

For a modest and heterodox mainstream economics: an academic manifesto

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Suddenly, in the aftermath of the 2008 global financial crisis, the king was naked. Or we all realized that he was. Neoclassical economics was dominant since the late 1970s, but proved unable to explain and predict the behavior of economic systems. Based on assumptions of the *homo economicus* and of rational expectations, and adopting a hypothetic-deductive method, it concluded that markets were efficient and self-regulated. They were not. It also concluded that the existing market failures were eventually minor and did not questioned the logic of that revealed truth. It would be enough to guarantee markets, to protect property rights and contracts, to defend competition, and to keep public finances balanced that financial and price stability, fast growth, and fair income distribution would materialize. But this was also false. Although hypothetical-deductive economic models may show that income is distributed according to the marginal productivity of factors, non-regulated markets were never even fair; although market competition was supposed to automatically cause fast growth, historical experiences of catching up were always combined with active state action; although inflation was supposed to be under control provided that budget deficit and the supply of money were under control, these two implicit correlations were not confirmed by regression analyses; although deregulated financial markets were supposed to guarantee financial stability, the enormous increase of asset bubbles and financial crises, or, in Kindleberg’s wording, the recurrence of euphoria, manias and panics 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 in the obscured these shortcomings. A triumphant neoliberalism saw in these events the confirmation of the victory of free

markets over regulated markets in coordinating economic systems.

On the academic realm, as part of the neoliberal wave, the supposed “superiority” of formal, hypothetic-deductive mathematical models over the necessarily more modest historical-deductive Keynesian and classical models turned the former increasingly attractive to the economic departments of the main universities, who saw such abstract models as a device for distinguishing graduate from undergraduate teaching and for defining “scientific” research. In the 1990s, while Nobel prizes were awarded to economists in retribution for their mathematical exercises, the dream of free and perfect markets and the ideological claim that this ideal was embodied in the Anglo-Saxon model of capitalism came to be seen as true. In the same 1990s, when neoliberal hegemony was complete, an ideologue of capitalism felt secure to assert that all countries would have to adopt the same growth standard – a “golden straightjacket”. The demand, now, was for neoliberal reform. The simple criterion to distinguish the good from the evil, the competent from the “populist” or backward administrations, was to discern if they were market friendly or not, if they were “reformist” or not.

It is true that the economic performance of the 30 Neoliberal Years of Capitalism (1979-2008) was substantially inferior to the previous 30 Golden Years of Capitalism (1949-78). It is true that the financial crises had almost disappeared in the rich countries and had diminished in the developing ones during the well regulated Bretton Woods year in which Keynesian economics was mainstream. It is true that in the following 30 years financial instability increased dramatically in developing as well as in rich countries, and was “crowned” by the 2008 global financial crisis. It is true that this crisis was not so severe as the 1930s’ Great Depression because governments everywhere adopted radical heterodox monetary and fiscal policies to circumscribe it, and because in some large middle income countries like China or India new developmentalist policies proved effective in bringing back high rates of growth. It is true that the two chiefs of government of developing countries that in the 1990s were portrayed as the heroic “reformists” – Boris Yeltsin in Russia and Carlos Menem in Argentina – led their countries to major economic crises. It is true that the successful control of inflation was not principally the outcome of inflation targeting schemes, but rather the fast increase of productivity in China and the fall of the manufactured goods it exports. It is true that the financial crises in developing countries (that are mostly currency crises, not banking

crises) were not principally the consequence of profligate administrations, but of large current account deficits or exchange rate populism. Or, in other words, it was not fiscal populism (when the state expends more than it receives) that proved highly destabilizing to developing countries, but it was the growth with foreign savings strategy (the nation state expends more than what it produces) recommended by conventional economists. It is true that behavioral and experimental economics proved recurrently in experimental research that the *homo economicus* was just a fiction. All this was true. Everything condemned neoliberalism and neoclassical economics. Everything pointed out the economists should be less arrogant and ambitious in economic theory terms and more foot to the floor in order to understand or interpret economic systems. Everything suggested that in order to understand economics and promote growth we should go back to classical economics and to structuralist development economics, and in order to understand and assure equilibrium and real full employment (not NAIRU...) ¹ we should return to Keynes, Kalecki and Minsky. Yet, it was necessary a financial crisis as deep as the 2008 crisis to make politicians and citizens to lose confidence in orthodox economics and policymaking, and to make an increasing number of economists to 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, as we will see, orthodox policymaking is less unrealistic than neoclassical macroeconomics. What is necessary is not just one theoretical alternative, but several ones – a plural alternative. Not as formal and fully encompassing alternatives as the neoclassical, not alternatives assuming the *homo economicus* and using the hypothetic-deductive method which is inconsistent with a social science as it is economics, but more modest (less plagued by certainty) historical-deductive alternatives that open room for sensible interpretations and reasonably good predictions on how economic systems work.

In this essay, I argue in favor of a new mainstream – a mainstream that is *modest* in respect to the truth, *plural* because open to different approaches to a very complex and changing reality, and *heterodox*, because all orthodoxy is wrong and intolerant.

¹ NAIRU (non-accelerating inflation rate of unemployment) is a concept that allowed neoclassical economist high rates unemployment (more than 6% of the labor force in the US) as necessary to keep inflation under control. This concept is a classic example of ideological self-indulgency.

The neoclassical core

“Mainstream economics” – the economics that is taught in the graduate programs of the more prestigious universities since the late 1970s – today is a varied and often contradictory constellation of knowledge. Neoclassical economics remains the *core* of this thought, but in order not to fully lose contact with reality their supporters do not hesitate in coopting – and so legitimizing its teaching in graduate programs in economics – many lines of thought as behavioral and experimental economics, new institutionalism, and game theory, that in rigorous terms are 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, they are part of the mainstream. This cooptation process or this chain of compromises went so far and the contradictions within the mainstream became so deep that David Colander (2000) felt authorized to declare the death of neoclassical economics.² He is wrong; it is still alive, and probably will be for some time in the academia, in so far as academic economists insist in building a science as mathematical as physics or even as mathematics itself. It is not saying that there are no more orthodox economics that we will defeat it.

What do I mean for the neoclassical core? Essentially it includes the Walrasian general equilibrium model, rational expectations’ macroeconomics, and endogenous growth models. I exclude Marshallian microeconomics from this core not because I forgot it, but because, according to my view of economics, this microeconomics as well as game theory are not part of economics but of economic decision making theory. If I had to nominate only five major economists, Alfred Marshall would be in the list because of his diagrammatic theory of markets. Yet, contrarily to what he intended, and contrarily to neoclassical thinking, his microeconomics combined with Leon Walras’ general equilibrium model did not provide the “microfoundations” for macroeconomics, because they are unnecessary; because, if one rejects the economics based on methodological individualism or on the hypothetic-deductive method as I do, microfoundations make no sense. Instead of such useless foundation, Marshall developed an extraordinary method to analyze markets and make economic decisions. What he is presenting in his graphic analysis is not *how* economic systems do work.

² Colander (2000: 227) additionally weakens his claim by adding: “what I am sentencing to death the content of neoclassical economics... What I am declaring death is the term”.

Given his passion for historical analysis, he knew well his theory's limitation when it comes to understand trade and industry. Yet, unintended consequences may be wonderful ones. 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 as it is economics is supposed to do), but to offer a way of reasoning and making market decisions. It is not by coincidence that after his major contribution many economists, beginning with Lionel Robins (1932), decided to call economics "the science of choice". It is not, it is the science of economic systems; but this definition shows how strong Marshall's influence was. It is not by accident that since the 1970s text books on microeconomics bring a large part on game theory that is overtly a decision theory.

The neoclassical core is formed by a cluster of hypothetical-deductive models that aim to offer a closed and embracing view of a timeless economic system. In the same way that mathematicians and statisticians part from some axioms to develop their methodological science, the core neoclassical economists part from the assumptions of perfect rationality or self-interest and of competitive markets to deduce the whole economic system. The main outcome of such methodological individualism was the general equilibrium model. But an incomplete model, an economic system where there is no money! Just a nice and ingenuous abstraction. Marshall was wise enough not adopting it. But the model was rational, consistent. It brought enormous attraction 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 much more powerful model explaining economic systems emerged – macroeconomics – using a historical or empirical approach, suitable to a social science. For that reason – and also because it assumed the need of permanent state regulation of markets to achieve stability and full employment – neoclassical economists reject it. "This model lacks microfoundations!" they exclaimed. And they went ahead in their search of the grail – of a macroeconomic model consistent with microfoundations or with individual rational behavior. In the 1970s Robert Lucas was the hero of such achievement. On the basis of this model, where rational expectations played a major role, it was possible to demonstrate mathematically that economic policy is ineffective because neutralized by the agents' expectations.

But, additionally, there was the problem of making the model dynamics and endogenous. Robert Solow had devised a growth model that was consistent with the neoclassical assumption of full substitutability of labor for capital and vice versa, but in his theory technological progress was exogenous. In 1986 Paul Romer mastered the immense 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 demonstrate it formally, mathematically – and this is the only thing that counts to neoclassical economics.

After these two additions, economics was complete. All problems have been solved. I never heard this claim in relation to the endogenous models, but in relation to macroeconomics, I personally heard Robert Lucas saying it in the 1980s, in a visit to São Paulo; since he had resolved all macroeconomic theoretical problems, he had changed his focus to growth theory...

The University of Chicago based New Classical Economics and the MIT and Harvard University based New Keynesian Economics. They are in the core of neoclassical economics. Although the former is more orthodox or more radical in so far as market failures are taken seriously into consideration by the later, while essentially ignored by the former, both schools are in 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 a domestic and eventually irrelevant debate.

This is the neoclassical core. Essentially, it is an arrogant castle in the air, without empirical legitimacy; a product of Platonism – the absurd belief in rational ideas existing independently from reality; an intolerant truth; a new version of the Middle Age' scholastic. Actually, the neoclassical core is a non-falseable model that cannot and needs not to be empirically or historically demonstrated. The implicit *truth criterion* is not conformity to reality empirically verified, but internal coherence, logical consistency – the criterion of methodological sciences. If reality is not in conformity to the model, this does not mean that the model is wrong. It just says that the market is wrong, that there are market failures that, once solved, the model will turn true, flawless...

Do I intend to mean that all neoclassical macroeconomics theorizing is useless? Yes. Or, in the words of Willem Buiter (1999: 1) who adds technical competence to having been external MPC member at 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 to the “purity” of rational expectations’ macroeconomics and tried to be more empirical. Edward Prescott made the apparently more “successful” attempt in this direction, but his “real business cycle” theory that became dominant in the universities since 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 to 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 orthodox works of Paul Krugman and Joseph Stiglitz, that 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”.

Other branches

Within the mainstream there are other serious and respectable branches. I already referred to Marshallian microeconomics and game theory that actually belong to a diverse science, economic decision making theory. The same applies to new institutionalist economics, in so far as the institutional economist limits himself to understand the existence of activities not economically well coordinated by markets. In

this case, this is just a new and valuable development of the reasoning on market failures; it is part of the economic decision-making science. Yet, if the new institutionalist economist is more ambitious, if he starts with absurd assumption that “in the beginning was the verb”, and intends to explain the existence of organizations and the state itself by using the transaction costs concept, he goes astray and his reasoning turns just part of the ideological justification of markets.

Another different science within the mainstream but that is also not in its core is econometrics – an essential tool for economic research. As microeconomics, it is also a methodological science. Thus, complex mathematics may be fully legitimate. We know that it is a limited tool, that it is full of pitfalls. But we cannot dispense 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 not be mistaken with the neoclassical core. First, the Applied Microeconomics School which is often confused with the New Economics group, but should be distinguished because it is substantially more empirical and less theoretical. It is formed 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; education performance with expenditures in education, or with a specific education method, or with the education of parents; etc., etc.

These economic studies are part of a school that could be called Applied Microeconomics School. They are essentially ad-hoc. There is not a real theory behind, but just some specific hypotheses. As Colander (2000) underlines “modern applied microeconomics consists of a grab bag of models with a model for every purpose”. Most research just demonstrates the obvious, but some may be quite interesting and helping in policymaking. Their practitioners who today are largely dominant in the universities believe that their studies are based on neoclassical economics. Because they don’t clearly distinguish neoclassical economics with just economics – the sum of knowledge that economists share independently of the school of thought on how market

economic system works. And because they were taught that no econometric should be made if behind it there is not a theory to explain what is being searched. 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 the economic systems through a system of equations. They are called general equilibrium models, but are not really based in the Walrasian general equilibrium model. Instead, they are based on a useful planning tool, the 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 are good, and if the data is trustful, and, specially, if they are viewed with caution and used with prudence.

Third, within the mainstream but not in the neoclassical core, we have the economic policymakers and the analyses and propositions they make. Policymakers are in large number, they work for government, for multilateral organizations, for banks, for big business enterprises, for newspapers and specialized magazines, and also for universities. The radical dismissal of the neoclassical core that I am proposing will only make sense if we distinguish and separate economic policymakers and their ideas. Because 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 the PhD programs in economics, and that they assume to apply, is wrong? The explanations for this apparent paradox are simple. First, most of them are highly intelligent people; PhD programs in economics are very selective. Second, these programs don’t teach them sensible economics, but to think abstractly. Mathematics and micro decision theory are very helpful on this matter. Third, they don’t apply the absurd macroeconomic and growth models that they were taught in the graduate program, but the much more reasonable and modest economics that they have learned in the undergraduate text books. The macroeconomics in these text books remain essentially Keynesian, despite the monetarist and – what is much worse – rational expectation biases that appear here and there in these text books.

The fact that economic policymakers do not use neoclassical macroeconomics in their work has been for long clear to me. I always was in disagreement with the orthodox economists that offer a universal solution to all problems – “to cut public expenditures” –, but I also always rejected the opposite tendency of vulgar Keynesians proposing “to increase government expenditures” as a cure to all evils. I was often in disagreement with IMF’s excessive severity in adjustment process, and since the early 1990s I have been a critique of the Washington consensus on how to promote growth and stability in developing countries. But much of the mistakes involved in such policies derived rather from ideological preconceptions than from theoretical claims. Besides, my disagreement – and, I believe, of most heterodox economists – in relation to these policies was often a question of degree. They are not always and essentially wrong as it is the neoclassical core.

But how can I state so firmly that orthodox economic policymakers don’t use neoclassical economics? Or that they use it very limitedly?. Or that the neoclassical core is only relevant to policymakers is so far as it is the ideological foundation of neoliberalism to which they mostly adhered in the 30 Neoliberal Years of Capitalism? My personal experience in the policymaking area helped me to understand that. Reading Alan Blinder’s (1998) wonderful book on the independence of central banks, in which the eminent economist and policymaker writes that, contrarily to his colleagues central bankers, he believed in the macroeconomic models, but that he always used four of them, was a good indication of how precarious is economic theory, and on how important, in policymaking, is intelligence, open mind, wide scope of reasoning, and prudence. But the definitive “proof” of my ancient conviction, that is also the definitive “confession” of the failure of the neoclassical macroeconomic core by one of his 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 Bush son administration he was president of the Council of Economic Advisers), and, for his surprise, he realized that in Washington economic policymakers did not use the “scientific models” that the 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 else but John Maynard Keynes! None else but the greatest and more influential economist of the twentieth century. But, according to the neoclassical bias, did not produce real science. Because, when a neoclassical economist reads *The General Theory*, his experience is resumed 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 to be so wrong is the fact that they use Keynes’ theories rather than the one produced by the “scientists” that teach in the major universities. To be based on Keynes does not represent a guarantee of being right, but it is a guarantee of not being necessarily wrong; usually it is when your economics and your reasoning are based on the *homo economicus* and on rational expectations.

Critique

Endogenous growth models are just irrelevant. This is not the case of the general equilibrium model and of neoclassical macroeconomics which, besides being necessarily wrong, are dangerous because, despite the fact that policymakers do not take them seriously into account, they have some influence in policymaking. Alan Greenspan proved to be competent as a central banker. Yet, he was unable to predict and to avoid the 2008 crisis because he believed in the general equilibrium model or in the efficient markets theory on finance. The relative disconnection between neoclassical macroeconomics, be it “new classical” (full), 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.

When I say that neoclassical macroeconomics is *necessarily* wrong I am not going too far. This kind of economic reasoning makes no sense. This does not mean that I believe that all neoclassical models are wrong. For sure there is some truth in this or in that specific concept, in this or in that specific model. But the general model, the whole approach, is wrong. Not only because the theory does not fit reality. This is the final

reason, but, in order to demonstrate that, I would have to do what heterodox economists (Keynesians, Schumpeterians, behaviorialists, Marxists, old institutionalists, etc.) having been doing for years and years. I would have to demonstrate case by case why the models do not correspond to the economic systems that they are supposed to explain. This is a work of Sisyphus, because does not matter the evidence that the neoclassical models do not correspond to reality, the neoclassical economist will argue – or will keep well protected in his heart and mind – that, nevertheless, the model is rational, is coherent, is consistent, is mathematical – and so, is right. In Brazil inflation was very, very high in the 1980s, and had no relation with the money supply (which was fully endogenous) nor even with budget deficit, but monetarist economists insisted in their monetary explanation. Today, again in Brazil, the growth with foreign savings policy, extremely high interest rates, and a non-neutralized Dutch disease make the exchange rate highly overvalued and cause gradual deindustrialization, but despite the evidence that the current account deficits do not cause growth, and despite of the numbers showing the premature diminution of manufacturing in GDP, orthodox economists continue to recommend the growth with foreign savings, dismiss the high level of the interest rate, and ignore the Dutch disease.

The right and definitive critique against 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 developed more extensive this methodological critique in a previous work, *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 methods, that correspond to two types of sciences: the *methodological* sciences that have no object but help thinking (like mathematics, econometrics and economic decision making theory) and the *substantive* sciences that have an object or a system to explain, and must be subdivided in two subtypes: the natural and the social sciences. Both substantive sciences are supposed to be studied empirically, scientists are supposed to use the *scientific method* that parts from the definition of hypotheses that are subsequently checked with the real world. If the observation of reality permits the scientist to devise regularities and tendencies that reasonably confirm his hypotheses, he will be able to define concepts and make first generalizations or “laws”, from them

deduce second and third level generalizations and, in this way, gradually building a science.

In the natural science, this empirical-deductive method has been highly successful; in the social sciences as it is economics, not so much, for well-known reasons: because, differently of atoms or of cells, individuals are free, and so, unpredictable; because they learn and change behavior; because institutions also change their behavior, and because they are uncertain on their decisions. For sure they are rational, but making them rational does not make them either certain or predictable. Not only because they act based on emotions, but also because they ignore or have a limited knowledge of the consequences of their actions. They are reasonably rational, but not rational optimizers; they are rational decision-makers – men and women that make choices under uncertainty.

If economic agents are rational decision-makers, not optimizers, you cannot develop a science hypothetic-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 concerned, the hypothetical-deductive method is definitively unacceptable. It allows for mathematical models, models that seem much more precise, but that, in fact, are just an illusion, a way of satisfying our arrogance, a way of restricting knowledge to an elected few, a device to make you seem a true scientist. Such models are not just unable to explain economic system; they lead to error; they are ideological tools to justify radical economic liberalism and to reject the much needed market regulation? – the essential condition to markets – this wonderful coordinating institution – work well.?

When a neoclassical economist searches to understand and analyze a given economic system, he starts from the general equilibrium model. He knows that the model cannot be directly applied. He acknowledges the existence of market failures.³ Since they part of perfect competition, one of the favorite actions of neoclassical economists is to look for market failures – is to identify market failures and explain them with elegant formal

³ The disputes among new classical, new Keynesians, and the “new consensus” are irrelevant. They are all rational expectations neoclassical economists.

models; this has been an unlimited source of Nobel prizes for their authors. Although he knows that the general equilibrium model does not help to explain what he intends to explain, his fate is to start from it. Because it was in this way that he learned to reason. Because only in this way things stay in the right place and the economic phenomena may be organized and examined in an understandable way by him. The alternative is to do what the competent heterodox economist does. It is to begin his analysis of the economic system assuming that it is a concrete social system, with a history, and to use more modest or less encompassing models. Is to start from a model where market failures are part of it. But this is almost impossible to the orthodox economist. He must start from the general equilibrium and to abandon, one by one, the model's simplifying assumptions. This is a long and extenuating task, full of traps. So, and because in "his heart" he does not believe that this is really necessary, – that he must take distance from his beloved general equilibrium – he stops the exercise and turns back to something near to general equilibrium.

The alternative

Keynes understood well the pitfalls of theories based on *homo economicus*. When he referred to the "animal spirits" of the business entrepreneurs, he was saying that they take decisions – choices under uncertainty. When he emphasized the role of uncertainty in economics, he was rejecting the "precise" predictions that are originated from hypothetical-deductive reasoning. When Hyman Minsky (1975) put uncertainty in the core of his Keynesian analysis of financial crises, he was confirming this view. When Paul Davidson (1982) criticizes rational expectations macroeconomics because economic process are "non-ergodic", he is eventually criticizing rational expectations' assumptions: 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 fine if stochastic processes were ergodic: if its statistical properties such as mean and variance can be deduced from a single, sufficiently long sample of the process. In fact, Davidson

argues, they are not: they are non-ergodic. Economic and social action can only be *made* ergodic in consequence of the abusive adoption of the hypothetical-deductive method.

Since we must reject neoclassical economics because it is the mistaken attempt to apply the hypothetic-deductive method to a social science, because it is just a castle in the air, which is the alternative? For sure, not other orthodoxy: a Keynesian orthodoxy, 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 propose just one approach to a complex reality that must be viewed and analyzed under different points. Instead of an orthodoxy, it is necessary a heterodoxy or plural economics or political economy. Instead of the product of the hypothetical-deductive method, the outcome of the historical-deductive method.

But this does not mean relativism (anything goes), nor 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 the framework developed by the mercantilist and the classical economists to understand capitalist development – a framework to which the central contributions were made by Adam Smith, Marx and Schumpeter – and they need another broad framework to understand the business cycle and macroeconomic policy, in the line of Keynes and Kalecki. These five major economists did not sit in one armchair to deduce their models. Instead, they use the historical-deductive method to build it. In the eighteenth century, Adam Smith realized that some major economic change was 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 or rentier 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, understood their intrinsic instability, and proposed a new macroeconomic approach to understand macroeconomic systems based on the observation of economic aggregates. The models or, as I prefer to say, the theoretical frameworks that these great economists developed were historical-deductive. They are encompassing and illuminating

frameworks that opened room for the economic analysis of specific and dated economic systems. They are historical frameworks, because they are based on the observation, because they result of the definition of concepts, and of the verification of regularities and tendencies. Not all fully defined or arithmomorphic concepts, but, as Georgescu-Roegen (1971) remarked, many of them – the more relevant – dialectical concepts that are open to different interpretations. Not strong regularities, not definitive tendencies, but sufficiently frequent regularities and tendencies that allow the economist to build his models. Models that, as Sheila Dow (1996) proposed and Victoria Chick (2004) applied to Keynes' *General Theory*, must be *open* as the economic systems that they intend to portray are open. They should not intend 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 interpretations, the hermeneutic method, is possible? I am not saying that. But I am suggesting that, in social sciences, the hermeneutic and principally the dialectical method are very much helpful. The cause and effect scientific method, the definition of falsifiable hypotheses followed by the empirical verification, is viable and fruitful, but not as much as in the natural sciences. On the other hand, the careful interpretation of facts that may mean several things, and the careful use of concepts which are never so clear and definite as we would like are much necessary. And, what is more important, these facts and concepts are often contradictory. This is particularly true in economics, because all actions involve gains and losses, trade-offs, that we are never fully capable of sizing up. Investment apparently requires previous savings, but in so far as you invest, you increase savings. In the short-short run an appreciation of the national currency stimulates investment in consumption goods and cheapens the importation of capital goods, but immediately after, in the short run, it hinders investments in tradable goods because competitiveness falls down. Fiscal expansion may help economic recovery or just cause inflation, depending on the moment it is decided. Economic agents act rationally in markets, or try to do so, but never fully reach to be rational. And so on. There is not just one truth, but several truths, depending on the moment, depending on the circumstances, depending on the objectives.

The consequence 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 never may be sure

that they got it. It is a science where the logic of justification should be obeyed, but where the logic discovery is more important or, at least, so important. One of the reasons why the scientific method is limited is that economic facts are often new – and new historical facts require new theories to explain them. That is the reason why I always combine the historical-deductive method with the method of the historical new facts. Existing models may be able to explain some phenomena like, for instance, inflation, but, in a given moment, a new historical fact occurs – agents start indexing formal and informally prices – and this historical new fact requires a new theory – the theory of inertial inflation – to explain it.

Economists may be frustrated with this view. They would like to master harder kind of knowledge. A knowledge that has beginning, middle and end, where causes and effects are well disposed, where all relevant variables were taken into consideration. A knowledge that can be precisely expressed with mathematics. But this is just a neoclassical and Platonist illusion. An illusion that the young Paul Krugman (1983) shared, and that was fruitful enough to permit him to formalize a model of international trade based on imperfect competition and increasing returns to scale that explained large volume intra-industry trade (and probably was the main reason for his Nobel prize), but that was misleading enough to allow him to say that the Murphy, Schleifer and Vishny's (1989) nice formalization of the big push model was more significant than Rosenstein-Rodan's (1943) original model. Anyway, mature Krugman (2009) learned the lesson of reality, and now argues for a modest economics. In his words:

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 that I am presenting to neoclassical economics the "Keynesian-structuralist economics". I don't call it just "Keynesian", because there is not in Keynes a theory of economic development; I call it "structuralist" because economic development involves a process of structural change, because it does not explain all societies but only capitalist societies, and because capitalist societies are better understood if we understand them in terms of its structure as formed by three interrelated and permanently changing structures: the economic, the institutional, and

the ideological instance. The relation between these three instances is not just of cause and effect, but 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, not attempting to make hypothetic-deductive rationality consistent with institutions, but old institutionalist – based on the institutionalism that characterized the nineteenth century German Historical School and in the early twentieth century 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 were implicitly institutionalist in so far as they 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 policymaking or institutional reform. Institutions obviously matter, because they are created and reformed to change behavior and the social systems in which human action takes place. To the extent that modern societies increase state capacity – make the law 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 (the Marxist, the German Historical School and the American Institutional School) suffered from one limitation: they were not sufficiently abstract or general to stop the neoclassical argument that they are not scientific. In fact, the historical-deductive method that they used is not friendly to much formalization. Keynes and Kalecki made a long way in responding to this limitation, building an extraordinarily general framework to understand economic system, 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 other name that 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 happening, because new institutions are being defined. It is for that reason and for the contradictory character of most variables that it takes into consideration that the historical-deductive, the dialectical and the hermeneutic methods are so necessary for it. And also because these methods are more germane to the logic of discovery in so far as new hypotheses are always being required to respond to the 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 criterion of truth just the positivist empirical confirmation of cause and effect relations, nor of just logical consistency as 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 intend to be true but to be modestly true and practical: offer often reasonable predictions.

Summing up, the alternative to neoclassical economics is a Keynesian-structuralist economics, is a cluster of open and relatively consistent models that we hope to be true, although knowing how the truth is evasive in the highly complex and permanently changing modern economic systems. It is the outcome of a historical deductive, pragmatic, dialectic and hermeneutic method that develops precarious because non-definitive models, although useful, provide reasonable prediction and help policymaking.

According to this view, the economist *vis à vis* the economic system is like the doctor facing 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 much uncertain, as more complex and difficult to size up is the patient’s illness or the economic system’s problem. The only difference is that medicine is a considerably more developed science than economics.

Mainstream?

Competent heterodox economists – economists that are open minded and reject all orthodoxies – are able to be again in the mainstream. When I say that, most of my friends in the profession doubt. The neoclassical core would be inexpugnable in so far as economic departments in the major universities are self-referred. But Keynesian economics was mainstream between the 1950s and the 1970s. Why not again? Why a modest and pragmatic heterodoxy cannot replace an arrogant and useless orthodoxy? The 2008 crash and the long term recession that followed it represent a major window of opportunity to Keynesian-structuralist economics. In 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 on the fact 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 the academia, as well as in policymaking. A new mainstream economics must make sense to civil society, that is, to a politically organized society. It must make sense 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 group of policies that are consistent and feasible, they will prove to be more effective and more serious than the corresponding orthodox model and policies. The proposal of the Ten Theses on New Developmentalism based on a structuralist development macroeconomics is a good example of what I am saying.⁴

Given the two bastions of the mainstream – the policymaking and the university – the first bastion to be conquered is the societal or the policymaking one. Today, civil society in each country is much more open to alternative economic theories and policies than the university. Essentially because in all rich countries and in most middle income countries, civil society is open, democratic. The same does not apply to the economic departments in the major universities in these countries. They are self-referred, closed to the rest of society. In so far as neoclassical economics is a mathematical theory, it is supposed to be uniquely right, absolutely right. What makes most of their adherents

⁴ The Ten Theses on New Developmentalism apply principally to middle income countries. They were discussed in a workshop in São Paulo and originally subscribed by 80 leading heterodox economists dealing with macroeconomics and development. The document is open to additional subscriptions in www.tentheseonnewdevelopmentalism.com.

intolerant, intrinsically authoritarian, and explains why in these self-referred departments, heterodox thinking, dissent, was banned. Sooner or later this bastion will also fall, or will be changed from within, but this will only happen after the neoclassical core, as I here defined, gets deflated and is discarded.

When this change eventually takes place, graduate courses in economics will not limit themselves to open and close mathematical models, but, instead, will continue to develop methods and, particularly econometrics, will discuss past schools of economic thought, and, the debates that are in the frontier of economic research, and will adopt widely the case method. When you don't have a precise science, the case method is a wonderful method of researching economic systems, of teaching and of thinking economically.

Heterodox economics may become again dominant, but not all heterodox economists. As there are a lot of incompetent orthodox macroeconomists there are also many incompetent heterodox macroeconomists. Besides, among competent heterodox economist there is a reasonable number that were born to criticize, not to build models and develop policies. By definition, the critical economist is never in the mainstream. He is always against whichever power system is in place, and the mainstream is a power system. On this matter, incompetent neoclassical economists are more acceptable than heterodox ones because what they say is more conservative, or not so much against common sense. While, for instance, the incompetent orthodox economists offer budgetary contraction as a remedy to everything, the incompetent heterodox economist proposes fiscal expansion. Both are wrong, biased, but the heterodox mistake is more damaging.

The basic obstacle that heterodox economists face to become mainstream is of political nature. Orthodox economists are often also conservative economists while non-radical heterodox economists are often democratic socialists or social-democratic. Given that, I always remember comment made 30 years ago by Michel Rocard, the outstanding politician of the French Socialist Party. He said: "the challenge that socialist face is to be more competent in running capitalism than capitalists". The modest and heterodox mainstream economics ahead will not be socialist, nor, 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 obstacle for a new mainstream economics, but not an insurmountable one.

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