscal policy... New classical and new Keynesian research has had little impact on practical

macroeconomists who are charged with the messy task of conducting actual monetary and fiscal policy. (2006: 19, 21)

According to Mankiw, policy economists use in their policymaking some kind of “economic engineering” – some simple analyses and corresponding policy propositions. And who is the late economist who inspires such non-scientific economic engineers? None other than John Maynard Keynes! None other than the greatest and most influential economist of the twentieth century. But, according to the neoclassical bias, Keynes did not produce “real science”. Because, when a neoclassical economist reads *The General Theory*, his experience is as summarized by Mankiw: it “is both exhilarating and frustrating... analysis seems incomplete as a matter of logic. Too many threads are left hanging.”

Actually, what makes neoclassical policymakers not so wrong is the fact that they use Keynes’s theories rather than those produced by the “scientists” that teach in the major universities. Being based on Keynes is no guarantee of being right, but it is a guarantee of not being necessarily wrong, while being based on the neoclassical core hinders your thought, and leads you astray in policymaking unless you are intelligent and intuitive enough to ignore this core and its foundations: *homo economicus* and rational expectations.

Reasoning and theory

Endogenous growth models are just irrelevant. This is not the case with the general equilibrium model and neoclassical macroeconomics, which, besides being wrong, are dangerous, because, even though policymakers try not to take them into account, this neoclassical core eventually influences policymaking. Alan Greenspan proved to be competent as a central banker and is a competent macroeconomist. Yet he was unable to predict or to avoid the 2008 crisis. Not because his macroeconomic policies were wrong, but because he believed in the general equilibrium model or in the efficient markets theory, and supported the deregulation of the financial system. The relative disconnection between neoclassical macroeconomics – be it “new classical” (full disconnection), or “new Keynesian” or “new consensus” (partial disconnection) – and policymaking does

not happen by chance. It happens because policymakers' intuition is that this kind of science does not really hold true.

When I say that neoclassical macroeconomics is necessarily wrong because it adopts an *inadequate method*, I am not going too far. When I use the wrong method I cannot arrive at the truth. I know very well that even when I use the right method I will not be assured of arriving at the truth. It is for that reason that I believe that economics should be a modest science.

This does not mean that I believe that all neoclassical models are wrong. For sure, there is some truth in this or that specific concept, in this or that specific model. But they may be correct and useful in so far as they are cases of *reasonings*, not in so far as they are *theories* explaining and orienting economic behavior, but just logical relations between economic variables. According to the historical-deductive method economic theories are generalizations of actual economic behavior that suggest cause and effect relations, which open room for the prediction of outcomes if the respective causes happen.

According to this definition the law of comparative advantages is just an economic reasoning, not a theory. It just says that trade among to countries that produce the same to tradable goods will be rational for both countries even if one of them has absolute advantage in the production of the two goods, provided that in one of them the less efficient country has comparative advantage. As reasoning, there is nothing to object: it is correct. But from it we cannot deduce that the countries that derive policy from it will develop faster. Historically, empirically, it was the opposite that was true. If in the nineteenth century Germany had believed in the argument of the Ricardian economists that its comparative advantages were in agriculture, it would not have still in that century a manufacturing industry substantially more efficient than the British one. An economic reasoning is a static logical exercise, while economic theories, in particular growth theories, are theories that assume economic systems under continuous economic and social change.

Another example is Robert Mundell's neoclassical trilemma. It is again an interesting reasoning, but it is not a generalization of how countries make policies,

because they do not work in the three points of the triangle but somewhere inside it, so that they can combine the three policies. The triangle of impossibilities is a piece of hypothetical-deductive and normative reasoning that, like the law of comparative advantage, may be very dangerous to the countries that apply it hoping to achieve growth with stability. Although Schumpeter made no distinction between economic theory and economic reasoning, he probably had an intuition of it when he called the “Ricardian vice” the inference of policy prescriptions directly from highly abstract models. Theories are built to predict behavior and orient policy. If they don’t, if they are mere reasonings, they give rise to the Ricardian vice, and, so, they are not good theories.

The whole neoclassical approach is wrong because it is a sum of reasonings derived from the hypothetical-deductive method, and, for that reason, are not committed to reality but to a concept of rationality. Heterodox economists (Keynesians, Schumpeterians, behaviorialists, Marxists, old institutionalists, etc.) have been trying for long to show case by case, why each neoclassical model does not correspond to the economic systems that they are supposed to explain. This is a Sisyphean task, because, even if the evidence shows that the neoclassical models do not correspond to reality, the neoclassical economist will argue – or will retain the deep conviction in his heart and mind – that, nevertheless, the models are rational, coherent, consistent, mathematical – and so “are correct”. In Brazil inflation was very, very high in the 1980s, and bore no relation to the money supply (which was fully endogenous) or even to budget deficits; yet monetarist economists insisted on their monetary explanation. Today, again in Brazil, the policy of growth with foreign savings, extremely high interest rates, and a non-neutralized Dutch disease render the exchange rate highly overvalued and cause gradual deindustrialization; yet, despite the evidence that current account deficits do not cause growth, and despite the numbers showing the premature shrinking of manufacturing’s share of GDP, orthodox economists continue to recommend growth with foreign savings, dismiss the high level of the interest rate, and ignore the Dutch disease.

The hypothetical-deductive and the historical-deductive methods

The correct and definitive critique of the general equilibrium model and rational expectations macroeconomics is not empirical but methodological. In so far as neoclassical economics uses an inadequate method, the outcome is necessarily mistaken. I have developed this methodological critique more extensively in a recent article “The two methods and the hard core of economics” (Bresser-Pereira 2009). There are two basic scientific methods, the hypothetical-deductive and the historical-deductive, which correspond to two types of science, the *methodological* sciences that have no objective but to aid thought (like mathematics, econometrics and economic decision-making theory), and the *substantive* sciences, which have an object or a system to explain, and must be subdivided in two types, namely, the natural sciences and the social sciences. Both substantive sciences are supposed to be studied empirically; scientists are supposed to use the *scientific method*, which proceeds from the definition of hypotheses that are subsequently tested against the real world. If the observation of reality permits the scientist to infer regularities and tendencies that reasonably confirm his hypotheses, he will be able to define concepts and make first generalizations or “laws”, deduce from them second-level and third-level generalizations, and so gradually build a science.

In the natural science, this empirical-deductive method has been highly successful; in the social sciences such as economics, it has been less successful, for well-known reasons: because, unlike atoms or cells, individuals are free and, so, unpredictable; because they learn and change their behavior; because institutions also change their behavior; because a general uncertainty permeates individual behavior and economic analysis. For sure, individuals are rational, but making them rational does not make them either certain or predictable – not only because they act on the basis of emotions, or because they ignore, or have a limited knowledge of, the consequences of their actions, but because their objectives go beyond immediate gain. They are reasonably rational, but not rational optimizers; they are rational decision-makers – men and women who make choices under uncertainty.

If economic agents are rational decision-makers, not optimizers, you cannot develop a science hypothetical-deductively as mathematicians or decision theorists do. If the elements with which you work are highly predictable, as in the

case of the elements dealt with by physicists, the deductive aspect of your empirical-deductive method may be empowered. That is why theoretical physics is a successful branch of physics. But when human beings are involved, the hypothetical-deductive method is definitively unacceptable. It allows for mathematical models, models that seem much more precise but in fact are just an illusion, a way of satisfying our arrogance, a way of restricting knowledge to an elite, a device to make you seem a true scientist. Such models are not just unable to explain economic systems but lead to error; they are ideological tools to justify radical economic liberalism and to reject much-needed market regulation – the essential condition for making the wonderful coordinating institution that is the market work well.

A favorite activity of the neoclassical economist is to identify market failures and explain them with elegant formal models; this has been an unlimited source of Nobel prizes for their authors. Yet this does not help the economist much when he seeks to understand and analyze a given economic system. He starts reasoning from the general equilibrium model, but he knows that the model cannot be directly applied, that he has to consider the existence of market failures.² He also knows that he is supposed to abandon one by one the simplifying assumptions that are present in general equilibrium theory. This is a laborious task, full of traps. For that reason, and also because, in his heart, he does not believe that it is really necessary to move away from his beloved general equilibrium, he soon stops the exercise and moves back towards it. Because it was in this way that he learned to reason. Because only in this way do things stay in their right place and economic phenomena may be organized and examined in a way he understands. The alternative is to do what the competent heterodox economist does. It is to begin his analysis of the economic system by assuming that it is a concrete social system, with a history, and to use more modest and less encompassing models to understand it. It is to start from a model that includes market failures. This, too, is a laborious task that few heterodox economists are able to perform well. But the

² The disputes among new classical economists, new Keynesian economists, and the “new consensus” are irrelevant. They are all rational-expectations neoclassical economists.

fact that they don't need to get rid of misleading economic reasonings before analyzing each actual economic system works in their favor.

The alternative

Keynes understood well the pitfalls of theories based on *homo economicus*. When he referred to the "animal spirits" of business entrepreneurs, he was saying that they take decisions, that they make choices under uncertainty, taking into consideration not only their economic interests but also their penchant to invest and grow. When he emphasized the role of uncertainty in economics, he was rejecting the "precise" predictions that stem from hypothetical-deductive reasoning. When Hyman Minsky (1975) put uncertainty at the center of his Keynesian analysis of financial crises, he was confirming this view. When Paul Davidson (1982: @@) criticizes rational expectations macroeconomics because economic process is "non-ergodic", he is ultimately criticizing the assumptions of rational expectations: that

such expectations generate efficient, unbiased forecasts, which do not display any persistent errors when compared to actual outcome overtime, and, so, that information exists and is available for processing by all decision makers. This information, consisting primarily of quantitative time series data, it is assumed, is a finite realization of a stochastic process; from these data the probability distribution of actual outcomes today and for *all* future dates can be estimated.

All this would be fine if stochastic processes were ergodic: if their statistical properties such as mean and variance could be deduced from a single, sufficiently long sample of them. In fact, Davidson argues, they are not: they are non-ergodic. Economic and social action can be *made* ergodic only as a consequence of the illegitimate adoption of the hypothetical-deductive method.

If we must reject neoclassical economics because it is a mistaken attempt to apply the hypothetic-deductive method to a social science, because it is just a castle in the sky, what is the alternative? For sure, not another orthodoxy: not a Keynesian orthodoxy, or a Marxian orthodoxy. In substantive sciences, and particularly in the social sciences, there is no room for orthodoxies. They are wrong by definition, because they admit just one approach to a complex reality that must be viewed and

analyzed from different points of view. Instead of orthodoxy, a heterodox or plural economics is needed: not the product of the hypothetical-deductive method, but the outcome of the historical-deductive method. But this does not mean relativism (anything goes) or plain pragmatism (whatever works) but good pragmatic thinking that values theory and believes in the possibility of truth. The economist needs a broad theoretical framework, such as that developed by the mercantilist and the classical economists to understand capitalist development – the framework to which the central contributions were made by Adam Smith, Marx and Schumpeter. The economist also needs another broad framework to understand the business cycle and macroeconomic policy, along the lines of Keynes and Kalecki. These five major economists did not deduce their models sitting in an armchair. Instead, they built them using the historical-deductive method. In the eighteenth century, Adam Smith realized that certain major economic changes were making England richer than China, and was able to explain why and how to distinguish asset wealth from production wealth. Marx, almost one hundred years later, fully understood the capitalist revolution, and was able to develop a major model of capitalist development based on capital accumulation and technical progress. Schumpeter distinguished the non-active capitalists, or rentiers, from the active capitalists, as Marx had done, called the latter “business entrepreneurs”, and gave them a central role in profit realization, innovation and economic growth. Kalecki and Keynes analyzed the rich national economies after World War I, acknowledged their intrinsic instability, and proposed a new approach to understanding macroeconomic systems based on the observation of economic aggregates.

The theoretical frameworks that these great economists developed were historical-deductive. They are encompassing and illuminating frameworks that opened the way for the economic analysis of specific and historical economic systems. They are historical frameworks, because they are based on observation, because they result from the definition of concepts and from the verification of regularities and tendencies: not all fully defined or arithmomorphic concepts, but, as Georgescu-Roegen (1971) remarked, largely – the more relevant ones – dialectical concepts that are open to different interpretations. Such frameworks

assume the existence of regularities and tendencies that allow them to build models, but not strong regularities, not definitive tendencies. Models that, as Sheila Dow (1996) proposed and Victoria Chick (2004) applied to Keynes's *General Theory*, must be as *open* as the economic systems that they seek to portray. They should not aim to include all the necessary variables, because the researcher knows that this is impossible. More than that: because it is dangerous and arrogant to reduce social reality to closed models.

Does this mean that economics is not a science but just one of the humanities, for which only interpretation (the hermeneutic method) is possible? There is no simple answer to this question. What I am suggesting is that in social sciences the hermeneutic method and, even more, the dialectical method are very helpful. The scientific method, the framing of falsifiable hypotheses followed by empirical verification, is viable and fruitful in the social sciences, but not as much as it is in the natural sciences. On the other hand, the careful interpretation of facts that may mean several things, and the careful use of concepts that are never so clear and definite as we would like but are dialectical, are both necessary. Facts and concepts that are often contradictory. This is particularly true in economics, because all actions involve gains and losses, trade-offs, such that we are never fully capable of sizing up all the possibilities. Investment apparently requires previous savings, but in so far as you invest, you increase savings. In the short run an appreciation of the currency stimulates investment for the production of consumption goods, as wages increase in real terms, but immediately afterwards it hinders investment in tradable goods because it reduces competitiveness. Fiscal expansion may help economic recovery or just cause inflation, depending on the moment it is implemented. Economic agents act rationally in markets, or try to do so, but never attain complete rationality. There is not just one truth, but several truths, depending on the moment, on the circumstances, on the standpoint you are looking things, and on the objectives.

The outcome of all that is that economics is or should be a *modest* science – a science that is committed to the truth, but whose participants know well that they may never be sure that they have attained it. It is a science where the logic of justification should be observed, but where the logic of discovery is more

important or more necessary. One of the reasons why the scientific method is limited is that new economic facts are always occurring and require new theories to explain them. That is the reason why it is necessary to combine the historical-deductive method with the method of the *new historical facts*. Existing models may be able to explain some phenomena like, for instance, inflation, but, at a given moment, a new historical fact occurs – for instance, agents start indexing prices formally and informally – and this historical new fact requires a new theory – the theory of inertial inflation – to explain it.

Economists may find this view frustrating. They would like to master a harder kind of knowledge. A knowledge that has a beginning, a middle and an end, where causes and effects are well defined, where all relevant variables have been taken into consideration; a knowledge that can be precisely expressed with mathematics. But this is a Platonist illusion. As Paul Krugman has remarked (2009):

As I see it, the economics profession went astray because economists, as a group, mistook beauty, clad in impressive-looking mathematics, for truth... But what's almost certain is that economists will have to learn to live with messiness. That is, they will have to acknowledge the importance of irrational and often unpredictable behavior, face up to the often-idiosyncratic imperfections of markets and accept that an elegant economic "theory of everything" is a long way off.

I call the alternative to neoclassical economics that I am presenting "Keynesian-structuralist economics". I don't call it just "Keynesian", because in Keynes there is no theory of economic development; I call it "structuralist" because economic development involves a process of structural change starting with the capitalist revolution of each country, and because capitalist societies are best understood in terms of their structure as formed by three interrelated and permanently changing instances: the economic, the institutional, and the ideological. The relation between these three instances is not just that of cause and effect, but is a dialectical relation that must be viewed historically: in the early periods of economic development, the economic instance tends to prevail over the other two, but, in so far as the basic institution of capitalist societies – the modern state – is formed, the role of ideas and of institutions increases.

Keynesian-structuralist economics is institutionalist. Not new institutionalist, attempting to deduce organizations from transaction costs, but old institutionalist, historical institutionalist, as were the nineteenth-century German Historical School and, in the early twentieth century, the American Institutional School. As Geoffrey Hodgson (2001) emphasizes, old institutionalism is not and should not be all-encompassing. These two schools of thought were overtly institutionalist; the political economy of Smith, Marx and Keynes was implicitly institutionalist in so far as these economists thought in historical terms. According to the structural-Keynesian approach, institutions correspond to the economic structure of society, but this correspondence is loose enough to allow for policymaking or institutional reform. Institutions obviously matter, because they are created and reformed to change behavior. To the extent that modern societies increase state capacity, making the legal system more legitimate and public administration more effective and efficient, the power of democratic politics in reforming institutions increases.

The three schools of thought that, before Keynes, were structuralist or historical and institutionalist (Marxism, the German Historical School and the American Institutional School) suffered from a limitation: they were not sufficiently abstract or general to refute the neoclassical argument that they were not scientific. In fact, the historical-deductive method that they used is not amenable to much formalization. Keynes and Kalecki made a great effort to overcome this limitation, building an extraordinarily general framework to understand economic systems, but they did not “solve” the problem because, given the method they used, only limited generalization and formalization were possible and necessary. It was probably because Keynes understood the strength and generality of his ideas that he called his theory “the general theory”, although he knew that it was not as general as the general equilibrium model.

Keynesian-structuralist economics (or whatever name we give to the new mainstream that is required) is an approach to economics that is permanently changing, because society is permanently changing – because new historical facts are occurring, because new institutions are being generated. It is for that reason, and because of the contradictory character of most variables, that economics must acknowledge that the historical-deductive, dialectical and hermeneutic method is

so necessary. And also because this method is more germane to the logic of discovery in so far as new hypotheses are always required to respond to new historical facts.

All this corresponds to a pragmatic approach. Not pragmatism in vulgar terms, but in terms of the historical pragmatism of Peirce, James and Dewey – a pragmatism that says “no” to simple positivism as well as to Platonism; a pragmatism that is not relativist because it believes in the possibility of the advance of science, but, in dealing with substantive sciences, does not have as a criterion of truth either the positivist empirical confirmation of cause and effect relations, or the logical consistency that Platonists ask for and are satisfied with. Instead, the pragmatic approach wants models that work, models that are explicative and, mostly, have predictive power; it wants broad analyses and simulation studies based on simple economic relations that do not seek to be absolutely true but to be modestly true and practical, to offer often reasonable predictions.

To summarize, the alternative to neoclassical economics is a cluster of open and relatively consistent models or theories that we hope to be true, while knowing that truth is evasive in highly complex and permanently changing economic systems. It is the outcome of a historical-deductive, pragmatic, dialectic and hermeneutic method that develops models that are precarious because non-definitive, but useful because they provide reasonable predictions and orient policymaking.

According to this view, the economist examining *an* economic system is like the doctor examining a patient. He has to examine the problem carefully, ask for tests, take into consideration the several possible theories explaining the symptoms, and finally make a decision that will be as uncertain, as complex and as difficult to size up as the patient’s illness. The only difference is that medicine is a considerably more developed science than economics.

Mainstream?

Competent heterodox economists – economists who are open minded and reject all orthodoxies – are able once again to belong to the mainstream. When I say that, most of my friends in the profession express doubts. The neoclassical core would

be ineradicable in so far as the economic departments in the major universities identify with it. But Keynesian economics was mainstream between the 1950s and the 1970s. Why not again? Why cannot a modest and pragmatic heterodoxy replace an arrogant and misleading orthodoxy? The 2008 crash and the long-term recession that followed it represent a major opportunity for Keynesian-structuralist economics. At the peak of the crisis there was a general return to Keynes and Minsky on the part of policymakers and analysts. Today there is a consensus to the effect that the crisis was not as deep as it could have been because Keynesian policies were adopted.

Mainstream economics is dominant because it prevails in academia as well as in policymaking. A new mainstream economics must make sense to citizens – not only to economists but also to businessmen, politicians, intellectuals, labor leaders, journalists, and the middle class. When heterodox economists are able to build a modest but realistic model of the economy under analysis and to propose a set of policies that are consistent and feasible, they will show themselves to be more effective and more serious than the corresponding orthodox model and policies.

Of the two bastions of the mainstream – policymaking and the university – the first to be conquered is the societal or the policymaking one. Today civil society – society politically oriented – is much more open to alternative economic theories and policies than is the university. Essentially this is because in all rich countries and in most middle-income countries civil society is open and democratic. The same does not apply to the economic departments in the major universities in these countries. They are self-referential, closed to the rest of society. In so far as neoclassical economics is a mathematical theory, it is supposed to be uniquely and absolutely correct. This makes most of its adherents intolerant and intrinsically authoritarian, and explains why in these self-referential departments heterodox thinking, dissent, was banned. Sooner or later this bastion will also fall, or will be changed from within, but this will happen only after the neoclassical core, as I define it here, is deflated and discarded.

When this change eventually takes place, graduate courses in economics will not limit themselves to the presentation of mathematical models and econometrics.

Econometrics will remain central, but, besides that, postgraduate economists will discuss schools of economic thought and the debates that are at the frontier of economic research, and will widely adopt the case method. When you don't have a precise science, the case method is a wonderful method for understanding economic systems, for teaching and thinking about economics.

Heterodox economics may again become dominant, but not all heterodox economists. Just as there are a lot of incompetent orthodox macroeconomists, so there are many incompetent heterodox macroeconomists. Besides, among competent heterodox economists a reasonable number were born to criticize, not to build models and develop policies. For them it is difficult to be part of the mainstream.

The basic obstacle to becoming mainstream that heterodox economists face is of a political nature. Orthodox economists are often also conservative economists, while non-radical heterodox economists are often social democrats. In that connection I have always remembered a comment made 30 years ago by Michel Rocard, the outstanding politician of the French Socialist Party: "the challenge that socialists face is to be more competent in running capitalism than capitalists." The modest and heterodox mainstream economics of the future will not be socialist or necessarily social democratic, but it will not be "pure" science, and probably will be critical of social injustice and of unsustainable development. This fact poses an additional problem for a new mainstream economics: in order to be mainstream Keynesian-structuralist economics must be accepted by citizens in general and in particular by businessmen who tend to be conservative. This is an obstacle but not an insurmountable one; it is possible to combine growth and reasonable stability with a reduction of inequality and the protection of the environment.

References

Blinder, Alan S. (1998) *Central Banks in Theory and Practice*. Cambridge, MA: MIT Press.

- Bresser-Pereira, Luiz Carlos (2009) "The two methods and the hard core of economics", *Journal of Post Keynesian Economics*, 31 (3) (Spring): 493–22.
- Buiter, Willem (2009) "The irrelevance of most 'state of the art' academic monetary policy", in *Vox Research Based Policy* (<http://www.voxeu.org/index.php?q=node/3210>). Originally posted Maverecon on 3 February.
- Chick, Victoria (2004) "On open systems", *Brazilian Journal of Political Economy* 24(1): 3-16.
- Colander, David (2000) "The death of neoclassical economics", *Journal of the History of Economic Thought*, 22 (2): 127–43.
- Davidson, Paul (1982) "Rational expectations: a fallacious foundation for studying crucial decision-making process", *Journal of Post Keynesian Economics*, 5 (2) (Winter, 1982–1983): 182–98.
- Davis, John B. (2007) "Turn and return of orthodoxy in recent economics", University of Amsterdam, copy, available at http://www.hisreco.org/assets/pdf/2007/12_Davis.pdf.
- Dow, Sheila C. (1996) *The Methodology of Macroeconomic Thought*. Cheltenham: Edward Elgar.
- Georgescu-Roegen, Nicholas (1971) *The Entropy Law and the Economic Process*. Cambridge, MA: Harvard University Press.
- Hodgson, Geoffrey M. (2001) *How Economics Forgot History*. London: Routledge.
- Kocherlakota, Narayana (2010) "Modern macroeconomic models as tools for economic policy", The Federal Reserve of Minneapolis, downloaded May 2010.
- Kocherlakota, Narayana (2010) "Modern macroeconomic models as tools for economic policy", The Federal Reserve of Minneapolis, downloaded May.
- Krugman, Paul (1983) "New theories of trade among industrial countries", *American Economic Review*, 73 (1): 31–43.
- Krugman, Paul (1999) *Development, Geography, and Economic Theory*. Cambridge, MA: MIT Press.

- Krugman, Paul (2009) "How did economists get it so wrong?", *The New York Times*, 6 September.
- Mankiw, N. Gregory (2006) "The macroeconomist as scientist and engineer", *Journal of Economic Perspectives*, 20 (4): 29–46.
- Milberg, William (2004) "After the 'New Economics', a pragmatist turn?", American Institute for Economic Research 2004 paper.
- Minsky, Hyman P. (1975) *John Maynard Keynes*. New York: Columbia University Press.
- Murphy, Kevin M., Andrei Shleifer and Robert W. Vishny (1989) "Industrialization and the Big Push", *Journal of Political Economy*, 97 (5): 1003–26.
- Robbins, Lionel (1932 [2007]) *An Essay on the Nature and Significance of Economic Science*. London: Macmillan and Auburn and Mises Institute.
- Romer, Paul (1986) "Increasing returns and long run growth", *Journal of Political Economy*, 94 (5) (October): 1002–37.
- Rosenstein-Rodan, Paul (1943) "Problems of Industrialization in Eastern Europe and South-Eastern Europe", *Economic Journal*, 53 (June): 202–11.