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Complexity, the evolution of macroeconomic thought, and micro foundations David Colander	2
Models and reality: How did models divorced from reality become epistemologically acceptable? Asad Zaman	20
<i>ECONOMICS 101 (editors invite more papers on Economics 101)</i>	
The value of “thinking like an economist” Bernard C. Beaudreau	45
An essay on the putative knowledge of textbook economics Lukas Bäuerle	53
World population: the elephant in the living room Theodore P. Lianos	70
The carbon economy – rebuilding the building blocks of economics and science John E. Coulter	84
Breaking the golden handcuffs: recreating markets for tenured faculty M. Shahid Alam	91
Reinforcing the Euro with national units of account Gerald Holtham	102
Neoliberalism vs. China as model for the developing world Ali Kadri	108
Classifying “globally integrated” production firms from a worker/citizen perspective John B. Benedetto	128
<i>REVIEW ESSAY</i>	
Tony Lawson, economics and the theory of social positioning Jamie Morgan	132
<i>INTERVIEW</i>	
Ecological and feminist economics: an interview with Julie A. Nelson Julie A. Nelson and Jamie Morgan	146
Board of Editors, past contributors, submissions, etc.	154



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Complexity, the evolution of macroeconomic thought, and micro foundations¹

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Economics has always had an underlying tension between two visions of economics. One is an equilibrium vision that conceptualizes the economy as relatively stable and focuses on the forces that push the economy toward a long-run equilibrium. The other is a complexity vision that conceptualizes the economy as in constant flux, evolving in ways that we cannot predict. Both visions focus on competition, but the equilibrium vision focuses on competition as a state or market structure, while the complexity vision focuses on competition as an unending process. The two visions are not mutually exclusive, and an economist can see both as useful reference points when trying to understand the economy. Which is more useful depends on the question being asked.

While the two visions can be simultaneously held, generally, in setting a research agenda, one or the other dominates, and in recent years the equilibrium vision has dominated. This domination has influenced economic methodology and the way economists approach policy questions. Nonetheless, the complexity vision is still held and respected within the mainstream profession as demonstrated in the Nobel Prizes given to economists whose work reflects a complexity vision, such as Herbert Simon, Frederick Hayek, Douglas North, Eleanor Ostrom, and Ronald Coase. Their work is considered mainstream, but is seen as part of a separate tradition in economics that is not so much an alternative to standard mainstream economics, but rather another, less explored, parallel track. One of the goals of this paper is to encourage exploration of this alternative track.

Differences in theoretical methodology: equilibrium vs. complexity vision

The two visions draw lessons from theory differently, and are associated with quite different research programs, especially as they relate to policy. The equilibrium vision sees formal theory as providing a necessary blueprint for policy. Franklin Fisher (2011) nicely captures this view. He writes,

“It is not an overstatement to say that they (the general equilibrium welfare theorems) are the underpinnings of Western capitalism... So elegant and powerful are these results (G.E.’s exploration and proofs of existence, uniqueness, and optimality) that most economists base their conclusions upon them and work in an equilibrium framework.”

In the equilibrium vision, without formal theory, policy has no scientific foundation. It takes the position: Better to have an inadequate formal theory than no formal theory at all.

¹ This paper was the keynote address at the “Microfoundations for Macroeconomics: retrospect and prospect” workshop at the the University Nice Cote d’azur, GREDEG CNRS.

The complexity vision sees developing a useful tractable formal general theory as currently far beyond our capabilities and instead focuses on gaining partial insights into the complex dynamics of the economy in whatever way it can – agent based models, simulations and general exploration of non-linear dynamic models. Since formal dynamics is analytically so difficult, the complexity vision is content with informal theory especially when talking about the aggregate economy. It takes the position: When guiding policy it is better to recognize that we have no directly useful formal theory than to confine policy analysis to an inadequate formal theory. Within the complexity vision ultimately, because the formal specification of the economy is so beyond our current analytic capabilities, even the best economic policy is based on heuristics, not scientific theory, and thus, in a formal scientific sense, is ungrounded. Policy advice should not be presented to policy makers as otherwise.

The complexity approach to policy holds that, because of the complexity of economic theory's relationship to the real world, policy discussions are best separated from scientific discussions. Policy discussions should be based not directly on formal scientific theory, but, instead, on educated common sense – a wide ranging knowledge of economic scholarship that includes a good understanding of where researchers are in advancing formal theory, a good understanding of the history and institutions of the economy, a detailed familiarity with empirical data about the economy, and a philosophical understanding of the role that ethical views play in arriving at policy advice. Good policy is based on far more than just economic science.

The complexity approach divides economic analysis into two separable fields: science, whose goal is to discover the truth, and applied policy, whose goal is to solve real-world problems. The two fields are separated by a firewall to reduce the possibility of policy views influencing scientific judgments.² The goal is to allow specialization and gains from trade. The same economist could do both science and policy, but the two activities would use different methodologies, and would require different skill sets.

While the complexity methodology downplays the importance of formal theory in directly guiding policy, it is not against formal deductive theory, abstract mathematics, or sophisticated empirical research. But the goal of that theoretical research is a scientific goal – to better understand the economy; the goal is not to guide policy (although some policy guidance might follow as an unintended consequence). Thus, the complexity vision's scientific research agenda is consistent with a vigorous and highly abstract theoretical and empirical research agenda that, if anything, because its focus on complex dynamics, is even more mathematically and statistically complex than the current research agenda associated with the equilibrium vision. In that sense the complexity methodology is quite different from the critical realist methodology espoused by heterodox critics of economics such as Tony Lawson.

Critical realists criticize equilibrium methodology for its emphasis on abstract mathematics; complexity theory embraces mathematics. Complexity economists criticize the equilibrium methodology for the way it uses theory in thinking about policy, not for its use of mathematics. Whereas the equilibrium methodology treats formal theoretical results as central to its applied policy research, the complexity methodology uses formal theory more as a fable or heuristic, which may or may not be relevant for policy. Within the complexity vision, formal theory is best thought of as a thought experiment that can be useful both for thinking creatively about

² I expand on this distinction in Colander and Freedman (2019).

policy problems and for preventing logical mistakes in reasoning. But, because the theory is only tenuously related to the real world economy the theory is meant to capture, the results of formal theory are not to be thought of as a blueprint for policy.

The distinction among the equilibrium, complexity and critical realist/heterodox views of the equilibrium methodology can be seen in reference to the well-known “searching for the keys under the lamppost joke.” The standard interpretation of the joke embodies the critical realist view. It is that economic theorists are out there in La-la-land, doing highly abstract economic research unrelated to the real world.

“Isn’t it stupid – searching where you haven’t lost the keys just because that’s where the light is?”

“Isn’t it stupid – working on models that you know are so far from reality that they can’t possibly describe reality: representative agent super rational choice models, when it’s obvious that the action is in interactive effects; Isn’t it stupid to work with strict rationality models, when it’s obvious that people are at best boundedly rational?”

From a complexity standpoint, a research strategy of “searching where the light is” is far from stupid. Where else but where the light is can one do formal theory? Where the complexity vision has a problem with the current equilibrium methodology is with its attempt to apply the abstract theory, developed where the light is, directly to policy. That’s the equivalent to searching for the keys where you did not lose them, and deserves the critical realists’ scorn. The complexity vision sees the goal of theorists searching in the light to be discovering potential patterns that help them understand the economy. While the goal is not to guide policy the discovered patterns might be helpful to applied policy researchers exploring in the dark. Theorists are developing an abstract knowledge of economic topographies, exploring abstract topographies where there are the equivalent of rocky cliffs, where there are smooth deltas, rolling hills, and where sudden storms changed the topography quickly, as a small creek becomes a raging river. This leads to a second role for theorists—to develop creative abstract policy solutions to deal with different topographies. These abstract solutions may or may not be transferrable to the real world. But the exploration can suggest other solutions that might work. The goal of this part of policy theorizing is creative design of policy.

An engineering methodology

The exploration of creative policy solutions uses what I call creative design engineering methodology. Engineering methodology differs from scientific methodology; it is a heuristic methodology used by a craftsperson. Billy Vaughn Koen (2003) defines it as “the strategy for causing the best change in a poorly understood or uncertain situation within the available resources” (p. 7). Koen argues that this definition is operationally equivalent to a second definition – “use the best available engineering heuristics to solve problems”.

Because complexity engineering is designed to deal with policy, it does not attempt to be value free; instead it attempts to be value transparent. Whereas scientific methodology eschews philosophical methodology, engineering methodology incorporates it as the best way to integrate values into the analysis. Compared with scientific methodology, engineering methodology is much less constrained, and loose. It is an educated common sense

methodology in which “anything goes” as long as that “anything” is useful in arriving at a possible solution to a problem. It is a creative methodology that is not afraid to deviate from current scientific conventions. Context, not fixed methodological rules, determines method, and the guiding heuristics are determined not by specialized philosopher of science methodologists, but rather by the researchers themselves. Here is what has seemed to work in a similar case; maybe it will work in this case.

In place of a formal theory to guide policy, the complexity policy methodology uses an informal general theory that focuses on change and process, not on equilibrium, as its general framework for thinking about applied policy. For complexity economics that informal general theory is best described as a multi-level evolutionary theory that has much in common with the multi-level evolutionary theories used in evolutionary biology. One may talk about institutionally constrained equilibria, but such equilibria will be seen as part of an evolutionary system and not as a final resting place of the economy. By equilibrium theory standards, the complexity vision theory is more a conceptual theory than a formal theory.

The theoretical debate within complexity economics is not about whether the evolutionary theory is correct; the debate is about the nature of that evolution. Most complexity economists assume that the economy’s evolution is multi-level, which means that, while the economy is assumed to have developed from the decisions of rational agents, the nature of rationality has evolved to fit the institutions that coordinate agent’s actions to promote the group’s interest as well as its own. Where rational agents have found it useful, they have cooperated and developed behavioral norms, and have built institutions based on those behavioral norms. These institutions and norms have solved coordination problems; it is their existence that prevents chaos so, to reasonably discuss policy, one must have a model that includes them. Developing a precise model of this evolutionary system is impossible since these norms and institutions have become nested in other norms and institutions in complex ways. Over time, the nature of the bounded rationality changes as institutions and norms evolve. Appropriate policy evolves as the economy evolves.

The behavioral constraints nested in these institutions significantly complicate what is meant by agent rationality; within some specification of evolutionary theory, just about any agent action can be considered rational. This possibility undermines the usefulness of any simple individual rational choice model that doesn’t incorporate real world institutions and norms. The current real-world rationality must be discovered empirically. Thus, the complexity vision is consistent with behavioral economics, whereas the equilibrium vision is tied to traditional individual rationality approach to behavior.

Rational individuals solve problems by coordinating their actions, creating institutions that solve some problems but add others. These institutions compete and collaborate, creating an ever increasing array of new coordinating institutions as technology changes and as new discoveries are made. Thus, the complexity vision sees the economy as an evolving complex system that exhibits all the characteristics that evolving complex systems exhibit – multiple basins of attraction rather than a unique long run equilibrium, natural selection, mutations, adaptations, sensitive dependence on initial conditions, path dependence, and potential phase transitions that cannot be deduced from the study of individual agents separate from their interaction. It thinks about policy informally within an evolutionary framework. The complexity scientific research program is designed to abstractly explore that multi-level evolution.

Macroeconomics and complexity

So far, I've talked about general methodological differences between the complexity vision and the equilibrium vision. Let me now turn specifically to macro theory and consider how the complexity approach to macro and the equilibrium approach to macro differ. Probably, the biggest way in which they differ is in what they see as the central question that macro theorists are trying to answer.

Since the equilibrium vision starts with the assumption that, in the absence of constraints, the economy will gravitate toward a predetermined desirable Pareto-optimal equilibrium, which is assumed not to include large fluctuations in output, the questions it tries to answer are: Why are there significant fluctuations in the macro economy? Why doesn't the economy settle down to an equilibrium reflecting agent's desires? Why are there business cycles and fluctuations? And, if there are undesirable fluctuations how can we stop them? It sees fluctuations as being caused by exogenous shocks imposed by government or by technology. Its explanation for why these fluctuations are not eliminated is that institutions prevent the competitive market from solving the problem. Institutions and norms that lead individuals to deviate from self-focused individual rationality are the problem in the equilibrium vision. These institutions and norms impose price rigidities, and constraints on behavior, which prevent the market from achieving a global Pareto-optimal equilibrium.

The complexity vision is trying to answer a quite different question: It has no trouble explaining undesired fluctuations because it does not start with the assumption that Pareto optimal equilibrium would be achieved by the market in the absence of outside shocks and institutions. In the absence of the imperfect institutions that have evolved, the complexity vision would expect chaos and enormous fluctuations in a system. Institutions are a key part of the way an economy coordinates agent's actions. Thus, the macro question the complexity vision is trying to explain is not: Why does output fluctuate? Instead, it is trying to explain why the economy is as stable as it is. Its base assumption is that in the absence of some additional stabilizing forces, the economy would be chaotic and highly unstable. Within the complexity vision, markets do not exist in a void, and thus cannot solve coordination problems unless the underlying institutional structures, such as property rights and norms of behavior, upon which markets are built, have been developed. Markets are institutions; they are not the default reality.

The complexity vision explanation of why fluctuations are as small as they are is the institutions that have developed. Where fluctuations have posed problems in the past, agents in the system have self-organized and created institutions and norms that reduce fluctuations. One of those institutions is the market. In the complexity vision markets are seen as endogenously developed coordination devices. Thus, whereas in the equilibrium vision institutions are a cause of fluctuations, in the complexity vision institutions are what prevent chaotic fluctuations.

These institutions include not only markets, but also government. So whereas the equilibrium vision sees government as exogenous to the system, the complexity vision sees both government and markets as having endogenously evolved. A theory that does not include endogenous government and markets sheds little light on real-world problems. Since institutions provide the stability to the system, they cannot be assumed away in any useful analysis of real-world problems. This makes abstract theorizing about how a market economy would operate without the current institutions, such as is done by researchers holding an

equilibrium vision of the economy, for other than general thinking about abstract issues, of little use.

How the complexity vision was lost in macro

These complexity/equilibrium differences do not fit into the traditional Classical/Keynesian distinction. In fact, the complexity/equilibrium distinction has essentially no correspondence to the Classical/Keynesian distinction.³ There are Classical economists who emphasized a complexity vision and there are Classical economists who emphasized an equilibrium vision. Similarly with Keynesian economists. There are complexity Keynesians and equilibrium Keynesians. But somehow, the complexity interpretations of both Classical and Keynesians have been lost, and the Keynesian/Classical debate has been between a Keynesian equilibrium vision and a Classical equilibrium vision.

It didn't have to be that way. Within both Classical and Keynesian economists, there were both complexity and equilibrium advocates. In fact, up until the 1930s within Classical macroeconomics, the complexity approach was dominant. But starting in the 1930s internal incentives within the profession were moving the profession toward the equilibrium vision and away from the complexity vision. This occurred in both microeconomics and the emerging macroeconomics. One aspect of this is the movement from Marshallian methodology, which followed a Classical methodological approach and which downplayed the importance of equilibrium theory to policy, to a Walrasian methodology, which made general equilibrium theory central to policy.

You can see Marshall's complexity vision methodology in his view about the role of pure theory in economic reasoning. He writes:

"It seems strange to me to be asked my views as to the study of pure economic theory; as tho' that were a subject on which I were fit to speak. For indeed I was never a partisan of it; and for more than a quarter of a century I have set my face away from it. As early as 1873 (I think it was the year) Walras pressed me to write something about it; & I declined with emphasis. The fact is I am the dull mean man, who holds Economics to be an organic whole, & has as little respect for pure theory (otherwise than as a branch of mathematics or the science of numbers)..." (Letter from Alfred Marshall to W.A.S. Hewins, October 12, 1899, in Coase, 1994, pp. 172– 173).

This dismissive view of general equilibrium theory was held by the majority of economists up until the 1930s and 1940s. It held that the pure general equilibrium theory of economics wasn't worth developing not because it wasn't important, but because economists didn't have the analytical tools to deal with it. Using the tools they had, the results were trivial, obvious, or irrelevant. That left macroeconomics to be verbally debated, not to be debated in terms of formal models or equations.

³ That's why in my work on the evolution of modern macroeconomic theory, (Colander, 1996; 2006) I emphasized used different classifiers – Walrasian and Post Walrasian, rather than Classical and Keynesian – with the Walrasian economists maintaining a commitment to equilibrium methodology, and Post Walrasians maintaining a commitment to complexity methodology. There can be either Keynesian or Classical Post Walrasians.

In the 1930s that started to change, as economists abandoned the earlier Classical liberal/Marshallian methodology which had a strict firewall between scientific theory and policy. Instead, they began using a Walrasian methodology that drew policy conclusions directly from scientific theory. The development of macro occurred at this time, and its evolution was significantly influenced by those methodological developments, which shifted the profession from a Marshallian to the Walrasian methodology.

Classical economists didn't formalize their micro analysis into a formal macroeconomic theory because they didn't believe that their micro reasoning about individuals and firms translated to aggregate results in useful ways. They fully accepted what would later become known as the fallacy of composition argument. The aggregate economy was far too complicated for formal theoretical exposition based on an analytically tractable micro foundation.

Rather than a formal theory, Classical economists advanced some general insights about the workings of the macro economy: Say's Law, the Quantity Theory of Money, and the dichotomy between the real and nominal sector. These three insights were developed not as a formal theory, but simply as some insights to correct simplistic, logically incorrect, arguments that had been made by lay people (and some economists) about the workings of the aggregate economy.

For example, lay people often argued that if people saved, it would mean that there would not be enough aggregate demand to buy the aggregate supply. Say's Law countered that simplistic argument, and pointed out that, in the aggregate, supply was intricately related to demand through financial market interconnections between saving and investment. Classical economists fully agreed that that interrelationship between aggregate supply and demand was noisy and unstable. All Say's Law implied for policy was that the interconnection was definitely something to keep in mind when thinking about macro policy, and that the simplistic arguments, which held that saving would necessarily imply a shortage of aggregate demand, were not correct. Similarly, with the lay arguments that confused price level with relative price, or held that an increase in money supply would necessarily make society richer. Such arguments missed the insight that the wealth of nations resided in real output and that one needed to account for price level changes in determining relative prices over time, and in determining whether a change in the aggregate wealth of a society over time has occurred.

Classical economists recognized that there were all kinds of ways in which that equality between aggregate supply and demand could be broken, and that "Say's law" was fully consistent with widespread temporary unemployment, business cycles, and recessions.⁴ The same was true for the Classical propositions about the neutrality of money and the quantity theory. In short, these Classical insights are best understood not as formal theories, but rather as general insights about the aggregate economy that were meant to be understood in the context of the debate in which they were used, not to be used as part of a precise equilibrium theory about how the real-world economy would operate. If you are always moving from one

⁴ Petur Jonsson (1997) makes this point clearly. He notes that Say wrote "In the first place my attention is fixed by the inquiry, so important to the present interests of society: What is the cause of the general glut of all the markets in the world, to which merchandise is incessantly carried to be sold at a loss? What is the reason that in the interior of every state, notwithstanding a desire of action adapted to all the developments of industry, there exists universally a difficulty of finding lucrative employments? And when the cause of this chronic disease is found, by what means is it to be remedied? On these questions depend the tranquility and happiness of nations." This is hardly a statement that a person who believed that Say's Law implied that there could be no unemployment would make.

equilibrium to another, and you never arrive at any equilibrium, being precise about final equilibrium that the economy is aiming for is not all that important.

Consistent with the view that Classical economists did not have a formal macro theory, Classical practical guidance on short run macro policy did not follow directly from these theories and laws. Early Classical policy discussions, such as the bullionist/anti-bullionist debate, reflected an institutionally rich understanding of policy by individuals knowledgeable in both the abstract theory *and* the current institutions. Walter Bagehot's (1873) discussion of monetary policy, which blended institutional insights and theoretical insights into insightful pragmatic policy guides, is an example of what I mean by the Classical applied policy methodology. It reflects a complexity vision – it is a practical, educated common sense approach to policy. These Classical applied policy works blended theoretical Classical macroeconomic insights with deep institutional knowledge and arrived at useful guidance on the conduct of monetary policy. No formal general theory is required or is even seen as useful.⁵

Cutting edge Classical economists knew that their three propositions were not an acceptable theory for short run aggregate fluctuations. They were simply insights about general tendencies and what the logic of the model implied. Good Classical economists knew that they had no formal equilibrium theory of the aggregate economy. But they did have a set of policy precepts that were based on past empirical evidence and insights, not on theory. Based on that evidence, in the 1920s and 30s, they assumed that a fall in aggregate output would be of short duration since that was their experience with past fluctuations. Their policy suggestion of government not stepping in was based on experience, (and concern about whether government would or could effectively do something to reduce the depression) not on any formal theory.

With the Great Depression of the 1920s in Europe and 1930s in the US, Classical economists' policy precepts were rightly being questioned; the empirical pattern had changed. As the depression continued, their assumption that in long run the fall in output fluctuations would resolve themselves on their own, which for the most part was unexamined, was requiring a "long run" that was much longer than policy makers would accept.

In response to these developments theoretical Classical economists started exploring the interconnection between micro decisions on supply and their relationship to aggregate demand in more detail. For example, economists such as Ragnar Frisch (1933) began formally exploring macro dynamic sequence models, starting from micro foundations, in which interconnected industries transmitted a negative demand or supply shock in one industry into other industries, setting up a potential feedback reinforcement loop that could lead to depression-like conditions. Today, we would see that work as part of a study of complex non-linear dynamics of a system with multiple basins of attraction. But at that time, their formal work was generally ignored since most economists didn't have the mathematical background to understand this advanced work, and it didn't lead to any specific policy recommendations. Keynes was a cutting edge Classical economist, who followed a Classical liberal methodology. But unlike other Classical economists working on macro dynamics, he was not interested in developing a carefully spelled out formal theory of dynamics connected to micro foundations. He was more interested in capturing the big picture and conveying it to other

⁵ Since data was limited and statistical techniques were not yet developed, formal empirical work also played little role in the policy debates. The policy debates were "armchair" debates done without formal theory.

people in a way that would help him win his policy arguments. Thus, while following a Classical liberal methodology in some ways, he was willing to violate the classical firewall between theory and policy. This violation made it impossible to separate out the theoretical and policy differences between Classical and Keynesians, so that they could both have the same scientific theory, but different policy views. Losing the firewall made it almost impossible to have a non-ideological debate about the theory. Both sides had policy agendas built into their “scientific” theories which made neutral objective discussions of them impossible.

Keynes was a skilled advocate and marketer of ideas, and in his *General Theory* he developed a highly simplistic informal aggregate theory that could be represented in a simple graphical aggregate demand/supply expenditure model (the Keynesian Cross) that emphasized aggregate adjustment via output fluctuations rather than adjustment by price level fluctuations. Keynes’ alternative model had multiple equilibria dependent on exogenous shocks to demand. Rather than supply creating its own demand, in Keynes’s alternative model, demand created its own supply. So rather than both sides agreeing that aggregate supply and demand were interconnected, which could create serious dynamic adjustment problems, we had two opposing theories each connected to specific policies.

To contrast his new theory with existing views, Keynes created a Classical straw man equilibrium theory of the aggregate economy. As opposed to saying that Classical economists had no theory of dynamic adjustment, and that he was adding one possible adjustment mechanism, he attributed an equilibrium theory of the aggregate economy to Classical. Essentially, the argument he made was the following: Say that all three Classical propositions hold. Then, as long as there is no deviation between aggregate supply and demand, the economy remains in equilibrium. But if, for some reason, aggregate demand slightly differed from aggregate supply, and both AS and AD were perfectly inelastic, classical theory in its most formal specification, had no dynamic adjustment mechanism to bring them into equilibrium. (Price level influences on aggregate output in the absence of an international sector, were ruled out by Classical assumptions that aggregate supply and demand were perfectly inelastic, and were interconnected by Say’s Law.)

This Classical equilibrium theory didn’t capture the thinking of cutting edge Classical economists, but rather set them up to be refuted by his alternative theory. Keynes suggested that Classical economics explained unemployment and the depression, not as being caused by dynamic adjustment problems that they could not analytically model, but rather as being explained by a partial equilibrium model in which too high wages were the culprit for unemployment. This belief could be easily shown to be an unsupportable theory. He suggested that their macro model consisted of three propositions that kept the aggregate economy in constant equilibrium. This required attributing a formal equilibrium model to them. Keynes created a straw-man Classical equilibrium theory based on rational agents, and showed how the three Classical insights that made up this straw man characterization came to the conclusion that the macro economy would always be in aggregate equilibrium at full employment, and that had the policy implication that government demand management policy could not affect the aggregate output of the economy.⁶

⁶ Classical economists responded to this argument by arguing that technically their model was not totally illogical; price level adjustment could technically bring about equilibrium via the Pigou effect. They also agreed that as a practical matter the Pigou effect was too small to achieve the desired equilibrium, and that “on the checkerboard of real life” it was irrelevant.

Where Keynes went beyond other complexity-vision Classical economists was in proposing an alternative dynamic adjustment mechanism. He argued that, faced with excess demand, rational suppliers would cut real output, which would reduce income, which would further reduce output. The economy would find itself in a downward output spiral. In principle that downward spiral could continue forever. But, Keynes, following Kahn, argued the spiral would stop because of psychological laws governing micro behavior as captured by relatively stable marginal propensities to save and consume. Because people saved a relatively fixed proportion of their income, as aggregate output fell, aggregate demand would fall by less. In each round of the process the disequilibrium would decrease, and the economy would asymptotically approach an equilibrium. In this Keynesian model individual rationality did not bring about aggregate equilibrium; agent irrationality – the habit captured in the constant marginal propensity to consume – did.

Had this multiplier storyline been seen as Keynes' key contribution to macroeconomics (which it was initially by some economists) macroeconomic theorizing would have followed a quite different path than it did. But the mathematics involved in formally working on dynamics and interrelating them into an equilibrium model were treacherous. It required going into issues involving complex dynamics that were technically far beyond the capabilities of most economist of the time. Richard Goodwin, in his work on matrix multipliers, was an exception. He captured the problems, writing "Combining the difficulties of difference equations with those of non-linear theory, we get an animal of a ferocious character and it is wise not to place too much confidence in our conclusions as to behavior" (Goodwin, 1950).

To have a full theory Classicals needed to spell out that dynamic adjustment mechanism in which price level adjustment could not bring about equilibrium. So in the complexity version of macro history, Keynes's contribution was to point out that Classical economics had no aggregate dynamic adjustment mechanism. This was consistent with models that cutting edge Classical theorists were working on directly from supply side considerations. Had it been presented this way Keynes would not have been seen as offering an alternative to Classical theory, but rather as offering an extension of Classical theory, which incorporated dynamics. His multiplier model offered one possible dynamic story, but there were many more alternative ones. Had macro economists followed their complexity vision, researchers' focus would have been on developing alternative dynamic stories, and then testing them empirically. Macroeconomic science would have become much like weather science where there is only one science based on general laws, but many models that reflect different dynamics. There would be no Classical/Keynesian theoretical debate about equilibrium models; there would be debates about alternative dynamic adjustment theories.

As should be clear in the tone of my writing, as much as I admire Keynes, I also blame him for this movement from a complexity focus to an equilibrium focus. By not making it clear that his insights about the problems of Classical theory were understood by other cutting edge Classical economists, and that neither his, nor their, theory led to any firm policy conclusions, he led the profession into fruitless debates about formal equilibrium theories. Had he maintained the Classical firewall between science and policy, the policy debate would have been separated from the theoretical debate. The policy debate would be seen as a debate about subtle issues involving politics and sensibilities, not about macro theory. People could reasonably differ about sensibilities, and that debate would not, and could not, be settled by scientific methods.

Perhaps fittingly, in creating a straw man to attack, Keynes set Keynesian economics up for failure. By that I mean that the equilibrium characterization of Classical economics also led to Keynes' theory being interpreted in a similar equilibrium setting. This undermined any complexity vision interpretation of Keynes' ideas, which was the revolutionary part of Keynes' thinking. Instead of being the entre into dynamics, the multiplier was integrated into static equilibrium and the debate became about equilibrium models, not dynamic models. This forced Keynesians to answer the equilibrium macro question – why the aggregate economy would not move to a Pareto-optimal equilibrium, rather than to answer the complexity macro question of why dynamic forces could cause problems that the current institutions did not resolve.

The equilibrium characterization was quickly built into the standard Keynesian model, which shifted from the dynamic multiplier model, to a multi-market general equilibrium model, the essence of which was captured in what was called the four quadrant diagram, which showed goods/market/money, market/bond market dynamic adjustment to equilibrium. That four quadrant diagram, which demonstrated (with a lot of hand waving) equilibrium as being asymptotically reached, soon gave way to the IS/LM version of the model that totally hid the assumed dynamic adjustment underpinnings of the argument, and presented both the Classical and Keynesian models in equilibrium space. The LM curve captured money market equilibrium; the IS curve captured goods market equilibrium. The model was in “general equilibrium” when the two curves intersected. As a geometric exposition of how to solve comparative static equations, all this was very nice, but the model obscured all the dynamics that would have been the focus of debate in the complexity interpretation of both Keynesian and Classical economics. In the IS/LM model, in which the multiplier worked instantaneously, the multiplier dynamics were hidden in the shape of IS curve. Multi market equilibrium was characterized as being as easily achieved as a single market equilibrium.

The IS/LM model became the totem for what came to be called NeoKeynesian macro. This model totally obscured issues of dynamics. NeoKeynesians and NeoClassicals were differentiated on their beliefs about the shape of the LM and IS curves, not on their beliefs about dynamic adjustment processes, which is where the complexity vision put the differentiation. The entire complexity debate about dynamics, based on judgements about how the dynamic adjustment process worked, was lost.

Most of what went under the name macroeconomic theory in the 1950s and 60s explored equilibrium issues. This presented a serious problem for Keynesian economics.⁷ For long run full employment equilibrium not to be the outcome, one had to posit some price adjustment inflexibility in the system – fixed wages, fixed prices, or below zero equilibrium interest rates are examples. But if one's model does not include the need for institutions which impose those constraints, those inflexibilities created by institutions seem ad hoc. In a Walrasian general equilibrium model, Keynesian economics loses to a Classical model, which is precisely what happened with the New Classical revolution.

⁷ Some Keynesians pointed these problems out. But those who worked on these dynamic interpretations, such as and G.L.S. Shackle (1949) and Paul Davidson (1970), soon found themselves banished to the realm of heterodoxy. They had few followers; their explanations of Keynes's ideas did not fit the sweet spot of theory within the institutional structure of the economics profession of the time, which required theory to be specified in tractable equations that economists of the time could follow and work with – not too complex, not too simple. IS/LM found that sweet spot.

Much of modern macroeconomics still conceives of the macro problem in that equilibrium framework. In it the difference between Keynesian and Classical economics became differences of assumptions about an equilibrium model: Keynesians assume inflexibilities in the system that prevent equilibrium from being reached; Classical don't. This essentially made Keynesian economics an addendum to the Classical straw man that Keynes had created to have something to critique. NeoKeynesian economics became a straw man attack of a straw man creation. The belief that Keynesian economics actually involved revolutionary thinking – that what we should be studying are dynamics, not equilibrium – disappeared.

Micro foundations and the fallacy of composition

Let me now turn to where micro foundations fits in the history I am recounting. Micro foundations, in some form, has always been part of macro and always will be part of it. While the ideas in Keynes' *General Theory* acquired the name macroeconomics, (it was first called macro dynamics) from the beginning, much of the discussion in the *General Theory*, and debate about it, involved discussion of micro issues, and it is often said that the General Theory is 70% micro.⁸ What differentiates that micro foundations discussion from what has become known as micro foundations is how the micro discussions are connected to macro results.

In the complexity vision, there is no reason for macro results to follow directly from micro decisions. In fact, such a connection would not be expected. Any differences can be resolved by appeal to the fallacy of composition – what is true for the parts is not necessarily true for the whole. So in the complexity vision micro decisions and macro results are related, but not in any simple way. The fallacy of composition black box allowed micro decisions to be connected to macro results in many different ways.

In the *General Theory*, Keynes invoked the reasoning behind the fallacy of composition often. Thus, we can see him talking about animal spirits guiding the economy, beauty contest coordination problems, and the distinction between uncertainty, which cannot be hedged, and risk, which can be hedged, throughout the book. Those discussions were based in the complexity vision, not an equilibrium vision of macro. The scientific complexity macroeconomic research program is to unpack that fallacy of composition black box to better understand how they are connected. It is that that modern complexity economists are doing with their studies of non-linear systems, agent based models, and network models.

Complexity and current macro policy

The policy complexity macroeconomic research program has two components—one concerned with the practical problem of guiding current policy, and the second concerned with exploring ways in which the fallacy of composition black box can be changed so that micro decisions lead to desirable macro results.

⁸ For the most part, that micro foundations discussion involved modifications and adjustments to the point that Classical economists had no acceptable theory of aggregate dynamics, and that if Keynes was talking about a long run unique equilibrium model, Keynes had not fully specified his alternative theory.

I don't have much to say about the first of these, but I will provide a brief summary of my thinking. Much of the current macro policy debate concerns which model to use to guide monetary and fiscal policy decisions. There are two general positions: one is that one should use some variant of a VAR model. The other is that you should use a formal model – either DSGE or IS/LM – and empirically estimate the relevant structural equations. The complexity approach comes out strongly on the side of using a variant of the VAR model, because it is data, not theory, centered. The reason is that, from a complexity vision, that entire structural macro modeling project is problematic because it doesn't take account of the institutional complexities that play a central role in the dynamics of the system. Those institutional complexities are too complicated to formally analyze from first principles, so a more macro analysis is needed. Put simply, from a complexity perspective, the macro economy is too complex to formally model from first principles, taking into account the complex dynamics that the complexity vision believes need to be taken into account. This hold for both structural IS/LM type models and DSGE type models.

The modified VAR modeling approach that is associated with the complexity approach can be seen in the work of David Hendry and Katarina Juselius, which elsewhere I have called the European approach to macro econometrics. (Colander, 2009) Unfortunately, their work is not seen as central to standard econometrics by many econometricians, especially those in the U.S., where their work is often little known. I have hope that this approach will become more popular in the future because it is closely related to what is being called a data science empirical approach, which is gaining wide acceptance outside of economics. I see data science as the complexity approach to empirical work, and I contrast it with the current more structural econometric approach.

The two approaches differ by the role they see for the interaction of theory and data. Both see empirical work as central, but the US standard econometric approach sees formal theory as a necessary guide for our understanding of data. Econometrics has its methodological foundations in logical positivism – it puts theory first and is designed to test theories and shed light on causality. It interprets data through the lens of formal theory. The complexity approach follows a data science approach that puts data first. It agrees that data has to be interpreted through some lens, but the appropriate lens in economics is not an inadequate general equilibrium theory. It is, instead, an educated common sense lens. Data science methodology is meant to find patterns in the data without first subjecting the data to any predetermined theoretical lens. Theory is still important, and a loose sense of theory cannot be avoided in the initial collection of data and in interpreting empirical data. But to the degree possible the goal of data science is to let the data speak.⁹

For most economists, trained in econometric methodology, this “data first” approach to empirical work seems questionable, and unscientific. It has a long history within the broader statistics community from which econometrics developed. Econometrics simply took a different path, and, as econometric practices within the economics profession developed, it emphasized certain asymptotic aspects of statistics – those aspects that fit with testing and relying heavily on theories – and downplayed other potential methodologies that exist in the statistical research inventory, such as non-parametric empirical analysis, bootstrapping and a variety of other methods that fall within the data first approach.

⁹ It is, however, recognized that different people with different theories will interpret the same empirical data differently. These differences are natural, and can only be resolved by philosophical methods – engaged discussion and debate amongst researchers – not by scientific methods.

The distinction I am drawing between data science and econometrics is, of course, far too stark. Both econometrics and data science blend theory and data, and both have their roots in formal statistics. Moreover applied econometrics is changing, as more and more econometricians have started using data science methods such as bootstrapping, and non-parametric econometrics and formally dealing with cases where they accept that the assumptions needed for asymptotic econometrics do not exist. This is not surprising; the two empirical approaches come from the same statistical bag of tool. The difference is simply a matter of separate evolutions; the state of the arts in statistics and econometrics have evolved differently, and “statisticians” (researchers trained in a formal statistics department) developed different traditions and emphasis that did econometricians, (researchers trained in economics departments) as institutional incentives facing researchers have pulled them in different directions and led them to look at different type problems. Advances in computational technology are now pulling the two closer together.

Actually, even in practice, I am not sure that the differences between the structural IS/LM, DSGE and VAR models matter all that much, because of the ad hoc way in which I suspect structural models are brought to the data. Here is my suspicion: In applied policy models such as those used at central banks, to make the abstract DSGE and IS/LM models fit that data, macro modelers make adjustments to the pure theoretical models. With sufficient adjustments it is not clear how much the core model is guiding the results, and how much the intuition of the modeler is guiding the results. In both cases the intuition of the modeler plays a central role in determining the model’s results. I have not kept up with macro econometrics and my assessment is based on the assumption that current practices have not changed from past practices.

Let me explain where my suspicion comes from. I came of age in the macro econometric modeling in the 1960s when large structural IS/LM macro models were central to macroeconomics. Each of the major models of the 1960s had 100s of equations that captured the various sectors of the economy, but which were, by today’s standards, rudimentary. The model would be divided into sectors and subgroups of researchers would estimate the equations that specified the sector. The head modeler would put the equations together, and run the model. Inevitably the initial results were so far from believable that they had to be modified and adjusted. So researchers would go back to the drawing board and adjust or tweak the underlying equations and run the aggregate model again. And again, and again... Adjustments would continue until the model came out with a reasonable forecast.

I am not suggesting that this was a bad way to do macro empirical policy work; I certainly had no better way. But, the process of adjustment in the model’s conclusions suggested to me that the head modeler’s intuition, not the structural model, was not driving the results. I lost faith in macroeconometrics of the time. My sense was that in the end, there were so many modifications and ad hoc adjustments made to the structural model that no one had any idea of precisely what structural model was being tested. One was simply fitting the model to the data.

The actual results were generally not that bad. But that was not because of the model. It was because of the modeler. Since working with data and the model provide the modeler with an intuitive sense of how the economy works, over time, modelers gained an understanding of the economy, which leads to better predictions than predictions made by individuals who did not immerse themselves in the data.

Differences in policy methodology: complexity micro foundations and creative theorizing

While I see less of a role for macro theory in guiding actual monetary and fiscal policy analysis, I do see theory as having an important role in a complexity vision of applied policy research that it does not currently play. The role does not involve guiding monetary and fiscal policy. The role is, instead, a creative design role – exploring how theoretically institutional changes might affect the compositional black box through which micro decisions of agents are connected to aggregate results, in a way that leads to preferable outcomes. That role is what I call creative theorizing. If we find that the institutions are not working, then the policy role for complexity economists is to theoretically and empirically explore how those institutions might be changed to better coordinate the system. For macroeconomics, this means that the key policy question is: Can we develop institutions that would better coordinate aggregate results?

Monetary and fiscal policy can be seen as policies that might better coordinate agent's decisions so that they lead to more desirable aggregate results. They change the compositional black box, with the government trying to counteract agent decisions that lead of undesirable results. Fiscal policy involves the government varying its spending in a countercyclical way to smooth out fluctuations. Monetary policy involves the central bank modifying the financial environment affecting agent decisions to smooth out fluctuations. From a complexity vision standpoint, they are proxy policies that modify existing institutions to attempt to better coordinate aggregate decisions on spending. But, assuming no transactions costs, (as is the case with most economic theories of markets) there are, theoretically, much better ways to bring about the desired coordination. Creative theorizing explores those alternatives in the search for alternative methods.

The theoretically neatest way to do this would be to create markets in the output dimension one wants to control. To solve a coordination problem with an existing market, you simply create an additional market by creating property rights in the outcome that you want to coordinate, and allowing trading those outcome rights, and presto, the new market solves the coordination problem. The policy role I see for complexity economists involves exploring those alternative market institutions theoretically, and then seeing if any of these alternative institutions can be actually used in practice.

Complexity policy macro economists would explore many possible institutional structures, seeing how they work in abstract models, and determining whether they have analogs that might be possible to implement. So, from a complexity standpoint, a major role for macroeconomic theorists that they are not playing is the role of design engineer. In that role they explore ways of adjusting institutions, or creating new institutions, that make it so that micro decisions lead to preferable aggregate outcomes.

Here is the reasoning: If one wants different aggregate results, one need to explore policy changes that will lead agents to make different choices than they currently make. The policy research agenda is to explore alternative institutional structures that will better coordinate individual decisions. Instead of asking, what will the result of individual actions for the aggregate economy, one asks, how can we solve coordination problems? One way to do that is through backward induction and mechanism design, in which one specifies the aggregate results one wants, and explores alternative institutional structure would lead to that outcome

in a model. This is what I call the Coase method. One posits zero transactions cost and creates a property right structure guiding agent behavior to the desired result.

Let me give an example. Say you desire a system that has zero inflation. Such a system would require that all price changes be relative price changes, not price level changes. That could be achieved by an institutional structure in which whenever someone raised their price, someone else would be required to lower their price by an offsetting amount.

It was precisely such an approach that I developed with Abba Lerner and Bill Vickrey in the 1970s when inflation was seen as a major problem. The plan was called MAP, which stands for market anti-inflation plan. It consisted of assigning property rights in appropriately defined value added prices, so that any agent wanting to change their value added price had to pay another agent to change their value added price by a countervailing amount so that the index of prices would not change.¹⁰ Any agent wanting to raise (or lower) their price would have to offset the effect of that by buying the right to do that from someone who lowers their appropriately weighted price by an offsetting amount. With a MAP system in place, all price changes had to be relative price changes, not absolute price changes. Theoretically, the market solved the inflation problem. (The proposal works equally well for stopping deflation. If there are deflationary pressures, an individual lowering his or her price would have to pay someone else to raise theirs by an offsetting amount.)

If there was inflationary pressure, the price of raising prices would be positive, and that price would offset any inflationary pressure. So with MAP there could be no inflation, no expectations of inflation, and no acceleration of inflation. Instead of a tradeoff between inflation and unemployment, there would be a tradeoff between the price of raising price and unemployment, so if unemployment was being used to hold down inflation, that unemployment could be eliminated since the MAP program was holding down inflation. In this model, monetary and fiscal policy had a role to play in fighting inflation, but it was an indirect, not a direct, role. Monetary and fiscal policy might affect the price of raising price, and thereby change the steady state equilibrium unemployment rate of the system.

My interest in the plan was primarily theoretical – to try to better understand the inflationary process. Bill Vickrey and Abba Lerner both thought that MAP was implementable, and strongly advocated it. The profession did not agree, and it lost interest in the plan. I argued that even if the plan was not worth implementing, it was nonetheless important in terms of our theoretical understanding of macroeconomics. It removed the issues of price controls and incomes policy from the theoretical debates, and put it in the practical implementation debate involving transactions costs. Markets have costs, as do all methods of coordination. All coordination problems can be thought of as problems of missing markets, but they may be missing because they have too high transactions cost compared to the benefits they provide. Within the missing market policy frame, an incomes policy was simply a replacement for a missing market, and is as consistent with macro theory as is any other policy. The debate about incomes policies and price controls should be about alternative goals of policy and about transactions costs of different policies, not about macro theory.

The backward induction approach to policy is not limited to inflation. To show the usefulness of the backward induction approach to micro foundations, let me discuss another “macro

¹⁰ Obviously there are a number of technical issues involving price indices that I won't go into here. They are discussed in a number of articles and a book I did with Abba Lerner, (Lerner and Colander, 1980). The price index stabilized was value added per unit input prices, not output prices.

policy solution” that follows from it, and what that solution means for the debate about activist monetary and fiscal policy. Within the complexity approach aggregate output fluctuations occur because there is no explicit coordination mechanism in the economy to determine aggregate spending. If there are unwanted fluctuations, then the fluctuation is being caused by a faulty institutional structure that is not coordinating individuals’ demands in a way that is socially desirable. The backward induction solution to the problem would be to create property rights in spending, so that anyone who wanted to increase their spending would have to buy the right to do so from someone else who decreased their spending by an offsetting amount (and vice versa). A system with such a property right system in place could eliminate aggregate income fluctuations.

If spending were too low, the price of spending rights would be negative, spending would be subsidized, leading agents to spend more. If spending were too high, the price of spending rights would be positive, leading agents to spend less. The greater the deviation of desired spending with actual spending the higher the price of spending rights would be. With property rights in spending, price level changes would not be needed to stabilize the economy. The appropriately designed spending rights market would adjust the price of spending to an aggregate level consistent with the desired aggregate level of spending.

Let me be clear; I’m not advocating that such a market to be created. But I am arguing that thinking about the aggregate output fluctuation problem in this way suggests the uselessness of the debate about whether an activist monetary and fiscal policy is consistent with macro theory. In the complexity research program there is no theoretical foundation to macro policy needed; we know how to theoretically solve the problem with the market – create property rights in the action needing coordination. Whether that is a good policy is a practical institutional question, not a theoretical question. Policy research would explore the costs of various coordinating mechanisms compared to the cost of fluctuations.

There are many variations of this spending market plan that could be developed. Fiscal policy is a partial solution that involves one agent – government – doing all the adjustment. Theoretically, it would be preferable to have all agents doing the adjustment based on their cost of adjusting spending. In theory, the market in spending rights achieves this end. Thinking about such abstract alternatives and whether those abstract markets suggest any practical alternatives is what is meant by creative theorizing.

Let me emphasize once again that I am not advocating implementing these markets as an actual policy.¹¹ Rather they are presented as examples of the type of creative policy thinking that I believe macro economists should be doing as part of complexity policy analysis. By thinking about abstract policies that help “solve” the coordination problem, creative theorizing directs debate away from theoretical debates about what causes the coordination failure within an hypothesized economy that has never and can never exist, and toward the question: what can we do to reduce coordination problems and achieve a more desirable result.

¹¹ One macro market that I believe is worth exploring is a countertrade market in which as part of a broad trading agreement countries use these markets to keep their international trade balance within agreed upon limits. It would operate in a similar fashion to Keynes’s Bancor plan and require surplus countries to share in the adjustment process. See Colander, 2017.

Conclusion

This has been a fast and broad-brush overview of macroeconomics, complexity and microfoundations. It differs significantly from the standard history of macro, in that it sees the relevant theoretical debates as debates about dynamic adjustment and policy debates about pragmatic methods of coordination. It leads to a suggestion for an increase in mathematical complexity of theoretical macroeconomics, but no direct application of the models to policy, which are seen as institutional based decisions that theoretical macro models shed little direct light on. Macro models are used as reference tools, not direct guides to policy. Economists are a long way from such a complexity approach, but I remain optimistic that in the long run they will adopt it, perhaps even before we are all dead.

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Models and reality: How did models divorced from reality become epistemologically acceptable?

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1. From surrogate to substitute models

The problem at the heart of modern economics is buried in its logical positivist foundations created in the early twentieth century by Lionel Robbins. Substantive debates and critiques of the content actually strengthen the illusion of validity of these methods, and hence are counterproductive. As Solow said about Sargent and Lucas, you do not debate cavalry tactics at Austerlitz with a madman who thinks he is Napoleon Bonaparte, feeding his lunacy. Modern macroeconomic models are based assumptions representing flights of fancy so far beyond the pale of reason that Romer calls them “post-real”. But the problem does not lie in the assumptions – it lies deeper, in the methodology that allows us to nonchalantly make and discuss crazy assumptions. The license for this folly was given by Friedman (1953, reproduced in Maki, 2009A): “Truly important and significant hypotheses will be found to have ‘assumptions’ that are wildly inaccurate descriptive representations of reality”. In this article, I sketch an explanation of how economic methodology went astray in the 20th Century, abandoning empirical evidence in favor of mathematical elegance and ideological purity. Many authors have noted this problem – for instance, Krugman writes that the profession (of economists) as a whole went astray because they mistook the beauty of mathematics for truth.

To begin with, it is important to understand that modern economics is entirely based on models. There is a lot of merit to the idea that economic knowledge must be encapsulated in models. This is because economic systems are complex and interactive. We may well have strong intuitions about some local aspects of the system, but when we put all our intuitions about the different parts together, something unexpected may emerge. This is now well known as the phenomenon of complexity, and emergent behavior. This also explains the central importance of mathematics in modern economics. When we want to piece together parts of a complex system into a whole, mathematics is necessarily and inevitably involved, because the required integration cannot be done intuitively and qualitatively. The central hypothesis which drives this paper is that the relationship between economic models and reality shifted over the course of the 20th century. The nature of this shift can be described by borrowing some insightful terminology from Maki (2018). He defines two types of models. One is a *surrogate model*: such a model is a simplification which attempts to match some complex reality, and can be judged by the degree of resemblance it achieves. The second type is a *substitute model*: the imagined mini world of the model is a substitute for the target maxi real world, rather than an attempt to approximate the latter. As Maki (2018) notes: “surrogate models can be wrong (or right), while substitute models cannot even be wrong about the world (since they are not presented and examined as being about the world).” Our main thesis in this paper is that economists started to use models as surrogates, but eventually fell

¹ I would like thank Alan Hájek for his stimulating and insightful comments, Edward Fullbrook for encouragement and Lars Syll for his help in finding useful quotations to buttress my arguments.

in love with their own creations, and began to treat them as substitutes for the real thing. The goal of this paper is to sketch how and why this happened.

2. A middle-brow history of methodology

Our goal in this essay is NOT to add to the debunking of economics – this task has been done in many books and essays, and the debunking has been contested by many other books and essays. A balanced state of the art survey is available in Uskali Maki (2002) who opens the book with:

“Fact or fiction? Is economics a respectable and useful reality-oriented discipline or just an intellectual game that economists play in their sandbox filled with imaginary toy models? Opinions diverge radically on this issue, which is quite embarrassing from both the scientific and the political point of view.”

Instead of joining this debate, we take the second option as a given: economics is an intellectual game that economists play with toy models. We are interested in the meta-question of how did this become possible? What are the trends in history of thought which allowed the development of models completely divorced from reality?

A book length detailed treatment of the answer to this question has been provided by Manicas (1987) in “A history and philosophy of the social sciences.” The central thesis of this “embarrassingly ambitious” book challenges the very notion of “social science”, suggesting that it was built on the wrong foundations. A very brief outline of the central ideas of this book is as follows.

1. The practices of the modern sciences which emerged in the sixteenth and seventeenth centuries were incorrectly characterized. For various historical reasons, this remained unrecognized in the more refined and sophisticated ‘philosophies’ of science which subsequently came to be articulated.
2. Social sciences took their modern shape in the early 20th Century as the result of a deliberate attempt to apply the ‘scientific method’ to the production of knowledge about human societies. But the understanding of the scientific method was deeply flawed. As a result, the “methodology” adopted for use in social science was also deeply flawed.

According to Manicas, “The upshot is the possibility of a thoroughgoing revolution in the received ideas of science, natural and social. It allows us to ask whether there is a huge gap between the ideology of science and practices in the physical sciences, and whether, more disastrously, the social sciences have been ideologically constituted in the sense that they were based on a *misconception* about what the physical sciences are.”

In a commentary on Rodrik’s (2015) defence of economic methodology, Maki (2018) writes that “The portrait of economics offered by philosophers of economics... (is)... too refined for practicing economists, but the degree of refinement... (in understanding economic methodology)... currently held by practicing economists is often too low.” The message of Manicas (1987) is central to understanding current methodology of social science, and leads to the possibility of a thoroughgoing revolution. However, reading and understanding this

book requires background in history and philosophy which very few economists have. As a partial remedy, I have attempted to provide a coarse-grained and crude summary of some of the highbrow philosophical ideas which have driven the development of methodologies in the social sciences in general, and economics in particular. The goal is to explain how it became possible to think that it is reasonable to develop models without connecting them to external real world structures. We begin with a rough description of what this methodology is, based on an experiential view, rather than a theoretical perspective.

3. The methodology of economics

What is the methodology economists use to arrive at knowledge about the economic system, used to analyze, explain, and decide upon policy? This question is not as easy as it appears on the surface for a number of reasons. The problem in understanding methodology comes from the easily documented fact that the justifications offered for methodology, the textual explanation of the methodology, the actual practice, and what we economists think we are doing, are all different. Maki (2002) is an anthology of an extensive discussion from diverse viewpoints on the extent to which economic knowledge is fictional or factual. The goal of this essay is not an in-depth exploration of methodology. In order to cut through confusion, I will take a practitioners' approach, namely, the methodology that I learned as a graduate student, and adopted in my own research. This "experiential" or folk-methodology, is full of incoherent and contradictory elements. In a gentle critique of Rodrik's (2015) defence of economic methodology, Maki (2018) concludes as follows: "Economists are *desperately* in need of a better self-understanding, a more adequate portrait of their discipline, including its methods of modelling." As students, we arrive at a very clear, explicit, and detailed understanding of economic practice, as it is expositied by textbooks and teachers. Reflections of methodology are discouraged, because we know exactly what we are doing, and methodological discussions seem hopelessly ambiguous, imprecise, and unrelated to the work of producing good economic models. It is precisely because of this lack of reflection that incoherent and inconsistent methodological approaches continue to dominate the profession. The folk-methodological principles outlined below are part of what I and fellow graduate students and later, colleagues in Economics Departments learned, used, and taught, often without explicit articulation.

3.1 Baconian science

Without much reflection or discussion, we are trained to think of Science in Baconian terms. Scientific laws are obtained by induction from a pattern of observations. Deeper discussions on whether we need induction, or "abduction" to the best explanation never take place. There is general positivist outlook which suggests that "unobservables" should be shunned in scientific theories. No one seems to be aware that, due to developments in physics, the positivists themselves had moved on to much more sophisticated formulations of the notion of "observability" before abandoning it as a hopeless cause. In economics, we continue to use unobservable and observable in the primitive sense, are not aware of the philosophical difficulties that emerge on attempting to reconcile these terms with accepted theories of physics. Even more interesting, we do not even reflect on the fact that the Baconian approach is not actually followed in development of the textbook models of economics. There is no description of large amounts of empirical data which is synthesized into theories. Without discussion, students assume that this preliminary spadework has already been done, and we are studying theories which distil masses of empirical evidence, without explicit mention. In

fact, this is not the case, as I learned much later. The axiomatic theories we study fly in the face of massive amount of empirical evidence uncovered by behavioral economists and psychologists, and these conflicts are routinely ignored; see Zaman and Karacuka (2012C) for a survey.

3.2 Axiomatic-deductive model of science

In the 1930's, Lionel Robbins re-constructed the foundations of economics, replacing the earlier "welfare approach" by the scarcity approach now universally adopted; see Cooter and Rapaport (1984) for details and discussion. This was parallel to efforts made throughout the social sciences to make humanities more scientific by adopting the "scientific method". Unfortunately, "scientific method" was defined as it was (mis)understood by logical positivists. According to positivists, science was based on a set of certainties (facts, and scientific laws) and logical deductions from these postulates. Lionel Robbins (1932) expressed this "received view of scientific theories" as follows: "The propositions of economic theory, like all scientific theory, are obviously deductions from a series of postulates. And the chief of these postulates are all assumptions involving in some way simple and indisputable facts of experience." This continues to be the standard understanding of what science is among economists, and provides the justification for the strongly held belief that economics is a "science." Practitioners (like myself) are blissfully unaware that this view is now dead and buried, abandoned by its most ardent defenders. For example, the opening paragraph of Suppe (2000)

"The Received View on Theories was the epistemic heart of Logical Positivism. Twelve hundred persons were in the audience the night it died. It was March 26, 1969-opening night of the Illinois Symposium on the Structure of Scientific Theories. The Received View had been under sustained attack for a decade and a critical mass of main protagonists had been assembled to fight it out. Carl Hempel, a main developer of the Received View, was the opening speaker and was expected to present the Received View's latest revision. Instead he told us why he was abandoning both the Received View and reliance on syntactic axiomatizations (Hempel, 1974). Suddenly we knew the war had been won, and the Symposium became an energized exploration of where to go now."

The article goes on to discuss the reasons for the failure of the received view, which we briefly summarize here. Positivists saw human knowledge as being encapsulated by sentences, or propositions. As per ideas of Wittgenstein, sentences were actually "pictures" of facts about the world, or logical consequences of such elementary propositions. Struggles to articulate this idea precisely failed upon detailed examination of the "correspondence rules" between sentences and events in the real world. It turned out that "correspondence rules were a heterogeneous confusion of meaning relationships, experimental design, measurement, and causal relationships some of which are not properly parts of theories". The conclusion is that scientific theory cannot be understood as a "linguistic entity" as per positivist precepts. So economic knowledge cannot be encapsulated in "propositions", as asserted by Robbins.

3.3 The positive-normative distinction

Despite the fact that philosophers have abandoned the positive-normative distinction ever since Quine's (1963) attack on the two dogmas of empiricism, economists continue to base methodological foundations for economics on this dichotomy. More recently, the fact/value demarcation has been successfully attacked and demolished; see Putnam (2002) for a detailed argument, or Zaman (Feb 19, 2020) for an elementary exposition. Nonetheless, most economists only read one methodological essay in their lives, and that is Friedman's (1953) essay. In accordance with dominant and popular views at that time, Friedman presents economics as a positive science, strongly differentiated from normative ideas. This essay continues to be cited approvingly in economics textbooks, in two contexts. One is to justify assumptions which are literally false. The second is to assert that the body of economic theory is "positive": that is, it is purely objective and factual, without any appeals to subjectivity and values. Few reflect on the direct contradiction between these two widely held beliefs about economics. In contemporary textbooks, most authors are aware that the positive/normative distinction is no longer sustainable and avoid direct discussion, or appeal. However, unguarded expressions can still be found. More importantly, the realization that a large number of normative values are hidden within apparently objective axiomatic frameworks does not exist in the folk-methodology of economics. Thus, the re-thinking required by this realization has never been done. This is despite the fact that many mainstream economists have pointed out how values are involved in every aspect of economic thinking. A particularly clear demonstration is available from the book length treatment in Hausman et al. (2016). Also, Zaman (2012B) shows how three different normative principles are involved in elevating scarcity to the fundamental economic problem. Folk-methodology in economics completely ignores these issues, and treats the positive normative distinction as unproblematic. Economists continue to believe that economic theory is free of values and that values do not belong in science; see for example Hands (2012) for evidence.

3.4 Canonical assumptions: optimization and equilibrium

Methodological writings of economists display allegiance to the Baconian view of science. It is asserted that our theories are derived from studying the world, and are "positive" – they are factual and objective descriptions of external reality, not tainted by normative ideals. In addition, it is asserted by Friedman, and repeated by countless followers, that the only valid test of theories lies in their ability to predict accurately. The implication is that if theories do not predict accurately then they should be rejected. The fact is that these methodological ideals are only for display. The actual practice pays no heed to these principles. The folk-methodology of modern economics is based solidly on two principles: optimization and equilibrium. As long as a model is built where all agents are maximizing some objective function, and we can calculate equilibrium outcomes, this model qualifies as a valid economic model. If a model does not obey these conditions, then it is defective. The assumptions of the model can be completely bizarre and outlandish, since Friedman (1953) provided us with a license to use such assumptions. Note that this means that we have abandoned the Baconian idea of deriving laws from observed patterns in the data. Furthermore, the predictions of the model need not have any correspondence with reality. This is despite methodological professions to the contrary. The model is judged purely by allegiance to the canonical assumptions of optimization and equilibrium. Keynes remarked that "Economists are unmoved by lack of correspondence between their theories and facts." The revolutionary insights of Keynes were rejected because they could not be fitted into an optimization and

equilibrium framework. Both the Samuelson-Hicks synthesis, and the more recent New Keynesian school of Macroeconomics reconcile Keynesian unemployment with the optimization and equilibrium framework by jettisoning essential aspects of Keynesian theory. This is what prompted Keynes to say that I am not a Keynesian.

More recently, Romer (2016) remarked on the lack of scientific attitude of economists: “Macroeconomic theorists ignore mere facts by feigning an obtuse ignorance.” For example, in an interview with Evans and Honkapohja (2005). Sargent stated that: “But after about five years of doing likelihood ratio tests on rational expectations models, I recall Bob Lucas and Ed Prescott both telling me that those tests were rejecting too many good models.” The response to conflicts with data was to stop doing tests, and resort to calibration, rather than modification of the models. It is obvious that the standards to assess “good models” are not based on predictive ability. Rather, they are based on conformity to certain theoretical predilections as well as aesthetic conventions, conveniently summarized as optimization and equilibrium. A further reason for the disconnect between models and data is the nature of economic models themselves, as we discuss next.

3.5 Substitute versus surrogate models.

Maki (2018) makes the useful distinction between substitute and surrogate models. The methodological statements by economists support the view that economic models are surrogate models. This means that such models are built as simplifications of, and approximations to, a complex reality. Thus, the results of the model are subject to the test of comparison with actual reality. If the degree of approximation between model results and reality is found wanting with respect to some target objective, models must be modified and improved. In contrast, substitute models are ends in themselves. They are subject to internal criteria for coherence, and consistency to a set of aesthetic principles built around the core of optimization and equilibrium. More explicitly, the methodology for construction of models is described explicitly by Lucas as follows:

“Unlike anthropologists, however, economists simply invent the primitive societies we study, a practice which frees us from limiting ourselves to societies which can be physically visited as sparing us the discomforts of long stays among savages. This method of society-invention is the source of the utopian character of economics; and of the mix of distrust and envy with which we are viewed by our fellow social scientists. The point of studying wholly fictional, rather than actual societies, is that it is relatively inexpensive to subject them to external forces of various types and observe the way they react. If, subjected to forces similar to those acting on actual societies, the artificial society reacts in a similar way, we gain confidence that there are useable connections between the invented society and the one we really care about.”

The distinction between economists and anthropologists is important here. Anthropologists study real societies while economists study artificial societies they make up. There is no target reality we are modeling. We can freely start an article by saying let us assume our society consists of a single consumer who lives infinitely long and produces and consumes only one good. In principle, as Lucas says, and as Friedman says, models with wildly inaccurate assumptions will be tested by their predictions. In fact, our textbooks and teachers never actually carry out such tests. We start with an artificial model, an invented primitive

society, deduce results, and never match them against reality. So, the last sentence in the Lucas quote above is false. No one actually checks to see if the artificial society reacts in a similar way to the actual society. In the terminology of Maki, economics models are substitute models – they take the place of reality, and the question of comparisons, or tests by predictions does not arise. Thus, methodological practice (usually unarticulated) of economics deviates from scientific methodology in two ways. One is that our models are not derived as attempts to understand, explain, or model some target complex reality – they are constructed as artificial societies which are substitutes for this complex reality. Secondly, the results obtained are not compared with any target reality to assess validity or adequacy of our models. Internal coherence and consistency. Given that models are not derived from a study of reality, and models results are not compared to reality, there is little wonder that Ronald Coase said: “Existing economics is a theoretical system which floats in the air and which bears little relation to what happens in the real world.”

Bergmann (2007) describes the disconnect between economic models and reality as follows. She explains how biologists spent thousands of hours studying dolphins to learn some principles of their behavior. In contrast, economic theories of firms are based entirely on mental considerations, without any study of firm behavior at all:

“The material about business behavior that students read about in economics textbooks, and almost all of the new theoretical material developed by mainstream professionals and published in the profession’s leading journals was composed by economists who sat down in some comfortable chair and... simply made it up.”

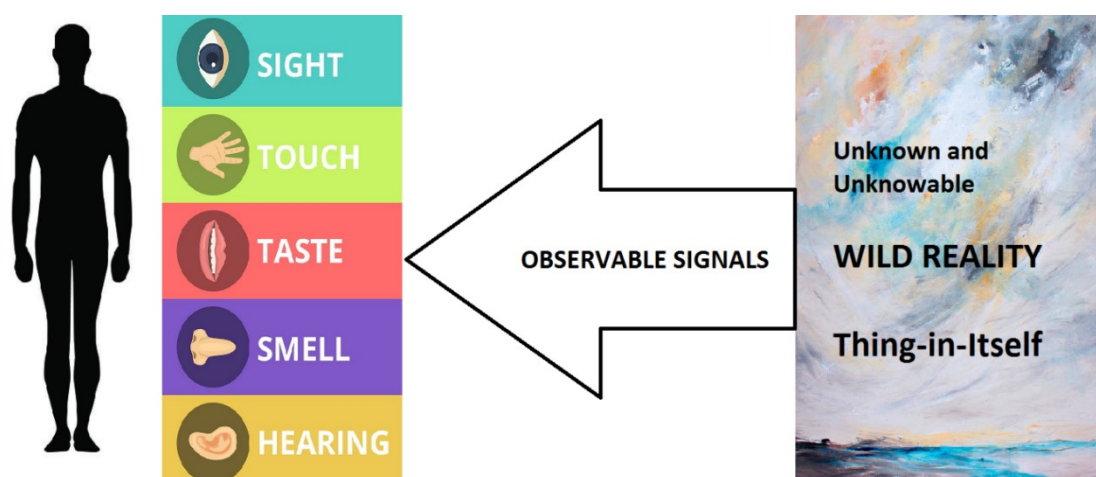
4. The empiricist philosophy and its errors

Manicas (1987) writes in the introduction that:

“Critical to this, in my view, is the critique of empiricism (chapter 13), not merely as an untenable philosophy of the human sciences, but as a philosophy of any science. It will be clear from part I, I hope, how developments in the eighteenth century and then in last two decades of the nineteenth century made the victory of ‘empiricism’ in the twentieth century such an easy one, even though, as in the philosophy of Helmholtz, there were ‘realist’ alternatives which fully acknowledged the Kantian (and Humean) critique of ‘metaphysics’. Critical in regard to an alternative realist human science is the context and program of Marx and Engels. I argue that a crucial failure in their philosophy, fully explainable, is the absence of a clear and adequate theory of science. On the other hand, I think that it is only very recent developments which have made a fully coherent ‘realist’ alternative plausible. The philosophy of social science offered in chapters 13 and 14, then, is an attempt to assimilate the recent debate, in both the philosophy of natural science and as regards the competing conceptions of the possibility of a human science, and to provide a comprehensive sketch of such an alternative. This account draws on a variety of recent and much better developed sources. Its goal is synoptic. It is, accordingly, but a sketch or perhaps a sketch of a sketch, meant to provoke and enlarge the recent debate in philosophy of the psychology and philosophy of the social sciences”

The quote shows that “empiricism” is at the heart of the failure in constructing a valid realist philosophy of science. An invalid empiricist philosophy was then applied to construct the social sciences afresh on empiricist foundations in the early 20th with disastrous results that persist today. The detailed book length treatment provided by Manicas (1987) is presented as a sketch of a sketch, meant to provoke debate. We now propose to provide a one-page summary of some critical ideas of empiricist philosophy for an audience of economists, which may perhaps be regarded as a cartoon version of the sketch. Because of the vital importance of the task, we proceed without further apology for crude over-simplifications of subtle and sophisticated philosophies.

Rejection of authority and tradition as sources of knowledge in the Enlightenment led to the rise of Empiricist philosophy. Among the early Empiricist philosophers, David Hume combined faith in observations (facts) with a skepticism about what could not be seen. This variety of empiricism holds that observations are all that we have. We cannot penetrate through the observations to the hidden reality which generates these observations. Here is a picture which illustrates the empiricist view of the world:

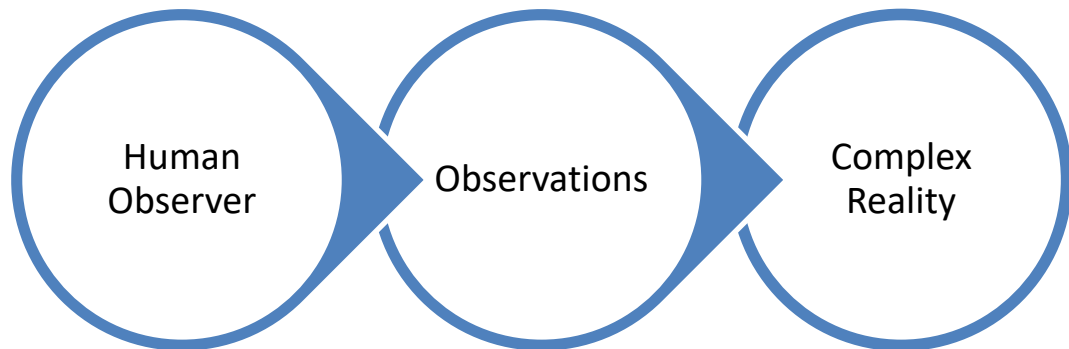


The wild and complex reality generates signals which we observe using our five senses. The aspects of reality which we can observe are the only things that we can know about reality. The true nature of hidden reality, as it really is, independent of our observations, is unknown and can never be known to us. A critical idea in the development of the theory of knowledge (epistemology) came from Immanuel Kant. Kant is a towering figure in the history of Western philosophy. His philosophy is too complex to be described in a few words, and he certainly was not an empiricist. However, he introduced some distinctions and dualisms which were extremely influential in later developments. Of importance to our arguments are the Noumena (which is the wild reality), and Phenomena (which is what we can perceive/observe about the reality). The Encyclopedia Britannica defines the terms as follows:

Noumenon, plural **Noumena**, in the [philosophy](#) of [Immanuel Kant](#), the [thing-in-itself](#) (*das Ding an sich*) as opposed to what Kant called the phenomenon—the thing as it appears to an observer. Though the noumenal holds the contents of the intelligible world, Kant claimed that man’s speculative reason can only know phenomena and can never penetrate to the noumenon.

Kant was enormously influential in de-railing the philosophy of science. Prior to Kant, philosophers understood science in the natural way: science is about looking through the

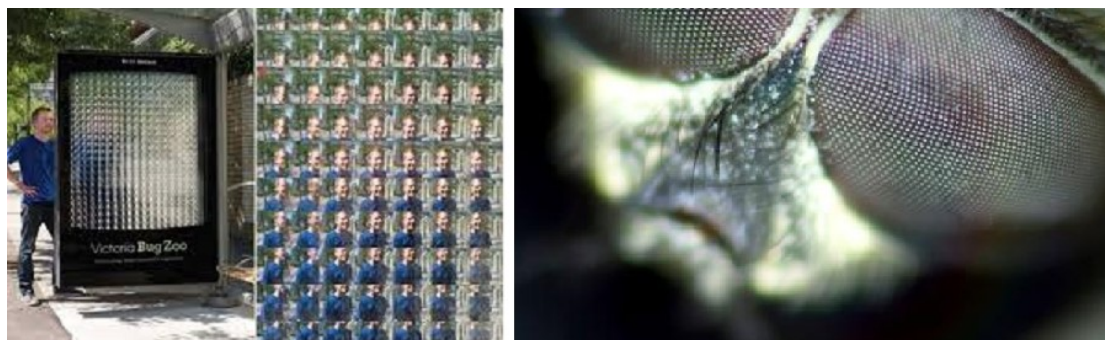
appearances in order to understand the hidden reality. However, Kant argued that this was an impossible task. All we have is appearances (phenomena), and we cannot look through them to get at the underlying hidden realities (noumena). He proposed that instead of studying the relation between appearances and reality, we should study the relationship between our thought process and the observations of the real world:



The Kantian separation between the models in our minds which explain the phenomena we observe, and the hidden reality was a fateful step towards the development of models completely de-linked from reality. To get a deeper understanding of Kant, we provide several arguments favoring his views. Think about how a simple computer camera looks at the world. The area being looked at by the camera is represented as a square two-dimensional patch which is say 1000 x 1000 pixels. At each pixel, if the camera detects light, it puts a 1 and if it does not, it puts a 0. So, we end up with a picture of reality which is a 1000 x 1000 matrix of 1's and 0's. This is the OBSERVATION. Now how can we translate these observations into a picture of reality? This is the basic problem of computer vision – taking a stream of numerical inputs from the camera and translating it into a picture of reality. For example, a particular stream of 1's and 0's may be interpreted as a picture of a tree, by a computer vision program. As human beings, we face a similar problem. We don't actually see the world out there. What we see is a reflection of the world within our eyes. Our minds process the image on our retina into a picture of the external reality. Before Kant, most people thought that the image in our minds matched the external reality. What Kant said was that we have no way of knowing this. We have no way of knowing the external reality. All we can see is the image of it on our retina, and the interpretation of it in our minds. A Kantian model, which we will label a mental model later, explains how we convert streams of 0's and 1's into an image of reality.

For understanding the nature of models, we will need to keep these three things in mind. Reality generates observations. And our minds interpret observations as a picture of reality. Most of us think that the picture in our minds is exactly what the reality is. When I look at a tree, I do not say that my mind has interpreted an image on my retina as a tree. I say that there is a tree out there in external reality which I am seeing. However this is an over-simplified understanding. For example, when I see a mirage, I interpret the image on my

retina as water, but in fact there is no water in external reality. Similarly, a fly has a compound eye, and sees the world in way which is very different from how we see it.



As opposed to Kant, traditional philosophy is concerned with the question of how the image we have formed in our mind (by interpreting the observations) relates to external reality (not to the bitstream of observations). Traditional philosophy would ask: which is the “correct” picture of external reality? What the fly sees or what we see? What Kant says is that there is no way to learn the answer to this question. We have no separate access to external reality apart from our observations. So instead of thinking about whether our mental pictures match true reality, we should think about how we process the stream of sensations we receive into an image (a model) of the world. Favoring Kant, evolutionary biologists argue that the picture that we see of the world tends to highlight those aspects which matter for our survival, and ignore or neglect those aspects which don't. This means that the representation of reality that is captured by our senses has less to do with the true external reality, and more to do with our own survival. The point of all this is that the naïve idea that what we see is just a true picture of reality is not necessarily correct.

This idea of Kant, that we can and should abandon looking for truth – the true picture of reality – has had a powerful effect on the philosophy of science today. Especially in economics, models that we build have no relation to reality. Rather the models in use are ways of organizing our own thoughts about reality. [Robert Aumann](#) expresses exactly this view about the nature of economic models:

“In my view, scientific theories are not to be considered ‘true’ or ‘false.’ In constructing such a theory, we are not trying to get at the truth, or even to approximate to it: rather, we are trying to organize our thoughts and observations in a useful manner.”

Since we do not even try to get at the truth, there is no surprise if our models are hopelessly bad at approximating reality. Furthermore, the IDEA that we do not need to try to match reality, has led to the impossibility of correcting bad models to make them better. All that happens is that bad models are replaced by more complex models which are even worse.

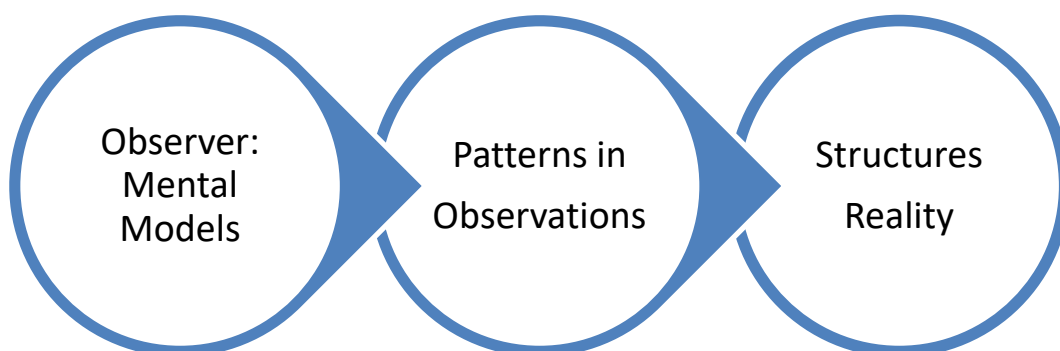
5. Three types of models

When presenting the history of thought over the course of centuries, broad patterns and trends can only be seen if we allow the details to go somewhat out of focus. It is this kind of oversimplification that we undertake, in order to arrive at a simple trichotomy of models, which

is not fully supported by a concrete and detailed examination of specific models, which often operate on more than one level. Nonetheless, this coarse-grained approach is helpful in understanding the answer to our main question: how did models divorced from reality become epistemologically acceptable?

In understanding the answer, it is important to start with the basic premise of Manicas (1987) that even though classical empiricism is not tenable as a philosophy of any science, some its central concepts became the main drivers of the methodology of modern economics. To understand this clearly, it is helpful to contrast empirical modes of understanding with realist modes. This section is based on the Introduction to Bhaskar (2008) “A Realist Theory of Science”. In particular, we aim to explain Figure 0.1 The Logic of Scientific Discovery, which diagrammatically distinguishes between (1) classical empiricism, (2) transcendental idealism, and (3) realism. We transform this diagram into three types of models for greater clarity.

For the purposes of our discussion, we can classify models into three different types, corresponding to the following diagram. The simplest type of model is a pattern in the data that we observe. A second type of model is a “mental model”. This is a structure we create in our own minds, in order to understand the patterns that we see in the observations. The third type of model is a structure of the hidden real world, which generates the patterns that we see. Some examples will be helpful in clarifying these ideas about the typology of models.



Empirical Models: The simplest kind of model consists of a pattern that we see in the observations. For example, if we see the sun rise every day for many years, this is a pattern in our experience. It leads us to conjecture the law that “the sun rises every day” – where the law extends beyond the range of our experience and observations. This is just a guess, based on patterns we see in the data. A regression model is an excellent example of an empirical model. It identifies patterns in the data, without any concern for the underlying realities. For example, a regression of Australian consumption per capita on China’s GDP gives an excellent fit –

Australian Consumption = $a + b$ Chinese GDP per capita + error (high R-squared, significant t-stats)

This shows us that there is a pattern in the data – increases in China's GDP go along with increases in Australian consumption. The regression cannot answer the question of why there is this pattern. Any two series of data can display correlation – time series measuring numbers of sunspots sighted on the sun's surface can correlate with a wide variety of economic phenomena. The regression model which picks up this relationship has nothing to say about the reasons for the correlation. Given any kind of data, we can always find some regression relationship. Zaman (2012) in "Methodological Mistakes and Econometric Consequences" presents many examples of strong regression patterns which are meaningless; for example:

Pakistani Consumption = a + b Survival rate to age 65 of Females + c Pollution Levels by Carbon Monoxide + error

In terms of classification – we can find many different kinds of patterns in any arbitrary set of data. Whether or not the patterns have meaning depend on the real-world processes which generate these patterns. This is something which Real Models are meant to explore.

Real (Structural) Models: The empirical models look at the surface structure, the appearances, the data that is based on observations. Structural models try to explore the hidden structure underneath the appearances. Consider for example a regression of consumption per capita on GNP per capita

$$C = a + b Y + \text{epsilon}$$

From the point of view of an empirical model, this is a pattern in the data. The names of the variables do not matter. If the consumption is Australian and the GNP is Chinese, the pattern is the same as if both variables belong to the same country. The names of the variable, and the relationships between them, matter only when we think about real structural models. For example, if we think that consumers earn incomes, and then spend some proportion of the income on consumer goods, this is a real structural relationship which explains why we see the pattern in the regression relationship. This structure justifies regressing Australian consumption on Australian GDP, but not on Chinese GDP. Also, if the determinants of GDP are the production processes, while consumption is determined by incomes, we cannot reverse the variables and run a regression of GDP on Consumption. Consumption is not a determinant of GDP. For an empirical model, C on Y and Y on C are the same patterns. Correlations are symmetric, but causal relationship are one directional. Real Structural Models attempt to find hidden real variables which cause the patterns that we see. For example, the tendency of consumers to consume a proportion of their income is the hidden cause for the surface data relationship between consumption and income within a country.

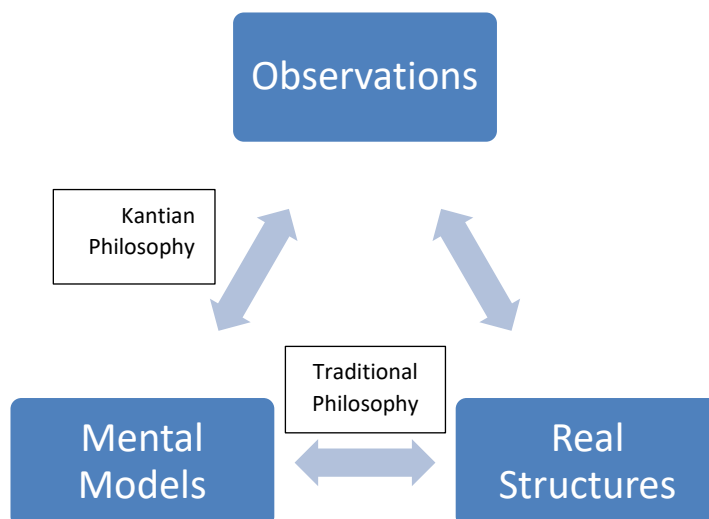
Mental Models: A pattern in the data is just a pattern – there is no explanation for it. This is the Baconian model of science. If we see a pattern in the data, we deduce that a law holds which generates this model. Any pattern that we see could be a law. A mental model imagines a structure of reality which could be an explanation for the reality. For example, an aggregate consumption function can arise from individual consumers who optimize utility derived from consumption bundles subject to budget constraints. It could also arise from consumers who make completely random consumption decisions, while staying within their budget. Any imaginary structure of reality which leads to observations which match what is actually observed, is a mental model.

Originally, mental models were designed by thinking about what the nature of hidden reality could be, and then trying to build a mental model to match that hidden structure. However, post-Kant, the main idea became different. Trying to match hidden reality was abandoned, and instead, the goal of the model building became to create a match to the observations. As a result, many concepts which are of vital importance to modelling reality were abandoned or misunderstood. For example, the idea of causation is of great importance in understanding reality. Rubbing a match against sulfur on the matchbox causes the match to burn. Learning about causation is of extreme importance in learning to navigate the world we live in. Our mental models are supposed to be representations of reality. For complicated historical reasons, economists FORGOT this basic idea about the nature of mental models, that they are supposed to capture the hidden real mechanisms which generate the observations. This has been an empiricist tendency starting from Hume. The idea that we cannot talk about hidden unknown realities has deep roots in Western intellectual rejection of God and religion; this angle is explored further in Zaman (2015).

As already discussed, Kant suggested that we can create a Copernican revolution in philosophy by changing the focus of our inquiry into the world. Philosophers have thought for ages about the problem of how we can find out if our mental models match the reality, the hidden unknown structures. But this is the WRONG question (according to Kant and the empiricists). We can never find out the answer, because the true hidden structures of reality will NEVER be observable. So, we should abandon this ancient question. Instead, we should focus on the question of how our mind organized the observations into a coherent picture of apparent reality (precisely as Aumann quoted earlier states). The diagram below shows the Kantian shift of focus. Traditional philosophy is concerned with the question of whether or not our mental models MATCH the hidden structures of the real world. This is the question of whether or not our models are TRUE. Kant and the empiricists said that this was impossible to know. We should only be concerned about whether or not our empirical models provide a good fit for the observations. So, the question itself was changed. Instead of asking if models match reality (and hence, whether or not they are TRUE), we ask whether the output of the models provides a match to the observations. This shift in concern about how models should be evaluated also encapsulates Friedman's methodological concern that models should predict well, and need have no match to reality.

The following diagram explains the current Empiricist views about models and reality. All that matters about mental models is that they should provide a match to the observations. It does not matter whether or not they match the true structures of reality which produce the observations.

Due to the influence of Friedman's methodological essay – described in Maki (2009 B) – this view has been widely accepted in Economics, and the methodology of modern Economics and Econometrics is based on this wrong idea about the nature of models. We now discuss this issue in the context of economics.



6. Economic methodology is empiricist

In the previous section, we discussed three types of models. *The first type* is based purely on patterns in observations, and does not attempt to go beyond what can be seen. This is an “observational” or Baconian model. *The second type* attempts to look through the surface and discover the hidden structures of reality which generate the observations we see. As Manicas (1987) remarks, it is only recently that a stable misconception of science, which persisted over centuries, has broken down. This has made it possible to develop realist philosophies of science. The previous section is based on Bhaskar (2008), but see also Manicas (2006) for another approach to a realist philosophy of science. *The third type* of model creates depth and structures in our minds which create the patterns we see in the observations. These may be called Kantian, or mental models. The Kantian perspective, reinforced by Max Weber’s ideas about use of scientific methodology in social sciences, has been widely accepted in Economics. The methodology of modern Economics is largely Kantian (and Weberian), while Econometric models are largely Baconian. The key defect of both of these approaches is that they GIVE UP on the idea of finding the truth. We now discuss this issue in greater detail.

The deep and abiding influence of Milton Friedman (1953) on methodology in economic theory has been discussed at book length in Maki (2009). For our rough sketch, the main point we wish to extract is that Friedman recommends the abandonment of the search for truth: “Truly important and significant hypotheses will be found to have ‘assumptions’ that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions.” What Friedman expresses, in ambiguous and inconsistent language, is the idea that an assumed structure of reality which is a mental model designed to match observations, need not match the true hidden structures of reality. All that matters is that observable implications of the model match our observed data. This idea is called “saving the appearances”. For example, if we imagine that there is a heavenly sphere surrounding the earth and the moon is pasted on that sphere. Motions of the moon occur because of the rotations of the sphere. According the idea of “saving appearances”, as long as the observed motion of the moon matches the predictions of our model, we need not be concerned with whether or not the heavenly sphere actually exists.

This is the fundamental methodological mistake at the heart of economics: the idea that we can make up any crazy model we like. As long as our models produce a match to the observations, it does not matter if we make wildly inaccurate assumptions. This has led to DSGE models, currently the dominant macroeconomic models, which have been called crazy by many authors. Economists make completely unrealistic assumptions without any discomfort, because of Friedman's idea that "wildly inaccurate" assumptions will lead to truly important and significant hypotheses. In a previous portion of this article, we documented the fact that economists are not bothered by conflicts between their models and reality. Below we provide quotes which document the crazy models that now dominate economics because of adherence to Friedman's Folly: the crazier the assumptions, the better the model.

Solow: Suppose someone sits down where you are sitting right now and announces to me that he is Napoleon Bonaparte. The last thing I want to do with him is to get involved in a technical discussion of cavalry tactics at the battle of Austerlitz. If I do that, I'm getting tacitly drawn into the game that he is Napoleon. Now, Bob Lucas and Tom Sargent like nothing better than to get drawn into technical discussions, because then you have tacitly gone along with their fundamental assumptions; your attention is attracted away from the basic weakness of the whole story. Since I find that fundamental framework ludicrous, I respond by treating it as ludicrous – that is, by laughing at it – so as not to fall into the trap of taking it seriously and passing on to matters of technique.

Narayana Kocherlakota: Minneapolis Federal Reserve President (2010-2015), "[Toy Models](#)", **July 14 2016** "The starting premise for serious models is that there is a well-established body of macroeconomic theory... My own view is that, after the highly surprising nature of the data flow over the past ten years, this basic premise of "serious" modeling is wrong: we simply do not have a settled successful theory of the macroeconomy."

Olivier Blanchard IMF Chief Economist (2010-2015), "Do DSGE Models Have a Future?", August 2016 "DSGE models have come to play a dominant role in macroeconomic research. Some see them as the sign that macroeconomics has become a mature science, organized around a microfounded common core. *Others see them as a dangerous dead end...*" and "There are many reasons to dislike current DSGE models. First: They are based on unappealing assumptions. Not just simplifying assumptions, as any model must, but *assumptions profoundly at odds with what we know about consumers and firms.*"

All of these authors are expressing the same complaint, in different forms. Mental Models are not Real Models. The only job mental models have to do is to produce a match to the observed data. Whether or not mental models are realistic has no bearing on whether or not they are good models. There is complete lack of concern about whether our mental models make assumptions which are realistic. Of central importance to the concerns of this paper is the weakness of Solow's attack on Lucas and Sargent's assumptions. After describing how these assumptions miss essential aspects of the economic system, he suggests that we dismiss them because they do not pass the "smell-test"! He cannot attack their models for making wildly unrealistic assumptions, and for massive predictive failure, because his own widely popular and universally taught Solow growth model has the same defects! No wonder defenders of the DSGE models simply respond to Solow by saying that these models smell good to them!

The mystery of how models based on false assumptions can help us "understand" and "explain" the real world has been the subject of a long and complex methodological debate.

For example, a leading methodologist, Mary Morgan (2012) writes that “Despite the ubiquity of modelling in modern economics, it is not easy to say how this way of doing science works. Scientific models are not self-evident things, and it is not obvious how such research objects are made, nor how a scientist reasons with them, nor to what purpose.” In the “Explanation Paradox”, Julian Reiss (2012) writes that it is widely accepted that: (1) economic models are false; (2) economic models are nevertheless explanatory; and (3) only true accounts explain. A whole subsequent issue of the *Journal of Economic Methodology* is devoted to the attempt to EXPLAIN how all THREE of Reiss’ premises can be true. Alexandrova and Northcott (2013) – philosopher-outsiders – point out the obvious: economic models do not explain. However, this simple explanation falls on deaf ears; economists are too much addicted to meaningless mathematical models to realize that these models are mental structures which are “hanging in the air, having no contact with reality”.

7. Baconian science versus real structures

As we have discussed, models used in econometrics and economics can be classified into three broad categories. Because the methodology is never discussed explicitly, these distinctions are never discussed, resulting in extreme confusion. Below we discuss the difference between Baconian models of patterns in observations versus real structural models, as a preliminary step to discussing economic and econometric models.

The most primitive Baconian understanding of science is that science looks for patterns in observations. These patterns are the goal of scientific endeavor. When we find a pattern, that pattern is a potential scientific law. We can test the law by making predictions based on this law. If the prediction holds true, that means the pattern extends beyond the data that we see. It is a confirmation that there is an underlying law which generates the pattern. There are three basic principles of Baconian science

1. A scientific law is a pattern in the data, revealed by regression methods.
2. We can test scientific laws by prediction and forecasting – if the pattern holds beyond the observed range of data, then it is valid.
3. To “explain” an observation means making it part of a pattern; that is, observations which fit a regression model are explained by the regression.

The deep problems that emerge from taking models as just patterns in the data are explained in Zaman (2012) “Methodological Mistakes and Econometric Consequences”. Today, students of econometrics run regressions of anything on anything else, and make conclusions on the basis of the patterns shown by the data. It is a mistake to take an observed pattern as the scientific law. What we need is the underlying mechanism, often hidden, which explains the patterns we see. A REAL philosophy of science is based on REAL models. Real models postulate the existence of (hidden) entities and effects which cause the patterns that we see in the observations. For example, we see apples fall to the ground, and we postulate that there is a (invisible) force of gravity which the earth exerts on the apple to cause it to fall. This actually follows from a previous law of inertia which says that objects at rest will stay at rest unless a force acts on them. In a similar way, when we see consumers purchasing items using money earned by working, we assume that they have a (internal, unobservable) preference for the items they purchase, so they are prepared to work in order to get these objects. As opposed to the idea of Empirical or Observational Models, Real Models are characterized as follows:

Real structural models

1. Real Models postulate the existence of real objects (observable or not) with real properties, which create the patterns we see in the data. Scientific hypotheses concern these objects and effects, which describe the structure of hidden reality.
2. Often, hypotheses cannot be tested directly. We look for indirect ways of testing hypotheses regarding the existence of unobservable objects and effects. For example, gravity explains falling apples. We look for effects of gravity in other places, like tides, and planetary orbits. If one hypothesis explains a lot of different phenomena, it serves as an indirect confirmation.
3. Explanation is causal explanation. We say that what we observe is caused by hidden real objects and effects. The hidden preferences of the consumer lead him to choose object X over object Y.

A simple way to understand the difference between Baconian models and Real Structural models is to think about models of consumer behavior. A real model is based on hypotheses (which may or may not be correct) about motivations for human behavior. For example, microeconomic theory assumes that human beings have utility functions: possession and utilization of consumer goods in different combinations gives them pleasure of varying degrees. This is a hidden structure, not observable. Based on this utility function, and on the budget constraint, consumers make their purchase decisions. So the observed pattern of consumer choices depends on the hidden structure of preferences and intensity of pleasure that we feel due to the consumption of commodities. An empirical model (or observational, Baconian model) cannot invoke hidden structures of reality. So, we are confined to look only at choices. We can observe choices, and impose certain rationality conditions such as the axioms of revealed preference. These axioms refer only to observed patterns of choices, and not to the underlying hidden preferences. There are patterns that we see in observed consumer choices, and these patterns ARE the scientific laws. It is not that these patterns can be explained with reference to the underlying hidden real structures.

8. Econometric models: structural versus reduced form

In this section, we briefly consider the methodology of econometrics, which is based on Baconian or observational models. That is, econometric models tend to look only at what is available on the surface, as measured by observations, without attempting to discover the underlying reality which generates these observations.

The Structural Simultaneous Equations Models (SSEM) developed at the Cowles Foundation in mid-20th century, took causality seriously. The causal structures were derived from economic theories. Theory specified which variables were exogenous, and which variables were determinants of the endogenous variables. Equations were developed for the endogenous variables which represented the structure of the economy. To be more precise, regression equations were CAUSAL equations. Once this is understood, it is clear why $C = a + b Y$ (read as: Y is a CAUSE of C) cannot be inverted to $Y = (1/b) C - (a/b)$. The history of how this knowledge was lost, and how the causal structures implicit in the early days of SSEM were forgotten and abandoned, has been summarized in Chapter 5 of Pearl (2000). One of the key elements, also highlighted by Hoover (2004), is the lack of a mathematical language for expressing causality. Causal information was present, and used correctly, but never explicitly written in the equations. In particular, the “equal (=)” sign has

been held responsible for the damage. To better understand this, let us introduce the notation of $\Leftarrow$ or $\Rightarrow$ as a causal arrow, together with equality. Now the consumption equation could be written as: $C \Leftarrow a + bY + \text{Error}$. All variables on the right hand side of a causal equation must be causal determinants of the left hand side. A causal equation is very different from a standard regression equation, and cannot be manipulated algebraically in the same way. It may be useful to highlight the causal factors by putting them in square brackets: $C \Leftarrow a + b[Y] + [\text{Error}]$. Now the terms in square brackets are causal determinants of C . This equation cannot be re-written as $C \Leftarrow a + [bY+u] + [\text{Error} - u]$ even though the two expressions on the RHS are algebraically equivalent. This is because $bY+u$ would not be a causal determinant of C . Similarly, the status of the error term is clarified by putting it into square brackets. This is now a meaningful term, a causal factor, which means it must correspond to something which exists in the real world. Generally, people have a tendency to consume about $a+bY$ from their incomes. Discrepancies from this level are CAUSED by other factors. The cumulative effect of all these ignored factors is measured by the error term. The error term is not a figment of imagination of the modeler, but a physical and measurable real-world variable, even though it is not directly observable because we are uncertain about the function $f(Y)$ which determines the consumption. In terms of the classifications given in Three Types of Models, Econometric Models can be classified as follows:

1. **Real Models:** These provide explicit models of the underlying, unobservable causal structures which generate the data. The original SSEM did aspire to this ideal, where the causal information was understood, and partially stated in the models, in the form of exogeneity, endogeneity, structural equations with inclusion and exclusion restrictions. However, the algebraic forms of equations did not explicitly and mathematically recognize the qualitative causal information. Partially because of this, but also due to other factors, causality was removed from the picture in interpretations of regression models. Present day Structural Equations Models are capable of carrying causal information, but are not used or interpreted in this way.
2. **Observational Models:** These are models which deal purely with observable probabilistic structures given in the data. The “Data-Generating Process” contains the probability information, but not the causal information. Sim’s VAR models as originally formulated, are a perfect example of purely observational model. Sims discovered, to his chagrin, that even the simplest use of such models required input of causal information, which must be assumed, as it is not directly observable from statistical data distributions. This forced a reluctant move from VARS to SVARS, which incorporate minimal causal information in an ad-hoc and casual way. How causality enters data analysis is discussed in some detail in an elementary exposition of the Simpson’s Paradox, given in Zaman and Salahuddin (2020).
3. **Mental Models:** Because it is impossible to interpret data without causal assumptions, all regression models make causal assumptions. However, these assumptions are implicit, arbitrary, and unrecognized, because they are not part of the explicit modeling process. As a result, models often carry absurd causal implications without any explicit recognition of this fact. A more detailed explanation is available in Zaman (2010)

To understand how explicit consideration of causal information affects our analysis of economic theories and corresponding regression models, we discuss some simple examples. Simple regression models ($Y=a+bX+\text{error}$) between any two variables X and Y are examples of Baconian models; these are purely relationship between data points, which do not refer to the real world processes which produced X and Y . These models are often called reduced

form models, because the underlying structures of external reality are reduced to the form in which they affect the observations. From the reduced form perspective, it does not matter if Y is the consumption of Australia, and X is the GNP of China, because only the data, and not its real world interpretation matters. Similarly, the pattern in the data is reflected equally in regressions of Y on X and of X on Y. It is only when we consider the structure of the real world that we can differentiate between the following three regressions:

1. Consumption of Australia regressed on GDP of Australia
2. GDP of Australia regressed on Consumption of Australia
3. Consumption of Australia regressed on GDP of China

The first regression is in structural form because it takes GDP as exogenous and C as being determined by GDP. This comes from knowledge of the real world which is not contained in the data. The second regression reversed the exogenous and endogenous variables. As reduced form equations, the two are equivalent, but as structural equations, the second is wrong. The mistake about exogeneity and endogeneity cannot be detected directly from the observed data. The third equation makes no sense from a structural point of view. In the external real world, we do not expect the GDP of China to have any strong causal effect on the consumption in Australia. However, when considered purely as a “reduced form” pattern in the data, the third equation turns out to be a very strong relationship; see Zaman (2010) for more details. As Baconian models, and as reduced forms, all three equations are equally acceptable. As structural form models, only the first one is acceptable.

Prior to the 1970s, econometric models were constructed as “structural” models. That is, the relationships among the observables were constrained by what was known about the hidden structures of external reality. For example, a consumption function related consumption to income and other determinants of consumer behavior. Consumption functions included variables known to affect consumption decisions, and excluded variables which were not relevant to consumption decisions. These were known as inclusion and exclusion restrictions in structural models. In addition, consumption was regressed in income, and not the other way around, because income is a determinant of consumption, and not the reverse. Similarly, the level of investment was taken to be a function of the interest rate, because it was thought that investors borrow money in order to make investments.

It is very important to understand that structural form of conventional econometric models is NOT what we have previously called REAL structural form models. The difference is that real structural models consider the HIDDEN structures of reality. For example, suppose there is a hidden variable E which is the expectations of the consumer about the future, which can be pessimistic or optimistic. Suppose this variable plays an important role in consumer decisions. A real structural model would take into account this hidden variable. Econometric structural models only look at the relationships between the observables. Suppose that C is affected by E, and expectations E are formed by looking at some variables V which reflect the state of the economy. Then econometric structural models will remove the unobserved E from the picture by linking consumptions to V. As opposed to this, recently introduced SEM (Structural Equation Models) techniques allow us to put unobservable factors into our models. These SEMs are very close to real structural models, in our terminology. The only difference is that loss of understanding about causality has led to loss of the causal interpretation these models, which has severely handicapped uses and functions of the SEM methodology; see Pearl (2008) for historical details.

In the 1970s OPEC placed an embargo on oil exports to Western countries which had supported Israel during the Yom-Kippur war. This generated huge shocks to the economies as oil prices doubled in a few months. As a result, nearly all econometric models failed badly to predict the consequences. These models may be roughly classified as “real models” based on causal information encapsulated via the informal Cowles Commission methodology. Predictive failure of these models led to two sets of critiques of econometric models, which went in opposite directions. One was the Lucas critique which led to the development of mental models, uncorrelated with reality. These eventually turned into the DSGE models which are the basic for macro policy today, even though they make assumptions wildly inconsistent with known realities. The other was the Sims critique, which led to abandonment of real causal structures, and a retreat to the surface patterns of observations as in empirical and Baconian models. We discuss this further in somewhat greater detail.

8.1 Sims critique: atheoretical VAR models.

Sims thought that the structural assumptions which were reflected in econometric methodology of inclusions and exclusions, and endogeneity and exogeneity was the source of the failure. Structural models of the consumption function include GNP but exclude fertilizer prices because we believe that consumption decisions are strongly affected by the former but not by the latter. Instead, Sims argued that we should include ALL variables, because we did not know the hidden structures of the economy. Also, structural models take consumption as endogenous, because it is determined by GNP, but they take Investment as exogenous. Keynes argued that investors make decisions about how much to invest based on expectations about the future which are not anchored to any real variables (animal spirits, recently re-labelled as irrational exuberance). Sims argued that these decisions about endogeneity of consumption and exogeneity of investment reflected inaccurate knowledge of the hidden underlying structures of the economy, and should be dropped. By dropping all structural restrictions, we come to a purely observational and empirical model, which only reflects patterns in the data, without any concern for the underlying economic structures. To illustrate the nature and consequences of VAR models, consider the following model, taken from an actual M.Phil. thesis of an economics student. Over a period $t=1,2,\dots,T$ we collect data on four variables, with the goal of assessing the level of cotton productions. The true relationship would be nonlinear, but we can ignore this complication for our present discussion:

$W(t)$ = Fertilizer Used at time t , $X(t)$ =Rainfall at time t , $Y(t)$ =Acreage devoted to Cotton Production, $Z(t)$ =Annual Cotton Production,

An econometric structural model would explain $Z(t)$ as a function of $W(t)$, $X(t)$, $Y(t)$, on the basis of our knowledge that the quantity of cotton produced would be related to the inputs used to produce it via the production function. A real structural model would dig deeper into the real processes by which cotton is produced. Rainfall effects would depend on the season in which the rainfall occurs, and the fertilizer efficiency would depend on many factors, observable and others. Structural models come from our knowledge of external reality. In contrast, Sims argues that since our structural knowledge is not reliable, we should use all variables as regressors for all variables. To be fair to Sims, he means to use this as a method for exploratory data analysis, as a preliminary step. However, even in this regard, examining purely data-based relationships will tend to highlight and pick up spurious relationships. For example, the VAR model that Sims methodology suggests here has four equations. Each variable is explained by lagged values of all the four regressors. For example, Fertilizer used

at time t ($W(t)$) is a function of $W(t-1)$, $X(t-1)$, $Y(t-1)$, $Z(t-1)$. Similarly, Rainfall at time t would depend on the amount acreage devoted to cotton in the last period, as well as $W(t-1)$, $Z(t-1)$. These equations make no sense, because they ignore basic realities about the world we live in. Any numbers produced by these atheoretical VAR models are pure noise, random correlations between data which have no real causal mechanisms behind them.

An interesting note on VAR models is that there is nothing you can do with them. The main use of VAR models is to calculate impulse responses. That is, if you give a shock to one of the variables, how will the system respond. This is a causal question – if we make a change in amount of fertilizer applied, how will the other variables respond across time. It turns out that a purely a-theoretical approach is incapable of answering this question. This was not realized initially by Sims. To get answers to causal questions, we must put in assumptions about causal sequencing. Without any knowledge of how the four variables in the system are linked causally, we cannot calculate the impulse responses. In the four variable VAR system described above, we would find that increasing fertilizer can result in increased rainfall for the next few years, if we make arbitrary assumptions about causal sequencing. To get (barely) sensible results out of the VAR system, we must specify that rainfall, fertilizer, and acreage are exogenous and affect production, while cotton production does not causally effect the other three variables. These causal sequencing relationships come from our knowledge of external reality, and cannot be obtained from the data. When the causal sequencing is added to the VAR model, it is called a structural VAR model. This basically defeats the purpose of creation of the VAR model, which was to avoid “arbitrary” causal assumptions, and inclusion/exclusion assumptions. The problem is that the observations do not reveal the real structures which generate them. Calculations of impulse responses requires knowledge of this structure.

We have now discussed two types of econometric models based purely on patterns in observations. One of the types is the VAR models of Sims. These are basic Baconian models. VAR models only look at the patterns in the data, and do not incorporate any information about external reality. We have seen that this leads to absurd equations, which allow for the impact of fertilizer input on rainfall several years later. In contrast, the original regression models developed at the Cowles Commission (SSEM) incorporate information on underlying unobservable causal relationships. The SSEM models have the following three properties. The first property is a concession to the empiricist philosophy and overlaps with VAR methodology, while the second two show how real world unobservable information is reflected in the SSEM:

1. Cowles Commission SSEM only model relationships between observable variables. However, unlike VAR models, structural equations are meant to be causal, reflecting true relationships between real world factors.
2. SSEM incorporate information about exogeneity and endogeneity which comes from knowledge about real world.
3. SSEM also incorporate inclusion and exclusion restrictions, which come from knowing about the causal relationships regarding which variables directly impact causally on others, and which do not have such causal effects. For example, we can exclude levels of fertilizer from the determinants of rainfall.

More recently, structural equations models (SEM) have been developed which improve on the SSEM by allowing unobservable factors to enter models, thereby creating the “real models” of our trichotomy. These have now come into vogue in nearly all social science areas except for

econometrics and economics. These are very different from the SSEM because when we introduce unobservables into our models, then all estimation procedures must be changed. Even though these models represent real models, failure to interpret them as causal models within econometrics has led to limited use and considerable confusion about their meaning. Pearl (2000) provides some details about how statisticians inflicted causal blindness upon themselves.

The incorporation of unobservable variables and unobservable causal relations is both a weakness and a strength. It is a strength because it captures hidden structures of reality which are essential for understanding how the world works. It is a weakness because one can never arrive at certainty regarding these unobservables. Conjectures about unobservable variables and causal effects can receive confirmation from data, but can never be verified as being “facts” in the same sense that surface observations can be verified. As shown in Zaman and Salahuddin’s (2020) discussion of the Simpson’s Paradox, discovery of deeper real structures can always upset and reverse causal relationships which appear to be strongly confirmed by the data. It was this weakness which was attacked by Sims when he proposed dropping all unverifiable assumptions about unobservables to construct VAR models. The Lucas critique went in the opposite direction. Lucas attributed predictive failure of econometric models to their failure to consider the deeper structural relationships which drove the economy. Since these deeper relationships were unobservable, it was necessary to “imagine” them to create better models. While VAR models abandon reality by eliminating it, the mental models of Lucas abandon reality by substituting alternative realities, creating “post-real” models in the terminology of Romer (2016). This is discussed next.

8.2 The Lucas Critique and mental models

Lucas argued, correctly, that the hidden underlying deep structures of the economy were not captured by regression models based purely on observations. He argued that we could improve regression models, and prevent forecast failures, by capturing this hidden structure. However, the hidden structure he incorporated was not based on analysis and study of the external real world. Rather, he developed mental models in line with conventional economic methodology, which has been described in greater detail in Section 2 of this paper. Referring to DSGE models based on Lucas’ approach, Solow (cited in Zaman 2018) writes that “A thoughtful person, faced with the thought that economic policy was being pursued on this basis, might reasonably wonder what planet he or she is on.” Actually, Lucas has provided a detailed description of the planet on which mental models of economists are based. As described in his quote in section 2, this planet is populated solely by rugged individualist *homo economicus*, who cares only for consumption and nothing for social relationships, and calculates and maximizes his personal benefits to the last penny.

The problem does not lie with the bizarre assumptions, but with the failure to cross-check results with reality. Empirical evidence that individuals do not maximize utilities should lead to modifications of the models. Overwhelming evidence against utility maximization is gathered in a survey by Zaman and Karacuka (2012C). In light of this evidence, it is necessary to revisit the artificial planet of the *homo economicus*, and repopulate it with humans who have more complex motives. However, economists simply ignore this evidence.

This process of cross-check and correction does not happen with models of economic theory. As Maki (2018) has observed, economists produce *substitute models*. Even though methodological texts contain claims that model outcomes are cross checked against reality,

this does not take place in practice. Instead of judging models by match to reality, models are judged on the basis of their conformity to aesthetic criteria, among which optimization and equilibrium are the most important. As the Sargent interview quoted in Section 2 shows, Lucas and associates responded to too many rejections of “good models” by the data, by stopping testing of the models. This is the key characteristic of mental models – they are never cross-checked against reality. This paper is motivated by the historical puzzle this poses. How did a methodology emerge which ignores this simple, basic, and common-sense requirement for good modeling?

9. Concluding remarks

Friedman’s (1953) defense of bizarre assumptions is actually valid within a correct scientific methodology. If we think that the main driver of economic behavior is utility maximization, it would be a good first step to construct a model based on these assumptions. We are now abstracting from complexity of human motivations. We want to see how much mileage we can get out this simplification. If we cross-check with actual reality and find this model adequate, then we have made a marvelous discovery – we do not lose much realism, but we gain substantially in understanding, by a massive simplification. On the other hand, if we find significant shortcomings in the match between model outcomes and reality, then we can introduce further complexity into the model to improve the match.

As we have seen, economists do not follow the Feynman principle that: “**It doesn’t matter** how beautiful your theory is, **it doesn’t matter** how smart you are. If **it doesn’t** agree with experiment, it’s wrong”. This paper is motivated by the desire to understand how this is possible? How did a methodology emerge which claims to be scientific and yet allows one to ignore conflicts between outcomes of theory and the data? The solution to this puzzle lies in the empiricist philosophy which led to vast misunderstanding of science that persists to this day. In this paper, we have provided a sketch of how the Kantian disconnect between observables and reality translated into a misunderstanding of science which has never been corrected. This misunderstanding, which has evolved over time in many different ways, is responsible for a methodology which produces models divorced from reality.

The abandonment of efforts to match real structures has led to disaster, as models of economic theory have grown progressively distant from reality. Attempts to fix the problem have failed to address the cause. Economists look at bad models, and say we should replace these by better models. But the process by which models are evaluated, the underlying methodology, is not examined. The real problem lies much deeper than bad models and ludicrous assumptions. Bad assumptions would quickly be replaced by better ones if the methodology insisted on correction of models to match reality. The real problem is the lack of a progressive methodology. When our mental models are attempts to approximate reality, then, when they fail, we try to improve the match to reality. Our models become better as approximations to the hidden structures of reality. However, when we abandon efforts to match reality, our mental models can become progressively worse as approximations to reality while becoming better at providing a match to observations. This is precisely what happened to Ptolemaic astronomy. The original assumption of planets attached to orbiting heavenly spheres failed to match observations. So small spheres affixed to the big spheres were introduced. Similar ad-hoc corrections improved the fit to observed orbits, but made the models wildly inaccurate as approximations to the real structure of underlying reality.

The problem can be fixed only if we adopt a realist philosophy of science. Critical realism offers an extremely useful alternative to current economic and econometric methodology. A realist philosophy has the possibility of learning from experience. Even if we start with ridiculous assumptions, we will modify them in face of empirical evidence to the contrary. In complete contrast, economists stubbornly stick to assumptions known to be false because the standard methodology says that false assumptions are not a problem for models. There is no hope for progress in economics until we abandon Friedman's methodological prescriptions according to which the more ludicrous our assumptions, the better our model.

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The value of “thinking like an economist”

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Abstract

In a recent Facebook posting by masterclass.com, Nobel Prize laureate Paul Krugman invites the public at large to his master class where he will teach you how “to think like an economist”. This raises the obvious question, what is the value/utility of this masterclass, priced at \$120.00? In other words, what is the value of the information/knowledge that is provided? In this essay, we ask and attempt to answer the following question, namely what is the value of thinking like an economist? We argue that based on economics’ track record in its many sub-fields (micro, macro, international trade), its value is seriously in doubt, to the point of questioning the legitimacy of its sticker price. We argue that a more appropriate masterclass (i.e. one worth the money) would be one offered instead to economists (scholars and professionals) entitled: “Learning how human beings actually think/ behave and how physical systems actually behave.”

1. Introduction

In a recent *Facebook* posting by masterclass.com, Nobel Prize-winning Columbia University professor Paul Krugman invites the public at large to his masterclass where he will teach you “to think like an economist”.

For Nobel Prize-winning economist Paul Krugman, economics is not a set of answers, it’s a way of understanding the world. In his Master Class, Paul teaches you the economic principles that shape political and social issues – like access to health care, the tax debate, globalization, and political polarization. Heighten your ability to read between the lines and decipher the underlying economics at play (Masterclass.com).

This raises the obvious question, namely is there any value in thinking like an economist? After all, as the prospectus seems to indicate, economics is not a set of answers, but rather a way of thinking, a way of understanding the world around us. As the old adage goes, the proof of the pudding is in the eating. If thinking like an economist does not necessarily lead to good or right answers, then why even bother?

This essay takes a critical look at the track record of economics in a number of key fields, in search of a metric with which to measure the value or worth of “thinking like an economist.” This will then be followed by a critical discussion of the bedrock of modern economics, namely the axiomatic underpinnings of consumer and producer theory.

2. The value of thinking like an economist by field

In this section, we examine, in summary form, the contribution of economics by field, in search of questions and answers. After all, the ultimate purpose of science is to ask and answer questions. We begin with the question of economic growth.

2.1 The value of thinking like an economist: the case of economic growth

Growth is, by far, the bread and butter of modern economics, and indeed of all of the economics from Adam Smith to the present. It is noteworthy to point out that *The Wealth of Nations*, whose complete title was *An Inquiry into the Nature and Causes of the Wealth of Nations*, was first and foremost about growth, the growth of material wealth.

This raises the obvious question, what has 250 years of growth theory produced? After all, that's an awfully long time to be working on a problem. The answer is, not much. As Krugman himself argued in a 2014 *New York Times* piece, new growth theory, introduced in the 1980s with much fanfare (and a recent Nobel prize), has so much as fizzled out. In a nutshell, growth economics (old and new) has been a monumental failure, with paradoxes and puzzles galore, and no clear path for the future. For example, there's the Solow Residual, the Productivity Slowdown, and the Information Paradox. In short, not much to show for centuries of work, and even less to merit accolades and/or prizes.

2.2 The value of thinking like an economist: the case of macroeconomics

Truth be known, modern economics is largely the by-product of what we refer to as the gilded age of economics, namely the Keynesian epoch (1936-1976), when the profession had a set of policy tools that, in the eyes of the public, were efficacious – that is, that actually worked. As a result, governments invested heavily in information collection, and universities throughout the world created economics departments, offering newly-minted undergraduate and graduate programs. Economics had arrived so to speak, largely as the result of its success, of its new track record.

As it turned out, the resulting glory proved to be premature, as it lacked a consistent set of micro-foundations. Keynesian policies appeared to work, but no one knew exactly why. Moreover, as far as the Great Depression was concerned, the jury was still and is still out. So, we were left with a set of policy measures that appeared to work, but we knew not why? Nor did we know the underlying cause(s).

This fragile state of existence came to a screeching halt with the precipitous fall in growth in the 1970s, known as the productivity slowdown. The resulting use of fiscal policy failed to restore prosperity, and in little time, the bottom fell out of Keynesian economics, being replaced by the neoclassical consensus. In a nutshell, the government was powerless, and should as such, stay out of the affairs of the nation. Instead, it should balance its budget and pursue policies that are conducive to price stability.

The resulting ideology held sway for a quarter century, until the Financial Meltdown of 2008, when the profession was once again confronted with its past failures. The public reaction was predictable. Even the Queen of England entered the fray, asking England's leading macroeconomists why was it that they had failed to anticipate anything close to the crisis. Now, ten years after the fact, the underlying causes of the Meltdown remain shrouded in mystery.

2.3 The value of thinking like an economist: the case of international trade

The Productivity Slowdown did more than reawaken interest in growth, it also led to a series of policy heuristics, the purpose of which was to restore growth to post-WWII levels. One such

heuristic was free trade, which was touted by many as the answer to slow growth. More trade would be growth increasing, or so it was argued.¹

Unfortunately, most if not all of the trade-related policy measures enacted in this period were without any basis in science. Being a trade economist himself (and having won a Nobel prize for his contributions to the field), Paul Krugman should know that the scientific track record of international economics (trade and finance) is dismal, bordering on shameful. Despite two centuries of theories and hypotheses, not one has been proven scientifically, including his own work. In short, trade theory teaches us nothing useful about the real world. Which is not to say that it is not elegant and logically appealing. The problem lies with its usefulness.²

The recent rise of nationalism in the U.S., Britain and elsewhere, is a testimony to bankruptcy of international economics and a good measure of the immense costs of our ignorance. Free trade was supposed to work wonders for all concerned. Post-WWII growth rates were supposed to return. Clearly, it has failed to deliver.

2.4 The value of thinking like an economist: the case of microeconomics

Microeconomics is the systematic study of resource allocation in a world in which needs and wants are assumed to be unlimited, and where resources or the need to meet them, are limited. In contemporary microeconomics, the emphasis is on a particular institutional form, namely free markets where prices are called upon to “do the job” so-to-speak. As such, it stands to reason that price theory would be not only front and center, but be the standard against which success, or lack thereof, would be measured. After all, if prices are a mystery, so then is the whole process of market-based resource allocation, and thus all of microeconomics.

This then begs the question: do we have a good theory of prices and by good, we mean one that is tried, tested and true? In other words, do we understand prices beyond the obvious, namely that excess demand can lead to higher prices, while excess supply, to lower prices? Unfortunately, the answer to this question is an unqualified no. Despite decades of theorizing, the introduction of game theory, the advent of experimental economics, big-data and unparalleled computing facilities, prices remain a mystery to us. In short, while we have many models/theories of prices, we have few that actually work, as evidenced by the fact that non-economists resort to rule-of-thumb pricing models such as simple mark-up pricing techniques.

Nowhere is this “deficit” more obvious than in macroeconomics where, from the Keynesian revolution onwards, short-run price formation has been at the center of the debate, with the majority of scholars simply assuming that they were fixed. Another “price hotspot” is competition policy where price lies at the center of the debate over market structure and social welfare. Again, the lack of a good model of price formation makes the task of evaluating the social welfare implications of market structure difficult, if not impossible. On a broader level, it has contributed to a debate over the effects of industry structure (efficient

¹ An informal survey of regional free-trade agreements (FTAs) revealed that “promoting growth” was by far the most common objective, with no mention of greater gains in welfare from lower trade barriers. As such, trade policy is about growth, while trade theory is about welfare.

² Another glaring problem is its focus on final goods and services, when in actual fact, trade is fundamentally about value added, something the WTO-OECD has recently acknowledged. Value chains have, from time immemorial, been global in scope, with Britain’s 19th century trade flows being a perfect example (imported cotton, exported textiles).

structure versus Mason/Bain concentration) which has never been, nor will be resolved without a good understanding of prices. As such, analysts are unable to judge whether any given price (especially in concentrated industries) is excessive relative to the associated cost.

It goes without saying that the very core field in modern economics has a questionable track record, scientifically speaking. While it is elegant in its axioms and construction, logical in its reasoning and exhaustive in its breadth, it has been less than successful where it counts, namely shedding light on real-world phenomena.

2.5 The value of thinking like an economist: the case of income distribution

The field of income distribution has been a contact sport since a German political economist by the name of Karl Marx declared that because labor and labor alone was physically productive, any part of the final product allocated to the owners of capital was a form of theft. Invoking the most basic principle of property law (i.e. that of enjoying one's property), he went on to construct a model of social behavior based on class conflict.

Mainstream writers (classical political economy) responded in kind with what became known as neoclassical distribution theory, based on very non-scientific developments, namely the decreeing of capital as physically productive and thus deserving of its share of the proverbial pie. From this point on, anything and everything was or could be productive, and its remuneration would track its marginal product. The pinnacle of its success came with the KLEMS approach in the 1970s where capital, labor, energy, materials and services were deemed to be physically productive.

Despite its simplicity, this approach held sway for over three-quarters of a century. However, its usefulness, not to mention, relevance, has come under increasing fire, in response to (i) excessive executive compensation (ii) worker-less factories and (iii) falling wages despite rising productivity. One could go as far as to argue that the field of income distribution is currently in a full-blown crisis, as evidenced by the popularity of Thomas Piketty's *Das Capital*-inspired best seller, *Capital in the Twenty-First Century*.

2.6 The value of thinking like an economist: the case of economic development

For over three-quarters of a century, the economics profession concerned itself with one of the most pressing questions of the modern era, namely how to close the gap between the rich and the poor, between the first world and the third world. Riding the Keynesian wave of optimism in the post-WWII period, it was felt by many that having resolved (purportedly) the problem of the business cycle, the West could now bring an end to poverty. In other words, the lessons learned in the North could now be used as a guide to pulling the South out of its poverty.

While laudable, success in mentor-mentee-type relationships (which this was) in general requires a good understanding on the part of the mentor of his/her own past and factors that contributed to his/her success. Unfortunately, this is where things came unhinged. First and foremost was the fact that the West had not understood its past, its own rise out of poverty, and its industrial revolution(s). However, equipped with what it felt was a good understanding of wealth creation (neoclassical production theory), it set went on its merry way, focusing for the most part on capital. In keeping with the Solow-Swan model of growth, the key was believed to lie with a rising capital-labor ratio.

The end result was as disappointing as its central premise was simplistic, if not fallacious. Economic development as a field has been a complete and utter failure. Various measures of poverty have shown the lack of any gains over the course of the past half-century (China excluded). The lack of success has ushered in the current rhetorical approach, based in large measure on slogans. A good example is the multilateral organizations' (WTO, IMF, and World Bank) slogan of "freedom" as a solution to virtually every problem. Free trade has now become the universal panacea to poverty.

If by "learning to think like an economist," it should be understood, learning and integrating the aforementioned microeconomics, macroeconomics, economic growth, income distribution and economic development into one's thought patterns, then the question of value or worth is very much real, and one that deserves to be discussed in more detail. Clearly, if the proof of the pudding is in the eating, then the value of thinking like an economist is very much in doubt. If it leads to more questions, or equivocal outcomes/conclusions then its value is questionable, to say the least. If it is motivated by its track record (that is, ability to solve key societal problems) then again, its value is very much in doubt.

This raises the question, why? Why has economics as a field of inquiry performed so poorly? Why has thinking like an economist failed to provide answers to these and other pressing questions? Why have economics and economists in general fallen from grace over the past three decades – roughly from the productivity slowdown in the 1980s? In the next section, we attempt to answer this question.

3. The problem of weak first principles

Economics is both a social and non-social (pure and applied) science, social in its quest to understand human behavior in the realm of goods and services, and non-social in its understanding of material processes – that is, the way in which goods and services (our bread and butter) are produced. It therefore stands to reason that for it to be successful, it must decipher how human beings think, and second, how inanimate material processes behave. It must understand the mechanical and physical laws that underlie production processes. In short, before it can begin to say anything of value, it must understand its subject(s). Has it?

In this section, we argue that it hasn't on both counts, namely consumption and production.³ In short, modern consumer and producer theory is vestigial in nature, dating back to the mid-19th century, to a time when social sciences were virtually unknown and our understanding of production was devoid of science altogether. That this was the case back in the 1860s and 1870s is not the issue. Rather, what is at issue is the failure of economics to evolve, whether it be internally, or via the other related scientific disciplines (psychology, sociology, process engineering, applied physics). It is worth noting that all of these related fields have witnessed great progress over the intervening time period (e.g. the laws of thermodynamics)

3.1 Weak first principles: the case of consumer theory

For a college freshman, or any layperson for that matter, taking their first microeconomics course is akin to traveling to another planet or universe where the inhabitants are less

³ By consumption and production, it should be understood, mainstream consumer and producer theory.

evolved (more primitive), and where the laws of physics are, for all intents and purposes, suspended – in short, a case of social science fiction. It is a voyage back to a simpler time, a dark ages of sort, when behavior was ascribed to spirits, and motion, to something referred to as *vis visu*.

In short, s/he learns that we as a species are concerned uniquely with something we call utility, measured in utils. There is no reason given as to why we are so intent on maximizing it, but instead are told that it has to do with our fundamental nature. While simplicity and reductionism do have a place in formalization, it is not and should not be seen as the end result. Unfortunately, this is where consumer theory comes up short for this is precisely where the analysis ends. Everything and anything is and can be a source of utility.

While we can forgive the likes of William Stanley Jevons, Francis Ysidro Edgeworth and Alfred Marshall their simplicity in formalizing behavior in the 19th century, it becomes a matter for discussion/debate whether we can do the same in the 21st century, given the advances made in the related behavioral sciences of psychology and sociology. For some reason, the profession has remained impervious to outside influences, with the result that today, despite having similar interests and concerns, economists and psychologists/sociologists do not see eye-to-eye, and have little-to-no common ground. Reducing Homo-sapiens to a mere utility maximizer/automaton has not earned economics any brownie points in the rest of the social sciences.

In the end, it boils down to one thing, namely that the ultimate purpose of the social sciences is to learn how members of our species think—or attempt to understand the way they think and hence, behave. Given its track record in so far as consumers are concerned (or economic agents), it is not at all clear that we economists have succeeded in that part of our mission.

3.2 Weak first principles: the case of producer theory

The same criticism applies to producer theory where output is modeled as an increasing function of capital and labor. While this may have been acceptable to mid-19th century political economists, it is orthogonal to our (non-economic) current understanding of material processes. Broadly-defined physics has shown us that all material processes, bar none, are energy based, and that modern-day labor and capital, not being sources of energy, are organizational inputs (read: non-physically productive). In short, the laws of physics (kinetics and thermodynamics) are what govern production processes. There can be no exceptions and no violations. Again, the role of the economist in so far as production is concerned is to understand the behavior of material processes. Once more, it is not at all clear that we have succeeded.

4. Why are economics' "fundamental axioms" archaic?

This is an interesting question and one that I don't have the answers to. What I do know however is that despite major advances in its partner fields (related fields), it has remained impervious to incorporating these advances. Not surprisingly, this has created a rift between it and the other social sciences, not to mention the pure and applied sciences. On a personal note, in numerous interdisciplinary faculty meetings, I have heard more than my share of barbs aimed at economics and *homo oeconomicus*. I suspect that were I to be in an applied science faculty, I would have heard similar barbs directed at production theory.

So here goes. First, I believe that one of, if not the most important reasons has to do with the very history of capitalism, specifically with its ability to self-correct (avoid collapse) and more importantly, achieve full employment. The mid-19th century was plagued by recurrent recessions and depressions, leading many to argue that it was fundamentally unstable.⁴ Not surprisingly, this led to a quest on the part of classical political economists (read: the mainstream) to prove to the world that free markets were not flawed, and that capitalism could and did lead to first-best outcomes.

The task was daunting, to say the least. Any proof had to be bullet-proof, given the fact that the evidence seemed to show/point to the contrary. Unfortunately, what was lost in the exchange were the very principles that govern and guide scientific inquiry. In short, the theorists of the time had to engage in a form of reductionism – that is, reducing complex phenomena to simple ones, all in the name of proving their conclusion. Enter neoclassical consumer and producer theory. Only by stripping *homo oeconomicus* of his humanity and production processes of their underlying laws of physics could a system of equations be derived/formulated in order to prove existence and stability.

The need to do so was heightened by the events of the early 20th century, namely WWII and its aftermath (especially in England) and the Great Depression. Again, the onus was on showing that capitalism was fundamentally stable, and that recessions and depressions were of man's doing (read: government).⁵

Post-WWII developments did little to change this general direction. Two however stand out, namely Paul Samuelson's *Principles*, and *Game Theory*, both of which served to increase the formalization of what was an archaic base. Introducing static and dynamic optimization techniques (Lagrangians, Hamiltonians, etc.) borrowed from thermodynamics only made matters worse, sucking up all the oxygen in the room. Ibid for game-theory, which despite much fanfare, has failed to be a game changer.

While Keynesian economics provided the profession with its finest hour, public relations-wise, it had a deleterious effect on our understanding of investors, markets and the economy as a whole. Animal spirits, beauty contests, sunspots and rigid prices became the order of the day. Instead of being the opening salvo of more detailed analysis, these became the rallying cry for a greater role for government in all matters economic.

And last, the development of computable general equilibrium techniques, while a welcome development in any other setting, has further entrenched what are archaic axioms in economic analysis, owing again to its ubiquitous need for simplicity. The result is a quest to mimic the data with what are parsimonious models, the value of which is very much in doubt.

In conclusion, our need and/or desire for answers to the pressing question of the existence and stability of market economies has de facto prevented us from developing more realistic and complex models of behavior, both for human behavior and physical systems, making for the current "scientific" underdevelopment in economics as a social science. Advances from related fields have been and continue to be ignored, all in the name of the formalization needed to demonstrate the viability of a system which continues to be characterized by

⁴ The bulk of what can be defined as radical political economy (Owen, Sismonde de Sismondi, Malthus, and Marx) was motivated by this issue.

⁵ In this period, the proofs moved away from solving systems of equations, to topography (i.e. fixed-point theorems).

periodic crises. Put differently, formalization has retarded economics' evolution as a more complete social and non-social (pure and applied) science.

Summary and conclusions

As the ad puts it, Paul Krugman can teach you how to think like an economist. The question, however, is whether anyone would truly want to, given what is a questionable track record in key areas, and second, what is a set of fundamental axioms that serve not science, but a class of scholars who, for the last two centuries, have put ideology ahead of knowledge. One wonders, what is the value of proving that a system is stable, if its underpinnings are and continue to be orthogonal to the world it seeks to explain?

In closing, it could be argued the very notion of "learning to think like an economist" is a direct contradiction and violation of the purported nature and purpose of social sciences, namely that of understanding human behavior, or put simply, understanding how *homo oeconomicus* thinks and behaves. It therefore follows that if economists think any different from their subjects, then there is something blatantly wrong. Economists, like other social scientists, are charged with the task of showing how the way we as a species think and behave, affects the world (aggregate) around us.

As the old adage goes, the proof of the pudding is in the eating. And, increasingly, few are eating. Economics' heyday, as far as a way of thinking, came to an end with the demise of Keynesian economics. In the current context, much of that which economists have to offer invariably turns around the question of freedom versus government intervention. And for most of the post-Keynesian era, the former became the dominant ideology. Today, a decade after the Financial Meltdown, the profession is equivocating between both positions. The unfortunate part of this debate is the lack of bullet-proof fundamentals, making it more one couched in hunches, prejudices, heuristic principles and beliefs, personal anecdotes, etc. – in short, not the stuff of science.

Given the conclusions of this essay, we feel that more could be gained by turning the tables on masterclass.com and Paul Krugman by proposing a masterclass for economists on "how human beings think and behave" followed by a second course on "how material processes behave" – that is, are organized and operated. For only when we economists have a better understanding of human behavior in the field of consumption and the behavior of physical systems in the field of production, can we begin proselytizing to the world – that is, begin to ascribe a dollar value to it.

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An essay on the putative knowledge of textbook economics

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Abstract

The article pursues the two related questions of *how* economists pretend to know and *why* they want to know at all. It is argued that both the form this knowledge has taken and their motivation for knowing have undergone a fundamental change during the course of the 20th century. The knowledge offered by important contemporary economic textbooks has little in common with objective and explicitly scientifically motivated knowledge. Rather, their contents and forms follow a productive end, aiming at the subjectivity of their readers.

Keywords economic education, philosophy of economics, Foucault, neoliberalism

JEL codes A10, A13, A20, B13, B40

1. Introductory remarks

The subject of this essay is the knowledge of economists. More precisely, it is not the content, but the form of their knowledge. It seems to me that this form took a decisive turn in the 20th century and that what economists pass on in textbooks today has little to do with knowledge in a scientific sense. In this way, however, they no longer follow an understanding of knowledge that prevailed, for example, in the early tradition of neoclassical theorization. Secondly, this change in the concept of economic knowledge is based on a change in the fundamental will or motivation of economists. What is the primary purpose of their activities? I think that this question can neither be answered from an inner-disciplinary, nor from a merely inner-scientific perspective. Rather, it must be reflected today in the light of the politico-economic context of economic science and education.

The theses of this twofold change in the understanding of economic knowledge as well as in its underlying motivation will be presented by referring to a particularly strong contrast: on the one hand, using the example of those who introduced a consistent mathematical methodology into economics at the end of the 19th century, thereby establishing the neoclassical tradition which is still dominant today; on the other hand with reference to contemporary textbook literature, which presumably sets out to introduce newcomers to the science of economics. The reference to didactic literature is based on a characterization of economics as a textbook science, which as such is constitutively dependent on the mediation of canonized knowledge (Bäuerle, 2017).

The claim is not made here to meticulously elaborate the two different cultures of knowledge and will. Rather, the possibility of a systematic demarcation should be raised so that this border and its historical realization can become the object of reflection and criticism. In this sense, the basic intention of this essay is not to present a detailed empirical work, but rather to offer a basic interpretation scheme for a multitude of findings in current economic textbook research (Graupe, 2019, 2017; Graupe & Steffestun 2018; Bäuerle 2019, 2017; Maeße, 2018; Zuidhof, 2014; Giraud, 2014, 2011; Peukert, 2018; van Treeck & Urban, 2016).

This essay is inspired by a study carried out by Silja Graupe (2017), in which she draws a distinction between different epistemic cultures in early neoclassical economics on the one hand and contemporary economic textbooks on the other. In contrast to Graupe's work, this essay will focus on a conceptual selectivity of two forms of economic knowledge and related forms of will. To this end, I shall rely on Michel Foucault's examination of political economy and its concept of knowledge in particular, and finally on thoughts of Philip Mirowski and Edward Nik-Khah (2017), who also attest to a drastic shift in economic science in the post-war period with regard to its underlying concept of knowledge.¹

The question that should guide us through the first part of my presentation is: What understanding of economic knowledge underlies the most important textbooks today? I limit myself to three highly internationally popular textbooks of introductory courses (Econ101) (Bäuerle, 2017, p. 253 f.): the archetype of the genre, Paul Samuelson's *Economics*, Gregory Mankiw and Marc Taylor's *Economics*, who hold about 20% of the international market share (cf. *ibid.*) and finally the *Principles of Economics* by Robert Frank, Ben Bernanke and Louis Johnston.

2. The knowledge of economic textbooks

Samuelson/Nordhaus address my leading question as follows:

"Our primary goal is to emphasize the core economic principles that will endure beyond today's headlines [...] there are a few basic concepts that underpin all of economics [...] We have therefore chosen to focus on the central core of economics – on those enduring truths that will be just as important in the twenty-first century as they were in the twentieth" (Samuelson & Nordhaus, 2010, pp. xviii-xix).

The two textbook authors are obviously interested in basic economic principles that apply to the entire economics discipline. "Eternal truths" which are valid independently of time and are not subject to any historical conditionality. In older editions, Samuelson emphasizes that they also claim validity independently of spatial situations (Russia, China, USA) and political affiliations (Republicans / Democrats) (Samuelson, 1976, vii). The knowledge of economists is therefore a knowledge that promises universal validity, it is context-free. Frank et al. illustrate the supposed natural-law quality of economic truths by referring to an example from everyday life:

"Most of us make sensible decisions most of the time, without being consciously aware that we are weighing costs and benefits, just as most people ride a bike without being consciously aware of what keeps them from falling. Through trial and error, we gradually learn what kinds of choices tend to work best in different contexts, just as bicycle riders internalize the relevant laws of physics, usually without being conscious of them" (Frank et al., 2013, p. 7).

¹ In the case of the latter, I follow the changes mentioned not only with regard to economic *education*, but also with regard to economic *research*.

In the understanding of the textbook authors there seems to exist, beneath the surface of human action – all human action, a sphere of laws to which that action is as bound just as natural objects are bound to natural laws. These are the economic laws or principles that the textbook aims to explain. But what remains to be done for the economist in the context of a principally law-governed economics?

“Economists try to address their subject with a scientist’s objectivity. They approach the study of the economy in much the same way as a physicist approaches the study of matter and a biologist approaches the study of life: they devise theories, collect data and then analyze these data in an attempt to verify or refute their theories. [...] The essence of any science is scientific method – the dispassionate development and testing of theories about how the world works. This method of inquiry is as applicable to studying a nation’s economy as it is to studying the Earth’s gravity or a species’ evolution” (Mankiw & Taylor, 2014, 17; emphasis L.B.)

Adhering to the model of the natural sciences, Mankiw and Taylor state that as economists they are also using “the” scientific method. As scientists using scientific methodology, theories appear and are tested which explain “how the world works”. Economic science discovers these truths and passes this knowledge on in the context of textbooks and accompanying courses. It thus seems to be a decidedly scientific undertaking, which the textbook authors quoted here agree with. In that last quotation of Mankiw and Taylor we also saw an explicit reference to the basic attitude of their action and thus also the results of this action (economic knowledge) as specifically scientific activity and knowledge: scientific objectivity.

3. Objectivity as an epistemic virtue

Following the work of Lorraine Daston and Peter Galison (2007), I would now like to introduce objectivity as an epistemic virtue as a second step – in order to subsequently be able to judge whether the knowledge of economists corresponds to this understanding of scientific action.

What is an epistemic virtue? The purpose of all epistemic virtues is stated by Daston and Galison in sharp demarcation from self-knowledge with world-knowledge: “Epistemic virtues in science are preached and practiced in order to know the world, not the self” (Daston & Galison, 2007, p. 39). Epistemic virtues therefore serve as a guideline or ideal for the development of a certain scientific attitude with the aim of recognizing the world: “they are norms that are internalized and enforced by appeal to ethical values, as well as to pragmatic efficacy in securing knowledge” (ibid., pp. 40-1). Virtuous epistemic action – if understood in this particular context as an attitude – is especially demanding for the scientist. Epistemic virtues define how the formation of a scientific self is to be accomplished; a self that cultivates certain traits of character and prevents others: “The mastery of scientific practices is inevitably linked to self-mastery, the assiduous cultivation of a certain kind of self” (ibid., 40). Finally, Daston and Galison examine and understand these virtues in their historical contingency as “fashions” of scientific practice subject to cultural, intellectual, historical, technical, and economic processes of change.

Against this background, Daston and Galison reconstruct how objectivity as an epistemic virtue gained strength during the course of the 19th century, and how it became decisive for a multitude of sciences and their members. What did it mean to be objective back then?

“To be objective is to aspire to knowledge that bears no trace of the knower – knowledge unmarked by prejudice or skill, fantasy or judgment, wishing or striving. Objectivity is blind sight, seeing without inference, interpretation, or intelligence” (ibid., p. 17).

The acquisition of knowledge can only be achieved if the opposite pole of the objective, the subjective, is kept out of the act of perceiving (ibid., p. 36 f.). Only a knowledge freed from subjective influences allows one to hope that the object can actually be grasped in its own way and subsequently represented. Thus, the epistemic virtue of objectivity requires the scientific self to control itself in such a way that the cognitive process is not “polluted” by personal desires, experiences and prejudices. The paradox of the objective scientific self is its obedience to an epistemic rule that makes it the enemy of itself. A “will to willlessness” (ibid., p. 38) commands the objective self to decided self-negation, a kind of epistemic asceticism.

Crucially, the scientist must *consciously* carry out this self-restriction in order to be able to attain knowledge. The epistemic virtue of objectivity for the scientific self demands a constant distrust of itself; and this distrust must be carried out at every moment of scientific practice in the most precise way. Although in an extreme form the permanent self-exclusion from the epistemic act presupposes a conscious self-relationship. The objective self must know where and when it is transforming the object with subjectivity in order to protect it from it. In its bipolarity, the relationship between self and world is inseparably bound up and must be practiced virtuously for the purpose of knowing the world.

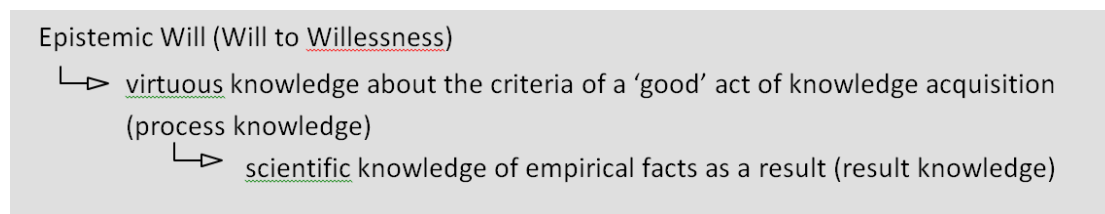
An anchor and guarantor of this scientific balancing act, as already mentioned with regard to the “will to willlessness”, is the belief in the strength and freedom of the human will:

“the will asserted (subjectivity) and the will restrained (objectivity) – the latter by a further assertion of will. In Jena and Paris, London and Copenhagen, new ideals and practices of the willful, active self took shape in the middle decades of the nineteenth century” (ibid., p. 228).

The will for objective knowledge aims at knowledge of the world. However, this knowledge has no ultimate, metaphysical quality. It is rather the *result* of a virtuous epistemic process in an empirical confrontation with the world (cf. ibid., pp. 213-215): “objectivity was conceived in the sciences [...] as an epistemological concern, that is, as about the acquisition and securing of knowledge rather than the ultimate constitution of nature (metaphysics)” (ibid., p. 215). This limitation of the primary motivation of scientific inquiry also manifested itself in a shift of the scientific ethos away from the truth-seeking genius to the indefatigable worker, the objective observer.

In the overall view, in connection with the epistemic virtue of objectivity, two forms of knowledge are thus produced: based on a scientific will to knowledge, the scientist must first have and put into practice a virtuous knowledge of what is necessary for a “good” scientific process. If sufficiently considered, the act of knowledge or research then carried out promises to be a scientifically (i.e. objectively) assured knowledge as a result.

Figure 1 Hierarchy of wills and knowledge of objective knowledge, based on Daston & Gallison (2007)



4. Objectivity in neoclassical economics

Did scientific developments and the epistemic virtue of objectivity have an influence on economists during the course of the 19th century? And if so, in what form? In his volume "More Heat than Light", Philip Mirowski has worked out what comprehensive influence the developments in the natural sciences of the 19th century had on the development of marginalism and thus also on the formation of neoclassical theory, which still sets the tone today. This influence also includes the enthusiasm for the objective ideal of knowledge, even if Mirowski does not make this facet the main object of his investigation. Although he reproaches the application of field formalisms and the development of mechanical analogies in the field of economics at the expense of internal coherence in the area of origin (i.e. analytical mechanics) (Mirowski, 1989, pp. 229-31, pp. 272-74), he consistently emphasizes the *epistemic* intentions and convictions that guided the mathematical economists in their revolution. It was confidence in the increased *cognitive faculties* of objective natural sciences that allowed the marginalists to adopt mechanical-mathematical methodologies into the science of political economy. This confidence is shared by the fundamental works of early neoclassical economists such as Leon Walras:

"Pure mechanics surely ought to precede applied mechanics. Similarly, given the pure theory of economics, it must precede applied economics, and this pure theory of economics is a science which resembles the physico-mathematical sciences in every respect. If the pure theory of economics [...] is a physico-mathematical science like mechanics or hydrodynamics, then economists should not be afraid to use the methods and language of mathematics. The mathematical method is not an experimental method; it is a rational method" (Walras, 1965[1874], p. 71).

Further, William Stanley Jevons:

"[John Stuart; L.B.] Mill [...] speaks of an equation as only a proper mathematical analogy. But if Economics is to be a real science at all, it must not deal merely with analogies; it must reason by real equations, like all the other sciences which have reached at all a systematic character" (Jevons, 1965[1871], p. 101).

And finally, Irving Fisher:

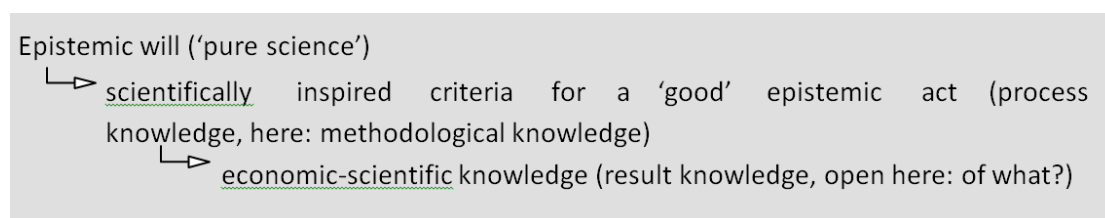
"There is a higher economics just as there is a higher physics, to both of which a mathematical treatment is appropriate [...] The introduction of

mathematical method marks a stage of growth – perhaps it is not too extravagant to say, the entrance of political economy on a scientific era [...] Up to this time political economy had been the favorite field for those persons whose tastes were semi- scientific and semi-literary or historical” (Fisher, 1965[1892], p. 109).

In order to enter a scientific state, political economy had to incorporate the exact methods of the natural sciences, according to the unanimous opinion. What the marginalists undoubtedly differ in is the degree and quality of scientific objectivity they applied to their own work. Although the pronounced imagery and analogies to the analytical mechanics of the works of Jevons, Edgeworth, Walras or Fisher, for example, suggest that they are committed to the epistemic virtue of *mechanical* objectivity (Daston & Galison, 2007, ch. 3), the methodological remarks or chapters rather show a sympathetic proximity to what Daston and Galison call “structural” objectivity: a kind of radical form of objectivity, which hoped to keep subjectivity in total control by consistently escaping into purely abstract, usually mathematical methodology and a scepticism towards pictorial representations of phenomena and empirical observation in general (Daston & Galison, 2007, ch. 5). If this interpretation is true, then confidence in the methods of the natural sciences in economics even led to the loss of a concretely experienced, empirically accessible world (see Düppe, 2009, 50ff. for theoretical considerations and Pühringer & Bäuerle 2019 for its empirical manifestation in economic education).

Regardless of the question of how the epistemic virtues of the marginalists showed itself in individual cases, they were all guided by *epistemic* virtues and were thus interested in the most *successful epistemic process* possible (Mirowski & Nik-Khah, 2017, p. 25). And a universal benchmark for successful epistemic processes seemed to have been found for many sciences in the field formalisms of Lagrange and Hamilton between 1850 and 1870 (Mirowski, 1989, pp. 35, 201, 217). The mathematical revolution in economics was led by epistemic convictions which in the middle of the 19th century seemed to carry great explanatory potential in the natural sciences with regard to the functioning of the world (“Laplace’s dream”). Thus, in connection with Daston and Galison’s observations on the one hand and Mirowski’s on the other, the thesis could be formulated that a “will to willessness” as of the 1870s also led to the decision for alternative methodologies in political economy and was finally reflected in the change of name of the discipline to “economics”.

Figure 2 Hierarchy of will and knowledge of objective economic knowledge, based on Daston & Gallison (2007)



5. The knowledge of economists

The occasional confession contemporary textbooks make with regard to this decidedly scientific, partly also objective tradition is to be doubted on closer inspection. In order to be

able to formulate and prove this doubt, I would like to present an understanding of knowledge, which in my opinion is suitable to classify the one found in economics textbooks. It originates from Michel Foucault's lectures on the birth of biopolitics and was developed in the immediate discussion of economic science. What kind of knowledge does the discipline of political economy develop according to Foucault?

"The question here [in political economy, L.B.] is the same as the question I addressed with regard to madness, disease, delinquency, and sexuality. In all of these cases, it was not a question of showing how these objects were for a long time hidden before finally being discovered, nor of showing how all these objects are only wicked illusions or ideological products to be dispelled in the light of reason finally having reached its zenith. It was a matter of showing by what conjunctions a whole set of practices – from the moment they become coordinated with a regime of truth – was able to make what does not exist (madness, disease, delinquency, sexuality, etcetera), nonetheless become something, something however that continues not to exist [...] It is not an illusion since it is precisely a set of practices, real practices, which established it and thus imperiously marks it out in reality" (Foucault, 2010[1978], p. 19).

Foucault negotiates economic knowledge as a "dispositive", as a template of thought which, through the radiance of its true character on the one hand and its animation by human practices on the other succeeds in appearing in reality. Because people attribute truth to dispositifs and begin to align their actions with their immanent laws of truth and falsehood, non-existence – one could also say abstraction – becomes real in the sense that it shapes experience. For Foucault, it is this primarily productive character of dispositifs which puts them at the heart of his power-theoretical considerations. Dispositifs of knowledge are dispositifs of power, whereby Foucault emphasizes:

"We must cease once and for all to describe the effects of power in negative terms: it 'excludes', it 'represses', it 'censors', it 'abstracts', it 'masks', it 'conceals'. In fact, power produces; it produces reality; it produces domains of objects and rituals of truth. The individual and the knowledge that may be gained of him belong to this production" (Foucault 1995[1975], p. 194).

Knowledge, one could formulate in reference to this understanding of power, is a *production task*. Its content indicates both what is and what ought to be, whereby what exists is identical with what ought to be. The peculiarity of this production task thus consists in the fact that it pretends that what is to be known, and thus what is to be produced, already exists: as truth. As the last sentence of the above quote underlines, for Foucault the most important product of modern practices of power is the modern subject itself (cf. also Foucault, 1983, p. 208). The subject must act at the same time as the actor, as well as the target of the production task, for power to be developed at all. Whoever appropriates true knowledge of man, such as their true nature, true preferences, true motivations, etc. makes them the *subject* of this knowledge, as subordinate (lat.: *sub-iectus*). And the specific content of knowledge indicates the character of this subjectivity. With the execution of subjection to a specific knowledge, the production task installed in knowledge is realized: the subject processes or produces itself on its basis.

Against the background of such an understanding of subjectivity, knowledge, power and truth, Foucault now reflects on the science of political economy as the decisive supplier of dispositifs of knowledge that set the tone for modernity. According to Foucault, it is the true laws of the economists to whom (initially Western) societies have increasingly devoted themselves since the end of the 18th century and who know how to distinguish between right and wrong actions. While at the time of political economy, knowledge, however, still referred to the leaders of territories and promised to evaluate their actions, the emergence of neoliberal thinking in the first quarter of the 20th century brought about an increase in the significance of economic knowledge for a potential *totality* of human action. This conceptual expansion, for example by the Chicago School of Economics and the leading figure of neoliberal theorization, Friedrich Hayek, is followed by a global expansion of economic knowledge in terms of its historical effects, so that today it has assumed the rank of a “general style of thought, analysis and imagination” (Foucault 2010[1978], p. 219). This style of thinking, which is actually a form of knowledge, is also characterized by the paradoxical peculiarity of wanting to be realized, although it is assumed to already exist:

“Neoliberalism is [...] understood not only as ideological rhetoric or as politico-economic reality, but above all as a political project that aims to create a social reality that at the same time presupposes it as already existing” (Bröckling et al., 2000, p. 9; my translation).

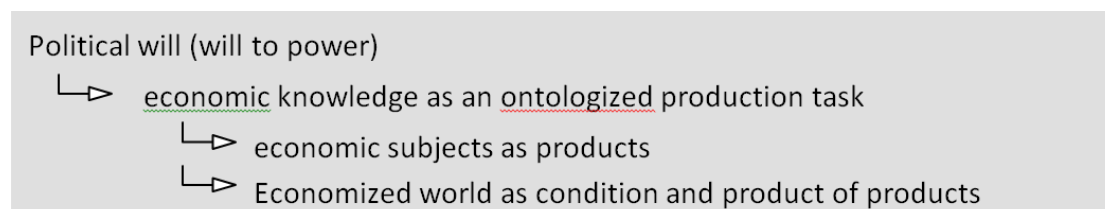
It is this quality as “already existing” that settles “true knowledge” on an ontological level. It is objective at best in the sense of the English “objective” or the romanic – here Spanish – *objetivo*: as goal or purpose (of a production process of subjectivity). In this sense, the subject should submit to an “objective” knowledge (of a certain subjectivity) that has always been fixed. It does not subject itself to a fundamentally open epistemic process, but to a self-contained truth.² It does not submit to an epistemic virtue, but the act of submission itself now appears as a virtue (Lemke, 2001, p. 85). As guided by this purpose and will, there are also no limits to the production task inherent in economic knowledge, such as those of an object to be recognized, or in extreme cases: of a world to be recognized. The driving force behind this process is not the “will to willessness”, but Nietzsche’s “will to power”, to which Foucault also refers (1991). *Not the understanding of the world, but the creation of the world is the purpose of this will and its form of knowledge*. For this purpose, this form of will is inherent in the constant increase of its processual efficiency, as well as the expansion of its sphere of action (Foucault 1991b[1978], p. 100).

In terms of content, it is *economic* virtues that the subject is presented with and advised on in the form of true knowledge. The emerging subjects are economical “in nature”. As such they process a quantified, market-shaped world through a *ratio*, a calculating thinking, in order to always achieve an indeterminate surplus in this calculating execution. As mentioned at the beginning, I don’t want to and cannot go into the specific contents of what constitutes economic knowledge. However, I like to refer to a discussion of this specific subjectivity, which in my opinion is also reflected in economic textbook literature, namely the *money subject* Karl-Heinz Brodbeck (Brodbeck, 2009, ch. 5) speaks of.

² On the basis of the specific content of economic knowledge (see below), the subject emerging at the moment of his subjugation reflects him- or herself as well as the world surrounding him as ultimately limitlessly objectifiable.

In the combination of its political, unlimited form with an economic, unlimited content lies the remarkable effectiveness of economic knowledge as it can be observed today in processes of economization in various areas of social and private life.³ As the next but one chapter will show, economization processes today find an important starting point and catalyst in the context of academic economic education.

Figure 3 Hierarchy of will and knowledge in contemporary economic education based on Foucault (2006)



6. The information of economists

After encountering Foucault as a first sceptic of a purely scientifically understanding of knowledge in economics, I would now like to introduce Philip Mirowski and Edward Nik-Khah, two further scholars who historically trace the knowledge and will of economists and attribute to them a shift from an epistemic to a productive attitude.

In their volume “The knowledge we have lost in information” (2017) they elaborate upon a fundamental change in the cultures of knowledge and will of economists after World War II. This change found its conceptual manifestation in the term *information*. The term spans a bridge from a political project of *The Market*⁴ as a central coordination mechanism for social processes to an understanding of the subject which encompasses individuality within this political frame of reference only as a semi-conscious or subconscious reaction to external information (e.g. prices). The processing of information is no longer conceptualized as a conscious act of perception and decision-making. Rather, thinking in the sense of *computing* becomes a collectively unconscious process. And as the specific instance of this collective computing power, The Market comes into play, whose signals for market participants in turn gain the quality of imperatives for action. The central figure for this specific understanding of information integrating macro- and microeconomics was Friedrich Hayek:

“Hayek came to portray knowledge as completely disengaged from the consciousness of the knower. This was the Hayek of ‘Competition as a Discovery Procedure’, wherein he deemed much of agents’ conscious knowledge as irrelevant to the operation of the well-functioning economy. In this incarnation, some knowledge could only be discovered by the market, and so in this final phase Hayek conceived ideal intentionality of individuals as acquiescing in the market’s signals” (Mirowski & Nik-Khah, 2017, p. 152).

³ With regard to empirical case studies in various social contexts see Schimank & Volkmann (2012).

⁴ With this notation I follow those of Mirowski/Nik-Khah (see next but one quote) and those of Ötsch (2019). On the one hand, it points to the anthropomorphic character of The Market, which is granted human abilities as an independent actor. On the other hand, it refers to the metaphysical character of The Market with superhuman qualities and abilities, which, among other things, give it a primacy over political processes and action (Ötsch, 2019, 10 ff.).

Markets and individuals were understood by Hayek as information processors, but without giving market participants themselves, scientists or others the opportunity to look into the black boxes of these processing procedures. Thus, the *results* of market-shaped and collectively unconscious processes became the only point of orientation. According to this understanding, truth is not the result of a conscious and human process, but the result of the market:

“For orthodox economists today, truth is not a matter of morality, nor of individual standards of veracity, nor even coherence with some simplistic notion of the scientific method. For the orthodox economist, core doctrine dictates truth is the output of the greatest information processor known to humankind – namely, The Market. [...] the wise market participant always defers to the pronouncements of the market” (Mirowski & Nik-Khah, 2017, p. 7).

With regard to its qualities as a social coordination mechanism, but also with regard to its “intelligent”, superhuman services of information processing, the market is considered superior in principle by its advocates. In the light of this a priori superiority, not only alternative forms of shaping society, but also scientific foundations or even criticisms of the market are discredited as “fatal conceit” (Hayek 1988[1974]). What remains to be done for economists when taking such self-imposed humility towards The Market for granted? Mirowski and Nik-Khah use the example of three variants of the concept of economic information to show that economists, in sharp distinction to the founding figures of neoclassical theory, mutated from explorers to *producers* of market-organized processes:

“Before 1980, many people believed that The Market was something that has always existed in a quasi-natural state, much like gravity. It seemed to enjoy a material omnipresence, sharing many characteristics of the forces of nature, warranting a science of its own. [...] Where economists once placidly contemplated markets from without, situated in a space detached from their subject matter, so to speak, now they are much less disciplined about their doctrines concerning the nature of economic agency, and much more inclined to be found down in the trenches with other participants, engaged in making markets” (Mirowski & Nik-Khah, 2017, pp. 144, 148).

According to Mirowski and Nik-Khah, during the course of the 1980s, economists, released from the detachment of an objective science, began to install and permanently improve markets as information processors in various social configurations (ibid., p. 130), thereby emphasizing that this productive credo originates from a genuinely political intention or program:

“The Market (suitably reengineered and promoted) can always provide solutions to problems seemingly caused by the market in the first place. This is the ultimate destination of the constructivist political program within neoliberalism” (Mirowski & Nik-Khah, 2017, p. 57).

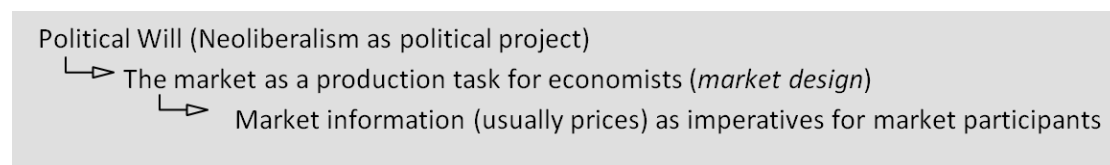
While the will of economists was expressed as decidedly scientific before 1980, it was now a *political* will with social-technical intent which underlay their work. Mirowski and Nik-Khah trace this shift back to the decidedly political intentions of neoliberal thinkers and their post-war institutions, highlighting Friedrich Hayek and the Mont Pèlerin Society as key institutions.

Similar to Foucault's analysis of the modern subject, the politically minded humility towards the achievements of the market springs from a neoliberal subject whose specific activity no longer lies in understanding or thinking, but rather in subjugating individual and collective life to the truth of a superhuman information processor:

"Neoliberalism influenced the way computational themes would enter economics: the agent would become one small cog in the grand market mechanism. [...] Consequently, knowledge no longer looks like it did in the Enlightenment roots of political economy. What happened to the Kantian subject, able to reason for herself, autonomous, and hence an end in herself? Economists' fascination with information has inadvertently debased their treatment of knowledge – first, for the agent and then, ultimately, for the economists themselves. Now all we have left is information. It was a seemingly technical notion that, reified, was the progressively removed from the grip of the agent who, in turn, would be denied anything that could reasonably be signified as 'understanding' or even 'thought'. This neoliberal subject was banished from the realm of ends, denied any optimality that makes sense, fated to slave away on a supremely complex calculation, churning through a subroutine, Truth always eluding its grasp" (Mirowski & Nik-Khah, 2017, p. 240).

In shaping the thinking and acting of a neoliberal subject, the introduction of an economic *information* concept precisely realized the active notion of the term as a verb (lat.: *informare*): form, shape, imprint (Mirowski & Nik-Khah, 2017, p. 45). Just as in Foucault's understanding of the subject, such informational subjectivity primarily aims at the *production* of reality, although Mirowski and Nik-Khah rather subordinate this production task to a political project of The Market, while for Foucault the subject itself is the cornerstone of the neoliberal project.

Figure 4 Hierarchy of will and knowledge of contemporary economic theory formation based on Mirowski & Nik-Khah (2017)



7. Knowledge and information of economics textbooks

Taking up the theoretical remarks of the last two sections, I would now like to conclude by underpinning the thesis of a primarily *productive* nature of economic textbook knowledge.⁵ The "knowledge" captured in them is not the result of a conscious epistemic process which students should also be enabled to undertake. *The knowledge of textbooks is rather to be understood as a production task for a particular subjectivity.* It is intended to initiate and guide a process of subjectivation which is largely carried out *by students themselves*. As a productive task of (self-) guidance, the underpinning and realizing virtue of this process is to be understood as *political* and not epistemic in nature. It is about *shaping the world*, not

⁵ I did this in detail in Bäuerle (2019: ch. 5).

understanding it.⁶ The focus lies on the antithesis of a knowledge of the world – self-knowledge (cf. Daston & Galison, 2007, p. 41) – but as a self-knowledge that is not open to speculation or imagination but always presupposes what is to be recognized as inner truth. This productive intention of economic textbook literature becomes understandable in the context of the political project, which both Foucault and Mirwoski and Nik-Khah addressed, which aims at an economic (self-)government of social processes.

Even though it can certainly not be assumed that all textbook authors deliberately guide and initiate the production task of a certain form of subjectivity, the ones I have focused on here are sometimes very explicit: “Our ultimate goal is to *produce economic naturalists* – people who see each human action as the result of an implicit or explicit cost-benefit calculation” (Frank et al., 2013, p. viii; emphasis L.B.). For his part, Mankiw emphasizes that he does not reflect his didactic work in an academic context, but in a political one. He connects the productive intention directly with the concept of information:

“In making these decisions [choosing textbook contents, L.B.], I am guided by the fact that, in introductory economics, the typical student is not a future economist but is a future voter. I include the topics that I believe are essential to help *produce well-informed citizens*” (Mankiw 2016, p. 170; emphasis L.B.).

Samuelson is also known to have at least partially discussed and developed his textbook from a political point of view:⁷

“Let those who will write the nation’s laws if I can write its textbooks” (Barnett & Samuelson, 2007, p. 143).

“The coin for which he [any ambitious scholar, L.B.] works is influencing the mind of a generation” (Samuelson, 1977, p. 870).

If these political intentions are compared with the specific contents of their textbooks, they appear to be central building blocks of an *education for the market*. Zuidhof, on the basis of a discourse analysis of ten international introductory textbooks, comes to the conclusion that they do not foster an understanding or even criticism, but rather to the creation of markets (Zuidhof, 2014, p. 180). In this way they seem to be encouraging the market-constructivist, decidedly neoliberal aspirations of the economic sciences since the 1980s, as reconstructed by Mirowski and Nik-Kah.

Even if further quotations of this nature could be cited from Frank et al., Mankiw, Samuelson/Nordhaus and other textbook authors, this does not tell us anything about *how exactly* the process of shaping a certain subjectivity is ultimately designed, carried out and perceived. In the volume mentioned above, Silja Graupe addresses precisely this issue of the *modus operandi* of subjectivation or, as she calls it, of influencing processes. She shows that in the introductory chapters of the textbooks by Mankiw/Taylor and Samuelson/Nordhaus

⁶ This is one possible explanation for the fact, that eminent economics textbook literature does not cover important facets of the real world, such as economic crises (Kapeller/Ötsch 2010), or only covers them in a paradigmatically pre-determined way (Liu et al., 2019 with reference to climate change).

⁷ An in-depth analysis of the process of the creation of the first 10 editions of Samuelson’s textbook suggests that political considerations had an important influence on the development of the book (Giraud, 2014).

alone, over ten linguistic techniques known to the cognitive sciences are implemented, all of which have in common the ability to fundamentally change the emotionality, personality and value base of the readers exposed to them (Graupe, 2017: Section 4.1; see also Graupe & Steffestun 2018). The fact that at least Mankiw & Taylor (2014, p. 17) have some knowledge of the kind of effect of their textbook has is suggested by their didactic orientation towards so called “threshold concepts” by Meyer and Land, who characterize the potential impact of such concepts as follows:

“We would argue further that as students acquire threshold concepts, and extend their use of language in relation to these concepts, there occurs also a shift in the learner’s subjectivity, a repositioning of the self.” (Meyer & Land, 2005, p. 374).

“The shift in perspective may lead to a transformation of personal identity, a reconstruction of subjectivity. In such instances a transformed perspective is likely to involve an affective component – a shift in values, feeling or attitude” (Meyer & Land, 2003, p. 4).

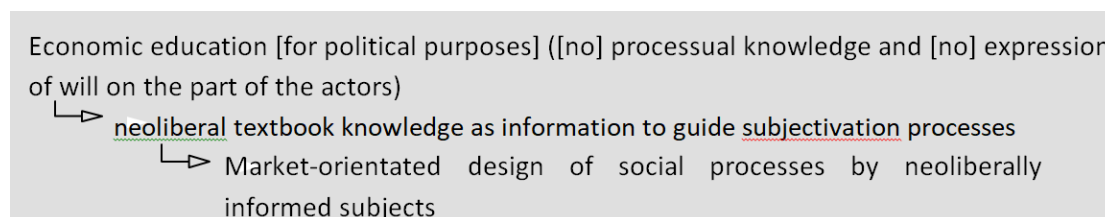
Although these remarkably overt references and the findings of Graupe suggest that the didactical editing of the textbook has undergone an exact weighing against the background of their persuasive potential, it seems important to me at this point to stress that intentionality on the part of textbook authors is by no means necessary for (economic) education to have a productive effect in the above-mentioned sense. If students are primarily informed rather than educated, it certainly helps the underlying subjectivation process if it is *not* consciously being addressed or recognized. In this sense, also teachers, faculties or publishers can assume the role of *recipients* of information (of curricula, PowerPoint slide sets, material to be dealt with) and thus pick up and promote what is currently given, normal, dominant.⁸ An already established discursive power in terms of content and structure can thus be consolidated and expanded without conscious decisions by individual discourse participants.

This brings us to the adjective in the title of this essay. In my opinion, the knowledge conveyed in economic textbooks can be described as “putative” if the concept of knowledge is to contain a certain essence of consciousness; strictly speaking, a *consciousness of process* regarding the genesis and thus also the limits of the known. Such a processual awareness existed in the context of knowledge production in the 19th century. Cognitive processes were closely observed and controlled in order to attain pure, objective knowledge. This consciously controlled quality of knowledge is lost in the moment it is elevated to the status of an “eternal truth” and becomes, as it were, a blueprint for the creation of the world. The actors in this process – in this case students – usually have no awareness of the process in which they are involved when learning “eternal truths”. The textbooks examined here, at least, do not contain any possibilities with which they can enlighten themselves about or distance themselves from the peculiarities of a productive understanding of knowledge. In this way, students take part in a process they are not able to understand. Luckily, as recent empirical subjectivation research finds, students do not take their teachers’ stories for granted at all but rather develop creative ways in dealing with a curriculum that does not serve their original interests (Pühringer & Bäuerle, 2019). Nevertheless, a risk of abandoning their own will by accepting a will that is initially foreign to them remains. And this is precisely what the specific intention of the “will to

⁸ Sociology of science attests, that economics in particular has a strong tendency towards such self-referential, academic modes of reproduction that amplify the same signal (Maeße, 2013).

power” entails: “The will which aims at power and which acts in power seeks the will of others as a counterpart. The former aims at overcoming the latter as will” (Gerhardt, 1996, p. 25; my translation). At the threshold of this overcoming lie the “eternal truths of economics”, which at the moment of their acceptance and reproduction allow individuals to emerge as economic subjects.

Figure 5 Hierarchy of will and knowledge of contemporary economic theory formation based on Mirowski & Nik-Khah (2017)



8. Conclusion

The will and knowledge of early neoclassical economists, according to the thesis developed here, was epistemic in nature. Early neoclassical knowledge was the result of an epistemic process executed on the basis of conscious decisions.⁹ The driver of this epistemic process was the “will to willlessness” on the part of the scientific subject, which formed itself according to the epistemic virtue at hand – right up to its own self-banishment from the cognitive process. Subjectivity was considered a disturbance in the realization of the epistemic virtue of objectivity. On the other hand, the knowledge of important contemporary economic textbooks, such as those quoted here, must be systematically distinguished from epistemic processes. The knowledge contained in them is not the result of an epistemic process, but an imperative blueprint for the production of economic subjectivity among readers. (Economic) subjectivity thus no longer appears as a danger to (objective) knowledge, but as a continuous imperative in a market-shaped world.

Nevertheless, as the present essay suggests, with the study of the history of economics, as well as with the theoretical penetration of its epistemological preconditions, there exist ways and means to break through the boundaries of this understanding of knowledge as well as through those of objective, apparently selfless modes of knowledge. This study can show that the formation of this or that understanding of knowledge is based on decisions that are by no means already decided, but can be judged and made again and again by people. This freedom cannot be deprived of the human will and is a constitutive cornerstone of enlightenment. And this freedom can certainly not only be practiced in order to alter economic thinking, but in order to transform collective economic action in a willful, conscious manner. To see the self-declared truths of economists as one of the major threats to enlightened, critical sociality and individuality will be crucial in the sense of preserving and strengthening the latter, because:

“The truth, as conceived by modern economists, has not set anyone free.
Instead, it brought about the death of the Kantian subject, and a subsequent

⁹ Of course, this does not mean that the decisions automatically led to epistemic processes fulfilling the self-declared criteria, norms or “virtues” (cf. Mirowski, 1989, pp. 229-31, 272-74).

lifeworld hollowed out the humanist concerns that many people mistakenly think are heart and soul of a science of economics.” (Mirowski & Nik-Khah, 2017, p. 2).

With a strengthening of this kind of willful judgement in economic education, perhaps economists could again contribute to an awareness of forms of knowledge of the economic, which not least enable for a responsible shaping of social processes in a time driven by manifold crises.

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World population: the elephant in the living room

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Introduction

There is no doubt that the climate change and the unequal distribution of income (and wealth) are the two major problems of our time with tragic consequences if we fail to deal with them in time and in the right way. However, the general public is either not interested or not informed or feels powerless and therefore indifferent and inactive. In recent worldwide demonstrations it is estimated that four million people participated which is only one in two thousand and, among the young generation, two in a thousand.

Scientists, and among them economists and ecologists, predict that in the next decades, if we remain inactive, dramatic changes with tragic consequences, such as natural catastrophes, famines, wars, local conflicts, social unrest and even extinction of the human race within the next one hundred years (Fenner, reported by Firth, 2010) will take place. The ideas suggested by economists and ecologists to cope with the two problems mentioned above have usually been grouped in four action plans, namely New Economics, Green Growth, Degrowth and Steady State Economy. Sometimes they are referred to as theories or hypotheses but these are misnomers because they refer to the future and they cannot be tested empirically.

The purpose of this paper is to comment on the above policy plans, to defend the steady state economy (SSE) proposal and suggest a variation of it that will make the implied policy plan more viable. It is intended as a constructive contribution to Herman Daly's perspective by introducing in the steady state economy the condition of population stability at the optimal size.

Causes and cures of the environmental problem

The obvious cause of environmental degradation and of climate change, specifically, is the growth of GDP. The root cause behind GDP growth is, according to some writers, a fixation with economic growth and a generalized culture of greed for higher profits and higher consumption levels. Other writers repeat the Marxian thesis that growth is the essence of the capitalist system and growth is simply unavoidable as long as the capitalist organization of society remains dominant. Another "cause" of the problem is, by implication, the inability of technological innovations or of their application to production to proceed fast enough and to make possible a relative or absolute decoupling of production and resource use.

A summary index of the environmental condition of our planet is the difference between biocapacity (BC) and the ecological footprint (EF). Table 1 presents these differences for every fifth year for the 1961-2015 period. The last year of ecological equilibrium, i.e. $BC=EF$, was 1969 (not shown). From 1970 on the difference is increasing and in 2015 the ecological footprint exceeds biocapacity by 68%. Also presented on Table 1 are the world population (Pop), the gross world product (GWP) and gross world product per capita. Population and

gross world product both increase monotonically and so does GWP per capita. Comparison of GWP with (BC-EF) shows that the two variables are very closely connected. As GWP grows the gap between biocapacity and ecological footprint increases. Also the increase of per capita GWP is closely related to the increasing ecological deficit.¹

Table 1 Population, Gross world product, Ecological footprint, and Biocapacity, 1961-2015.

Year	Population (mil)	GWP (bn USD)	GWP/Population (000 USD)	Ecological footprint (mil ha)	Biocapacity (mil ha)	Biocapacity – Ecological Footprint
1961	3075	11683	3800	7035	9611	2576
1965	3325	14609	4394	8155	9736	1581
1970	3685	19040	5167	10052	9992	–60
1975	4066	23004	5657	11098	10117	–981
1980	4437	27840	6274	12284	10336	–1948
1985	4843	31662	6538	12778	10752	–2026
1990	5285	37887	7169	14221	11056	–3165
1995	5710	42198	7390	14716	11173	–3543
2000	6198	49999	8172	15749	11484	–4265
2005	6517	58108	8196	18001	11691	–6310
2010	6956	65955	9516	19862	11938	–7924
2015	7380	73590	9972	20504	12148	–8356

Sources: (a) World Bank, GDP in 2010 US\$. (b) Global Footprint Network.

The negative relationship between the ecological surplus, i.e. the difference between biocapacity and ecological footprint, is shown in Figure 1. It shows very clearly that every increase in gross world product as a result of population growth or of per capita consumption or both increases the ecological deficit. It also indicates how difficult it is to decouple production from the use of resources.

Figure 2 shows the relationship between gross world product and biocapacity. Large increases in production are associated with very small increases in biocapacity and this implies that modern technologies are much more efficient in the production of commodities than in raising the productivity of resources. It can be interpreted as a practical refutation of the idea that resources are made, not given.

¹ Various publications, such as those of the Intergovernmental Panel on Climate Change (IPCC) and the World Meteorological Organization (WMO), contain very frightening detailed reports about the present condition of the Earth.

Figure 1

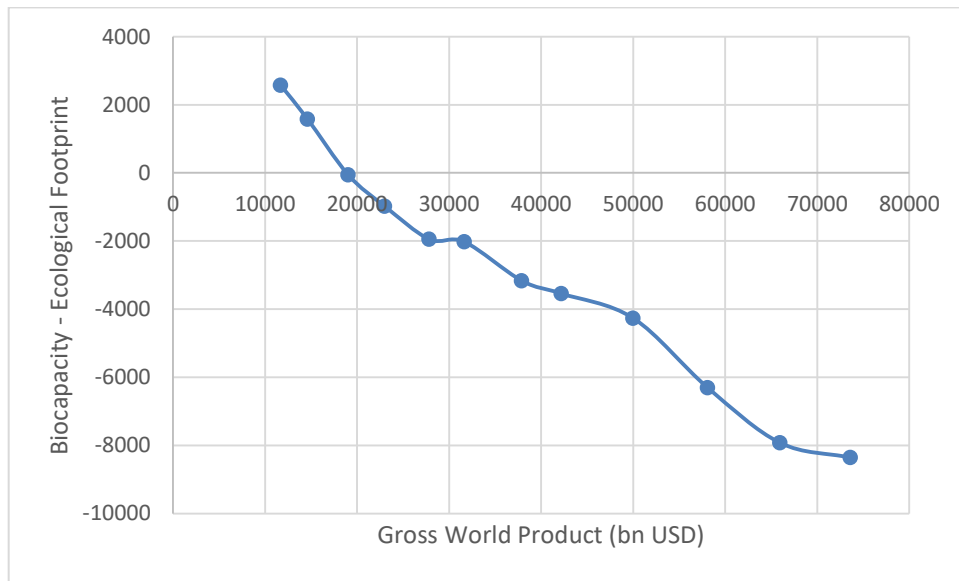
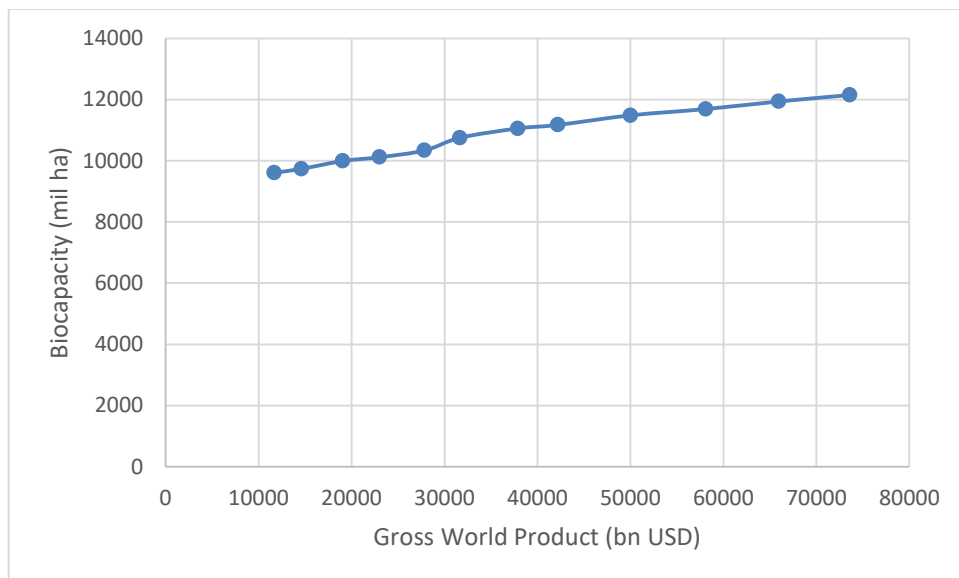


Figure 2



Depending on their interpretation of the cause of the problem various schools of thought have been developed that come under various names, such as Green New Deal, Green Growth, New Economics, Degrowth, Ecomodernists, new Socialism, the Simpler Way, and various action plans or policies have been proposed to minimize the negative impact of producing GDP with or without growth. These proposals include the following:² reduction of fossil fuels, limits on carbon emissions, downscaling affluent economies and material flows, home and commercial insulation, renewable heating, private and public investment to secure a clean economy, decoupling GDP from resource use relative or absolutely, reducing work

² There is a voluminous literature on these issues but a feeling of what is involved can be obtained by consulting the following: NEF(2019), Jackson (2009), Kallis (2011), Kallis, Kerschner and Martinez-Alier (2012), Latouche (2009), Trainer and Alexander (2019), Foster, Clark and York (2011)

hours and sharing available jobs, changing the monetary system, zero interest rates, communal management of resources, a “simpler way” society involving renewal energy and localized production, getting rid of market forces and finally transforming the capitalist system to a socialist one.

Some of these policy suggestions are consistent with others, some are contradictory, some are very imaginative but unrealistic and some imply very drastic changes that are unlikely to be adopted within a reasonable time period. Some of these policies are obviously promising, e.g. house insulation, but others do not seem to make sense within the existing institutional framework, e.g. zero interest rates. One of them, the green growth suggestion, is not supported by the available empirical evidence (Hickel and Kallis, 2019).

It is recognized that some policies, particularly those associated with the degrowth agenda, will cause fierce opposition by powerful interests which will use their political power to repeal serious reforms. But according to Kallis (2011, 2015) degrowth is not just a policy, it is rather a political alternative that seeks popular support for radical changes. Thus, the advocates of degrowth see the solution of the environmental problems in a major restructuring of the socioeconomic system and in that respect degrowth resembles the socialist point of view that the cure of all problems is to be found in a socialist transformation of society.

The elephant in the living room

What is really surprising with the studies mentioned above (and of course with many others) is that they fail to see the protagonist of the environmental drama of our time, i.e. they fail to see or refuse to admit the effect of the world population growth. They do not see the elephant in the room or, for some reason, they do not want to talk about it.³

There are at least three studies using different methods that have come to a similar conclusion, namely that if everyone on the Earth is to have a decent living standard the world population should be reduced to around three billion, i.e. to forty percent of its present size (Daily et al., 1994; Pimentel et al., 1994; Lianos, 2013; Lianos and Pseiridis, 2015). Independently of the exact size of optimal population most scientists would agree that humanity has in its hands, to use Ehrlich’s title, a population bomb. However, politicians and governments never refer to overpopulation and instead of promoting the idea for population reduction they encourage its growth by providing moral support and material subsidies for the third and fourth child. The same is true for religious leaders. Also, there is an unjustified feeling among the general public that population control and reduction is something wicked, and therefore whoever suggest measures in favor of small family size becomes immediately unpopular. Even some academic circles that should not be uninformed show a blatant prejudice against arguments for population reduction.⁴

³ Of course, there are many studies, particularly those who examine environmental problems in relation to ecological Kuznets curve, where population is the central factor. See, for example, Galeotti et al. (2011), Casey and Galor (2017).

⁴ On a personal note, three academic journals that have “population” on their titles turned down a paper of mine in which I was arguing for population reduction without bothering to send it out for peer review because, as the editors said, the subject of my paper, i.e. population, was outside the scope of their journals!

The steady state economy

In the steady state economy (SSE) model population becomes a central factor by been required to remain constant. The best known advocate of SSE is Herman Daly who defines the steady-state economy

“as an economy with constant population and constant stock of capital, maintained by a low rate of throughput that is within the regenerative and assimilative capacities of the ecosystem. This means low birth equal to low death rates, and low production equal to low depreciation rates.... Alternatively, and more operationally, we might define the SSE in terms of a constant flow of throughput at a sustainable (low) level, with population and capital stock free to adjust to whatever size can be maintained by the constant throughput that begins with depletion of low-entropy resources and ends with pollution by high-entropy wastes” (Daly, 2008).

Before Daly, Kenneth Boulding (1964) introduced the Green Stamp Plan in order to control population growth. According to this plan every boy and girl is given 110 stamps which can be sold and bought in the market and thus demand and supply will check population and bring equilibrium. This plan is devised to keep population constant at a time when the world population was approximate 3.3 billion, that is, 56% less than its present size. Of course, the idea of the SSE is much older and can found in the works of Plato (*Laws*) and Aristotle (*Politics*). Both philosophers present well defined models of a sustainable city given the available territory (that is, the equivalent of resources for that time) and adjustment of population so that citizens can enjoy an acceptable standard of living that can be sustained (Lianos, 2016). Also, J. S. Mill (1970) devoted a chapter on the stationary economy.

It should be noted that in the above quotation, Daly actually gives two definitions of the SSE. In the first, population and capital are constant. In the second, it is the flow of throughput which is constant at a sustainable level and population and capital are free to change. The two definitions imply different consequences for the standard of living people can enjoy. If population is kept constant, improvements in productivity will allow higher per capita income whereas the constant flow of throughput may allow bigger population size with a constant per capita income. However, in both cases population controls will be necessary.

The above definitions are not without problems. In the first definition population and capital are required to be constant. However, in a market economy investment (and therefore capital) is determined by market forces and population change is mainly the result of private decisions within families. How can they be kept constant and in the proper proportion except in a command economy? In the second definition, how will it be decided what is the sustainable throughput and if that can be estimated, how is it to be realized in a market economy? Also, what is the proper population size for that level of throughput? It seems to me that Daly's model of a SSE is not realistic except in a command economy (see also Smith, 2010 and Trainer, 2016). In the context of a market economy, the problem of Daly's model of SSE is that it does not have enough constant parameters and thus it remains undefined. Regardless of the way Daly defines it, the SSE does have a realistic version, as shown in the next section.

Also, Daly seems to believe that a SSE will necessarily suffer from unemployment. This follows from his question “If we must stop aggregate growth because it is uneconomic, then

how do we deal with poverty in the SSE?” (Daly, 2008, p.4). His answer is redistribution by putting limits to minimum and maximum incomes. If population is constant there is no need for growth for the purpose of absorbing the increasing labor force. There is no economic argument on the basis of which a SSE will suffer from unemployment just because it is a steady-state. However, unemployment may result from changes in technology or in consumers’ tastes that change the structure of demand and require transfers of labor and resources from one industry to the other. Also, the type of redistribution suggested is questionable. A limit on maximum income would create problems of economic motivation and of bureaucracy. It would also keep the minimum limit low. Redistribution of income can take place through a system of taxes and subsidies and other means depending on the inventiveness of the government.

One major point in Daly’s list of ten-point policy summary is that “the SSE could benefit from a move away from our fractional reserve banking system toward 100% reserve requirements. His slogan is “Nationalize money, not banks” (2017). This can be achieved by treating differently demand deposits from time deposits. For demand deposits the reserve requirements would be 100%. In this case, however, consumers and business would deposit money only for security and for their transactions. Also, the banks would have to charge a fee and this would be their only source of revenues from accepting and handling demand deposits and this may discourage people to deposit. In the case of time deposits (savings accounts), according to Daly, there would be no required reserve and all savings can be loaned to potential borrowers. The banks will profit from the difference between the interest rate paid by borrowers and received by savers. Now, banks would bring together savers and borrowers but they cannot change the money supply and the risk of financial crisis disappears. This suggestion is not without problems. There are two important cases where Daly’s suggestion appears to be too restrictive. One case has to do with the time structure of time deposits that may not coincide with that of the demand from borrowers. In this case the banking system will leave borrowers unsatisfied while time deposits are resting within the banks. The other important case is the inability of the system to finance new firms. In a capitalist steady-state economy there will certainly be changes in consumers’ tastes, new products will be introduced and new technologies will be applied to production. Therefore, new firms will be created and old ones will disappear. A banking system with 100% reserve requirement will make difficult the financing of new firms. The stability of the financial system can be protected by other means without sacrificing the advantages of fractional reserves.

Daly offers a few other policy suggestions that might improve the existing situation in many countries. However, they do not define a steady-state economy. The heart of the matter is the size of population that needs to be determined at a level that would be in harmony with ecological balance. This raises the question of what is the optimal population size and, if it can be determined, how can it be achieved.

Steady state economy with optimal population size

The steady state economy with optimal population (SSEOP) can be presented by splitting the analysis in two sections and examining first the transition period and then the final state.

The transition period

In the transition period two constant parameters are involved, the maximum GDP (Y^*) compatible with ecological equilibrium and a socially accepted standard of living (SL^*). In reality these two quantities are variables but for a given point in time they can be assumed constant, and for the purpose of analysis constant at their present values.

The maximum GDP can be estimated using the data for the ecological footprint and the biocapacity of the planet. The acceptable standard of living requires a consensus that may be difficult to be universally accepted. But reasonable and informed people can easily agree on a level of income that allows a comfortable living, as for example the level of income enjoyed by the average citizen of the less developed European countries. On the basis of 2015 data, if the ecological footprint were to be equal to biocapacity the gross world product (Y^*) should be 43.4 trillion US\$ instead of 73,6 trillion. Accepting an average GDP per capita of 15 thousand which is approximately that of the less wealthy countries of Europe gives an optimal population of 2.89 bl. The reduction of population to sustainable level may take several generations depending on the annual rate of reduction. One cannot escape the conclusion that humanity has come to a very critical stage where tough decisions must be taken.

When the optimal population has been reached and the transition period has ended the size of population should become the constant parameter and all other elements of the model become variables and may be free to change depending on the rate and the type of technological change, and the changes in consumer preferences, on the condition that ecological equilibrium is observed.

The transition period to the SSE will not be a walk in the Athenian Agora or in the gardens of Versailles. It will raise serious problems and it will necessitate inventing proper government policies. The problems related to the reduction of aggregate effective demand and to the viability of pension funds (public and private) because of changing age structure of the population are often cited as the most obvious.⁵ But it is better to face difficult problem that can be solved, even in less than perfect ways, than let the present tendencies destroy the ecosystem and undermine the well-being of the now younger and the future generations.

It is worth noting that the transition to SSE does not need to reduce per capita GDP if population reduces faster than GDP. Thus the effects of degrowth in terms of declining GDP may not have the effect of lowering per capita welfare.

The steady state economy in equilibrium

When the transition period comes to an end, the economy can be said to be in equilibrium with optimal population, a satisfactory standard of living and ecological equilibrium. As soon as that situation is reached, the SSE requires only two restrictions, namely the size of population to remain constant and ecological equilibrium to be observed. Technological advances that increase the productivity of inputs, including labor, without harming the environment may lead to higher production and thus to a higher standard of living or in more leisure. Also changes in people's preferences may increase the standard of living if the structure of demand favors commodities and services for the production of which fewer

⁵ For a recent review of the problems and benefits of population decline see Gotmark et al., 2018.

resources are needed. In general, as long as population remains constant, all disturbances causing disequilibrium will be self-adjusting.

The brief description of the SSEOP given above raises two difficult questions. First, how population can be reduced so drastically during the transition period? Second, how will the economy function after reaching the equilibrium position, or, as the question is often put, is the steady state economy a socialist or a capitalist economy? This question is often stated in the form: can a SSE be capitalist?

How can population be reduced?

From the ancient times to the present several ways have been suggested for population control including moral abstinence, guidance to the young, delaying marriages, availability of contraceptives, abortions by consent, voluntary sterilization, economic incentives and disincentives, and coercion. The fact is, however, that these methods to the extent they were applied they have not given the expected results. Actually, in some countries incentives have been given for population growth rather than reduction. It is often suggested (e.g. Conly, 2016) that education and economic incentives may be effective. However, as was mentioned before, there are powerful interests (Churches, the military, politicians, etc.) that favor population growth and therefore attempts to reduce the size of population will meet fierce resistance.

Another way for reducing world population (very unlikely to be adopted at the present time but when the disastrous effects of population growth become more apparent may become necessary) is by monetizing the problem and creating a market for human reproduction rights. One model for implementing such a program can be described as follows.⁶

Every couple is given three shares by the government, with each share giving the right to give birth to half a child. Each share represents the right of the couple to participate in the creation of the next generation and all couples have the same rights.

These rights are tradable in the world market. Thus, a couple in Canada that wishes to have two children can buy one share from a couple in China. Similarly, a couple that wishes to have three children would have to buy three shares etc. If all couples wish to have two children no trade will take place and therefore the one-and-a-half policy becomes in practice one-child policy. However, it is certain that there will be people in all countries that would be willing to buy and others than would willing to sell shares. Thus, the one-and-a-half child program will at the same time become a program of income transfers probably from relatively rich people to relatively poor, within each country and between countries.

This plan has two advantages and one important disadvantage. The advantages are that essentially it would be cost free and it treats everybody equally. The disadvantage is that it is coercive. Of course, controlling the family size in this way violates a basic human right. Many people would be very skeptical about introducing laws that forces families to reduce the

⁶ For a more extensive exposition see Lianos (2017; 2018).

number of their offspring. For example, Conly (2016), who rejects the claim that people have a fundamental right to have as many children as they want, refuses to accept enforcements on the number of people that a family may have. However, the offence of this violation should be weighed against the alternatives. There is, also, an intergenerational social justice issue involved in this discussion. By forcing people to have less children than they might want to have will certainly reduce the level of utility (happiness) they enjoy. However, if they are allowed to have as many children as they want, the level of utility of the future generations will be much lower given the limited resources that would be available to them just because the present generation contributes to overpopulation. In a real sense, the present generation by its numbers and its consumption habits is using resources that will be lost for the future generations. This is no different from the act of a thief who steals corn from the barn of a neighbor. Although freedom is a fundamental right the thief is imprisoned. One might say that the comparison is not valid because the present generation has no intention of stealing resources from the future generations or harming the natural environment and therefore there is no deceit involved. This defense is not convincing because it is difficult now to find people that are not aware of the critical situation to which the Earth has been brought because of overpopulation. Finally, it should be pointed out that a policy or a rule, if applied generally, is not conceived by the public as a coercive restriction. We do not feel that our freedom of choice is violated when we are required by law to enroll our children to school or to drive on one side of the road or even to fight in a war and be forced to kill.

In defense of this plan I would like to quote J. S. Mill's "very simple principle" that "the sole end for which mankind are warranted individually or collectively in interfering with the liberty of action of any other member is self-protection. That the only purpose for which power can be rightfully exercised over any member of a civilized community, against his will, is to prevent harm to others" (Mill, 1961). It is clear from the analysis presented in this paper that the sole purpose of the one-and-a-half child policy is to prevent the present generation from harming the next ones.

In the history of the world, social problems have been solved or were limited to manageable proportions by command rules, by economic incentives, and by a combination of both. Of course, monetizing a problem will not necessarily lead to the best solution, but a second-best solution is often better than letting things run their own course. Under the present circumstances, if population growth is left unchecked Parfit's repugnant conclusion will certainly be reached. Our suggestion for the one-and-a-half child policy is a combination of command and economics that also allows some choice.

Can a steady state economy be a capitalist economy?

This question has been asked recently by Richard Smith (2016) in a critique of Daly's version of steady state economy and his answer is in the negative. His answer is based on a brief analysis of three basic characteristics of capitalism. First, the producers are dependent on the market. Second, competition is the motor of economic development. Third, "grow or die" is a law of survival in the marketplace. In short, he concludes that "the growth imperative is virtually a law of nature, built-into any conceivable capitalism. Corporations have no choice but to seek to grow" (p. 31). He ends his paper with a dramatic appeal: "It's time to abandon the fantasy of a steady state economy, go back to the drawing boards and come up with a *real* "new macroeconomic model", a practical, workable post-capitalist ecological economy, an economy by the people, for the people, that is geared to production for need, not for profit.

“Socialism?”, “Economic democracy?” Call it what you like. But what other choice do we have? Either we save capitalism or we save ourselves. We can’t save both” (p. 42).

It seems to me that Smith and many other authors who argue in a similar way are right in saying that in capitalism firms must grow in order to avoid the danger of being forced out of the market by competitors. The need to grow and survive makes profit maximization an economic law. However, they neglect the factors that make profits possible. In brief, profits are the result of three factors: (1) Technological advances and applications in the production process and/or management that give an advantage over competitors, (2) extension of the market because of exports or changes consumers’ taste or increasing population, and (3) abundant labor supply that allows, in Marxian terminology, the extraction of surplus value.

The core of the capitalist system is labor exploitation and the extraction of surplus value. Exploitation and surplus value are uniquely related to abundant labor supplies. Capitalists, more than politicians, religious leaders and the military, want an increasing labor supply that will be exploited to produce surplus value and at the same time extend the market. The huge migration flows from relatively poor European countries to USA, Canada, Australia and the European North during the twentieth century, provide evidence for the need of capitalism for labor supply and hence for increasing population. Jason Hickel’s (2019) “scarcity machine” actually provides examples of some instances (as the enclosures in England and the European colonization of Africa) where people were artificially deprived of their means of subsistence and they were forced to supply their labor to capitalist enterprises for low wages.

The argument I am going to present is intended to show that a steady state economy with constant population can be capitalist. Suppose that the long run equilibrium of the economy is reached, that is, GDP is at the level that guarantees ecological equilibrium and population is constant at a level that, given the technology of production, is just sufficient to produce GDP. In a steady state economy the wage rate and the profit rate are negatively related. A higher (lower) wage rate means a lower (higher) profit rate. If the profit rate is zero and technology and consumer tastes remain constant, capitalists will receive their normal profit, i.e. profits which are just sufficient to induce them to remain in the industry. In this case, wages are at the maximum that can be attained in a capitalist economy. Capital owners will cover their costs, replace depreciated capital and receive their normal profit.

If profits are positive or if there are opportunities for positive profits (because of technological progress or shifts in consumer preferences) capitalists will try to exploit these opportunities, but this at the same time will raise the demand for labor and, given that population (and therefore labor supply) is constant, wages will increase and the expected profits will fall. At the new equilibrium, profits will fall to their normal level and wages will increase to the maximum attainable level. Consequently, an obvious and important byproduct of this process will be a huge improvement in the distribution of income.

The story I am reciting may be seen as a typical textbook perfect competition model. That is true, but holding population as a constant parameter gives drastically different results. This is an important result that partly explains why many economists do not discuss population constancy or reduction even in the face of the threat of environmental catastrophe. Also, it explains the pro-natalist culture that has been promoted throughout the world⁷.

⁷ It is not implied that this is the only factor that contribute to population growth.

Can a steady state economy be a socialist economy?

In principle, a steady state economy with optimal population can certainly be a socialist economy. However, an exact model of such an economy cannot be specified because it is not always clear how a socialist economy is defined. If a socialist economy is defined as one with private capital ownership but with an extensive public welfare system, the answer is in the positive and in fact a plan for population reduction has more chances to be materialized than in the capitalist economy. If a socialist economy is defined as a command economy, that is an economy whose structure and activities are decided by a central planner, the steady state economy would be much easier to implement but at the expense of individual liberties, depending on the administrative powers of the central planner.

The steady state economy in other socioeconomic systems

The growing ecological problems and the increasing economic inequalities worldwide have led to a search for alternative ways of organizing society. Terms like “Economic Democracy” (Smith, 2010, p. 41), “Direct and Popular Democracy” (Kallis, 2015, p. 4), are often used but their content is not specified. The same is true for “The Simpler Way” that suggests “a small scale, highly self-sufficient, self-governing and primarily collectivist local economy” (Trainer, 2016, p.62), and also for the “Radical Ecological Democracy” that suggest “collectives and communities at the center of governance and economy” (Kothari, 2014).

It is difficult to see if these suggestions lead to viable and efficient alternative ways of social organization but no foreseeable factor should prevent these alternatives from achieving a steady state economy status.

Conclusion

All the environmental problems created by the human activities are wholly and solely related to the increase of the world GDP. With given technology and consumer preferences, any improvement in the condition of the environment has to come from a reduction of GDP. This is undeniable and is the basis on which the degrowth literature as well as that of the steady state economy are based. The degrowth approach begins with a reduction in GDP ignoring the fact that this will immediately reduce per capita income if population remains constant or if it reduces relatively slowly. Thus, degrowth goes hand in hand with a reduction of welfare. In contrast, the steady state economy suggestion begins with a reduction in the size of world population which will in turn reduce GDP but not per capita income. The criticism that a steady state economy cannot be a capitalist economy does not seem to be valid. In fact, the argument made above is that the steady state economy is compatible with a variety of social systems.

Prospects for the future

Given the size and the urgency of the environmental problems (of which the climate change seems to be the most threatening), it is reasonable for practical men to ask what are the prospects for the future. The evidence from the recent past suggests that policies for GDP reduction or stability in a world scale are very unlikely to be adopted in the near future. Any

government that attempts to follow or simply suggests such policies will fall the next day. Policies for population reduction may be a little less unpopular in some countries but they are also unlikely to be seriously considered. At present, there is no reason to have any ray of hope for a better future, no reason to be optimistic.

The only hope we can have is a *Deus ex machina* or rather a *Deus ex technologia*. If we can have enough solar, wind and nuclear energy within a reasonable time period the disastrous climate change may be avoided. However, although that will be a relief it will not solve the scarcity problem. Growing GDP to allow a growing standard of living of a growing world population will very soon reach the limits of resource availability. In fact, we are already at that stage.

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The carbon economy – rebuilding the building blocks of economics and science

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Over 500 astronauts have had the privilege of observing Earth from space, and some have reported what must be acknowledged as a new “worldview” of human existence in its physical environment. Fresh application of what we know in science to the overview of human activity gives revolutionary insights. The Earth’s ecosystems existed before and without humans. The water cycle and carbon cycle operate naturally. White (2014) has observed that the astronauts’ “Overview Effect” of the dynamic natural exchanges is humbling and mind changing. It raises the thought that it is merely the human-generated activity observed as an extra overlay which can be subjected to economic analysis.

A modern Copernican view of economics is that not man and money are the center of the world, but that the textbook macroeconomic cycle of goods and services produced and consumed is a mere cog in the global carbon cycle. Modern science can now view the planet as a materially finite complex set of ecosystems, take stock of the key elements – carbon, oxygen and hydrogen – and garner a pragmatic picture of wellbeing. Much clearer and a quite different perspective from the self-centered presumption set out in *The Wealth of Nations*.

Money is a moving measuring stick

There is a rising crescendo of disbelief among commentators from many fields on the value of National Accounts and the failures of economics to reflect or encourage what is good for wellbeing.¹ The “P” for “Production” in the GDP originally assumed that goods and services would be paid for by consumers who had done their own productive work elsewhere in the macroeconomic cycle. But now that circle has gone pear-shaped into a pyramid of debt. Stimulus favors the rich. \$5 in Harlem has to go further than \$5 in The Hamptons.

The concept of the flows of economic produce, measured in money (Quesnay, 1758) preceded science’s discovery of atoms (Dalton, 1802), entropy (Carnot, 1824) physical work (Coriolis, 1826), and the quantification of energy (Joule, 1848). Because of the precedence of economics over science, money has become the default object of attention, with strategies of manipulating money rather than producing and consuming goods and services. Now economic decisions are made that are unhinged from the material world they are supposed to represent. Analogous to Plato’s cave allegory, money is the mere (distorted and enlarged) shadow of actual reality. And as material reality depreciates, the shadows cast are being made bigger and given hyperbolic interpretations by financial gurus scanning the swirls of indicators for positive signs.

It is crucial to correcting current misunderstandings to expose the physical reality – to set it out in the open. In line with Plato’s prediction, accountants will fail to recognize real

¹ <http://www.thesolutionsjournal.com/node/237446>.

commodities without the shadows they projected, but scientists can assess the real state of the physical economy in the context of the Earth's natural operating systems.

Carbon economy and carbon cycle

Human activity takes place in a space that is oddly shaped and seemed beyond relevance, if not comprehension to the early economists. The planet's biosphere is a volume comparable in shape to an apple skin – a layer limited to some kilometers above and below sea level – and for all practical purposes of economic analysis has no imports of goods or exports of waste. Oxygen and hydrogen are by far the main 2 elements and carbon holds to key to life and economy, trading its bonds with hydrogen for new bonds with oxygen, driving metabolism and motion and economic production.

To simulate what we know of land, sea and air, an aquarium-style prism serves well, simply acknowledging it is not to scale, and does not feature the circulation of sea and air around the globe. This is the model adopted by the UN Intergovernmental Panel on Climate Change.² The UNIPCC measures flows of carbon. The 2 main natural flows are the ocean-atmosphere gas exchange and the photosynthesis / respiration on land.

This natural carbon cycle is harnessed by humans to drive their circular economy of production and consumption. There is an insightful way set out by Yang and Zhang (2016), not using carbon atoms, but carbon's chemical bond exchange from hydrogen to oxygen. There is a realization that the circulatory systems of Earth can be harnessed and the human economy has been a small spinoff, with for many thousands of years up to 1750, the carbon bond trading by humans mainly only from organic carbon – carbohydrates, proteins and various forms of biomass. The industrial revolution seemed to introduce totally new technologies but the driving force of economic activity remained the carbon for oxygen trade. Adopting this perspective, the global carbon cycle and the circular flow of what economists think of as production and consumption can be fitted on the same page with the same units. The global economy exchanges carbon for oxygen and can be accounted on a yearly basis. A summary, lumping land and ocean transactions together, is presented in Figure 1.

Although IPCC researchers are on the right track counting carbon atoms, it is more useful to identify carbon atoms' in chemical bonds. Different fuels and different technologies use carbon atoms differently, and a more accurate picture is given by considering carbon as a catalyst and accounting for the number of bonds broken in oxygen molecules.

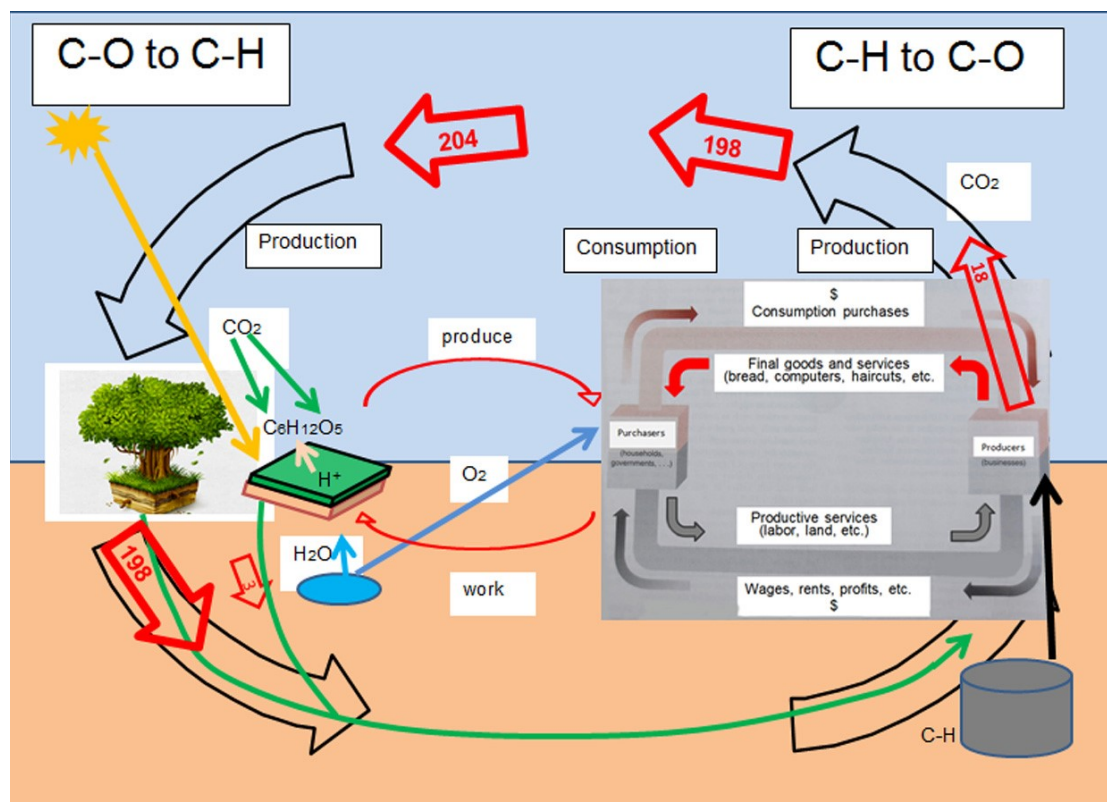
“Work” in physics and economics

It is a useful, pertinent question to inquire, what do people on Earth do? The objective, scientific answer is that they “do work”. “Work” was defined in physics in 1826 by Coriolis as the force expended (by a person, animal or machine) to lift a heavy bucket up a mine shaft against the force of gravity. Work is force times distance, $W=Fd$. To examine the economists “circular flow of the macroeconomy” in a clear objective way, all the goods and services said to be “produced” are simply the embodiment of the work done in converting C-H bonds in foods and fossil fuels to C-O bonds. There is no other cost – not wind, hydro, solar,

² http://www.ipcc.ch/pdf/assessment-report/ar5/wg1/WG1AR5_Chapter06_FINAL.pdf.

geothermal, tidal energy are all free. The exception is nuclear energy which is excluded here for simplicity.

Figure 1 The “circular flow of the macroeconomy” (Samuelson’s canonical textbook 2009) fitted into the global carbon cycle. The numbers refer to estimate for 2014 of oxygen-oxygen bonds broken to form carbon-oxygen bonds, times 10^{38} .



It is a reflection of 18th century perspectives that wind, hydro- and solar power are classed as “energy” inputs to economic processes. It is solely the man-made material and man-derived abstract technology that is the cost of harnessing the forces of natural ecosystems. Even hydrocarbons and carbohydrates have an *in situ* status in a reference system where they can be regarded as free. It is the work done in transforming them to fuel and food that can be regarded as costs.

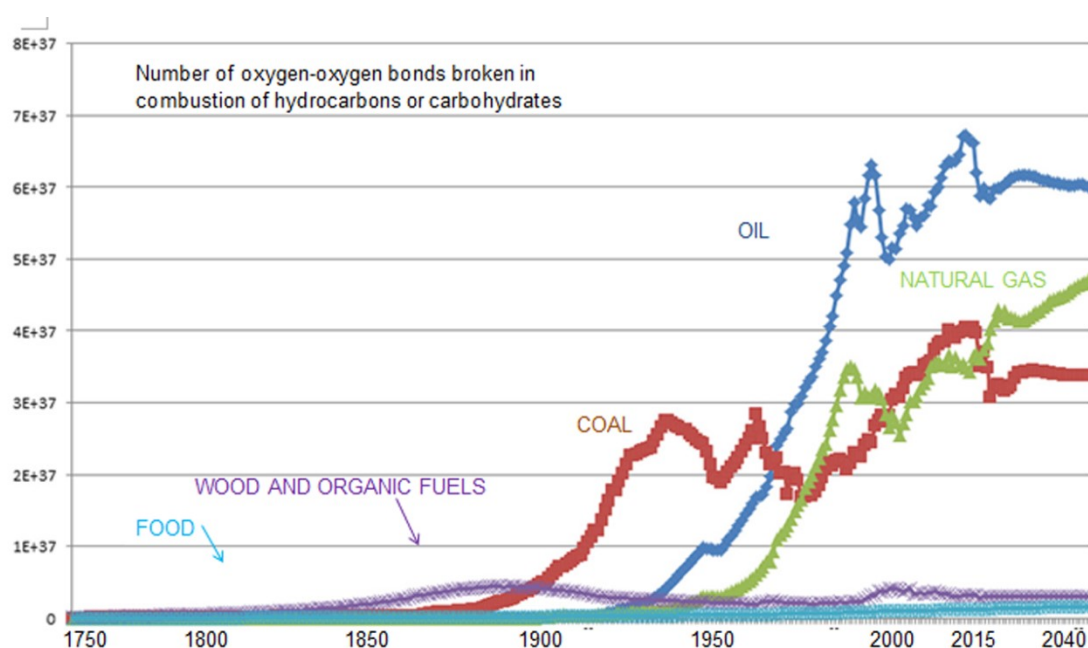
Viewed historically the aggregation of forces at work is the only input to an economic process. Building on the original classic example of the relative cost of hunting a deer or beaver, the cost of the hunting tool is also in work done making it. What economists consider to be capital equipment Costanza (1980) showed can also be recognized as accumulated embodied work done – work done not only by human labor but also done by chemical reactions. From the cost of producing individual materials (Cole and Kernan, 1996) to global production (Gutowski et al., 2013) whole cities and national economies have risen up out of raw materials by work done on them. In 1843 Joule introduced the “mechanical equivalent of heat” but in modern science “heat” is a sensation and temperature measures the work done on molecules (of mercury or some standard substance) set between benchmarks chosen by humans. When we cook with gas, or refine iron ore in blast furnaces, at the scale of molecules, work is being done.

A study of the US economy serves as a useful example. Taking 1750 as a starting point, there was no national economy – merely economic activity of villages and small regions of minimal impact. The two drivers of economic activity were food for human work and firewood for boilers in primitive industry. With the advent of the steam engine it took another 110 years before coal overtook firewood as the fuel for steam engines. Another 90 years, petroleum surpassed coal as the largest component of the American economy's appetite. Ten years later natural gas also overtook coal and even as the economy burgeoned, natural gas stayed more important than coal.

A work theory of costs

There is an amazing truth that economists were too blind to see in America's development. In the Old World, economists began in the eighteenth century with an assumption that the factors of production were land and labor. The "Labor Theory of Value" was popular. With our knowledge of science now it would have better mileage as the Work Theory of Costs. Then as mechanization was introduced, the factors were called land, labor and capital, and large machines supplanted labor's value. But with the fresh start in the New World, an original thinking observer could note that the "land" had been there for millennia, and the changes could be monitored simply by tracking the carbon processes – first in farm production and consumption, and the combustion of firewood, then with the fossil fuels. At the level of atoms, carbon-hydrogen bond attractive force being broken and releasing part of that force as oxygen bonded with carbon is a common denominator – a universal base currency. This is graphed from 1750 to 2015, with the Y axis units denoting 10^{37} carbon bond exchanges (also counted as oxygen molecules broken).

Figure 2. The US economy 1750 to 2014 and projected to 2040, driven by carbohydrates and hydrocarbons. Y axis is number of O_2 molecule bonds broken in the forming of C-H bonds



In Old World economics, fuels are a “consumable” added to the fixed cost of capital equipment. But derived from the Overview Effect experienced by astronauts observing the Earth objectively, all activity is directly or indirectly traced to the forces at work, farming, mining and building cities and infrastructure. For the short history of American development, today’s capital equipment was built from yesterday’s activities. What is there now is the result of 265 years of activity. Prigogine used the analogy of the industriousness of ants. An above-ground ant’s nest is a manifestation of the work done by the “ant economy”. In one study a population of 8 million ants built an infrastructure of their “economy” doing the “work” of lifting 40 tons of soil an average elevation of 4 meters.³ Similarly, astronauts flying over continental USA easily appreciate the urban build-up on the east coast, the carefully tended farmlands of the midwest, and the vacant deserts beyond that.

Figure 3 A termite “economy” where the gross “product” (work done) can be estimated as the mass of soil x elevation of center of gravity.



Planning future “work”

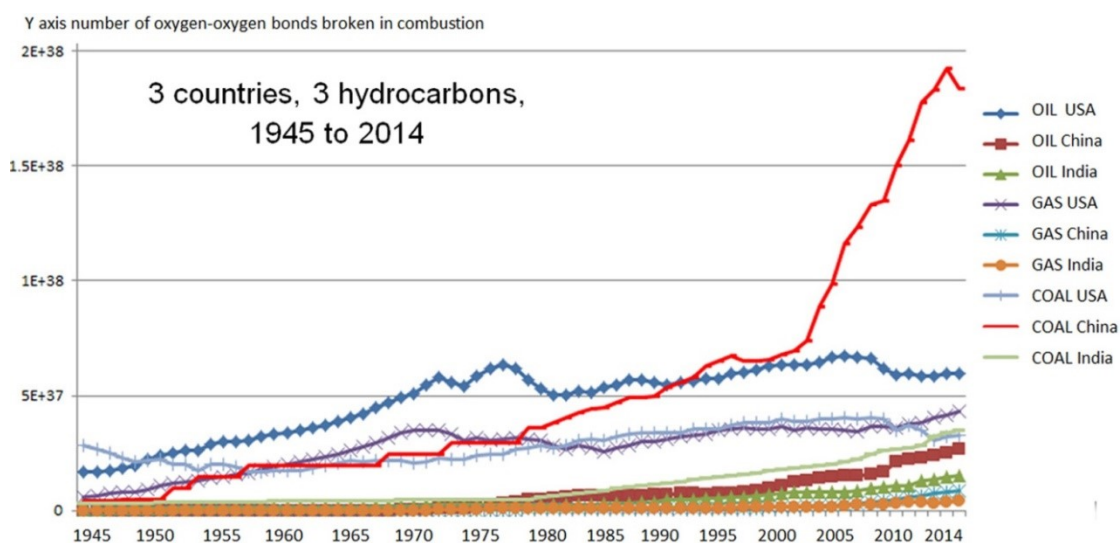
This perspective is important, not because it records history, but because it focusses sharply on questions of what is planned for the future. Using the fuels we have, what work will we do? How can we convert that to capital equipment? We can build on what we have but we cannot change the past. In fact we look at an ants nest and admire the industry that must have been employed but we also do not know if it was knocked down when half built and then rebuilt to its current size. Looking at modern America as a whole from space, we cannot know of the wasted activity of the Civil War of the eighteen sixties or the environmental damage done in the Dust Bowl of the nineteen thirties. But where America is now in development, and where it is headed is written in current and forecast fuel plans.

³ <http://www.swarm-intelligence.it/wordpress/giant-ant/>.

The US government projects how much fuel it will need ahead from historical fact in 2015 through to the future in 2040.⁴ This can be readily adjusted to also include carbohydrate foods. The reality is that in 2015 coal and gas both constituted 25% of all the carbon-hydrogen combusted. The main fuel was oil, 37%, leaving 13% for non-fossil fuel in the form of organic matter for machinery and carbohydrate foods for humans (including that indirect food for animals then directly transformed into complex C-H bonds in protein). The question then arises bluntly for policy makers, what changes can be made to optimize the mix in coming years. It transpires that despite boasting low carbon economy policies, there in the spreadsheets of the government agencies, coal in the United States will still be a major component driving the economy in 2040. Converting the official data published in BTU (British Thermal Units) coal carbon-hydrogen bonds will rise slightly in 2040. It is projected that natural gas will increase 16% in that period.

When analysis of the carbon bond exchanges is applied to other economies, immediate concerns appear that may not show up in orchestrated financial projections. India is unapologetic on its plans to boost coal fired power stations because the need for electricity has priority over pollution. Projections on China vary widely but the plans now being implemented for more coal fire power point to large increases. Officialdom hides behind the positive news that emissions per unit of GDP will decrease without laying on the same page their plans for GDP growth. The faction of economists bent on increasing consumption (of final consumer goods) will escalate coal consumption. If we just take the 3 hydrocarbons, oil, gas and coal, for 3 economies, USA, China and India, and look at the recent past, 1945 to 2014, using the unified, object currency of carbon bond exchanges with oxygen, Figure 4 shows the factual picture:

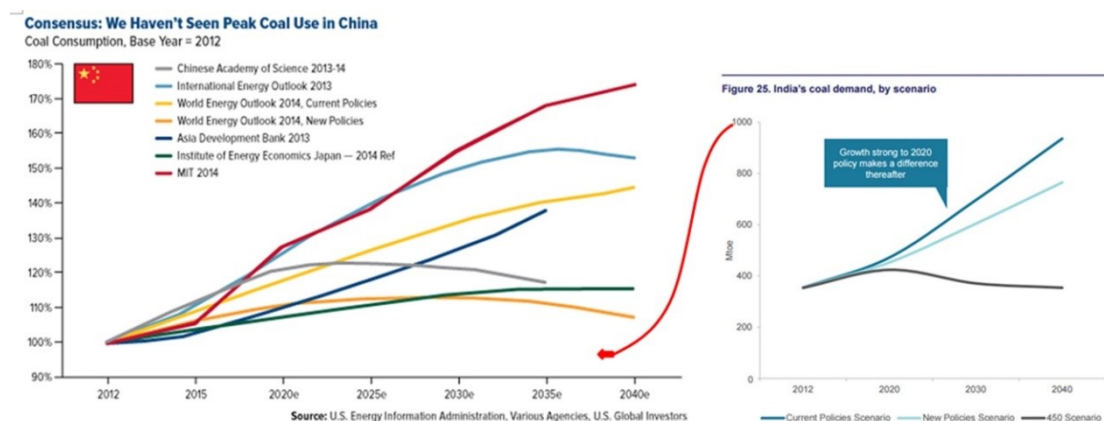
Figure 4. The US oil, gas and coal combusted, copied from Figure 2 for the period 1945 to 2014 with the addition of those 3 hydrocarbons for China and India. Starting 1950 after the establishment of PRC, Chinese coal climbs to overtake US and from 2002 increases dramatically to 2013 where 3.6 billion tons of coal released 8 billion tons of carbon atoms from their C-H bonds to break 1.8×10^{38} O-O bonds.



⁴ <http://www.eia.gov/forecasts/aeo/data/browser/#/?id=1-AEO2015&cases=ref2015&sourcekey=0>.

Where will China go with coal in the next 25 years? What about India? On the one hand governments want to present a responsible picture that does not threaten global environmental health. But in a different arena enterprises are boasting plans and contracts for new power stations, with output stated in gigawatts and rarely likely demand for coal. Thus there is a wide range of projections and the units of measurement vary, making comparisons of estimates confusing, and losing the concepts of scale because each report seems “important”. Figure 5 reproduces 2 graphs: a publication of China coal forecasts to 2040 measured in percentage increase, and a graph from a report showing 3 scenarios for India coal for the same period, measure in tons. In fact the whole of the Indian graph would easily fit under the lines on the China graph, because India’s high scenario reaches 1 billion tons in 2040, a quarter of China’s 2012 consumption.

Figure 5. 2 graphs from different reports on the scenarios for coal consumption in China and India. The two graphs are not to scale and the comparison must be made in the historically factual tonnage of 2012, China 3700 million, India 650 million. In fact India’s highest estimate in 2040 is “only 1000 million, way under the data in the China graph.



The use of units taken from chemistry has the advantage of unifying comparisons, and also makes the discussion “clinically clean”. Dollar values are notoriously fudgable. And the physical units for fuels in mass and volume are also messy to compare. The exact characteristics of “oil” and its byproducts, and the range of energy densities in different types of coal can be used to the advantage of experienced presenters to show the scenario advantageous to themselves. The unit of accounts should not be just carbon atoms, as set out by the IPCC. For policy makers and general public, a universal, scientific, objective currency unit of carbon bonds with hydrogen broken and then joined to oxygen is a concept that needs to be accepted. It is the bond swap carbon does, oxygen for hydrogen that releases a real “stimulus package” of electromagnetic force able to do work. During the industrial revolution biologists, with Pasteur as champion, led the general public to the realization that invisible germs were real and important in managing our daily behavior. These tiny invisible bonds between carbon-hydrogen and between oxygen-oxygen are the foundations of our life blood, and if we can grasp the concept, at the magnitude of human perceptions, kilograms and liters for individual lifestyles, and the strings of 37 digits needed to express national and global accounts, we will better understand the wealth of nations and the costs involved in maintaining and conserving wellbeing.

The diagram of the Circular Flow of Macroeconomic Activity purports to report on the physical production of bread, computers haircuts, “etc.” and then lapses into their “equivalents” in purchase prices (Samuelson, 2009). The gross activity can be counted directly in the universal currency of bond exchange, O-O to O-C. This unit is universal, scientific, objective and cannot be magically created. There are challenges in defining final and intermediate production phases and what to count when. Not a simple exercise but less daunting than the tasks of the army of public servants and private accountants in the National Accounts data supply chain. A first attempt at comparing annual dollar and chemical bond exchange accounting for the USA years 1990-2017 shows how drop off in physical activity signals the Financial Crisis that appears in GDP data a year later.⁵ This approach of measuring physical activity has now been applied to the Chinese economy 1991-2018 and published in the journal of China’s Ministry of Ecology and Environment (World Environment, 2020.02). The early warning of changes in GDP serves as a useful index for policy planners.

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⁵ <https://www.thesolutionsjournal.com/article/reckoning-gdp-counting-chemical-bond-exchanges/>.

Breaking the golden handcuffs: recreating markets for tenured faculty

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Abstract

The present system of tenure in American colleges – combined with a dramatic rise in the proportion of non-tenure-track faculty – has led to a near-closure of active markets for most tenured faculty. In turn, this produces two types of mismatches: some departments / colleges have faculty they do not want; and some faculty would much prefer to relocate to another department / college. These mismatches are not without costs for professors and departments / colleges. Without abolishing or diluting tenure, colleges can alleviate these inefficiencies by initiating reforms to recreate markets for tenured faculty. This can be accomplished by establishing computerized job exchanges that allow departments / colleges to engage in mutually beneficial trades – across disciplines and across colleges – of tenured professors who are currently mismatched.

Key words college, university, tenure, tenure-track, faculty, professor, market closure, trading professors, inefficiency, markets, professional sports, trading players, expand markets

JEL codes I 21, I 23, L 31, L 33, L 38

1. Introduction

In recent decades, critics of academic tenure have argued that it reduces faculty productivity by creating sinecures; it also restricts the ability of administrators to restructure their colleges in response to changing markets and priorities.¹

While this paper shares these concerns, we take the view that the advantages of tenure outweigh its costs.² Therefore, instead of diluting or abolishing tenure, we should be looking at ways of mitigating the two problems it creates: erosion of faculty incentives and institutional rigidity. Taking a fresh look at the system of tenure for college professors, we argue that it is likely to lead to a contraction in markets for most tenured faculty. In turn, this creates two kinds of faculty mismatches: one from the standpoint of the college and another from the standpoint of the faculty. Over time, a college's inability to fire its tenured faculty is likely to create growing mismatches between its existing faculty and its needs.³ In addition, since

¹ The attacks against tenure have been led mostly by legislators, trustees, and writers from conservative think tanks; in polls a majority of college administrators too express a preference for contractual faculty appointments over tenure. For recent attacks against tenure, see Lindsay (2016), Wetherbe (2013), Riley (2011) and Taylor (2009). In 2011, *Time* weighed in on this issue without taking sides (Rotherham, 2011). In November 1997, the chairman of the Massachusetts Board of Higher Education described tenure as a "scam" and called for its abolition (McPherson and Schapiro, 1999). According to one survey, about 69 percent of administrators at four-year private colleges said, "they would prefer that a majority of faculty work under long-term or annual contracts (Stripling, 2011)." In 2016, the State College of Florida abolished tenure. In 2015, Wisconsin passed legislation that effectively destroys tenure at public colleges and universities; in 2017, bills were introduced to abolish tenure for new hires in Missouri and for all faculty in Iowa [Johnson (2016), Schuman (2016) and Flaherty (2017)]. More recently, Gardner (2018) has written that tenure "faces more peril now than it has in nearly 70 years".

² For the advantages that flow from tenure's protection of academic freedom, see de George (2003), Brown and Kurland (1990), and Van Alstyne (1990).

³ Colleges includes all post-secondary academic institutions, including community colleges, four-year colleges and universities.

stagnation or near-stagnation in the number of tenured positions is likely to lead to the near-closure of active markets for tenured faculty, this is likely to produce mismatches – now from the standpoint of tenured professors – between their current locations and affiliations and where they want to be. It is our contention that colleges can mitigate both of these mismatches by instituting arrangements that allow tenured professors in one department or college to trade their positions with professors based elsewhere. These trades need not affect the security that goes with tenure if the proposed trades are initiated by professors and occur with their consent. If these trades also occur with the consent of the relevant departments and colleges, then all-round gains are assured. In effect, this proposal calls for easing the near-closure of markets for most tenured faculty especially in times of stagnation in the overall size of the faculty: the flexibility this creates should benefit professors, departments and colleges alike.

The rest of this paper is developed in four sections. Section two examines how tenure – under conditions of a stagnant or a slowly expanding pool of tenured positions – creates near-closure of active markets for most tenured faculty: and why this is likely to have an adverse impact on the productivity of tenured faculty. Section three proposes the idea of “trading” tenured professors: it shows that this institutional innovation has the potential for alleviating the dual problems caused by tenure to the degree that it expands active markets for tenured professors. Section four compares our proposal for exchanging tenured professors with the arrangements in professional sports for trading players. A concluding section offers some thoughts on why the several solutions proposed in this paper for increasing the mobility of tenured professors have not been tested at least in the United States.

2. Markets for tenured faculty

Consider how tenure may greatly narrow “active” markets for most tenured faculty, where the active market consists of new hires in any time period. Some of the costs of tenure derive from this narrowing of the active market for tenured professors.

In a labor market with a fixed number of tenured faculty positions over time, labor turnovers could be set in motion by firings, resignations, retirements or deaths.⁴ Given the rarity of firings in academia due to tenure, this is unlikely to be a significant source of turnovers in the ranks of tenured faculty.⁵ Resignations are likely a more important source of turnovers, and these may arise from two sources. Some resignations may occur as faculty leave academia to take up non-academic jobs, but these are likely to be confined to faculty in business, medical and engineering schools. Resignations may also result as top-tier colleges raid each other's faculty as they attempt to improve their academic rankings, but this is likely to create opportunities principally for the “stars” in any field. This leaves us with retirements and deaths as the chief potential sources of turnover for the non-stars in academia. However, it is unlikely that retirements and deaths (especially of senior faculty) will create openings for senior

⁴ According to the report of the American Association of University Professors (AAUP), 2012-2013, the number of full-time tenured and tenure-track positions at American colleges increased by 26 percent between 1975 and 2011, while the number of non-tenure-track positions rose by 300 percent over the same period (Curtis and Thornton, 2013, p. 4). At Harvard University, the size of the Faculty of Arts and Science increased by a mere 2.5 percent between 2008 and 2017; it had increased by 28 percent between 1998 and 2008 (Nakada and XU, 2018).

⁵ According to Hucheson (1996: 13), only two percent of the total turnovers of tenured faculty in 1987 was due to dismissals for cause or retrenchment. In other words, 98 percent of the turnovers were due to retirements, resignations, and deaths.

tenured faculty. Colleges prefer to replace retiring and senior deceased faculty with tenure-track hires. There are several reasons for this. Generally, tenure-track hires will cost less than senior hires; they also give the college administrators greater flexibility since they can be fired before they receive tenure; in fast-changing fields, the administration may prefer to replace retirees with recent graduates in order to stay current; existing faculty too may prefer tenure-track hires since they will not affect their seniority or chances for promotion.⁶ In other words, firings, resignations and retirements will most likely shrink the ranks of tenured faculty, although this shortfall may be made up over time by the granting of tenure to tenure-track faculty.

To summarize: the near closure of active markets for most tenured faculty is the product of two circumstances. The institution of tenure nearly ends the firing of tenured faculty: in and of itself, this greatly reduces the turnovers that are set in motion by firings even in stable job markets. A second factor is also at play. Faculty who retire, die or resign (to take up positions in government or business) are often replaced by appointments in the ranks of tenure-track and adjunct faculty. Apart from the “stars” in the tenured ranks, this greatly restricts the movement all other tenured faculty. The degree of market closure for tenured faculty can be inferred from average years that they have spent in their current positions. According to a report prepared by the American Federation of Teachers (AFT), the tenured faculty at all institutions of higher education in the US spent 16.6 years in their current positions in 1992; in 1998, this had gone up to 18.1 years (AFT, 2003: 15).⁷ Alternatively, the degree of market closure may also be inferred from data on faculty retention. The retention rates for tenured full and associate professors in the State University System of Florida over 1977-1978 were 94.97 and 96.2 percent respectively (Christal and Hector, 1980:3). The immobility of tenured faculty may have worsened in the years following the Great Recession, especially for faculty in the social sciences and humanities. According to Cassuto (2011), there was a 40 percent decline in tenured openings over 2009-2011 in the fields of history and English.

It would appear that only one set of tenured professors are exempt from this market closure. This consists of faculty whose presence in a department visibly enhances its reputation and, therefore, its ability to recruit high-quality students and faculty: they are the “stars”. Colleges ambitious to increase their rank – or the rank of particular departments – will often be willing to offer special deals, including higher salaries, reduced teaching loads, research funding, job for a spouse, etc. – to lure these stars from their current positions. What this means is that most of the tenured faculty who lack this “star” quality are likely, once they receive tenure, to be stuck in the departments and colleges that first gave them tenure. Tenure for them becomes golden handcuffs.

The system of tenure, then, creates two kinds of rigidities: the first is the rigidity in the composition of tenured faculty and, over the last few decades, stagnation or near-stagnation in its size; the second concerns the greatly reduced mobility of tenured faculty that results from the market-closure just described. College administrators are quick to perceive the first

⁶ Note that in an industry with a fixed number of jobs, the firing of one employee may set off a chain reaction. Firm A fires one employee and hires a replacement from firm B. In turn, the employee who left B is replaced by an employee from firm C; and this process goes on. In academia, because of the near-absence of firings from the ranks of tenured faculty, this chain reaction is muted. Further, retirements do not start such a chain reaction since these – as explained earlier – often lead to replacements by tenure-track or adjunct appointments.

⁷ The average years of service in 1992 and 1998 by type of institutions were as follows: private research: 17.1 and 18.7; public doctoral: 16.9 and 19.0; private doctoral: 15.3 and 18.2; private liberal arts: 17.3 and 18.9; public two-year: 15.5 and 16.5.

rigidity but the second rigidity that concerns the faculty receives very little attention in debates about the pros and cons of tenure. Consider the costs of these rigidities in turn.

The first produces mismatches between the desirable and actual size and composition – across departments and ranks – of college faculty. Colleges offer a large number of programs and a far greater number of courses. Over time, enrolments in these programs and courses are likely to vary, rising in some and falling in others. Unlike corporations, however, colleges cannot respond adequately to the changing demand for its products. The inability to fire tenured professors in programs with declining enrolments reduces a college's ability to make new hires, and, therefore, expand faculty in programs with growing enrolments.⁸ Colleges handle these mismatches, as best they can, by capping enrolments in expanding programs; more often, they parcel out teaching in the expanding programs to poorly paid adjunct faculty and graduate students. It may be argued that these stop-gap hires may compromise the quality of teaching.

In public discourse, tenure is nearly always blamed for creating a pampered professorate. It is claimed that the professors slack off once they receive tenure, and some are even said to pass into the category of “deadwood”. The concerns over faculty productivity resulting from the job security afforded tenure are a bit exaggerated; but this is not an issue that we cover in this paper.⁹ This paper is concerned with a second source of disincentives that may affect faculty performance, one that receives no attention in the literature on tenure. Tenure not only removes the threat of being fired; barring the “stars”, it also greatly narrows – as we have shown above – the ability of the tenured faculty to move to another college and/or location. This loss of mobility is likely to dampen the motivation to maintain excellence in research and teaching. A strong interest in moving is likely to induce a professor to keep up her research productivity, change her research focus or develop new teaching interests in order to take advantage of changing market conditions. Understandably, these incentives vanish when opportunities for moving disappear for all but a small number of “star” faculty.

The virtual closure of active markets for tenured professors creates a second type of faculty mismatch. It would be easy to identify several sources of these mismatches: the termination or downsizing of an existing program as senior professors retire, changes in the direction of a program that may result from new hires, irreconcilable conflicts among professors in the same program, conflicts between faculty and college administration, decline in quality of students, changes in the professor's research interests, a divorce or marriage or changes in health that require moving to warmer / colder climates. The loss of a doctoral program can derail a professor's research projects that depend on the participation of doctoral students. Alternatively, the departure of one or more research collaborators may lead to the loss of

⁸ These mismatches may exist not only between departments, but also within departments as the demand for some fields rises while other fields go out of favor.

⁹ The concerns over the adverse impact of tenure on faculty incentives are a bit exaggerated. First, the security of tenure may encourage professors to take up long-term and high-risk projects with higher payoffs. Thus, while the number of publications – the conventional measure of re-search output – is likely to decline with tenure, this may be offset by higher quality. Second, negative sanctions might not be very important in academia where most professors are self-motivated and generally enjoy what they do. Third, college professors quite literally face an ongoing evaluation of their knowledge and teaching skills from their primary audience-the students. Though less frequently and more subtly, professors come under similar pressures from their colleagues. Fourthly, professors – especially in the stem fields – who may derive some part of their income from grants must continue to maintain an impressive research record or risk losing their grants. Finally, most colleges place their faculty under considerable pressure to perform by linking their annual raises to their teaching and/or research performance. The linking of teaching load to research output is also likely to have the same effect on research output.

research funding. Given the vanishingly small probability of moving to another college, the persistence of these mismatches can have an enduring adverse impact on faculty morale and the quantity and quality of research output.

Once conflicts emerge within academic departments, the low exit opportunities are likely to exacerbate them. When professors who cannot get along – whatever the reasons – are forced to compete for the same resources, their conflicts are likely to intensify and even become nasty. In such situations, professors have been known to spend inordinate amounts of their time on political activities aimed at harming their rivals; they may even recruit graduate students as these conflicts play out. Occasionally, these conflicts engulf a whole department, seriously undermining its teaching and research output, especially in the natural sciences where productivity may depend on team work amongst professors.

To sum up this discussion: the system of tenure leads to a virtual closure in the active markets for tenured professors. This creates two kinds of mismatches within a college: one burdens a college with professors it does not want; a second forces professors to retire on jobs they do not want. With the abolition of a mandatory retirement age for professors in 1994, these mismatches may have increased as some professors are choosing to stay longer on their jobs. These mismatches create rigidities that have adverse effects on faculty incentives and morale.

3. Trading professors

In the United States, the solutions to the problems of tenure that have been discussed or implemented have involved abolishing tenure or diluting it with post-tenure reviews.¹⁰ We propose mechanisms to alleviate the dual mismatches created by tenure without compromising the basic principle of tenure, viz. job security until retirement.

We have shown that inasmuch as academic tenure leads to a near-closure of markets for most tenured professors – and this in turn creates the two mismatches discussed above – we can overcome these mismatches by re-creating markets for tenured professors. In the simplest case, if the mismatches of two colleges are complementary, this opens up the *possibility* of a bilateral exchange of professors between the two colleges; this complementary will exist when a “surplus” professor at college A is wanted at another college B, and a “surplus” professor at college B is wanted at A.¹¹ In order for these trades to be effected, however, the professor at A must be willing to move to B, and vice versa. Since this dual complementarity between two colleges may be too restrictive, we will have to allow multilateral exchanges in order to increase the pool of feasible exchanges, with the proviso that a college effects exchanges only when they balance out year by year; but additional flexibility may be introduced by allowing accounts to balance over time. While the possibility of such exchanges is fairly obvious, we still have to identify mechanisms that can bring them about quickly and at low cost. Four such mechanisms are examined in this section.

Model I: Broadening the Locus of Tenure. Some fraction of faculty mismatches could be solved right at home by opening up the presently impermeable boundaries between

¹⁰ If post-tenure reviews are to have any teeth, they must allow for dismissal, and once these reviews admit dismissal as an outcome, this amounts to the replacement of tenure with renewable fixed-term contracts.

¹¹ Since bilateral exchanges may be restrictive, we will later examine ways in which the multilateral trading can occur.

departments to internal exchanges of professors. Under current practice, the movement of professors across departments is rare. This is unfortunate, since disciplines were supposed to serve as tentative systems for classifying and organizing knowledge, not to become barriers to mobility of faculty across disciplinary boundaries. There are different ways in which these barriers could be removed or made more permeable. One solution might be to appoint and tenure a professor to a block of related disciplines within some division / faculty of the college. Provided a professor consents to this and is given adequate time to prepare for it, a college may relocate *all or part* of a professor's teaching time to any discipline within the block in which she holds tenure.¹² If such a relocation does not work out, it may also be reversed. In order to facilitate faculty movements across disciplines within a defined block, research should receive the same weight in all the disciplines within a block; this will encourage faculty to engage in cross-disciplinary research and encourage them to study societal problems from more than one perspective. Alternatively, a college may allow or incentivize professors to relocate all or part of their teaching and/or research between departments within a division of the college or even across divisions within a college. Colleges may work out a set of minimal conditions that would have to be met before a professor may apply for such relocations.

This interdisciplinary mobility gives the college freedom to address the problem of mismatches by moving professors across disciplines. This also creates new degrees of freedom for professors who wish to change the direction of their pedagogical and research interests. This freedom may create new energy as professors at different stages in their careers prepare to move across disciplines or expand the scope of their scholarly interests. It will also encourage them to look across disciplinary boundaries within their own college to enter into collaborations that will make these transitions smoother.

Model II: Consortium of Colleges. The exchange of professors amongst a group of colleges could be also internalized if they formed a consortium and re-defined tenure in a specific discipline to be tenable, when trades become necessary and feasible, at any of the member colleges in the consortium. Alternatively, they could combine this with *Model I* and broaden the locus of tenure to include several interrelated disciplines. Understandably, the colleges that become part of such a consortium will seek to ensure that they have comparable standards for tenure and promotion. For such exchanges to be completed under existing tenure arrangements, the colleges could only work with professors who volunteer to be relocated; they could also offer inducements to professors they wanted to relocate. In the long run, however, the colleges can gain greater control over relocations by offering tenure contracts which stipulate that tenured professors could be transferred – with sufficient advance notice – to any member college in the consortium. This arrangement could also accommodate exchanges initiated by professors who prefer to relocate to a member institution provided she is wanted by the target institution.

Model III: Clearing House. In the absence of a consortium of colleges, exchanges may be organized by setting up a Clearing House. Every college prepares a list of professors, with their ranks, fields and sub-fields, who have agreed to be relocated together with information about their preferred destinations. In addition, each college prepares another list specifying the fields, subfields, and ranks in which it needs faculty. These lists are then forwarded to the Clearing House which processes the data to identify all the feasible matches or near-matches for professors on the roster. Once these matches have been identified, the Clearing House

¹² Alternatively, a tenure-track professor may initially be tied to one department within the block, and upon receiving tenure, she may be incentivized to take part or all of her teaching time to another department where there is a shortage of faculty.

sends each college (A) a list of colleges (say B, C, and D) that have professors who wish to relocate to college A. In other words, the Clearing House identifies for each college a set of colleges with which exchanges are feasible; these sets may turn out to be empty for some colleges. Once the colleges have this information, they can start negotiations for the exchanges to be completed. In order to lower transaction costs, the colleges would have to adopt a common set of procedures for negotiating these exchanges.

The exchanges do not have to balance bilaterally. Suppose there are three institutions (A, B and C) such that one professor moves from A to B, a second moves from B to C, a third moves from C to A. The bilateral exchange – exchange between any pair of the three colleges – does not balance. However, once the exchanges between all pairs have been completed, each college's exchange is in balance; it loses one professor and gains one. These exchanges may still fail to balance financially if the professors exchanged have unequal salaries. A college may well decide to undertake exchanges which involve additional outlays provided the long-term gains from this trade (academic and financial) are greater than the extra financial costs.

Model IV: Offering Subsidies. Next consider a proposal for mediating exchanges more directly, without the intermediation of a Clearing House. The existence of mismatches – as seen by colleges – implies that a mismatched faculty is worth less than what she costs to her host college. Suppose that a college wants to replace a tenured professor, X, with graduate students and adjunct professors; and this replacement, if it could be affected, would save 50 percent of the cost of X. However, the college cannot fire X because of tenure. In such a situation, the college could still be better off if it offered to pay anything less than 50 percent of X's salary, for some fixed number of years, to any college that would hire her. Since X now enters the market with some percentage (say, 30 percent) of her salary paid by her current college, this may open up a market for the "subsidized" tenured professor.¹³ Such an arrangement, under the current tenure contracts, can only work with the consent of the tenured professors.¹⁴

4. Trading players in professional sports

The exchange of professors proposed in this paper has some similarities with the trading of players in professional team sports in the United States. These similarities are examined with respect to baseball.

When the first professional baseball league was organized in 1876, the players were free agents; they could switch teams at any time, even in the middle of a playing season. This troubled the team owners since it gave each player the power to collect the entire price he commanded on the market. The team-owners organized into a cartel and turned the tables against the players with the introduction of the infamous "reserve clause" in the contracts signed by players. Under this clause, a team could renew its contract with any player simply by submitting a contract to the player on or before March 11; it did not matter if the player did

¹³ The subsidy and its duration would have to be negotiated; among other things, it would depend on the savings to the host college, the age of the professor, and the market conditions.

¹⁴ Such an arrangement may of course carry an odium; it may, incorrectly, cast aspersions on the academic competence of a professor. Mismatches, as explained above, may emerge for a variety of reasons unrelated to a professor's competence. Further, these exchanges could remain confidential, at least in the professor's original department.

not sign the contract. This meant that a player was bound for his playing career to the team with which he first signed a contract. A team could drop a player at the end of a playing season but the player could not quit his team for a new one. This system operated unchanged for nearly a hundred years. It was revised in 1976 to allow players with six years of major league service to enter into new contracts. The impact of this change would be marginal for most baseball players, since their careers in professional baseball did not normally last much longer than six years.¹⁵

Although they work differently, tenure and the reserve clause operate to limit the active market for professors and players respectively. While teams have the option to drop any player at the end of the playing season – a condition that college administrators might envy – this is not an option they want to exercise. If teams began to drop players, this would create a market for free agents, and as this market grew in size it would undermine the collective market power of the teams. The team-owners understood this, and made sure not to drop too many players from their teams. This means that team-owners are stuck with their players and must face up to the problem of mismatches faced by college administrators: they have players they want replaced by others. It appears, however, that team-owners have been more inventive than college administrators in finding a way out of their predicament. Since the earliest days of the reserve clause, they have overcome the problems of mismatches by trading their own players against players from another team, or selling them outright to another team. As Horowitz (1992: 499) writes,

“Major League Baseball is a cartel whose members can benefit by trading assets – ballplayer contracts. Each winter the cartel members and their GMs [general managers] meet, and effecting exchanges has always been a principal order of business. Some in-season trades are made from ‘waiver’ lists – baseball’s sales catalogues – that name the players a club wants or is willing to sell or trade.”

This is an option that college administrators have chosen not to explore.

5. Concluding remarks

Why hasn’t the option examined in this paper – establishing mechanisms for allowing and enabling trade in professors – been a part of the discourse on tenure?¹⁶

Perhaps, the problem lies with our semantic sensibilities. We don’t want to “traded”, to be thought of as commodities, although that is what we are when we enter the markets for labor; this language degrades our humanity. But the economic theory of labor markets also treats us as commodities – the same as cabbages or cars. However, if the semantics of “trading” is problematic, we can substitute this with “rotating”, “swapping”, or “mutually advantageous exchanges”. But I doubt if this is the chief impediment to a discourse on re-creating markets for tenured professors.

¹⁵ See Quirk and Fort (1992: chapter five) for a history of the reserve clause in baseball.

¹⁶ An earlier version of this paper was first posted on the website of Social Science Research Network on March 21, 2012. Alam, Mohammad Shahid, Tenure and Turnover: Re-Creating Markets for Tenured Faculty (May 9, 2015). Available at SSRN: <https://ssrn.com/abstract=2018902> or <http://dx.doi.org/10.2139/ssrn.2018902>

Is it the case that the trades we are proposing are not practical because the matching of professors to departments / colleges is highly idiosyncratic? In order to pass muster, these matches require several levels of approvals – by the department faculty, the dean of the division, and the college provost and president. While these idiosyncrasies certainly exist, this has not prevented departments / colleges from hiring *tenured* faculty when there is a demand for senior faculty. It is doubtful that the pool of computer-enabled matches – drawn from the entire pool of tenured faculty who are interested in relocation – would be smaller or less promising than applicant pools generated by advertisements and personal contacts. Once the potential candidates for trades have been identified by computers, there is no reason to suppose that the screening or vetting of candidates from *this* pool by any college or its subunits should be any different than it is for regular appointments.¹⁷ In doubtful cases, colleges may choose to engage in trades on a trial basis for a period of one, two or three years. If the interpersonal skills of these “traded” professors create problems, they would be free to go back to their original departments.

If professional sports teams can trade players, it may well be easier for colleges to trade professors. Arguably, the challenges involved in the matching of “traded” professors to departments-colleges are not nearly as great as the matching of “traded” players to teams. The success of a sports team depends as much on the individual skills of players as it does on how well one player’s skills and personality – that is, his temperament and interpersonal skills – complement those of other members in the team; this is because success in team sports – whether football, basketball, baseball or soccer – depend even more on the cooperation and the coordination of all the players in a team than on the skills of individual players. With a few exceptions, academic departments do not operate like teams in team sports. In their teaching duties, professors operate almost entirely as individuals; they may not always be able to teach their preferred courses but they *are* free to choose the way they teach these courses. When some professors work as teams in their research activities, more often than not the members of a research team do not belong to the same college. All things considered, then, it is unlikely that the idiosyncrasies of academia constitute an obstacle to trading professors.

More plausibly, it could be argued that the problem of faculty mismatches is not quite so serious. Colleges have been handling this problem with retirements, occasional resignations, and deaths; they can also increase departures by terminating tenure-track professors, and creating conditions for early retirement of senior faculty by offering incentives for early retirement or increasing their workload. More importantly, colleges have been addressing the problem of mismatches by limiting the ranks of tenured and tenure-track faculty, and instead steadily increasing the share of non-tenure track faculty who now are responsible for as much as sixty percent of the teaching in some of the top-tier colleges, and even higher percentages of the teaching in community colleges. It is important to note that this option is not available to professional sports teams; their players cannot be split into two or more groups that are somehow the equivalent of tenured professors, adjunct professors, and PhD students.

¹⁷ When making tenured appointments, the faculty does not have the benefit of long familiarity with the candidates that it does when granting tenure to its tenure-track faculty. But this could be partly remedied by inviting letters from the colleagues of outside candidates for tenured appointments; the letter-writers may be asked to speak to their colleague’s interpersonal skills and idiosyncrasies. On the other hand, we are well aware of a major risk of the tenuring decision: the risk of slacking off because of the job security created by tenure. One can nearly eliminate this risk in the case of traded tenured professor, since we also have a record of their scholarly and pedagogical achievements *after* the grant of tenure.

Another factor contributing to the absence of trades in academia might be the weaker competitive pressures among colleges compared to sports teams. Mostly, this is because professional sports teams are privately-owned, for-profit enterprises, while nearly all colleges are non-profit enterprises that generate significant portions of their revenues from state subsidies and private donors. In addition, competition among sports teams is much more intense because, unlike colleges, their performance is tested visibly and regularly – during the playing season – by losses and wins, which quickly translate into losses and gains in revenues. It is true that colleges also compete to maintain or improve their ranking – which in turn depends on multiple factors – and attract donations. But the reputation of colleges is built and lost slowly, and a slow slippage in reputation can scarcely generate the kind of pressures on the board of trustees that owners and managers feel when their teams begin to lose revenues and a fan base.

According to one survey in 2011, 69 percent of college presidents in four-year private colleges would prefer a majority of their faculty to work with annual or long-term contracts (Stripling: 2011). It is likely that the trustees at these colleges are even more strongly opposed to tenure, and the college presidents moderate their views since they have to deal with professors who strongly favor tenure. Given this preference for abolishing tenure among college administrators, it is perhaps understandable that they have not launched any initiatives to mitigate the rigidities that accompany tenure, since this would weaken their long-term agenda for abolishing tenure.

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Reinforcing the Euro with national units of account

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Preface

Europe is committed to a single currency but the Euro is not working well. Persistent stagnation weakens European ties among deficit countries while persistent requirements for bail-outs or debt relief weakens support in Germany and other countries which are net contributors to Union budgets. Many policy-makers see the solution in closer fiscal integration but a referendum on closer integration now would be in difficulty everywhere.

The point of the Euro is to facilitate the operation of the single market and reduce transactions costs, but its drawback is that it leaves countries that have slower productivity growth and therefore faster growth of unit labour costs without adequate means of adjustment. Competitive deflation is the only current solution and it is proving extremely costly in economic and human terms. This monograph proposes a solution that makes possible a sustainable single currency in the current Europe of nations. It does not in itself solve all current problems. Issues of historic indebtedness and fragility of banking systems would remain outstanding but this solution would make their recurrence much less likely.

Responding to Euro crisis: a better way

Stanley Jevons, one of the more distinguished economists in the history of the subject, credited with the “marginal revolution” in the 19th century, wrote as follows.

“It is in the highest degree important that the reader should discriminate carefully and constantly between the four functions that money fulfils, at least in modern societies. We are so accustomed to use the one same substance in all the four different ways that they tend to become confused together in thought. We come to regard as almost necessary that union of functions which is, at the most, a matter of convenience, and may not always be desirable. We might certainly employ one substance as a medium of exchange, a second as a measure of value.”¹

To save the Euro as a single currency, as a unique money, we have to analyse what that means. What are the functions of money? First it is legal tender, a means of facilitating transactions and settling accounts; second it is a store of value; third it is a unit of account, the way we keep score and compare the value of one thing to another. Jevons identified a fourth function, that of a standard of value, though that is more obscure and we confine our attentions to the first three.

When we consider the successes and failures of the Euro, we observe that these divide according to which of the functions of money most concern us.

¹ Stanley Jevons, *Money* (Kegan Paul, Trench, Trübner and Company, 10th edition, 1893), p.16.

Consider first the advantages of the Euro:

- Transactions costs are reduced: currency exchange is eliminated within the EU;
- Price transparency is enhanced in single market: everything is priced in Euros;
- International seigniorage is obtained when other countries use the Euro as a reserve currency;
- Branding for EU: many see the Euro as a signifier of European unity and a concrete symbol of the Union.

The first advantage relates to the Euro as legal tender; the second relates to it as a unit of account and the third to its role as a store of value. The fourth is not a pure monetary function and is derivative of the other three.

The disadvantages are fewer but very powerful:

- Europe is not optimal currency area, i.e. an area where a single monetary policy and external exchange rate is compatible with achieving stable inflation, full employment and a sustainable balance of payments in all parts of the area.
- If prices and competitiveness get out of line among countries, there is no way to adjust except competitive deflation

Everyone should agree that the principal trouble with the Euro is that deficit countries cannot devalue and are condemned to competitive deflation that exacerbates, rather than relieving, their debt burdens. They cannot gain competitiveness relative to Germany without outright deflation if Germany itself wishes to have low or no inflation. And no exercise of thrift or structural reform on their part will restore competitiveness in a world of deficient demand, without impossible strains on the social fabric.

Separating the functions of money

The solution is to distinguish two of the functions of money: legal tender and unit of account². Note that the most important of the advantages claimed for a single currency stem from its function as legal tender, i.e. means of exchange and of settling debts. With a single legal tender, the transactions costs of currency conversion are eliminated. Providing a common unit of account is also one of the advantages of the Euro but that gain does not require the Euro to be the unique unit of account. Europe at present can support having a single way to settle bills. Yet it cannot currently sustain having a single unit of account because that removes a necessary means of adjusting relative price levels. We have arrived at the situation described by Jevons over one hundred years ago where having one “substance” fulfil all the functions of money has indeed become worse than unnecessary; it has become undesirable.

² This possibility has been discussed in other contexts. Willem H. Buiter “Is Numéraireology the Future of Monetary Economics?” *Open Econ Review* (2007) 18, pp. 127-156, discusses the idea in the context of finding ways around the lower bound on interest rates. He questions whether the numéraire or unit of account would be used in private transactions, an issue acknowledged in this paper. He also questions the policy objective of stabilising prices in the numéraire, which this paper does not propose. Einzig P. (1949) *Primitive money in its ethnological, historical and economic aspects*. Pergamon, Oxford (2nd edition, 1966) gives historical examples of societies where the medium for settling transactions was different from the unit of account.

The issue may be put as follows: how can Europe preserve a single legal tender and primary store of value while restoring the capacity to adjust relative price levels that used to be conferred by realignments among national currencies? Note that when a currency was realigned with others, what was changing was its relative value, its relative position as a unit of account – not its position as legal tender. Each country can keep the Euro as its sole legal tender but should introduce a national unit of account (Nua). The government would make all its contracts with suppliers and its wage agreements, payable in Euro but indexed to the Nua. It would try to persuade other economic agents to do similarly with some combination of tax incentives and moral suasion.

The Nua could exist simply as an index number although it could also be turned into a free-standing unit of account by specifying a rate of conversion of Euro into Nua. In the latter case, the government could even legislate that all contracts between residents, all domestic price lists and all wage slips should be expressed in both Euros and Nuas. Contracts that did not specify a Nua prices could be made unenforceable at law. One could start at par (1 Euro = 100 Nua). Communications with non-national, non-residents would not be affected, nor would sight or any short-term bank deposits usable to settle transactions, which would be fixed uniquely in Euro.

The government should take the power to reset the Nua index or the the relationship between the Euro and the Nua by decree, subject to certain protocols or rules of the game, agreed with other Eurozone countries. In all cases where agreements are Nua-indexed, the Euro price would change. In dual price arrangements, agents would expect the Nua price to be preserved when the conversion rate changes. The government will adhere to this principle in its own transactions and rely on public and market pressure to enforce it more generally. It could also confer tax advantages on contracts that index to the Nua. It can thereby effect a change in the price level without having a separate circulating currency. Of course, that can be strictly enforced only for those deals where the government is a participant. In other cases the government would rely on whatever tax incentives it could devise and on moral suasion, an appeal to people to play the game in the collective, national interest. No doubt, some people would seek to resist a decline in their receipts or earnings in Euro by attempting to peg their prices or wages to the Euro. Yet that risk exists with a national currency, where inflation may well follow any depreciation. The risk is lower, the more depressed is the economy. And public, consumer pressure should induce commercial organisations to play along.

If people more or less played the game, producers would find their relative wage costs had fallen and margins on foreign sales, where prices were fixed in Euros, were better than margins on domestic sales, fixed, for the moment at least, in Nuas. Domestic goods would be cheaper than imports. The desired competitiveness consequences of devaluation would be achieved. In effect the Nua acts as a co-ordination device that facilitates a change of the general price level in Euros while reducing the need for inflation or recession as a means of bringing it about.

There is no assurance that altering the Nua value would have the desired effect on the general price level. Like a devaluation it would probably work better in circumstances where weak aggregate demand restricts the ability to push up or maintain prices. Devaluation itself can fail and result only in inflation if there is a determined resistance to any reduction in real wages. But if it worked to any degree, it would represent an improvement on the current situation. In any case, if a deficit country cannot operate a Nua arrangement, it is unlikely to be able to sustain indefinitely the austerity demands of an unembellished single currency.

The Nua would apply to prices for current goods and services. There would be no attempt to alter the value of existing bank deposits. However other financial instruments, like equity and bond prices, would be dual-priced. That means yields on securities originating in a country thought to have an excessive price level would be higher owing to the perceived risk of devaluation. That should provide a natural corrective in countries where borrowing is rapid and domestic inflation is higher than the European average. The sanctity of bank deposits however is necessary to preserve a single medium of exchange and to prevent speculative bank runs and switching of deposits within the Euro-zone. Cheques or drafts drawn on short-terms deposits are, like notes and coin, legal tender and it is essential that only Euros exist as legal tender.

Given a unified banking system, all banks would pay the same for their reserves but would be forced to discriminate in their lending, which would be double-denominated so would effectively be in Nua. This system would be enhanced by common banking regulation and a truly unified banking system but does not require common fiscal policy.

Effects on the banking system

Banks in such a system would be changed institutions. Their liabilities in the form of deposits would be in Euros though they could also issue double-denominated bonds. Many of their assets would be effectively in Nua, implying a currency risk in any country where devaluation was at all likely. That would have two consequences. Banks would have to hold sufficient capital to remain solvent in the event of devaluation and they would have to hold or have access to sufficient Euro reserves to meet liquidity requirements, i.e. demand for payments in Euros. This would achieve two reforms that have been urged on banks and central banks since the last crisis³. A capital ratio of at least 20 per cent would be de rigueur in such a system and banks would have to hold enough reserves at the ECB or to have adequate Euro collateral to meet foreseeable Euro demands. The banking system would become more like the reserve-constrained system described in economics text books – which has seldom corresponded to reality. In recent years banks made loans being confident they could always borrow reserves from the central bank and the latter always supplied, relying on interest rates to control demand and therefore regulate the volume of credit. When loans entail unshiftable risk their supply will be genuinely constrained by bank capital or reserves. To the extent that banks finance loans by issuing Nua bonds they become pure intermediaries between savers and borrowers and do not expand the money supply.

Such a system could lead to the growth of new financial intermediaries. If you wanted a mortgage loan you would want it in Nua. The banks would be reluctant to make long-term Nua loans when their liabilities were short-term Euros. To finance it they would have to issue Nua bonds themselves or some other financial institution would do so. Who would hold these bonds? Pension companies, whose liabilities are pensions, denominated in Nua, would need Nua assets. They would buy the bonds of banks or other financial intermediaries who would in turn lend to house purchasers. Long-term financial obligations would all tend to be in Nua therefore. But all transactions would be in Euro. The house you buy would have a Euro price. You would raise a mortgage from the financial intermediary denominated in Euro but the contract would specify that repayments are indexed to the Nua. The mortgage money would

³ There have been a number of suggested reforms, some going further than the implications of this paper's proposals. See: Laina, P. (2015) "Proposal for Full-Reserve Banking: A Historical Survey from David Ricardo to Martin Wolf." University of Helsinki.

arrive in your bank as a certain number of Euros and you would pay for the house in Euros. If the Nua was devalued later, you would not care because your debt is in Nua. The financial intermediary would not care because it is charging you more interest on its Nua mortgage to you than it is paying the pension companies on the bond they are holding. Finally the pension companies would not care because the pensions they pay out follow the Nua too so it is immaterial that the Euro value of the bond they are holding has gone down.

It is largely banks who must carry exchange risk in this system. Importers and exporters could hedge risks, at a price. Of course other people can opt to carry exchange risk if they wish but they will not have to do so. The banks will exact a price for that risk because they will charge more for Nua than for Euro loans. This is likely to restrict the size of the banking system because it will lead to disintermediation whereby other institutions borrow and lend directly in Nuas.

The effect would be to turn banks into utilities whose activities supply the standard means of exchange and not only provide the maturity transformation that was their historic role, converting short-term savings into longer-term loans, but also shoulder the bulk of exchange risk in a country – at a price. They would have a smaller role in speculative lending and could not risk gearing their balance sheets – activities that other institutions not involved in supplying the exchange medium would take over. This would achieve, as a by-product, the kind of banking reform and separation of financial functions that many economists advocate.

There would be some asymmetric features of the situation that some would find disturbing. Freedom of contract means people who wished to take out Euro loans could do so, just as some people today opt to borrow in foreign currency where there is a lower interest rate. However, it must be supposed that most people and companies whose pay or revenue would follow the Nua in the event of realignment would opt to borrow in Nua. That means bank shareholders would bear the brunt of devaluation or the gains from revaluation. For most people, Euros would be ‘outside money’; as debtors they would be indifferent to realignments but as depositors they would be concerned. Devaluation would entail a positive real balance effect whereby bank deposits became more valuable in terms of what they would buy domestically while a revaluation would have the opposite effect.

At the same time the claimed advantages of a single currency would be preserved. Prices would be quoted in the same numeraire in all countries, supporting the single market; there would be no need to change currencies to travel abroad, bank deposits, cash and coin would have the same significance everywhere, reducing many transactions costs.

Monetary policy

The ECB would of course retain responsibility for managing the Euro. Since all prices in Europe would be quoted in Euro as well as Nua, European inflation in Euro would be well defined and the ECB could continue to target it by setting policy interest rates as it does now. Doing so would set deposit rates across Europe so there was no tendency to move deposits between countries. However, bank loans would be priced differentially according to the perceived risk of realignment. The rates would be set by the banks and the market and the ECB would not intervene.

Those different interest rates would be appropriate in circumstances where devaluation was anticipated because of higher domestic inflation than in Europe generally. The perverse effect of the current single currency, whereby higher inflation implies lower real interest rates providing a positive feedback and still higher inflation, would therefore be eliminated. At present, of course, it is not the case that countries requiring devaluation have relatively high inflation. They are generally uncompetitive so the level of domestic prices is too high but they are also depressed so high interest rates is the last thing they need. But in the proposed system, the remedy is at hand – a substantial devaluation of the Nua. That would improve competitiveness and demand via net exports. The positive real balance effect could also stimulate domestic demand. The prospects for a devaluation are diminished after a significant one has occurred so Nua interest rates would fall.

The ECB would remain indifferent to such developments, concentrating on managing the Europe-wide Euro inflation rate. In practice, however, the EU would need to develop rules of the game covering Nua revaluations. These would happen on the initiative of national governments but on the understanding that certain objective criteria were fulfilled as monitored by the Economic and Financial Committee of the EU. The intention would be to eliminate devaluations to gain competitiveness gratuitously when a country's circumstances in terms of unit labour costs or employment were no worse than its neighbours.

Speculative attacks in such a system could only take the form of trying to borrow in Nua or shorting Nua-denominated financial instruments. Such attacks would automatically drive up the interest rate on the loans or instruments concerned.

If the ECB followed a hard currency policy and delivered low European inflation, the Euro would be a good store of value. Indeed the ECB would be relieved of the need to make unlimited loans against very poor collateral as it has to do at present to hold the Euro together. Given a hard-currency policy for the Euro, devaluations of Nua would be much more common than revaluations within the EU. As the sole store of value and medium of exchange for Europe, the Euro would be an attractive currency world-wide, earning seigniorage and fulfilling its symbolic function for the EU.

Of course, as already conceded, announcement of such a system would not resolve all current problems. That may require some debt forgiveness and for some European banks to be declared insolvent. In a perfect world, surplus countries would also expand domestic demand with looser fiscal policies allowing heavily indebted deficit countries to grow without themselves having to resort to further debt finance. Since that will not happen, the convalescence of the European and world economies will be long - even with banking reform and Nuas operating alongside the Euro. Yet with those innovations, the Euro can be preserved, the European Union can resist further erosion and the slow march back to stable prosperity can begin.

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Neoliberalism vs. China as model for the developing world

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Unlike India where the caloric intake for much of the rural population remains below the Sub-Saharan levels despite two decades of around 5 percent yearly average growth (Patnaik, 2018), China's standards of living have steadily risen. In much of the developing world, no matter the growth rates, high or low over the last four decades, one witnesses either higher relative or absolute poverty. Contrariwise, Chinese development, dubbed a miracle, alleviated poverty. It is rather a real and not an inexplicable miracle. Furthermore, unlike the dominant dictum that attributes the Chinese breakthrough to the market reforms of 1980, the process began as early as 1949. *Post facto*, these 1980 measures were manifestations of resilient socialist adjustments to China's securitisation. As to the Maoist period, the real yearly average rate of growth was nearly 6 percent until 1977. That rate would have been higher if we were to smooth the huge slump of 1961 and 1962 – the years of parting with the Soviet Union – which would otherwise bring the yearly average significantly closer to the 8 percent rate experienced since 1980 (National Bureau of Statistics – China, various years).

There are two issues of note here. First, the Maoist period built the foundation of the knowledge economy, which would later prepare China to internalise advanced technology and exhibit enough productive capacity to become the factory of the world. In technical jargon, the significant Chinese elasticity of supply arising after 1980 did not spring from thin air. It had roots in the social and productive infrastructure built under Chairman Mao, specifically self-sufficiency in agricultural production, which freed the hands of the state to finance industry and garner science-laden productive resources. The past was alive in the present and, to be sure, it was neither the person of Mao nor Deng, but revolutionary ideology that charted the recent course of history. Whether Deng's cat was catching mice or whether China was feeling the stones as it crossed the river, it did so under the ironclad fist of the communist party and its realistic thought. To speak differently, to falsify the structural continuity in modern Chinese history is an ideological position that aligns with imperialism.

Secondly, unlike the developing world where the war of national liberation was more about the “national” than the “liberation” (the latter term filters into an internationalism that emancipates man), China's national liberation war, its development being part of its security structure, doubled for international anti-imperialist war. At first, it was Mao's virulent internationalism. Later, its socialism with Chinese characteristics, which in most cases meant a publicly owned or controlled private sector, combined with its immense structure, its nationalism and self-liberation transmuted into internationalism. The more China developed and improved its living conditions *qua* security, the more the global power composition shifted against the imperialist centre. At later stages of its development, its strategy of all round internal development, as aptly envisioned by Mao, exteriorised in development for others and

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peace abroad. Furthermore, by reasserting the rights of people to sovereignty in Syria, Iran and Venezuela, its national development transpires into internationalism.

A posteriori, popular democracy surfaces as the masses in China exert power over the state to redistribute in their favour and, of late, to preserve the environment. Although the nuclear deterrent is means for sovereignty, the real security bolstering sovereignty is the steady development imparted upon the living security of the working class. On its own, the supersonic nuclear weapon displayed in China's military parade on its 70th anniversary is just inanimate matter. The communist party is aware of that, and as Lin Biao (1965) had rightly remarked, "China has a spiritual atom bomb, the revolutionary consciousness that people possess, which is a far more powerful and useful weapon than the physical atom bomb." The directional causality is pellucid. In a process of accumulation by waste, imperialism would necessarily aggress and waste China, irrespective of whether China is capitalist or communist so long as it accumulates the national capital formation. For China, it is preferable to fight a people's war of self-defence with more sophisticated weaponry.

However, despite its success, little is done to exhibit the anti-neoliberal macro-foundations of the Chinese model. The reason may be that just as China quietly climbed, it expects others by the demonstration effect and under its growing international clout to replicate its experience. Another reason may be that China has vast financial resources at its disposal and, its provocative ownership of assets in an otherwise US-led capital owned/controlled world, undermine the cornerstone of capital's power, its private property. Already China finances Iran and Venezuela against the US-imposed embargo to overcome the sort of financial containment that was the Achilles heel of the Soviet bloc. Whatever the reasons for its resilience may be, and many will be valid, the interface between those and China's actual power as it erodes the ideological heritage of the Western hemisphere, the conceptual stock that promoted capital's expansion for over 500 years culminating in neoliberalism, will leave room for social alternatives to grow. As new ideas of socialisation arise upon new international relations in the global environment, the old wealth of Europe, its historical surplus value stocked not only in commodities, but in the current dicta such as others are of inferior races or cultures, will come undone. The reaction of US-led imperialism to redress the loss of ideological wealth, which is European in terms of structure and less so American, can likely be acted out with more imperialist violence led by the now rising fascist Europe. The US-European conglomeration cannot be weaned from a wealth principally bred by imperialist violence. Under the weight of fetishism, an orderly workout to disassemble empire that pre-empt the possibility of bigger conflagrations will prove difficult.

In China's poverty alleviation, the social wage tallies with social productivity as opposed to the fiction of marginal productivity setting some micro wage relative to a price dictated by capital's historical imperatives. Scarcity, free competition, prices clearing markets and full employment assumptions will be laughed out of social science. Chimeras such as Serbs must fight Croats and Sunnis-Shias because of historical hatred, or that tribalism in Africa is primordial and awaits the bombs of white man to let peace reign must disappear. Imperialism is sociological and the wealth of Europe is its dominant ideology. As developing countries adopt sovereign macro-policies and loosen the grip of the empire, the transference between the declining power of the empire and its declining image, its ideological power, become the ferment of a conceptual revolution.

In what follows, I will draw on some salient characteristics of the Chinese model to critique the conceptual constructs of neoliberalism. Evidently, I will not be able to cover the whole gamut

of China's development experience. Allegorically, China felt too many stones as it crossed the river, and although it crossed, it also tumbled here and there as well. That is why I will principally focus on the macroeconomic foundation of development as practiced by the Chinese communist party.

Situating the issues

The global crises disclosed after four decades of neoliberalism are phenomenal. They are yet to impose a reconsideration of the received mode of analysis based on the claim that economic development depends primarily on the creation of an enabling environment for the private sector, including free markets, and free flows of trade and finance, while restraining the social interventionist role of the state. Although developing countries were presumably set to develop after the implementation of neoliberal policies applied gradually as of the late 1980s, the *ex-post* evidence accumulated so far points to the contrary. Apart from China, most developing world growth has been anti-developmental. In the case of the poorest nations, growth was pinned almost entirely on the export of primary products and, in light of the variability in that alongside foreign-capital biased national institutions, indigenous resources have been permanently disengaged. Neoliberal development has been counterproductive, precarious and uneven, both within countries and across regions (Fine and Saad-Filho, 2016).

Socially inclusive growth, the new mantra of the International Financial Institutions (IFIs), cannot logically include anyone in growth without the restitution of nationally committed social agencies. Since the core of neoliberalism empowers financialised private sector concerns over the social ones, and since private capital inherently exhibits stronger ties with the international financial markets, it does not follow to posit that growth will ever trickle down or be socially inclusive. Moreover, because profit-driven wealth depends on the simultaneous act of cost reduction and rising productivity growth, and because openness generally decimates the latter condition, wealth will grow by the liquidation of national assets, a lower share of wages and or immiseration of the working population (Kadri, 2019). The wage shares of much of the global working population has experienced steady decline since 1980 (ILO-KILM, 2015 and 2019). Although on average developing world's long-term growth rates were lower in comparison to the post-war age of capitalism, the profit rates were significant and rising (UN-WESS, 2011; Milberg, 2008; Fine, 2010). That the sources of growth stem more from the share of wages rather than productivity growth gives new meaning to the Cambridge Golden rule – nearly all the additional growth goes to capital, implying that the rate of profit grows at a positive rate, while the rate of growth in incomes is negative. Strengthening institutions, as per Palpacuer (2008), requires a favourable international balance of forces and ideology. This hollow growth scenario often generates considerable inequality and reverse development. The obvious example would be Egypt, which after 30 years of 5 percent positive real GDP growth, experienced rising poverty, child malnutrition and revolts (UN, 2010).

For reasons to do with space, let us posit that to construct socially inclusive and poverty alleviating macroeconomic frameworks or to improve the quality of productive capital, living standards and people's institutions, two *a priori* hypotheses should be considered: first, the scope and scale of coordinated and purposeful economic activity concurring with working-class based policy, the sort of activity that mediates the national concerns into the regional or international ones (Nayyar, 2008). The activity whose aim is to lift people from absolute

poverty is best tested in relation to how effective is labour in the formulation of state policy. This is China's contribution to the area of development as a human right. Secondly, yet more decidedly, a less widely recognised condition for development, which is security *cum* sovereignty. Security is defined as the totality of peoples' democratic and national securities that compose the substance of sovereignty. This second hypothesis revolves around the idea of how sovereignty, the synergy between the welfare of the population and national defence, transpires into autonomy over policy. This condition prevails to a large degree in China.

The first hypothesis addresses the idea that in a globalised environment, sound development cannot take root in a single country while its neighbours are wallowing in disaster, or while imperialism uses methods of destroy-to-grab. The very concept of working class negates the national identity. In other words, development should be rooted in a policy transcending the national framework and supported by common measures that ensure the welfare beyond national borders. These policies are measures effected and designed by working peoples. They are about the capture of value from an economic cycle that strengthens forms of resistance to imperialism or national resilience (Dragsbaek-Schmidt and Hersh, 2018). In China, the rise in wages (wages in manufacturing trebled over the last 12 years Trading economics 2019)² and the bridging of regional disparities were co-aligned with intra-regional investment and closer integration frameworks (Jacques, 2012). Altogether, one observes a Chinese virtuous economic spiral upon which social tensions taper down, thus solidifying the national front.

The second hypothesis highlights the sovereignty of a people over their human and natural resources. Although in today's frame of neoliberal reference, that supposition does not count for much, it is worthwhile to recall, that such was a class and national liberation struggle right at a time when the developing world enjoyed more power in the international arena. The memories and symbols of class have stored the successes of these times. These could be readily re-ignited. In somewhat anachronistic fashion, it is this second hypothesis that still coheres with the Chinese nationalist development model. Despite some transitional social and environmental costs, China's nationalist development model outperforms the laissez-faire model of neoliberalism. To restate the standard refrain, it is only through the inclusion of China's alleviation of poverty that the world poverty averages appear low (Jacques, 2012).

The reason for the perverse neoliberal transformation in the developing world can be anything but straightforward. It may be attributed to social psychology or the adoption of obscurantist ideologies, frames of reference by which people inflict upon themselves undue levels of misery over long periods of time. Time, the continuum in which social action incrementally builds by the dominant ideology of capital, is partitioned by analytical reasoning and, alternatively, the masses may endure short-term pain for the promise of long-term happiness. The reason can also be less complex, as if people disdain the formulae of economics, the calculation of the rates of resource allocation or the arithmetic, by which their absolute and relative living standards decline over time *vis-à-vis* the perceived costs of overcoming divisive identity politics or revolutionary transitions. Whatever the reasons, class, the abstract by real social relation, the weight of history and power, the predicate of the social product in real time, vanishes under the received notions that individual behaviour and effort erected by the virtue of some cultural symbol, race or tradition, determine a "fair share" of the wealth. Whereas wealth is determined by social time, the time into which peoples' lives are crammed to

² China Average Yearly Wages. Overall wages rose from around 32,000 to 82,000 from 2009 until 2018. <https://tradingeconomics.com/china/wages>

produce, that reality escapes the working class by its lack of cohesiveness. The momentum of faulty conceptual construction already borne out in the eclecticism of mainstream theory and transfigured onto ideology finds little opposition in much of the Marxist theory rooted in Eurocentrism.

However, the mechanics of income transfers are as pellucid as the ostentatious displays of wealth. Even the conventional press feels at ease trumpeting the immense inequality, the one versus the ninety nine percent, albeit without emphasis upon the dividedness of the working class as the root cause of disparity. For instance, although job creation is key to poverty alleviation, there will be no mention that neoliberal policy retrenches public expansion and public investment, lowers the regulatory benchmark, the discipline of the excesses of the private sector, and unleashes short-gestation period investment that create little or no “decent” jobs;³ but why decent jobs? As noted by the experience of the majority of countries, more jobs are created under the neoliberal recipe than otherwise, but these are overall poverty-wage jobs flourishing in the informal sector.

Under neoliberalism, the official unemployment rate shows remarkable improvement, albeit in the presence of rising poverty; as should naturally occur because the wage share is declining. By surrendering state investment and regulation to the externally-tied private sector, the sector whose capital circuit is the international market, the economy also sheds many of the decent jobs. This occurs because capital can foist its own criterion for labour demand. The privately constructed benchmark for hiring arises upon the productivity of a non-existent or asocial-individual set against the money value he produces for the firm, as opposed to the real social man whose social productivity is the true criterion for job creation. Add to the faulty private measures for employment, the combination of higher taxes and less public investment together with a monetary policy that supplies credit for the financial class, the share of wages can only experience a downward spiral. As wages drop below the historically determined decent subsistence levels, the economic process becomes hollow, the sort that generates economic growth alongside poverty and poverty-wage employment. Uninterrupted hollow growth, the liquidation of human and natural resources to buttress profits, is de-development or, lumpen development as per Gunder-Frank (1972).

To recognise what is lost under neoliberalism is to recognise what has been achieved under China’s sovereign development model. China exhibits an autonomous economy, combining an expansive public sector growing side by side with a much smaller private sector (Gabriele, 2020). It is an economy disciplined by restrictions on the capital account, control of management as opposed to markets, free economic zones inter-laced with an organic socialist economy, and other labour favouring *rigidities*. Here, I highlight *rigidities* to portray it in a positive light. The constructs of “flexible and rigid” of the neoliberal vernacular are a jargon of deception. Neoliberalism is neither about a free product market nor about a flexible labour market. This absolutism pertains to logic but not history. Neoliberalism is a value drainage mechanism visited upon weak un-sovereign states. It more than encroaches on public assets and resources, and dissipates national resources. Instead of a virtuous productivity-rising and wage spiral determined by the power of the working class, to ensure a higher share of the surplus, one notices the fragmentation of labour or the flight of resources. In addition to an effective state regulatory framework, the lingering rigidities from China’s socialist past had channeled a proportion of economic wealth into the social cushion necessary to hedge the private market mechanisms (Gabriele, 2020). For instance, certain

³ See Mishkin (2009) and Obstfeld (2009) for IMF positions on openness to welfare.

life-time employment contracts for the progeny of the national liberation war heroes and other job security measures that garner efficiency at both firm and social levels mitigated the transition to the mixed market. Assessing developments in the social structure of China, there was more continuity than discontinuity.

Relying on its principle of “Sustainability Led by Science and Technology,” more than half of the tech-content of Chinese exports now emerges by national means (Cheng and Ding 2017). China is engaged with its more tech-advanced trading partners in a way that upgrades its own science-productivity content (Freeman, 2018). China conducts itself in a similar manner with its partners in the least developed economies through its Belt and Road project (BRP). China’s infrastructural projects, in complement to the host productive sector, churn out higher productivity of output per capital invested in the developing world. China’s assertion of its particularity bolsters multiplicity. Its investment in infrastructure such roads, railroads, ports, dams and airports synergise local capacity. Just as it practiced a socialism with Chinese characteristics, it pollinates the knowledge sphere with a socialism that may yet flourish in a socialism with Arab or African characteristic.

That US imperialism destabilises Chinese partners along the BRP has an implacable bearing on the under-valorisation of global resources, on cheapening the inputs of the developing world. The US disrupts vital asset areas and resource flows along China’s trading routes. Its wars of destabilisation raise the rate of militaristic or financial rents, in contrast to the industrial and productivity rents of China. In the recent past, US-sponsored wars have tipped power balances in favour of the US-led camp and dollarized the planet. The higher risk and risk premia raise flows to the dollar zone and increase dollar assets and demand. The flows of surpluses into T-bills are significant but secondary to the category of US global imperial rents wrought from US strategic control, which afford it universal dollar-seigniorage.

Destabilising the BRP by infusing proxy wars to restructure power balances and expand fictitious credit (money without a corresponding real value), also engenders real-value snatch. Fictitious US capital prompts further imperialist expansion to underwrite the excess credit, while imperialist war, the pure waste economy, on its own creates new value and mobilises existing surpluses. The US entraps the real value produced elsewhere without effort through its control of finance and financial channels. Financial hegemony accelerates the turnover cycle of money capital (Hilferding, 1910), forcing the real economy into a higher metabolic production plateau; the economy overconsumes cheaper natural and human inputs per unit of output. It lowers labour income shares in the social product, imposes debts to induce austerity, and manipulates capital flows to reduce the prices of national assets elsewhere. These are symptoms of dislocation under neoliberalism.

Neoliberalism injects insecurities that corrode autonomy and sovereignty. China dodged the trend. That China is sovereign and that China develops and that it could serve as a model is not only a matter conditioned by its size; it is principally how its social forces rearticulate to realise development. Specifically, China’s communist party is innately predisposed to national development through the state.

Neoliberalism and autonomy

Prior to neoliberalism or during the post-war age of capitalism, most economies derived a certain degree of autonomy from regulated capital and trade accounts. Development was

about national resource retention, mobilisation and recirculation of real and financial wealth. As autonomy eroded in the neoliberal age, development faded. Much falsification of fact followed, especially as the contribution of national security, sovereignty and policy autonomy to development was clouded over by empirical studies that treated the historical agency in charge of national resources as the empirical equivalent of economic symbols such as tariff or quota reduction. After all, it is people organised in some form of social relation that impose tariffs, and it is the quality of these social relations, qua social classes, which constitute historical subjects; it is the primacy of class that invites research. The mainstream economists treated people engaged in development as if they were things – and oddly they were vindicated insofar as capital was a personification of commodities, and or, labour's ideology was that of capital's. As commodified intellectuals they reasoned at the behest of the commodity. The logical forms purportedly reflecting economic variables and instruments such as tariffs or interest rates acquired a life of their own and they dictated social processes. However, behind the movement of these prices, there were estranged institutions governed by the reason of the commodity as self-expanding value, manipulating social and thereafter economic conditions in the interest of the commodity and, not so much, the “perceived” interest of a narrow minority. The truth of the matter is the social-natural calamity, or the overwhelming historical moment of waste, makes it in no one's interests to remain stuck in the capital relationship.

Falsification of fact specifically flourished in describing the relative success enjoyed by the Asian first-tier tigers. In much empirical research, security or the US military umbrella was treated formally, as if simply another variable in an equation, once positive and otherwise benign or bearing some measurable effect on development (Kadri, 2017). It was not considered as a decisive historical act, the conjuncture of institutionalised decisions leading to increasing the rate of growth of productive capital formation, whilst enhancing the share of labour from total income. It was not viewed in terms of an exercise of power in international relations mediating exigencies in the global accumulation of capital – here the role of Taiwan and South Korea in the containment of China. Whereas whatever success there may be can be attributed to their functional roles as imperialist police stations, their relative success was speciously attributed to the emulation of American free market and enterprise.

It is true that a few countries enjoying a certain level of security by the extensions of global defence treaties, like the first-tier tigers, reap development benefits from the “market expansion side” of capital accumulation – the preferential trade statuses they enjoy with the US and Europe. However, these states also serve as advanced US securitisation bases in an outstanding cordon sanitaire or “as hyped models of development to be mimicked by others,” when paradoxically, because of overproduction and logically by the adding up fallacy, not “all countries” can copy these Asian models. Often, the projection of these Asian success stories purposely confound development with hegemonic security ties, especially as development gathers the support of a population willing to self-sacrifice for empire. As a first-tier Asian tiger serves to extend the hegemony of the US, it will receive much aid, albeit, to extend higher rates of commercial or super-exploitation to its more populated neighbours in South Asia. It will mean narrowly material as opposed to internationalism-infused development; development for the few well-armed northern states, South Korea and Taiwan, at the expense of a growing swathes of poorer countries around.

In making sure that South Korea should be part of a cordon sanitaire to contain Chinese advance, the US even tolerated the implementation of land reform, which was later crucial to decreasing income inequality and released resources for comprehensive development

(Burmeister, 1990). In point of fact, the handful of developing countries that rose to first world rank had avoided the IFI's neoliberalism or free market recipes and enjoyed significant imperialist privileges and aid. However, the autonomy they enjoyed is on loan and instrumentalised by US-led capital.

The reign of commodities

Mind-gripping ideas, in particular, the mystification of reality through strands of super-inflated individualistic and identity politics have clouded the social nature of production. These have further distorted the cosmopolitan nature of human civilisation, the universality of knowledge and the political processes that command social development. The expansion of these cultural phenomena fulfil imperialism's requirement for real underdevelopment and deprivation of cultural development. Intertwined with the imperialist military bases and NATO's reach, these cultural spinoffs write off the security and autonomy or sovereignty (used interchangeably) of the subdued nations. Imperialism operates with the rationing of social infrastructures of knowledge, the barring of modernisation of the hinterland and education, in addition to masking over the true subject of history. The victory of the US in the cold war was a victory for that obfuscation. It was not the financial class that won, it was democracy. Planned markets controlled by labour through the state are said to have failed, and the smarter more efficient market of free enterprise won. History is not a chess game and what has really won was capital as the weight of history, the same old relation trailing from the long sixteenth century, the indefatigably aggressive force that tears down the walls protecting less developed formations. The glitz of capital's war machine and consumerism has also prevailed. Whether through identification with power or by the plight of a superfluous population beseeching capital for an unpainful early death, a mass euthanasia, the post-cold war era reintroduced the reign of the commodity with full force. The absence of socialist alternatives or lack of ideological exposition to other successful social alternatives such as the Chinese, let mass consciousness slip into a state of defeatism.

On the economic plane and through resource divestiture, neoliberalism instilled inimical growth in the productive forces, including the productive capital stock, employment and growth in the incomes of the poorest working strata. Biased institutional change botched up broader participation in the decision-making process as the state retreated and vacated grounds for the imperialistically-funded civil society. Neoliberalism as an ideology does not function by selecting people who are corrupt and in the business of promoting their self-interests. An ideology creates the historical context into which it is only possible for corruption to grow; corruption defined as the transfer of public into private wealth.

Social remedies for the inequality and unemployment debacle, the only solution for labour absorption under capital, faded from the scene. The instruments of neoclassical economics, the conceptual tools of neoliberalism that set policy regimes and, the benchmarks for the formations of macro prices, such the exchange or interest rates, were conceived of as bereft of social agency and, just like the commodity *cum* fetish, with its aura to rule over society, people were treated as excessive things. All the same, macro prices are instruments that serve the allocation of resources and the distribution of income according to the political and ideological power balances ruling the social structure. No serious effort was adopted to demystify the short leash extended to history by the commodity, not even as the crisis in nature reacted with vengeance against mankind.

The context for resource allocation

If we posit that poverty jobs are not jobs that the productive economy creates, but work that many conduct to simply remain alive, then the weak response in job creation to growth over the period 1980-2014 contradicts the law of labour demand as derived demand (measures exclude China; ILO-KILM, 2015). This otherwise chronically low elasticity of labour demand to income illustrates that growth was hollow. The lower share of wages illustrates that income is politically generated rent (Marx, 1867; Kalecki, 1943). Weak and divided labour earns meagre social wages, irrespective of the supposed moral component propping wages; capital has no morals. The wage is social. Productivity is social and it presupposes the wealth level, but not the shares distributed to labour and capital. The high rate of decent work unemployment mirrors the anti-labour bias at the heart of dominant theory. The putative hypothesis states that labour demand is the sum total of each firm's demand as it levels worker productivity with the wage rate; that is assuming particular productivity exists and is measurable, which is rather fantasy. Reverted to its mainstream theoretical reasons, unemployment is the product of an economic efficiency criterion that equates/identifies the private with the public spheres. Nevertheless, declining investment quality, labour saving capital with high-tech composition, and slowing rates of growth in productive industrial stock and agriculture, sap demand for labour. More precarity emerges (Standing, 2006). While central business-cycle recessions set upon the West every decade or so with two successive quarters of negative growth, the cycle of the developing world is intrinsically anaemic. It is underlain by monetary and fiscal leakages, and a production process whose import dependency and labour-saving technology immanently shed labour. The expansion of poverty employment for the private sector, the sort of work that falls outside the effective units of labour required for production of commodities destined for exchange, is means of suppression and control. Job creation is subject to the hegemony of capital as labour demand adheres to capital's primacy of politics. Capital calibrates the jobs it provides or subtracts with the steadiness of its rule in mind. The poverty jobs transmit high rates of profit, but altogether at much lesser rates than commercial exploitation or deaths by wars of encroachment and hunger.

The overwhelming majority of jobs no longer classify as work that delivers effective labour units in production, which sell on the market for a decent wage. The job market is a two-tier system of well-paid agents of capital unleashed against the immiserated masses. The command of capital over the cycle of labour power reproduction through pauperisation, de-subjectification, and quasi-enslavement, is more and more the corner stone of the labour process. It may be as well to recall that without publicly accountable production processes, and because what is efficient for the private sector is necessarily inefficient for the public sector, decent jobs and development fail to be met. The causes of failure are in the way power, control and decision making are articulated between the various classes, in particular the positioning of the working class vis-à-vis the imperialist class.

As to the business cycle, developing economies have been performing way below potential or have had to set resources aside. Capacity idles, demand for subsistence rises, penuries of basics abound. Neoliberalism hands down a higher rate of resource under-utilisation to less autonomous and less capitalised formations. The regulation or formation of the price system in dependent economies follows the world exchange and interest rates, namely US capital determined, in proportion to openness, as opposed to national forces shaping macro prices. China averts much of the diktat of world prices and imposes selective measures of openness, or it opens up as the economy withstands the shock. However, for security-exposed formations, the externally determined macro prices (world interest and exchange rates) shift

resources into their externally integrated sectors. Their monetary policy to hold down the rate of inflation by keeping the interest rate unduly high, which otherwise arises upon the trade and fiscal deficits alongside external borrowing, consistently lowers the living wage for the majority. The lower wages happen not only because of the higher prices of essential commodities or low purchasing power, fighting inflation raises unemployment as credit rationing lowers demand and output altogether. It also lowers the wage share of labour not only because fewer people are employed, but also because the state taxes the workers to subsidise the pegged exchange rate as the rich transfer their overvalued national currencies abroad at the fixed dollar rate. The dollar peg subsidises the wealth of the rich more so than the bread of the poor. Rephrased, managing the exchange rate peg with the dollar furnishes the national comprador with a mechanism to shift national assets abroad through national currency subsidised by additional taxation or by a share of the declining wage bill.

Policies of condensed capital, the neoliberal policies, design incomes to flow as geopolitical rents that dichotomise a developing economy. Rents flow to a highly capitalised modern sector where few jobs relative to the capital are created and, in an adjunct manner, decent job expansion occurs mainly through patronage in the public sector. In public perception, patronage as a social pacification measure connotes inefficiency. However, in the absence of social welfare programmes, public employment meeting social concerns amounts to a long-term developmental payoff. Subjected to fiscal austerity and leakages, including real capital and labour flight, the public sector's growth has been less than commensurate with high rate of new entrants into the labour force. The contradiction of capital with population growth is acute in Africa and the Arab world. Tangentially, the weak financial intermediation between money assets that accrue from geopolitically determined rents and the build-up of physical capital and a healthy rise in income associated with rising productivity (wealth), the rest of the economy leans ever more heavily towards the service and informal/low pay sectors.

From the point of view of capital, inter-working class conflict spun around constructed identity or deepening labour force differentiation boosts the risks and the short term rents in all the economic sectors making the present more valuable than the future. Speculation and finance overwhelm industrial investment. In that sense, the scourge of identity politics, notably plays in favour of the capital and its comprador. But still, it may be relevant to recall the overarching condition of geopolitical risk and its impact on inter-temporal preferences, institutional capital-bias, and the already inherent uneven development, also contribute to making the financial rent fallout more valuable than investment in an industrial or an environmentally sound future. The combined effect of nationally bred divisions and externally imposed threats upon small weakened states write off the future. Needless to say, within an un-sovereign institutional context, presumptive redistribution allowing for lesser concentration of private wealth and greater interest in development is highly unlikely, save the presence of a working class. The comprador control the developing state, while their assets are the liquidated national wealth stock lodged abroad in dollar form.

The financial returns of the comprador, pre-determined by geopolitical rent channels, are material grounds for their unity in imperialism and dividedness at home. Formulaically, the incremental growth of the dollar wealth of the comprador forces each of the comprador classes into a race to convert national assets into dollar assets, no matter the dire effect on production. The comprador deconstruct their own states setting the stage for the more surplus value-intense accumulation by waste (Meszaros, 1995; Kadri, 2019). Comprador capital is an inter-conflicting relationship that draws rents from dismantling the national productive

structure at the behest of foreign powers. In other words, the comprador meets abroad in the common pool of dollar investment or savings, but collides at home over shares in rents. Contrariwise, the wealth of China's leading national class originates in national production and is national currency denominated. China's capital recirculates nationally and rises as higher plateaus of living standards obtain to the working class. In weak states serving as repositories for raw material and war, the comprador's inherent function is to liquidate labour. In its partnership with imperialism, the comprador acts to set aside or neutralise national resources that could bolster national platforms in international negotiations or raise competitiveness. The premature deaths or exodus of labour as a result of souring living conditions epitomises resource usurpation.

The neoliberal side of accumulation driving capital's gains is to be found in the institutionally-imposed inter working-class divisions assuming various identities, which are reconstituted socially as a result of competition for rents around the state. In processes of blatant de-development or for states at lower ends of markets, these are financial or merchant rents as opposed to socially-abiding productivity generated rents. The former form of rent dissolves wealth, the latter builds it. Rent orchestrated by the dominant ideology surfaces in the disarticulation attendant upon the retreat of social consciousness as economic conditions worsen; not that the departures of consciousness from social being is unusual, but the degree to which the formal or metaphysical conceptualisation guide the making of events is unprecedented. The forms of thought with which people fathom the environmental calamity, for instance, the idea that sorting trash with more efficient machines help, are unreal and ahistorical.

Because of financialisation, the ideological response required to restore nationalism in the practice of development is more elusive than ever. Much of the phenomenal culture of consumerism, more aptly, self-consumption, is co-supportive of intra-national wars. Conflicts visited upon the developing world are industries of waste and means of imperial hegemony, which conjointly with the stresses that ensue from lack of labour-oriented institutional development further divorce the working class from active politics. Naturally, there will be no popular democracy of the sort that daily negotiates projects for the masses at the bosom of the state.

Circuitously, the usurpation of national resources mirrors the feebleness of the masses in the state. As trade and capital accounts are set free, developing countries' control over their monetary/macro policy becomes a negotiated settlement measured in relation to the depth into which they have sunk into foreign currency denominated debt. The symptom of central banks underwriting the expansion of credit to inflate asset prices, pegging to the dollar, financing internal borrowing with external savings, albeit, side by side with capital flight, literally shrink output and the wage shares. The speculative pressure on real capital assets lay grounds for only ephemeral, or quickly gestating, investment. Consequently, developing countries distort the path of their productive assets, depriving future generations of bequeathed wealth or holding future labour as collateral against fictitious – unpayable – debts.

China as élan for development

Chinese experience, its autonomy over policy, the nexus of security and development characterising its post-independence path, revolutionises development and sheds new

understanding of the nature of the agency of development in relation to policy. China confronts an imperialism that has to raise the oppression, which boosts the rate of exploitation while holding to a higher degree of power that captures surpluses via the financial channels. Such imperialism thrives mainly by war. China is subjected to a protracted assault, especially as it alleviates poverty and nurtures the power to retain surplus through the development of its own finances (Kadri, 2017). China's technical development strengthened the grounds upon which the masses successfully fight a people's war. In my discussion so far, the premise for always developing the capabilities of people's war holds primacy because for US-led imperialism: war is the state of becoming of its capital. Just as there was a monophysitism, a union of god and man in religious mythology, there is a union of militarism with the US-European capital formation. For the developing world, a twining of security and development, the kernel of the Chinese model, presents itself as an immediate alternative.

Conversely, instead of investment in infrastructure, and plant and equipment, the neoliberally reared model erodes autonomy and shifts the accent in development to stabilisation efforts, especially the stability of central capital's rule (in a complex whole structured in dominance it will not be peripheral, but central capital), so that the destabilisation of the periphery often serves the stability and war revenues of central capital. Adherence to the conceptual framework of the mainstream sways resources away from social and economic pacts into the repressive state apparatus, which is the phenomenal brutality of the politics of neoliberalism and militarism, as opposed to the individualised cases of state-cruelty. By this I may draw on Libya or Iraq as for analogy, these regimes repressed hundreds or thousands of political opponents, but the US bombed and starved millions to death and gathered more power to conduct more of the "saving-people" operations elsewhere!⁴ These are different planes of repression. It is the totality of capital and its belligerence, the structure of white US and Europe as opposed to the idea of whiteness (ideological whiteness), which imposes all modes of repressions downstream, including the practices of states acting in self-defence.

Immiseration, the womb of interworking class violence in the absence of alternative ideology, is the womb of the power that stabilises the grounds for financially strapped, profit rate concerned, imperialism. Institutions remoulded with neoliberal concerns in mind, and developing under the onus of un-intermediated and highly erratic economic growth, "privatise" the state. The subaltern image of such rule is the growth of social schisms along identity fault-lines. Here is the springboard for the neoliberal income maldistribution and the political strongholds that privately own the public sector.

To formally address the interface of available resources to development without prioritising the type of the historical agency mediating the decision to develop is to be held hostage to the dominant concepts. The mainstream formalises the relationship between macro and social variables. These in turn become devoid of socio-historical content. Formalism is a relationship of variables to variables, as opposed to socially organised agents, the dynamic social relation in which every part is a dynamic whole of a larger whole. To be sure, the so-called mathematical rigour of mainstream economics was a pernicious attempt to conceal ideological proclivity. This is true in particular, with the outlandish proposition that growth will trickle down without labour's command over the channels of allocation and distribution. Most

⁴ Physicians for Social Responsibility as quoted by MintPress News, "Do The Math: Global War On Terror Has Killed 4 Million Muslims Or More". A recent study suggests the "War on Terror" has had two million victims, but reporter Nafeez Ahmed claims this may be only a fraction of the total dead from Western wars, <https://www.mintpressnews.com/do-the-math-global-war-on-terror-has-killed-4-million-muslims-or-more/208225/>.

important, the economic efficiency criterion of the mainstream is set against non-existent atomistic, abstract agents, or private rather than social considerations; once more, formal and unreal. Capital generates wealth and much has been produced under private sector tutelage, foremost is the un-compensable damage to man and nature, which far exceeds any benefits of capital.

The replication of that model requires a re-articulation of the power structure in favour of working people, which co-laterally implies a joint national front against the comprador and its patron imperialism. Although much comprador is turning to China for financing, the growth of China itself undercuts the foundation of compradorial classes because it undermines US-led hegemony and financialisation. In terms of surplus retention, the emulation of China requires industrial, trade and capital accounts measures that lock in resources and recirculate the surplus value nationally (Weeks, 2000). Regulated financial flows are the safety latch of Chinese development. Other points of political economy from the imperialistically homogenised economy that the Chinese model overcame can be tersely put as follows.

Investment and capital formation

Investment, more precisely the net incremental increase to capital formation, builds by the demands of growth and returns. Under the weight of shrinking credit to the working class and jobs, neither conditions are adequate to induce investment in long-term productive capital. For the financialised private sector, the macro context of openness, combined with weakening industry and uncertainty, further facilitates the shedding/liquidation of real assets for financial gains. In terms of the quality of investment, investment in plant and equipment and its corresponding industrial culture, neoliberalism induces dependency through a reduction in the quality of the capital stock (Saad-Filho, 2005). It promotes ignorance of engineering and machinery knowhow, which would alternatively, in the transition to socialism, temper the overly entropic rate of social nature. The imported capital asset/technology is consumed or stands for a consumption item rather than a production item. These corrosive symptoms of dependency cannot be detected in the money value of output per worker nor the output capital ratio. As output rises by geopolitical rents or foreign savings (debts), productivity falsely appears high; oddly, Qatar because of high oil revenues ranks as one of the most productive countries (UN, 2015). The efficiency of investment per unit of capital, the returns per dollar invested, also falsely appears high. The salient measure, the Incremental Capital Output Ratio – ICOR, or how much does it take in investment to generate an extra unit of output falls, signalling higher efficiency.

The interrelated challenge that China tackled in relation to the productivity-investment nexus and how to stabilise/increase incremental growth in quality investment is of manifold nature, but I here I list several points.

- Insecurity requires a state as provider of security. A first question arises in how to put back the state in designing and articulating factor inputs with output and the market for such output. The state in China with its overwhelming ownership of productive assets manages the inter-industrial input-output relations at social prices that respect the value of direct producers while guaranteeing growth in industrial investment. State ownership of productive assets is doorway to security because socially designed prices allocate incomes to buttress the security of the working class.

- A related question appertains to industrial culture and the indigenisation of productive knowledge (Ajl, 2019). It may be all fine to measure the real dollar growth in productivity and investment, but the real impetus for development remains how much of this knowledge is home spawned as opposed to borrowed for consumption. In other words, how much of the depreciated capital stock is replaced or refurbished with indigenised as opposed to borrowed know-how would be to zoom in on the inputs of the department that produces the means of production. Such was the real impetus which drove the rise in the national component in the composition of commodities in China.
- Economic growth as per the Chinese model reduces poverty by the degree to which it subsidises and/or reaches the poor in terms of jobs, goods and services and/or overall consumption. Not to forget, for China, the peoples' guns or security are the guarantors of the consumption bundle.
- Tautologically, economic growth and investment are co-determinant. The design of markets, whether within the nation or abroad, creates the demand that may not dampen investment at short intervals. In China, BRP expansion and sound infrastructural projects lay the groundwork for industry to expand, employ more productive techniques and scale up the value chain. The offshoot of that in labour shedding is dealt through parallel expansion of labour-intensive sectors, such as agriculture, other labour-absorbing industries, the arts, etc., and a social criterion for productivity whose scope rewards all round development objectives.

Macro policy

Macroeconomic policy, as per the Chinese model, is about intermediating economic into social wealth. Various, neoliberalism undermines social efficiency (Gottschalk 2004). It also circumvents the boomerang into developmental payoff of state investment in the social sphere. Yet despite its supposed inefficiency, the IFI-obedient public sector, including indebtedness, grew in size for most of the developing world. Its growth was led by the area of security-infrastructure spending. The efficiency the IFI desire is the sort that disciplines the labour process. Meanwhile, at the behest of the neoliberal class, the public sector spearheaded the wage compression and supposedly the leaner employment crunch. The private sector, in turn, did not fill the void left behind, hence the excess slack in real and human resources. Moreover, the application of indirect taxes as per the directives of the IFI channelled incomes upward and eroded the demand component of the economy. The recipe for austerity is to lock the national currency with the dollar and to tax the economy beyond its capacity in order to fund the peg with the dollar.

Theoretically, the IFIs touted the assumption of crowding out as justification to shrink credit to the working class; what is rarely said is that the misery inflicted is a necessary application of the law of value because without the former there will be no profits. Moreover, in reality crowding out is a fallacious proposition (Weeks, 2014). As typically true of dichotomous fallacies, whatever substantive private investment was there, it piggybacked on public or major state funded projects – it was crowded in.

It is not only that in times of low growth/poor development, the public sector offers a welfare cushion through public employment, but as the functional arm of the state institution, the public sector is the most capitalised institution. Under Chinese-like regulated capital and trade accounts, public investment can be underwritten by national financial resources and state-

owned banks. Government spending and investment as functions of appropriate monetary policy, expand growth and employment while their costs can be duly monetised. Differently, neoliberalism taps financial resources from savings and tax revenues, retarding the growth employment nexus.

Chinese macro policy, through its state-owned development banks, creates the credit space for economic growth. They fund the linkages between foreign investment and local production through the application of rules to foreign investment by which its returns nest in the area of knowledgeability. Its regulated capital account is key to its success. Contrariwise for most of the developing world, the capital account is open or loosely regulated (Helleiner, 2006). The under-priced developing country raw materials or unfinished products/inputs shift value for low prices through the open capital account to the developed world. Draining capital from poorer nations amounts in one indicative measure to losses in real or potential life expectancy. Opening the capital accounts in developing/security-exposed states surrenders the control of national finance to the international market (Helleiner, 1994). In standard macro analysis and under current account deficits, the national interest rates have to rise to disincentivise outflows, but they also dis-incentivise national capital formation.

Under neoliberalism, finance is shallow for the working population and deep for the globalised class. The former cannot borrow as much as the latter. Credit, the forerunner of economic activity, is either externally controlled or rationed to the working masses. Entrapping the moneyed value within the national economy is the crux of the national security/development nexus. The interaction of the exchange and interest rates in relation to the regulation of the capital account empower the state over the process of money creation and its bearing upon income distribution. Monetary policy is not just about the expansion/contraction of money supply. It is a tool of capital, a manifestation of the law of value, supplying credit to some classes more than others and affecting growth and the income distribution structure.

Unemployment

In a developing country context, labour demand is more than just derived demand; it is development derived. In addition to the low growth-poor development cycle, under the private job growth benchmark, unemployment must remain a socio-economic burden. Unemployment is not a supply side problem. There are not too many people relative to spare capacity. Also, the mismatches between skills required by the employers and those provided by job applicants are minor in comparison to the depressive cycle of the product market. Unemployment is cyclical and has to be tackled by shifting the productivity benchmark for labour demand from private or abstract, to social. It is best approached as China's employment policies do. China has re-absorbed much of the working age population under its transformative model of de-alienating technological innovation; hence, relieving the backlog between mechanisation and the creation of superfluous labour through social jobs and social spending.

In the lower-end economies of the global division of labour, war and militarism become the means to create jobs, in which, as I have said above, the labourer simultaneously serves as living and literally as dead labour. The effects of conflict on employment, on the resolution of unemployment by plainly disposing of the unemployed, is natural to capital, but the process accelerates under the neoliberal mantra. The Chinese model of humanising resource deployment, the necessity to include planning schemes tallying employment with existing

spare capacity are possible because of the adequate levels of Chinese state ownership and control.

Declining agriculture

The climate calamity, austerity and war uproot people from direct production in agriculture. Just as primitive accumulation did, these measures deracinate and socialise labour and resources on a massive scale. Imperialist wars and wars of encroachment or recolonisation, in particular, uproot and disperse the human and physical assets of whole nations. However, just as the forms of primitive accumulation intensify in different shapes in response to the crisis of capital, so does their key form of exploitation, commercial exploitation, whose striking appearance was slavery in the past and is the capitulated or bombed state in the present. The eradication of sources of independent support for labour, especially in rural areas, is a principal strategy of capital because it caps the independence of the working class. A point of departure would be a discussion of the rate of exodus from the land in China, at a heavy cost to the farming community, and the rate of absorption in decent employment, while wages rise. Such transformation remains under-investigated.

Discussion on the topic of agricultural decline is rarely framed in relation to waste accumulation or to the point that the creation of value occurs in the destruction of idle or active value. True, imports of cheaper agri-products undercut national agriculture (Bernstein 2010). As an offshoot of trade policy, agricultural trade treaties negotiated by the weaker parties compromise food and national securities (Bush, 2007). The insecurities are themselves impetuses for an attendant rise in militarism. The context undermining national agriculture is laid down by the law of value as means of immiseration through policies of pegged exchange rate, the single interest rate, and the declining wage bill. The pegged exchange rate may appear to subsidise imported foods, the single interest rate may stay too high to mitigate capital flight or dampen investment, especially in agriculture, and the agricultural incomes may be undermined by rising inflation and weakened rural political organisations, but the appearance of prices are the reified operatives of capital. These are symptoms of the policies associated with capital adhering to the reason of the commodity. They are the economic façade whose social outcome reaccentuates uneven development between metropolis and hinterland. In China, it is the control of these prices that counteracts the decay of rural areas.

Closing comment

Development transpires by the power labour exercises in the class struggle: the true gauge of democracy. That China had developed and alleviates poverty is *ipso facto* proof that labour votes daily in the state. It is popular democracy, as the late Samir Amin (2016) stressed, which imparts the autonomy in policy. Control of value flows in money form through capital account control and autonomous industrialisation are central to the Chinese development experience. Put differently, autonomy is how much people, principally through their state, control the decisions that shape their lives. Elsewhere, the emulation of the European democratic model resulted in a marked absence of democratisation as materialisation of working-class power in the state. European democracy has evolved as a resource allocation mechanism paying off sections of the working class to solidify central capital's rule and/or expanding empire and imperialism. European welfare states are manifestations of the

European circles of capital, which is a social democracy breeding fascism at home and imperialism abroad (Browder, 1933). Browder's main point is that social democracy arrives at this state through an emphasis on the sphere of circulation as opposed to homogenising production and wage conditions across the globe. With whichever means achieved, the forms of working-class control over the state are democratic provided they impart positive sum improvement in the living conditions of nationals and extra-nationals, the non-national boundary innate to the definition of an internationalist working class. The reason I say with "whichever means possible" is because violence is, necessarily but not exclusively, a means to defend oneself against capital.

Re-distribution is effective by the degree of popular participation, as opposed to political processes, such as ballot box charades destined to rebreed the same capital class. In a developing context often subjected to imperialist assault, the effectiveness of monitoring or embedding the gains from development becomes a learning process that grows by the distance the ruling national class keeps from imperialism: delinking. Autonomy itself is the decolonisation of development, including a reinvention of the concepts, language and practice of development. Accountability to popular democratic forms of organisation raises the social payoff of redistribution over time. Autonomy is the common thread that holds together the macro themes of sound development through the nationalisation of knowledge and resources. The question then becomes: why were national institutions non-autonomous and/or why did the national bourgeoisie in so many places betray the national agenda?

As forms of social organisation, classes supersede and instrumentalise the state and its national boundary. History proceeds by auto-negation. Under capital, accumulation is highly entropic, it overconsumes man and nature, and grows new sprouts as it decimates others. China is the last new shoot whose very development is anathema to capital, the five hundred years build-up of waste-wealth by an egregious law of value. The subject of the historical surplus value culminated in the ideology known as neoliberalism.

International relations have now evolved into a central contradiction between a globalising China versus a protectionist US (Lauesen, 2018). The assault on Iran, Syria, Venezuela and the war in Ukraine are hurdles facing China's market expansion. Iran's assault compromises China's energy deficit and security. China defends itself by socialising and bolstering its home front. It aids those falling under the onus of imperialist sanctions and raises standards of living at home. However, the dominance of western ideology infiltrates every nook and cranny of the global social mind. Visions are blurred. While the history of bombing the Congo for tin or the slavery and deforestation associated with sugar cane are a bigger enterprise than the sale of the coke can, received theory focuses on an illusory price system and its instantaneous account to show that the coke business is bigger. The array of waste-commodities' realisation for prices in social time is reduced to the fairy tale of the one high-end commodity selling in a high-end market. Truly, "what is represented in ideology is therefore not the system of the real relations which govern the existence of individuals, but the imaginary relation of those individuals to the real relations in which they live" (Althusser, 1994). All it took to produce a commodity, all the real and difficult relations, disappeared in that moment of sale. What has disappeared also is priceless *social responsibility*.

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Classifying “globally integrated” production firms from a worker/citizen perspective

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Modern economists frequently describe production chains, and the firms involved in them, as “globally integrated.” This global integration can refer to one firm that is vertically integrated with production steps performed in many countries. It can also refer to multiple firms involved in different steps of the production of a product, firms which are spread out across many countries, each involved in one aspect of the product’s design, manufacture, and/or retail. “Globally integrated production” is implicitly or explicitly contrasted with earlier times, in which production of a product tended to be done completely in one country.

Nonetheless, from the perspective of a worker/citizen of any one country, describing firms or production chains (henceforth referred to only as “firms”) as “globally integrated” can be rhetorically misleading. I propose a three-part definition of global integration that better captures what it means to describe a firm as “globally integrated.” This definition will be based on whether the worker/citizens of a particular country are included in the “global integration” of production to the extent to which they are also included as consumers.

This definition is especially important for those who believe that there is global underemployment, and that economies may not trend quickly toward full employment and full capacity equilibria (for example, John Maynard Keynes’ view, or currently, a post-Keynesian view). In such a view, lost jobs in a country are not quickly replaced by other jobs, and so finding ways to employ a broad section of the population may require policy actions, and not just the assumed magic of the market. However, no matter what assumption one makes about employment and equilibria, the new proposed definitions are more accurate than simply calling a firm “globally integrated.”

The worker/citizen perspective

In evaluating what it means for a firm to be “globally integrated,” consider the perspective of country X’s worker/citizens, defined as members of country X, who cannot easily move to other countries for employment. They depend on labor income for their livelihood (or must request assistance). Worker/citizens are distinguished from managers and investors, who either can depend on much higher labor incomes (allowing more savings to see them through tougher times), or receive a significant share of their incomes from investment returns, including investments in other countries.

From the worker/citizen perspective, calling a firm “globally integrated” obscures whether the firm is using workers from country X at similar levels at which it sells to consumers in country X. For worker/ citizens, it may be important to understand whether a firm is an employer, and not just a provider of products for consumption. With that concern in mind, this paper proposes breaking down the definition of “globally integrated” into three categories from this

¹ This essay is meant to represent solely the opinions and research of the author.

worker/citizen perspective, i.e., whether the firm uses workers from country X in a similar amount to which it sells to consumers in country X. (Some allowance for mark-ups to cover manager and investor compensation would not change this basic metric.)

Firm type 1: A globally integrated firm

From the perspective of the worker/citizen of country X, a truly globally integrated firm may have production and sales in many countries, but it pays workers from country X at least the same amount as it sells to consumers in country X. For example, if workers in country X contribute \$1,000 of the value of a product, consumers in country X then consume not much more than \$1,000 in sales of the product. I suspect that many users of the term “globally integrated” firm intend this image when they use the term “globally integrated.” After all, who in country X could object to global integration of firms if workers in country X are contributing their share of global production?

Importantly, though, this is not the only type of globally integrated firm.

Firm type 2: A domestically-owned, but foreign-producing, firm

A second type of firm is one that is owned by country X investors, but has most of its production in other countries, even though a much larger share of its sales are in country X. For example, this firm might use only \$10 of country X labor to produce one unit of a product with \$1,000 of global production costs, and then retail it at \$2,000 in country X’s consumer market.

Since it is headquartered in country X, the firm will have some legal, marketing, and managerial employees in country X. If the firm also handles retail, it will have some retail employees (drawn from the worker/citizen labor pool). However, the vast majority of the receipts from sales of the product will likely end up in the hands of country X’s managers and investors as well as foreign workers, and not the worker/citizens of country X.

From the perspective of a worker/citizen of country X, calling firm type 2 “globally integrated” obscures the reality that this kind of firm likely results in a net outflow of money from country X’s worker/citizens to (1) other countries and (2) managers/investors in country X. In other words, the “global integration” is not integrating the worker/citizens of country X. Using the term “globally integrated” to describe such a firm to country X’s worker/citizens may be an attempt to invoke an image of firm type 1 (in which country X’s worker/citizens are producers and consumers in roughly equal amounts) to describe something very different, i.e., firm type 2 (in which country X’s worker/citizens are mostly consumers).

Firm type 3: A foreign-owned, and mostly foreign-producing, firm

A final type of firm would be like firm type 2, but the headquarters of the firm would be in a foreign country. There might be some production in country X, and maybe some managerial employment as well, but likely less than in the case of firm type 2. For firm type 3, then, the same logic as used on firm type 2 applies, and again, from the perspective of worker/citizens in country X, this is not a firm that has integrated them.

This type of firm is separated here from firm type 2 for two reasons. First, doing a little production in country X does not turn a foreign firm into a domestic firm, from a worker/citizen

perspective. From the perspective of country X's worker/citizens, the question is whether the firm pays out roughly as much to country X's worker/citizens as it takes from them in sales receipts. If the firm does not, then it has not integrated the worker/citizens of country X, and falls into this category. Secondly, the country of headquarters (or key production links) potentially raises further issues, discussed below.

Country of ownership

For centuries, commentators and policymakers in many countries have had concerns about foreign ownership of production of particular products. Using a term like "globally integrated" implies that a firm's country of headquarters, or particular production stages, does not matter. The term implies that all work is spread out across the globe in some sort of random distribution, or by an efficient process, rather than treating those possibilities as hypotheses that could be either true or false. Saying "globally integrated" minimizes key questions like:

- Are all the headquarters of firms in some sectors located in particular countries?
- Do some countries' firms control key chokepoints in production chains?
- Could government policies, or anticompetitive behavior, have helped shape which countries ended up with which firms, or which parts of the production chains?
- Does having these firms in control of key chokepoints or in headquarters or as large employers provide other, perhaps long-run, benefits to the countries with those firms?

If the answer to any of those questions (or similar ones) is "yes", then there is some bumpiness in the distribution of global integration, and thus some caution is warranted before using a term like "globally integrated". Worker/citizens in country X, who are relatively more tied to the fortunes of country X than investors from country X (who may have international investments), may have more reason to be concerned about that bumpiness than investors do.

Conclusions

Describing a firm as "globally integrated" is often technically correct but nonetheless can also be rhetorically deceptive. For many of the world's citizens, a hypothetical statement like "products are increasingly made globally" hides the reality that "products are increasingly made globally, *but not here*." Like Dark Ages Europeans living near Roman ruins, in many countries, worker/citizens live near empty buildings that once housed factories. Those factories produced products that are still consumed in their countries, and are still made by human workers. However, now those workers are in other countries. And no new factories have blossomed in those communities to make products or services to trade for those imports.

Doctrinaire free traders may argue that worker/citizens in country X benefit from any trade deficit, as consumers, or that the net benefit to country X as a whole outweighs the cost to some workers. Without arguing these hypotheses (the truth of which depend on, among other things, the assumption that markets quickly move to full-capacity equilibrium), the separate point of this paper is that describing firms as "globally integrated" is potentially deceptive, and weighted in favor of assuming, rather than testing, the doctrinaire position. The term "globally

integrated” implies, to worker/citizens of country X, that somehow they are integrated into these global production chains. However, they may not be, except as retailers and consumers. Whether it is to one’s long-run benefit to be a consumer and not a producer is a separate question that should not be hidden by calling a firm “globally integrated”.

Occasionally, some economists may observe that particular countries are not in the “global” production chains of particular products. Such observations are important, and it is also important to characterize such exclusions using terms that do not have misleading connotations. Calling a firm “global” if it does not include the worker/citizens of country X (except as consumers) may imply to those worker/citizens that their exclusion from the firm’s production chain is their fault somehow, as the firm is otherwise “globally integrated”. A more accurate, and older, term might be simply calling such a firm “foreign”, as the word “foreign” accurately characterizes the relationship between the firm’s production and the worker/citizens of country X.

In other words, before accepting a statement about whether a firm is “globally integrated,” worker/citizens should ask, “when you say globally integrated, does that include me?”

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REVIEW ESSAY

Tony Lawson, economics and the theory of social positioning

Tony Lawson (2019) *The Nature of Social Reality: Issues in Social Ontology*. London: Routledge 266pp. PB ISBN 978-0-367-188931 \$35/£29.99

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Tony Lawson's latest work *The Nature of Social Reality* is an unusual read from the standard point of view of an economist, but nonetheless one fully worth the effort. Why this is so requires some extended discussion of context, providing a narrative that explains the development of his thinking.

Lawson's "ontological turn" and "reorienting economics"

Lawson has come a long way since his early work on industrial decline in the UK (Kilpatrick and Lawson, 1980; Lawson, 1982). Today, he is best known as a critic of mainstream economics, but not just this (see Pratten, 2015). As Edward Fullbrook (2009) notes, his work effected an "ontological turn" in the methodology, philosophy and history of economics, and his broader project has focused on "reorienting economics" theory, methods and practice. Prior to Lawson's *Economics & Reality* (1997) and *Reorienting Economics* (2003) the methodological terms of debate for economics had been dominated by appropriation, application and discussion of the relevance of philosophy of science to economics: matters of positivism, empiricism, verification, confirmation, paradigms, scientific research programs and so forth. Whilst not repudiating the general relevance of these foci, Lawson cut across them as sources of concern.¹ His driving questions have been deceptively simple:

1. What must we take the social world to be like in order for the way we construct theory and pursue method to be appropriate as ways to interrogate reality?
2. Whilst acknowledging the context issue of transience and fallibility of knowledge, do theory and method conform to or sit awkwardly with what we think we know about the way reality is?

According to Lawson, mainstream economics has suffered from persistent "explanatory failure" because economists rarely ask question 1 and have evolved a general approach that, in terms of question 2, does in fact sit awkwardly with what we know about the way reality is (Lawson 2015). This becomes most obvious in periods of crisis, but is notable in any time and place where relative stability in patterns of behavior breakdown (Lawson 2009). The principal reason for this is that economic theory and method adopt positions that translate an interest in patterns into a focus on or a convergence to some typical or stable situation and this has involved, in different ways, a set of "closed system" characteristics. This is most obvious in

¹ Though to be clear, realism as a general philosophical position is similarly critical of positivism and empiricism, and is attuned to sociology of knowledge effects on the theory and practice of economics (paralleling Kuhn and Lakatos); but given that complex causal processes produce variety in outcomes or events, realism places less reliance on inductive-informed regular outcomes or given patterns as sufficient explanation that can lead to nomological "laws" and it questions whether falsification can be adequate as a test of knowledge credentials (if outcomes can be irregular). For recognized mainstream issues of testing and replication see Hoffler (2017) and Ioannidis et al (2017).

formal mathematical theorems or proofs of theory and in modelled applications, such as econometrics, which require two closure conditions and a third facilitating condition to pertain, though most economists today are unaware of the conditions in their most basic form, since they are now embedded in practice – becoming matters of axiom use or discussion of technical aspects of methods (for example, Lawson, 1997, pp. 77-81):

1. Extrinsic closure: there should be no unelaborated conditions or influences, and so the theorized “system” contains all relevant variables and is “closed off” from other interference i.e. the system is isolated;
2. Intrinsic closure: the isolated system should behave consistently in its internal state; behavior is coherent and behavior repeats.

Characteristic 2 is most easily produced if behaviors are individuated and this depends on “atomism” (separation, i.e. after the isolation of the system there is an isolation of its parts). Furthermore, for some significance to be attributed to this (the repetition of behavior or outcomes, the prediction of future behavior or outcomes) then a reduction is required; i.e. an assumption that the atomistic relation is determinate, repeated and *will be* reproduced.² These two characteristics, however, typically require a further characteristic be applied. Characteristic 1 and 2 essentially create an unstructured structure, but the point of theory is to demonstrate some set of relations and the purpose of applications is to test or reproduce that relation set. As such, what is additionally required is (and this should be familiar to any economist conversant with equilibrium or with DSGEs or the fundamentals of economic models in general – for example, imposing conditions for “well-behaved” data and assumptions regarding “data generating processes”, such as stationarity):

3. The aggregation condition: a set of restrictions that result in combinations or composition that lead to or converge on outcomes, i.e. that ensure stable responses.

For Lawson, the fundamental problem here is that economic *theory* has been formulated in accordance with event regularity, whilst economic *applications* have overwhelmingly focused on identifying and predicting such event regularities. The point, however, is that regular-as-law-like outcomes are deemed to be basic to the scientific credentials of economics. Yet, socio-economic reality is *not* law-like in event or outcome terms, and a focus on regular-as-law-like outcomes tends to neglect appropriate focus on the *real* conditions that produce observable relations and events. Hence, “explanatory failure”, and whilst these fundamentals remain, hence *persistent* explanatory failure (Lawson, 2017).

In essence, critique of fundamentals identifies a fundamental problem and the identification of a fundamental problem is always controversial and provocative. The mainstream response has been *ad hominem* attacks or silence. Lawson’s project has perhaps been more effective in explaining the mainstream rather than changing it. However, fundamental critique is not easily channeled to only *one* part of a discipline, such as the mainstream and this has been the case with Lawson’s original intervention. Once his critique of closure (the “ontological turn”) began to percolate through the discipline it started to draw selective attention and invite positive comment. This has been most notable among economic philosophers and methodologists (see for example, Syll, 2016; Syll and Morgan, 2019; Davis and Morgan, 2018). Advocates of economics that categorize themselves (or have been categorized by

² So, theory that conforms to methodological individualism, for example, may be both atomistic and reductive.

others as) outside the core of the mainstream have also found Lawson's work attractive, on the basis that they have tended to be more attuned to history and philosophy of the subject and were often already in opposition to the dominant "neoclassical" strand of the mainstream.

Lawson's work has produced a different kind of provocation for non-mainstream, alternative or "heterodox" economists, albeit based on the same initial fundamental critique. The critique of closure has also been an invitation to self-critique and in inviting economists to address his two driving questions, Lawson has essentially encouraged *all* economists to ask the further question: to what degree is the critique relevant to my/our work (rather than is merely useful in targeting the economics with which I disagree)? For non-mainstream, alternative or "heterodox" economists, candid assessment has not always proved to be convenient, and this perhaps explains Lawson's somewhat fractious relationship with them, despite that he is also acknowledged to be an important source of ideas for proponents. In any case, candid assessment confronts a number of conundrums, not least because there is some degree of ambiguity or at least nuance regarding the nature of closure and what the problem for *all* economists is; avoiding construction of theory and use of methods that *in some sense* fall foul of Lawson's critique has not been easy over recent decades – given that *most* economists have a disciplinary training and have been required to pursue careers in economics as a discipline, and so have been encouraged (and socialized) to develop their own work in a format that is recognizable *within* the discipline, perhaps, though this is a matter of dispute, despite their broader school of thought affinities and oppositions.

Still, though Lawson's critique of closure may be universally unsettling, quite what the implications are is not as simple as might initially appear. To reiterate, for Lawson, closure conditions have nuance. The critique has been broad based, if one considers what "regularity" might mean. In primitive form, regularity means some version of "whenever x then y"; but this *whenever* can extend across a number of possibilities: regularity need not mean absence of variation, nor does it necessarily mean single definite outcome; it can mean "normal" behavior, convergence, cycles and other ways of defining fully determined or stochastic systems. In context, Lawson's underlying point has been that in economics outcomes are *fixed* (as regular) and the form of "fixing" seems awkward when one considers the nature of human being and human systems. Moreover, the main concern of economics has been with outcomes rather than real causes and this has been used to justify the form of fixing (its assumptions and tools etc.). For Lawson, the fundamentals in their various guises (the explicit or implicit ontology) contrast with an observed reality of structured historical process or "open systems".

In this observed reality of historical process ("real historic time", as some economists refer to it in contrast to abstract periodized time), we often seek some degree of security and create grounds for stable behaviors (through organization, law, regulation, convention and habit, since much of the point of society is to make possible our activity, to shape the world we live in for given purposes, based on individual or collective planning and goals), but we do so based on situations that are culturally diverse and where degrees of reflexivity can always be applied to organization, law, regulation, convention and habit.³ Such a system evolves through cumulative causation – a perpetual *potential* for shifting and transformation of social reality. It is always possible to break out of the bounds of how things are usually done, and unintended consequences may simply undermine how things have typically turned out in the past. It is for all these reasons that for Lawson the answer to his second driving question (do

³ For an account of process see da Graça Moura (2015).

theory and method conform to or sit awkwardly with what we think we know about the way reality is?), is “yes” it sits awkwardly. The basics of theory and method encourage economists to attend to the wrong things in the wrong way.

However, there has been more to Lawson’s critique and its reception than one might expect, despite its fundamental nature. If there is nuance to the nature of regularity, equally there is nuance in how one situates theory and method in terms of the actual degree and significance of closure, once one acknowledges that there *are* ontological issues to address. This is no more than a subset of the issue of pluralism (for issues of pluralism see Fullbrook, 2016, 2008). Even a “structured pluralism” that rejects an “anything goes”, which otherwise extends a desirable “epistemic relativism” to a self-defeating “judgmental relativism”, clearly has scope for reasonable disagreement regarding closure. For those who have taken the time to consider the issues (philosophers, methodologists, engaged non-mainstream economists), there have been at least three arising issues in terms of which reasonable disagreement has been pursued:

1. The degree to which extrinsic closure is a *necessity* of theory (something that any adequate theory should work with and allow for, rather than should deny); a key issue has been what, if anything, makes economics more or less distinctive in terms of this condition?
2. Whether, in fact, different theory and methods *do* fall foul of intrinsic closure; for example, does it apply equally to non-linear formulations, does it apply to endogeneity and path-dependent “evolutions” or versions of econo-physics or complexity theory?⁴
3. What difference does a self-aware use of methods (recalling these are techniques) with clearly understood limitations make, if placed within a sophisticated methodology (highlighting that a methodology is the overall frame of reference, attitudes and practices one brings to the use of methods)?

For example, whilst remaining broadly sympathetic to Lawson’s concerns, both Uskali Mäki and Nancy Cartwright make much of the extrinsic closure issue, albeit not necessarily using this terminology. Mäki distinguishes “realism” and “realisticness” as foci and explores the nature of economic models as one version of the necessity to simplify reality in order to interrogate it. Here, based on his original tenets, Lawson might respond that there is a danger of misdirection or obfuscation regarding the fundamentals of theory and method – there is, for example, a major difference between articulating a theory or keeping a mental model at the back of one’s head (to use the original Keynesian phrase) and specifying a model whose primary constituents begin from known falsities, merely in order to expedite the formal specification.

Reference to Keynes, however, highlights a key facet of how Lawson’s work has evolved in response to how it has been received. In his early work, he drew heavily on founding figures from schools of thought and this perhaps was partly because founding figures tended to explicitly discuss matters of methodology, and so are eminently quotable in making a case for ontology, but it is also likely because rooting his work in theirs provided a useful authoritative point of departure that facilitated achieving a readership.⁵ Over the years, however, Lawson

⁴ For example, for the issue of complexity see Davis and Hands (2020).

⁵ Lawson, of course, still draws on early key figures but not in quite the same way or with quite the same frequency.

has responded to the reception to his work in two ways. He has developed a number of concepts and alternative methods that build on his commitment to open systems. For example, demi-regs, contrast explanation, and his Population-Variation-Reproduction-Selection (PVRS) model or system (for example, Morgan and Patomäki, 2017). And he has pursued the argument that heterodox economics is united by more than opposition to the mainstream on purely theory or political grounds (Lawson, 2006). It is united by implicit commitment to the ontology he advocates: real historic process or “open systems”. According to Lawson, this is embedded in the main methodological works of key figures in heterodox schools of thought. As such, contemporary advocates compromise their own founding commitments when they adopt closed system approaches – there is, returning to Lawson’s two driving questions, a “mismatch” between (new) theory and method and what we think we know about the way reality is (Lawson, 2013; 2015).

Again, the point has not been unequivocal. Once the closure critique is recognized, perhaps the most contentious issue has been the nature and role of mathematics and analytical statistics in economics, since this is closely associated with the development of the modern mainstream (from marginalism to Samuelson and so on in the popular narrative) and this for Lawson (mathematical modeling – the relentless focus on “formulation”) epitomizes the fundamental problem of economics. However, many non-mainstream economists use mathematics, analytical statistics and continue to model and this raises a variety of questions rooted in the three areas of reasonable disagreement already stated. For example, can one symbolically encapsulate a system as an abstraction, rather than based on falsely posed idealized axioms (see Fullbrook, 2019)? Can one explore data patterns and their breakdown to empirically support theory that refutes irrealist forms, such as perfect competition (for example, Shaikh, 2016; Patomäki, 2017)? Can one knowingly use closed system based analytical statistics to trace out part of a problem for further investigation and as one constituent in mixed methods (see Olsen and Morgan, 2005)? Can one adopt an “Open Systems, *ceteris paribus*” strategy (OSCP), looking for periods and places of relative stability (Setterfield 2016)? Can one match institutions to some period of relative stability in outcomes (see Nell and Errouaki, 2013)? Does the breakdown of relative stability based on the performative purpose of institutions act as a useful signal when properly contextualized (see Nasir and Morgan, 2018)? Can one demonstrate that there are some few real regularities at a totalising systemic level (Brown, 2014; Shaikh, 2016)?

To be clear, Lawson is not anti-mathematical in any knee-jerk, ill-informed or pejorative sense. His first degree was in mathematics and he has spent decades teaching econometrics at Cambridge, UK (see Morgan, 2016). His point has been that theory and method have basic ontological problems built into them and theory and method have adopted mathematical (and analytical statistical) forms that reproduce those basic ontological problems. These have co-evolved, reinforcing each other, though the adoption of particular mathematics is not the *only* reason that economics is as it is (its history and political economy are more complex). At base, Lawson is deeply skeptical regarding the *unthinking* default to mathematical expression, as if merely the use of mathematics made economics a science, and is critical regarding the overwhelming appropriation of resources for related research (as though this was the best economics had to offer). Still, Lawson continues to make the important point that it is all too easy to apply the fundamental critique to others’ work and provide justifications for one’s own that treat that work as some exception. The ongoing issue remains whether this *is* the best economics can offer, whether resources should be applied to other forms of research and whether much of the justification is simple self-serving and obfuscating self-delusion because of socialization and the need to conform to the career strictures of the discipline (on

the role of mathematics, see, for example, Mirowski, 1991; Weintraub, 2002, 2004; Pratten, 2004a, 2004b; Velupillai, 2005, 2007; Davidson, 2014; Katzner, 2014). The diplomatic response at the moment is that it remains an open question, one that Post Keynesians, Original Institutionalists and so forth respond to differently at the level of theory, and where there does seem to be a difference between these and the mainstream even if it is not always easy to suggest what that substantively is. Though mainstream economics claims to have undergone a “credibility revolution” and to be in a “post-formalist” period of significant innovation and diversity, it remains parlous in theory and mainly committed to use of mathematics to express that theory and use of analytical statistics as method to test it (compare Angrist and Pischke, 2010; Caballero, 2010; Boylan and O’Gorman, 2007; Blaug, 2003).

Lawson is not responsible for the state of the field but he is an important figure in reminding us all that we should each take responsibility for our own role in contributing to the state of the field. All he can do is offer argument, which economists are free to address and adopt. His major and enduring contribution has been to require economists to take seriously the issue of justification, to take seriously their own and others’ implicit and explicit ontological commitments – heterodox or otherwise (see Jo et al., 2018; Lee and Cronin, 2016; Morgan, 2015a). What it means to be “serious”, of course, brings together sociology of knowledge, personal ethics and matters of justification of theory and practice in the here and now. As provocation, Lawson’s work stands in contrast to the more evasive if popular work of people like Dani Rodrik (2015), whose argument reduces to: “we have a suite of models and select the most appropriate, whilst innovating as new data becomes available”, an argument which translates into “this *is* the best that can be done and we are doing the best that we can...”. This is simply sophisticated confirmation of the status quo, and given the way economics continues to violate its own precepts (see Hoffer, 2017; Ioannidis et al., 2017; Morgan, 2019, 2015b) and its inability to provide consensus on the explanatory success of economics (rather than imposed consensus on what the mainstream will designate as legitimate economics) his position speaks to scientism rather than explores the fundamental problem of what is appropriate science.

For Lawson, an appropriate social science works with rather than against how we think reality is. This serves to make sense of the evolution of his work from *Economics & Reality* (1997) to *Reorienting Economics* (2003), which identify the problem of closure, contrast this with an ontology of structured historic process and offer alternative concepts and methods, to *Essays on the Nature and State of Economics* (2015), which generalizes his critique and reaffirms his invitation to all economists to consider the nature and consistency of what *they* do. Lawson, of course, like everyone else, is ultimately responsible for what *he* does, and to his credit (if not always to his benefit) he has sought to consistently develop his own work, moving on without repudiating that work. From Lawson’s point of view, adequate study of economy is based on a social science division of labor and not on a different ontology. This division, however, is permeable, and so it is a relatively small transition to start to work more broadly on the nature of social reality and to place economic phenomena within that context, and this is what Lawson has increasingly done over recent years, culminating in his recent *The Nature of Social Reality* (2019a).

Lawson and the theory of social positioning

The Nature of Social Reality (2019a) introduces Lawson's theory of social positioning, or rather provides an extended opportunity to lay out and explore facets of that theory, given that it has been gradually developing over years of discussion with colleagues in Cambridge (so whilst it is Lawson's theory, it is also to some degree a product of collective effort). There has been a Cambridge realist workshop since 1990, and in 2002 some of the regular participants (periodically joined by visiting scholars) formed a Cambridge Social Ontology Group (CSOG). This group meets regularly and engages in Socratic style dialogue in which they explore some designated subject matter, according to the question "what is the nature of x?".⁶ So, money, technology, the firm and many other subjects have formed the basis of discussion over a number of years, and through this discussion a social theory has been developed that informs how such subjects are explored and what in general is said about them. As such, the theory has evolved and provides a kind of broad interrogative methodology. As I understand it, the methodology/theory is not intended to be a replacement for economic theory or other methods and research; it is rather a way to engage with primary issues of social ontology, which provides another perspective, an original way to think about some of the fundamental characteristics of any given phenomena, including one's we consider to be "economic".

As social ontology, Lawson's theory of social positioning is rooted in philosophical realism, but in published form its main foil has been the work of John Searle, and this is clearest based on an early essay comparing and contrasting Searle's and Lawson's work (initially Lawson 2012), which is reproduced as an initial "general conception" at the beginning of *The Nature of Social Reality* (see also Lawson, 2016a). Searle has been one of the most significant philosophers of the last 60 years – and is famous for his work on Speech Acts, his Chinese Room refutation of functional behaviorist theory of the mind (*inter alia* undermining the adequacy of the Turing test of AI consciousness), as well as more recently for his work on constitutive rules, institutional facts and the construction of social reality (for example, Searle 2010). Searle considers Lawson a serious philosopher and not just a dilettante who has extended beyond his own area of expertise, and whilst he does not agree with all aspects of Lawson's work (for example, Searle, 2016), this at least provides some good reason to read Lawson with an open mind on matters of general social ontology as philosophy (if any were needed – there are I suspect few other "economists" whose work gets referenced in journals such as *Synthese*).

In setting out his social ontology, Lawson first distinguishes between socio-philosophical ontology (the general case of how social reality is constructed and reproduced) and socio-scientific ontology (specific "existents"). In keeping with his previous work, he affirms that social reality is essentially a "totality"; a complex, differentiated, often collaborative yet also contestable, divisive, conflictual and competitive *evolving* totality: a structured historic process. This reality is "social" in so far as it "comprises all those phenomena whose existence depends necessarily on us", and it is real in so far as it makes a difference to what we can do and how we do it, yet "exists only in being reproduced and/or transformed through the sum of our individual practices" and where "each of us, when we come to act, find this social reality to be present and given to us", which we draw on in order to act (to some purpose, to which the whole cannot be reduced and based on the possibility of error, ignorance etc.; Lawson 2019a: 11). Social activity occurs within this frame and any given

⁶ Participants, however, are quite diverse (recently, for example, Bacevic, 2019; Derbyshire, 2019; Aydin et al., 2018; C. Lawson, 2017; Elder-Vass, 2016). For a Cambridge tradition see Martins (2014).

activity has a context, and social positioning is the collective term Lawson uses to designate “the way it all comes together” (Lawson, 2019a, p. 12).

To anyone familiar with philosophy and theory of the social, Lawson is clearly working with a longstanding problematic: is, and if so, in what sense, can society be *real*, in what sense does it depend on us, what kinds of things has it allowed us to do (that are “social”), how do we do those things (in what specific contexts and how do these fit into larger frameworks or contexts), to what degree are we restricted in our scope to affect this social reality and so forth. This is most familiar as the agent-structure problem (well known to Marxist philosophers), but is broader than that and has inspired a variety of theory positions (critical realists, such as Margaret Archer (and to some degree Lawson), structurationists, such as Anthony Giddens, field and habitus theorists, such as Pierre Bourdieu, systems theorists, such as Niklas Luhmann, social constructivists in the tradition of Peter Berger and Thomas Luckmann, or along Searle’s lines, work on constructivism in the post-structuralist tradition, Actor Network theory, more recent work by “new materialists” and so on and so on). Lawson, as noted, mainly uses Searle as foil, but it is important to emphasize his combination of concepts and his claims about them (“the way it all comes together”) are original and comprise an original theory.

There are several generic components or concepts that are basic to the theory of social positioning (Lawson, 2019a: pp. 12-18, 31-73):

- Emergence: organized combinations of people and artefacts create contexts that have irreducible properties in virtue of how they are organized, and ultimately the totality of social reality is an evolving variety of these “emergents” (as sub-totalities, creating a complex interconnected and in some ways nested social reality);
- Community: a generic term for a specifiable subset of society that is in some sense organized; people and artefacts take up (occupy, are assigned) “positions” in communities;
- Positions: these are typically purposive (exist for some reason, though this is not the same as suggesting that social reality is primitively functionalist); positions are usually named (have designations) and facilitate the formation of social identities and may involve associative markers (passports, wedding rings etc.); occupants typically have pre-existent characteristics or capacities that enable or fit the position, but are also granted additional powers or capacities in virtue of the existence of the position;
- Rights and obligations: these are “positional powers”, i.e. what one can do and what one is required or encouraged to do in virtue of occupying a position; they are constitutive of the position, but are also “other-affecting” and are typically “matched” (a mutual “I in terms of you”, and corresponding “me in terms of it”).

These initial concepts can appear highly abstract. They may also appear to be, as conveyed here, overly simplistic and somewhat inert. However, this is not the case. Limited descriptive reference to concepts is not argument that explains or justifies those concepts and can be misleading, since it invites the reader to draw inferences and fill in gaps that may not exist. It is through argument and development of the stated concepts and through supporting concepts that the whole becomes a sophisticated coherent theory of social positioning. Most importantly, the theory is given nuance and cohered with the general commitment to process by Lawson. So, there is always a historical explanation to the emergence of any community and its positions and powers (rights, obligations etc.). Communities have a degree of

coordination and endure, but also reorganize. Moreover, the concept of position carries a variety of possible characteristics: some facilitate others, some provide checks, some are interdependent, but all involve recognitions (and typically acquiescence, if not agreement, regarding rights and obligations). For Lawson, some form of “trust” is also fundamental, since trust provides a social glue that facilitates activity (the I to you, the me to it etc.). And it is through interaction that the whole is given life in its parts, so community activity is essentially “relational” and the form this relationality takes can be interrogated according to the way “practices” are influenced and pursued. This provides great scope to explore rules, habits etc. and for Lawson there is (and this is basic to thinking of powers as rights and obligations) an intrinsic “normativity” to community, positions and practices. This is explored in one way or another in all eight chapters of the book, but it is in Chapter Eight that perhaps the most important general significance of this is set out. Ultimately, every community is also a “moral community” and every agent is potentially an agent of constructive social change – a claim that speaks to sentiment expressed by George Elliot (writing of Dorothea) at the end of the novel, *Middlemarch*: “for the growing good of the world is partly dependent on unhistoric acts; and that things are not so ill with you and me as they might have been is half owing to the number who lived faithfully a hidden life, and rest in unvisited tombs”.

I would suggest, then, that *The Nature of Social Reality* is not simplistic, but it does provide a versatile framework to investigate social “existents”, free of unnecessary neologisms and the kind of forbidding weaponisation of language that can make some variants of continental philosophy unappealing. In order to fully appreciate this one needs to read the book. However, given *Real-World Economics Review* is an economics journal and intellectual curiosity may not be sufficiently persuasive as a reason in that context, it is also worth noting that the book’s substantive chapters, exploring specific instances of social positioning, are mainly focused on traditional economic phenomena. This is hardly surprising, since Tony Lawson is the author. What is interesting is that taking a social positioning point of view provides novel insight into longstanding areas of neglect and of dispute. In terms of the former, though economics has a theory of the firm (the theory of “market structure”, building from Marshall or from the categorizations set out by Roy Harrod, George Stigler etc.; or the work of Coase etc. on why firms might exist), it has paid relatively little attention to the nature of the firm, at least in the ontological sense that motivates Lawson (and which forms the basis of Chapter Three and Chapter Four). In terms of the latter (dispute), economists have been divided over the nature of money (credit, commodity etc.) for as long as the subject has existed and Lawson applies his theory to this in Chapter Five and Chapter Six (see also Lawson, 2016b).

Key to applying Lawson’s theory of social positioning are a series of questions and these are as deceptively simple as his original guiding questions were for the “ontological turn”: What is the relevant community and within it what is/has someone or something been positioned to do, how has this arisen and what rights and obligations (in the case of people) or system functions (in the case of objects, artefacts, social technologies) are fulfilled in or carried by a position, in what sense are these matched (what relations do they *relate* to) and how might they be explored as normative practices (and how might we judge these)...

In the case of the firm, Lawson argues that it and its incorporations form a community (with internal positions etc.), but that in a nested sense the firm itself is positioned, and has through its incorporations acquired legal personhood (through historic accident in the case of the UK, but because this proved functionally effective and politically persuasive-influential, the form has endured, spread and evolved). Legal personhood is a designation that has conferred

rights on corporations as parts of a firm, but the law works with useful fictions and that fiction has not been matched by real capacities of the firm (consciousness, conscience etc.). This has facilitated a pathological evolution of the firm, since the positioned practices of CEO, boards, governance etc. can work towards irresponsible activity and social harms and these positions do not constitute sufficient mechanisms to prevent these outcomes by firms – they are in a sense “out of control”. For example, many multinational enterprises are systematic tax avoiders.

Clearly, though Lawson is correct to suggest economists pay little attention to the nature of the firm, it is also the case that much of the material he covers will seem familiar to any reader. It is also discussed in different ways by business and economic historians, legal scholars, and by adherents to critical management studies, political economists and activists. But that is not really the point, the originality of Lawson’s work is not in being the first to notice that firms have no conscience, or that systemic interests in a capitalist economy can encourage those positioned within a firm to pursue known harms, even if there are some benefits to the existence of corporations: shareholder value theory and its critics, the discourse of corporate social responsibility etc. are decades old and critical political economy (from Marx to Polanyi) is older still. Lawson is not claiming originality of subject matter, he is rather demonstrating the originality of looking at the problem differently, bringing a systematic ontological type of inquiry to bear.

It is in terms of the concept of money that the value of rethinking a subject based on a different perspective of systematic inquiry is best illustrated in *The Nature of Social Reality*. Unlike the firm, money is an issue to which the question, what is its nature, has often been asked. However, much of that inquiry has overlapped with questions of how money is created, what functions it fulfils in an economy and what are money’s historic origins. None of these are irrelevant to what money “is”, and clearly form part of any relevant discussion of money as something that is positioned, but these questions are different in form than the primary question regarding the nature of money as something that *comes to be* positioned. For Lawson, there is a subtle difference between a credit theory of money, which implies money is and can only be credit (is derived from forms of credit and operates only as a credit-debt relation), and a theory of credit money, which implies contemporary money is positioned out of some forms of credit, but did not need to (everywhere and always) be positioned out of some forms of credit. Moreover, for Lawson, a positioning theory of money (the application of social positioning to money) places the greater emphasis on features of the community rather than the credit-debt relation feature of money in operation, it reveals that money is a “positioned item of trust” as a form of purchasing power and as a means of payment.

Lawson’s subtle distinction between credit theory of money and a money theory of credit (based on a positioning theory of money) has been subject to a variety of counter-arguments. Searle (2017), for example, focuses on the nature of electronic money, whilst Peacock (2017) and Ingham (2018) reassert credit theory, partly on the grounds that history suggests that money has always been credit and any historical example that suggests otherwise is dubious. Lawson has various responses to these points (see also Lawson, 2018a; 2018b), and he has subsequently pursued his distinction in regard of Modern Monetary Theory (Lawson, 2019b; Fullbrook and Morgan, 2020). In all cases, there seems to be some misunderstanding and talking at cross purposes, since Lawson does not seem to be denying that credit is positioned as money or that it has a kind of reciprocity, where one person provides a denominated unit of account that carries a value to some effect and another accepts this for some purpose (so it is meaningful to suggest money is a transferable credit). He is suggesting that, as *money*, this is

the purposive use of credit and one does not need an originating source of credit (the production of money out of debt) for the purpose to exist or to be fulfilled. In so far as credit (debt) has been an originating source, this has been contingent (and he continues to disagree with Ingham and Peacock regarding the historical record) and so when exploring what money is, it is important to look beyond this contingency to what the contingency is used for, since the nature of money cannot be reducible to a contingent source of what is positioned as money. For Lawson, other social constructions have been and may in the future be the case. One might note that this creates a curious kind of social reality argument for a realist: what is the case accords with some of what credit theorists say, but the fact this need not be the case requires us to consider what is the case in a descriptively different context. To some economists, the difference may seem semantic, pedantic or of a “so what” variety, but this is to miss the point that sometimes pedantry is warrantable accuracy. In any case, reading Lawson on the concept of money will make you rethink the issues, even if you don’t ultimately agree with him, and this surely affirms the value of social positioning as a way to think differently, and by no means suggests that as social ontology it is lacking as a general framework (given the differences expressed in critique are not philosophical, but matters of fact, interpretation and emphasis in this case).

To conclude, then, there are many reasons to read *The Nature of Social Reality*; intellectual curiosity, exploration of a relatively original (philosophical realist) social theory that stands alongside others, and as a resource to think differently about given economic phenomena (either in reading the chapters of the book or by applying similar thinking oneself). I leave final comment, however, to Lawson, who suggests that the book can also be read as a contribution to pluralistic yet unified social science, which, if we return to the subject matter of the previous section – the critique of closure, persistent explanatory failure etc. – stands in sharp contrast to contemporary economics (which has offered a very different project of economic imperialism with different connotations):

“Once social ontology is explicitly pursued, it is clear – and will be apparent from the essays or chapters that follow – that in actuality there is no reasonable non-arbitrary basis for distinguishing a separate discipline or science of economics (or sociology or politics or anthropology etc.)... The arguments that are made support a dismantling of the largely artificial institutional barriers currently in place in the academy and a take-up of projects that, for the contemporary perspective of relatively isolated social disciplines, are usually interpreted as inter-, trans- or post-disciplinary study” (Lawson, 2019a, p. 21).

The context of this, of course, is not the shedding of expertise, but rather the negotiable status of any division of labor.

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INTERVIEW

Ecological and feminist economics: an interview with Julie A. Nelson

Julie A. Nelson and Jamie Morgan [University of Massachusetts, Boston and Tufts University, MA. USA; Leeds Beckett University, UK]

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Julie A. Nelson is Professor Emeritus of Economics, University of Massachusetts, Boston and Senior Research Fellow at the Global Development and Environment Institute, Tufts University. Over the last 30 years and more she has established herself as a highly respected voice on many aspects of economics, most notably social and environmental policy, ethics, feminism and economics. Her work often has a methodological frame of reference, though she also has a longstanding record in applied economics (initially micro) and worked for the World Bank and the US Bureau of Labor Statistics early in her career. She is the author or co-editor of such well-known books as *Economics for Humans* (Nelson, 2018) and *Beyond Economic Man* (Ferber and Nelson, 1993).¹ Her published work spans the mainstream-heterodox divide (for example, *American Economic Review*, *Ecological Economics* and *Cambridge Journal of Economics*). Moreover, her collaborative work on various textbooks has shown a laudable commitment to pluralism and the transformation of the curriculum and pedagogy of economics (for example, Goodwin et al., 2019). She was the 2019 President of the Association for Social Economics and is the editor of the Economics and Business Ethics section of the *Journal of Business Ethics*.

Her work can be accessed at: <https://sites.google.com/site/julieanelsoneconomist/home>

Julie blogs at: <https://julieanelson.com/category/economics/>

She is interviewed by Jamie Morgan for *RWER*

Jamie: It seems that in the wake of the IPCC post-Paris report (2018) and 9th UNEP *Emissions Gap Report* (2018) that there seems finally to be genuine growth in public awareness of the urgency of fundamental environmental issues. As a longstanding advocate of an ecological approach to economics and a Research Fellow at the Global Development and Environment Institute, Tufts University you seem well positioned to comment on this.² Let's start with the Green New Deal in the USA.³ As a non-American observer, it strikes me that the discursive positioning of the Deal encapsulates much of the problem of inertia and misinformation that has dogged this subject for decades. How do you see this?

Julie: While I'm not sure exactly why you are skeptical about Green New Deal rhetoric, I'm guessing that you may be objecting to the somewhat optimistic, business-inclusive, and even "pro-growth" aspects of this program. For example, the legislation introduced in the U.S. by Representative Alexandria Ocasio-Cortez of New York and Senator Edward J. Markey of Massachusetts (my own senator), speaks of creating "millions of good, high-wage jobs" and

¹ See also Ferber and Nelson (2003).

² Global Development and Environment Institute <http://sites.tufts.edu/gdae>.

³ For an account of the Green New deal in the USA see <https://www.nytimes.com/2019/02/21/climate/green-new-deal-questions-answers.html>.
For a parallel UK organization see <https://www.greennewdealgroup.org>.

“unprecedented levels of prosperity and economic security.” It also mentions businesses as partners in the efforts, and speaks of “spurring massive growth in clean manufacturing.”⁴ Yet I suspect I see this “discursive positioning” as less of a problem than you do.

I believe we agree on the seriousness of climate change, and don’t need to rehash the facts here. And I suspect we also agree that orthodox economics thinking has in some ways encouraged the development of thoughtlessly resource-squandering, and cruelly inequality-encouraging, economies. And we very likely agree that really radical changes are urgently needed.

The question, then, is *what sort of* really radical changes do we need. I would like to posit that some proposals being put forth as “radical” are really not as radical as they seem, while others decried as half-baked may be actually more useful.

Jamie: Yes, I take your point. Perhaps the question was inexact. I was referring to and so reporting media skepticism (having observed the response from afar from Fox and such) rather than expressing skepticism. Still, your point is crucial it seems. The core issue is liable to be, for many nonmainstream and ecological economists, the nature and implications of a “pro-growth” and “business-friendly” orientation.

Julie: Exactly. While my thoughts are totally in line with those who point out the disastrous course our fossil-fuel-based economies are currently on, I’ve noticed that many people who position themselves as “radically” opposed to orthodox economics actually agree with some (actually very questionable) major tenets of orthodoxy. They often agree, for example, with the ideas that capitalist or market-using economies have a “fundamental drive” towards growth, and that businesses are forced by competitive markets to seek every last dollar of profits, societal concerns and the environment be damned. They then conclude that, to stay within ecological bounds and serve people, current economic structures must be totally dismantled. Usually some kind of state and/or cooperative alternatives are envisioned – though, given the size of the changes called for, the attitude in the end is often exceedingly pessimistic. From that point of view, the Green New Deal proposals may seem to be naïve and merely reformist.

But, many years ago, I became curious about where beliefs in things like growth- and profit-“imperatives” came from, and dug into their history. As I discuss in my book *Economics for Humans* (Nelson, 2018), what I discovered was that these beliefs were *invented by economists*. They were adopted because they gave our profession a veneer of physics-like scientificity, not because they reflected any research into how economics and businesses actually work. The causality, in fact, tends to run in the reverse: As these ideas of economists have become more popular and entrenched, they have changed how policymakers and business leaders think and behave.

Jamie: So, somewhat ironically our universal ahistorical self-interested (as selfish) economic agent in a methodologically individualist framing for model construction and policy relevance is a social construct? Something *performed* into reality (though not fully adequate as a description of the human in that reality) by the activity of economists?

⁴ <https://www.congress.gov/bill/116th-congress/house-resolution/109/text>.

Julie: Yes. Economic theory has become performative. When we observe people acting like “economic man,” this is not because it is our “nature” to be this way, so much as that we’ve been sold a bill of goods about how we are *supposed to* act in our economic lives. I’m not saying that people are *never* selfish, or that business leaders are *not* interested in profit. But if we don’t let ourselves be blinded by orthodox economic theory, we can see that human behavior generally reflects a mix of self-interest and other-interest, and that real business leaders have pursued a variety of goals. Lynn Stout’s excellent book *The Shareholder Value Myth* (2012) describes how economists’ narrow view came to take over business education and the business media.

Jamie: Your well-known interest in the underlying effects of language and metaphor seems relevant here. As I understand it, your concern is that critics sometimes think in binaries and so simply reverse the terms of theory and activity they criticize and this in a certain sense “plays the game” of the mainstream they criticize, and this applies to ecological matters as much as it does to any other social issue or economic concern.⁵ As such, you argue we should go “beyond dualisms”?

Julie: Yes. Back in the 1980s I was influenced by the work of Deirdre McCloskey on the rhetoric of economics, George Lakoff and Mark Johnson on the role of metaphor in how we understand and communicate about things, and Evelyn Fox Keller and Sandra Harding on the history and philosophy of science. And it became absolutely clear to me that the mainstream discipline of economics relies on a deeply gendered belief about what makes for good science. Economists like to think of economic life as confined to the market, driven by self-interest and competition, rational and controllable, and intrinsically governed by mathematics and physics-like “laws” not because the economy is intrinsically that way but because these ways of seeing it are all associated with masculinity and toughness. What about production in the home? Care for others and the environment? Human emotions, in the face of a future that is fundamentally unknowable? Ways of understanding that require hands-on investigation and broader sorts of reasoning? Acknowledging these things is, by comparison, seen as womanly and weak. And so those parts of reality and those parts of good science – which I define as open-minded and systematic investigation – were banished.

To give you a recent example, when in charge of the Paris climate summit in 2015, Christiana Figueres, the UN climate chief, said that “the self-interest of every country is what is behind all of these measures. It’s not because they want to save the planet” and “Humans don’t have a stronger guiding force than my own self-interest.”⁶ This is wrong, and I regard her pronouncements as evidence of the pernicious influence of economic thinking on policymaking. One can certainly find instances of countries acting out of compassionate or principled motivations and, revealingly, Figueres’s own motivations for working on climate, when she was interviewed about them, were quite emotional.

Yet the solution to having a weak, one-sided discipline of economics built around a macho ideal should not be to simply flip to the other side and advocate a “feminine” economy of cooperation and a discipline that uses entirely qualitative methods. Both those options “play with only half a deck.” We need to get away from this “either/or” thinking and realize that it’s really “both/and.” For example, I’ve tried in another essay that I entitled “Husbandry” (Nelson

⁵ Note from Jamie: see for example, Nelson (2015, p. 115).

⁶ For references and further discussion, see Nelson (2019).

2016) to revitalize the realization that caring and carefulness are not just for women, but for men (and any other gender), too.⁷

Jamie: So, binaries remain a problem in so far as they influence thinking and restrict the way we choose to proceed? You seem to be suggesting then, that the issue of dualisms encapsulates the way some protagonists are talking at cross-purposes and perhaps there is the potential for less antagonism and some progress? It seems entirely plausible that market systems do not reduce to a stereotype of vicious amoral capitalists seeking least cost in all circumstances. At the same time and as your allusion to “what we agree” indicates, scale and the general direction of travel *of a system* (variegated though its participants may be) have mattered and continue to do so (one does not get to negotiate with the planet). There have been effects at scale that we must address. This, I expect, is part of the difference between protagonists – since the direction of travel of observed capitalism *in toto* has had consequences and for some these are rooted in fundamental mechanisms – hence the critique (bleak though this can seem). Still, the meanings we apply in thinking *through* problems are not irrelevant, they influence our frameworks and we can think through these in more inclusive and constructive fashion?

Julie: What if we could reverse the tide? What if we could move towards a more human-centered (or, if that sounds too species-limited, life-centered) economy by beginning with the economic and political structures that are already close to hand?

If you read the Green New Deal proposals, while they work within existing structures, they are hardly prescriptions for complacency or inaction! The U.S. legislation calls for a massive 10-year mobilization that gets us to zero net greenhouse gas emissions. It defines its goal in terms of creating healthy, sustainable, equitable communities for generations to come.

These are important and radical changes! If you are disappointed that the Green New Deal doesn't call for dismantling capitalism and abolishing corporations, I think that it is worth considering whether it is capitalism in general, or more specifically a particular historical variant (i.e., neoliberal and short-termist) that is the problem. If you are disappointed that the Green New Deal doesn't call for “no growth,” I think it is worth considering whether the answer we are seeking is numerical (i.e., zero), or better framed as careful thinking about growth *of what* and *for whom*.

Jamie: There is certainly a whole set of issues and problems regarding political strategy and it seems clear economics plays and could play multiple roles. Ultimately, persuasion regarding plausibility in order to develop appropriate law for effective mechanisms of change is vital. To some degree this seems a basic dilemma for economics as theory and as you suggest, beyond this. A contrast which, for example, has influenced the reception of Kate Raworth's best-selling *Doughnut Economics* (2017) in the UK. It proposes a new set of metaphors and concerns for a new kind of economy, but is “agnostic” about growth and articulates a need to avoid language and commitments that alienate constituencies that may be required in order for pragmatic solutions to current problems to be found. This, of course, raises the counter expressed by Clive Spash and others – whether the result is a compromise that does not do enough to contest the impossible (the scale problem as a systemic issue: material-quantity economic expansion at a time of excess) and so tacitly encourages

⁷ Note from Jamie, Professor Nelson's essay was awarded the Independent Social Research Foundation essay prize in ‘What is the place of care in the economy?’ 2015. See <http://www.isrf.org/funding-opportunities/essay-competitions/#PrevComps>.

complacency – which is counter-productive, since we are now approaching (and may already have passed) seemingly some climatological and ecological tipping points. Does this resonate at all with the US experience?

Julie: There is no denying that the *outcome* we need – that is, to stop living beyond our resource means – is both a dramatic change from our current situation and most urgently required. So, it's really *the means* of bringing about that outcome that are in debate in the literature you refer to. While I've looked at Raworth's work, I'm not familiar with it in detail, or with the discussions in the UK that you refer to. But my sense is that the proposals in it that you describe, and the reactions to it by some critics, are similar to what I've said about the Green New Deal and some of its critics. That is, accusations of "compromise" and "complacency" often get thrown at proposals that fail to call for what someone feels to be more "radical" solutions, such as "dismantling capitalism" or "no-growth." Compared to some kind of ideologically pure image of, say, post-capitalist utopia, more pragmatic solutions are often derided as merely reformist and ineffectual.

I have just argued that the mainstream and many "radical" views are just two sides of the same coin – and both based on a highly contrived image of capitalism. So, if giving up those myths means I'm labeled as a pragmatist reformer, I'm happy with that. Just don't claim that because I want to start with the situation as it actually is, that I'm not aware of the urgency of change.

Jamie: And the politics of this in terms of persuasion? For example, Raworth's point regarding alienating constituencies?

Julie: Pragmatically speaking, we do have to think carefully about the best way to persuade the public to get on board. The Green New Deal proposals, for example, *are* pulling a bit of a bait-and-switch when they talk about "high wage jobs" and "prosperity." Politically speaking, this rhetoric is probably necessary – no one is going to propose legislation promising reduced employment and poverty. Realistically, though, what "prosperity" means while living sustainably in a resource-constrained world will necessarily be different from how many define it today. Barring some unforeseen technological miracle (which we should by no means wait around for), prosperity can no longer be about eating lots of steak, driving a big car and flying all over the world. But prosperity could still be about living reasonably comfortable and meaningful lives, and be increasingly about health, a clean environment, and economic security. It could also be about spending less time at work and more equally sharing family- and community- care. Starting such changes in our mind-sets needs to be part of any process that has the slightest whiff of a chance of diverting us from the catastrophic path we are on right now.

Jamie: And you have spent a whole career attempting to persuade people to think differently, not least economists. The first edition of your edited text with Marianne Ferber *Beyond Economic Man* dates to 1993 and feminist economics has now been around for nearly three decades. How do you feel about how it has developed over time?

Julie: Feminist economics has had a lot of growth and new developments since its inception. There have been some victories. I think it has had the most influence, among sub-fields of economics, in development economics. Recognition of the important contributions of women and girls in countries of the Global South, and the necessity of treating women as agents in development strategies has, I believe become the norm within many national and

international development agencies. That's not a complete victory, of course – much of the leadership remains male and some of the gender discussions are no more than window-dressing – but there has been progress. Feminist economics fairly quickly gained a place within the community of those favoring pluralist approaches. The International Association for Feminist Economics (IAFFE) has grown from its modest inception into a truly international group with lively annual conferences and a journal.⁸ I was invited to write a couple pieces for the *Journal of Economic Perspectives* and a long book review for the *Journal of Economic Literature*, all on feminist topics. The American Economic Association (AEA) has recently – and laudably – finally taken some action against harassment and unjust treatment of women and other groups within the profession. Noting that nature and women's traditional work have been treated the same way by the mainstream – i.e., both as infinite resources that can be exploited without any direct attention to them and without cost – has created a bit of a link between ecological and feminist economics.

Yet progress has been slow or stalled in other areas. In spite of those invitations I received to write for AEA journals, I'd say that for the most part the mainstream has assiduously ignored the feminist critique. Feminist perspectives have simply not become the topic of serious discussion or debate – much less become topics an economics department will make sure to cover when considering hiring or curriculum! Students wanting to do undergraduate or graduate study in feminist economics hence face very limited options. And within the field of feminist economics, I've been disappointed by what seems to be a flagging of efforts toward critique. That is, when we started out a lot of energy was put towards identifying biases within the discipline – biases in how it is defined, in its models, and in its methods, as well as in how it portrays women and women's traditional activities. Barbara Bergmann, for example, was relentless on this.⁹ And many of us put a lot of thought into how the discipline could be improved, not just in its study of women, but also more generally by shedding the macho biases that obstruct the accomplishment of useful and reliable research. What I've seen increasingly, though, in the *Feminist Economics* journal and the conferences, are a preponderance of papers that use fairly conventional methods to study “women and ____ (fill in the blank).”

And it's not like the mainstream no longer needs critique! Studies of the gender wage gap, for example, are increasingly dismissive of discrimination as a cause, preferring to focus on women's “choices” regarding work/family issues (no matter how constrained those choices may be) and on presumed “gender differences in preferences.” I recently wrote a series of articles (and a book) which reviewed the literature on gender and risk-taking and which shows that the “gender differences” claim is based far more in stereotyped beliefs than in fact.¹⁰ In fact, the data show that men and women are far more similar than different on this score. I would like to see more feminist economists engaging in direct and pointed critique of the biased, sexist, unscientific work that still comes out every day. There's a lot that could be done conceptually, and through meta-analysis and replication. We feminist economists early on pointed out that science is a social endeavor that grows and gains reliability through just such cross-critique – and that individual researchers *p*-hacking their way to a “publishable” result does *not* give objectivity. Yet I'm afraid that insight might be fading.

⁸ Visit: International Association for Feminist Economics <http://www.iaffe.org>.

⁹ Note, see the special issue of *Feminist Economics* on Barbara Bergmann 4(3), 1998: <https://www.tandfonline.com/toc/rfec20/4/3?nav=tocList>.

¹⁰ See Nelson (2014, 2018b).

Jamie: I hope that is not the case and perhaps on a more optimistic note, the way to ensure such problems are not perpetuated is to influence the education of economists. You have done a great deal here too. Perhaps you might comment on your thinking on pedagogy and your contributions to textbooks. Early on in our interview you define “good science” as “open-minded and systematic investigation”. Arguably, based on traditional positivist and didactic approaches in economics, an open-mind is perhaps the most difficult attribute to inculcate in young economists.

Julie: I would agree. It’s much easier to teach students how to shift curves, solve equations, and run regressions, than to carefully observe economic life and think deeply and critically about it. Students also tend to feel comfortable – and even feel powerful – when told “here, we are handing you the exact tools and models you need to use to understand how the economy works.” We thought very carefully about the approach to take in the textbooks I worked on – the *Economics in Context* principles textbooks authored by Neva Goodwin, myself, and others (see Goodwin et al, 2019a, 2019b, 2014).¹¹ To make the textbooks adoptable by instructors, they needed to cover the standard topics and models. To make the textbooks attractive to students, students needed to feel that they would be learning useful and practical things. Many “alternative” economics textbooks, I think, rather go over beginning student’s heads by launching directly into tendentious debates between different schools of economics, leaving students perhaps frustrated and confused. Our approach was, instead, to teach the mainstream material as *ways some people have thought up* to try to explain the economy, which have some value and areas of applicability but which also have some (often serious) limitations. Teaching mainstream models as particular and partial views rather than as “how the world works” is a subtle but fundamental shift. And then we went on to fill in some of the rest of the story by looking at other perspectives on the economy. For example, in the chapter on consumption, we followed the neoclassical model with discussions of the origins of consumerism, the effects of advertising, and the environmental impact of consumerism.

Another fundamental but important shift was to list an additional economic activity *before* the usual three of “production, distribution, and consumption.” We called this “resource maintenance,” emphasizing that you can’t even begin production unless you possess (and protect) stocks of natural, physical, human, and social capital. This made it easy to integrate issues of environmental damage, the contribution of unpaid household labor, and the social strain that comes from extreme income inequality. Once, when I was teaching from *Microeconomics in Context*, I mentioned at the end of the semester that most econ textbooks don’t include “resource maintenance” as a major economic activity. The students were incredulous! It just made perfect sense to them. I didn’t directly work on the later revisions of the textbooks, or on the European edition, but I believe these themes have continued.

I’m glad, too, that the *RWER* has become an important place to get the word out about pluralist approaches in economics. I’ve enjoyed this conversation with you.

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¹¹ See the Economics in Context Initiative (<http://www.bu.edu/eci/education-materials/textbooks/>) for more information.

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