

Innovations and Regress in Economic Theory

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Inspired by the 2015 IMF Annual Research Conference on “Unconventional Monetary and Exchange Rate Policies”¹ we ask how far the 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.²

Angeregt durch die IMF-Tagung ”Unconventional Monetary and Exchange Rate Policies“ im Jahr 2015 untersucht der Beitrag, inwieweit die Interpretation und Generalisierung von policy-Innovationen nach der Großen Rezession 2007-2009 im Rahmen der traditionellen Ökonomie aufgenommen wurden und sich in deren Ausbildungs- und Doktorandenprogrammen niedergeschlagen haben. Am Beispiel einer Reihe theoretischer und methodischer Innovationen wird gefragt, ob diese als Bausteine eines neuen und stärker politik- und damit realitätsbezogenen Denkens gelten können.

I. Introduction

This article is a sequel to our earlier reflections in this journal³ on the limitations of the current mainstream approach to economics. As we documented in the previous paper, an unprecedented concentration of Nobel Prizes and of appreciations – from Top Twenty journal rankings to textbooks and doctoral programs – has emerged, with a heavy dose of Americanization and standardization. If we examine only the past four decades, it is less than 10% of those who received awards 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 Prize winners’ on www.nobelprize.org, or via a glimpse at the titles of any of the top twenty journals.⁴ In the middle of the

1 Sixteenth Jacques Polak Annual Research Conference: Unconventional Monetary and Exchange Rate Policies, November 5-6, 2015, Washington, D.C. Papers available at www.imf.org.

2 Useful comments by György Szakolczi, the Editor, and two anonymous referees on the previous version are appreciated, with the usual caveats.

3 Csaba, L.: Orthodoxy, renewal and complexity in contemporary economics, in: Zeitschrift für Staats- und Europawissenschaften, 7/1 (2009), 51–82.

4 Less the *Journal of Economic Literature*, which is all-encompassing by its nature.

global financial crisis we asked whether new, more productive and policy-oriented approaches will emerge, as policy-makers have increasingly turned 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*⁵ to more recently *Paul Romer*⁶, a star professor at NYU. The first – in his Nobel lecture – explained the difference between *Adam 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 with a mathematical 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 to each component. Some, most notably *Colander et al.*⁷, tend to depict the mainstream as a changing frontier, allowing for the incorporation of new insights. In reality, the type of mathematics it relies on severely limits the type of questions that may be sensibly posited, and thus pre-judges the outcomes to a large degree. This is what the long exorcised *Ludwig von Mises*⁸ objected to as the formalization of economics: if axioms already contain 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 at the Great Recession with hindsight we may make an attempt to take stock and look 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. It is true that this holds more for the policy arena and less for the academe. In the latter innovations do occur, though, these tend to

5 *Lucas, R.E.*: Nobel Lecture: monetary neutrality, in: *Journal of Political Economy*, 104/4 (1996), 661–682, 664.

6 *Romer, P.*: Mathiness in the theory of economic growth, in: *American Economic Review*, 105/5 (2015), 89–93.

7 *Colander, D./Holt, R./Barkley-Rosser, J.*: The changing face of mainstream economics, in: *Review of Political Economy*, 16/4 (2004), 485–499.

8 *Van Mises, L.*: *Epistemological Problems of Economics*, ed. by Mises Institute, Auburn, 2003, 28–37. The German original was published in 1933.

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 disregarding reality still prevails, as we shall document below.

The purpose of the current exercise is not to offer yet another litany of complaints. Rather we try to cover innovations which abound in academe, if not in the curricula. The 2000s have 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 Zeitung*⁹, testifying the unbridgeable gap between standards of the pure academe and policy advice, let alone impacts on the media and public discourse. While some of the divergence is trivial, it is hard to deny that many Nobel Prize winners, or those close to winning, are 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. 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.¹⁰ Perhaps the most interesting novelty was condoning, rather than merely tolerating fiscal and monetary laxity as simultaneous and sustaining policies.¹¹ It is however remarkable, that those policy innovations – such as the changing role of the 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 deposits¹² – are being analyzed by research departments and conferences of banks and fiscal authorities, rather than in the academe

9 FAZ-Ökonomenranking: Deutschlands einflussreichste Ökonomen, Frankfurter Allgemeine Zeitung v. 04.09.2015.

10 For one of the rare exceptions cf. *van der End, J.W./de Haan, J./Kearney, I.*: Quantitative easing in the euro area: an exposition, in: Zeitschrift für Staats- und Europawissenschaften, 13/1 (2015), 87–99.

11 *Turner, A.*: The case for monetary finance – an essentially political issue, paper presented at the 16th Jacques Polak research conference of the IMF 2015, op. cit.

12 The ECB nominal lead deposit rate of *minus* 0.4 pp raised the eye-brows of even the customary supporters

proper. Likewise, contributions in the Top Ten journals do not revolve around such issues. It goes without saying that university curricula are silent on them.

What is truly new in the post-2008 period is 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, the US and Japan alike for longish periods. One may ask: did we arrive at 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*¹³, generalizing the lessons from fighting the Great Depression, was published more than three years after the crisis 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.

Therefore we aim to document the emergence of a new, *policy-relevant economics* in this paper. 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 those in the pre-crisis period.¹⁴ Similarly, the study of deeper roots of the crisis warns 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.¹⁵ Last but not 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.¹⁶

For these and other reasons there is a need to adopt an approach to *re-integrate theory and policy*. This is distinct from previous approaches in a number of planes.

of the radical QE line of Mario Draghi, especially in the banking community. Cf. *Financial Times*: Senior European bankers voice concerns over ECB cut, 09.03.2016, <http://www.ft.com/intl/cms/s/0/0f15cc6c-e5cb-11e5-a09b-1f8b0d268c39.html>. They note: such a move, by enhancing bank losses, translates in reality to lending rate increases, thus less, rather than more lending activity.

13 *Keynes, J.M.*: The General Theory of Employment, Interest and Money (1936). With an introduction to the new edition by P. Krugman, London, 2007.

14 *Ihrig, J.E./Meade, E.E./Weinbach, G.C.*: Rewriting monetary policy 101: what's the FED's preferred post-crisis approach to rising interest rates?, in: Journal of Economic Perspectives, 29/4 (2015), 177–198.

15 *Snowden, N.*: What really caused the Great recession? Rhyme and repetition in a theme from the 1930s, in: Cambridge Journal of Economics, 39/5 (2015), 1245–1262.

16 *Tanzi, V.*: Fiscal and monetary policies during the Great Recession: a critical evaluation, in: Comparative Economic Studies, 57/2 (2015), 243–275.

First, it avoids the lack of theoretical anchoring. Second, it tries to overcome 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 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.¹⁷ 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?*

II. 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 to the 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 % 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 end of the recession all major economies would 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 voodoo economics. The output of leading journals – the top twenty of IDEAS/REPEC – continue to be filled with speculative modelling, following much of the same lines and standards as in the pre-crisis period. Very few, if any, policy-oriented or policy-inspired papers were published 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*

¹⁷ Kolodko, G.W.: The new pragmatism, or economics and policy for the future, in: *Acta Oeconomica*, 64/2 (2014), 139–160.

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.¹⁸ After all, crisis management in Europe has often led to – temporary or even permanent – nationalizations. Especially bank bail-outs, but also 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 assessments¹⁹ 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 to 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 Prize winner *Paul Krugman*²⁰ rejects one of the fundamental features of Hungarian policies, i. e. the focus on stabilizing and even diminishing the public debt/GDP ratio²¹ as a focus of macroeconomic considerations, as well as the supply side approach characterized by the priority of flat taxes – a major ideological component of the second and third *Orbán* governments. For *Krugman* the key is exactly the opposite: sustaining fiscal and 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 forms the position of another Nobel Prize winner, *Joseph E. Stiglitz*.²² He takes a politically less and professionally more radical position

18 For Star Wars and others.

19 *Voszka, É.: Államossítások: Magyar fordulat vagy európai irány? (Nationalizations: a Hungarian U-turn or a new trend in Europe?)*, in: *Külgazdaság*, 59/11-12 (2015), 3-22.

20 *Krugman, P.: End This Depression Now!*, with a new preface, New York, 2012.

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

22 *Stiglitz, J.E.: Reconstructing macroeconomic theory to manage economic policy*, in: Laurent, E./Lecacheux, A. (eds.): *Fruitful Economics – Essays in Honor of Jean-Paul Fitoussi*, Hounds mills/New York, 2015, 20-49.

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 only, of course, a cyclical prescription. 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, 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 reflating the economy. But in the opposite case austerity may actually worsen the retrenchment, triggering disproportionate falls in output and employment.

As *Stiglitz* elaborates, 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 higher performance. Under real world conditions impoverishment breeds exclusion, illnesses and alienation, all features known from the socio-logical surveys on pockets of deep poverty. The latter tends 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, as in other disciplines, crises also present opportunities for testing old, established theories whilst developing new ones. It is certainly legitimate to observe at this point that no theory may or does 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. Participating 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 to interactions that hardly follow the logic of rational expectations, even if it was the priority.

On the contrary, as the late *Rüdiger Dornbusch*²³ explained in his last book a quarter of a century ago, communication can be, and often indeed is, as important as action. If for no other reasons but to shape the expectations of millions of agents, which react – or do not react – to actions and numbers, depending on their perception of reality.

III. The Empire Strikes Back

In economics – just like in the arts – each era is described by its mainstream. What is mainstream and what is heterodox is often a matter of retrospective judgement, 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, media techniques. These follow *balance of power changes*, both in the arts, politics and academia. In our case, the 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. Out of the journals which make it into the sanctuary of the *economics* entry, i. e. those who qualify at all to be considered within the limes²⁴, 19 out of 20 of the top rankings of IDEAS/REPEC are edited and published mostly in the United States of America. No similar concentration is to be observed in any other discipline, be that nuclear physics, medicine or chemistry, where US schools evidently take a leading position, but not such a monopoly.

This is an anomaly in itself, given the lack of replica in other disciplines. The latter means that not only BA and MA, but PhD level training follows American stand-

23 *Dornbusch, R.*: Stabilization, Debt and Reform: Policy Analysis for Developing Countries, Hertfordshire, 1993.

24 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). Often they publish different rankings for the same journal, even on the front page. For instance the highly stimulating *World Economy*, edited at the University of Nottingham, was once a quarterly journal. 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 190th in Economics. Likewise the prestigious *IMF Economic Review* is ranked relatively well, 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 at 2.33).

ards, structures and evaluation patterns, irrespective of the problems of local societies. They may well range from a reform of the Common Agricultural Policy to fight hunger, while in the US obesity or misuses of credit cards are among the preferred subjects of the top ten journals.

Poorer countries have adopted two alternatives. 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 of Europe.²⁵ The outcome is paradoxical: the increase in the number of economics and business degrees did not translate to a better 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-economics 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 be responsible for 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 voodoo economics.

The above state of affairs leads to yet another contradiction. On the one hand, over-simplified and abstract economics has led to an alienation of decision-makers from business and policy-making levels. In an unpublished study we analyzed the source material used in MA theses at the leading Corvinus University of Budapest. We found that in the past 5-6 years only 9% of those included at least one reference to academic economics sources, such as textbooks and required readings, rather than internet, wikipedia, daily press and interviews (mostly conducted within

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

the place of employment, and not caring much about the nuances of this sensitive research technique). The latter means that the function of Economics, as an academic discipline, is nowadays quite akin to what Political Economy of Capitalism and Socialism used to be under the *ancien régime*: teaching the proper creed, and forget it ASAP.

The bad news is that the draining of higher education goes on in much of Europe (except for the Scandinavian countries). 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 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, aiming at and delivering a much deeper understanding, analysis and thus improvement in/of real world affairs. These novelties – following the classics of *Thomas Kuhn*²⁶ – tend to be developed 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 1960s and 1970s.

IV. Innovations From Inside the Guild

Let us start with re-iterating the works of persons who have provided deeper insight into real world issues, including the global financial crisis. The book of Nobel Prize winner *Robert Shiller*²⁷ 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 a 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. All these lead to regular and inevitable over- and undershootings against any – conceived or real – equilibrium point. This approach is the exact opposite to the current ruling theory of efficient

26 *Kuhn, T.S.*: The Structure of Scientific Revolutions (1970), Chicago, 1996.

27 *Shiller, R.*: Irrational Exuberance, Princeton, 2005.

markets, elaborated by *Eugene Fama*²⁸ of Chicago, who was awarded the Nobel Prize in the same year as *Shiller* (2013). A holistic approach leaves little room for practical application of the more formalized models which still rule mainstream finance, like the *Black-Scholes* or *Miller-Modigliani* models operating with extensive formal mathematical apparatus.

Shiller continued explaining the financial crisis in a real best-seller, jointly published with another Nobel Prize winner, *George Akerlof*²⁹. 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 (in case the top twenty journals indicate academic excellence).

It is telling that works of these – and other – Nobel Prize winners do not figure in the curricula of the global economics programs, but more surprisingly also not in the top executive, business management, industrial organization or 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 teachers).

Much of the truly path-breaking innovations in economics could be found in academic volumes, rather than in articles, over-emphasized and over-appreciated in promotions. This should not come as a surprise for persons from the humanities and the more traditional social sciences. However, economics tended to emulate the natural sciences in the past half century or so by not only accepting formal mathematical presentation as the *sole or major criterion* of academic soundness, but also contributing 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 in the USA, it is only those articles published in top academic journals which count for promotion and external funding. Writing books, if appreciated at all, could be considered as a sort of outdated hobby, which is not positively prohibited, but does not really matter for academic appreciation. The role model was of course *Paul Samuelson*, but current formative personalities, such as *Robert Lucas* and *Eugene Fama*, tend to express their ideas exclusively in articles or collections of those (which look like books, but lacking an overarching structure, such as in monographs in a bibliographical

28 *Fama, E.F.*: Efficient capital markets-II, in: *Journal of Finance*, 46/5 (1991), 1575–1617.

29 *Akerlof, G.A./Shiller, R.J.*: *Animal Spirits: How Human Psychology Drives the Economy and Why It Matters for Global Capitalism*, Princeton, 2009.

sense). As top journals and authors tend to form a closed shop³⁰, they set standards for the current mainstream, thus it is legitimate for dissenters to show up elsewhere.

Fortunately academic publishers are keeping a more open approach than journals do. They continue to be oriented towards a diverse public, a market which looks 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 momentous contributions from the output of non-mainstream, still highly appreciated and much cited authors, i. e. works that offer a fundamentally different interpretation of economic affairs from the textbook view. One of the most appreciated³¹ and controversial book contribution of the recent years has been that of *Acemoglu* and *Robinson*³². In their presentation 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 worse, the high priest – editor in chief – joins forces with a political scientist rather than a mathematician or a physicist. This view is also the opposite to the usual modelling approach, where the time dimension tends to be secondary, or discussed as starting 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* revived a genre which seems to have gone under in economic analyses: the grand narratives in history. This is not the same as what is expected from positivist history writing, that is the meticulous collection of facts and details without a normative or synthetizing theoretical frame. For economists it is a – long forgotten – analytical instrument. Furthermore, it can and often should be complemented with the *conduct of 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.

30 For a classical analysis cf. *Hodgson, G./Rothman, M.*: The editors and authors of economics journals: a case of institutional oligopoly?, in: *Economic Journal*, 109/February (1999), 165–186. The trend has only strengthened since then.

31 Their uncorrected google scholar citation is over three thousand, or double the most cited monograph of the 2012 Nobel Prize winner Alvin Roth, as of 3 February, 2016.

32 *Acemoglu, D./Robinson, A.*: *Why Nations Fail: the Origins of Power, Prosperity and Poverty*, New York, 2012.

This return of the tradition stands in stark contrast to the self-interpretation of the guild, where *mathiness* forms the sex-appeal³³, where the quest is to find rules and laws that apply everywhere anytime, provided the axioms hold and the mathematics are fine. But optimality is not a concern neither in the historic nor the more descriptive approaches. Empirics and the general testing of theories can not be limited to coherence checks 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 asking whether 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 shied away from making forecasts, especially for the long run. A well-known example for what comprises a chaotic complex system 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 and thus to misleading or providing inconclusive policy advice. This issue was addressed by a recent important monograph in sufficient detail, allowing for 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 critic 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 proponent for a *revolution from within* that is accepting the formalized framework as a precondition for exposing ideas. The co-author *Roland Kupers* is an independent consultant and an associate fellow at Oxford's Smith School of Enterprise and

33 Romer, P., op. cit.

Environment. He accumulated many years of work experience at Royal Dutch Shell. As one would expect, the *Colander and Kupers*³⁴ monograph continued the methodological revolution. Rather than adopting the usual top-down approach of macroeconomics they offer a bottom-up approach. Also formative for this academic enterprise has been the 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 framework based on integrating government and the market, rather than – as usually – juxtaposing them. Part two develops one of the pet topics for *Colander, complexity economics*, which is a steep turn away from the reductionist and over-simplifying 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 a thorough revamping of economic education, much along the lines we advocated in our previous writings: namely the need to revive historically and institutionally informed analyses, rehabilitate case studies and other forms of inquiry close to business, and retain the exactness inherent in the proper use of mathematics as a tool, rather than its own objective.

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 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 making 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 different to 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

³⁴ *Colander, D./Kupers, R.: Complexity and the Art of Public Policy. Solving Society's problems from the Bottom-Up*, Princeton/Oxford, 2014.

the stars. 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 whom he or she 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 due to the experience of growing inequality and stagnant wages for the working poor in the United States, income distribution is back and en 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 academia. The book by *Thomas Piketty*³⁵ is comparable both in size and ambition to the principal work of *Marx, Das Kapital*, on which the volume is modeled. The oeuvre of *Piketty* has a lot in common with that of *Daron Acemoglu*. None of them were 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* gradually developed into an accomplished researcher at the MIT in Boston. While based at the Paris School of Economics, he also taught part-time at the LSE. 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 2014 Economics Nobel Prize winner, *Jean Tirole* of Toulouse. But both authors are worth studying not only for the extraordinary impact of their academic output, but also as prime examples of what *Thomas Kuhn*³⁶ describes as „*changes from within the guild*“.

Before publishing his *opus magnum*, *Piketty* spent over two decades studying inequality on a global scale. He started with re-calibrating the classical time series of *Simon Kuznets*³⁷, whereby the founder of these studies indicated 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.

35 *Piketty, T.:* Capital in the 21st Century, Cambridge/Mass., 2014.

36 *Kuhn, T.S.,* op. cit.

37 *Kuznets, S.:* Economic growth and income inequality, in: American Economic Review, 45/1 (1955), 1–28.

In doing so, the author introduced something revolutionary to the mainstream. Rather than speculating on the proper calibration and maths needed for a model, spelled out by *Paul Romer*³⁸ as decisive for academic soundness, he processes historical and current comparative statistics. Where others 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 usual 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* at Harvard. But he did not stop at this point, as most of his peers would and actually had. He revived the conservative academic tradition of synthesizing his findings in a bulky monograph, thereby lending an entirely new dimension to his results.

Conclusions and insights of *Piketty*'s work are unlikely to make it 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 a minimum). Among the many new insights we should underscore that the development of the United States is shown to be the exception 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 one of the rent-seekers. On the other hand, *Piketty* indicates that between 1980 and 2013 the situation of those from the bottom half of the population has not improved. All the increment accrued to those better off, especially the top 1%, was high above the levels justified by relative productivity or other contributions to wealth creation. In yet another new insight he revives the interest of economics in distributional issues, exorcised by the technocratic neoclassicals. He proves in meticulous detail that without state intervention inequalities inevitably grow, and have already reached 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 to the importance of values and choices for policy-formation.

Furthermore, another – but very differently conceived and concluding – *opus magnum* should be mentioned, that may lead to the rethinking of modern economics as we knew it. This is the synthesis of the lifetime output of *Deirdre*

38 Romer, P., op. cit.

McCloskey³⁹ of Chicago. Among the many great innovators she stands out for her detailed attention to providing those formal proofs which much of the profession takes for a pre-condition 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 the neoclassical thinking. If the latter follow the *Walrasian* project of *mécanique sociale*, thus taking 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 get combined in a fashion which leads to the explosion of wealth. Therefore all major changes since the Industrial Revolution need to be interpreted as changes in values and ensuing changes in the rules of the game, which in turn trigger an efficient combination of factors. In her reading innovation is an outcome of societal change, not the trigger of the latter – a sin in neoclassical thinking.

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 traditional institutionalist approaches. At the end of the day this also forms the main criticism of McCloskey⁴⁰ 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

39 McCloskey, D.N.: *The Burgois Era*, vols. I-III, Chicago, 2006-2016.

40 *Ibid.*: Measured, unmeasured, mismeasured, and unjustified pessimism: a review essay of Thomas Piketty's 'Capital in the 21st century', in: *Erasmus Journal for Philosophy and Economics*, 7/2 (2014), 73-115.

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% by competition and new entries.⁴¹

The return of real world considerations equals the 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 acknowledged: any collective choice, any major decision is by definition political. This has been the case when quantitative easing was used as a systematic, permanent instrument, rather than a cyclical policy tool.⁴²

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.⁴³ This is by and large the opposite to the textbook view of the international economy. True, it 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. This includes the role of culture in shaping institutions⁴⁴ or the difficulty of turning natural resources into good use in poor countries⁴⁵. In both cases old fashioned *political economy*, i. e. incorporating *collective choices* (and on occasion also *value judgements*) in economics while putting modelling and 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.

41 *Ibid.*, 85.

42 *Turner*, A., op. cit., 31–32.

43 *Bayoumi*, T. et al.: Direct spillover effects of unconventional monetary and exchange rate policies, paper presented at the 16th Jacques Polak research conference of the IMF 2015, op. cit., 25–26.

44 *Alesina*, A./*Giuliano*, P.: Culture and institutions, in: *Journal of Economic Literature*, 53/4 (2015), 51–82.

45 *Venables*, A.: Using natural resources for development: why has it proven so difficult?, in: *Journal of Economic Perspectives*, 30/1 (2016), 161–184.

V. 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 in 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 self-referentiality in a fast changing world.

Comparing what the global economics programs of standardized and americanized 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 detected 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 the minds of the younger generations, let alone that of policy-makers and business leaders. Those deciding over public – and private! – resources, public goods, regulation and the like, on welfare and competition, are institutionally constrained to build their policies on up-to-date insights from 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 an inadequate stand of knowledge, and only in part from interest-based signaling and screening. But academe and good universities must continue to remain open to diversity and innovation. However, as an active university professor, I can hardly disagree with the pessimistic account of *Colander*⁴⁶, 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 our analysis and bypassed such important areas as economics of transformation, development economics, institutional economics or new political economy and European studies. While all those areas are rich in detail and carry important insight, they are yet to

⁴⁶ *Colander, D.*: Why economics textbooks should, but don't, and won't change, in: European Journal of Economics and Economic Policies, 12/2 (2015), 229–235.

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*.