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INNOVATIONS AND REGRESS IN ECONOMIC THEORY

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**ABSTRACT: Inspired by the 2015 IMF Annual Research Conference on 'Unconventional monetary policies'/Sixteenth...2015/we ask how far interpretation and generalization of major policy innovations following the Great Recession of 2007-2009, have been received in the sanctuary of economics and its education through the 'global economics program' for PhDs. We survey a series of theoretical and methodological innovations which may be seen as building blocs for an emerging new, more policy relevant paradigm<sup>2</sup>.**

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This article is a sequel to our earlier reflections in this Journal/Csaba, 2009/ on the limitations of the current mainstream approach to economics. As we documented in the previous paper, an unprecedented concentration of Nobel Prizes, of appreciations – from Top Twenty journal rankings to textbooks and doctoral programs - has emerged, with a heavy dose of Americanization and standardization. If we take only the past four decades, it is less than 10 per cent of those awarded who were not working full or part time in the United States at the time of their decoration. The rule of formal exposition and modelling is easy to document, either through a mere listing of Nobel winners' appreciations on [www.nobelprize.org](http://www.nobelprize.org), or via a glimpse into the titles of any of the top twenty journals/less the *Journal of Economic Literature*, which is all-encompassing by its nature/. In the middle of the global financial crisis we asked if new, more productive and policy-oriented approaches will emerge, as policy-makers have turned increasingly their backs to the abstract and formal neoclassical school, the current mainstream.

This line of thinking has always made clear, that for it the fundamental condition for being accepted as academically sound and scientific is the mathematical exposition of any problem and „putting numbers” on each component. One may cite school molding personalities' claims to this end at will: from Robert Lucas/1996,p.664/ to more recently Paul Romer/2015/, a star professor from NYU. The first – in his Nobel lecture –

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<sup>2</sup> Useful comments by György Szakolczai, the Editor, and two anonymous referees on the previous version are appreciated, with the usual caveats.

explained the difference between Adams Smith/David Hume and himself in terms of his ability to put numbers on what used to be intuition. The latter – a formal doctoral student of Lucas - recently explained at great length, that the academic nature of growth theory rests on its deep and meaningful use of mathematical theory. He contrasts this with the current fashion of „mathiness”, which is just a formal resort to expressing economic ideas in a maths formula, whether or not it is helpful and supportive of the core argument.

Rather than allowing for the inclusion of obscure components as incentives, culture and the like, endogenous growth theory prides itself in its ability to give exact numbers for each component. Some, most notably Colander et al/2004/ tend to depict the mainstream as a changing frontier, allowing for the incorporation of new and new insights. In reality the type of mathematics it relies on, severely limits the type of questions that may be sensibly posited, thus pre-judges the outcomes to a large degree. This is what the long exorcised Ludwig von Mises/1933/2003, pp28-37/ objected to the formalization of economics: if axioms contain already the outcomes, much of the analysis is a game, rather than serious academic artwork. Furthermore, human and social behavior does follow a different logic from those of the smallest units of matter.

Looking back to the Great Recession with hindsight we may attempt taking stock and looking ahead. Beyond doubt, a large number of non-mainstream authors have reached to fame, and proponents of non-conventional approaches have also gained some respectability. True, this holds more for the policy arena and less for the academe. In the latter innovations though do occur, these tend to remain on the fringes. In the 'global economics program' of standardized curricula, both at MA and PhD levels, not much has changed. The rule of mathematical formalism – „mathiness” - and a large degree of disregard of reality still prevails, as we shall document below.

But the purpose of the current exercise is not to offer yet another litany of complaints. *Rather we try to cover innovations which do abound in the academe, if not in the curricula.* The 2000s has witnessed an unprecedented drift between economic theory and policy. While the former continues to be dominated by an ever more rigid, standardized and Americanized formal approach, 'real world economics' has increasingly turned its back on the self-referential output of 'pure economics'. This drift has neatly been documented by the latest ranking of the *Frankfurter Allgemeine*<sup>3</sup>, testifying the unbridgable gap between standards of the pure academe and policy advocacy, let alone impacts on the media and public discourse. While some of the divergence is trivial, it is hard to deny that many Nobel winners or close to Nobels are

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<sup>3</sup> FAZ Ökonomenranking: Deutschlands einflussreichste ökonomen.FAZ, 4 September, 2015.

entirely unknown in the world of policy-making, both at the national and corporate levels. The latter is only in part due to the over-technicized ways of fashionable and accepted academic expression. As we argued earlier the value-free technical approach to complex social phenomena is often inadequate for analytical purposes. This is so as it often does not allow for proper comprehension and description of many phenomena, such as financial crises.

The 'benefit of crisis' in this case implied that new, *unconventional theories and methods emerged, both in policy-making and in the academe*. Policy-making has revolved around the ideas of quantitative easing/QE, both in monetary and fiscal affairs. QE has long been a controversial subject, which received relatively little attention until recently.<sup>4</sup> Perhaps the most interesting novelty was condoning, rather than merely tolerating of fiscal and monetary laxity as simultaneous and sustaining policies/ Turner, 2015/. It is however remarkable, that those policy innovations – such as the changing role of European Central Bank with its stimulus package and unlimited supply of liquidity – or consequences of lastingly negative real and even nominal rates of interest on deposits<sup>5</sup> – are being analyzed by research departments and conferences of banks and fiscal authorities, rather than in the academe proper. Likewise, contributions in the Top Ten journals do not revolve around such issues. It goes without saying that university curricula are silent on such issues.

What is truly new in the post-2008 period that we observe sustaining policy practices firmly rejected in current macro textbooks, as exploding public debts, negative real rates of interest sustaining for 5-6 years, or quantitative easing at times of recovery. *These truly unconventional practices are observable in the EU, in the US and Japan alike for longish periods. One may ask: did we arrive to a watershed?*

Economic theory has yet to cope in full with the innovations. This should not come as a surprise if we consider that the *General Theory* of Lord Keynes/1936/2007/, generalizing the lessons from fighting the Great Depression, was published more than three years after the crisis proper ended in Europe. Likewise in the USA experimentation along the lines of the New Deal went on for years without ever finding a proper theoretical foundation.

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<sup>4</sup> For one of the rare exceptions cf van der END et al/2015/.

<sup>5</sup> The ECB nominal lead deposit rate of *minus* 0.4 pc raised the eye-brows of even the customary supporters of the radical QE line of Mario Draghi, especially in the banking community. Cf: Senior European bankers concerned over ECB rate cut. *Financial Times*, 9 March, 2016/online edition/. They note: such a move, by enhancing bank losses, translates in reality to lending rate increases, thus less, rather than more lending activity.

Therefore we aim in this paper to document the emergence of a new, *policy-relevant economics*. What we observe is that innovations in the field of monetary policy seem to have reached their limits, and the post-crisis policies are likely to be fundamentally different from the pre-crisis period/Ihrig et al, 2015/. Similarly the study of deeper roots of the crisis warn us against putting all blame on financial excesses, and traditional variables of the real economy, such as uneven technological progress and consequences of monopolistic competition are back on the agenda/Snowden, N., 2015/. Last but not at all least, detailed studies of fiscal policies have shown that the conventional – and widely shared – criticism of alleged over-doses of austerity in terms of fiscal policies is in part factually unsubstantiated, in part theoretically unfounded/Tanzi, 2015/.

For these and other reasons there is a need to adopt an approach *re-integrating theory and policy*. This is distinct from previous approaches in a number of planes. First: it avoids the lack of theoretical anchoring. Second, it avoids the over-theorized and non-contextual applications that are rooted in the current mainstream of the neoclassical synthesis. Third, it accepts as a fact of life that studying macroeconomic processes in general and of inter- or transnational processes in particular, is by its very nature a value-loaded exercise *that can not and should not be confined to assessing technical alternatives and options, feasibility studies and quantitative outcomes*/Kolodko, 2014/. While all these are indispensable and useful, this is not the entire ball-game. It matters, that we should be able to answer the question mostly swept under the carpet in the economics of the post-WWII period: *cui bono?*

### *Policy Against Theory*

The term 'unconventional' developed into a liberally used category. Many events and practices are being described by it, from Hungarian economic policy practices of the second and third Orbán Governments, via Greek crisis management and the ongoing quantitative easing of the FED. Negative real rates of interest, for instance, would have been inconceivable for decades. Likewise, public debt/GDP levels surpassing the 90 per cent threshold in many advanced economies, including the USA, the European Union and Japan, are a novelty. And so is the fact that *we see no indication of an 'exit strategy'* that would seriously and strategically aim at remedying the mounting of – public and private – debts in a strategic fashion. In 2009-2010, it was commonly presumed that with the recession gone all major economies will enact fiscal retrenchment, one way or another. In reality, nothing of the sort happened in most large economies.

On the one hand, observers of the policy arena tend to formulate the claim: „from now on, nothing of the received wisdom holds”. Perhaps understandably, guardians of academic chastity, in the theoretical departments and doctoral schools of economics, have reacted with a degree of unprecedented rigidity, *rejecting any room for revisionism as unscientific and woodoo economics*. The output of leading journals – the top twenty of IDEAS/REPEC – continue to be filled with speculative modelling, following much the same lines and standards as in the pre-crisis period. Very few, if any, policy-oriented or policy-inspired papers are brought in *The Journal of Political Economy*, in the *Quarterly Journal of Economics*, *The Economic Journal* or even in the more policy-oriented outlet, *American Economic Review*, especially if we exclude the Papers and Proceedings edition of May. The drift between practitioners and theorists, which has never been small, has developed into a Chinese Wall, and policy debates are being conducted in entirely different fora.

Unconventional is often just a hasty generalization. In most cases *policy-makers themselves call their actions this way*, in order to gain visibility and respectability, as opposed to traditional, worn-down and 'provenly inefficient' methods. It all started perhaps with President Reagan's 'supply-side revolution', that combined tax cuts with expenditure increases/for Star Wars and others/. Last but not at all least, crisis management in Europe has often led to – temporary or even permanent – nationalizations. Especially bank bail-outs, but other forms of policy interventionism – sometimes nicknamed as 'industrial policy' or 're-industrialization' in EU parlance – have gradually changed the face of the European social market economy. Critical assessments/Voszka, 2015/ describe Hungarian statism - a case in point – as an old-new model, reviving *state-led capitalism*, known from classical textbooks on comparative economic systems' analysis. In other words: if statism perpetuates, this is the most palpable outcome of what unconventional policies of the 2010-16 period translated on the ground.

Let us note: in the global economic literature *a different interpretation of the term* seems to have prevailed. In a much-publicized bestseller Nobel winner Paul Krugman/2012/ rejects one of the fundamental features of Hungarian policies, i.e the focus on stabilizing and even diminishing the public debt/GDP ratio<sup>6</sup> as a focus of macroeconomic considerations, as well as the supply side approach characterized by the priority of flat tax – a major ideological component of the second and third Orbán governments. For Krugman the key is exactly the opposite: sustaining fiscal and

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<sup>6</sup> Hungary's public debt/GDP ratio decreased from 82 pc to 75.6 pc in 2010-2015, while that of the Euro-zone increased from 83.5 to 92.1 to 2014 and around 93.5 pc.

monetary laxity, attachment to quantitative easing for long periods/not only during deep recessions, as with Keynes/ and rejection of the perceived need to balance public budgets

An interesting mixture is the position of another Nobel winner Joseph E. Stiglitz/2015/. He takes a politically less and professionally more radical position than his fellow Nobel winner. He develops a fundamentally renewed version of neo-Keynesianism. Unlike the traditional line, he focuses on the need to detect and remedy *structural weaknesses* as opposed to the traditional priority of reviving effective demand, which is of course a cyclical prescription only. He also highlights the need for diminishing inequalities, whose growth has been rampant, especially in the United States as his paper amply documents. Finally he also advocates activist governmental policies, but one not based on ideological prejudice, but on common sense. Impacts of government intervention depend crucially on *the size and working of the fiscal multiplier*. If an economy is sensitive to financial signals there is ample room for reflation the economy. But in the opposite case *austerity may actually worsen the retrenchment*, triggering disproportionate falls in output and employment.

As Stiglitz elaborates above, the lasting lagging of poor strata does not follow the predictions of the conventional microeconomic model. *It does not create incentives for additional work and more performance*. Under real world conditions impoverishment breeds exclusion, illnesses and alienation, all features known from the sociological surveys of pockets of deep poverty. The latter tend to reproduce itself – as in the ghettos of depressed urban areas – rather than create multiple incentives for moral and economic improvement, as the neoclassical textbook view would have it. From a macro perspective this is a recipe for stagnant consumption and low employment *in the long run*, not only for transitory periods.

Without further ado we may observe that in economics, like in other disciplines, *crises also pose opportunities for testing old, established theories and developing new ones*. It is certainly legitimate to observe at this point that *no theory may or do aspire to explain each and every case*. Even in the natural sciences, as in physics or chemistry, finding a counter-example or an outlier does not suffice to discard entire complex and sophisticated theoretical architectures, especially if we do find an explanation for the deviance within the given framework. Decision -making follows a series of considerations, and theoretical coherence is just one of the many factors. Participant observers would easily agree: other motives, as fitness to present the idea in the electronic media, considerations of electoral success, perceptions/of individuals and issues/, prejudices, or the sheer bad quality of preliminary data on which most actual

decisions do rest, *all translate in interactions that may hardly follow the logic of rational expectations*, even if it was the priority. On the contrary, as the late Rüdiger Dornbusch/1993/ explained in his last book quarter of a century ago, *communication can be, and often indeed is, as important as action*. If for no other reasons, because it shapes the expectations of millions of agents, which react – or do not react – to actions and numbers, depending of their perception of reality.

### *The Empire Strikes Back*

In economics – just like in arts – each era is described by its mainstream. What is mainstream and what is heterodox is often a matter of retrospective judgements, as contemporaries often do not agree. What becomes mainstream is prescribed by such factors as demand, fashions, style, taste, emergence of new instruments/of analysis/, new insights, news techniques. Not least, these follow *balance of power changes*, both in arts, politics and the academe. In our case standardized, formalized and Americanized set of norms has become the standard over the past quarter of a century.

What it means is easy to document. Academic journals are meticulously ranked by their publishers and other peers – all relying on a single business service consultancy, Thompson Reuters and the database produced by them. From among the journals which make into the sanctuary of *the 'economics' entry, i.e those who qualify at all to be considered within the limes*<sup>7</sup>, 19 out of 20 of the top rankings of IDEAS/REPEC are edited and mostly published in the United States of America. Let us note: no similar concentration of 'quality' is observable in any other discipline, be that nuclear physics, medicine or chemistry, where US schools obviously take a leading position, but not such a monopoly.

This is itself an anomaly, given the lack of replica in other disciplines. The latter means that not only BA and MA, but *PhD level training follows American standards, structures and evaluation patterns, irrespective of the problems of local societies*. The latter may well range from reform of the Common Agricultural Policy to fighting hunger, while in

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<sup>7</sup> We have come to so much over-appreciation of the rankings that the publishers themselves have difficulty in coping with inter-disciplinary journals/a growing crowd/. Oftentimes they publish different rankings for the same journal, already at the front page of it. For instance the highly stimulating *World Economy*, edited at the University of Nottingham was once a quarterly. By now it has turned into a monthly, which is a clear sign of both academic and business success. But in the ranking of Wiley and Sons, the publisher, it is ranked for 2014 only as 42th in IR, 58th in Business and finance and at the bottom, 190th in Econ. Likewise the prestigious *IMF Economic Review* is ranked relatively well, as 19th in Business and finance, but only as low as 70th in Economics, despite its respectable 5 year IF of 2.8/the top being 9.4 for *Journal of Economic Literature*, but exceeding the old and prestigious *Economic Journal* of the Royal Economic Society being 2.33 only/.

the US obesity or misuses of credit cards figure among the preferred subjects of top ten journals.

Poorer countries have adopted two options. They either copy and emulate American practices, including the use of textbooks and evaluation standards, or simply outsource doctoral training to the market leader, the United States. As a result, teaching economics has undergone a standardization unknown across its 300 years of history, becoming comparable to the trade of dentists or plumbers for that matter.

This situation has been exacerbated by the *indiscriminate expansion of schools in economic and business higher education*, triggered by the quantitative expansion of enrollment numbers. The latter materialized irrespective of size and quality of faculty, weakening accreditation standards and diminishing public funding for higher education in the whole Europe.<sup>8</sup> The outcome is a paradoxical situation: the increase in the number of economics and business degrees did not translate into a higher general understanding of economic issues. *On the contrary: it has contributed to the decline in the quality of public understanding of complex macro-economic issues and the ensuing spread of populism in east and west alike.* In an extreme - but not imagined – case someone earning a degree with three classes in macro or one class in finance can qualify as a CEO, or even a member of the cabinet in charge of economic and finance issues, or overseeing the activities of the central bank. In a world of 40 second tv-clips such level of economic education allows for the spread of woodoo economics.

The above state of affairs leads to yet another contradiction. On the one hand, *over-simplified and abstract economics has lead to an alienation of decision-makers, of business and policy-making levels alike.* In an unpublished study we analyzed the source material used in MA theses at the leading Corvinus University of Budapest. We have found, that in the past 5-6 years only 9 per cent of those included at least one reference to *academic economics* sources, including textbooks and required readings, rather than internet, wikipedia, daily press and interviews/mostly conducted within the place of employment, and not caring much about the nuances of this sensitive research technique/. The latter means, that the function of Econ, as an academic discipline, nowadays is quite akin to what Political Economy of Capitalism and Socialism used to be under th *ancien régime*: teaching the proper creed, and forget it ASAP.

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<sup>8</sup> It is certainly legitimate to observe: even if real value of expenditures on higher education were kept/which has rarely been the case/, the splintering of institutions, with heavy overhead and over-employment, especially in the administration/non-teaching units would have brought about a bankrupt situation anyway. But allowing for the massive inflow of private money on the American muster could have alleviated the situation. The latter was, however, resisted by the regulators, out of fear from 'commercialization' and 'selling out of souls', much along the French, German and Italian lines.



The bad news is that bleeding out of higher education goes on in much of Europe/except the Scandinavian countries/. Furthermore employers - in Hungary definitely, but increasingly in much of the rest of Europe - no longer differentiate between BA and MA level degrees. Furthermore, economics, business, finance or tourism all qualify for them as 'a degree with economic competences'. In this process supply-side and demand-side conditions compress anything that deserves the name of quality education in economics.

The good news, by contrast, is that in academic economics a series of innovative approaches emerged, which aim and also deliver *a much deeper understanding, analysis and thus improvement in real world affairs*. These novelties – following the classics of Thomas Kuhn/1970/1996/ - tend to be born outside the established high-brow framework. Still, non-mainstream contributions of today may and often do become the mainstream of tomorrow – as had been the case with Keynesianism in the 60s and 70s.

#### *Innovations From Inside the Guild*

Let us start with re-iterating the works of persons who have provided deeper insights in real world issues, including the global financial crisis. The book of Nobel winner Robert Shiller/2005/ of Yale, *published prior to the outbreak of the panic*, may serve as a lasting example of how *behavioral finance* may, and indeed does, contribute to the better understanding of real processes observed on capital markets today. Perhaps the most important defining feature of this approach is that, unlike neoclassicals, it does not presume absolute rationality and perfect information understood and processed by market agents. Instead it follows the century-old tradition of postulating the rule of socio-psychological factors, as herd behavior, the rule of perceptions over fundamentals, informational uncertainties and the rule of fashions over rational calculations. *These all lead to regular and inevitable over-and undershootings against any – conceived or real – equilibrium point*. This approach is exactly the opposite to the currently ruling theory of efficient markets, elaborated by Eugene Fama/1991/ of Chicago, who was awarded with the Nobel Prize in the same year as Shiller, in 2013. *A holistic approach leaves little room for practical application of the more formalized models* which still rule in mainstream finance, as the Black-Scholes or the Miller-Modigliani models operating with extensive formal mathematical apparatus.

Shiller continued his line in explaining the financial crisis, in a real best-seller, jointly published with an other Nobel winner, George Akerlof/2009/. It is perhaps no coincidence that *academic orthodoxy tended to marginalize finance long before the crisis and it has not changed its mind ever since/if the top twenty journals indicate academic excellence/*.

It is telling, that works of these- and other- Nobel winners do not figure in the curricula of the global economics program. More surprisingly, also not in the top executive/business management/industrial organization/EMBA programs. Even in emerging countries of the East and the South one can observe the exclusive reliance on standardized American textbooks, sometimes even of teaching modules and forms of examination/both of students and of teachers/.

Much of the *truly path-breaking novelties in economics could be found in academic volumes, rather than in articles over-emphasized and over-appreciated during promotions*. This should not have come as a surprise for persons from humanities and the more traditional social sciences. However, economics tended to emulate the natural sciences in the past half a century or so. Not only in accepting formal mathematical presentation as the *sole or major criterion* of academic soundness. But also it contributes to over-estimating the role and impact of journal articles, and thus the ritual over-appreciation of journal rankings cited above.

Let us note: in some cases and countries, such as in the United Kingdom and the Netherlands, but increasingly also in Germany and 'naturally' in the USA, *it is only articles published in academic top journals which count for promotion and external funding*. Writing books, if appreciated at all, count as a sort of outdated hobby, which is though not positively prohibited, but does not really matter for academic appreciation. The role model was of course Paul Samuelson, but current formative personalities, from Robert Lucas to Eugene Fama tend to express their ideas *exclusively* in articles, or collections of those/which look like books, but lacking and over-arching structure unifying them are by no means monographs in a bibliographical sense/. As top journals and authors tend to form a closed shop<sup>9</sup>, they set standards for the current mainstream, thus it is legitimate for dissenters to show up elsewhere.

*Academic publishers are fortunately keeping a more open approach than journals do*. They continue to be oriented to a diverse public, a market which does look for other things than the high-brow top journals offer, something of immediate and lasting social value. In the following I shall cherry-pick just a few of the truly momentuous contributions from the output of non-mainstream, still highly appreciated and much cited authors, i.e works which offer *a fundamentally different interpretation of economic affairs from the textbook view*. One of the most appreciated<sup>10</sup> and controversial items of the recent years has been that of Acemoglu and Robinson/2012/. In their presentation

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<sup>9</sup> For a classical analysis cf/Hodgson-Rothman, 1999/, the trend has only strengthened since then.

<sup>10</sup> Their uncorrected google scholar citation is over 3 thousand, or the double of the most cited monograph of the 2012 Nobel winner Alvin Roth, as of 3 February, 2016.

we may observe *a return to classical economics, that includes the study of historical and institutional factors*, not only as minor items which may well modify the quantitative outcomes.

Allowing for the historic narrative to rule is a heresy. All the more so if one of the authors- Acemoglu – is also editor of the *Journal of Econometrics*, which is one of the holy places of the current mainstream. To make things worst, the high priest – editor in chief – joins forces with a political scientist rather than a mathematician or a physicist. The long view is also the opposite to the usual modelling approach, where the time dimension tends to be secondary, or discussed as 'starting point and end point', without much attention to how we get from 'here to there', especially in the real world, and especially at the macro level.

Acemoglu and Robinson revive a genre which seems to have gone under in economic analyses: *the grand narratives in history*. This is not the same as positivist history writing expects, that is the meticulous collection of facts and details without a normative or synthesizing theoretical frame. For economists it is a – long forgotten – important traditional analytical instrument *to conduct case studies*. In the latter – still vividly used in business studies – description has a value of its own right. However the real thing comes after, when cases are categorized, similarities and differences explained and generalizable conclusions are drawn.

This return of the tradition is in stark contrast to the self-interpretation of the guild, when „mathiness” is the sex-appeal/Romer, P., 2015/, when the quest is to find rules and laws that apply everywhere anytime, provided the axioms hold and the mathematics is fine. *Optimality* is not a concern neither in the historic nor in the more descriptive approaches. *Empirics and generally testing theories can not be confined to coherence check* of the proposed argument.

In established mainstream approaches the dominant analytical tool is *comparative statics*. This implies the narrowing down of complex issues, in order to be able to come up with quantifiable results. How big or how small is the impact of A on B? What number can we put on the influence C had over the outcome D? A large part of the empirical literature is devoted to such exercises, which is indeed legitimate. The first question any businessman, any policy-maker or any sensible person to decide over economic matters would be in establishing if we talk about a mammoth or a mosquito. And although Milton Friedman has famously coined the dictum: if the prediction is OK, do not ask about the premises or the axioms, *most economists of the past two decades have shyed away from making forecasts, especially for the long run*. A well known example for what a chaotic complex system takes is the atmosphere, especially its

upper levels. Meteorologists are thus subject of frequent teasing, despite the fact that they tend to come from among the best mathematicians.

One of the conventional objections against including the complexity approach, long established in the natural sciences, especially biology, chemistry and physics, has been the following. *Once we include this way of thinking, the elegance – meaning the mathematical formulation of unilateral causality – becomes impossible.* Furthermore, as the claim goes, complexity leads to blurred mindsets thus to misleading or inconclusive policy advice. This issue is being addressed by a recent important monograph in sufficient detail, allowing for the policy applications in the first place.

The principal author of the book is David Colander, a frequent contributor to top American academic journals and a fervent critique of the a-historical approaches dominating the mainstream. As Past President of the Eastern Economic Association and of the History of Economic Thought Society, he has long been a forefighter for a 'revolution from within', that is accepting the formalized framework as a precondition for exposing ideas. The co-author Roland Kupers. He is an independent consultant, associate fellow at Oxford's Smith School of Enterprise and Environment. He accumulated many years of working experience at Royal Dutch Shell. As one would expect, the Colander and Kupers/2014/ monograph continues the methodological revolution. Rather than adopting the usual top-down approach of macroeconomics *they offer a bottom-up approach.* Also formative for the academic enterprise has been business experience in coping with the challenge of environmental complexity while running an oil business corporation.

The skeleton of the argument goes as follows. In part one the authors develop an analytical frame based on integrating government and the market, rather than – as usually – juxtaposing them. Part two develops some of the pet topics for Colander, 'complexity economics', which is a steep turn away from the reductionist and oversimplifying approach of the neoclassical mainstream. In part three the authors expand on what they term 'laissez-faire activism', i.e a government that is involved in *framing the scope conditions of economic activity, rather than micro-managing and over-regulating these.* Finally, in a post-script the authors call for the thorough revamping of economics education, much along the lines we advocated in our previous writings. Namely the need to revive the historically and institutionally informed analyses, rehabilitate case studies and other close to business forms of inquiry, and retain the exactness inherent in the *proper use of mathematics as a tool, rather than an objective on its own.*

It remains to be seen how far these 'revisionist' initiatives will penetrate pure economics departments and especially MA and PhD programs in the USA and later globally. However the path goes clearly along the Kuhnian lines, and offers a different and potentially more productive mixture of innovation, academic broadness and methodological rigor than the current one-sided emphasis on „mathiness” would allow. Still, the book is an excellent case for showing how informed business leaders may and do contribute to turning economics more relevant for real world issues than it currently is.

Revolution from within implies that qualitative change may and does come from those, who have already accomplished a lot within the pre-existing standards of scientific assessments. *They are, in ideal case scenarios, both insiders and outsiders.* That is: individuals who rise to fame within the existing arrangements. However they also offer something which is at variance with the established creed. *Potentially and later actually their findings outgrow the rigid frames of mainstream and new insights get accepted,* not least because of the previous reputation earned by the stars. Let us note: while in theory priority should be decisive, in practice academic position, political standing and media influence often over-shadow the measurements of science. It remains for the sparse practitioners of history of thought to dig out, who was first, against who has become famous or influential in spreading the ideas.

*Inequality of income and wealth* has long been a fundamental concern for economic analyses. With the rule of neoclassicals this feature has been crowded out of research agendas of top journals. However, especially since the financial crisis of 2007-2009, but also not least to the experience of growing inequality and stagnant wages for the working poor in the United States, income distribution is back in vogue. Scandals about executive pay, or of bankers' drawing their exorbitant compensation from public money provided for the bailout of their institution, have created mass media interest.

Under this angle it has become perhaps inevitable that a thorough study of global inequalities has made headlines, and not only in the academe. The book by Thomas Piketty/2014/ is comparable both in size and ambition to the principal work of Karl Marx, *Das Kapital*, on which the volume is modelled.

The *oeuvre* of Piketty has a lot of common with that of Daron Acemoglu. None of them are born US citizens. They both earned their basic degrees outside the US. Still, both of them made career in the most competitive market in academic economics. Piketty developed in an accomplished researcher gradually in the MIT of Boston. While based at the Paris School of Economics, he also teaches part time at the London School of Economics. These commitments and positions are clearly indicative of *his deep and*

*organic integration in the UK/US academe*- a feature which unites him with the 2013 Economics Nobel winner, Jean Tirole of Toulouse. Thus, both authors are worth studying not only for the extraordinary impact of their academic output, but also as a prime case of what Thomas Kuhn/1970/ describes as „*changes from within*” the guild.

Before bringing out his *opus magnum* Piketty spent over two decades studying inequalities on the global scale. He started with re-calibrating the classical time series of Simon Kuznets/1955/, whereby the founder of these studies were indicating the tendency towards falling inequalities in the long run. Piketty extended these series both back in history and for the six decades that elapsed since.

In so doing the author introduced something revolutionary for the mainstream. Rather than speculating on the proper calibration and maths needed for a model, spelled out by Paul Romer/2015/ as decisive for academic soundness, he processes historical and current comparative statistics. Where those show lacunae, *he takes the pains of visiting archives to excavate missing source material and re-constructing time relevant statistics* from the fragmentary but available written sources for several countries. Thus he produces prime sources, as is usually done in history and statistics. Following this, he published a series of articles, and those in the top ten journals, including the *Journal of Economic Literature* as well as the *Quarterly Journal of Economics* of Harvard. But he did not stop at this point, as most of his peers would and actually had. He revives the conservative academic tradition of synthesizing his findings in a bulky monograph, thereby *lending an entirely new dimension for his findings*.

Conclusions and insights of Piketty’s work are unlikely to make into any curriculum, even at the PhD level, owing to the size and complexity of the argument/not because of the mathematics, which is kept to the minimum/. Among the many new insights we should underscore, that *the development of the United States is shown to be exceptional* rather than the standard, as US textbooks and many academics would have it. In case of the US the role of inherited wealth is much smaller, thus American capitalism is one of entrepreneurs rather than of rent-seekers. On the other hand Piketty shows, that in 1980-2013 the lot of the „bottom half” has not improved. All the increment accrued to those better off, especially to the top 1 per cent, way above the levels justified by relative productivity or other contribution to wealth creation. In yet another new insight *he revives interest of economics in distributional issues*, exorcised by the technocratic neoclassicals. He proves in meticulous detail: without state intervention inequalities inevitably grow, and have already reached the 1913 levels. Therefore one may indeed worry for the future of democratic capitalism based on middle classes and welfarism. Thereby – like Stiglitz – he transcends the purely methodological focus of the current

mainstream and brings us back the importance of values and choices for policy-formation – like Stiglitz does.

Furthermore, mention should be made of yet another- but very differently conceived and concluding - *opus magnum*, which *may lead to the rethinking of modern economics as we knew it*. This is the synthesis of the lifetime output of Deirdre McCloskey/2013-2016/ of Chicago. Among the great many innovations she stands out for *her detailed attention to providing those formal proofs which much of the profession takes for a precondition of economic soundness*. While the author winds up the sweeping criticism on the one-sidedness and ensuing misleading outcomes of the widely used formal analytical techniques, including the cult of statistical significance, she also takes the pains of presenting a formal explanation of the counter-propositions she makes. These are truly path-braking in their nature.

Her claim questions the entire logic behind neoclassicals. If the latter follow the Walrasian project of *mécanique sociale*, thus take factors and their combinations as independent variables, and socio-economic outcomes as dependent ones, *she reverses causation*. In her view it is basically ideas and values which explain why innovations translate into technological progress and trickle down of created wealth in some societies, but not in others under similar or comparable conditions. In her reading it is wrong to take factors and their quantities as given. *In reality it depends on values, perceptions and incentives if those actually do get combined in a fashion which leads to the explosion of wealth*. Therefore all major changes ever since the Industrial Revolution need to be interpreted as changes in values and the ensuing changes in the rules of the game, which in turn trigger efficient combination of factors than before. In her reading innovation is an outcome of societal change, not the triggerer of the latter a sin the neoclassicals.

Let us note: *being inside the mainstream, as Piketty seems to be, at least with one leg, implies inevitably his disregard for the incentive issue, so focal in Austrian and traditional institutionalist approaches*. At the end of the day this is also the crux of the criticism of McCloskey/2014/ of the Piketty view of contemporary capitalism. She is underscoring: if we disconnect - or simply by adhering to mainstream traditions abstract away from - *the formative components of change, which is innovation and wealth creation rather than distribution of rents*, we may and do end up even in errors of measurement, which should be the pride of the neoclassical economist. If we disregard the unprecedented expansion of wealth and overcoming much of the problems of rampant, absolute poverty, which used to rein until the post-WWII period, *we simply adopt a distorted angle, often missing the point*. It is simply wrong to abstract away from the fact that the

pie for every worker in a rich country has increased 30 times since 1800, mostly owing to human capital betterment, while return on physical capital was kept down to 5 to 10 per cent by competition and new entries/op.cit., p.85/.

*Return of real world considerations equals to return of the political, exorcised by Samuelson, Arrow and their disciples for the sake of technical elegance.* And this is where we have started the argument. Speakers of the initially cited IMF conference openly acknowledge: any collective choice, any major decision is by definition political. This has been the case when quantitative easing was used as a systematic, permanent measure, rather than a cyclical policy tool/Turner,A., 2015,pp31-32/. Empirical evidence has shown the impact of monetary shocks – as interest rate increases in the US – to be small, the interest parity manageable and spillovers from the globe de facto fully accommodated by monetary policy measures/Bayoumi et al., 2015, pp25-6/. This is *by and large the opposite to the textbook view of the international economy*. True, the latter was written at a different time for a different world, when 'unconventional', i.e non-standard measures counted automatically as 'unprofessional'. While the latter approach still rules in economics departments and doctoral school exams, it no longer appeals as policy-relevant, or simply as real-world relevant research.

To be fair: it must be admitted that the erosion – or flexibility – of the mainstream has allowed for the *rediscovery of some of the classical subjects for economic analysis*. The latter include the role of culture in shaping institutions/Alesina - Giuliano, 2015/, or the difficulty of turning natural resources into good use in poor countries/Venables, 2016/. In both cases old fashioned *political* economy, i.e incorporating *collective choices* - and on occasion also *value judgements* - in economics, while putting modelling/formal aspects back to their original roles as analytical *instruments* – rather than objectives - of analysis, is clearly observable. This is encouraging, as signals – even if rare signals – from the top twenty journals may be a prelude to broader changes within the profession and promotions alike.

### *Theory and Policy: A Happy Encounter?*

In this paper we attempted to present an overview – even if a fragmentary one – on what has become available on the aftermath of the Great Recession in terms of broad economic theories. We have noted the ever more closed shop of pure economics, applying the neoclassical axioms and methods with even more rigor than in the pre-crisis period. We contrasted this with unconventional solutions adopted by policy-makers and with innovative theories, developed by school molding personalities of the profession. Both seem to be opposing the practices of the top twenty journals and top US universities, regressing into sustaining self-referentiality in a fastly changing world.



Comparing what the 'global economics program' of standardized and Americanized economics PhDs offer we have demonstrated a contrarian trend. Even without addressing the traditional dissenters, whose work we appreciate but have not covered here, as the flourishing fields of multi-disciplinary approaches and business economics, developmental and political economics, we could show a fair degree of renewal in the professional output, if not yet in the curricula.

This state of affairs may continue for a long time, *exacerbating the drift between contemporary levels of academic knowledge and its imprint on minds of the young generations, let alone that of policy-makers and business leaders*. Those deciding over public – and private! – finances, public goods, regulation and the like, on welfare and competition, are institutionally constrained to build their insights on up-to-date insights from the academe. We can only hope for slow and incremental improvement under the pressure of crisis situations around the globe which emanate, at least in part, from inadequate stand of knowledge, and only in part from interest-based signalling and screening. *But academe and good universities must continue to remain open to diversity and innovation*. True, as an active university professor, I can hardly disagree with the pessimistic account of Colander/2015/, pointing to the fact that 'local incentives', such as standardization, convenience as well as institutional inertia are likely to lock in the existing curricula for a long time to come.

In the current paper we have intentionally *narrowed the focus of analysis* and deleted such important areas as economics of transformation, development economics, institutional economics or the new comparative economics, new political economy and European studies. While all those areas are rich in detail and carry important insights, they are yet to be espoused by the 'high brow' academic economics profession. Adopting their – unjustified – narrow approach in space and scope we still managed to *illustrate the ongoing erosion of the ruling paradigm*. Thus, as on previous occasions, change is more a matter of 'when', rather than 'if'.

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